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EFFECTS OF ACCESS TO LEGAL SAME-SEX MARRIAGE ON MARRIAGE AND HEALTH: EVIDENCE FROM BRFSS

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ABSTRACT

We exploit variation in access to legal same-sex marriage (SSM) across states and time to provide novel evidence of its effects on marriage and health using data from the CDC BRFSS from 2000-2016, a period spanning the entire rollout of legal SSM across the United States. Our main approach is to relate changes in outcomes for individuals in same-sex households (SSH) [i.e., households with exactly two same-sex adults], which we show includes a substantial share of gay and lesbian couples, coincident with adoption of legal SSM in two-way fixed effects models. We find robust evidence that access to legal SSM significantly increased marriage take-up among men and women in SSH. We also find that legal SSM was associated with significant increases in health insurance, access to care, and utilization for men in SSH. Our results provide the first evidence that legal access to SSM improved health for adult gay men.

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1. Introduction

Advancements in civil rights for and changes in social attitudes about lesbian, gay, bisexual, and transgender (LGBT) individuals in the United States represent some of the most striking social changes in recent decades. In the policy arena these shifts have been reflected in a major Supreme Court ruling in 2015, *Obergefell v. Hodges*, which effectively legalized same-sex marriage (SSM) in the eyes of the federal government throughout the US. Prior to *Obergefell*, however, a series of state-level court rulings and legislative actions dating back to a 2004 court case in Massachusetts created a complex legal patchwork of access to legal SSM across space and time, such that nine states adopted same-sex marriage without the associated federal rights and responsibilities (such as equal tax treatment, immigration rights, and others). A June 2013 Supreme Court ruling in *United States v. Windsor* gave federal marriage recognition to same-sex couples that wed in states with legal recognition but did not require other states to recognize those marriages. This uncertain legal landscape was exacerbated as more states adopted marriage equality in late 2013 and throughout 2014 until nationwide legal access to federally recognized same-sex marriage was granted in June 2015 with the *Obergefell* ruling (see Figure 1).

Despite the strong theoretical possibility that these fundamental changes in access to marriage had meaningful effects on the lives of sexual minorities, we know strikingly little about how these policies affected outcomes. This

knowledge gap is due primarily to data limitations: very few datasets include information that could plausibly identify gay men, lesbians, and bisexual individuals, and most of the small number of datasets that do include sexual orientation information did not pre-date legal access to SSM, thus precluding most gold-standard quasi-experimental approaches to study this research question. A handful of studies have examined how legal access to SSM has affected population-wide health outcomes such as sexually transmitted infections or youth suicide ideation and attempt, but little work has studied outcomes such as direct take-up of marriage, health insurance, healthcare access, and health.

In this paper we provide new evidence on these questions by using data from the 2000-2016 Behavioral Risk Factor Surveillance System (BRFSS) and exploiting spatial and temporal variation in access to legal SSM induced by state and federal court rulings and legislative activity. The BRFSS did not include direct questions about sexual orientation over most of our sample period, so our analysis relies on a novel method for indirectly identifying a sample that is most likely to be directly affected by legal SSM. Specifically, we follow previous BRFSS-based research that makes use of information on the sex composition of adults in the household – which is ascertained in the household screener portion of the survey – to identify a sample that is likely to contain a nontrivial share of gay and lesbian couples. That is, we identify a treatment group composed of individuals who report exactly two same-sex adults in the household and exactly

zero different-sex adults in the household. Through a variety of direct and indirect measures, we estimate that 11-28 percent of individuals in households with exactly two same-sex adults are likely to be sexual minorities. For individuals in this household structure who also report being married, the relevant share is even higher: 46-60 percent.

We also make use of individuals in other household configurations as plausible falsification samples. For example, we estimate that about 99 percent of individuals who report exactly one male adult and one female adult in the household are heterosexual (most of whom are likely to be straight couples). The key advantage and innovation of our measurement approach is that it allows us to identify a meaningful sample size of 'same-sex male adult' and 'same-sex female adult' households that pre-dates all of the policy variation in legal access to SSM in a dataset that includes key family structure and health-related outcomes that have not previously been studied in this literature. This allows us to consider new and important health outcomes while at the same time properly addressing issues of causal inference. We also address several potential concerns with our approach, including the possibility that household structure itself (i.e., the probability of observing any household with exactly two adults or exactly two same-sex adults) could be endogenously related to legal access to SSM, and we show that this is not pervasive.

Our main empirical approach is a standard difference-in-differences (DD) model that examines the sample of individuals in same-sex households and leverages state variation in the timing of legal access to SSM with state and time fixed effects, controlling also for individual characteristics and other aspects of the policy environment toward sexual minorities (e.g., nondiscrimination laws, domestic partner/civil union laws, state bans on same-sex marriage, and others). The DD model assumes that these outcomes for individuals in same-sex households would have trended similarly in states with and without legal access to SSM in the absence of the change in legal regime. We first focus directly on marriage take-up, and we then consider a range of health-related outcomes, including having any health insurance plan, having a regular doctor, having a checkup in the past year, having unmet medical needs due to cost, self-rated health, bad physical and mental health days, substance use, and preventive care utilization.

We report several core findings from this research. First, legal access to SSM is not meaningfully associated with living arrangements in the population, suggesting that our focus on individuals in same-sex households is unlikely to be biased by composition effects. Second, we find clear evidence that legal access to same-sex marriage in one's state is associated with a statistically significant increase in the likelihood that an individual in a same-sex household reports being married, an effect on the order of 79 and 41 percent relative to the pre-reform

mean for women and men, respectively. This effect is highly robust to various sample and model specification choices and obtains significance only after adoption of legal same-sex marriage in an event-study framework. It is also unique to individuals in same-sex households: that is, it is not meaningfully observed in the falsification sample of households with one man and one woman. Third, we find that legal access to SSM in one's state is associated with statistically significant increases in the probability of having health insurance, reporting a usual source of care, and having a checkup in the past year for men (but not women) in same-sex households. Finally, we find no systematic effects of legal access to same-sex marriage on unmet medical needs, physical or mental health, substance use, or preventive care utilization for men or women in same-sex households. Overall our results using a novel measurement strategy provide the most comprehensive evidence to date that legal access to SSM significantly increased marriage take-up for gay and lesbian couples and also significantly improved healthcare access and utilization for adult gay men in same-sex households.

The remainder of the paper proceeds as follows. Section 2 discusses the mechanisms through which legal access to same-sex marriage could affect marriage and health-related outcomes and reviews the literature on policy determinants of marriage and on the effects of marriage on health. It also reviews a small and emerging literature on the effects of access to legal SSM. Section 3

then describes the data and outlines our empirical approach. Section 4 presents the results, and Section 5 offers a discussion and concludes.

2. Mechanisms and Previous Literature

2.1 Literature on the policy determinants of marriage

This paper contributes to a large literature on the policy determinants of marriage. Scholars have demonstrated the empirical significance of the tax-cost of marriage (Alm and Whittington 1999), unilateral divorce laws (e.g., Peters 1986, Friedberg 1998, Wolfers 2006), blood test requirements (Buckles et al. 2011), and minimum age requirements (Blank et al. 2009) on different-sex marriage rates. Our paper adds to those literatures by studying a much more direct change to the overall cost of getting married – access to the legal institution for same-sex couples – than has been typically studied. In this sense, our work is related to prior studies of anti-miscegenation statutes and the effects of *Loving v. Virginia* on interracial marriage (Fryer 2007, Gevrek 2014).

2.2 Literature on the effects of legal access to same-sex marriage on marriage take-up

A handful of studies in economics and demography have specifically documented how legal access to same-sex marriage affected family formation outcomes,

including take-up of marriage by sexual minorities.[1] Carpenter (2018, forthcoming) examines the experience of Massachusetts' legalization of same-sex marriage through the state Supreme Court in 2004 using data from the Massachusetts BRFSS and finds that it significantly increased marriage take-up by lesbians and gay men, with larger effects for lesbians. Some research has also examined how same-sex marriage policies – including bans on same-sex marriage – affected heterosexual individuals. Trandafir (2014) finds no relationship between the adoption of legal same-sex marriage in the Netherlands in 2001 and rates of different-sex marriage. Trandafir (2015) examines relationship recognition in a larger sample of countries for the period 1980-2009 and finds that legal same-sex marriage did not affect family formation (as measured by marriage, divorce, and extramarital births) for different-sex couples. Langbein and Yost (2009) study the US context through 2004 and find that same-sex relationship policies have no effects on different-sex marriage, divorce, and abortion rates or on the percentage of children born out of wedlock; Dillender (2015) finds a similar null result using more recent data and focusing on marriage rates (i.e., flows instead of stocks).

2.3 *Literature on the relationship between marriage and health*

[1] We do not review here studies in economics that have addressed the tax consequences of legalizing same-sex marriage (Alm et al. 2014, Fisher et al. 2016), including potentially important effects on labor supply and employment (Isaac 2017).

By examining health outcomes, our study also contributes to a large literature in economics, sociology, psychology, and demography on the effects of marriage on health. This literature is summarized elsewhere (see, for example, Ross et al. 1990, Waite and Lehrer 2003, Wood et al. 2007), but several specific findings merit mention. Marriage is the primary social relationship for half of American adults (U.S. Census Bureau 2016), and is consistently linked to lower risk of death from various causes, higher survival rates from diseases like cancer, and better physical and mental health (Aizer et al. 2013; Goodwin et al. 1987; Hu and Goldman 1990; Lillard and Waite 1995; Sorlie et al. 1995; Trovato and Lauris 1989). Conversely, mortality risks are highest for the never married (Hu and Goldman 1990; Lillard and Waite 1995; Sorlie et al. 1995), and multiple studies observe physical and mental health penalties as a result of the loss of a spouse (Lillard and Waite 1995; Kapiro et al. 1987; Stroebe and Stroebe 1987).

Certainly, not all marriages are equally beneficial. Recent studies examining the heterogeneity of effects on health across marriages find that marital quality is an important mediator of the effects of marital status on health (Williams 2003), especially in older age (Umberson et al. 2006). Marriage is also less beneficial for women compared to men in different-sex couples, and for nonwhites compared to whites (see for review Kiecolt-Glaser and Newton 2001; Ross et al. 1990). Prior to the widespread legalization of marriage for same-sex couples, the lack of legal and social recognition of same-sex couples was

associated with poorer mental health outcomes (Herdt and Kertzner 2006). At the same time, legal same-sex relationships have been shown to be associated with more positive mental health outcomes and less psychological distress for individuals in same-sex couples (Riggle et al. 2010, Wight et al. 2013).

2.4 *Mechanisms through which marriage affects health*

There are multiple direct and indirect mechanisms through which marriage has been theorized to produce individual health effects. Healthier individuals have been theorized to select into marriage and to be more likely to stay married, although prospective studies following young adults as they select into marriage find limited evidence of selection effects (e.g., Waldron et al. 1996). Marriage may also confer protective effects on health. These protective effects are theorized to operate primarily via the provision of physical, financial, and emotional support by a spouse (Ducharme 1994; Sherbourne and Hays 1990; Uchino et al. 1996). One example of tangible financial support provided through marriage is employer-sponsored health insurance, which is the predominant source of insurance coverage in the United States. Historically, most firms have not offered same-sex partners access to the same health insurance benefits as would be available to different-sex spouses of employees, though more firms increasingly were offering such benefits in recent years: 40 percent in 2017, up from 22 percent in 2008 (Claxton et al. 2017). Alternatively, drawing on the health capital framework (Grossman 1972), individuals may invest in health care

and adopt less risky behaviors and healthier lifestyles as a function of their personal investment in the marriage (Sherbourne and Hays 1990). At the dyad level, spouses may reinforce positive lifestyle behaviors such as healthy diet, exercise, and nonsmoking or engage in direct social control of a spouse's health related behaviors (Umberson 1987; 1992; Wilson 2002). In other instances, an unhealthy individual may directly and negatively affect the health of a spouse through disease transmission or, indirectly, through caregiver effects (Schulz and Beach 1999).

2.5 Literature on the effects of legal access to same-sex marriage on health and healthcare access

A small number of studies have specifically examined the effects of legal SSM on health and related outcomes using quasi-experimental variation.[2] For example, Dee (2008) examined same-sex relationship policies in Europe (mostly domestic partner registration laws, not full marriage equality) and found that they were related to reductions in sexually transmitted infections (such as syphilis) but not to changes in the prevalence of non-sexually transmitted diseases. Francis et al. (2012) find a similar but less robust result using variation from the United States on state bans on same-sex marriage. Raifman et al. (2017) find that the

[2] We do not review here a substantial literature in psychology that is primarily qualitative in nature and has examined how SSM affected mental health outcomes. We also do not review here studies in public health that have primarily leveraged cross-sectional variation in legal access to SSM at a point in time, as these studies cannot account for the other characteristics about states with legal SSM (e.g., a more progressive climate) that may independently contribute to outcomes.

availability of legal same-sex marriage in the United States is associated with significant reductions in youth suicide attempts among sexual minority students using data from the Youth Risk Behavior Surveys. Hatzenbuehler et al. (2010) used individual panel data from the National Epidemiologic Survey on Alcohol and Related Conditions spanning a period when many states implemented bans on same-sex marriage in 2004 and 2005 and found that sexual minority individuals in those states were significantly more likely to experience a range of psychiatric disorders compared to the associated change experienced by heterosexuals and compared to the associated change for sexual minority individuals in other states that did not adopt bans on same-sex marriage. Hatzenbuehler et al. (2012) use prospective data on 1,211 sexual minority men from a community health center in Massachusetts before and after the state's legalization of same-sex marriage in 2004 and found that sexual minority men had significantly decreased medical care visits, including mental health care visits and mental health care costs. Moreover, this effect was observed for both partnered and non-partnered sexual minority men. Finally, Gonzales (2015) examined how New York State's marriage equality adoption affected health insurance profiles of same-sex couples using data from the American Community Surveys. He found that legal access to SSM in New York was associated with statistically significant increases in the probability of having employer sponsored insurance for individuals in same-sex couples. This finding supported previous studies finding increases in health

insurance coverage following relationship recognition laws (i.e., domestic partnerships or civil unions) for lesbian women in California (Buchmueller and Carpenter 2012) and women in same-sex couples in the Current Population Survey (Dillender 2015).

3. Data Description and Empirical Approach

3.1 Data description

Our primary data come from the Center for Disease Control and Prevention's 2000-2016 waves of the Behavioral Risk Factor Surveillance System (BRFSS). These telephone surveys are designed to be state representative and focus on health outcomes, access to health care, and health behaviors. The annual sample size of the BRFSS is very large: in recent years it has surpassed 400,000 observations in a single year. Interviews are performed throughout the calendar year. In addition to questions about health, the survey also includes standard demographic characteristics such as age, race/ethnicity, education, and marital status.[3] Our sample includes all adults aged 25 years and older.[4]

[3] The survey was done exclusively via landlines through 2010; in 2011 a cellphone sample was added. Because the household screener with questions about the number of adult men and adult women in the household was not administered to the cellphone sample, we drop all cellphone interviews from the sample since, as we describe below, our method for identifying the target sample requires information on sex composition of adults in the household. The share of cellphone interviews increased from about 12 percent in 2011 to 45 percent in 2016. In results not reported but available upon request, we do not find statistically significant relationships between the timing of legal access to same-sex marriage in one's state and the likelihood of being in the cellphone sample from 2011-2016, suggesting that composition bias is unlikely to be a serious

For our purposes a key feature of the BRFSS is that information on the sex composition of adults in the household is ascertained during the household screening portion of the interview (which is used to determine eligibility for the rest of the interview). Specifically, one randomly selected adult in the household is asked to state the number of adult men in the household and the number of adult women in the household. We use this information to construct treatment and control groups using the simple reasoning that gay and lesbian couples are much more likely to have a household composed of exactly two same-sex adults than are heterosexual couples. SAME-SEX MALE HOUSEHOLD is an indicator variable equal to one for individuals who report their household contains exactly two adult men and exactly zero adult women; SAME-SEX FEMALE HOUSEHOLD is defined analogously. DIFFERENT-SEX HOUSEHOLD is an indicator variable equal to one for individuals who report their household contains exactly one adult man and one adult woman. A notable advantage of this indirect approach for identifying individuals who are disproportionately likely to be members of gay and lesbian couples is that respondents do not have to explicitly self-identify as a sexual minority to the interviewer. This plausibly lessens

concern. We also drop the small share of observations with missing data on demographic characteristics or outcome variables.

[4] Results adding 18-24 year olds were similar. Because the relationship between sexual orientation and our household structure measure should be weaker for younger adults (who are more likely to be students and/or co-residing with another same-sex adult for reasons other than a romantic relationship), we drop 18-24 year olds from the main sample.

concerns about gay men and lesbians shielding their sexual orientation from the survey.

Regarding marital status, the BRFSS asks respondents: "Are you…". The response options include: married; divorced; widowed; separated; never married; and member of an unmarried couple. The limitations of this question for understanding the effects of legal access to SSM have been discussed elsewhere (Carpenter and Gates 2008), but we note that the question did not explicitly ask about *legal* marital status (so couples who live as if they were married might respond 'married'). Note also that an individual could have multiple marital statuses at any one point in time (e.g., previously divorced but now married); interviewers were instructed to code the one that is accurate for the respondent that appears earliest in the list. Our primary outcome of interest is an indicator variable for being married (which we define as MARRIED).

Regarding healthcare access, we use four measures that are common in the literature. Specifically, the BRFSS asks respondents if they are covered by any kind of health insurance plan; we use this information to create an indicator variable called INSURED. We do not observe the type of plan nor in whose name the plan was secured or purchased. Individuals are also asked if they have "one person you think of as your personal doctor or health care provider"; we use this information to create an indicator variable called USUAL SOURCE OF CARE. Individuals are also asked if there was a time in the past year when they

needed to see a doctor but could not due to cost; we use this information to create an indicator variable called UNMET MEDICAL NEEDS. Finally, regarding healthcare access, individuals are asked when they last visited a doctor for a routine checkup; we use this information to create an indicator variable called HAD A PAST YEAR CHECK UP.

The BRFSS also asks respondents about their self-rated health, asking individuals the following question: "Would you say that in general your health is…". Response options include: excellent, very good, good, fair, or poor. We follow prior work by examining two outcomes: EXCELLENT OR VERY GOOD HEALTH and FAIR OR POOR HEALTH. Individuals are also asked about health-related quality of life with the following question: "Now thinking about your physical health, which includes physical illness and injury, for how many days during the past 30 days was your physical health not good?" Individuals are then asked: "Now thinking about your mental health, which includes stress, depression, and problems with emotions, for how many days during the past 30 days was your mental health not good?" Individuals are asked to state the number of days for each of these questions; we create indicator variables for AT LEAST 14 BAD PHYSICAL HEALTH DAYS IN PAST 30 (as a measure of severe physical distress) and AT LEAST 14 BAD MENTAL HEALTH DAYS IN PAST 30 (as a measure of severe mental distress). Individuals are also asked: "During the past 30 days, for about how many days did poor physical or mental health

keep you from doing your usual activities, such as self-care, work, or recreation?" We define AT LEAST 14 ACTIVITY LIMITED DAYS IN PAST 30 accordingly.[5]

We also considered a range of substance use outcomes, as there is a large body of evidence indicating that sexual minority adults have higher substance use rates than heterosexual adults, possibly as self-medication and coping responses to stress (Gonzales & Henning-Smith 2017). Specifically, we create an indicator for whether the individual reported consuming any alcohol in the past month (DRINKER), an indicator for whether the individual reported consuming five or more drinks on average for men and 4 or more drinks on average for women (BINGE DRINKER), and an indicator for whether the individual reports smoking cigarettes every day or 'some days' (SMOKER).

Finally, we considered a range of preventive care utilization measures. We examine indicators for having had a flu shot in the past year (FLU SHOT), which is ascertained for all BRFSS respondents. We also consider cancer screenings: whether adult women had a Pap test in the past year (PAP TEST), whether women age 50 and older had a mammogram in the past year

[5] We also considered as additional outcomes a range of employment and household income measures available in the demographic section of the BRFSS questionnaire, including employment, labor force participation, and household income. There were no meaningful associations between legal SSM and these outcomes. These results are available upon request.

(MAMMOGRAM) and whether men and women age 50 and older ever had a colon cancer screening (COLON CANCER SCREENING).

3.2 Empirical approach

To estimate the effect of legal access to same-sex marriage on outcomes, we begin by estimating standard two-way fixed effects models that rely on plausibly exogenous variation in the timing of legal access to SSM across states. These models are estimated on the sample of individuals in same-sex households (separately for men and women) and take the form:

$$(1) \quad Y_{ist} = \beta_0 + \beta_1 X_{ist} + \beta_2(\text{LEGAL SAME-SEX MARRIAGE})_{st} + \beta_3 Z_{st} + \beta_4 S_s + \beta_5 T_t + \beta_6 S_s * \text{TREND}_t + \varepsilon_{ist}$$

where Y_{ist} are family structure, health insurance, access to care, and health outcomes for individual *i* in state *s* at time *t*.[6] LEGAL SAME-SEX MARRIAGE$_{st}$ is an indicator variable equal to one for individuals living in states and times where same-sex couples could legally wed. Z_{st} is a vector of other state-time varying economic and demographic variables that includes: the state-month unemployment rate, the state-month population count, dummy variables for when states expanded Medicaid under the Affordable Care Act, and state income eligibility thresholds for the Children's Health Insurance Program (to proxy for public insurance program generosity in the state). Z_{st} also includes other aspects

[6] We estimate linear probability models for dichotomous outcomes for simplicity and ease of interpretation, but results are not sensitive to this choice.

of the state policy environment for LGBT individuals and couples, including: statutory bans on same-sex marriage, constitutional bans on same-sex marriage, legal access to civil unions or registered domestic partnerships, nondiscrimination protection covering sexual orientation in employment, religious freedom restoration acts, and 'bathroom bills'.[7] S_s is a vector of state dummies to control for time invariant characteristics about states; T_t is a vector of year and month dummies to control for secular national period effects.[8] We also control for state-specific linear time trends where we interact each state fixed effect with a variable TREND that equals 1 in 2000, 2 in 2001, and so forth. These trends remove variation caused by factors that are state-specific and trend linearly over time. The coefficient of interest is β_2 and in the presence of state, year, and month dummies and linear state trends is identified from within-state deviations in outcomes from a smooth linear trend coincident with variation in the timing of policy adoption across states. The key identifying assumption is that outcomes would have evolved identically in states with and without legal SSM had they not been

[7] Domestic partnership/civil union policies are coded as one of four mutually exclusive categories depending upon a given policy's combination of coverage and strength. Coverage, whether the policy applies to all couples or same-sex couples only, is coded following Dillender (2014). Strength, whether the policy extends all of the state-level rights associated with marriage or a limited set of those rights, is coded following Badgett and Herman (2011).

[8] To account for the change in the BRFSS sampling frame in 2011 we also include a dummy variable for all observations in 2011 and later.

adopted. Throughout, we use the BRFSS sampling weights, and we cluster standard errors at the state level (Bertrand, Duflo, and Mullainathan 2004).[9]

4. Results

4.1 Evidence on household structure and sexual orientation

Table 1 provides evidence that individuals in same-sex households are differentially capturing samples that are likely to include gay and lesbian couples.[10] We begin with the most direct evidence using data from the 2014-2016 BRFSS in which 31 states at some point (depending on the survey wave) released information on self-reported sexual orientation in the public use files. For these states (shown in Figure 2) we can directly correlate our household structure measures with self-reported sexual orientation to see if individuals in SSH are indeed more likely to report a non-heterosexual orientation than individuals in DSH.

Table 1 shows that this is clearly the case. The format of Table 1 is as follows: each entry in the table shows the share of the relevant sample (captured in each row) in the 2014-2016 BRFSS who reported any sexual orientation other than heterosexual (in columns 1 and 3 for women and men, respectively) or who

[9] Specifically, we follow Simon et al. (2017) in assigning sample weights by reweighting the fraction of each individual's assigned BRFSS sample weight over the sum of all weights in their survey wave.

[10] A substantial literature in demography also uses household structure to understand nonmarital and marital cohabitation. See, for example, Casper and Cohen (2000).

reported a lesbian or gay sexual orientation (in columns 2 and 4, respectively). The top row of Table 1 shows the shares for all adults in the states who released the sexual orientation information on the public use files and conforms to prior work: about 0.8 percent of women identify as lesbian, while about 2.3 percent of women identify as lesbian, bisexual, or 'other'. For men, 1.7 percent identify as gay, while about 3 percent identify as gay, bisexual, or 'other'.

In the next rows of Table 1 we separately examine individuals in different-sex households and individuals in same-sex households. Of individuals in different-sex households, we find that virtually none (0.1-0.2 percent) identifies as lesbian or gay, and only about 1.2-1.4 percent identify as anything other than heterosexual. If we further restrict attention to individuals in different-sex households who also report being married, the share identifying as anything other than heterosexual falls even further. In contrast, for individuals in same-sex households, Table 1 indicates that 11 percent of women and 27.8 percent of men in those household configurations identify as something other than heterosexual. Put differently, more than one in ten women in same-sex households and more than one in four men in same-sex households are likely not heterosexual (and also likely in a same-sex relationship). This means that for our reduced form models below, estimates of the association between legal access to same-sex marriage and outcomes for women (men) in same-sex households should be scaled up by approximately a factor of ten (four). When we further examine individuals in

same-sex households who also report being married, the share choosing a non-heterosexual identity jumps even more: 46.4 percent for women in same-sex households and 59.7 percent for men in same-sex households. Moreover, to the extent that there is underreporting of non-heterosexual orientations in the BRFSS due to stigma or other reasons, this is likely an underestimate of the amount of signal in these samples.

Thus, Table 1 provides strong evidence that household structure conveys meaningful information about a correlate of sexual orientation. These patterns motivate our use of household structure for defining treated and falsification samples for our difference-in-differences models below.[11]

4.2 Descriptive statistics

Having documented a meaningful relationship between household structure and sexual orientation, we next present descriptive statistics for the individual demographic characteristics, legal SSM variable, and health-related outcomes. Figure 3 shows trends in the share of men and women in SSH who report being married over our sample period. Several patterns are notable. First, the share of

[11] In additional results not reported here but available upon request, we also found several other less direct patterns that indicated that household structure is correlated in predictable ways with what one would expect regarding different reproductive and sexual health behaviors for individuals of different sexual orientations in the BRFSS. For example, women in same-sex households are less likely to report being currently pregnant than women in different-sex households. Men in same-sex households are more likely to report having ever been tested for HIV than men in different-sex households. And both men and women in same-sex households are less likely to have children under 18 in the household than men and women in different-sex households. These same patterns have been documented previously in other work using BRFSS household structure in this way (see, for example, Carpenter 2004).

men in SSH who report being married is higher than the associated share of women in SSH, consistent with the patterns in Table 1 that there is more slippage between sexual orientation and household structure in our measure for women. Second, both shares increase over the sample period, with the share of men in SSH who report being married increasing from about 10 percent to 20 percent and the share of women in SSH who report being married increasing from about 5 percent to 10 percent.

Table 2 presents descriptive statistics for key variables for women in same-sex households (column 1), women in different-sex households (column 2), men in same-sex households (column 3), and men in different-sex households (column 4). Approximately 10 percent of the sample lives in a state with legal access to same-sex marriage over the study period. A much larger share of men and women in different-sex households reports being married than men and women in same-sex households. Regarding health care and health, Table 2 shows that men and women in same-sex households are significantly less likely to be insured, less likely to have a usual source of care, and more likely to report unmet medical care needs due to cost compared to men and women in different-sex households. Men and women in same-sex households are also significantly less likely to report excellent or very good health, more likely to report fair or poor health, and more likely to report 14 or more bad health days than men and women in different sex households. Men and women in same-sex households are also

significantly more likely to report substance use (e.g., smoking and binge drinking) than men and women in different-sex households. Finally, men and women in same-sex households report significantly less preventive care (e.g., flu shots, Pap tests, mammograms, and colon cancer screenings) than men and women in different-sex households.

4.3 Effects of legal same-sex marriage on household composition

Before presenting estimates of the relationship between legal access to same-sex marriage and marriage take-up, we begin in Table 3 with an analysis of how the marriage equality variation relates to various measures of household composition. This analysis is important for interpreting any effects we observe on marriage and health-related outcomes because our empirical strategy relies on treatment and comparison groups defined by household structure and household sex composition. If these variables themselves were meaningfully related to the variation across space and time in legal access to same-sex marriage, composition bias would be a concern.

Table 3 shows that this is unlikely to be the case, particularly for men. Specifically, in Table 3 we report the coefficient on the LEGAL ACCESS TO SAME-SEX MARRIAGE indicator variable from a fully saturated model of equation (1) that includes all the individual level controls; state and time varying characteristics and policies; state, month, and year fixed effects; and linear state-specific time trends. We consider four measures of household composition in

Table 3: the total number of adults the respondent reports in the household in column 1; an indicator variable for whether the respondent reports there are exactly two adults in the household in column 2; an indicator variable for whether the respondent reports there are exactly two different-sex adults in the household in column 3; and an indicator variable for whether the respondent reports there are exactly two same-sex adults in the household in column 4.

The overall takeaway from Table 3 is that the relationship between legal access to same-sex marriage and all of these outcomes is very small in magnitude both absolutely and relative to the pre-reform means of the variables. Only one of the eight estimates is significant (the probability a woman lives in a household with two same-sex adults), and that effect size is 4.3 percent of the pre-reform mean. This could be consistent with an increase in partnership and cohabitation behavior among sexual minority women, though again the effects here are very small. For men we find no evidence of possible composition bias related to the timing of legal access to same-sex marriage in a respondent's state.[12]

[12] Appendix Table 1 makes a related point and shows the relationship between average demographic characteristics of the individuals in same-sex households as a function of legal access to same-sex marriage (and all the other controls in equation (1)). Appendix Table 1 reveals little evidence of systematic selection for individuals in same-sex households when legal same-sex marriage is in an individual's state: the estimated changes in respondent characteristics associated with legal same-sex marriage are generally small and insignificant, though there is some evidence that the sample of individuals in same-sex households has lower education levels after legal same-sex marriage in the respondent's state. This pattern and the well documented fact that higher educated people are more likely to have good jobs that offer health insurance suggests that the estimated improvements in health insurance, access to care, and utilization we document below for men in same-sex households associated with the timing of legal same-sex marriage are unlikely to be driven by the changing characteristics of men in same-sex couples.

4.4 Effects of legal same-sex marriage on marriage take-up

Table 4 provides direct evidence on the relationship between legal access to same-sex marriage and marriage take-up among individuals in same-sex households. The format of this table is as follows: each entry in the table is a coefficient on the LEGAL ACCESS TO SAME-SEX MARRIAGE indicator from a fully saturated regression model that includes the individual demographics; state characteristics; other state LGBT policy variables; state, month, and year fixed effects; and linear state-specific time trends. Column 1 reports results for women, while column 2 reports results for men. The top panel reports results for the sample of individuals in same-sex households, our primary treatment group. The bottom panel reports results for a comparison sample that should be largely unaffected by legal access to same-sex marriage: individuals in different-sex households.

The results in Table 4 return strong evidence that legal access to SSM was associated with statistically and economically significant increases in marriage take-up among individuals in same-sex households. For women in the top panel of column 1, for example, we estimate that legal SSM was associated with a significant 3.7 percentage point increase in the likelihood a woman in a SSH reported being married. Relative to a pre-reform mean of 4.7 percent, this is a 78.7 percent proportional effect. Recall that approximately 10 percent of women in same-sex households would identify as something other than heterosexual; thus, the treatment effect on the treated sample of non-heterosexual women is

closer to a 37 percentage point increase in marriage take-up. Measured another way, over our sample period the share of women in SSH who reported being married increased by about five percentage points (see Figure 3); our estimate in the top panel of column 1 of Table 4 suggests that legal access to same-sex marriage can explain about 74 percent of that increase. The bottom panel of column 1 of Table 4 confirms that the marriage effects we estimate in the top panel are unique to women in same-sex households; there is no meaningful relationship between legal access to same-sex marriage and being married for women in different-sex households.

The results for men in column 2 of Table 4 are similar to the results for women. Specifically, we estimate that legal access to same-sex marriage is associated with a 5.7 percentage point increase in the likelihood that a man in a same-sex household reports being married (top row, column 2). This result is statistically significant and is large relative to the pre-reform mean of 13.6 percent. Given that Table 1 showed that about 27 percent of men in same-sex households would identify as something other than heterosexual, the treatment effect on sexual minority men is closer to a 21 percentage point increase in marriage take-up. Measured another way, over the sample period the share of men in SSH who reported being married increased by about ten percentage points; our estimate in the top panel of column 2 of Table 4 can explain well over half of this overall increase. The bottom panel of column 2 of Table 4 confirms that the

marriage effects we observe in the top row are uniquely observed for men in same-sex households.[13]

We explore the robustness of the main findings of Table 4 in a variety of ways. First, in Figures 4 and 5 for women and men, respectively, we show graphically the estimates from an event study model in which we replace the single access to legal SSM indicator with a series of event time indicators representing time relative to adoption of legal SSM in the individual's state (we observe exact interview date in the BRFSS, so policy timing can be assigned with high accuracy).[14] The results in Figures 4 and 5 clearly demonstrate that increases in marriage among individuals in same-sex households follow access to legal same-sex marriage in one's state. Importantly, there is no evidence of systematic differential trends in marriage in the period leading up to legal access to same-sex

[13] Appendix Table 2 provides an expanded set of coefficient estimates corresponding to the top panel of Table 4. Most other LGBT-related public policies are not meaningfully related to the likelihood individuals in same-sex households report being married. In Appendix Table 3 we report marriage effects by demographic group to examine descriptively the heterogeneity in marriage take-up. We also present the share of individuals in same-sex households in each of the relevant demographic groups who report a non-heterosexual orientation (again extrapolating from the 2014-2016 BRFSS data). The results in Appendix Table 3 indicate that legal access to same-sex marriage had broad-based effects at increasing marriage. For both men and women we find meaningful (but not always statistically significant) estimated increases associated with legal access to same-sex marriage for both young and older adults (columns 1 and 2), white and nonwhite adults (columns 3 and 4), less educated and more educated adults (columns 5 and 6), and individuals in households with and without children present (columns 7 and 8). We consistently estimate that the marriage take-up effect is larger for women in SSH without children present, which is unexpected since the tangible returns to marriage are likely larger for households with children. One possible explanation is that women in SSH with children are more likely to have crossed state lines to get a marriage license (precisely because the returns to doing so are nontrivial); this would cause the legal access variable to be mismeasured for women in SSH which could bias the estimate toward zero.

[14] A limitation of this approach is that we do not observe cross state migration, so we cannot know if the person lived in that same state many years before the interview.

marriage, which is indirect evidence in support of the parallel trends assumption required for identification. This is perhaps not surprising given that much of the policy variation was driven by relatively unexpected judicial decisions.

We report a series of other robustness analyses in Table 5. The format of Table 5 is as follows: each entry is the coefficient on legal access to SSM from a separate regression that includes all the individual and state controls as well as the state, month, and year fixed effects and linear state trends (unless otherwise noted). We report results for women in SSH in the top panel and for men in SSH in the bottom panel. Each column shows the results of a different robustness analysis relative to the baseline model for marriage from the top panel of columns 1 and 2 of Table 4; we reprint those baseline estimates in column 1 of Table 5. In column 2 of Table 5 we report estimates from a model that excludes the linear state trends but retains all other controls, including the state and time fixed effects. Those models return notably larger estimated effects of access to legal same-sex marriage on the likelihood individuals in same-sex households report being married, suggesting that our models with linear state trends are conservative estimates of the effects of legal access to SSM on outcomes. In column 3 we revert to the baseline model with linear state trends and show estimates from models that recode California as turned 'off' (i.e., not having legal access to same-sex marriage) until the *U.S. v. Windsor* decision in June 2013. Our baseline model codes California as 'on' in the summer of 2008 for the short period when

same-sex marriage was legal in the state before a statewide vote in 'Proposition 8' ended same-sex marriages. In column 4 we show estimates where we drop California entirely from the estimation. Both estimates from columns 3 and 4 of Table 5 show that our core results on marriage take-up are not sensitive to the treatment of California (and in fact are even larger when we exclude the state entirely). In column 5 we show estimates from models that exclude the BRFSS sample weights; results are robust to estimating unweighted models. In column 6 we report results where we add 18-24 year olds back to the full sample; estimates continue to suggest significant effects of legal access to same-sex marriage on marriage take-up. Finally, column 7 of Table 5 shows results from a model where we drop anyone who – when asked about their employment status – responded that they are a student. Excluding students returns similar estimates of the effect of legal access to same-sex marriage on the likelihood individuals in same-sex households report being married, which is perhaps not surprising since our main sample selection criteria exclude 18-24 year olds.

4.5 Effects of legal same-sex marriage on health-related outcomes

Having documented that legal access to same-sex marriage significantly increased marriage among women and men in same-sex households, we now turn to examining effects on health-related outcomes. We present these health-related outcomes in Table 6. The format of Table 6 is as follows: each entry is the coefficient on LEGAL ACCESS TO SAME-SEX MARRIAGE from a fully

saturated regression (including linear state trends). We present estimates for women in same-sex households in column 1 and for men in same-sex households in column 2. Each row is a different outcome variable, and the relevant sample sizes for men and women are reported after each outcome. We also reprint the marriage take-up estimate in the top row of Table 6 for reference. We separate the health-related outcomes into the various categories: insurance and access to care; self-rated health and bad health days; substance use; and preventive care.

The results in Table 6 provide little evidence that legal access to same-sex marriage had meaningful effects on health-related outcomes for women in same-sex households, despite the clear evidence in the prior tables that the policy increased marriage take-up for this sample. For health outcomes in column 1 of Table 6 few estimates are statistically significant, and most are small in magnitude. The handful of estimates that are statistically significant are as likely to indicate that legal same-sex marriage was associated with *worse* outcomes (more smoking, less flu shots) for women in SSH as they are to indicate that legal same-sex marriage was associated with better outcomes (lower probability of reporting 14 or more bad mental health days, lower probability of reporting 14 or more activity limited days) in this sample.

For men in same-sex households, in contrast, we estimate that legal access to same-sex marriage was associated with statistically significant improvements in insurance coverage, access to care, and utilization. Specifically, we estimate in

column 2 of Table 6 that legal access to same-sex marriage increased the probability a man in a same-sex household reported being insured by 4.2 percentage points. Relative to the pre-reform mean, this is a 5.4 percent effect (compared to the 42 percent marriage take-up effect in the top row of column 2 of Table 6). Similarly, we estimate that legal access to same-sex marriage significantly increased the likelihood a man in a same-sex household reported a usual source of care by 4.2 percentage points and had a checkup in the past year by 7.3 percentage points (6 and 12 percent effects, respectively).[15] Turning to health outcomes, we find no relationship between legal access to same-sex marriage and self-rated health, substance use, or preventive care take-up for men in same-sex households. Thus, we find consistent evidence for men in same-sex households that legal access to same-sex marriage not only increased marriage take-up but also significantly increased health insurance coverage, access to healthcare, and utilization of checkups.[16]

[15] In results not reported but available upon request, we found that restricting attention to adults age 65 or older – the vast majority of whom are eligible for Medicare – continued to return strong evidence that men in same-sex households were significantly more likely to report being married when legal same-sex marriage was available, but they were not significantly more likely to report being insured, having a usual doctor, or having had a past year checkup when legal same-sex marriage was available. This suggest that the effects of legal same-sex marriage on having a usual doctor and on past year checkups for men in same-sex households in the full sample is most likely due to the price effect coming through increased health insurance as opposed to other effects of marriage (e.g., the 'nagging spouse' effect).

[16] Appendix Table 4 shows the results from the same robustness exercises as in Table 5 on the outcomes indicating a significant relationship with legal access to same-sex marriage for men in same-sex households (insurance, usual source of care, and checkup in past year) and shows that they are largely robust. Appendix Figures 1-3 also show event study figures for those same outcomes. Although we do not find evidence of systematic pre-trends in outcomes prior to legal same-sex marriage in the respondent's state, few of the 'years since legal same-sex marriage'

5. Discussion and Conclusion

The results above provide novel evidence on how legal access to SSM is associated with marriage take-up and health-related outcomes for adults in same-sex households. We show that a nontrivial share of these individuals are likely to be sexual minorities. We consistently estimate that legal access to SSM is associated with statistically significant increases in the likelihood that men and women in same-sex households report being married. These increases in marriage were also associated with significant increases in the likelihood of reporting health insurance coverage and various measures of access to healthcare such as having a usual source of care for men in same-sex households. These effects were robust to numerous sample and specification checks, including event study estimation. Notably, we did not find that legal access to same-sex marriage significantly affected substance use, self-rated health, or preventive care for

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event time dummies are individually statistically significant. In results not reported, we also considered other robustness tests to further probe the result that legal access to same-sex marriage increased health insurance, access to care, and utilization for men in same-sex households. For example, we estimated difference-in-differences models restricting attention only to men who reported being married or a member of an unmarried couple. This approach assumes legal access to same-sex marriage had no effects on partnership, but an advantage is that we are more confident that men in same-sex households who report being married or a member of an unmarried couple are sexual minorities (see Table 1). Results from that sample continued to suggest that legal access to same-sex marriage significantly increased marriage, insurance, access to care, and utilization. We also estimated a fully interacted triple differences model where we included individuals in different-sex households and examined the coefficient on the interaction between being in a same-sex household and having legal access to same-sex marriage, controlling for a full set of state by same-sex household, year by same-sex household, and state by year fixed effects. These models also returned evidence of significant increases in marriage, health insurance, access to care, and utilization for men in same-sex households associated with legal access to same-sex marriage. These results are available upon request.

individuals in same-sex households. One possibility is that it will take more time and experience with legal same-sex marriage to see improvements in these health outcomes.

We consistently estimate that legal access to same-sex marriage did not systematically improve healthcare access and health-related outcomes for women in same-sex households. The null finding for women in same-sex households for health insurance contrasts with results from Buchmueller and Carpenter (2012) and Dillender (2015) who found that lesbians in same-sex relationships were significantly more likely to be insured when states extended marriage-like status to same-sex couples compared to the associated change for heterosexual women in different-sex relationships. Why might this be the case? There are several possibilities. One issue may be related to how migration and state of residence are measured in the BRFSS. If lesbian couples were more likely to travel across state borders to obtain a marriage license than gay men, this would dampen the apparent association between legal access to marriage in the observed state of residence and both marriage and health-related outcomes for women in SSH. We know from other work that lesbian households are much more likely to have children in the household which could have increased the incentive for legal marriage even if it were not available in one's home state. Because we lack data on prior residence and location of marriage, we cannot test this hypothesis directly, but it is an important question for future work. Also, our measurement

of same-sex households was a much better indicator for men in same-sex households versus women in same-sex households.

Overall our results are the first to span the entire rollout of legal access to same-sex marriage in the United States. We offer the most comprehensive quasi-experimental evaluation of legal access to same-sex marriage on adult outcomes, and our findings suggest that access to marriage benefited the health of some sexual minorities. Recent efforts to dismantle legal access to same-sex marriage may negatively affect family and health outcomes, particularly among men in same-sex households. Ongoing research on same-sex marriage should continue to use innovative techniques and data sources collecting sexual orientation to monitor the impacts of same-sex marriage on various economic, health, and demographic outcomes.

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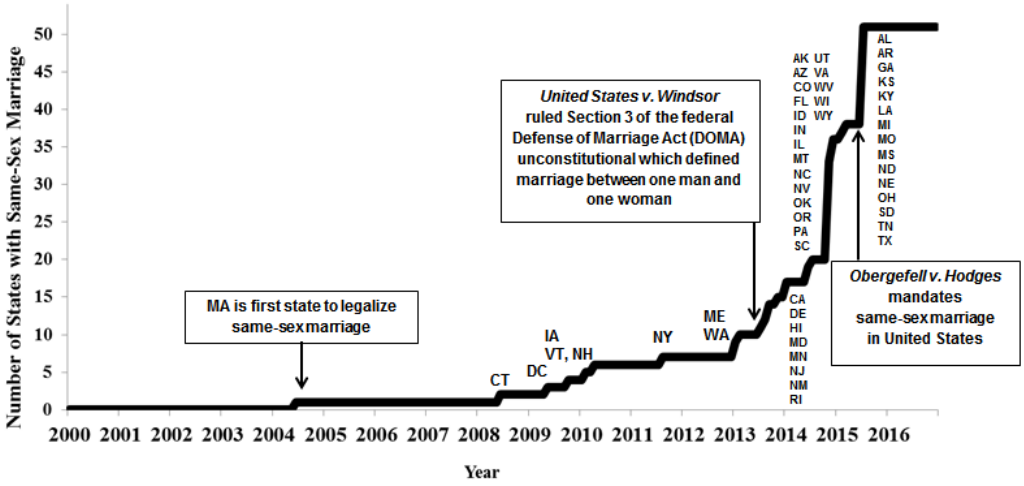
Figure 1
Timeline of Legal Access to Same-Sex Marriage in the United States



Source: National Conference of State Legislatures, the Human Rights Campaign and various news sources.

Figure 2
States that Released Sexual Orientation Data to the BRFSS
2014-2016

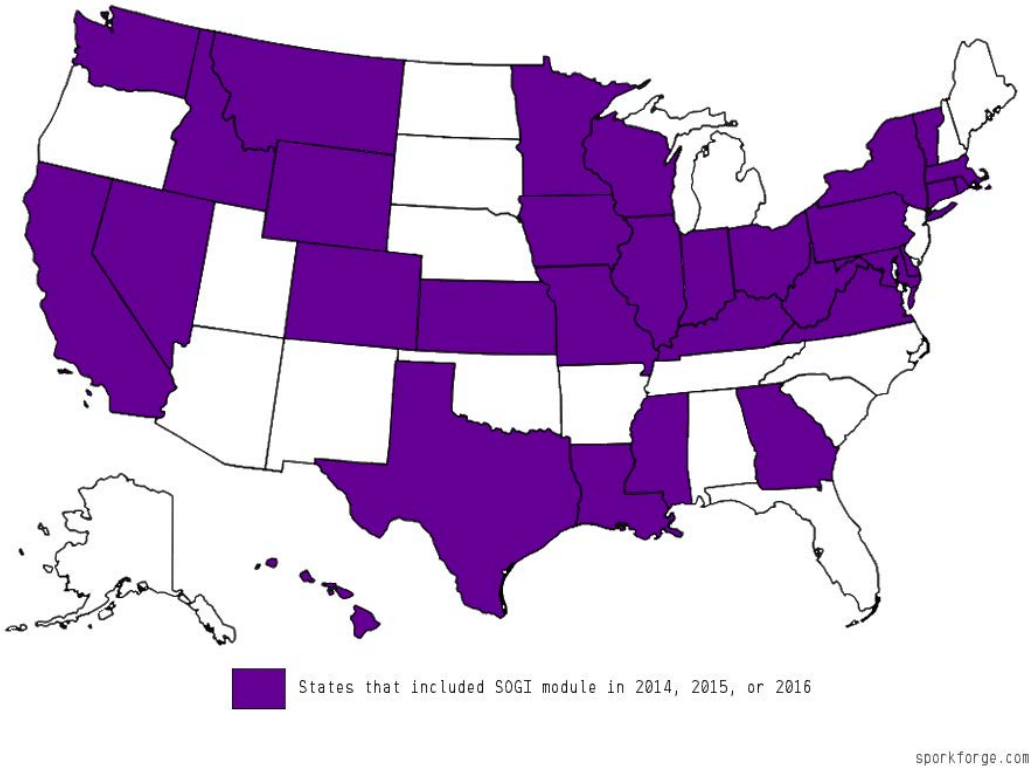


Figure 3
Trends in Share Married Among Individuals in Same-Sex Households
2000-2016 BRFSS, Adults age 25+ in Same-Sex Households

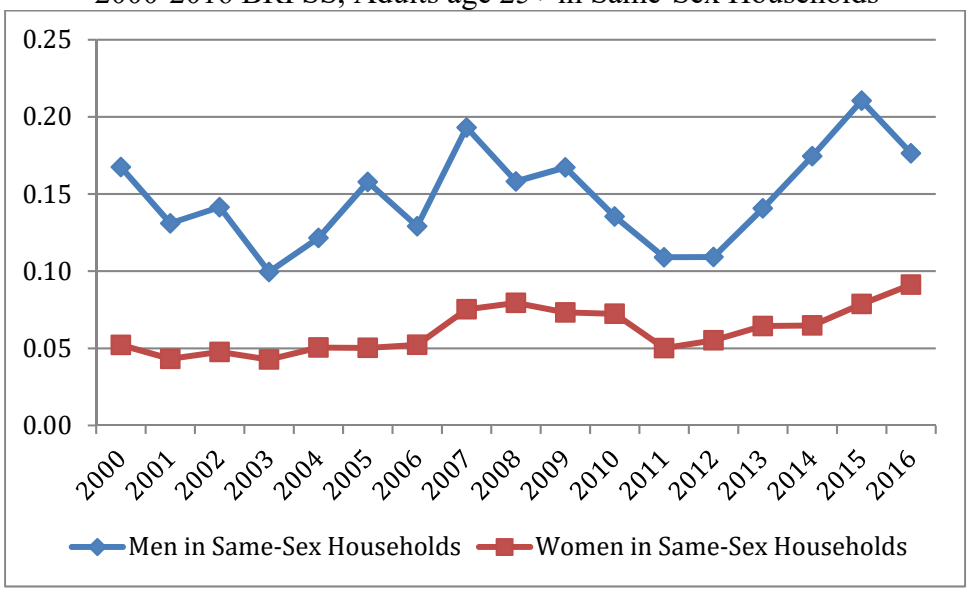


Figure 4

Event Study: Access to Legal Same-Sex Marriage and Marriage Take-up, Women
2000-2016 BRFSS Data, Women age 25+ in Same-Sex Households

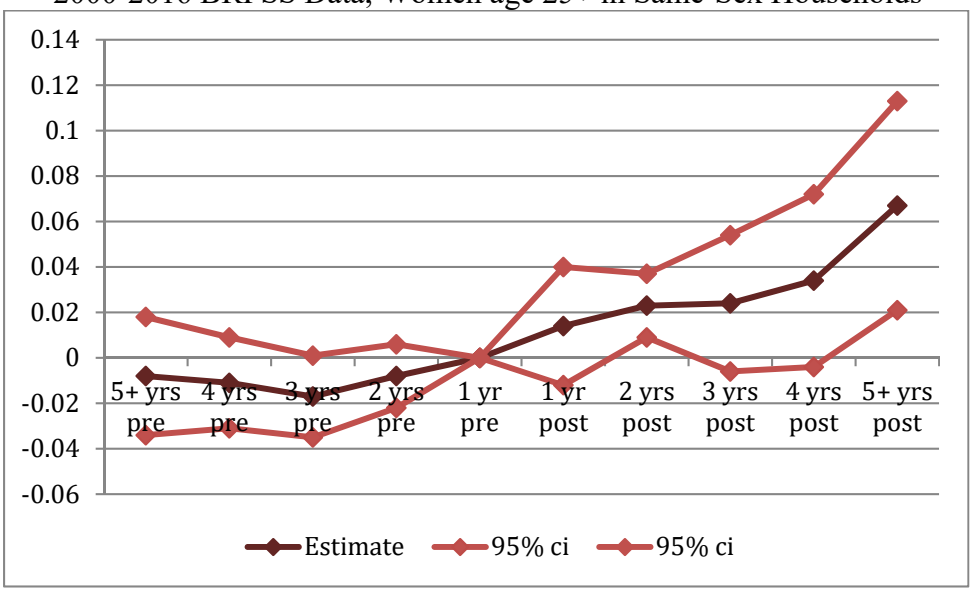


Figure 5

Event Study: Access to Legal Same-Sex Marriage and Marriage Take-up, Men
2000-2016 BRFSS Data, Men age 25+ in Same-Sex Households

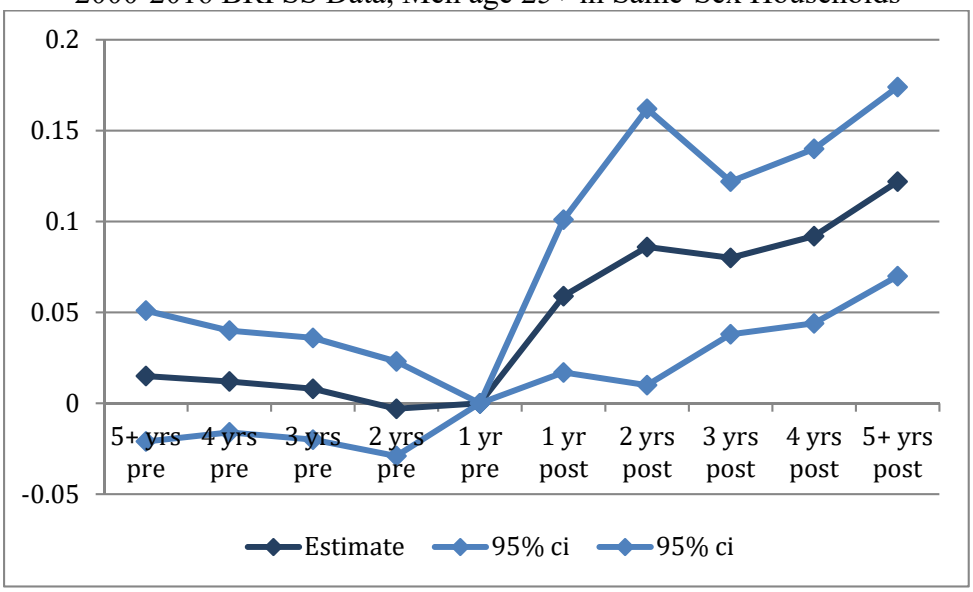


Table 1: Direct evidence on household structure proxies and self-reported sexual orientation

BRFSS states that asked the SOGI module in 2014, 2015, or 2016; Adults age 25+

	(1)	(2)	(3)	(4)
	Women	Women	Men	Men
Outcome is ➔	Not heterosexual (lesbian, bisexual, or 'other')	Lesbian	Not heterosexual (gay, bisexual, or 'other')	Gay
Sample is ↓				
All respondents in states and years when the SOGI module was asked (31 states from 2014-2016) [N=338,613]	.023 (.150)	.008 (.087)	.030 (.170)	.017 (.128)
All respondents reporting exactly one man and one woman in the HH [47% of top row sample]	.014 (.117)	.001 (.036)	.012 (.108)	.002 (.048)
All respondents reporting exactly one man and one woman in the HH and who report being married [42 % of top row sample]	.011 (.103)	.001 (.024)	.010 (.099)	.001 (.031)
All respondents reporting exactly two same-sex adults (and zero different-sex adults) in the HH [i.e., two adult women in cols 1 & 2; two adult men in cols 3 &4] [3% of top row sample]	.110 (.313)	.082 (.275)	.278 (.448)	.248 (.432)
All respondents reporting exactly two same-sex adults (and zero different-sex adults) in the HH and who report being married [0.4% of top row sample]	.464 (.499)	.433 (.496)	.597 (.491)	.576 (.495)

Notes: Author calculations, 2014-2016 BRFSS landline respondents.

Table 2: Sample Characteristics
2000-2016 BRFSS Data, Adults age 25+

Variables	(1) Women in SSH	(2) Women in DSH	(3) Men in SSH	(4) Men in DSH
Same-sex marriage legal	.118* (.305)	.100* (.302)	.095* (.264)	.089* (.286)
Married	.060* (.225)	.869* (.339)	.146* (.318)	.881* (.325)
Member of an unmarried couple	.056* (.218)	.031* (.173)	.128* (.302)	.031* (.174)
Age < 50	.454* (.471)	.538* (.501)	.604* (.441)	.534* (.501)
High school degree or less	.423* (.467)	.367* (.484)	.388* (.440)	.364* (.483)
BA or more	.290* (.429)	.355* (.481)	.354* (.431)	.392* (.491)
White	.608* (.461)	.791* (.409)	.687* (.418)	.776* (.419)
Has a health plan	.841* (.346)	.903* (.297)	.793* (.365)	.897* (.305)
Has a usual source of care	.855* (.333)	.891* (.314)	.731* (.401)	.815* (.390)
Had a past year checkup	.752 (.407)	.754 (.433)	.640* (.434)	.666* (.474)
Unmet medical care due to cost in past year	.180* (.363)	.111* (.315)	.149* (.322)	.083* (.277)
Excellent or very good health	.434* (.469)	.574* (.497)	.510* (.451)	.555* (.499)
Fair or poor health	.241* (.404)	.143* (.351)	.186* (.351)	.143* (.352)
At least 14 bad physical health days in past 30	.166* (.351)	.110* (.314)	.122* (.295)	.094* (.293)
At least 14 bad mental health days in past 30	.154* (.342)	.099* (.300)	.124* (.297)	.067* (.252)
At least 14 activity limited days in past 30	.183* (.369)	.119* (.325)	.163* (.335)	.133* (.341)
Current smoker	.222* (.393)	.150* (.359)	.305* (.415)	.177* (.384)
Past month drinker	.417* (.466)	.598* (.502)	.626 (.437)	.630 (.485)
Past month binge drinker	.094* (.273)	.060* (.238)	.163* (.329)	.103* (.306)
Had a flu shot in past year	.383* (.460)	.390* (.490)	.334* (.426)	.368* (.484)
Had a Pap test in past year	.521* (.473)	.614* (.489)	--	--
Had a mammogram in the past two years, among 50+	.744* (.400)	.824* (.385)	--	--
Ever had a colon cancer screening, among 50+	.569* (.453)	.639* (.484)	.557* (.458)	.646* (.480)
N	124,219	1,514,900	48,545	1,149,147

Notes: Weighted means (standard deviations). * indicates the difference in means between column 1 vs. column 2 or column 3 vs. column 4 is significant at p<.05. Sample size varies across outcomes because not all questions were asked in each survey year; the reported sample size is the largest one used for any of the descriptive statistics.

Table 3: Legal Access to Same-Sex Marriage Is Largely Unrelated to Composition of Treatment & Comparison Groups

BRFSS 2000-2016, Adults age 25+

Coefficient on Access to Legal Same-Sex Marriage, Fully Saturated Model

Outcome is ➔	(1) Total # of adults in the HH	(2) Household has exactly two adults	(3) Household has two different-sex adults	(4) Household has two same-sex adults
Women				
Pre-reform mean of the outcome	2.070	.567	.521	.046
Access to Legal Same-Sex Marriage	.012	-.008	-.010	.002*
	(.013)	(.006)	(.006)	(.001)
R-squared	.101	.066	.080	.009
N	3,293,318	3,293,318	3,293,247	3,293,247
Men				
Pre-reform mean of the outcome	2.181	.625	.590	.034
Access to Legal Same-Sex Marriage	.017	.005	.004	.001
	(.015)	(.005)	(.004)	(.001)
R-squared	.081	.052	.054	.004
N	2,006,858	2,006,858	2,006,807	2,006,807

Notes: † significant at 10%; * significant at 5%. Each entry is the coefficient on Access to Legal Same-Sex Marriage from a separate regression. All models control for: dummy variables for 5 year age ranges, dummy variables for education (less than high school, some college, college or more, and refusal to provide educational attainment), dummy variables for race/ethnicity (black, Asian, Native American, Hispanic, other race, and race not ascertained), the state unemployment rate, state population, ACA Medicaid expansions, and CHIP income eligibility thresholds. Models also include state, month, and year fixed effects as well as linear state-specific time trends and a dummy variable equal to 1 for individuals interviewed after the 2011 changes in BRFSS methodology. Results use the BRFSS sampling weights as described in the text. Standard errors are clustered at the state level.

Table 4: Legal Access to Same-Sex Marriage Significantly Increased Marriage Take-Up Among Individuals in Same-Sex Households

Outcome is: Married

BRFSS 2000-2016, Adults age 25+

Coefficient on Access to Legal Same-Sex Marriage, Fully Saturated Model

	(1) Women	(2) Men
Share of individuals in SSH in 2014-2016 BRFSS who identify as something other than heterosexual [Henceforth, non-heterosexual share in SSH] ➔	.110	.278
Individuals in same-sex HHs		
Pre-reform mean	.047	.136
Access to Legal Same-Sex Marriage	.037*	.057*
	(.015)	(.028)
R-squared	.025	.061
N	122,902	48,138
Individuals in different-sex HHs		
Pre-reform mean	.868	.880
Access to Legal Same-Sex Marriage	-.002	.005
	(.003)	(.003)
R-squared	.058	.074
N	1,508,557	1,144,768

Notes: * significant at 5%. See notes to Table 3.

Table 5: Effects on Marriage Take-Up Are Robust
Sample is: Individuals in Same-Sex Households; Outcome is: Married
BRFSS 2000-2016, Adults age 25+
Coefficient on Access to Legal Same-Sex Marriage, Fully Saturated Model

	(1) Baseline model (from Table 3)	(2) (1), without linear state trends	(3) (1), code CA as off until *US. v. Windsor*	(4) (1), drop CA entirely	(5) (1), without sample weights	(6) (1), with 18-24 year olds included	(7) (1), without students
Non-heterosexual share in SSH ➔	.110	.110	.110	.111	.110	.114	.110
Households with two adult women							
Access to Legal Same-Sex Marriage	.037*	.041**	.047**	.041**	.038*	.033*	.037*
	(.015)	(.015)	(.012)	(.012)	(.015)	(.013)	(.015)
R-squared	.025	.023	.025	.027	.031	.024	.025
N	122,902	122,902	122,902	119,399	122,902	136,557	121,109
Non-heterosexual share in SSH ➔	.278	.278	.278	.270	.278	.261	.278
Households with two adult men							
Access to Legal Same-Sex Marriage	.057*	.062**	.077**	.068**	.048*	.055*	.056*
	(.028)	(.023)	(.019)	(.022)	(.022)	(.026)	(.028)
R-squared	.061	.055	.061	.066	.051	.077	.062
N	48,138	48,138	48,138	46,510	48,138	55,055	47,435

Notes: * significant at 5%; ** significant at 1%. See notes to Table 3.

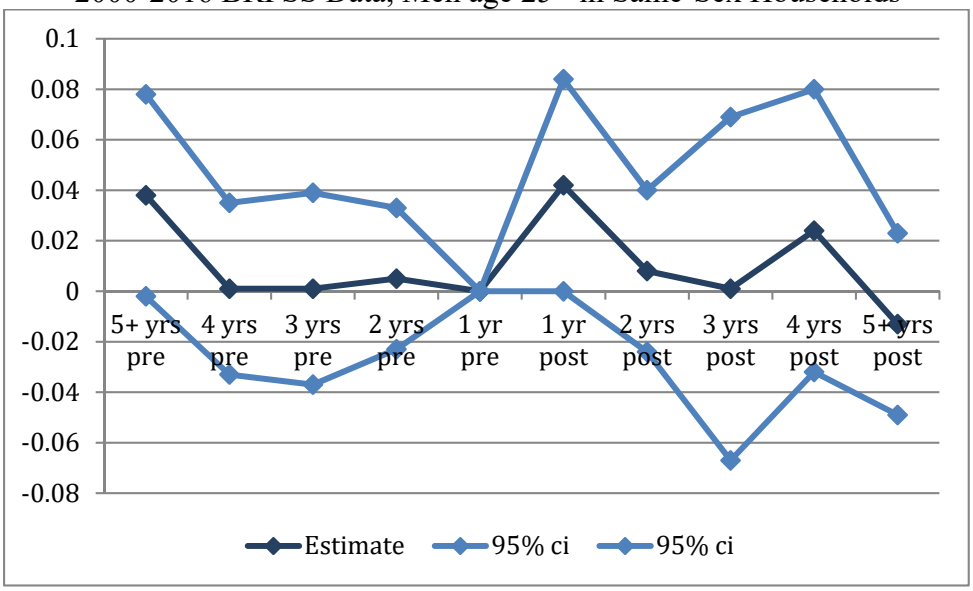
Table 6: Effects of Legal Access to Same-Sex Marriage on Access to Care and Health
Sample is Individuals in Same-Sex Households
BRFSS 2000-2016, Adults age 25+
Coefficient on Access to Legal Same-Sex Marriage, Fully Saturated Model

	(1) Women in SSH	(2) Men in SSH
Married (N_w=122,902; N_m=48,138)	.037* (.015)	.057* (.028)
Insurance and Access to Care		
Insured (N_w=123,677; N_m=48,299)	-.020 (.026)	.042** (.016)
Has a usual source of care (N_w=120,477; N_m=46,491)	.002 (.006)	.042* (.017)
Had a checkup in the past year (N_w=103,227; N_m=38,822)	-.004 (.012)	.073** (.016)
Unmet medical needs in past year (N_w=119,996; N_m=46,278)	.026 (.020)	.040 (.028)
Self-rated health and bad health days		
Excellent or very good health (N_w=123,950; N_m=48,433)	-.026 (.016)	.050 (.054)
Fair or poor health (N_w=123,950; N_m=48,433)	-.004 (.011)	.002 (.018)
14+ days bad physical health in past month (N_w=118,085; N_m=46,035)	.007 (.009)	.019 (.017)
14+ days bad mental health in past month (N_w=118,801; N_m=46,080)	-.019* (.008)	.023 (.017)
14+ days activity limited days in past month (N_w=71,884; N_m=24,207)	-.029* (.012)	.042 (.031)
Substance use		
Drank alcohol in past month (N_w=118,149; N_m=45,709)	-.008 (.007)	-.002 (.010)
Binged in past two weeks (N_w=46,352; N_m=26,290)	-.014 (.010)	-.026 (.053)
Smoked cigarettes in past month (N_w=122,389; N_m=47,864)	.024** (.007)	-.022 (.034)
Preventive care		
Had a flu shot in past year (N_w=109,295; N_m=42,634)	-.022* (.009)	.029 (.027)
Had a Pap test in past year (N_w=68,359)	-.035 (.033)	--
Had a mammogram in the past two years, among 50+ (N_w=43,601)	-.005 (.015)	--
Ever had a colon cancer screening, among 50+ (N_w=43,296; N_m=14,596)	-.011 (.017)	.010 (.027)

Notes: * significant at 5%; ** significant at 1%. See notes to Table 3. Sample sizes vary across the outcomes, as not all outcomes were asked in every year. N_w (N_m) indicates the sample size for the women (men) in same-sex households.

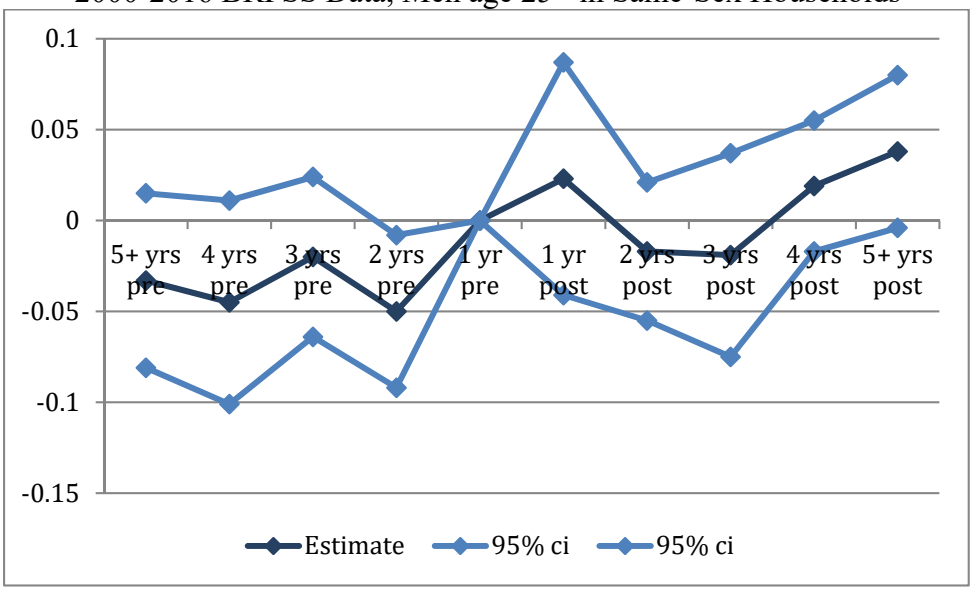
Appendix Figure 1

Event Study: Access to Legal Same-Sex Marriage and Insurance Coverage
2000-2016 BRFSS Data, Men age 25+ in Same-Sex Households



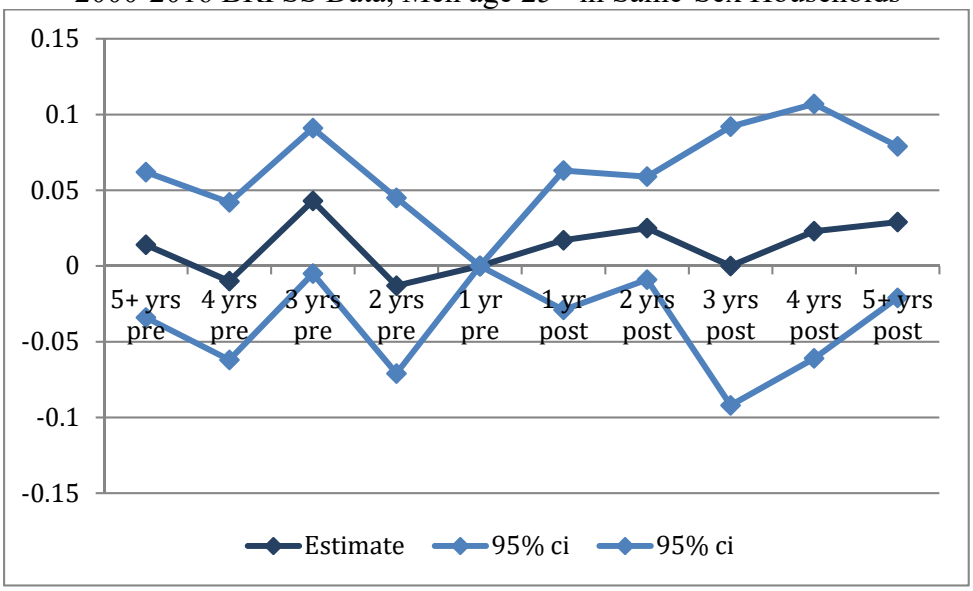
Appendix Figure 2

Event Study: Access to Legal Same-Sex Marriage and Usual Source of Care
2000-2016 BRFSS Data, Men age 25+ in Same-Sex Households



Appendix Figure 3

Event Study: Access to Legal Same-Sex Marriage and Past Year Checkup
2000-2016 BRFSS Data, Men age 25+ in Same-Sex Households



Appendix Table 1: Legal Access to Same-Sex Marriage and Demographics of Individuals in Same-Sex Households

BRFSS 2000-2016, Adults age 25+

Coefficient on Access to Legal Same-Sex Marriage, Fully Saturated Model

	(1) Under 50	(2) White	(3) High school or Less	(4) College graduate	(5) Any kids <18 in Household
Households with two adult women					
Pre-reform mean	.518	.624	.442	.293	.347
Access to Legal Same-Sex Marriage	.019	.004	.039**	-.005	.022*
	(.012)	(.008)	(.013)	(.011)	(.009)
R-squared	.071	.148	.086	.072	.195
N	122,489	122,668	123,572	123,572	123,646
Households with two adult men					
Pre-reform mean	.708	.697	.369	.363	.146
Access to Legal Same-Sex Marriage	.011	-.015	.050*	-.054*	.007
	(.016)	(.023)	(.022)	(.023)	(.011)
R-squared	.093	.112	.081	.079	.066
N	48,114	47,843	48,289	48,289	48,321

Notes: * significant at 5%; ** significant at 1%. See notes to Table 3.

Appendix Table 2: Expanded Set of Coefficient Estimates, Marriage Take-Up
Sample is: Individuals in Same-Sex Households; Outcome is: Married
BRFSS 2000-2016, Adults age 25+
Coefficient on Access to Legal Same-Sex Marriage, Fully Saturated Model

	(1) Women	(2) Men
Pre-reform mean	.047	.136
Access to Legal Same-Sex Marriage	.037*	.057*
	(.015)	(.028)
Strong Civil Unions/Domestic Partnership Policy Available to Same-Sex Couples Only	-.004	-.027
	(.016)	(.024)
Strong Civil Unions/Domestic Partnership Policy Available to All Couples	.029**	-.036
	(.010)	(.034)
Weak Civil Unions/Domestic Partnership Policy Available to Same-Sex Couples Only	-.008	.035
	(.013)	(.029)
Weak Civil Unions/Domestic Partnership Policy Available to All Couples	.018	-.034
	(.013)	(.035)
Nondiscrimination policy in private employment	-.016	-.057*
	(.021)	(.023)
Statutory Ban on Same-Sex Marriage	.008	.011
	(.015)	(.033)
Constitutional Ban on Same-Sex Marriage	.001	.005
	(.011)	(.018)
Religious Freedom Restoration Act	.006	-.015
	(.007)	(.021)
Medicaid Expansion	.016	.043
	(.014)	(.028)
CHIP Eligibility Threshold for a Family of 3	.000	.000
	(.000)	(.000)
R-squared	.025	.061
N	122,902	48,138

Notes: * significant at 5%. See notes to Table 3.

Appendix Table 3: Effects on Marriage Take-Up, by Demographics
Sample is: Individuals in Same-Sex Households; Outcome is: Married
BRFSS 2000-2016, Adults age 25+
Coefficient on Access to Legal Same-Sex Marriage, Fully Saturated Model

	(1) Under 50	(2) 50 and older	(3) White	(4) Nonwhite	(5) High school or less	(6) College graduate	(7) Any kids <18 in Household	(8) No kids <18 in Household
Non-heterosexual share in SSH ➔	.153	.093	.135	.065	.061	.191	.099	.115
Households with two adult women								
Pre-reform mean	.055	.037	.037	.063	.051	.043	.068	.036
Access to Legal Same-Sex Marriage	.045*	.026*	.040†	.032†	.032†	.041	.029	.037**
	(.019)	(.012)	(.021)	(.017)	(.017)	(.030)	(.020)	(.012)
R-squared	.030	.022	.030	.033	.027	.055	.048	.020
N	43,595	78,112	84,494	37,292	49,793	38,514	35,783	86,933
Non-heterosexual share in SSH ➔	.275	.280	.317	.162	.112	.506	.156	.292
Households with two adult men								
Pre-reform mean	.113	.187	.111	.192	.174	.112	.402	.090
Access to Legal Same-Sex Marriage	.044	.061†	.056†	.070	.067	.084*	.134	.052†
	(.037)	(.031)	(.031)	(.043)	(.043)	(.034)	(.157)	(.031)
R-squared	.073	.057	.054	.092	.085	.086	.143	.063
N	21,263	26,595	36,706	10,893	18,641	17,542	5,786	42,269

Notes: † significant at 10%; * significant at 5%; ** significant at 1%. See notes to Table 3.

Appendix Table 4: Health Insurance and Access to Care Effects for Men in Same-Sex Households Are Robust

Sample is: Men in Same-Sex Households

BRFSS 2000-2016, Adults age 25+

Coefficient on Access to Legal Same-Sex Marriage, Fully Saturated Model

	(1) Baseline model (from Table 6)	(2) (1), without linear state trends	(3) (1), code CA as off until *US. v. Windsor*	(4) (1), drop CA entirely	(5) (1), without sample weights	(6) (1), with 18-24 year olds included	(7) (1), without students
Non-heterosexual share in SSH ➔	.278	.278	.278	.270	.278	.261	.278
Insured							
Access to Legal Same-Sex Marriage	.042**	.035*	.027	.043*	.010	.047**	.042*
	(.016)	(.017)	(.023)	(.016)	(.012)	(.015)	(.017)
R-squared	.112	.110	.111	.114	.117	.115	.114
N	48,299	48,299	48,299	46,665	48,229	55,087	47,594
Usual Source of Care							
Access to Legal Same-Sex Marriage	.042*	.043**	.031	.039†	.007	.046†	.051**
	(.017)	(.014)	(.019)	(.020)	(.012)	(.026)	(.019)
R-squared	.138	.136	.138	.129	.127	.138	.136
N	46,491	46,491	46,491	44,906	46,491	52,696	45,839
Checkup in Past Year							
Access to Legal Same-Sex Marriage	.073**	.051**	.045	.074**	.039**	.066*	.071**
	(.016)	(.018)	(.031)	(.016)	(.012)	(.025)	(.016)
R-squared	.103	.101	.103	.103	.101	.093	.103
N	38,822	38,822	38,822	37,452	38,822	42,944	38,373

Notes: † significant at 10%; * significant at 5%; ** significant at 1%. See notes to Table 3.