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ABSTRACT

Medical divorce occurs when couples split up so that one spouse's medical bills do not deplete the assets of the healthy spouse. It has not been studied in the economics literature, but it has been discussed by attorneys and widely reported in the media. We develop a model of medical divorce that demonstrates that divorce is optimal when a couple's joint assets exceed the exempted asset level. We use the Affordable Care Act's Medicaid expansion which removed asset tests to qualify for Medicaid as exogenous variation in the incidence of divorce (as it was only implemented by some states). We find that the ACA expansion decreased the prevalence of divorce by 11.6% among those ages 50–64 with a college degree. These results are robust to numerous placebo checks including older subsamples (who qualify for Medicare regardless of assets) and earlier years (before the expansion was implemented). Our results suggest that Medicaid expansion reduced medical divorce.

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Introduction

Medical divorce has been discussed as a strategy to preserve the assets of a couple when one spouse gets sick and his or her care results in high medical bills that could potentially bankrupt the couple. During the debate over the Affordable Care Act (ACA), the *New York Times'* Nicholas Kristof opened a column with the headline "Until Medical Bills Do Us Part" (2009). He told the story of a friend whose husband was diagnosed with early-onset dementia. She faced the prospect of his care draining their entire retirement savings, following which she would wind up a destitute but healthy widow with a long life expectancy. Given this, she considered legally divorcing her husband to shield her assets. He would eventually draw down his assets and be poor enough to qualify for Medicaid, and she would be able to provide for herself in retirement.[1] After the passage of the ACA, states that expanded Medicaid eliminated the asset test for individuals facing a costly, long-term illness such as cancer or dementia.[2] Thus, couples living in Medicaid expansion states would not face the dilemma of medical divorce. We examine whether divorce prevalence fell in states that expanded Medicaid and eliminated asset tests, and find significant decreases in divorce among couples ages 50–64, with a college education.

Medical divorce has not been studied in the economics literature, but it has been discussed by attorneys and widely reported in the media. Medical divorce can result from many medical situations. Any medical condition that requires extensive treatment and costly care could make divorce rational. For example, Goldman (2008) relates a story of a woman in Indiana who received a cancer diagnosis and divorced her husband in order to qualify for Medicaid and receive cancer treatment.[3] In addition, a traumatic brain injury where the injured spouse required nursing care could put a couple into the situation of contemplating medical divorce. In these cases the couple's joint assets are too much for the unhealthy spouse to qualify for Medicaid, and in order to receive medical care for that spouse the couple decides to divorce.

The 2010 ACA's Medicaid expansion ostensibly fixed the underlying problem, as it expanded Medicaid to cover all adults under 65 with incomes up to 138% of the poverty line, regardless of assets (Sung, Skopec, and Waidmann 2015).[4] Previously, many states had stringent asset requirements. For

[1] Others have written about this as well. See Kaplan (2014) and Olver and Lee (2010).

[2] This asset test was not necessarily eliminated for those seeking long-term nursing home care, and would not apply to elderly people. Any remaining limits were reasonably uniform across states (e.g., see Watts, Cornachione and Musumecisee 2016, Appendix Table 2) and so should not affect our results.

[3] See: Goldman (2008). Other examples of cancer and medical divorce are related in St. Pierre (2011), Paquette (2014) and Egan (2018).

[4] The Affordable Care Act uses Modified Adjusted Gross Income (MAGI) to determine eligibility for Medicaid. MAGI is Adjusted Gross Income (AGI) plus untaxed foreign income, non-taxable Social Security benefits, and tax-exempt interest. According to healthcare.gov, for many people, MAGI is the same as AGI.

example, in Missouri, a permanently disabled individual was required to have total non-exempt resources[5] under $2000 to qualify (Missouri Department of Social Services 2020). Nine states are community property states where assets and liabilities are split evenly between the spouses. In states that expanded Medicaid, as long as the sick spouse had a low income, the healthy spouse could keep his or her retirement assets intact. However, this fix would take effect only if the couple's state implemented the ACA Medicaid expansion, which the Supreme Court made optional in National Federation of Independent Business v. Sebelius. Those in non-expansion states still had an incentive to divorce for medical reasons. One couple in Tennessee (a non-expansion state) even used their situation to lobby their governor to expand Medicaid (Wilemon 2014). Despite these anecdotes, we know of no estimates about the prevalence of medical divorce, as many couples who use this option refrain from publicly communicating their motives.

This paper will use the partial Medicaid expansion as plausibly exogenous variation in access to public health insurance that does not require asset drawdown. It will then compare the changes in prevalence of divorce in states that expanded to Medicaid to states that did not expand, before and after implementation. We find that states that expanded Medicaid had an 11.6% reduction in divorce among college-educated individuals aged 50–64. Our estimates also suggest that medical divorce was prevalent among older, high-income couples. The remainder of the paper discusses the literature, data and methods, and results.

Medicaid Expansion, Health Insurance and Divorce

Our paper contributes to a large literature on the relationship between the welfare system as a whole and the Medicaid program specifically and marriage and divorce. One of the early papers in this literature found that from 1969 to 1985 the cross-sectional correlations between marital status and welfare benefits were weak in statistical significance but increasing in magnitude over time (Moffit 1990). More specially relevant to this paper, extending Medicaid eligibility beyond single-parent families in the 1980s and 1990s increased the probability of marriage (Yelowitz 1998).

The partial expansion of Medicaid in states in the 2010s under the ACA provides a plausible source of exogenous variation, and many recent papers have examined its impact on health and economic outcomes. Medicaid expansion increased health care consumption and diagnosis of chronic conditions (Wherry and Miller 2016) and cancer (Soni, Simon, Cawley, and Sabik 2018); reduced disruptions in health insurance coverage (Goldman and Sommers 2020), reduced federal disability program participation (Chatterji and Li 2016; Anand, Hyde, Colby, and O'Leary 2019), and ultimately reduced mortality rates (Miller et al. 2019). Others, however, have found minimal effects on labor force participation, employment status, or hours worked (Leung and Mas 2018; Gooptu, Moriya, Simon, and Sommers 2016; Kaestner,

[5] Most states do allow the healthy spouse to keep some assets as "exempt resources." Craig Reaves (2010), past president of the National Academy of Elder Law Attorneys, concurring with other experts, opines that "the numbers are just too low for many couples to feel good about this option."

Garrett, Gangopadhyaya, and Fleming 2017; Buchmueller, Levy, and Valletta 2019). There is some evidence that those with lower educational attainment are working less in states that expanded Medicaid (Moriya et al. 2016).

The ACA's Medicaid expansion also improved household's acute financial situation. It reduced health spending (Levy, Buchmueller, and Nikpay 2017) and also improved credit scores and reduced unpaid bills, medical bills, over limit credit card spending, delinquencies, collection balances, and public records inquiries (Brevoort, Grodzicki, and Hackmann 2017; Miller, Hu, Kaestner, Mazumder, and Wong 2018; Hu, Kaestner, Mazumder, Miller, and Wong 2018).

There is one paper similar to ours which studies the impact of the ACA's partial Medicaid expansion on marriage and divorce rates (Hampton and Lenhart 2019b). That paper, however, focuses on those with low educational attainment, unlike our paper which focuses on those at the other end of the educational attainment spectrum.

Others have examined the impact of divorce on health insurance more broadly. Lavelle and Smock (2012) found approximately 115,000 women annually lose private health insurance after divorce, and more than half of them become uninsured. Couples with spousal health insurance but no other options therefore often stay married to avoid being uninsured (Sohn 2015; Chen 2019a). In work closely related to ours, Chen (2019a) found that Medicare eligibility (at age 65) and the 2006 Massachusetts healthcare reform increases (2019b) both divorce rates. No literature has examined the impact of Medicaid coverage expansions on medical divorce specifically.

Other sections of the ACA affected marriage and divorce rates. The parental mandate that allowed young adults to stay on their parents' health insurance reduces marriage prevalence and increases the likelihood of being single (Chatterji, Liu, Yörük 2019), decreased childbearing and marriage rates (Heim, Lurie, and Simon 2017), and increased the prevalence of divorce (Abramowitz 2016). The ban on health insurers discriminating on pre-existing conditions also lowered marriage prevalence (Hampton and Lenhart 2019a).

Demographers have documented an increase in "gray divorce," divorce rates among older couples. This research shows that divorce rates among people 55–64 have more than doubled since 1990, increasing from 5.1 to 12.0 per 1000 in 2017 (Brown and Lin 2012; Allred 2019). However, much of this increase was driven by remarriages ending in divorce (Brown and Lin 2012). In subsequent work, Lin, Brown, Wright and Hammersmith (2018; 2019) found that marriage quality and economic disadvantage partially explained the increase in gray divorce, as did increasing acceptance of both cohabitation (Brown and Wright 2017) and divorce by older adults (Brown and Wright 2019). They also found that chronic health conditions were not associated with gray divorce.

This research also contributes to the broader ongoing literature on the household financial consequences of the American health care system. This literature has historically focused on medical debt and extreme outcomes like bankruptcy (Himmelstein, Warren, Thorne, and Woolhandler 2005; Dranove and Millenson 2006; Himmelstein, Warren, Thorne, and Woolhandler 2006; Gross and Notowidigdo 2011; Mahoney 2015; Dobkin, Finkelstein, Kluender, and Notowidigdo 2018; Himmelstein, Woolhandler, and Warren 2018; Caswell and Goddeeris 2020), and so we contribute by turning the focus towards medical divorce as a potential approach to limiting spousal impoverishment.

This research is also related to the literature on the asset allocation decisions of elderly people to qualify for Medicaid long-term care. Greenhalgh-Stanley (2012) found that state adoption of asset recovery for Medicaid patients decreased homeownership and housing assets as a percentage of total wealth. In older work, Norton (1995) finds that elderly people receive transfers from family members in order to avoid Medicaid eligibility. Norton and Kumar (2000) found that the Medicare Catastrophic Coverage Act of 1988 did not prevent spousal impoverishment when one spouse entered a nursing home. This suggests that there are incentives for older married couples to divorce when one spouse gets sick.

Model of Medical Divorce

We develop a simple model to motivate our analysis of medical divorce. Older couples who have been married for a long period of time have likely accumulated marriage-specific capital. In the Becker, Landes, and Michael (BLM) (1977) model of divorce, children are considered “marriage-specific capital,” and couples who have lived together longer and have marriage-specific capital are less likely to divorce over time. Given income pooling in the household, we can consider assets such as a house and retirement savings as marriage-specific capital. Income pooling is the appropriate choice here since this is how the government treats the assets of married spouses when it comes to the asset test to qualify for Medicaid.[6] Here we modify the BLM model of the decision to divorce to account for the choices facing older couples where one experiences a health shock requiring expensive care in states that expanded Medicaid and those that did not.

Following Weiss and Willis (1997) and Charles and Stephens (2004) the decision to divorce occurs when the value of being married is less than that of obtaining a divorce. We are assuming that couples pool income and assets within the household. Let the value of marriage, U_c^M, for a couple *c* be a function of their jointly held assets, V^M. U_c^M can be considered the gains from being married as in Weiss and Willis (1997) and Charles and Stephens (2004). For convenience we assume that all income is derived from these assets. Given that these couples are older, we ignore marriage-specific capital such as children shared by spouses.

[6] Bargaining models such as Manser and Brown (1980) and McElroy and Horney (1981) offer an alternative to the income pooling model of the household.

Analogously, let the value of being single, U_i^D, be a function of the division of the jointly held assets, where spouse *i* receives share π ($0 \leq \pi < 1$) of the assets, and spouse *j* receives share $(1-\pi)$. Here, following Becker (1973; 1974), we are assuming that these values are linear in assets, meaning that the goal of an individual is to maximize the assets available to them, all else being equal.

$$U_c^M = V^M$$

$$U_i^D = \pi V^M$$

$$U_j^D = (1-\pi) V^M$$

This condition assumes that each spouse derives the same value from marital assets V^M.

These couples can reside in states that have asset tests for receiving Medicaid (non-expansion states) or states that expanded Medicaid that no longer require asset tests. In addition, some states have community property regulations where the value of jointly held assets is split equally upon divorce ($\pi = 0.5$).[7] In the majority of states, π is determined by an adjudicated settlement known as equitable distribution of property (which does not necessarily mean that the distribution has to be equal). For our purposes, all divorces are the result of mutual consent. Couples choose between remaining married and divorcing to maximize utility.

As long as the value of marriage is greater than or equal to the sum of outside opportunities for the spouses, the couple will remain married. This can be restated as:

$$U_c^M \geq U_i^D + U_j^D$$

$$V^M \geq \pi V^M + (1-\pi) V^M$$

Where the left-hand side is the value of assets of remaining married and the right-hand side is the value of assets split between the couple. Divorce happens whenever the sum of the husband's and wife's outside opportunities is greater than the value of being married.

Suppose that one spouse gets sick (which reduces resources for the couple by *S*) and requires expensive care. In the absence of asset tests, the cost of that care, valued at *C*, is provided by Medicaid. This added to the value of marital assets gives:

$$U_c^{M\prime} = V^M - S + C$$

Since $V^M - S + C \geq \pi V^M + (1-\pi) V^M - S + C$ assets remain mostly intact, the sick spouse receives the care they need, and the couple remains married.

[7] There are nine community property states where assets and medical liabilities are split 50/50 between spouses: Arizona, California, Idaho, Louisiana, Nevada, New Mexico, Texas, Washington and Wisconsin. As shown in Table 3, our results are robust to excluding them.

In the states that did not expand Medicaid, the couple is allowed an exemption, E and the remaining assets are spent down so that sick spouse can qualify for Medicaid.[8] The value of marital assets is therefore the maximum amount that can be retained (i.e., the exemption). In this case, the value of remaining married is given by $U_c^{M'} = E^M - S + C$. The value from being divorced now becomes $U_j^{D'} = \pi V^M$ for the healthy spouse (who is not going on Medicaid and therefore is not subject to any attest test or exemption) and $U_i^{D'} = -S + C + \phi E^M$ for the sick spouse, where $.5 \leq \phi \leq 1$. This last assumption assumes that exemption for a single is some fraction, bounded below by one-half and above by one of that for a couple.[9] The sick spouse is required to spend down their post-divorce assets of (1-π)V^M.

The couple would choose to divorce when the value from marriage is less than the sum of the value of outside opportunities.

$$U_c^{M'} \leq U_j^{D'} + U_i^{D'}$$

Substituting in for the value of the assets (and cancelling out S), when these marital assets are allocated to long-term care:

$$E^M - S + C \leq \pi V^M - S + C + \phi E^M$$

$$(1 - \phi)E^M \leq \pi V^M$$

Assume that the couple lives in a community property state that did not expand Medicaid such as Wisconsin. In that case $\pi = .5$ and $(1 - \phi) = .67$, and as long as 1.34 E^M is less than the value of total marital assets V^M, the couple has a financial incentive to divorce. Thus, households with a high value of assets, V^M, will most often have a financial incentive to divorce in the case of an illness that requires long-term medical care. These "medical" divorces are benevolent in the sense that assets are preserved for the healthy spouse in order to qualify the sick spouse for Medicaid.

The maximum Medicaid spousal asset exemption a state could set in 2017 was $120,900, and the local exemption applies to spouses in all states that did not expand Medicaid (Medicaid 2017). So a couple in this situation would seek a medical divorce if their retirement assets were greater than this amount. According to analysis based on the Survey of Consumer Finance, 31% of households aged 55–64 have retirement assets over $100,000 and 20% have retirement assets over $200,000 (GAO 2015).[10] Therefore, it is plausible that a substantial number of individuals would have sufficient assets to consider medical divorce. From the model, we therefore hypothesize that Medicaid expansion will lower divorce prevalence for the near-elderly with sufficient assets compared to states that did not expand Medicaid.

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[8] In Kansas the asset limit for a couple is $3000 and for an individual is $2000.

[9] In Kansas the asset limit of an individual is $2000, thus $\phi = .67$.

[10] According to estimates from the Current Population survey, there were 23.9 million households aged 55–64 in 2015.

Data and Methods

Our research will examine whether divorce incidence differed among older couples in states that did and did not expand Medicaid under the ACA. Information on Medicaid expansion status comes from Kaestner, Garrett, Gangopadhyaya, and Fleming (2017) and the Kaiser Family Foundation (2020). Given the anticipatory nature of medical divorce, we drop a handful of states from our sample: those that had a prior full expansion of Medicaid and those that expanded Medicaid but after the original launch in January 2014.[11] This leaves us with 38 states:[12]

- Traditional expansion states without a full prior expansion: Arizona, Arkansas, California, Colorado, Connecticut, Hawaii, Illinois, Iowa, Kentucky, Maryland, Minnesota, Nevada, New Jersey, New Mexico, North Dakota, Ohio, Oregon, Rhode Island, Washington, West Virginia

- Non-expansion states: Alabama, Florida, Georgia, Idaho, Kansas, Mississippi, Missouri, Nebraska, North Carolina, Oklahoma, South Carolina, South Dakota, Tennessee, Texas, Utah, Virginia, Wisconsin, Wyoming[13]

This paper's primary data is the Current Population Survey's (CPS) Merged Outgoing Rotation Group (MORG) for 2000–2016, a consistently estimated survey over the past several decades with a large sample size (National Bureau of Economic Research 2019). We use this data because the US Census Bureau (2017) uses the CPS to report on America's Families and Living Arrangements. This version of the CPS also contains income variables, which will be used below in a robustness check. We also supplement this with seasonally unadjusted monthly state unemployment rates from the Bureau of Labor Statistics' Local Area Unemployment estimates (U.S. Bureau of Labor Statistics 2020).

We limit the sample to those with a four-year college degree or equivalent (hereafter referred to as a college degree) and age 50–64. This is because these individuals are more likely have been married for a number of years, to have substantial assets, to be at greater risk of a degenerative medical condition, and to be eligible for the Medicaid expansion. They are also less likely to be single mothers, which avoids confounding our results with other modes of Medicaid eligibility.

The empirical method is a straightforward state-month-year difference-in-differences estimation:

$$y_{ismy} = \alpha + \gamma Treated_s + \delta Implemented_{my} + \sigma\left(Treated_s * Implemented_{my}\right) + \mathbf{state}_s + \mathbf{yearmonth}_{my} + \beta UR_{smy} + \mathbf{X}_{ismy} + \varepsilon_{ismy}$$

[11] Full prior expansion: Delaware, Washington DC, Massachusetts, New York, and Vermont. Late expanders: Alaska, Indiana, Louisiana, Michigan, Montana, New Hampshire, Pennsylvania, and Maine. In Table 3, we add back Delaware, Washington DC, New York, and Vermont as regular expanders as a robustness check. (We continue to omit Massachusetts as its prior expansion was before our "pre" period.)

[12] In Appendix Table 6 and Appendix Table 7 we use one treatment state at a time (and all of the control states) and one control state at a time (and all of the treatment states) and find broadly consistent results. As every state's asset test policy is slightly different, this helps check that our results aren't being driven or muddied by a handful of states.

[13] See Appendix Figure 1 for a color-coded map of states by expansion status.

y is the prevalence of divorce for individual *i*, living in state *s* in month *m* and year *y*. Ideally, we would use divorce incidence by this age group for the given state, month and year. However, official vital statistics records do not exist at this level of granularity.[14] *Treated* equals 1 if a state expanded Medicaid in 2014 and 0 if it did not. *Implemented* equals 1 if the year is 2014 or after. The coefficient on *Treated*Implemented* (λ) is our primary difference-in-differences estimate.

Other specifications include additional controls, including **state** fixed effects (instead of the treated dummy), **year-month** fixed effects and the state-month-year level unemployment rate. **X** includes individual level controls, such as age and educational attainment fixed effects (within the college educated population, i.e., master's degree, professional degree, doctorate) and dummies for race and gender. Regressions are weighted using the CPS survey weights. Robust standard errors are clustered at the state level.

We drop the years 2012 and 2013 from our analysis, as it is ambiguous whether these years are treated or not. Individuals knew about the partial Medicaid expansion and whether their states would or would not expand, but the policy had not yet been implemented. While most other papers that study the ACA Medicaid expansion include these years as untreated years, we believe that our paper is fundamentally different from those because of the anticipatory nature of our outcome variable. Other outcomes (e.g., unpaid medical bills, diagnosis of chronic conditions, employment) measure the current state, whereas preemptively getting a medical divorce before draining joint financial reserves is anticipatory on a multiyear scale. This makes is difficulty to assign treatment or control status to 2012 and 2013, and so we omit them from the primary specification.[15]

Divorce Prevalence Given Medicaid Expansion

Before we address the role of Medicaid expansion, we will first examine the relationship between age and the prevalence of divorce. The BLM (1977) model predicts that those who have been married for longer periods of time are less likely to get divorced. Thus, we would expect older couples to be less likely to divorce since they likely have longer marriage durations. Using the National Longitudinal Survey of Youth (NLSY 79), Aughinbaugh, Robles, and Sun (2013) found that the probability of divorce decreases as educational attainment and age at first marriage increase. Brown and Lin (2012) used data from the American Community Survey and found that the divorce incidence for adults aged 50 or over had more

[14] We reached out to the National Center for Health Statistics (NCHS) to inquire about state-level divorce incidence for our age group of interest. Their response was that "the collection of detailed marriage & divorce data was suspended in the mid 1990s due to budget issues….there's no age or other demographic information available from NCHS….[Survey data also] can't provide counts of marriages & divorces in a given year."

[15] Despite being conservative here, Appendix Table 6 shows that our results are actually robust to including 2012–2013 as treated or control years.

than doubled between 1990 and 2010. However, they found that much of this divorce rate was driven by people in second marriages. In their study, as marriage duration increased, divorce rates declined.

We estimate the prevalence of divorced individuals using the CPS data for the years 2008–2011 (our pre-treatment period) both for those with and without a college degree, finding that divorce increases almost monotonically from age 20 to age 55, and then declines only slightly by age 64.[16] This greater prevalence corresponds to the age at which rates of chronic health conditions are higher. The challenge in interpreting this stylized fact is that many other things are changing as individuals age. One of the contributions of our study is to use the variation in expansion and non-expansion states' in health insurance availability to examine one potential reason that divorce rates are higher for those over the age of 50.

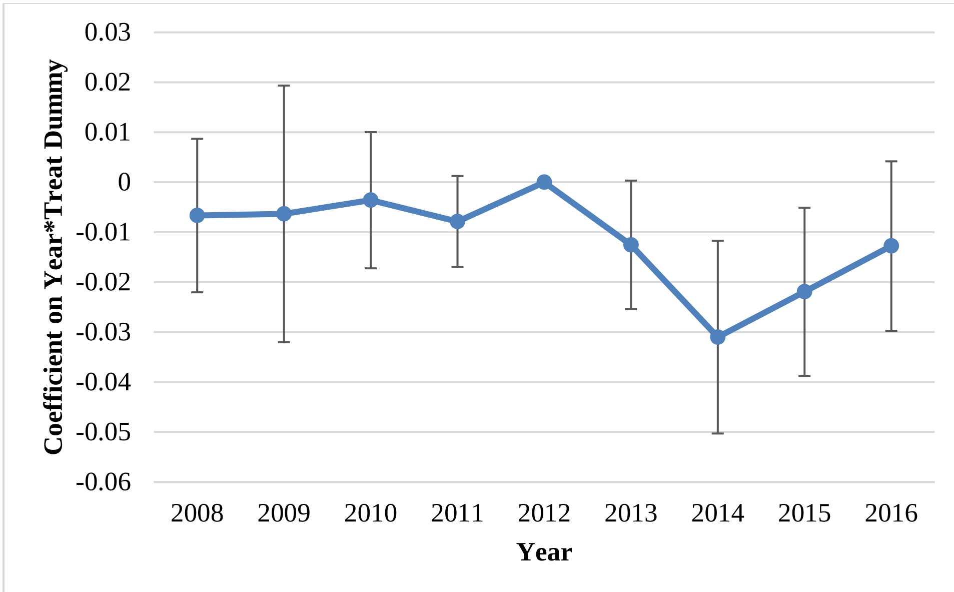
Next we construct an event study figure. The empirical method is a straightforward state-month-year difference-in-differences estimation:

$$y_{ismy} = \alpha + \sum_{t=2008}^{2011} \sigma_t \left(Treated_s * year\_t_y \right) + \sum_{t=2013}^{2016} \sigma_t \left(Treated_s * year\_t_y \right) + \mathbf{state}_s + \mathbf{yearmonth}_{my} + \beta UR_{smy} + \mathbf{X}_{ismy} + \varepsilon_{ismy}$$

As above, *y* is outcome variable of interest (e.g., a divorce dummy) for individual *i* living in state *s* and surveyed in month *m* and year *y*. The regression includes demographic controls **X** (age and educational attainment fixed effects and dummies for race and gender); the monthly state unemployment rate (*UR*); state and year-month fixed effects (**state** and **yearmonth)**; and the traditional Medicaid expansion treated state indicator (*Treated*) interacted with a dummy for each year (*year)*. The coefficients are relevant to 2012, which is the omitted category. Robust standard errors are clustered at the state level.

[16] See Appendix Figure 2.

Figure 1: Event Study



Notes: Sample is college-educated age 50–64. Dependent variable is a divorce dummy. All regressions include demographic controls (age and educational attainment fixed effects and dummies for race and gender), the monthly state unemployment rate, and state and year-month fixed effects. Coefficients are relative to 2012. The whiskers show a 95% confidence interval.

Figure 1 shows this event study for our main analysis. The beginnings of a gap in divorces in 2013 is intuitive. The Supreme Court decision was announced in the summer of 2012, and it was clear relatively quickly which states would continue to expand as planned in 2014 and which states would not.[17] Couples that were going to divorce to shield assets likely did so before they would have actually qualified for Medicaid. So once it was known whether a state would expand or not we would expect to start seeing some divergence, which we do.[18]

Table 1 shows the summary statistics and raw difference-in-difference estimates for our outcome of interest and various control values.

[17] While the point estimates decrease in magnitude in 2015 and 2016, we cannot reject the null hypothesis that each is equal to the 2014 point estimate, even at the 10% level.

[18] Given the concern of potentially different trends in the pre-period, in Appendix Table 1 we regress divorce on a treatment group specific time trend, a general time trend, and varying control specifications, per Akosa Antwi, Moriya, and Simon (2013) and Maclean and Saloner (2019). In none of the regressions do we find a statistically significant difference in these pre-period trends.

Table 1: Summary Statistics

	Control States 2008–2011	Control States 2014–2016	Treated States 2008–2011	Treated States 2014–2016	Difference in Differences Coefficient
Divorced	0.137	0.147	0.138	0.131	-0.0174*** (0.00357)
Married	0.746	0.729	0.723	0.727	0.0213*** (0.00602)
Widowed	0.0289	0.0298	0.0280	0.0274	-0.00151 (0.00237)
Never Married	0.0756	0.0804	0.0961	0.101	0.000604 (0.00419
Age	56.58 (4.221)	56.72 (4.310)	56.54 (4.228)	56.72 (4.292)	0.0378 (0.0683)
Female	0.495	0.518	0.492	0.507	-0.00804* (0.00472)
Black	0.0908	0.104	0.0540	0.0641	-0.00342 (0.00680)
Unemployment Rate	8.111 (2.227)	5.161 (0.960)	8.907 (2.334)	5.724 (1.190)	-0.233 (0.439)
College Degree Only	0.620	0.639	0.616	0.618	-0.0170** (0.00805)
Master's Degree	0.276	0.265	0.276	0.275	0.00990 (0.00763)
Professional Degree	0.0543	0.0434	0.0548	0.0497	0.00575 (0.00355)
Doctorate	0.0503	0.0532	0.0539	0.0581	0.00136 (0.00268)
N	30,114	24,745	43,888	31,663	

Notes. Sample is college-educated age 50–64. Weighted means (and standard deviations for non-dummy variables) for treated and control states, before and after Medicaid expansion. Difference-in-difference coefficients are calculated from weighted OLS, with robust standard errors are clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1.

Divorce prevalence dropped significantly in the treated states relative to the control states (p<.001). While two of the difference-in-differences coefficients on the controls are significant (one at the 5% level and one at the 10% level), they can be reasonably explained by a standard Bonferroni multiple hypothesis adjustment.

Table 2 shows our regression results across a variety of specifications, including with and without controls for age, educational attainment, race, gender, monthly state unemployment rates, and indicator variables for treated states, interacted with indicators for each year.

Table 2: Regression Results

	(1) Divorced	(2) Divorced	(3) Divorced	(4) Divorced	(5) Divorced
Treated * Implemented	-0.0174*** (0.00357)	-0.0162*** (0.00359)	-0.0160*** (0.00364)	-0.0160*** (0.00359)	-0.0270** (0.0110)
Observations	130,410	130,410	130,410	130,410	130,410
R-squared	0.000	0.012	0.015	0.015	0.016
Mean	0.138	0.138	0.138	0.138	0.138
Demographic Controls		X	X	X	X
Unemployment Rate		X	X	X	X
State Fixed Effects			X	X	X
Year Fixed Effects			X		
Month Fixed Effects			X		
Year-Month Fixed Effects				X	X
State-Specific Time Trends					X

Notes: Sample is college-educated age 50–64. Dependent variable is a divorce dummy. 2008–2011 and 2014–2016. Demographic controls include age and educational attainment fixed effects and dummies for race and gender. Robust standard errors in parenthesis are clustered at state level. Weighted. *** p<0.01, ** p<0.05, * p<0.1.

The estimate found above is extremely robust across difference specifications, including controlling for individual demographic differences; adding state, year, and month fixed effects; and also controlling for the unemployment rate. The estimate in column (4), a 1.6 percentage point increase, is statistically significant (p<.001). It represents an 11.6% decrease on the pre-period mean for expansion states of 13.8%. Column (5) adds state-specific time trends. While this results in a substantially less precise estimate, it is still statistically significant (p=.014) and is actually larger in magnitude. While it is reassuring that our results are consistent using these specifications, we prefer the cleaner results of Column (4) as our primary estimate, especially given concerns about potential biases of treatment group-specific time trends (Wolfers 2006; Lindo and Packham 2017).

Table 3 repeats our analysis with alternate specifications, subsamples, and outcome variables.

Table 3: Additional Result and Specifications

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Outcome variable	Divorced	Divorced	Divorced	Divorced	Married	Widowed	Never Married
Notes	No weights	Including Early Expanders	Above Median Income	Dropping 50/50 states			
Treated * Implemented	-0.0150***	-0.0163***	-0.0184***	-0.0150***	0.0189***	-0.000802	0.000735
	(0.00419)	(0.00343)	(0.00451)	(0.00486)	(0.00550)	(0.00201)	(0.00420)
Observations	130,410	147,678	120,594	92,643	130,410	130,410	130,410
R-squared	0.014	0.015	0.026	0.017	0.028	0.016	0.009
Mean	0.137	0.136	0.154	0.130	0.723	0.0280	0.0961

Notes: Sample is age 50–64. Dependent variable is a relationship status dummy as indicated. 2008–2011 and 2014–2016. All regressions include individual level controls, the monthly state unemployment rate, and state, year-month fixed effects. Column (2) adds back DE, DC, NY, and VT. Columns (1)-(2) and (4)-(7) are for those with at least a college degree. Column (3) is for those with weekly income of at least $797.30. Robust standard errors in parenthesis are clustered at state level. Columns (2)-(7) are weighted. *** p<0.01, ** p<0.05, * p<0.1.

Column (1) shows consistent results without using the survey weights. Column (2) adds back the dropped prior expansion states (except Massachusetts whose prior expansion was substantially earlier and more comprehensive). The estimated results become slightly larger. Column (3) looks at individuals with earnings of at least $797.30/week (the median), as we expect those with greater income to also have greater wealth and therefore may be more inclined to divorce for financial reasons. While this skews the sample and uses a more endogenous cutoff, the result is still broadly consistent and statistically significant. Column (4) omits the nine community property states where assets and medical liabilities are split 50/50, as individuals can shelter less of their assets in those states, and therefore have less incentive to divorce. Here the results are also negative and significant.

Columns (5)–(7) then look at the impact of Medicaid expansion on other marriage-related outcomes. As expected, we see an increase in marriage prevalence that is analogous in magnitude to the decrease in divorce prevalence (i.e., we cannot reject that the two are equal in magnitude). We also do not observe any statistically significant change in the prevalence of widowhood or never having been married, neither of which should be should be directly affected by Medicaid expansion.

While this main result may seem large, given the relatively small magnitude of Medicaid expansion on actual take-up of Medicaid (e.g., in Wherry and Miller 2016), we stress that a couple does not have to actually enroll in the Medicaid expansion to be treated. All a couple has to do is consider their options (likely while they are still on private insurance), and weigh the costs and benefits of divorce if there is a significant asset test in their state of residence. Unfortunately, given the paucity of comprehensive data on couples that consider medical divorce and no studies that we are aware of, there is no way to directly compare our results with those of the literature on the more direct effects.

We have estimated the number of individuals who stayed married as a result of the partial Medicaid expansion. In 2016, there were approximately 8.5 million individuals age 50–64 with at least a college degree in the states that expanded Medicaid on schedule. A 1.6 percentage point decrease in divorce prevalence would have meant that in the states that expanded Medicaid on-schedule, approximately 136,000 fewer individuals (i.e., 68,000 fewer couples) divorced. While this estimate is larger than we had anticipated, there are no other robust estimates of the magnitude of medical divorce for comparison.[19] Thus, our results indicate that medical divorce is a significant contributor to the incidence of divorce among older adults.

Robustness Checks

While Figure 1 suggests that the prevalence of divorce followed parallel trends until the passage of the ACA, we estimated placebo regressions following Slusky (2017) to check this assumption. This test uses the same grouping of states as above but compares them using different years of data before and after a "placebo" Medicaid expansion. For example, instead of comparing 2014–2016 to 2008–2011, we compared 2010–2012 to 2004–2007. Table 4, Panel A shows the results of these regressions.

Table 4: Placebo Results

Panel A: Placebo Time Periods (All for those 50–64 with a college degree)

	(1)	(2)	(3)	(4)	(5)
Control Years	2004–2007	2003–2006	2002–2005	2001–2004	2000–2003
Treated Years	2010–2012	2009–2011	2008–2010	2007–2009	2006–2008
Treated *	-0.00116	-0.00113	-0.00137	0.00208	-0.00116
Implemented	(0.00438)	(0.00477)	(0.00633)	(0.00661)	(0.00711)
Observations	124,292	121,217	117,978	113,066	106,540
R-squared	0.014	0.015	0.016	0.017	0.017
Mean	0.140	0.140	0.139	0.139	0.140

[19] In presenting the results of this paper, we have been consistently shocked by the number of individuals who have approached us and reported that medical divorce was recommended to their parents. These anecdotes, combined with the results of this paper, have also led us to believe that medical divorce is far more common than previously reported.

Panel B: Placebo Subsamples

	(1) Low Education	(2) Below Median Income	(3) Below 25th Percentile Income	(4) Age 25–49	(5) Age 65+
Treated * Implemented	-0.00327	-0.00491	-0.00177	-0.00202	0.00438
	(0.00428)	(0.00694)	(0.00877)	(0.00455)	(0.00611)
Observations	293,121	120,593	59,565	233,033	74,890
R-squared	0.008	0.015	0.014	0.027	0.025
Mean	0.175	0.171	0.166	0.0649	0.109

Notes: Dependent variable is a divorce dummy. 2008–2011 and 2014–2016, All regressions include individual level controls, the monthly state unemployment rate, and state, year, and month fixed effects. Robust standard errors in parenthesis are clustered at state level. In Panel B, Column (1) is for those 50–64 with educational attainment less than a college degree. Column (2) is for those 50–64 with weekly earnings less than $797.30. Column (3) is for those 50–64 with weekly earnings less than $480.76. Columns (4) and (5) are for those with at least a college degree. Weighted. *** p<0.01, ** p<0.05, * p<0.1.

Columns (1)–(5) then compare the prevalence of divorce for earlier in time periods of 3 control years, 2 dropped years, ambiguous years, and 3 treated years.[20] None of the coefficients are statistically significant at even the 10% level nor are the point estimates larger in magnitude than our estimated results. Indeed, the placebo point estimates are all an order of magnitude smaller. Additionally, divorce prevalence (shown in the "mean" row) has remained relatively constant for the college-educated 50–64 population.

Table 4, Panel B contains the results of estimating our model on four placebo populations for whom we do not expect Medicaid expansion to affect medical divorce. If Medicaid expansion reduces financial distress in households and improves overall health, this would likely reduce divorce rates for everyone in states that expanded Medicaid. As a result, if our estimates are capturing changes in medical divorce, we should observe a significant impact of Medicaid expansion for only those who were older and had both increased rates of illness and higher levels of assets.

Column (1) is the other half of the 50–64 sample used for the main regressions above: those with less than a college degree. These individuals are much less likely to have sufficient retirement savings and other assets that would create incentives for a medical divorce. As expected, we do not find a statistically significant effect here. Column (2) is analogous to column (3) in Table 3: those 50–64 with less than median ($797.30) earnings per week. Column (3) takes this a step further and looks at those with earnings below the 25th percentile ($480.76). Here we also do not see any statistically significant effects. The intuition here

[20] To be more conservative in ensuring we do not have statistically significant placebo results, we use year and month fixed effects separately in the specification for Table 4.

is the reverse of above – that currently low income individuals are unlikely to have substantial assets and therefore unlikely to get divorced for financial reasons.[21]

Columns (4) and (5) examine those with at least a college degree but outside our target age range. Those 25–49 are likely too young to have the kind of degenerative health problems that would potentially lead to medical divorce, and likely do not have sufficient retirement assets. Those 65 and over were not eligible for the ACA's Medicaid expansion which was only designed to cover those too young to qualify for Medicare. In neither case do we see statistically significant results.

In Table 5, we include triple and quadruple difference specifications, using those over 65 or without college degrees as the additional differences. The triple differences follow the following specification:

$$\begin{aligned} y_{ismyt} = \alpha + \gamma Treated_s + \delta Implemented_{my} + ThirdDim_t + \left(Treated_s * Implemented_{my}\right) \\ + \ (Treated_s * ThirdDim_t) + \left(Implemented_{my} * ThirdDim_t\right) \\ + \ \mu\left(Treated_s * Implemented_{my} * ThirdDim_t\right) + \mathbf{state}_s + \mathbf{yearmonth}_{my} \\ + \beta UR_{smy} + \mathbf{X}_{ismyt} + \varepsilon_{ismyt} \end{aligned}$$

where *t* refers to the third dimension (either those 50–64 or with a college degree) and *ThirdDim* is a dummy variable for whether the individual meets that criteria or now. μ is the coefficient of interest.

Columns (1) and (4) in Table 4 contain the respective difference-in-differences for those other samples. Column (1) in Table 5 shows our main difference-in-differences result from above, estimated on the college-educated sample age 50–64. Columns (2) is a triple difference specification estimated on the college-educated sample age 50 and above. It includes all the respective pairs of difference-in-difference controls (i.e., age and time, age and expansion states, expansion states and time). Column (3) is a triple difference for those of all levels of education attunement age 50–64 and again includes all pairs of difference-in-difference controls (i.e., education and time, education and expansions states, and age and education).

Finally, column (4) is a quadruple difference specification estimated on those of all levels of educational attainment age 50 and above, and includes all difference in difference and triple difference controls, as shown in the following equation:

[21] This also assumes that the sick individual will eventually substantially reduced labor supply (and therefore income) or stop all together when the individual needs to qualify for medical care. This is consistent with the consensus in the literature that health shocks reduce labor supply and earnings (e.g., see Lenhart 2019 and Parro and Pohl 2021).

$$
\begin{aligned}
y_{ismyae} = {} & \alpha + \gamma Treated_s + \delta Implemented_{my} + College_e + Age50to64_a \\
& + \left(Treated_s * Implemented_{my}\right) + \ (Treated_s * College_e) \\
& + \ (Treated_s * Age50to64_a) + \left(Implemented_{my} * College_e\right) \\
& + \left(Implemented_{my} * Age50to64_a\right) + (College_e * Age50to64_a) \\
& + \left(Treated_s * Implemented_{my} * College_e\right) \\
& + \left(Treated_s * Implemented_{my} * Age50to64_a\right) \\
& + \left(Implemented_{my} * Age50to64_a * College_e\right) \\
& + (Treated_s * College_e * Age50to64_a) \\
& + \theta\left(Treated_s * Implemented_{my} * Age50to64_a * College_e\right) + \mathbf{state}_s \\
& + \mathbf{yearmonth}_{my} + \beta UR_{smy} + \mathbf{X}_{ismyae} + \varepsilon_{ismyae}
\end{aligned}
$$

where *a* refers to age and *e* refers to education, and *Age50to64* is a dummy for whether an individual is in the age range of 50–64 with a college degree are the ones who are less likely to get divorced after Medicaid expansion. The triple and quadruple differences support our main hypotheses that individuals 50–64 with a college degree are the ones who are less likely to get divorced after Medicaid expansion.

Table 5: Triple and Quadruple Difference Specifications

	(1) Diff in Diff	(2) Triple Difference, All Ages	(3) Triple Difference, All Education	(4) Quadruple Difference, All Observations
Treated * Implemented	-0.0160***	0.00361	-0.00355	-0.00467
	(0.00359)	(0.00568)	(0.00434)	(0.00429)
Treated * Implemented * Age50to64		-0.0204**		0.000709
		(0.00756)		(0.00488)
Treated * Implemented * College			-0.0134***	0.00793
			(0.00442)	(0.00688)
Treat * Implemented * Age50to64 * College				-0.0214**
				(0.00948)
Observations	130,410	198,325	423,531	706,444
R-squared	0.015	0.019	0.012	0.021
Demographic Controls	X	X	X	X
State Fixed Effects	X	X	X	X
Year-Month Fixed Effects	X	X	X	X
Subsample	College-educated Age 50–64	College-educated Age 50+	All education levels, 50–64	All education levels, age 50+
Mean	0.138	0.138	0.138	0.138

Notes: 2008–2011 and 2014–2016. Dependent variable is a divorce dummy. All regressions include individual level controls, the monthly state unemployment rate, and state, year-month fixed effects. Columns (2) and (3) include all pairs of difference-in-difference controls. Column (4) includes all difference in difference and triple difference controls. Robust standard errors in parenthesis are clustered at state level. Weighted. *** p<0.01, ** p<0.05, * p<0.1.

The appendix includes additional robustness checks. In Appendix Table 2, we check our results for the subsample of each of the eight rotation groups in the CPS (as opposed to the MORG file which includes only the 4th and 8th), using the basic CPS abstracts from IPUMS (Flood et al. 2020) and find that our results are consistently negative and significant. In Appendix Table 3, we show that our results are negative and significant if we control for home ownership and as expected if we stratify by home ownership (hypothesizing that owners would be more affected by Medicaid expansion). In Appendix Table 4, we estimate the divorce models separately by race. The effects are all of the same magnitude, though not statistically significant for black people given the small number of black people in the sample with at least a college education. In Appendix Table 5, we show consistent results at the household level and using household weights.

In Appendix Table 6, we see if our estimates are robust to different treatments of the years between the Supreme Court decision and Medicaid expansion (2012–2013). The drop in divorce prevalence in 2014–2016 is so large that our results are broadly consistent regardless of how we treat 2012–2013, and we cannot reject that any of the treatment effects are the same as each other.

In Appendix Table 7, we include each treated state (along with all of the control states), one at a time. Across 20 treated states, 12 have negative statistically significant drops in divorce, 7 have statistically insignificant effects, and only 1 has a statistically significant (at the 5% level) increase. This strongly suggests that our results are not driven by a large drop in a few states, but rather significant drops in a majority of treated states. Similarly, in Appendix Table 8, we include each control state (along with all of the treated states), one at a time. Out of 18 results, 13 are negative and significant (all at the 1% level), 1 is not significant, and 4 are positive and significant (2 at the 1% level, 1 at 5%, and 1 at 10%). Both of these sets of results are important as every state's asset test rules are somewhat different, and we want to ensure that individual states are not driving our results.

In Appendix Table 9, we look at five-year age bands from age 25–64. We only see statistically significant results in the 50–54 and 55–59 ages, with the majority of our results coming from the 55–59 group. This makes sense as this group is the far enough from retirement for divorce to be beneficial but old enough to get sick and have sufficient assets.

In Appendix Table 10 and Appendix Figure 3 we repeat our analysis using the ACS from IPUMS (Ruggles et al. 2017) and find similar results, albeit smaller results (a 0.36 percentage point or 2.4% decrease, corresponding to 28,000 fewer divorced individuals or 14,000 fewer divorced couples). We favor the estimates from the CPS because it is the source of the US Census Bureau's annual estimates of marital status. According to the US Census Bureau (2016):

> Because of its detailed questionnaire and its experienced interviewing staff, the Current Population Survey (CPS) Annual Social and Economic Supplement (ASEC)[22] is a high quality source of information used to produce the official annual estimate of poverty, and estimates of a number of other socioeconomic and demographic characteristics, including income, health insurance coverage, school enrollment, marital status, and family structure.

As a result, we place more weight on the estimates we found using the CPS as compared to the ACS.[23]

[22] We acknowledge that this statement explicitly refers to the ASEC and not to the basic CPS MORG. However, as we show in Appendix Table 13 and explain below, are results are robust to using the ASEC.

[23] When studying the effect of the ACA's parental mandate on the marriage rates of young adults, Abramowitz and Dillender (2017) suggest that one should use flow measures in the ACS, as opposed to the stock measures. While this may be appropriate for their study on effects of an immediate policy (i.e., being able to stay on one's parents' insurance), we believe this is less appropriate in our case, where the timing of the anticipatory need for Medicaid is less clear.

In Appendix Table 11, we use the alternate Medicaid expansion classification of states from Courtemanche, Marton, and Yelowitz (2019). This classification is narrower than ours, using only 21 states instead of 38. Their list of “new expanders” includes only 8 states from our list: Arkansas, Kentucky, Minnesota, Nevada, New Mexico, North Dakota, Ohio, West Virginia, dropping Arizona, California, Colorado, Connecticut, Hawaii, Illinois, Iowa, Maryland, New Jersey Oregon, Rhode Island, Washington, and adding New Hampshire (which dropped as a late expander since it didn’t expand until later in 2014). Their list of “never expanders” includes only 12 states from our list: Alabama, Florida, Georgia, Kansas, Mississippi, Missouri, North Carolina, Oklahoma, South Carolina, South Dakota, Texas, Wyoming, and dropping Idaho, Nebraska, Tennessee, Utah, Virginia, and Wisconsin. Despite these restrictions, which cuts the sample size in half, our results across of the specifications in Table 2 are still negative and statistically significant.

In Appendix Table 12, we drop individuals who are parents of minor children (i.e., has any children under the age of 18). This is due to some states (such as Arizona) having eliminated assets tests for low-income parents before the passage of the ACA (Zhu 2020). Given that we focus in this paper on those 50–64, only 17% of the approximately 130,000 observations in our main sample are eliminated in this robustness check. Perhaps unsurprisingly, our results are consistent when estimated on this subsample.

In Appendix Table 13, we replicate our results using the CPS Annual Social and Economic Supplement (ASEC) which once a year (in March) asks questions about health insurance in the previous calendar year (Flood et al. 2020). We first show that our results are robust, using either the outgoing rotation groups or all of the rotation groups, though we lose substantial statistical power due to a smaller sample size. We then show the point estimates are consistent when controlling for any health insurance or for types of health insurance, but again we have less statistical significance due to reduced power. Finally, we can also stratify by health insurance, which can be thought of as another proxy for affluence and financial resources (along with the results shown above which stratify by education or income). The individuals potentially interested in medical divorce may have insurance now (even though their health is declining) but are worried about medical expenses later and so want to start the divorce process now. Consistent with this intuition, we find while there are no statistically significant results (and the point estimate is positive) for the 7% of the sample that does not have insurance, the results are strongly statistically significant for the remaining 93% with insurance and 87% with private insurance.

Overall, these robustness checks suggest that our estimates are identifying changes in the incidence of medical divorce.

Discussion and Conclusion

It is also important for us to put our results in the context of other (limited) research on gray divorce for the entire population (i.e., not just for the states that expanded Medicaid on-schedule). The Pew Research Center (Stepler 2017) estimates that the divorce rate for Americans age 50 and older is about 1% per year. In 2016 there were approximately 111 million Americans age 50 and older, but again using the sums of the CPS survey weights as rough estimates of population, there were 20 million Americans (18% of the 50+ population) ages 50–64 with a college degree. An annual divorce flow of 1% of the population 50 and older would be 1.11 million individuals, whereas using the estimated divorce rate per year derived from our estimates, 0.53%[24] of 20 million, is only 106,000 individuals per year (across the entire United States). So the decrease in divorce rates for all Americans age 50 and older is only 9.5%[25].

An alternate approach would be to look at the number of potential individuals in terms of self-reported health. The CPS ASEC (which per Appendix Table 13 produces robust results, as described above) contains a variable for self-reported health. In 2009, there were 17.9 million Americans 50–64 with a college degree, 12.6 million of whom are married. Of these individuals, 718,000 are in fair health and another 205,000 are in poor health. Multiplying 17.9 million by 0.53% yields 95,000 fewer divorced individuals, which is 10% of the 923,000 (718,000 + 205,000) married, college-educated individuals in poor or fair health when the ACA was enacted.

Ours is the first paper that we are aware of that examines and quantifies the extent of medical divorce for older couples in any context. We began our analysis by modeling the economic rationale for medical divorce. Our simple model indicates that whenever assets exceed the asset exemption in a state, couples have an economic incentive to divorce. Although vital statistics do not provide estimates of the divorce rate by age, we use the CPS to analyze divorce incidence. The ACA's Medicaid expansion provides an ideal natural experiment to examine whether there might be medical divorce. If medical divorce were truly rare, we should observe no significant differences in divorce in the treatment and control states. Our results indicate that medical divorce is far more prevalent than previously assumed.

We found that the ACA's Medicaid expansion reduced the prevalence of divorce among college-educated individuals ages 50–64 by 1.6 percentage points, which is an 11.6% decrease on the pre-expansion mean for the treated states. This suggests that Medicaid without asset limits for individuals significantly reduced the incidence of divorce among older and more highly-educated adults. Consistent with the

[24] 1.6 percentage points decline in divorce prevalence divided by 3 years equals a 0.53 percentage point decrease in the divorce rate.

[25] 106,000 individuals divided by 1.11 million individuals equals 9.5%.

predictions of our model, our results strongly suggest that medical divorce was reduced in the first years of the ACA.

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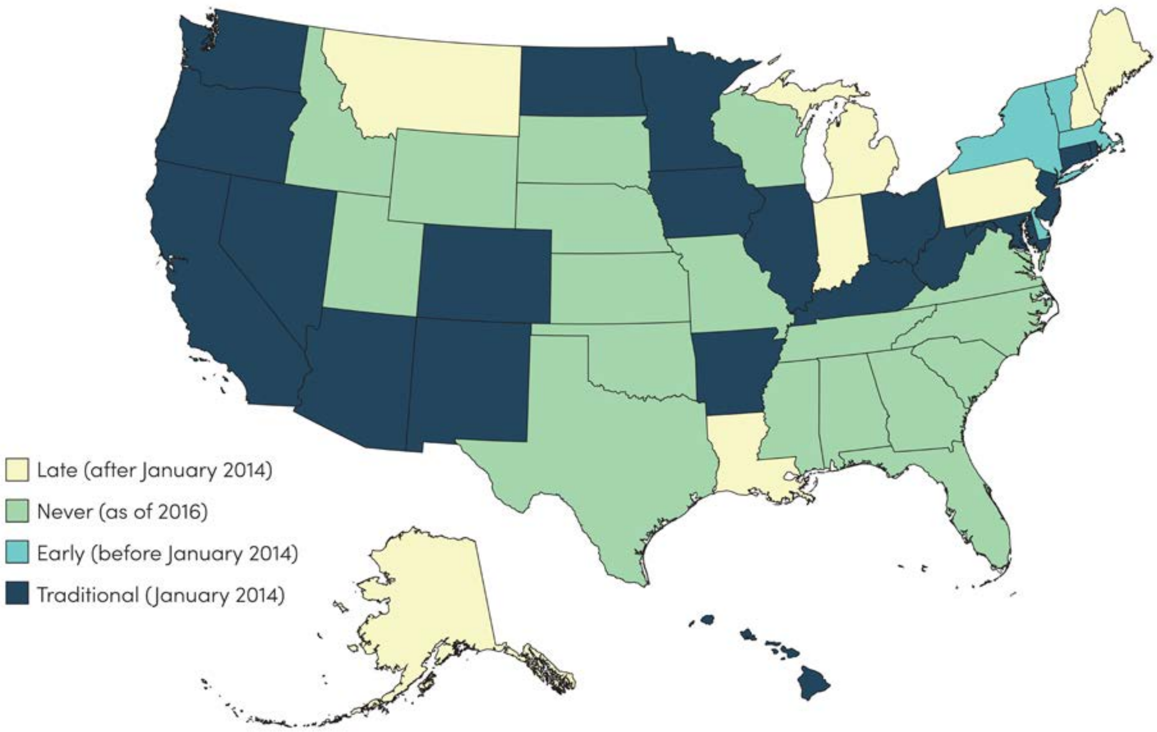
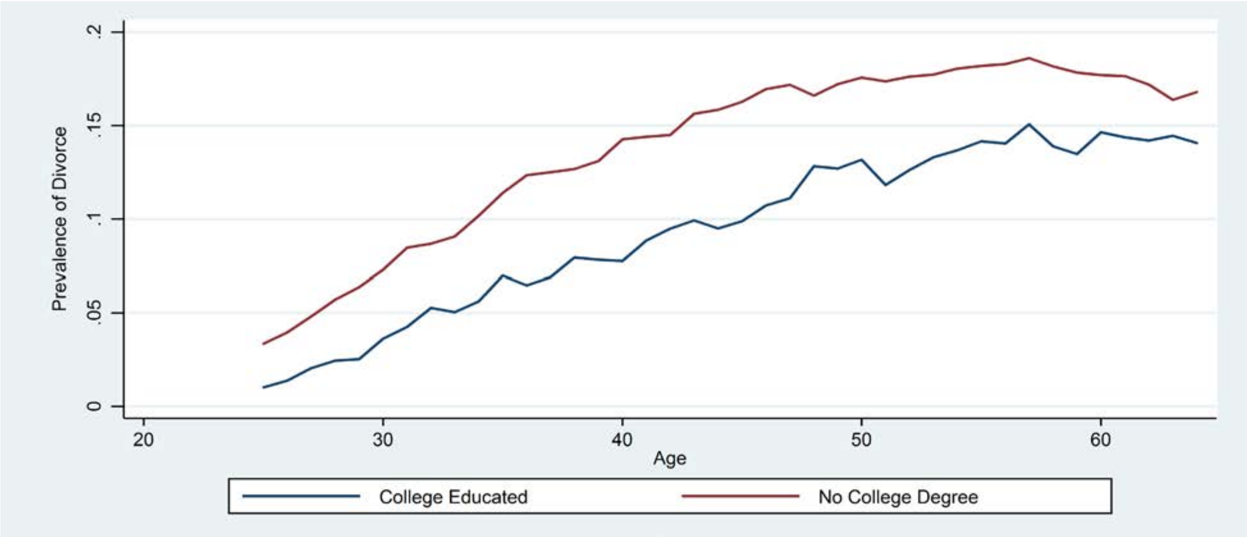
Online Appendix

Appendix Figure 1: States that expanded Medicaid by timing of expansion



Notes: Our treatment group (dark blue) is the traditional states that expanded Medicaid in January of 2014. Our control group (light green) consists of the never expanded. Early (light blue) and Late (yellow) expanders are dropped from the analysis.

Appendix Figure 2: Prevalence of divorce by age by education



Notes: Weighted average divorce prevalence for each age and educational group for the years 2008–2011.

Appendix Table 1: Checking for Pre-Trends

	(1)	(2)	(3)
Treated Group Monthly Linear Time Trend	3.52e-05	4.95e-05	-4.55e-05
	(7.67e-05)	(7.93e-05)	(7.53e-05)
Observations	195,208	195,208	195,208
R-squared	0.000	0.012	0.015
Demographic Controls		X	X
Unemployment Rate		X	X
State Fixed Effects			X

Notes: Dependent variable is a divorce dummy. 2000–2011. Robust standard errors in parenthesis are clustered at state level. Regressions also include a common monthly linear time trend. Weighted. *** p<0.01, ** p<0.05, * p<0.1.

Appendix Table 2: Results by Rotation Group

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Rotation Group	1	2	3	4	5	6	7	8
Treated * Implemented	-0.0155**	-0.0151**	-0.0138**	-0.0134**	-0.0157***	-0.0153***	-0.0167***	-0.0189***
	(0.00654)	(0.00728)	(0.00513)	(0.00507)	(0.00474)	(0.00364)	(0.00352)	(0.00366)
Observations	62,712	64,436	64,641	64,661	64,012	64,844	65,155	65,749
R-squared	0.017	0.017	0.016	0.017	0.016	0.017	0.016	0.016
Mean	0.142	0.142	0.141	0.140	0.137	0.135	0.137	0.136

Notes: Sample is college educated age 50–64. 2008–2011 and 2014–2016. Dependent variable is a divorce dummy. All regressions include individual level controls, the monthly state unemployment rate, and state and year-month fixed effects. Robust standard errors in parenthesis are clustered at state level. Weighted. *** p<0.01, ** p<0.05, * p<0.1.

Appendix Table 3: Results by Home Ownership

	(1) Controlling for home ownership	(2) Homeowner	(3) Renter
Treated * Implemented	-0.0142*** (0.00399)	-0.0154*** (0.00342)	0.00588 (0.0167)
Observations	130,410	115,323	15,087
R-squared	0.042	0.015	0.034
Demographic Control	X	X	X
Unemployment Rate	X	X	X
State Fixed Effects	X	X	X
Year-Month Fixed Effects	X	X	X
Mean	0.138	0.118	0.282

Notes: Sample is college educated age 50–64. 2008–2011 and 2014–2016. Dependent variable is a divorce dummy. All regressions include individual level controls, the monthly state unemployment rate, and state, year-month fixed effects. Robust standard errors in parenthesis are clustered at state level. Weighted. *** p<0.01, ** p<0.05, * p<0.1.

Appendix Table 4: Results by Race

	(1) All	(2) White & Black	(3) White	(4) Black
Treated * Implemented	-0.0164*** (0.00352)	-0.0177*** (0.00377)	-0.0184*** (0.00389)	-0.0171 (0.0212)
Observations	130,410	119,510	111,345	8,165
R-squared	0.013	0.013	0.013	0.040
Demographic Control	X	X	X	X
Unemployment Rate	X	X	X	X
State Fixed Effects	X	X	X	X
Year-Month Fixed Effects	X	X	X	X
Mean	0.138	0.145	0.142	0.201

Notes: Sample is college educated age 50–64. 2008–2011 and 2014–2016. Dependent variable is a divorce dummy. All regressions include individual level controls, the monthly state unemployment rate, and state, year-month fixed effects. Robust standard errors in parenthesis are clustered at state level. Weighted. *** p<0.01, ** p<0.05, * p<0.1.

Appendix Table 5: Household Level Analysis

	(1)	(2)	(3)	(4)
Treated * Implemented	-0.0160***	-0.0169***	-0.0129***	-0.0112**
	(0.00359)	(0.00386)	(0.00415)	(0.00459)
Observations	130,410	130,579	105,018	105,018
R-squared	0.015	0.015	0.004	0.062
Demographic Controls	X	X		X
Unemployment Rate	X	X	X	X
State Fixed Effects	X	X	X	X
Year-Month Fixed Effects	X	X	X	X
Level	Individual	Individual	Household	Household
Weights	Individual	Household	Household	Household
Mean	0.138	0.139	0.117	0.117

Notes: 2008–2011 and 2014–2016. In Columns (1) and (2), as above, the sample is college educated age 50–64 and the dependent variable is a divorce dummy. In Columns (3) and (4), the household is in the sample if it has at least one individual 50–64 with a college degree, and the dependent variable is a dummy for if it has at least one individual 50–64 with a college degree who is divorced. All regressions include the monthly state unemployment rate, and state, year-month fixed effects. Columns (1) and (2) including individual level controls. Column (4) includes dummy variables for each possible value of the median age, sex, black, and educational attainment for the in sample individuals in each household. Robust standard errors in parenthesis are clustered at state level. Column (1) is weighed by individual weights. Column (2) is weighted by the average household weight across individual for each household. Columns (3) and (4) are weighted by the household weight. *** p<0.01, ** p<0.05, * p<0.1.

Appendix Table 6: Including 2012–2013

	(1) Dropping 2012–2013	(2) 2012–2013 as Control	(3) Additional variable for 2012–2013	(4) 2012–2013 as Treated
Treated * Implemented	-0.0160*** (0.00359)	-0.0157*** (0.00432)	-0.0157*** (0.00360)	-0.0096*** (0.00304)
Treated * (2012–2013)			-9.41e-05 (0.00592)	
Observations	130,410	168,828	168,828	168,828
R-squared	0.015	0.015	0.015	0.015
Demographic Control	X	X	X	X
Unemployment Rate	X	X	X	X
State Fixed Effects	X	X	X	X
Year-Month Fixed Effects	X	X	X	X
Mean	0.138	0.138	0.138	0.138

Notes: Sample is college educated age 50–64. 2008–2016. Dependent variable is a divorce dummy. All regressions include individual level controls, the monthly state unemployment rate, and state, year-month fixed effects. Robust standard errors in parenthesis are clustered at state level. Weighted. *** p<0.01, ** p<0.05, * p<0.1.

Appendix Table 7: One Treated State at a Time

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treated State Included	AZ	AR	CA	CO	CT	HI	IL
Treated * Implemented	-0.00896**	0.00479	-0.0226***	0.00140	-0.00421	-0.0390***	-0.0132***
	(0.00317)	(0.00303)	(0.00431)	(0.00329)	(0.00333)	(0.00326)	(0.00300)
Observations	57,036	56,482	70,152	59,172	59,525	57,981	60,300
R-squared	0.017	0.018	0.016	0.018	0.017	0.018	0.018
Demographic Controls	X	X	X	X	X	X	X
State Fixed Effects	X	X	X	X	X	X	X
Year-Month FE	X	X	X	X	X	X	X

	(8)	(9)	(10)	(11)	(12)	(13)
Treated State Included	IA	KY	MD	MN	NV	NJ
Treated * Implemented	0.00490	-0.0475***	-0.0186***	-0.0112***	0.00199	-0.0182***
	(0.00423)	(0.00360)	(0.00440)	(0.00325)	(0.00451)	(0.00314)
Observations	57,242	56,553	59,642	58,786	56,923	59,715
R-squared	0.018	0.018	0.018	0.018	0.018	0.018
Demographic Controls	X	X	X	X	X	X
State Fixed Effects	X	X	X	X	X	X
Year-Month FE	X	X	X	X	X	X

Treated State Included	(14) NM	(15) ND	(16) OH	(17) OR	(18) RI	(19) WA	(20) WV
Treated * Implemented	-0.0108	0.0145**	-0.0228***	-0.0218***	-0.0191***	-0.0260***	-0.00375
	(0.00691)	(0.00568)	(0.00368)	(0.00356)	(0.00408)	(0.00319)	(0.00560)
Observations	56,872	57,051	58,838	57,540	57,955	58,261	56,705
R-squared	0.018	0.018	0.017	0.018	0.018	0.018	0.018
Demographic Controls	X	X	X	X	X	X	X
State Fixed Effects	X	X	X	X	X	X	X
Year-Month FE	X	X	X	X	X	X	X

Notes: All regressions include control states (AL, FL, GA, ID, KS, MS, MO, NE, NC, OK, SC, SD, TN, TX, UT, VA, WI, WY). Sample is college educated age 50–64. 2008–2011 and 2014–2016. Dependent variable is a divorce dummy. All regressions include individual level controls, the monthly state unemployment rate, and state, year-month fixed effects. Robust standard errors in parenthesis are clustered at state level. Weighted. *** p<0.01, ** p<0.05, * p<0.1.

Appendix Table 8: One Control State at a Time

	(1)	(2)	(3)	(4)	(5)	(6)
Control State Included	AL	FL	GA	ID	KS	MS
Treated * Implemented	-0.0312***	-0.0230***	0.00584***	-0.0251***	0.0147***	-0.0731***
	(0.00174)	(0.00176)	(0.00171)	(0.00196)	(0.00277)	(0.00251)
Observations	77,525	82,770	78,942	77,391	78,150	77,163
R-squared	0.016	0.016	0.016	0.015	0.015	0.016
Demographic Controls	X	X	X	X	X	X
State Fixed Effects	X	X	X	X	X	X
Year-Month FE	X	X	X	X	X	X

	(7)	(8)	(9)	(10)	(11)	(12)
Control State Included	MO	NE	NC	OK	SC	SD
Treated * Implemented	-0.0360***	-0.0547***	-0.00642***	-0.0230***	-0.0194***	0.00655*
	(0.00195)	(0.00341)	(0.00176)	(0.00364)	(0.00215)	(0.00326)
Observations	77,983	78,001	79,068	77,530	77,726	77,690
R-squared	X	X	X	X	X	X
Demographic Controls	X	X	X	X	X	X
State Fixed Effects	X	X	X	X	X	X
Year-Month FE	X	X	X	X	X	X

Control State Included	(13) TN	(14) TX	(15) UT	(16) VA	(17) WI	(18) WY
Treated * Implemented	-0.00552***	-0.0124***	0.00168	-0.0107***	-0.0225***	0.00947**
	(0.00175)	(0.00204)	(0.00220)	(0.00289)	(0.00188)	(0.00375)
Observations	77,803	83,345	77,352	80,009	78,735	77,594
R-squared	0.016	0.015	0.015	0.015	0.015	0.015
Demographic Controls	X	X	X	X	X	X
State Fixed Effects	X	X	X	X	X	X
Year-Month FE	X	X	X	X	X	X

Notes: All regressions include treated states (AZ, AR, CA, CO, CT, HI, IL, IA, KY, MD, MN, NV, NJ, NM, ND, OH, OR, RI, WA, WV). Sample is college educated age 50–64. 2008–2011 and 2014–2016. Dependent variable is a divorce dummy. All regressions include individual level controls, the monthly state unemployment rate, and state, year-month fixed effects. Robust standard errors in parenthesis are clustered at state level. Weighted. *** p<0.01, ** p<0.05, * p<0.1.

Appendix Table 9: Five-Year Age Bands

	(1) 25–64	(2) 25–29	(3) 30–34	(4) 35–39	(5) 40–44	(6) 45–49	(7) 50–54	(8) 55–59	(9) 60–64
Treated * Implemented	-0.00686**	0.00311	0.00543	0.00146	-0.00580	-0.0138	-0.0160**	-0.0259***	-0.00469
	(0.00305)	(0.00473)	(0.00430)	(0.00776)	(0.00901)	(0.00864)	(0.00611)	(0.00589)	(0.00717)
Observations	363,443	42,833	47,048	47,888	47,912	47,352	46,344	44,348	39,718
R-squared	0.033	0.009	0.012	0.013	0.014	0.013	0.017	0.018	0.019
Demographic Controls	X	X	X	X	X	X	X	X	X
State Fixed Effects	X	X	X	X	X	X	X	X	X
Year-Month Fixed Effects	X	X	X	X	X	X	X	X	X
Mean	0.0902	0.0155	0.0415	0.0663	0.0851	0.112	0.127	0.147	0.142

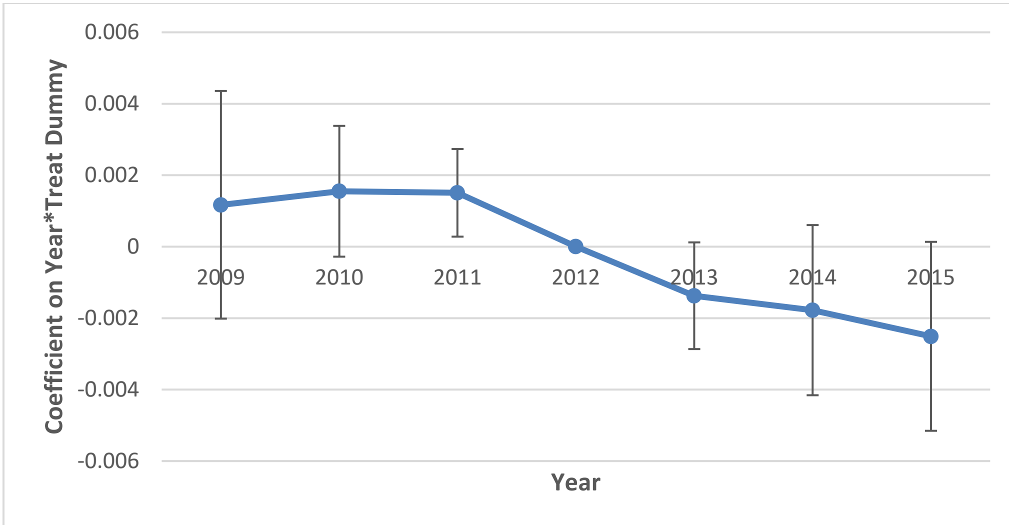
Note: Sample is college educated. 2008–2011 and 2014–2016. Dependent variable is a divorce dummy. All regressions include individual level controls, the monthly state unemployment rate, and state, year-month fixed effects. Robust standard errors in parenthesis are clustered at state level. Weighted. *** p<0.01, ** p<0.05, * p<0.1.

Appendix Table 10: Results Using the ACS

	(1)	(2)	(3)	(4)	(5)	(6)
Data set	CPS	ACS	ACS	ACS	ACS	ACS
Outcome	Divorced	Divorced	Divorced	Divorced	Divorced	Married
Treated * Implemented	-0.0206***	-0.00468**	-0.00383**	-0.00356**	-0.00356**	0.00626**
	(0.00435)	(0.00175)	(0.00162)	(0.00168)	(0.00168)	(0.00264)
Observations	93,840	3,662,901	3,662,901	3,662,901	3,662,901	3,662,901
R-squared	0.015	0.000	0.011	0.014	0.014	0.027
Demographic Controls	X		X	X	X	X
State Fixed Effects	X			X	X	X
Year Fixed Effects	X				X	X
Mean	0.140	0.147	0.147	0.147	0.147	0.719

Notes: Sample is college educated age 50–64. Dependent variable is a divorce or married dummy, as indicated. 2009–2011 and 2014–2015, with at least a college degree. Demographic controls include age and educational attainment fixed effects and dummies for race and gender. Robust standard errors in parenthesis are clustered at state level. Weighted. ACS refers to the 5–year microdata. *** p<0.01, ** p<0.05, * p<0.1.

Appendix Figure 3: Event Study using ACS



Notes: Sample is college-educated age 50–64. Dependent variable is a divorce dummy. All regressions include demographic controls (age and educational attainment fixed effects and dummies for race and gender), and state and year fixed effects. Coefficients are relative to 2012. The whiskers show a 95% confidence interval.

Appendix Table 11: Results Using Courtemanche, Marton, Yelowitz (2019) Classification

	(1) Divorced	(2) Divorced	(3) Divorced	(4) Divorced	(5) Divorced
Treated * Implemented	-0.0144**	-0.0142**	-0.0153**	-0.0152**	-0.0408**
	(0.00631)	(0.00569)	(0.00561)	(0.00554)	(0.0192)
Observations	62,603	62,603	62,603	62,603	62,603
R-squared	0.000	0.013	0.016	0.017	0.018
Mean	0.145	0.145	0.145	0.145	0.145
Demographic Controls		X	X	X	X
Unemployment Rate		X	X	X	X
State Fixed Effects			X	X	X
Year Fixed Effects			X		
Month Fixed Effects			X		
Year-Month Fixed Effects				X	X
State-Specific Time Trends					X

Notes: Sample is college-educated age 50–64. Dependent variable is a divorce dummy. 2008–2011 and 2014–2016. Demographic controls include age and educational attainment fixed effects and dummies for race and gender. New expanders are Arkansas, Kentucky, Michigan, Nevada, New Hampshire, New Mexico, North Dakota, Ohio, and West Virginia. Never expanders are Alabama, Florida, Georgia, Kansas, Mississippi, Missouri, North Carolina, Oklahoma, South Carolina, South Dakota, Texas, and Wyoming. Robust standard errors in parenthesis are clustered at state level. Weighted. *** p<0.01, ** p<0.05, * p<0.1.

Appendix Table 12: Regression Results for Only Those Without Children

	(1) Divorced	(2) Divorced	(3) Divorced	(4) Divorced	(5) Divorced
Treated * Implemented	-0.0181*** (0.00451)	-0.0166*** (0.00428)	-0.0162*** (0.00434)	-0.0164*** (0.00436)	-0.0239* (0.0127)
Observations	107,845	107,845	107,845	107,845	107,845
R-squared	0.000	0.009	0.012	0.013	0.013
Mean	0.154	0.154	0.154	0.154	0.154
Demographic Controls		X	X	X	X
Unemployment Rate		X	X	X	X
State Fixed Effects			X	X	X
Year Fixed Effects			X		
Month Fixed Effects			X		
Year-Month Fixed Effects				X	X
State-Specific Time Trends					X

Notes: Sample is college-educated age 50–64 without children under the age of 18. Dependent variable is a divorce dummy. 2008–2011 and 2014–2016. Demographic controls include age and educational attainment fixed effects and dummies for race and gender. Robust standard errors in parenthesis are clustered at state level. Weighted. *** p<0.01, ** p<0.05, * p<0.1.

Appendix Table 13: Regression Results Using CPS ASEC and Controlling for Health Insurance

	(1) Divorced	(2) Divorced	(3) Divorced	(4) Divorced	(5) Divorced	(6) Divorced	(7) Divorced
Treated * Implemented	-0.0240 (0.0148)	-0.0166* (0.00827)	-0.0152* (0.00835)	-0.0166* (0.00823)	0.0130 (0.0440)	-0.0179** (0.00676)	-0.0227*** (0.00682)
Observations	14,347	56,073	56,073	56,073	4,103	51,970	48,813
R-squared	0.025	0.017	0.022	0.027	0.034	0.017	0.017
Mean	0.131	0.140	0.140	0.140	0.227	0.133	0.126
Sample	All	All	All	All	Uninsured	Insured	Privately Insured
Rotation Groups	Outgoing	All	All	All	All	All	All
Extra Controls			Any Health Insurance	Types of Health Insurance			

Notes: Sample is college-educated age 50–64. Dependent variable is a divorce dummy. 2008–2011 and 2014–2016. Demographic controls include age and educational attainment fixed effects and dummies for race and gender. All regressions include individual level controls, the monthly state unemployment rate, and state and year fixed effects. Robust standard errors in parenthesis are clustered at state level. Weighted. *** p<0.01, ** p<0.05, * p<0.1.