1-2018

In Sickness and in Health: The Influence of State and Federal Health Insurance Coverage Mandates on Marriage of Young Adults in the USA

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January 2018

Abstract: We study the effects of state and federal dependent health insurance mandates on marriage rates of young adults, ages 19 to 25. Motivated by low rates of coverage among this age group, state governments began mandating health insurers in the 1970s to allow adult children to stay on their parents' insurance plans. These state level efforts successfully increased insurance coverage rates, but also came with unintended implications for the marriage decisions of young adults. Almost all state mandates explicitly prohibited marriage as a condition of eligibility, thereby directly discouraging marriage. Additionally, by making access to health insurance through parents easier, the mandates made access through spouses' employers relatively less attractive. To the extent that young adults were altering their marriage plans to gain access through potential spouses, they no longer needed to do so under the mandates, thereby implicitly discouraging marriage. When the dependent coverage mandate of the Affordable Care Act (ACA) was enacted, it effectively ended the state-based marriage restrictions, thereby encouraging marriage among young adults previously eligible for state mandates. On the other hand, for those who were not eligible for state mandates, the ACA represented an attractive new path to obtain coverage, thereby discouraging marriage for these young adults, just as the state mandates had implicitly done previously for others. Thus, the separate efforts at the state and federal level to address low coverage rates for young adults ended up interacting and influencing incentives for marriage in opposite directions. We study these interaction effects on marriage empirically using a new dataset we compiled on state-level dependent coverage mandates. Consistent with theoretical arguments, we find that, before the implementation of the ACA, state mandates lowered marriage rates by about 2 percentage points, but this pattern reversed upon the passage of the ACA. We also find that state mandates increased the probability of out-of-wedlock births among state-mandate-eligible women as compared to ineligible ones, but the ACA reversed this trend as well. Our study provides an important example where fundamental understanding of the effects of the ACA dependent coverage mandate can only be had with full consideration of the pre-existing state laws.

[1] We gratefully acknowledge financial support from the Baldy Center for Law and Social Policy at the University at Buffalo. We thank David Neumark and participants at the 2017 iHEA Boston World Congress, the 2017 Southern Economic Association Annual Meetings, and the Georgia State University brown bag seminar for helpful comments. Excellent research assistance was provided by Smriti Bhargava, Ozge Bostanci, William Coates, and Kar Man Teoh. All errors, however, are our own.

1. Introduction

Health insurance coverage rates in the USA historically have been lower for young adults than for other age groups. One reason for this is that young adults are generally healthy and have less need for health insurance. Another important reason, though, is that they have limited access to group health plans. Seeking to increase access for this demographic, state policymakers were the first to mandate health insurers to expand the ages through which children could remain on their parents' group health coverage (for which they had historically been ineligible). Research has suggested that these state-based expansions were successful in increasing the number of adult children on their parents' insurance plans (Levine, McKnight, and Heep 2011; Monheit et al. 2011; Depew 2015), but these gains came with implications (perhaps unforeseen) for the marriage decisions of young adults. There were two main factors. First, the vast majority of states explicitly prohibited marriage as a condition to maintain eligibility. Second, by opening a previously unavailable source of insurance, the state mandates implicitly made other sources of health insurance comparatively less attractive. One of these alternatives was access to health insurance through a spouse's employer. To the extent that young adults were previously induced into marriage to gain access to health insurance through their (potential) spouses, the introduction of the state mandates provided them an easier path to coverage through their parents. Thus, the state mandates created disincentives on marriage both explicitly and implicitly.

With the Affordable Care Act (ACA) in 2010, the federal government followed the states' lead by adopting its own dependent coverage mandate, requiring all health plans to expand coverage eligibility to children of policyholders who were under 26 years old. This opened a broad pathway to insurance for young adults, who saw sizable increases in coverage after the ACA's implementation (Cantor, Monheit, et al. 2012; Akosa Antwi, Moriya, and Simon 2013;

Sommers, Decker, et al. 2013). While the federal mandate was like those of the states in raising the eligibility age, it was different in not restricting eligibility on factors other than age. The implication is that the federal mandate effectively superseded the states' restriction on marriage; young adults could stay on their parents' health insurance and get married. The federal law, therefore, removed one of the factors discouraging marriage in states with mandates. So, unlike when the state expansions occurred, the federal law encouraged marriage (relative to before it passed) for young adults who otherwise would have been eligible under state mandates. Thus, the separate efforts at the state and federal level to address low coverage rates for young adults ended up interacting and influencing incentives for marriage in opposite directions.

Our paper studies these interacting effects on marriage for young adults using a novel dataset we compiled on state-level, dependent-coverage mandates. We find that, before the implementation of the ACA, marriage rates of state-mandate-eligible ("eligible") young adults were as much as 2 percentage points lower than ineligible ones. This pattern reversed upon the passage of the ACA, however, with marriage becoming almost 3 percentage points more likely for eligible individuals. We also find that the magnitudes of these estimates increase with the extent that state mandates are enforceable, and that larger effects are estimated for students, who are unlikely to be able to obtain coverage through their own employers, leaving spouses and parents as their primary access points for health insurance. Additionally, we estimate these changes in marriage behavior affect the likelihood of young women having children out of wedlock. Before the ACA, we find more out-of-wedlock births among state-mandate-eligible women as compared to ineligible ones, but this result reversed afterwards. Thus, our empirical results are consistent with the theoretical expectation that the state and federal mandates have conflicting influences on the marriage and family decisions of young adults.

In our view, this study makes several contributions. First, it provides evidence, both theoretical and empirical, for important interactions between the ACA and state mandates. Previous research has typically studied these policies essentially independently, even those focused on the ACA, which came into existence in the years following several state-level expansions. In these studies, discussion of the possible influence of state mandates typically has been minimal. Our result, however, provides an example where fundamental understanding of the ACA effects cannot be had without full consideration of the state laws. This shows that more effort should be made to consider the ways interaction effects might manifest themselves in incentives and in resulting measured outcomes.

A second primary contribution of our work is showing that the response of young adults' marriage behavior, in particular, to the ACA mandate depends on their eligibility statuses for the state mandates. This is in contrast to an analysis by Abramowitz (2016), who argued that the ACA mandate uniformly resulted in fewer young adults marrying. Our analysis agrees with hers in part: young adults who were not eligible for state mandates faced a change in their incentive to marry after the ACA passed, much like the one faced by state eligible young adults previously. They could obtain insurance through their parents' plans, reducing the incentive to marry to gain access. But this change in their incentives was the opposite of that experienced by eligible young adults, and our empirical analysis is consistent with this reasoning. So, the interaction of the ACA with the state mandates is a critical aspect of how the ACA affected marriage, and our analysis provides the full picture, including the interaction effect.

A third main contribution is the introduction of a new data set on state-level dependent coverage mandates, compiled via comprehensive review of the history of state statutes. As compared to the previous papers that have contributed original research on the details of the state

mandate laws (Depew 2015; Levine, McKnight, and Heep 2011; Monheit et al. 2011; Cantor, Belloff, et al. 2012; Hahn and Yang 2016; Dillender 2014), our legal research is much more thorough, allowing us to correct numerous incorrect and incomplete data points, and to code the laws back in history far longer than other authors. Additionally, our coding is consistently focused on dependent coverage under large group insurance plan policies. We argue this is important because other plan types do not provide the cost savings or potential tax advantages that large group coverage offers, and therefore is much less likely to affect decision making. Moreover, the large group market is by far the most substantial. The implication of this new data is that our estimates of the state mandate effects and their role on the ACA implementation are more accurate than they would have been had we relied on the mandate data produced previously. Moreover, this new data provides a more accurate history that can serve as a basis for future research on these policies.

Finally, the variation these state laws provide allows us to introduce a more credible identification strategy into the literature that studies the effects of the ACA dependent coverage mandates. A number of authors have followed a strategy similar to that of Cantor, Monheit, et al. (2012), Akosa Antwi, Moriya, and Simon (2013) and Sommers, Decker, et al. (2013) by comparing young adults under the age of 26 to those above that age. Our state mandate law data allows us to contrast marriage trends among those under 26 without relying on a comparison group of individuals above that age, thereby minimizing the differences between the groups we compare.[2] Moreover, because we tracked the history of the state laws back in time, and because states had different ages and requirements for being covered by their mandates, our approach

[2] Abramowitz (2016) includes a robustness check of only 23 to 25 year olds that purports to compare marriage rates for state mandate eligible and ineligible individuals before and after the ACA passed. She restricted eligible individuals to be unmarried, however, so there was no variation in the outcome variable for these young adults. The effects of this is that her analysis only estimates the change in marriage rates for ineligible individuals over time.

primarily relies on within-state time variation, thereby reducing issues that come with comparing across states.

2. Restricted Pathways to Coverage and the Rise of Mandates

Young adults in the USA have comparatively low health insurance coverage rates and this rate was even lower before the enactment of the ACA. For those aged 19 to 25 (inclusive), the age group we define as encompassing young adults in this study, only about 69 percent had coverage in 2008 and 2009 (Rodean 2012). This contrasts with 90 percent coverage for all children under age 19 in 2008 and 2009, and 88 percent for adolescents 10 to 18 in 2009 (Mach and Blumenthal 2010; Cullen and Salganicoff 2011). One of the root causes of such low coverage rates was limited access for young adults to group health coverage. In the USA, the predominant source of health insurance is employer-provided health insurance, but young adults often do not yet hold jobs that offer group plan access, nor do their spouses (and they are often not married, anyway). In contrast to children under age 19, young adults were generally prohibited from dependent coverage by insurance plan policies that limited eligibility to dependents under age 19. So young adults were in a situation where pathways that would allow them to access coverage were significantly restricted.

To address this coverage issue, both state and federal governments have sought to leverage the ubiquity of group coverage among young adults' parents by mandating that insurers raise plan "limiting ages," the ages at which young adults age out of eligibility for dependent coverage. This approach began in Louisiana in 1974, which raised its limiting age to 24 (so that those under 24 could access dependent coverage), and was followed over time by many other states. By the time the ACA went into effect, 32 states already had adopted some type of

dependent coverage expansion for young adults. Levine, McKnight, and Heep (2011) argued that these expansions were successful at reducing uninsurance among the populations they affected by about eleven percent, but Monheit et al. (2011) and Depew (2015) found that while they increased dependent coverage of young adults, on net coverage was approximately unchanged. Regardless of which is correct,[3] by 2009 young adults were still covered at far lower rates than other age groups, as the statistics on coverage given at the beginning of this section show, leaving room for the ACA mandate to potentially improve coverage significantly.

The ACA was passed and signed into law on March 23rd, 2010, and its expansion of young adult eligibility for dependent coverage became effective for policy years beginning on or after September 23rd, 2010. Under this federal mandate, the limiting age for insurance plans that offered dependent coverage became 26, effectively superseding more restrictive state limiting ages. According to Cantor, Monheit, et al. (2012), Akosa Antwi, Moriya, and Simon (2013) and Sommers, Buchmueller, et al. (2013), the mandate decreased uninsurance among young adults by about nine percent during its initial stages of implementation. Sommers, Buchmueller, et al. (2013) argued the effect rose to 21 percent by the end of the first full year of the mandate.

Though the ACA and state mandates were similar in raising plan limiting ages, the policies had important differences. The most apparent of these were the additional restrictions that were common among state mandates. While the ACA allowed any children to access coverage through their parents' plans as long as they satisfied the age requirement (and the plans

[3] The quality of the evidence these papers provide regarding the effect on health insurance coverage has been a point of dispute in the literature (Burgdorf 2014; Monheit, Cantor, and DeLia 2015; Burgdorf 2015). Model specifications and particulars of ages and states included in estimation samples have been the focus of this discussion. It is likely, however, that a more fundamental problem could be an issue, as all three analyses were based on data on state mandates that was faulty in a number of ways. As only one example, the first five states to adopted expanded dependent coverage laws, Louisiana, Connecticut, Georgia, North Dakota, and Tennessee, were all categorized as not having any expansion laws by Levine, McKnight, and Heep (2011) and Monheit et al. (2011), while Depew (2015) coded two of these, Georgia and Tennessee, as having no state mandates.

offered dependent coverage), state mandates typically required young adults to satisfy an age limit *and* be unmarried *and* state residents. Less common, though not unusual, other requirements included young adults being students, financially dependent on their parents, without an alternative source of health insurance, and/or without children of their own. Additionally, state mandates did not have uniform limiting ages; they varied across states and time, from as low as 21 in Idaho and Illinois, to 31 in New Jersey, and even no age limit in Iowa and Rhode Island for students.[4] Beyond actual features of the mandates, state regulations also differed from the ACA mandate in the scope of plans to which they applied. The ACA mandate applied to all insurance plans in the country, but state mandates did not apply for self-funded group health plans (meaning they did not hire a third-party insurance company, but paid the cost of the benefits themselves) since such plans were not subject to state authority to mandate benefits.[5] As a substantial portion of employees with employer provided coverage were in self-funded plans, this is an important limitation to the scope of the state mandates.

Study of the effects of the state mandates has thus far been limited, though the small literature is still growing. Beyond those estimating the effect of the mandates specifically on health insurance coverage (Levine, McKnight, and Heep 2011; Monheit et al. 2011), there have also been in depth discussions of the state laws' features (Cantor, Belloff, et al. 2012), and studies of their effects on wages (Dillender 2014) and labor supply (Depew 2015; Hahn and Yang 2016). Depew (2015) is of particular relevance to our study since it contains a (brief) discussion of the possible effects of state mandates on marriage, failing to find significant effects. His estimates did, however, have the same sign as our pre-ACA estimates. That our estimates are

[4] These are ages which young adults were no longer eligible. That is, to have coverage, they had to be below these ages.
[5] This is due to the Employee Retirement Income Security Act of 1974 (Pierron and Fronstin 2008; Levine, McKnight, and Heep 2011; Monheit et al. 2011).

somewhat larger and more precise is a result that would be consistent with improved measurement of the (more frequent than Depew was aware) changes in state laws that our improved data on the state mandates brings. The literature on the effects of the ACA mandate is substantial, and French et al. (2016) provides a review that also covers other features of the ACA beyond the mandate.

3. Marriage and the Interaction Between State and Federal Health Insurance Mandates

According to Becker's (1973) theory of marriage, an individual voluntarily enters into a marriage only if the value of the marriage to the individual, $V(m)$, is greater than, or equal to, the value of remaining single, $V(s)$. That is, a necessary condition for entering into marriage is $V(m) \geq V(s)$. Ideally, health insurance plays no role in an individual's evaluation of these alternatives, though its place as an object of value to individuals can be formalized in this framework by defining $v(\cdot)$ to indicate all of the value of a being married or single *except for* health insurance, which we indicate by I. Then we can restate the necessary condition to enter marriage as $v(m) + I \geq v(s) + I$. When marital state plays no role in the access of an individual to health insurance, as in this inequality, then insurance indeed does not play a role in the marriage decision, since its value is effectively canceled out. We refer to this scenario as the "ideal" case, and define the parameter θ to indicate this benchmark expected rate of marriage for young adults in this situation.

As our discussion in the section above makes clear for young adults, though, a combination of insurance plan policies and government mandates resulted in a reality where access to health insurance quite often *did* depend on marital status. When this happens, the marriage decision condition becomes imbalanced, missing I on one side or another, and so the

availability of insurance will make the difference between marrying or not for some individuals. For the purpose of our analysis of the effects of the state and federal mandates for dependent coverage, we will compare the implications of each of the four mandate coverage possibilities to the ideal case.

Case 1: *Ineligible for both state and ACA mandates.* Before the enactment of the ACA, young adults who were not eligible for a state mandate could not typically access group health insurance through their parents' employers. As a result, access to group coverage could only be had via own or spouse employers. For those who could access insurance through their employers, the marriage condition was balanced, so marriage decisions would not be affected. For many, though, access through their own jobs was not possible, leaving spousal coverage as the only remaining possibility. For the subset of individuals with potential mates, if access could not be gained through marriage, then the marriage decision is again unaffected, since insurance is not available in either state. But for those where access was available, the marriage condition is imbalanced, $v(m) + I \geq v(s)$, favoring marriage as compared to the ideal case where insurance is available whether married or not. Thus, for young adults ineligible for state mandates before the ACA enactment, the marriage rate is predicted to be higher than it would be in the ideal case. To state this conclusion in a manner that is consistent with our econometric model, we define three dummy variables, *Y*, *ELIG*, and *ACA*, to indicate young adults who are married, eligible for a state mandate, and eligible for the ACA, respectively. We then have:

$$E[Y|ELIG = 0, ACA = 0] > \theta.$$

Case 2: *Ineligible for state mandates, but eligible for the ACA mandate.* In this case, because the ACA allows young adults to access their parents' coverage regardless of their marital state, and a

large bulk of young adults' parents have employer provided coverage, a substantial portion of young adults who are considering marriage face a decision condition that is balanced as in the ideal case. That is, $v(m) + I \geq v(s) + I$, which implies that $E[Y|ELIG = 0, ACA = 1] = \theta$. Since marriage is not favored as it is in Case 1, this implies that the rate of marriage for these individuals (in Case 2) should be lower than for those in Case 1, which can be formalized as:

$$E[Y|ELIG = 0, ACA = 1] - E[Y|ELIG = 0, ACA = 0] < 0. \quad (1)$$

Stated differently, for those not eligible for state mandates, the ACA should result in a lower marriage rate.

Case 3: Eligible for a state mandate, but not the ACA mandate. Before the enactment of the ACA, state mandates provided access for a substantial portion of young adults to parental coverage that usually required them to remain unmarried to be eligible. In this case, which is more common, individuals with potential mates get married when $v(m) \geq v(s) + I$, where I is limited to the single state because of the explicit restriction in the law. This imbalance suggests the marriage condition is less likely to be satisfied than in the ideal case, resulting in a comparatively lower marriage rate. In cases where the state mandate did not have a marriage restriction, which is far less common, young adults face a balanced marriage condition, suggesting marriage rates should not be different than in the ideal case. On balance, then, for the whole group of individuals eligible for a state mandate but not the ACA, we expect a marriage rate lower than in the ideal case: $E[Y|ELIG = 1, ACA = 0] < \theta$. This implies also that marriage rates would be lower for these young adults than they would be in Case 1:

$$E[Y|ELIG = 1, ACA = 0] - E[Y|ELIG = 0, ACA = 0] < 0. \quad (2)$$

Hence, there is an unambiguous prediction for a lower marriage rate for state mandate eligible young adults as compared to ineligible ones before the ACA.

Case 4: Eligible for both state and ACA mandates. Like those situated as in Case 2, the ACA mandate implies marriage does not affect access for a substantial number of young adults, allowing them to decide on marriage without concern for insurance, so that approximately we have $E[Y|ELIG = 1, ACA = 1] = \theta$. This means that rates for those eligible for both types of mandates will be higher than those eligible for state mandates only, as in Case 3:

$$E[Y|ELIG = 1, ACA = 1] - E[Y|ELIG = 1, ACA = 0] > 0. \quad (3)$$

Stated as a prediction for the effects of the ACA, this claims that for individuals eligible for state mandates, the ACA passage should cause increased marriage rates for young adults as it effectively *removes the constraint* imposed by the state mandates.

The key lesson we learn from comparing these four cases is that both the state and ACA mandates play potentially important roles in the marital decisions of young adults. It also shows, critically, that their interaction also has important implications.[6] This second point is clear from the contrasting predictions in inequalities (1) and (3), which show that the direction of the predicted effect of the ACA mandate depends on eligibility for state mandates. Our attention to this point is one of the main distinguishing characteristics of our analysis of the ACA from that of Abramowitz (2016). While our reasoning for the predicted response for those individuals situated as in Case 2 is the same as hers, our inclusion of the state mandates in our analysis of the predicted effects allows us to uncover a much more complete picture of the effects of the ACA. As the discussion in this section shows, the predicted effects of the ACA are much more nuanced than argued by Abramowitz (2016).

[6] Our argument that health insurance mandates are influential on the marriage decision is consistent with other research that argues for a relationship between health insurance access and marriage in contexts beyond government mandates (Yelowitz 1998; Decker 2000; Zimmer 2007; Lavelle and Smock 2012; Peters, Simon, and Taber 2014; Sohn 2015).

Before moving on to our detailed discussion of our empirical analysis, we would like to note two important considerations. First, while we specify three distinct predictions for the effects of mandates on marriage, our strategy will not be to estimate and test each of these separately. Instead, we will only test inequality (2) individually, and will estimate and test a combined version of inequalities (1) and (3). Whereas inequality (1) states that the ACA reduces marriage rates among individuals not eligible for state mandates, and inequality (3) postulates that it increases marriage for those who were, the combined version states that the net difference between these effects is positive. That is, the ACA effect on state eligible individuals minus that on the ineligible is positive:

$$E[Y|ELIG = 1, ACA = 1] - E[Y|ELIG = 1, ACA = 0]$$
$$-(E[Y|ELIG = 0, ACA = 1] - E[Y|ELIG = 0, ACA = 0]) > 0. \quad (4)$$

This combined inequality represents an unambiguous prediction for post-ACA behavior that we test instead of testing inequalities (1) and (3) separately. The advantage of this approach is it allows us to improve our empirical strategy by using only a narrow age range of young adults for our sample instead of relying on a comparison group of older individuals, as we discuss more below. Second, one concern may be that young adults are likely to have low personal valuations of health insurance given their relatively good health, implying weak potential responses to changes in insurance availability. This concern is certainly valid, but the counter to this argument is that young adults are the age group for which adjustment of marriage plans is easiest. As adults age, desires for traditional family structure become stronger, making changes in timing costlier. We, therefore, argue the net result is that young adults represent a reasonable population for which to ask our question of interest.

4. Empirical Strategy

4.1 Data and Estimation

Our analysis relies on a dataset created by pooling annual, cross-sectional, public-use microdata from the American Community Survey (ACS) from years 2000 through 2015 (Ruggles et al. 2015). The ACS has a combination of two key features making it ideal for our analysis. The first is a large sample size, which is necessary given our focus on the relatively narrow age range of young adults. The second is a long period of coverage, which allows us to take advantage of a significant portion of the time variation in the state dependent coverage mandates to identify model parameters. To capture the effects of the state mandates and their interaction with the ACA, we restrict our sample to respondents with ages 19 to 25 (inclusive). This age group was predominantly targeted by state expansions, which typically increased limiting ages to the 23 to 26 range. We exclude those under age 19 since before the ACA and absent a state mandate, the traditional limiting age for access to parents' group coverage was 19. Those over 25 are excluded since the ACA set limiting ages nationally to 26 years.

In setting our focus on only those ages that are covered by the ACA mandate, we make a significant break with the previous literature that studies its effects. Earlier authors have primarily based their empirical studies on the framework used by Akosa Antwi, Moriya, and Simon (2013) and Sommers, Buchmueller, et al. (2013), and compare young adults up to age 25 (those covered by the ACA) to those who are age 26 or older (those not covered by the ACA). This approach has been perceived as necessary because the ACA itself does not have any variation in eligibility for ages below 26, but it has an important limitation in that it results in a comparison of groups with very different ages and life stages. Young adults in their early to mid-twenties likely have very different levels of demand for insurance and (of particular importance in our case) marriage than those in their late-twenties or early-thirties. In our context, though, we

do not have this limitation because the pre-existing state mandates create de facto variation in the effects of the ACA implementation. This creates considerable advantages for our analysis since it allows us to identify effects of the law using only individuals who were eligible for the ACA, and so the ages and life stages of individuals being compared are much more similar than those in previous studies.

Our main outcome of interest, which we denote by *Y*, per the notation defined in the previous section, is an indicator variable for whether an individual was married or not at the time of his or her ACS interview.[7] This contrasts with Abramowitz (2016), who uses an ACS question that asks if the respondent was married within the last year of the interview. This variable is only available in the ACS since 2008, so our use of marital status allows us to extend our sample and incorporate more state mandate variation. Additionally, we argue that given the nature of the state mandates, marital status is the more appropriate choice since the laws condition on marital status specifically. Thus, changes in marriage rates will fully reflect the effects of the policy, whether they come through flows into or out of marriage.

To estimate the effects of the state and federal coverage mandates on our marriage outcome variable, we use variations of the following difference-in-differences (DD) style econometric model:

$$Y_{iast} = \beta_1 ELIG_{ast} \times ACA_t + \beta_2 ELIG_{ast} + X'_{iast}\gamma + \alpha_a + \delta_s + \eta_s \times t + \theta_t + u_{iast}. \qquad (5)$$

Y_{iast} indicates the marital status of respondent *i* of age *a* living in state *s* in year *t*, while $ELIG_{ast}$ is a binary variable identifying individuals who are eligible for access to their parents' group health insurance coverage (if such coverage is offered). As specified by the indices, our measurement of eligibility is determined by three things: a young adult's age, state of residence,

[7] We count an individual as married whether or not their spouse is present, but not if they are separated.

and year of interview. For a given year, a young adult is coded as eligible if he or she is living in a state with a mandate in effect, and is younger than the mandate limiting age. So, for example, in the case of a young adult of age 24 living in Georgia in 2003, since Georgia mandated dependent coverage through age 24 for full-time students at that time, we would impute the individual as eligible (*ELIG* = 1). For another young adult in Georgia in the same year but age 25, we would impute him or her as ineligible (*ELIG* = 0). For an individual in a state without a mandate, like California, where there was no mandate at any point, then he or she would be imputed as ineligible regardless of age. Years in which the ACA dependent coverage mandate was in effect – 2011 and later – are indicated by ACA_t (since the ACA mandate did not go into effect until late 2010, we code the law as in effect for 2011, not 2010).[8] Age, state, and year fixed effects are denoted by α_a, δ_s, and θ_t, respectively, $\eta_s \times t$ represents state-specific, linear (or quadratic) time trends, and u_i is an error term. Additional individual-level demographic and environment control variables are contained in the vector X_{iast}. Individual-level controls include dummies for race, Hispanic ethnicity, gender, and the full interaction between age and gender.[9] Environment characteristics are measured at the age-state-year level and include the unemployment rate, to reflect macroeconomic conditions, and the male population ratio, as a proxy for the marriage market. We do not control for education or student status since these can

[8] Akosa Antwi, Moriya, and Simon (2013) and Abramowitz (2016) both used data with enough detail to differentiate between the period of enactment before required implementation from the period after, finding some pre-implementation effects. Given our annual data, though, we treat the ACA consistently with the way we code the state mandates, where we code changes that occur after the mid-point of the year as occurring at the beginning of the following calendar of the year. Additionally, Cantor, Monheit, et al. (2012) uses annual CPS data for their analysis of the ACA effect on health insurance, finding effects despite the fact that the only "after" period data they have is from 2010. The CPS, however, asks about health insurance without reference to any particular time period within the year. Given the widespread awareness of the ACA, and its relevance to the question about health insurance, it would not be surprising if responses were reflecting the later part of the year after the mandate had gone into effect. In our case, the ACA asks specifically about marital status at the time of the interview. Thus, including 2010 as post-ACA would be misclassifying the vast majority of observations for that year.

[9] Race categories (including omitted group) are white, black, native American, Chinese, Japanese, other Asian, other race, two races, and three or more races. Hispanic ethnicity categories (again, including omitted group) are non-Hispanic, Mexican, Puerto Rican, Cuban, and other.

be affected by eligibility criteria (via school enrollment requirements in state mandates) and are likely to be jointly determined with marital status.

Given our age restrictions and the range of years included in our sample, the dataset we use to estimate equation (5) consists of 2,994,479 observations of young adults, of which 49 percent were women and 15 percent were married at the time of their ACS interview. Summary statistics for our outcome and control variables can be found in *Table 2*. All sample statistics and regression models are estimated using ACS person weights, and all standard errors account for state-level clustering (Bertrand, Duflo, and Mullainathan 2004).[10]

4.2 Interpretation and Identification of Parameters

Equation (5) has two primary parameters of interest, β_1 and β_2. The effect of state mandates before the enactment of the ACA is measured by β_2, while β_1 measures the net difference in effects of the ACA on state mandate eligible and ineligible individuals. In terms of the predictions given in section 3, β_2 represents the effect in inequality (2), while β_1 represents that of inequality (4). The representations given in our empirical model, equation (5), however, are refined as compared to inequalities (2) and (4), and take advantage of the nature of the state mandates, which varied across states, time, and age groups within states, allowing for more credible measurement.

In the case of the effect of the state mandates before the ACA, rather than measuring the effect by simple comparison between eligible and ineligible individuals, the parameter β_2 has a differences-in-differences (DD) interpretation. The primary comparison used for identification is of young adults with ages lower than mandate thresholds to those with ages above within states

[10] All regression parameter estimates and standard errors are calculated using the "regress" command in Stata/MP 13.1 for Windows (StataCorp 2013).

with mandates that are in effect. To account for differences across age groups, a comparison group comprised of two types of young adults is used. One category of individuals used in this control group is those who lived in mandate states during periods before the mandates were adopted; the other is those who lived in states without mandates during periods when mandates were in effect (in other states). To state this more clearly, denote the age groups that are expansion eligible according to state mandates by A' and those that are not by A. Similarly, states with and without mandates are S' and S, respectively, and periods with mandates in effect are T', and T are those when they are not. Using these indices, we can state the identification of β_2 as coming from the following DD:[11]

$$\beta_2 = \begin{pmatrix} E[Y|ELIG = 1, ACA = 0, a = A', s = S', t = T'] \\ -E[Y|ELIG = 0, ACA = 0, a = A, s = S', t = T'] \end{pmatrix} - \begin{pmatrix} E[Y|ELIG = 0, ACA = 0, a = A', (s = S', t = T) \cup (s = S, t = T')] \\ -E[Y|ELIG = 0, ACA = 0, a = A, (s = S', t = T) \cup (s = S, t = T')] \end{pmatrix}. \quad (6)$$

Since this DD has inequality (2) as its basis of comparison, we therefore expect that $\beta_2 < 0$.

For the interaction effect of the ACA and the state mandates, β_1, identification in equation (5) is given by:

$$\beta_1 = \begin{pmatrix} E[Y|ELIG = 1, ACA = 1, a = A', s = S', t \geq 2011] \\ -E[Y|ELIG = 1, ACA = 0, a = A', s = S', t < 2011] \end{pmatrix} - \begin{pmatrix} E[Y|ELIG = 0, ACA = 1, a = A, s = S', t \geq 2011] \\ -E[Y|ELIG = 0, ACA = 0, a = A, s = S', t < 2011] \end{pmatrix}. \quad (7)$$

Here there are two important differences from the case of β_2. First, since the ACA affects everyone in our sample at the same time, there is no "control" group where the ACA effect is

[11] This result depends on the assumption that $E[X'\gamma]$, conditional on the included fixed effects, does not vary across age groups, states, and years.

theoretically zero. As discussed in section 3, though, the effect of the ACA on those who were eligible for state mandates is predicted to be positive, while the effect on those who were not eligible is predicted to be negative. The difference, therefore, should be positive, implying that we should expect $\beta_1 > 0$. Thus, despite that there is not a control group as there would be in typical applications of DD-style identification, we still have an unambiguous prediction that can be tested using the model in equation (5).

The second important difference is that identification here comes only from comparing across age groups over time *within* states with mandates. This is due to a conscious judgement on our part that within state variation is preferred to cross state variation. The need for judgement arises because the ACA effected everyone at the same time, so as compared to identifying effects of the state mandates, we have one fewer dimension of variation available to estimate the effect of the ACA. We, therefore, could identify the ACA effect two different ways, by comparing over time individuals of eligible ages across states with and without mandates, or by comparing over time individuals of eligible and ineligible age groups within states with mandates. We argue this second approach is preferable because the differences across states are likely to be more important than across ages, particularly during the time period the ACA was enacted when the country was still in the depths of the Great Recession. By focusing on within state variation to identify this parameter, we avoid issues with differences between states in the effects of the recession. Our preference for comparison within states for the β_1 parameter is why equation (5) includes state-specific time trends but not age-by-year fixed effects, which would add a cross-state component to its basis for identification.

Figure 1 presents a stylized example of the reasoning behind the signs we expect for β_1 and β_2. To make this example as simple as possible, we assume there is only one state mandate

before the ACA becomes effective. Since we argue the state mandates discouraged marriage before the ACA, we expect β_2 to be negative. Once the ACA is passed, we argue the federal mandate interacts with the state mandates, resulting in marriage increasing among individuals affected by state mandates, and decreasing among the unaffected. On net, then, the combination of these two effects (labeled D_1 and D_2 in the figure), and β_1's predicted sign, is positive.

Finally, in addition to discussing precisely how we identify the parameters of interest in our model, it is also useful to discuss one potential approach that we *did not* use: the triple-difference (DDD). Given the fact that we have three dimensions of variation in our data (age groups, states, and time), it is possible for us to estimate a DDD-style model. We argue, however, that this approach is not appropriate. For a DDD model, it is necessary to have two control groups that are not affected by any treatment. In our case, though, because the ACA affected everyone in our data, we do not have even one true control group, let alone two. It is simply because of the circumstances of the state mandates that the parameters of a DD-style model have unambiguous predictions, but this is not the case for a DDD-style model. If we were to adapt our approach to a DDD model, we would be (in principle) using individuals in non-mandate states to adjust for the changes over time in the difference between eligible and ineligible age groups that have nothing to do with the ACA. Individuals in non-mandate state *were* affected by the ACA though, and there is theoretical reason to expect their marriage rates to be influenced. The theory does not include predictions for changes over time in differences between groups, though, so the third difference would have an ambiguous effect on estimates. Thus, there would be no clear interpretations for the parameters of a DDD-style model. We, therefore, rely solely on the DD approach which provides for clearly interpretable estimates.

5. State Dependent Coverage Mandates

5.1 A New Dataset on State Mandates

Given the importance of the state mandates to our analysis, our study requires accurate characterizations of them: when, where, and how they went into effect. While several papers have performed analyses on these laws (Levine, McKnight, and Heep 2011; Monheit et al. 2011; Cantor, Belloff, et al. 2012; Dillender 2014; Depew 2015; Hahn and Yang 2016) we found their characterizations of the laws to be deficient, particularly with regards to the history of mandates and the timing of changes. We, therefore, performed an extensive and thorough review of the histories of state statutes, carefully uncovering state mandates and tracing back their histories. We found that the history of state mandates is far more extensive than was understood by previous researchers. Thus, in addition to contributing to our understanding of the effects of mandates on marriage, this study also introduces into the literature a new dataset of characterizations of the state dependent coverage mandates. This new dataset is much more extensive than was previously available, as we trace histories of laws in all but one case back to at least 1990. Moreover, we argue that it is much more accurate than previous efforts, and provide evidence of this via the extensive detail on our research provided as an online appendix to this work.

One state that is emblematic of the problems we found in previous authors' data on state laws is Louisiana. As we previously mentioned, Louisiana was the first state to establish a mandate, doing so in 1974. Three of the previous studies treat Louisiana as having no mandate in their analyses (Levine, McKnight, and Heep 2011; Monheit et al. 2011; Hahn and Yang 2016) while the other three treat it as first establishing a mandate in 2009 (Cantor, Belloff, et al. 2012; Dillender 2014; Depew 2015). Not only was the law already established at that point, but its substance did not change in 2009, either. The numbering system used by the state for its laws did

change, though, which might explain why these authors believed the law was established that year. They may have found a current version of the law and traced that back to the statute number change, and been unable to trace it any further back. Additionally, Cantor, Belloff, et al. (2012) and Depew (2015) mistakenly claim that there was a mandate for expanded coverage for non-students. This mistake likely resulted from a misinterpretation of a part of the relevant state statute, which did not apply to standard health insurance plans (it applied for limited benefits plans like certain dental and vision plans, and short-term, limited plans). In contrast, our research indicates that after the law was established, it did not change in a way that was meaningful for typical families[12] until after the ACA mandate went into effect, when the state adopted rules that essentially duplicated those of the ACA.

Table 1 reviews our research on the state mandates and compares it to the research of previous authors. Additionally, *Figure 2* graphically summarizes the time variation in the state mandates. Together, these make clear that the history of state mandates goes back much further than was known by previous authors, and that our research corrects numerous inaccuracies. The online appendix to this paper provides much more detail on the state mandates, including the text of relevant statutes and discussion of our interpretation of them. Additionally, for the purposes of clarifying our implementation of the mandates in our analysis, we consider some examples of mandate texts below.

5.2 Examples and Implementation of Mandates

In addition to serving as an example of why new research into the state mandates was needed, Louisiana also serves as a good example for the type of language found in the state

[12] A 1997 expansion allowed non-students experiencing mental health problems, a small portion of the population, to maintain parental coverage.

statutes, as it exhibits some typical eligibility requirements. From the Louisiana Revised Statutes, § 22:215.4, as effective July 12, 1974:

> *Students who are unmarried children who have not yet attained the age of twenty-four and who are enrolled as full-time students at an accredited college or university, or at a vocational, technical, vocational-technical or trade school or institute, or secondary school, and who are dependent upon the primary insured under any group health and accident or franchise health and accident insurance policy issued in this state for their support, shall be considered as dependents under the provisions of said policy.*

Here the law specified a limiting age of 24 and that it applied to unmarried students who were also financially dependent on their parents (as indicated by the line "who are dependent upon the primary insured…for their support"). The requirement that the child be unmarried was prevalent among states with mandates, 28 of which had some type of marriage restriction over their history. We also note that as part of the student requirement, the statute specified the types of schools that qualified and that full-time enrollment was necessary, language that is common among other states, as well. That said, not all states with school requirements required full-time enrollment. In some cases, part-time enrollment was sufficient, such as in Florida and Rhode Island.

While Louisiana's original mandate, having marriage, student status, and dependency requirements, was relatively restrictive, Texas' and North Dakota's original mandates are examples of mandates with fewer and different iterations of restrictions. Texas' mandate had fewer restrictions and a higher limiting age (though it came much later). Per the Texas Insurance Code article 26.84, as operational January 1, 2002: "If children are eligible for coverage under the terms of a large employer health benefit plan, any limiting age applicable to an unmarried child of an enrollee is 25 years of age". Here the Texas government set the limiting age relatively high at 25, and did not include a dependency or student status requirement, only requiring eligible children to be unmarried. Thus, Texas' mandate allowed as relatively large population to

become eligible as far back as 2002. North Dakota's mandate was the other side of the coin to Texas'. Like Texas, it did not require dependency, but mandated student status but not marriage. From the North Dakota Century Code § 26.1-36-22, as effective in 1985:

> *A policy that provides that coverage for a dependent child of an employee or other member of the covered group terminates upon attainment of the limiting age for dependent children specified in the policy does not operate to terminate the coverage of a dependent child while the child is a full-time student and has not attained the age of twenty-three years of age; or while the child is and continues to be both incapable of self-sustaining employment by reason of intellectual disability or physical disability and chiefly dependent upon the employee or member for support and maintenance….*

Thus, North Dakota's mandate was in between Louisiana and Texas in the sense that it was more expansive than Louisiana on dependency and both Louisiana and Texas on marriage, but less inclusive than Louisiana and Texas on limiting age, and less extensive than Texas on the student status dimension. Considering its position in the development of the state laws over time, coming more than a decade after Louisiana's mandate and more than fifteen years before Texas', its language being a middle ground of sorts seems quite natural.

Another less common, though not unusual, eligibility requirement is residency, either within a state or with a parent policy holder. Residency with a parent is a requirement that, in our view, would be extremely difficult for insurers to enforce, and so does not amount to a true restriction on eligibility. We implicitly treat state residency as being satisfied in all cases in our analysis, since we treat individuals interviewed in a given state as being residents of that state. One situation in which this might not be true, however, is when a student attends a school in a state that is not his or her state of residency, and ends up being interviewed there by the ACS. Additionally, in some cases, laws have requirements that a child be a state resident *or* a full-time student. In this situation, a student who attends school out-of-state would potentially be eligible

for coverage in his or her home state, but we would not impute that eligibility to him or her because we would not observe the student's home state in the data. Thus, eligibility of out-of-state students may be mismeasured to some degree in our analysis, though we believe this issue to be small given that out-of-state students represent a small portion of the population.

Implementation of the state mandate language into our quantitative analysis requires significant use of judgement regarding interpretation of language of the mandates, most of which is covered in detail in the online appendix. There are five important choices, however, that we applied systematically in our analysis and that merit being mentioned here. First, since we only observe the calendar year of the ACS interviews in our data, we had to use some rule to determine years in which mandates would be considered to be in effect, since they were implemented during various times of the year, not only at the beginning. To minimize misclassification, we assume mandates go into effect at the start of the years closest in time to the actual dates of implementation. So, for effective dates before July 1st, we code the law as being in effect for the whole year, while for dates on July 1st or after, we code the law as being in effect the *following* year. This rationale was also applied to the ACA mandate, which became effective in late 2010, and which we code as being effective beginning in 2011.

Second, our research focused on mandates covering the large group insurance market. Texas' mandate given above provides an example of a law that specifies it applies to such a market. The law makes clear that it applies to "large employer" plans (defined as 51 employees or more), and not to other types of plans in the small employer and individual markets. This is not unusual, but is not found in all states' laws. Some states specify mandates for plans in all insurance markets, some only for group markets, and some only for individual or small employer markets. For our research, we focused exclusively on the large group market since these types of

plans are the most desirable. They offer significant cost savings and, in many cases, tax advantages that make them much more likely to be the types of plans people will alter their behavior, and perhaps even their wedding plans, to obtain. Moreover, the large group market makes up the substantial majority of the health insurance coverage. Mandates for other markets are not recorded in our mandate dataset, and are not considered in our analysis.

Third, despite that state mandates had various types of restrictions on eligibility, we only consider individuals' states, interview years, and ages when imputing eligibility to individuals in our data. Since marriage is our outcome of interest, clearly we cannot use marital status as an eligibility restriction when imputing eligibility. Other requirements, though, – and most importantly that of being a student – are also inappropriate to use for eligibility imputation because they are jointly determined outcomes. For example, as state mandate might induce individuals into or out of student status, so using it to determine eligibility would introduce bias (which is a point also made by Depew (2015)). We, therefore, do not consider those requirements when dividing individuals into eligible and ineligible groups. The disadvantage of this approach is that some individuals who we impute as eligible would actually not be, and so any true effect of the mandates on marriage that exists would be diluted in our estimates. Nevertheless, we view this approach as conservative, and argue our estimates would therefore represent lower bounds to the true effects of the mandates. Additionally, in the case where a state specifies more than one limiting age, which typically occurs when there is one for students and one for non-students, the greater age is the one used for imputation. To do otherwise would be equivalent to using student status or other jointly determined characteristics in determining eligibility, an approach we avoid.

Fourth, when the ACA was implemented near the end of 2010, it effectively made state mandates irrelevant for nearly all the individuals in the age group we study since it had no eligibility criteria except for age,[13] which is set high enough that all the individuals in our data are ACA mandate eligible. Nevertheless, state mandates continued to exist in state laws and some states continued to alter their laws after the implementation of the ACA (as *Table 1* and the online appendix detail). The existence of the state laws may have had some additional effect, perhaps due to easier enforcement in some cases, but in our view these effects *must* be much closer to zero than whatever they were before the ACA. In our analysis, therefore, we "freeze" the state mandates as they were in 2010 for all the following years. That is, the *ELIG* variable in our estimation varies in response to changes in state mandates up-to and through the year 2010. From 2011 and onward, though, the ELIG no longer changes with state mandates, and only reflects the laws as they were in the year 2010. So individuals imputed as eligible in our analysis for the years 2011 and onward are imputed that way on the basis of the state mandates that existed in the year 2010. Our rationale for this approach is to avoid diluting our estimates of the ACA effect (which comes from the interaction of the *ACA* and *ELIG* dummies) averaging in effects of post-ACA changes in state mandates that must have effects of zero, or close to it. Moreover, this approach maintains the populations being compared to estimate the ACA effect as they were when the ACA was passed, so that the ACA "treatment" and "comparison" (eligible and ineligible) groups do not change in state or age characteristics over time.

Fifth, and finally, nearly all states had mandates that specified continued coverage on parental plans of children suffering from disabilities (generally for as long as the disability continued). The mandate for North Dakota, given above, serves as one example of this sort of

[13] Additionally, dependent coverage had to be offered by a parent's insurance plan, but this was also true of state mandates.

requirement. For our analysis, we do not consider these mandates for two reasons. One is the prevalence of these mandates, which provides little, if any, actual variation in the laws across states. The second is that the marriage market and decision is probably more complicated for disabled individuals, so the effect of the mandates on them would be very different from that of non-disabled young adults. Thus, we exclude disabled individuals from our sample, and our estimates should be viewed as applying for the non-disabled population of young adults.

6. Results

6.1 Main Results

Table 3 presents our main results from estimating equation (5), which show that the state mandates lowered marriage rates for eligible young adults as compared to ineligible ones, but that this pattern was reversed after the implementation of the ACA. This is consistent with our theoretical argument that the ACA did not uniformly lower the marriage rate. The columns present separate estimates for the main variables of interest from different specifications. Estimates in column (1) were produced by a model with state-specific, linear time trends, while the model in column (2) adds quadratic state time trends. In both cases, the estimates for the coefficient on *ELIG*, β_2, have the expected sign of negative and are statistically significant, indicating that during the period before the ACA was enacted, eligible individuals had lower marriage rates than ineligible individuals. Meanwhile, for the interaction term coefficient, β_1, which measures the effect after the enactment of the ACA, estimates are positive and significant, again consistent with expectation. This implies that the marriage rate gap between eligible and ineligible individuals narrowed in comparison to the pre-ACA period. For both coefficients, column (1) has the smallest estimates in magnitude in *Table 3*. Here, the state mandates are estimated to have lowered marriage rates for eligible young adults by 0.8 percentage points (p-

value < 0.05), while the ACA is estimated to have nearly completely closed the marriage rate gap, reducing difference 0.7 percentage points (p-value < 0.1). Compared to the average marriage rate in our sample, 15 percent, these represent changes in marriage rates of about 5 percent.

In column (3) we generalize the model specification by replacing the polynomial state time trends with state-by-year interaction dummies, and then in column (4) we add state-by-age interaction fixed effects. Like the first two models, the column (3) model produces estimates that are statistically significant and with signs that are consistent with our expectations. Marriage rates are estimated to be reduced by 2.0 percentage points due to the state mandates (p-value < 0.05), but this difference is increased by 2.8 percentage points (p-value < 0.01) by the ACA. This second estimate indicates that the ACA not only closed the gap between the eligible and ineligible, but eligible individuals' rates even slightly exceeded those of the ineligible. These estimates are sizable, representing changes of roughly 13 percent and 19 percent, respectively, as compared to the overall marriage rate in our sample. Once the state-by-age fixed effects are added to the model, the effect of the state mandates is no longer estimated to be statistically significant, though this is entirely because of reduced precision, as the point estimate is basically the same as that in column (2). The estimate for the post-ACA effect, however, is the largest of those presented in the table at 4.7 percentage points (p < 0.01). This estimate is substantial, but we emphasize that the post-ACA estimates reflect the combination of the ACA's effects on the two groups: the increase in marriage for eligible individuals plus the reduction for ineligible ones.

The model presented in column (4) is the most flexible one that we use, as it controls for differences in marriage rates between each state-by-age group. Despite this advantage, though, the model in column (3) is our preferred specification. Given that the ages of the individuals in our sample are so similar already, and that we already have age fixed effects in all models, we

argue the variation that is lost from introducing the state-by-age fixed effects is too costly to justify the limited benefit they bring in generalizing the specification.[14] It is extremely important, however, to account for differential time trends across states in as general fashion as possible given that our sample period overlaps the Great Recession. Since the model in column (3) includes this feature without also giving up the variation lost in column (4), we argue it represents the best approach. Nevertheless, we find the fact that both models produce the same sign patterns to be reassuring.

Columns (5) through (8) of *Table 3* present a second set of estimates based on the same set of model specifications as in the first four columns, but adding an additional dimension of variation to strengthen the tests of our hypotheses. Under the Employee Retirement Income Security Act (ERISA), states may regulate the health insurance plans that are purchased from insurance companies, but not self-funded plans (Pierron and Fronstin 2008; Levine, McKnight, and Heep 2011; Monheit et al. 2011), implying a limitation to the ability of states to impose mandates on health plans. Any effects we observe, then, should be stronger in states that have more insured individuals in plans under their regulatory authority. To explore this further, we use a variable measuring the state-level share of employees covered by fully-insured health plans based on the Medical Expenditure Panel Survey (MEPS) Insurance Component (Agency for Healthcare Research and Quality 2000 - 2015), which we interacted with our two main variables of interest.[15] Since this variable increases with the share of employees in the fully insured plans, estimates in columns (5) through (8) that have sign patterns that are the same as those of columns (1) through (4) would be evidence consistent with our theory and the evidence from the first four

[14] We present additional evidence of the robustness of our estimates to age in the next table.
[15] Estimates for the share of fully-insured plans were not made available by AHRQ for some state-year combinations due to insufficient sample sizes in the underlying MEPS Insurance Component data, nor any state for 2007 since the Insurance Component data was not collected that year (Agency for Healthcare Research and Quality 2017). Thus, models that incorporate the fully-insured variable have smaller sample sizes due to this data unavailability.

models. As *Table 3* reports, this is precisely what we obtain. All four models produce estimates with the expected signs and all except two are statistically significant at the 5 percent level or lower (just as in the first four models). These results indicate that, if all health plans were fully insured by third party companies, state dependent coverage mandates would lower marriage rates by about 1.8 to 5.1 percentage points, and the post-ACA effect would have ranged from 1.6 to 13, approximately. Thus, our results suggest the effects of the mandates are consistent with the varying reach of the states' regulatory authority.

Figure 3 presents the difference in marriage rates between eligible and ineligible young adults, as estimated by the same model as that of column (3), but splitting the *ELIG* effect across each year, not only before and after the ACA. We have added three lines to the graph to highlight important aspects. The vertical lines separate the fully pre- and post-ACA implementation periods, with the partial-implementation year, 2010, between the dashed and solid lines. The red horizontal line marks the estimate for the year 2009, the last fully pre-ACA period. This serves as a frame of reference for the post-ACA estimates since the theoretical prediction is that the difference in marriage rates would rise after the ACA, not necessarily rise above zero. The graph shows that in the year of partial implementation, 2010, the difference rose above previous years somewhat, but after full implementation in 2011 the estimates for each year, except one, are statistically significantly greater than the 2009 level. In fact, 2011 is the first year that the difference is estimated to be above zero, and this continues each year thereafter.

The plot of the estimates also allows the opportunity to examine the results for trends that might be spuriously driving our results. Upon review, the graph shows a notable jump in the estimates between years 2005 and 2006, one which would (obviously) not be explained by the ACA, nor by any other policy of which we are aware. While this jump persists across our various

specifications, three points suggest to us that this is not a critical concern for our estimates. First, if we exclude years 2005 and earlier from our analysis, we obtain estimates for the pre- and post-ACA effects that are similar to our main results. Second, all estimates of the effect of *ELIG* are negative through 2009, and positive estimates are only observed after the enactment of the ACA. While positive estimates are not required theoretically, their presence suggests the increase observed after the ACA is different than the increase in 2006 in an important way. Third, the 2006 jump persists even if we exclude the four states that we code as having expanded their mandates during 2006 (Colorado, Georgia, New Jersey, and New Mexico) from our analysis completely, implying that the jump is not driven by changes in *ELIG*. Thus, the 2006 jump appears to be unique to that particular year, though it should be kept in mind as a potential weakness in the evidence we present.

Moving on from our main estimates, we further evaluate the robustness of our results by narrowing the age range in the sample to ages 22 through 25. *Table 4* reports these estimates, which contain the same set of model specifications as those of *Table 3*. The main advantage of this additional test is the increased similarity in ages between the eligible and ineligible groups, which strengthens our controls for differential age trends. Obviously, our sample sizes are much smaller than what is reported in *Table 3*, so we lose some precision in our estimates, but the key patterns we observed before are still apparent. Specifically, the interaction term coefficient estimates are all positive and the coefficients on *ELIG* are all negative, and the magnitudes of the estimates are similar to those reported in *Table 3*. Moreover, the estimates reported in columns (5) though (8) are much larger than those reported in columns (1) through (4), which is also consistent with *Table 3*.

In *Table 5* and *Table 6*, we examine heterogeneous effects by estimating separate models by gender and student status. *Table 5* presents results for samples limited to each gender, where effects are quite similar for both men and women, indicating that our results are not driven by one specific gender. Moreover, results for both are comparable to the main results. *Table 6* reports estimates by student status, which are again similar in overall pattern to the main results. It is notable, however, that the magnitudes of the estimates for students are larger than those for non-students in most cases, particularly in the models with state-by-year interaction dummies. This result is consistent with the theoretical mechanism suggested in Section 3. Since students (usually) do not have the types of jobs through which they could access group health insurance, the availability of coverage through parents is likely to have a stronger effect on incentives and, thereby, behavior.

Effect on Out-of-Wedlock Births

We now turn to the effect of dependent coverage mandates on the probability of having a child out-of-wedlock, an important marriage related outcome for young adults. Although mandates that raise access to health insurance also raise access to family planning resources, for the cases where a pregnancy occurs anyway, the state mandate marriage restrictions represent an impediment to couples responding by marrying. We would expect, then, that out-of-wedlock births would be more likely among eligible women during the pre-ACA period. After the preemption of the marriage restrictions by the ACA, however, we would expect to see this pattern reverse. *Table 7* reports estimates for the effects of the mandates on out-of-wedlock births from our specifications that include state-by-year interaction dummies. Here we use only the women in our sample, and define the binary outcome variable as equal to one if a young woman is unmarried and has a child less one year old, and zero otherwise. We condition on the

children being under a year since there is no direct question about women's marital statuses when their children were born. By including the age requirement for the children, it is more likely that the mothers' marital statuses from the time of the interview would be the same as they were when the children were born.

Our preferred specification indicates that overall the state dependent coverage mandates increased the probability of a child out of wedlock by about 0.6 percentage points, but this was completely reversed after the ACA, when the difference in such births increased by 0.8 percentage points (both p-values < 0.01). These represent sizable effects of roughly 17 percent and 22 percent, respectively, given that the overall sample average for the out-of-wedlock birth variable is only 3.6 percent. We also present estimates by student status, found in columns (3) through (6). We argued previously that the behavioral effect of the mandates is likely to be stronger for students since they have fewer outside options for health insurance, and our post-ACA estimates are consistent with this, being larger than our estimates for non-students. The pre-ACA effects we estimate, however, are smaller. One possible explanation for this difference is that unmarried non-students are more likely to respond to a pregnancy by marrying. Recent demographic research supports this, as Gibson-Davis, Ananat, and Gassman-Pines (2016) found that births after midpregnancy-marriages (colloquially, "shotgun marriages") are much more common among mothers with lower education levels. Since lower-education mothers would be part of the non-student population, this might be a counteracting effect pushing the student and non-student estimates closer together.

7. Conclusions

In this study, we examined one of the unintended consequences of state and federal dependent coverage mandates that were aimed to increase the health insurance coverage of

young adults. We found that the state-based mandates reduced marriage rates for eligible young adults, resulting in about 13 percent fewer marriages than for ineligible ones. The ACA, however, reversed this trend as the difference between the groups increased by about 19 percent of our sample's overall average marriage rate. This modification of marital behavior also resulted in corresponding changes to the frequency of out-of-wedlock births, which increased for eligible individuals before the ACA, but then fell afterwards. Our study provides an important example of an interaction effect between the state and federal based mandates, underscoring the importance of studying these policies jointly. It also shows that the ACA mandate did not uniformly discourage marriage, as has been previously argued (Abramowitz 2016). It should be noted, though, that while our estimates have magnitudes that are sizable, they could still be thought of as representing lower bounds to the true effects. We impute eligibility for state-based mandates within a state and year using only age, ignoring other eligibility criteria to avoid introducing upward bias. The cost of this is the possibility that our estimates are diluted due to misclassification. True effects, therefore, are very likely larger than we estimate herein.

Considering these results as a whole, we have three concluding thoughts. First, in our view, our results highlight the importance of health insurance access. Being young and therefore relatively healthy, the population we study likely values health insurance much less than older population groups. Nevertheless, we are still able to demonstrate that young adults are willing to adjust their marriage behavior to gain access to high quality health insurance. Even if this adjustment is easier for this age group than older groups, it is still a testament to the importance of health insurance that such an important life decision can be influenced. Second, we believe these results should serve as a lesson for policy makers that marriage restrictions should not be included in policy eligibility unless the implications are fully considered and the potential costs

carefully weighed. By tying eligibility to marriage, which may seem innocuous, state lawmakers burdened young adults by making marriage decisions more complicated than they needed to be. The ACA, however, benefited young adults significantly by removing the link between the marriage decision and health insurance, allowing marital choices to be made more freely. Given the importance of marriage as a social institution, we view this as a significant positive of the ACA. Finally, for economists and other researchers seeking to study marriage and related outcomes, our research constitutes an argument that the state and federal dependent coverage mandates provide a source of exogenous variation in the decision to marry. Moreover, given the interaction effect we document between the state laws and the ACA, the combination of both types of mandates contains a significant amount of variation that could potentially be exploited. We investigated only one of the possible related outcomes, out-of-wedlock births, but believe these laws could serve as a base for study of additional outcomes that merit further attention from researchers.

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Figure 1: Stylized Example of the Interaction of Dependent Coverage Mandates

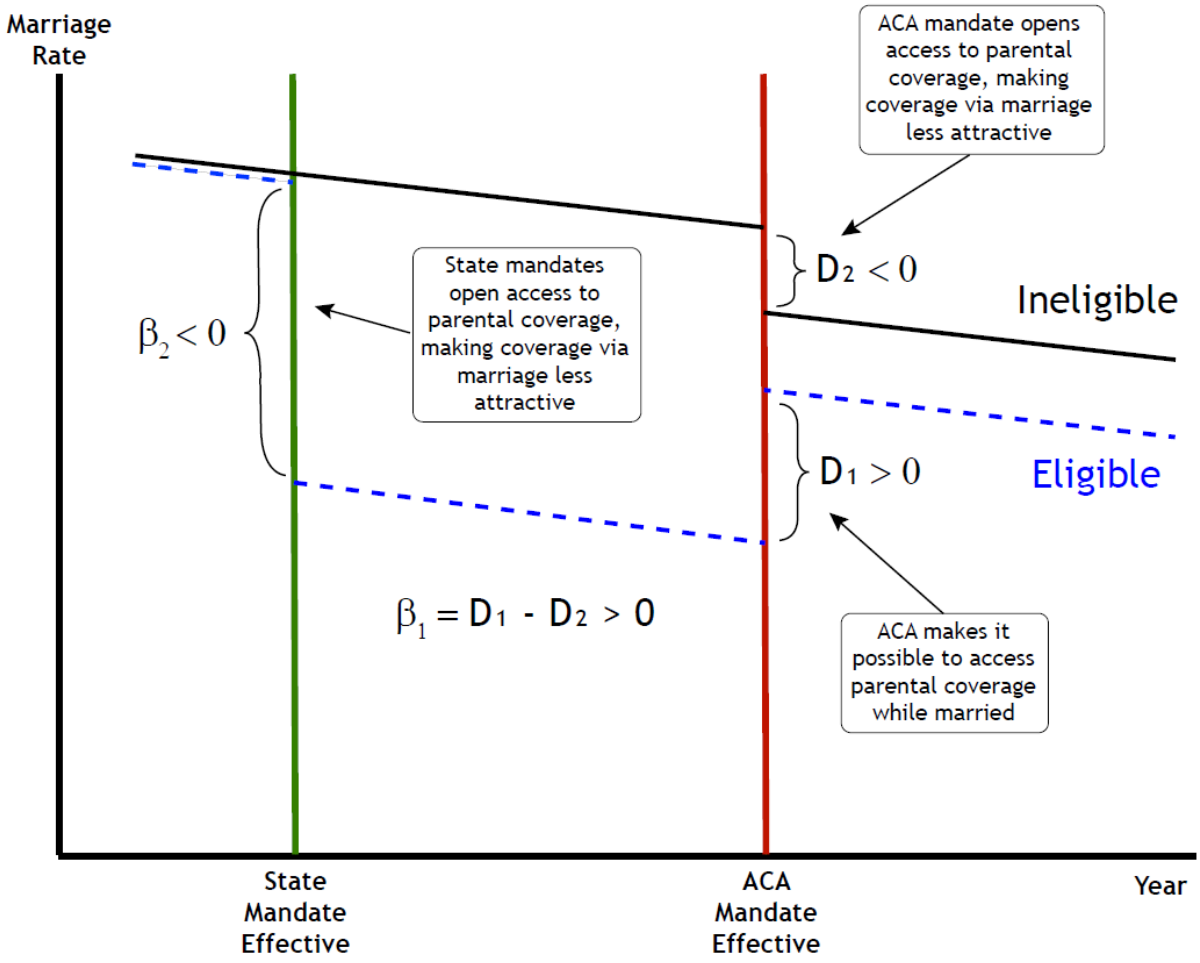
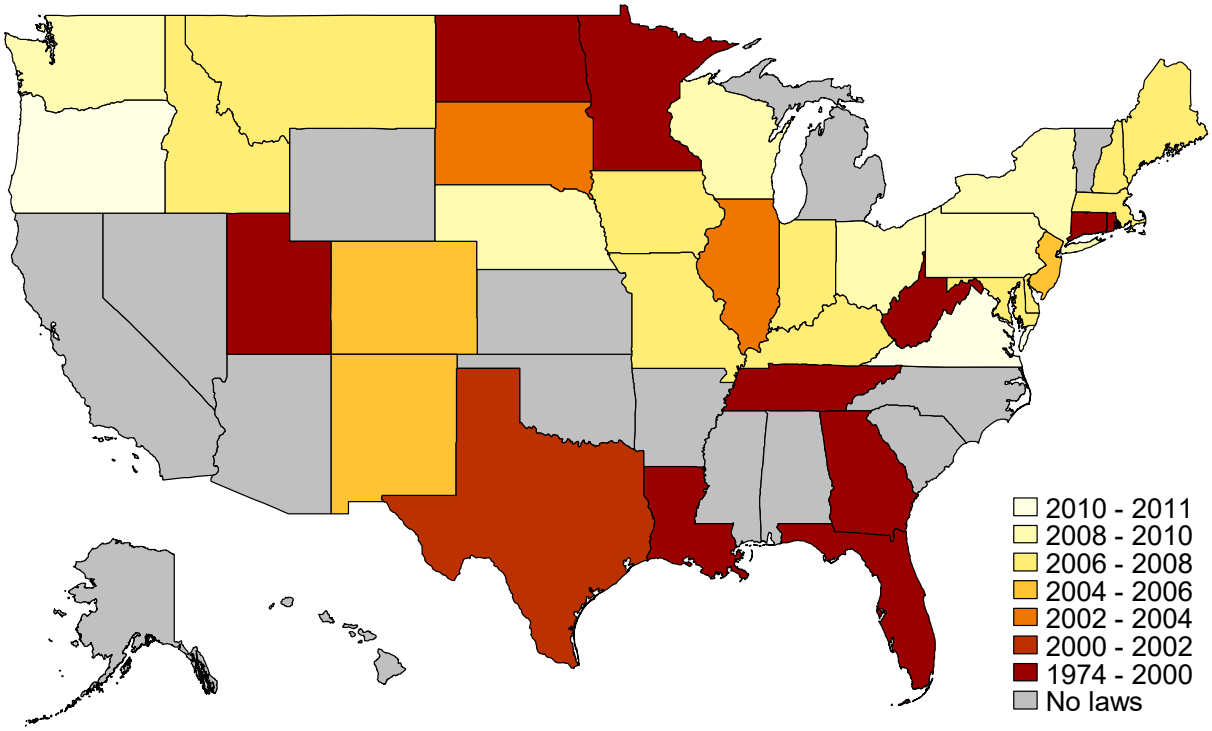
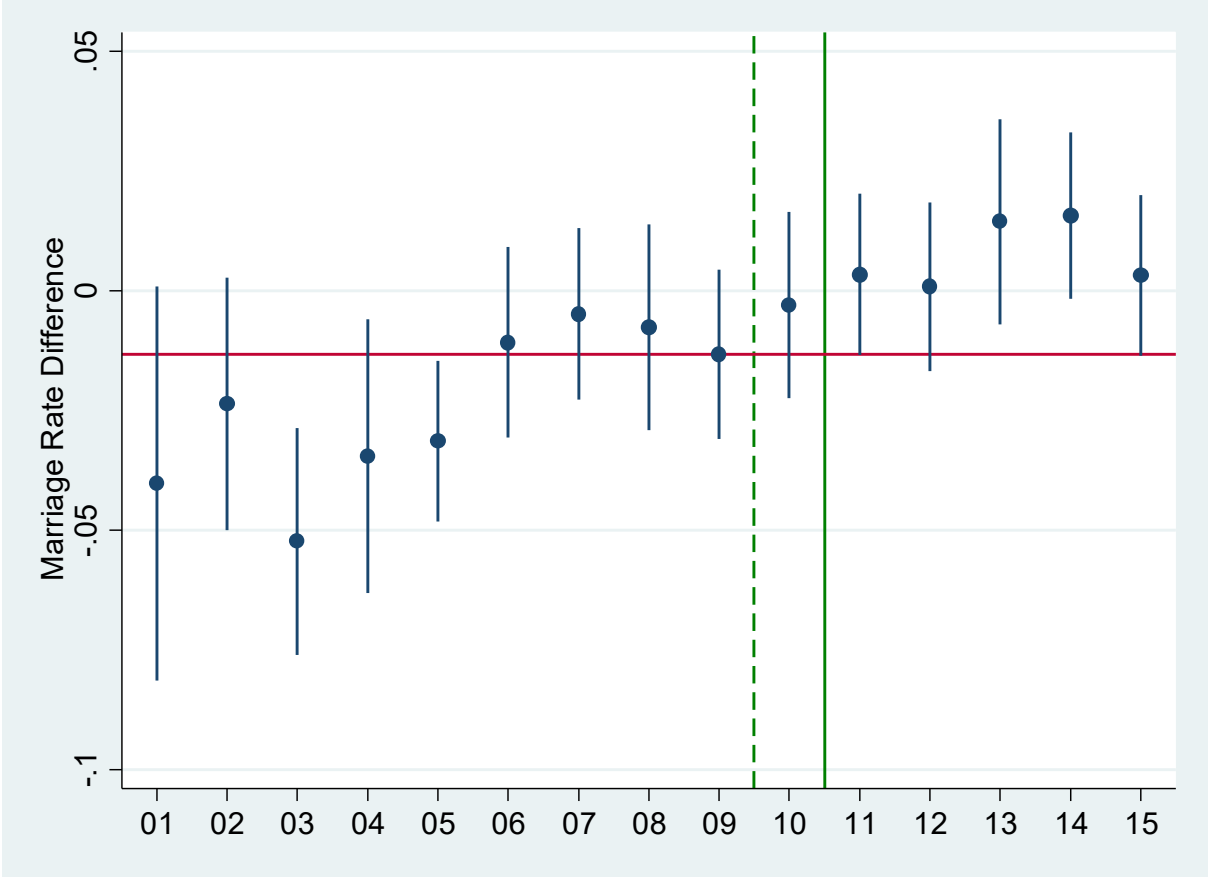


Figure 2: Year of Introduction of State Dependent Coverage Mandates



Note: No state mandates were introduced for the first time after 2011.

Figure 3: Change in Estimated Marriage Rate Difference Over Time



Notes: The red horizontal line marks the estimate in year 2009. Periods fully after ACA implementation are to the right of the solid, green vertical line. Periods fully before ACA implementation are to the left of the dashed, green vertical line. Blue bars around the point estimates represent 95 percent confidence intervals.

Table 2: Descriptive Statistics

	Full Sample (n = 2,994,479)		Eligible Group (n = 1,357,222)		Ineligible Group (n = 1,637,257)	
	Mean	SD	Mean	SD	Mean	SD
Dependent variables:						
Married	0.153	0.360	0.121	0.326	0.174	0.379
Has child out of wedlock (females only)	0.0363	0.187	0.0362	0.187	0.0364	0.187
Independent variables:						
Eligible	0.403	0.490	1	0	0	0
Age	22.003	1.994	21.655	1.854	22.238	2.0498
Female	0.493	0.500	0.494	0.500	0.493	0.500
White	0.706	0.456	0.718	0.450	0.698	0.459
Black	0.140	0.347	0.155	0.362	0.129	0.336
Asian	0.0507	0.219	0.0403	0.197	0.0577	0.233
Native American	0.00883	0.0935	0.00649	0.0803	0.0105	0.102
Other race	0.0950	0.293	0.0805	0.272	0.105	0.306
Hispanic	0.195	0.396	0.189	0.392	0.200	0.400
Male to female ratio	0.499	0.0299	0.502	0.0269	0.498	0.0317
Age-year-state unemployment rate	0.132	0.0488	0.138	0.0471	0.128	0.0495
Other characteristics:						
High school or less	0.501	0.500	0.494	0.500	0.505	0.500
Some college	0.361	0.480	0.378	0.485	0.349	0.477
College	0.125	0.331	0.116	0.321	0.131	0.338
More than college	0.0133	0.114	0.0113	0.106	0.0146	0.120
Currently student	0.404	0.491	0.439	0.496	0.381	0.486

Note: Sample restricted to young adults ages 19 through 25 who were not disabled. ACS person weights used in calculations.

Table 3: Estimates of State and Federal Dependent Coverage Mandate Effects on Marriage, Full Sample

Dependent variable: married	n = 2,994,479				n = 2,750,452			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Eligible under state mandates × ACA	0.00696*	0.0105**	0.0278***	0.0465***	…	…	…	…
	(0.00364)	(0.00434)	(0.00612)	(0.00564)				
Eligible under state mandates	-0.00792**	-0.00990**	-0.0203***	-0.00985	…	…	…	…
	(0.00369)	(0.00416)	(0.00761)	(0.0111)				
Eligible under state mandates × ACA × share of fully-insured plans	…	…	…	…	0.0164*	0.0222**	0.0706***	0.126***
					(0.00871)	(0.0107)	(0.0162)	(0.0121)
Eligible under state mandates × share of fully-insured plans	…	…	…	…	-0.0177**	-0.0214**	-0.0512***	-0.0344
					(0.00805)	(0.00873)	(0.0177)	(0.0238)
Linear state trend	Yes	Yes	No	No	Yes	Yes	No	No
Quadratic state trend	No	Yes	No	No	No	Yes	No	No
State-year FE	No	No	Yes	Yes	No	No	Yes	Yes
Age-state FE	No	No	No	Yes	No	No	No	Yes

Note: Standard errors reported in parentheses are clustered at the state level. All estimates produced using person weights. All model specifications also include dummy variables for state, year, age, race, gender, and ethnicity (see text for detailed list), plus the full interaction between age and gender, as well as sex ratio and unemployment rate, both measured at the age-state-year level. Sample includes young adults ages 19 through 25 who are not disabled. Statistically significant estimates for two-tailed tests at the one, five, and ten-percent levels are indicated by ***, **, and *, respectively. We explain in detail why including age-year fixed effects is inappropriate in the text. The sample size is smaller in columns (5) through (8) because the share of fully-insured plans is not available for some state-year combinations.

Table 4: Estimates of State and Federal Dependent Coverage Mandate Effects on Marriage, Ages 22 to 25 Only

Dependent variable: married	n = 1,650,581				n = 1,516,062			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Eligible under state mandates × ACA	0.00377	0.00522	0.0139***	0.0210***	…	…	…	…
	(0.00339)	(0.00385)	(0.00432)	(0.00446)				
Eligible under state mandates	-0. 00484*	-0.00592*	-0.0104**	-0.00513	…	…	…	…
	(0.00258)	(0.00303)	(0.00471)	(0.00911)				
Eligible under state mandates × ACA × share of fully-insured plans	…	…	…	…	0.00859	0.00995	0.0346***	0.0581***
					(0.00799)	(0.00948)	(0.0117)	(0.0107)
Eligible under state mandates × share of fully-insured plans	…	…	…	…	-0.00920	-0.0112	-0.0267**	-0.00850
					(0.00625)	(0.00695)	(0.0109)	(0.0196)
Linear state trend	Yes	Yes	No	No	Yes	Yes	No	No
Quadratic state trend	No	Yes	No	No	No	Yes	No	No
State-year FE	No	No	Yes	Yes	No	No	Yes	Yes
Age-state FE	No	No	No	Yes	No	No	No	Yes

Note: Notes to *Table 3* apply.

Table 5: Estimates of State and Federal Dependent Coverage Mandate Effects on Marriage, by Gender

Dependent variable: married	Women n = 1,489,792				Men n = 1,504,687			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Eligible under state mandates × ACA	0.00800*	0.0114**	0.0256***	0.0428***	0.00614	0.00947**	0.0298***	0.0498***
	(0.00446)	(0.00498)	(0.00666)	(0.00521)	(0.00383)	(0.00447)	(0.00747)	(0.00784)
Eligible under state mandates	-0.00988***	-0.0118***	-0.0200***	-0.0161*	-0.00611	-0.00816*	-0.0207**	-0.00399
	(0.00369)	(0.00422)	(0.00738)	(0.00894)	(0.00423)	(0.00466)	(0.00858)	(0.0140)
Linear state trend	Yes	Yes	No	No	Yes	Yes	No	No
Quadratic state trend	No	Yes	No	No	No	Yes	No	No
State-year FE	No	No	Yes	Yes	No	No	Yes	Yes
Age-state FE	No	No	No	Yes	No	No	No	Yes

Note: Notes to *Table 3* apply.

Table 6: Estimates of State and Federal Dependent Coverage Mandate Effects on Marriage, by Student Status

Dependent variable: married	Non-student n = 1,680,798				Student n = 1,313,681			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Eligible under state mandates × ACA	0.00446	0.0101**	0.0206***	0.0334***	0.00488	0.00584	0.0270**	0.0517***
	(0.00369)	(0.00382)	(0.00483)	(0.00584)	(0.00381)	(0.00495)	(0.00989)	(0.00560)
Eligible under state mandates	-0.00606	-0.00864**	-0.0142**	-0.00343	-0.00628	-0.00695	-0.0186*	-0.0163*
	(0.00366)	(0.00425)	(0.00652)	(0.0116)	(0.00381)	(0.00418)	(0.00975)	(0.00876)
Linear state trend	Yes	Yes	No	No	Yes	Yes	No	No
Quadratic state trend	No	Yes	No	No	No	Yes	No	No
State-year FE	No	No	Yes	Yes	No	No	Yes	Yes
Age-state FE	No	No	No	Yes	No	No	No	Yes

Note: Notes to *Table 3* apply.

Table 7: Estimates of State and Federal Dependent Coverage Mandate Effects on the Probability of Having a Child Out of Wedlock

Dependent variable: has a child out of wedlock	Full sample n = 1,489,792		Non-student n = 784,339		Student n = 705,453	
	(1)	(2)	(3)	(4)	(5)	(6)
Eligible under state mandates × ACA	-0.00797***	-0.0110***	-0.00729**	-0.00919**	-0.00814**	-0.0115***
	(0.00266)	(0.00242)	(0.00342)	(0.00315)	(0.00367)	(0.00321)
Eligible under state mandates	0.00599***	0.00232	0.00716**	0.00335	0.00574**	0.00312
	(0.00196)	(0.00244)	(0.00282)	(0.00276)	(0.00253)	(0.00292)
State-year FE	Yes	Yes	Yes	Yes	Yes	Yes
Age-state FE	No	Yes	No	Yes	No	Yes

Notes: Notes to *Table 3* apply. The sample only includes women. Having a child out of wedlock is defined as unmarried with a child under age 1.