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The effect of same-sex marriage laws on different-sex marriage: Evidence from the Netherlands

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Abstract

It has long been argued that the legalization of same-sex marriage would have a negative impact on marriage. In this paper, I examine what happened to different-sex marriage in the Netherlands after the enactment of two laws: in 1998, a law that provided all couples with an institution almost identical to marriage—registered partnership—, and in 2001, a law that legalized same-sex marriage for the first time in the world. I first construct a synthetic control for the Netherlands OECD data for the period 1988–2005 and find that neither law had significant effects on either the overall or different-sex marriage rate. I next construct a unique individual-level dataset covering the period 1995–2005 by combining the Dutch Labor Force Survey and official municipal records. The estimates from a discrete-time hazard model with unobserved heterogeneity for the first-marriage decision confirm the findings in the aggregate analysis. The effects of the two laws are heterogeneous, with presumably more liberal individuals as defined by their residence or ethnicity marrying less after both laws and potentially more conservative individuals marrying more after each law.

Keywords: Same-sex marriage, synthetic control, discrete-time hazard model.

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1. Introduction

Economists have long been interested in the effects of various policies on marriage behavior. Some of the policies studied are aimed directly at the marriage contract, such as no-fault divorce laws (Allen et al., 2006; Rasul, 2006) or minimum age requirements (Blank et al., 2009). Others alter the monetary incentives associated with marriage, such as welfare reforms (Bitler et al., 2004), income taxes (Alm and Whittington, 1999), blood test requirements (Buckles et al., 2011), or a reduction in the cost of premarital sex (Goldin and Katz, 2002). The common theme is that each of these policies altered the value of marriage relative to alternative arrangements. In this paper, I study a new policy that could affect the value of marriage, the legalization of same-sex marriage.

The argument that opening the institution of marriage to same-sex couples would affect the value of marriage was used to justify amendments to state constitutions, such as Proposition 8 in California or the Defense of Marriage Acts, laws meant to protect the federal or state governments from having to recognize a same-sex marriage performed elsewhere.[2] However, the effect of same-sex marriage on the institution of marriage is theoretically ambiguous. On the one hand, the legalization of same-sex marriage could reduce the incentives to marry if it changes social norms toward alternative family forms (Kurtz, 2004b). On the other hand, it could lead to more different-sex marriages by reigniting the interest in marriage, by reducing the pressure on government and employers to provide marriage-like benefits to cohabiting couples, or by pushing different-sex couples to reclaim the institution of marriage (Safire, 2003; Rauch, 2004).

[2] By the end of 2008, Congress and 40 states had enacted such acts (Stateline.org, 2009), with 30 states having constitutional amendments that specifically defined marriage as the union between a man and a woman.

The empirical evidence on the effect of same-sex marriage on the institution of marriage is limited and consists mostly of case studies (e.g., Eskridge and Spedale, 2006) or graphical analyses of aggregate data.[3] To date, only two studies attempt to address the issue of causality between same-sex unions and different-sex marriage. Graham and Barr (2008) reject the hypothesis that an increase in unmarried same-sex couples Granger-causes more different-sex unmarried couples using data on US states between 2000 and 2006. While addressing causality, the authors acknowledge that they cannot answer the question whether same-sex marriage would cause fewer different-sex marriages. Langbein and Yost (2009) estimate difference-in-difference models using US state-level data and find no statistically significant effects on marriage, divorce, abortion and out-of-wedlock births in states allowing same-sex marriage or civil unions. One concern is that these findings could be caused by time-varying factors that are correlated with both the introduction of same-sex marriage and the outcomes analyzed, such as trends in social norms.

In this paper, I study the effects of same-sex marriage on different-sex marriage in the Netherlands. There are several reasons for this setting. The Netherlands offers the longest time-series of same-sex marriages, being the first country to legalize same-sex marriage in 2001. Second, the Dutch legislature introduced in 1998 registered partnership, an institution identical to marriage in almost every respect and, unlike the Scandinavian registered partnership, also open to different-sex couples. This offers the unique opportunity to distinguish between a change in the marriage rate and a change in the rate of union formalization, and also for a partial distinction between the effects of same-sex marriage versus granting same-sex couples the same rights and benefits

[3] See Kurtz (2004a,b,c, 2006a,b) for interpretations of the aggregate data as showing a negative effect of same-sex marriage laws on marriage and family formation, and Badgett (2004a,b, 2009) for interpretations of no effect.

through an alternative institution. Finally, the argument that legalizing same-sex marriage would affect the institution of marriage was also used in the Netherlands.[4]

I first conduct my analysis at the aggregate level. Using data on the 16 OECD member countries that did not enact same-sex marriage or registered partnership laws by 2005 and the synthetic control method developed by Abadie and Gardeazabal (2003) and Abadie et al. (2010), I construct a counterfactual for the Dutch marriage rate in the absence of the two laws. This method, unlike previous approaches using aggregate data, takes into account both observed and unobserved determinants of the marriage rate. Regardless of whether I focus on all marriages or only different-sex marriages, a comparison of the Dutch marriage rate to the synthetic marriage rate shows an insignificant increase after the registered partnership law followed by an insignificant decrease after the same-sex marriage law. I then turn to a unique and highly confidential individual-level data set including demographic characteristics as well as information on marriage over the period 1995–2005 for approximately 10% of the Dutch population, which I use to estimate a discrete-time duration model for the age at first marriage of young Dutch. As in the aggregate data, the results from specifications with unobserved heterogeneity suggest an increase in the marriage rate after the introduction of registered partnership and a generally insignificant decline after the same-sex marriage law.

Even if the average effect of the two laws is insignificant, there can be differential responses across various groups in the population. In particular, I find different effects in samples stratified by region of residence and by ethnicity, two potential indicators of religiosity and conservative views. Individuals living in more conservative municipalities (the Dutch Bible belt) and those

[4] In personal correspondence with the author, Boris Dittrich, former member and floor leader of the Dutch Parliament, mentioned the use of these arguments during the debate on the same-sex marriage law in the Netherlands.

from more conservative ethnicities (Turks, Moroccans, other non-Westerns immigrants) tend to marry significantly more after each of the two laws, consistent with them reclaiming the institution of marriage. In contrast, individuals residing in the more liberal four largest cities tend to marry significantly less after each law, consistent either with an acceleration in the deinstitutionalization of marriage or with them learning about the availability of an alternative institution.

My findings indicate that neither the legalization of same-sex marriage nor the introduction of registered partnership have significant negative effects on the Dutch different-sex marriage rate in the aggregate, but they have several limitations. First, I can only estimate the short-term effects of the two laws given how recently they were enacted. Second, the short-term effect of the same-sex marriage law cannot be separately identified from the longer-term effect of the registered partnership law because of the close timing of the two laws. However, to the extent that these two effects are of the same sign, my results suggest that both are statistically insignificant. Finally, any extrapolation of these results to a different context would need to take into account the social and institutional differences with the Netherlands. Despite these limitations, I believe this paper makes an important contribution to our understanding of marriage behavior and to the same-sex marriage debate by providing the first causal estimates of the short-term effects of same-sex marriage laws on different-sex marriage.

2. Conceptual framework

In the standard economic marriage model (Becker, 1973, 1974), individuals choose between being in a relationship (which can only take the form of marriage) or not. This model can be extended to include alternative family forms such as cohabitation or registered partnerships. Any policy or societal change that reduces the benefits of marriage relative to these alternatives would presumably lead to fewer couples marrying.

It is theoretically ambiguous how different-sex couples might change their marriage behavior following the legalization of same-sex registered partnership and of same-sex marriage. On the one hand, the past few decades brought about a "deinstitutionalization of marriage" (Cherlin, 2004). This trend is characterized by changing social norms towards an increased acceptance of non-traditional family forms, leading to a declining marriage rate. The introduction of same-sex registered partnership and of same-sex marriage could accelerate the change in social norms and thus the decline in different-sex marriage (Kurtz, 2004b).[5] Individuals with different degrees of conservatism or religiousness might respond in potentially different ways to the changing social norms as previous research found a strong relationship between religiosity (or conservatism) and marriage attitudes and behavior (Allgood et al., 2008; Mahoney, 2010; Village et al., 2010). In addition, since registered partnership is available to all couples in the Netherlands, different-sex couples have yet another choice of family form which may reduce their incentives to marry.

It is also possible that the legalization of same-sex registered partnership and of same-sex marriage increases the different-sex marriage rate. The legalization of same-sex marriage can be interpreted an institutionalization of same-sex relationships (Lauer and Yodanis, 2010) that could reignite the interest of different-sex couples in marriage (Mello, 2004; Cahill, 2004; Safire, 2003). In addition, granting same-sex couples marriage-like benefits, either via marriage or registered partnership, could reduce the pressure on governments and employers to provide cohabiting couples similar rights to married couples. This, in turn, could slow down the decline in the relative value of marriage (Rauch, 2004). Finally, Akerlof and Kranton's (2000) identity theory suggests that some individuals might perceive marriage as an exclusive institution to which only certain couples (specifically, different-sex) have access. The introduction of same-sex registered

[5] Cherlin (2004) specifically mentions same-sex marriage as an indicator of changing social norms.

partnership might make marriage a "purer" institution, but the opening of marriage to same-sex couples could lead to a loss of identity. In response, some different-sex couples could enhance certain behaviors related to marriage (e.g., choosing a religious wedding ceremony) or "act out" (marry more) in order to "reclaim the institution of marriage."

Finally, these theoretical models suggest that any effect of same-sex marriage or registered partnership laws should be observed on new marriages and particularly first marriages, because most of the marriage-specific social and economic costs are already sunk in existing marriages.[6]

3. The Dutch legal environment

The road to same-sex marriage in the Netherlands was long and bumpy.[7] As early as 1991, Dutch gay rights organizations suggested the creation of a symbolic registry which could potentially evolve into an alternative to the marriage registry and to which municipalities would participate voluntarily. More than 100 of the 650 Dutch municipalities decided to participate within the first year. In response, the government set up a committee of legal advisers (the First Kortmann Committee) to inquire into the effects and the desirability of the legal recognition of same-sex couples. The committee recommended a Danish-style partnership and a bill to that effect was introduced in Parliament in 1993, but was held up because of the 1994 elections. The new governing coalition, which did not include Christian Democrats (the largest party opposing same-sex marriage), suggested a registered partnership open to both same-sex and different-sex couples to avoid discrimination on sexual orientation (Merin, 2002). As a result, registered partnership was

[6] They also suggest that it might be misleading to examine divorces in the aftermath of same-sex marriage laws. If there is a decline in different-sex marriage, then only couples who have relatively strong reasons to marry would choose to do so, thus reducing the divorce rate.

[7] The presentation in this section draws extensively on Merin (2002) and Curry-Sumner (2006).

designed to be an almost perfect substitute to marriage. Waaldijk (2004) compares the rights and obligations from both contracts and finds only three differences for different-sex couples. First, the paternity of a child born in different-sex marriage is automatically assigned to the man, but in a registered partnership he has to explicitly claim it, although this is rather a formality. Second, while both contracts can be terminated in court, registered partnerships can also be dissolved at the civil registry by mutual agreement.[8] Finally, couples in registered partnerships cannot engage in international adoptions, although this does not prevent one partner from adopting a child as a single individual and the other partner subsequently adopting the child as the partner of the adoptive parent.

There was still an argument that same-sex couples faced discrimination as they could not marry and a motion to this effect was introduced in 1996, leading the government to appoint a new panel of experts (the Second Kortmann Committee) to analyze the issue. In the meantime, the registered partnership bill was signed into law and become effective on January 1, 1998.

The Second Kortmann Committee report, released in October 1997, recommended the legalization of same-sex marriage while dismissing the arguments against it, particularly the issue of a possible negative effect on different-sex marriage: "The argument that a large part of the population would no longer be able to identify with marriage if it were opened up applies to an ever diminishing part of society. They can continue to identify with a marriage in church." (Kortmann Commissie, 1997, author's translation) The 1998 elections kept the same coalition in power and an agreement was reached on the introduction of a same-sex marriage bill during that term. That

[8] Starting from 2001, married couples can change their marriage into a registered partnership. Statistics Netherlands reports that more than 90% of these changes are followed by partnership dissolution by mutual agreement, a procedure called "flash divorce." This is a cheaper alternative to divorce when the parties agree on the division of property. Therefore, I only consider new partnerships in the empirical analysis.

bill was introduced in Parliament in 2000, was approved in September by the lower chamber and in December by the upper chamber, and became effective on April 1, 2001 (Merin, 2002).

In conclusion, both same-sex and different-sex couples can form registered partnerships starting from 1998 and same-sex couples are allowed to marry since 2001. For the purpose of this paper, the uncertainty in the timing of the laws makes them "exogenous" to marriage decisions, as individuals could not perfectly anticipate the enactment date of each law and marriages would already be planned by the time the laws were announced.

4. Aggregate-level analysis

4.1. Empirical strategy

Since the two laws apply to all Dutch residents, there is no control group within the Netherlands that provides the counterfactual marriage behavior of Dutch residents in the absence of the laws. At the same time, using a different country as a counterfactual can be problematic because of differences in attitudes toward marriage.[9] In the absence of an obvious control group, Abadie and Gardeazabal (2003) and Abadie et al. (2010) suggest creating a synthetic control, a weighted average of potential "donor" countries such that the averages of the synthetic marriage rate and its determinant variables closely match the corresponding numbers for the Netherlands during the "pre-intervention period" (before the enactment of the registered partnership law).[10]

Specifically, let X be a vector of marriage determinants, m the marriage rate, subscript 1 represent the Netherlands, subscript 0 the set of donor countries, and $Z_1 = (\bar{X}_1', \bar{m}_1)'$ and $Z_0 =$

[9] Belgium is an obvious choice for a control country, but Belgium enacted a registered partnership law in 2000 and a same-sex marriage law in 2003.

[10] Other cross-country studies using the synthetic control method include Billmeier and Nannicini (forthcoming), Cavallo et al. (2011), Lee (2011) and Nannicini and Billmeier (2011).

$(\bar{X}_0', \bar{m}_0)'$, where the overline represents means over the pre-intervention period. The synthetic control is the set of weights that minimize the weighted distance between the pre-intervention averages for the Netherlands and for synthetic Netherlands:

$$W^*(V) = \text{argmin}\sqrt{(Z_1 - Z_0W)'V(Z_1 - Z_0W)},$$

where V is a diagonal matrix of variable loadings. Similar to Abadie et al. (2010), the matrix V is chosen so as to minimize the mean squared error in the pre-intervention period:

$$V^* = \text{argmin}\sqrt{[m_1 - m_0W^*(V)]'[m_1 - m_0W^*(V)]}.$$

To summarize, the synthetic control is constructed by assigning a set of data-driven weights to potential donor countries such that the weighted average of their marriage rates and of their determinant variables closely match the corresponding averages in the Netherlands during the pre-intervention period. These weights are the result of a two-step optimization. In the first step, each variable is assigned a loading and the country weights are calculated as a function of these loadings to minimize the weighted distance between the synthetic control and the Netherlands. In the second step, the variable loadings are chosen so that the synthetic marriage rate matches the actual marriage rate as closely as possible, and the two steps are repeated until convergence is reached. By using both the determinants of the marriage rate and the pre-intervention marriage rate itself, the synthetic control method takes into account both the observable and the unobservable determinants of the dependent variable and produces an appropriate counterfactual for the evolution of the marriage rate in the absence of the two laws. See Appendix A1 for more details.

4.2. Data

The list of potential donors includes the 16 OECD member countries that did not enact a registered partnership or same-sex marriage law until 2005 and for which data was available: Australia, Austria, Czech Republic, Greece, Hungary, Ireland, Italy, Japan, Korea, New Zealand, Po-

land, Portugal, Switzerland, Turkey, the United Kingdom, and the United States.[11] Despite the likely differences between these countries and the Netherlands with respect to the rights offered to unmarried couples, this is arguably the set of countries most comparable to the Netherlands.[12]

The pre-intervention period includes the years 1988–1997. Marriage is measured as the crude marriage rate, defined as the total number of marriages per 1,000 inhabitants.[13] The variables included in the vector of determinants X can be classified into three groups. The first group of variables describes the number of people at risk of marriage and the probability that they will meet, or the thickness of the marriage market: the fraction of the population in the 25–44 age group, the fraction of the population living in urban areas, the sex ratio, and the life expectancy of both men and women. The second set of variables characterizes the attractiveness of individuals in the marriage market: the share of girls in total enrollment in secondary and in tertiary education, and total fertility rate. The variables in the third group, the unemployment rate of both men and women in the 25–34 age group and the GDP per capita, describe business cycle fluctuations. Finally, I use the annual growth rate of the marriage rate and the fraction of respondents who agreed with the statement "Marriage is an out-dated institution" in the World Values Survey

[11] The Civil Union Act in New Zealand came into effect on April 26, 2005, and the Civil Partnership Act came into effect in the United Kingdom in December 2005.

[12] The results are robust to restricting the set of donors to countries presumably more similar to the Netherlands, such as the European OECD member countries.

[13] The ideal measure would use only the population at risk, i.e., single individuals legally allowed to marry, but this is not commonly reported by statistical agencies. Appendix Figure A1 shows that both measures follow similar patterns in the Netherlands, with smaller relative increases and larger relative declines in the "correct" marriage rate.

to account for the long-term trend in the attitude toward the institution of marriage.[14] A list of the data sources for each country is provided in Appendix Table A1.

The first two columns of Table 1 list the averages of each variable for the Netherlands and for the group of potential donors (the unweighted average) for the period 1988-1997. The relatively large differences for certain variables suggest that the unweighted average of the potential donors might not be an appropriate control group.

4.3. Results

The means of all the variables for the synthetic control are listed in column 3 of Table 1.[15] In general, they are much closer to the corresponding values for the Netherlands (column 1) than the unweighted averages of potential donors (column 2). Indeed, the largest relative difference between columns 1 and 3 is almost half of the largest relative difference between columns 1 and 2. Finally, column 4 lists the loadings rescaled to sum up to one and shows that the variables with the largest contribution are mostly related to women and fertility.

In addition to the overall marriage rate, it is interesting to look at the different-sex marriage rate since the arguments in section 2 refer to the behavior of different-sex couples. It could also be argued that what should matter is the rate of unions (i.e., marriages and registered partnerships) rather than just marriages because some different-sex couples might choose registered partnership over marriage if they are perceived as near-perfect substitutes. Note that these three

[14] The results are robust to the inclusion of additional variables, such as the fraction of the population in the 20–39 age group, the crude birth rate, the share of girls total enrollment in primary education, the labor force participation rate of men and women in the 25–34 age group, the inflation rate, and the GDP growth rate.

[15] The weights of each donor country in the synthetic control are listed in Appendix Table A2. Four countries (in order: Austria, Italy, Switzerland and Australia) account for over 90 percent of the synthetic control.

measures of union formation are the same prior to 1998 and that the counterfactual in each case is the same: what the marriage rate would have been in the absence of the two laws.

Figure 1 plots the overall marriage rate in the Netherlands and synthetic Netherlands, while Figure 2 plots the different-sex marriage rate (panel a) and different-sex union rate (panel b) against the same synthetic rate. As expected, the actual rates are relatively close to the synthetic marriage rate between 1988–1997, the period used to construct the synthetic control. After the introduction of registered partnership, the three rates are all higher than the synthetic marriage rate, but they all fall below the synthetic rate at some point after 2001, the date when same-sex marriage was legalized.

To determine the statistical significance of the actual-synthetic difference after 1998, I conduct permutation experiments (Abadie et al., 2010) in which the Netherlands is assigned to the donor pool, one of the donors is considered "treated" and a synthetic control for this new "treated" group is constructed. I restrict the analysis to the donors with a reasonably close fit in the pre-intervention period as measured by mean square prediction errors ($MSPE$).[16] Figure 3 plots the gaps between the actual and the synthetic rates for the Netherlands (the dark lines) and for the donors with $MSPE$ at most five times (panel a) or twice (panel b) as large as the Netherlands (the grey lines). The gap for the Netherlands is always within the range produced by the permutation tests. In other words, if a random country were picked from these restricted donor pool, the chance of finding an actual-synthetic gap in the post-1998 marriage rate comparable to the one in the Netherlands is 10% (corresponding to the 10 solid lines in panel a) or 12.57% (corresponding to the 8 solid lines in panel b), levels similar to common statistical tests. Therefore, we can con-

[16] The mean squared prediction error is the mean squared error of the synthetic marriage rate relative to the actual marriage rate during the pre-intervention period: $MSPE = \frac{1}{T_0}\sum_{t=1}^{T_0}(m_{1t}^{*} - m_{1t})^2$.

clude that the evolution of the Dutch marriage rate, whether overall or only for different-sex couples, is not statistically different after the enactment of each law from its evolution in the absence of these laws.[17]

The particular setup of the two laws makes it impossible to separate the long-term effects of the registered partnership law (beyond the first three years) from the short-term effects of the same-sex marriage law. However, if these two laws have an effect on the institution of marriage, it is likely that they act in the same direction (see section 2). In this case, the results in this section suggests that each of the two effects is insignificant.

5. Individual-level analysis

5.1. Empirical strategy

The individual-level analysis mirrors the aggregate analysis by estimating the effect of the two laws on the probability of marriage. In particular, I confine the analysis to never-married individuals and thus first marriages because previously-married individuals are likely to attribute a value to marriage that might not be influenced by changes in its definition.[18] The baseline specification is

$$P(m_{is} = 1 | m_{is-1} = 0\ ) = h(X_{is}, RP_s, RPSM_s; \theta_i), \quad (1)$$

where m_{is} is an indicator for individual i marrying during year s, and X_{is} is a vector of observable and potentially time-varying characteristics. The main variables of interest are RP_s, a dummy variable for the period following the registered partnership law (1998–2000), and $RPSM_s$ for the period following the same-sex marriage law (2001–2005). This model has the structure of a dis-

[17] The Appendix details a second type of test that confirms this conclusion.

[18] This is not restrictive, as Appendix Figure A3 shows that the variation in the total number of marriages is driven by the variation in the number of first marriages.

crete-time duration model with first marriage as the event, $h(\cdot)$ as the hazard function and age at first marriage as duration. Formally, let T_i be the random variable representing the age at first marriage age of individual i, t the age of individual i in year s, and a_i the last observed age of the individual, all measured in full years at the end of year s. As in several other studies (e.g., Nickell, 1979; Ham and Rea, 1987), I assume that the hazard function $h(\cdot)$ has the logit form:

$$h(X_{is}, RP_s, RPSM_s; \theta_i) = P\,(T_i = t | T_i \geq t) = h_i(t; \theta_i) = \frac{1}{1 + \exp\{-y_i(t; \theta_i)\}}, \qquad (2)$$

where

$$y_i(t; \theta_i) = \theta_i + X_{it}'\,\beta + RP_t\lambda_1 + RPSM_t\lambda_2 + s\lambda_3 + \gamma(t), \qquad (3)$$

s represents a linear trend, θ_i captures the unobserved characteristics of the individual and follows a discrete distribution with two mass points, $\bar{\theta}_1$ and $\bar{\theta}_2$ (Heckman and Singer, 1984), and $\gamma(\cdot)$ represents duration dependence, the common way age influences the probability of marriage for any given person.[19]

The two coefficients of interest, λ_1 and λ_2, capture the effects of each law on the age-specific conditional probability in the corresponding period as compared to the period before 1998, measured as deviations from the long-term trend in the marriage rate.[20] The identifying assump-

[19] The most flexible form of duration dependence, a step function, leads to identification problems when unobserved heterogeneity is also modeled non-parametrically (Narendranathan and Stewart, 1993). Eberwein et al. (2002) argue that the actual functional form does not influence the results as long as it is flexible. Based on their suggestion, I add higher-order terms in $ln(t),\ t = age - 17$, until they become insignificant. This procedure yields a fourth-order polynomial, which produces almost identical estimates to a specification with a full set of age dummies in a model without unobserved heterogeneity (available upon request).

[20] An effect on the propensity to marry at every age during a particular period leads to an effect on the marriage rate in that particular period. To facilitate the comparison with the results in section 4, I will loosely interpret the coefficients in equation (3) as effects on the marriage rate for the rest of the discussion.

tion is that the two laws have only *level* effects (i.e., they do not change the trend in the marriage rate) and thus the marriage behavior before 1998 is an appropriate counterfactual for the marriage behavior in the absence of the two laws once the long-term trend is taken into account.[21] This assumption is inherently untestable, but two arguments can be made to support it. First, as it will be mentioned in section 5.2, the data covers a relatively short period of time (1995–2005), making a change in the long-term trend in the marriage rate less likely. Second, if the pre-1998 marriage behavior is an appropriate counterfactual for the post-1998 marriage behavior, then the actual pre-1998 marriage rate and the synthetic post-1998 marriage rate constructed in section 4 should be similar. This is confirmed by Appendix Figure A4, which shows that the 1998–2000 and 2001–2005 synthetic marriage rates are almost parallel to the pre-1998 marriage rate.

Finally, the likelihood function for a sample of N individuals is:

$$L = \prod_{i=1}^{N} [P(T_i = a_i)]^{\delta_i} [P(T_i > a_i)]^{1-\delta_i},$$

where δ_i equals one if person i is observed to marry and zero otherwise, a_i is the last observed age of the individual, and

$$P(T_i = a_i) = \sum_{k=1}^{2} \left\{ \pi_k h_i(a_i; \bar{\theta}_k) \prod_{t=18}^{a_i - 1} [1 - h_i(t; \bar{\theta}_k)] \right\},$$

$$P(T_i > a_i) = \sum_{k=1}^{2} \left\{ \pi_k \prod_{t=18}^{a_i} [1 - h_i(t; \bar{\theta}_k)] \right\}.$$

A random sample of never-married individuals (a stock sample) oversamples individuals who prefer to marry late and produces biased estimates, a situation known as the initial conditions

[21] A more flexible approach with a non-linear trend or with different slopes in each period would overfit the variation in the marriage rate over such a short period.

problem. Therefore, the likelihood function should be maximized in a flow sample of individuals (i.e., a sample of individuals who become at risk of marriage during the sample period).

5.2. Data

I construct the data using ten waves, 1996–2005, of the restricted version of the Dutch Labor Force Survey (*Enquête Beroepsbevolking*, *EBB*) and the January 2006 snapshot of the confidential Dutch Municipal Records (*Gemeentelijke Basis Administratie*, *GBA*). The EBB is an annual cross-sectional survey of the population 15 years of age and older. It includes information on educational attainment, ethnicity, and other demographic and labor market characteristics at the time of the interview, as well as a unique identification number that can be used to match individuals to other data sets maintained by Statistics Netherlands. The combined ten waves of the survey contain almost 950,000 individuals, approximately 6% of the average Dutch population between 1995–2005. To increase the probability that the highest educational level reported does not change over the sample period, I only keep from the EBB individuals at least 20 years of age during the interview year.[22] Using the unique identification number, these individuals are matched to their marriage and residence history for the entire 1995–2005 period provided in the GBA.[23] The resulting longitudinal data set includes information on ethnicity, marital status and residence over the entire period, and on educational attainment and school enrollment at the time

[22] About 15 percent of the sample was still enrolled at the time of the survey (approximately 9 percent full-time and 6 percent part-time). A small fraction of these individuals were enrolled in a lower level than their highest level completed (for example, persons with a college degree in science enrolled in professional business courses). The highest of the two levels was used in these cases. A complete picture of the Dutch education system is shown in Appendix Figure A7.

[23] For example, the marital status and residence of a person interviewed for the EBB in 2004 at age 24 is observed from age 15 (in 1995) until age 25 (in 2005). However, this person can only marry after she turns 18 (in 1998).

of the EBB interview. Finally, the data is augmented with the yearly unemployment rate at the regional level.[24]

As in the aggregate-level analysis, the control variables measure the attractiveness of an individual on the marriage market (5-year age cohort, education and ethnicity), the thickness of the market (location of residence, level of urbanization and ethnicity), business cycle fluctuations (regional unemployment rate) and the trend in the view on the institution of marriage (linear trend). Time is measured in calendar years because of how certain variables such as the regional unemployment rate are measured. Moreover, the strong seasonal pattern in marriages within a calendar year suggests that marriage decisions are commonly based on the calendar year (see Appendix Figure A5). Age is measured in full years at the end of the calendar year (so that individuals who turn 18 during the year are included in the sample), residence at the beginning of the year (under the assumption that most marriage decisions are made in advance), and the regional unemployment rate as the calendar-year average.

There are several limitations to this data set. First, there is no difference in the recording of same-sex and different-sex marriages. Second, I have no information on individuals not interviewed for the EBB, particularly the spouses of individuals in the sample. Finally, the coding of addresses changes over time and is aggregated at the street-number level since 2003. As a result, it is virtually impossible to identify the spouse of all individuals and I am unable to distinguish between same-sex marriages and different-sex marriages. This induces a small upward bias in the estimate of the different-sex marriage rate after 2001.[25]

[24] The Netherlands is divided into twelve provinces: Drenthe, Flevoland, Friesland, Gelderland, Groningen, Limburg, Noord-Brabant, Noord-Holland, Overijssel, Utrecht, Zeeland and Zuid-Holland.

[25] Same-sex marriages represent less than 2 percent of all marriages over the period 2001–2005.

Recall from section 5.1 that the empirical analysis requires a flow sample. However, the sample cannot be restricted only to individuals who turn 18 between 1995 and 2005 because of the increasingly high average age at first marriage in the Netherlands (from 29.6 for men and 27.4 for women in 1995 to 32.4 and 29.7 in 2005, respectively). Instead, I include all individuals who are first observed at an age such that the probability of having never married is close to one. Based on the aggregate distribution of Dutch marriages by age, this yields the intervals 18–24 for men and 18–22 for women, which account for about 10% of first marriages between 1995 and 2005, respectively.[26]

I conduct the analysis separately by gender because women tend to marry earlier than men and they are more likely to marry previously-married opposite-sex partners. This also implies that there is no one-to-one relationship between marriages in the sample of men and in the sample of women. The final sample includes 70,718 men and 53,883 women, for which descriptive statistics are listed in Table 2.[27]

Compared to the numbers in Table 1, the average person in the sample is younger than the average person in the population, with an average age at first marriage around 27 years for men and 25 years for women. Consequently, only 26.29% of men and 33.30% of women marry during the sample period (see also the Kaplan-Meier estimates in Appendix Figure A6). The distribution of education is skewed toward higher levels of education, mostly professional degrees,

[26] While a relatively small number of the individuals in the flow sample marry by the end of the sample period, the coefficients of interest measure the variation in the marriage propensity of individuals of the same age before and after the enactment of each law.

[27] All the statistics and the subsequent analysis use sample weights. These weights are constructed by rescaling the weights provided in the EBB to represent the probability of interview relative to the entire sample of ten waves, assuming that the population structure does not change significantly during the study period.

but with relatively more male university graduates, consistent with the shares of girls in secondary and tertiary education in Table 1. Approximately 83% of the sample are natives and almost 8% are Western immigrants from Europe (except Turkey), North America, Oceania, Japan and Indonesia. Immigrants from potentially more conservative areas such as the predominantly Muslim Turkey and Morocco, or from Aruba and Suriname, account for about 6% of both men and women. The fraction of the sample living in an urban area when first observed is slightly lower than in the population. One explanation is that single people tend to move to cities, where the marriage markets are thicker, but (married) couples tend to move out of the cities, where housing is cheaper (Gautier et al., 2010).

The individual-level data allows me to exploit the geographic heterogeneity with respect to attitudes toward marriage. One area of interest groups the four largest cities (Amsterdam, The Hague, Rotterdam and Utrecht), which have relatively low rates of fertility, marriage and church attendance, but high rates of divorce and non-marital birth. A second area is the so-called Dutch Bible belt (*De Bijbelgordel*), a group of municipalities with relatively high church participation and fertility rates, together with low rates of cohabitation, divorce and non-marital births (Sobotka and Adigüzel, 2002; de Jong, 2003). I include in the Bible belt the municipalities where the four conservative Christian parties participating in the 1998 election to the lower-chamber of the Dutch Parliament obtained more than 20% of the votes.[28] The 32 municipalities in the Bible belt are listed in Appendix Table A3 and the two areas of interest are highlighted on the map in Appendix Figure A8.

[28] Based on data from the Dutch Electoral Council (de Kiesraad). The four parties are the Reformatory Political Federation (Reformatorische Politieke Federatie, RPF), the Reformed Political Party (Staatkundig Gereformeerde Partij, SGP), the Reformed Political League (Gereformeerd Politiek Verbond, GPV) and the Catholic Political Party (Katholiek Politieke Partij, KPP). They obtained 5.17% of the votes at the national level and won 8 out of 150 seats.

As expected, a significantly larger fraction of individuals in the Bible belt marry as compared to the sample average (43.13% of men and 52.44% of women, compared to 26.29% and 33.30%, respectively), while the opposite holds for the individuals in the four largest cities (20.99% and 24.64%, respectively). Similarly, marriages contracted in the Bible belt represent a disproportionately high fraction in the total number of marriages (almost 7%). These statistics confirm that the four largest cities comprise some of the more liberal areas in the Netherlands, while the Bible belt municipalities include the more conservative. In the rest of the analysis, I include indicators for residence in one of these two areas among the control variables.

5.3. Results

I first estimate a model without unobserved heterogeneity. The estimation results are listed in columns 1 and 3 of Table 3. I then estimate the model with unobserved heterogeneity, shown in columns 2 and 4. Note that only coefficient ratios can be compared across specifications since changing the distribution of unobserved heterogeneity changes the variance normalization of coefficients in logit models (Mroz and Zayats, 2008; Nicoletti and Rondinelli, 2010).

As expected, the decline in the marriage rate is attributed to the two laws when not controlling for unobserved heterogeneity (columns 1 and 3).[29] The estimates show a drop in the marriage probability in each of the two periods for both men and women. Once the unobserved heterogeneity is taken into account (columns 2 and 4), the estimates suggest that there is no negative

[29] Suppose that there are two types of individuals: type-A, who wants to marry young, and type-B, who wants to marry late. Over time, individuals who marry will exit the sample, and these will predominantly be type-A. As a result, the sample will include an increasingly disproportionate number of type-B individuals at any age. Since type-B individuals marry less than type-A at any age, age-specific marriage rates will be decreasing over time. If unobserved heterogeneity is ignored, this decline is then incorrectly attributed to the two laws.

change in the marriage behavior of individuals after the enactment of each law, with the exception of young women after the same-sex marriage law. Even in this last case, the decline in the marriage probability relative to the long-term trend is less than half than in the case without unobserved heterogeneity. The first-marriage behavior of young Dutch should be expected to be more volatile than in the aggregate analysis, particularly in the case of women. If most of the variation in the number of marriages is due to first marriages, the relative change in the first-marriage rate will be larger because the denominator is smaller (only never-married individuals). Still, young women seem to be more responsive than men to changes in their environment, with larger relative increases and declines in their marriage hazard relative to the long-term trend.[30]

Some other results are worth noting, such as the fact that most of the patterns in the explanatory variables do not change with the inclusion of unobserved heterogeneity. Not surprisingly, the estimates indicate a negative long-term trend in the marriage rate and a negative effect of economic downturns via the unemployment rate. The relationship between education and marriage varies by sex, being almost an inverted-U for men, consistent with male hypogamy (men "marrying down"), and an almost linearly decreasing relationship for women, consistent with female hypergamy (women "marrying up").[31] Finally, the estimates suggest that certain groups have higher or lower propensities to marry irrespective of gender. For instance, immigrants from Turkey and Morocco or from other non-Western countries as well as individuals residing in the Bible belt are more likely to marry than natives. On the other hand, immigrants from a Western country or from Suriname and Aruba are less likely to marry, as are individuals living in urban

[30] The difference in the estimates is entirely driven by the oldest cohort of women (21 and 22 years old in 1995). Once these observations are excluded, all the results are qualitatively similar for both men and women.

[31] In both cases, the omitted category is "senior vocational," an intermediate level between "general secondary" and "higher professional".

areas, especially in the four largest cities. These patterns suggest that the marriage behavior of these groups could be impacted differently by the two laws.

5.4. Heterogeneous effects

To take into account the geographic differences in marriage behavior, I modify the hazard function in equation (3) as follows:

$$y_i(t;\theta_i) = \theta_i + X'_{it}\,\beta + \gamma(t) + \sum_j \left(RP_t\, D^j_{it}\, \lambda^j_1 + RPSM_t\, D^j_{it}\, \lambda^j_2 + s\, D^j_{it}\, \lambda^j_3\right), \qquad (4)$$

where j represents one of the three regions defined above (Dutch Bible belt, four largest cities, rest of the Netherlands), D^j_{it} is a dummy for individual i residing in region j at the beginning of year t, and the other variables are the same as before. In this specification, λ^j_1 represents the change in the propensity to marry after the enactment of the registered partnership law among individuals living in regions j relative to the period 1995–1997 in that same region, and λ^j_2 measures the corresponding effect for the same-sex marriage law. Finally, λ^j_3 measures the long-term trend in the age-specific marriage rate in region j.

The results are listed in Table 4 and indicate that individuals in the Bible belt have the highest baseline tendency to marry and the slowest-declining long-term trend among the three groups, while people in the four largest cities have the lowest baseline marriage probability and the fastest-declining long-term trend. The response to the two laws is also markedly different across the three regions. The marriage rate increases in the Bible belt after the registered partnership law and then even more after the same-sex marriage law. For example, the increase in the marriage rate of men after the legalization of same-sex marriage is about nine times the year-to-year variation in their marriage rate over the sample period. In contrast, individuals in the four largest cities marry less after the registered partnership law and even less after the same-sex mar-

riage law. The decline in their marriage rate after the same-sex marriage law is about twice as large as the yearly decline in their marriage rate. Finally, individuals living in the rest of the country marry more after the registered partnership law and then return to their long-term trend after the same-sex marriage law, similar to the overall results in the previous section.

Next, I study the marriage behavior of individuals of different ethnicities.[32] The hazard function has the same form as in equation (4), where j now represents one of the five ethnic groups (Dutch natives, Western immigrants, Surinamese/Arubans, Turks/Moroccans, other non-Western immigrants) and D_i^j is a dummy for individual i being of ethnicity j. The coefficients of interest have a similar interpretation as before but with respect to ethnicity j.

Table 5 presents the results. Relatively more conservative men (Turks/Morrocans, Surinamese/Arubans and other non-Western immigrants) marry more after the registered partnership law and then at least as much after the same-sex marriage law. For instance, the marriage rate of Turkish and Moroccan men increases by approximately 3.5 times the yearly variation in their marriage rate over the sample period after the legalization of same-sex marriage. Dutch natives and Western immigrants, on the other hand, tend to have a behavior similar to the overall population, with a marriage rate around or higher than its long-term trend after the registered partnership law and then falling after the same-sex marriage law. For example, the marriage rate of Western immigrant men falls by almost the same amount as the yearly decline in their marriage rate after the same-sex marriage law. For women, all immigrant ethnic groups experience an increase in their marriage hazard after the registered partnership law and a slight decline after the

[32] Appendix Table A4 shows various measures of religiosity for individuals of different ethnicities extracted from the World Values Survey and from the Longitudinal Internet Studies for the Social sciences. The Table shows that Turks, Moroccan, Arubans, Surinamese and individuals of non-Western descent are on average more conservative than native Dutch or individuals of Western descent.

same-sex marriage law while still above the long-term trend. Finally, native Dutch women show a pattern similar to the overall results.

These results indicate significant variation in the response to the two laws. The presumably conservative individuals residing in the Bible belt or of non-Western ethnicities seem to be affected by each law and have marriage probabilities significantly above their long-term trend in each period, consistent with them "reclaiming the institution of marriage" along the lines of Akerlof and Kranton's (2000) identity theory. Presumably liberal individuals such as those living in the four largest cities marry less after the introduction of each law, not always statistically significant, consistent either with an acceleration in the deinstitutionalization of marriage, or with them learning about the availability of an alternative institution.[33]

6. Conclusions

This paper contributes to the same-sex marriage debate by providing the first causal estimates to the question of whether opening the institution of marriage to same-sex couples would have negative consequences on the institution of marriage, particularly for different-sex couples. I focus on the Netherlands, the first country to legalize same-sex marriage in 2001. Overall, I do not find evidence of negative effects from the legalization of same-sex marriage or from the introduction of registered partnership. However, the response to the two laws varies across regions and ethnicities, with potentially more conservative individuals marrying statistically significantly more after each law and presumably more liberal individuals marrying increasingly less (though not always statistically significant) after each of the two laws. These patterns provide suggestive evidence in support of some of the theories of marriage behavior presented in section 2.

[33] Boele-Woelki et al. (2007) report case studies of cohabiting couples entering registered partnership after being informed of its existence and its similitude to marriage.

There are three caveats to the present study. First, any analysis to date can provide information only on the short-term effects of the two laws because of their recent enactment. Second, it is practically impossible to separate the short-term effect of the same-sex marriage law from the longer-term effect of the registered partnership law because of the timing of the two laws. Since the two effects plausibly have the same sign, my results suggest that both are statistically insignificant. Finally, any extrapolation of the results needs to take into account the social and institutional differences between other countries and the Netherlands. Despite these limitations, I believe my analysis makes an important contribution to our understanding of marriage behavior and to the same-sex marriage debate.

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Table 1: Descriptive statistics and loadings for the variables used in the synthetic control method

	Mean, 1988–1997			Variable Loading
	Netherlands	Potential donors	Synthetic Netherlands	
	(1)	(2)	(3)	(4)
Crude marriage rate	5.83	6.45	5.91	
Population, age 25-44 (%)	32.32	29.57	30.37	0.000
Urban population (%)	70.87	69.08	70.44	0.034
Sex ratio	0.99	0.97	0.97	0.000
Life expectancy, men (years)	74.26	71.58	73.49	0.012
Life expectancy, women (years)	80.21	78.13	79.88	0.000
Girls share in secondary education (%)	47.54	48.58	47.64	0.250
Girls share in tertiary education (%)	46.56	47.99	46.44	0.239
Fertility rate	1.57	1.75	1.57	0.281
Unemployment rate, men, age 25-34 (%)	5.67	6.98	5.74	0.036
Unemployment rate, women, age 25-34 (%)	8.17	8.94	8.34	0.039
GDP per capita	19,583.48	15,017.01	20,315.45	0.017
View on the marriage institution	21.20	13.53	14.73	0.000
Growth rate of marriage rate	-0.01	-0.01	-0.01	0.092

Notes: Columns 1–3 show the mean of the corresponding variables over the period 1988–1997, with the exception of the marriage views variable, which is averaged over 1988–2000 for the donor countries. Column 2 includes simple averages of the countries in the donor group and column 3 includes weighted averages using the weights produced by the synthetic control method. Column 4 lists variable loadings (the diagonal elements of matrix V), rescaled so as to sum up to one.

Table 2: Summary statistics

	Men (%)	Women (%)
First marriages		
Percent	26.29	33.30
Average age (years)	27.37	25.25
	(2.97)	(2.96)
Censored observations		
Percent	73.71	66.70
Average age (years)	28.19	26.96
	(3.70)	(3.23)
Birth cohort		
1970-74	41.04	23.79
1975-1979	39.54	51.19
1980-1984	18.43	23.79
1985-1989	0.99	1.23
Education		
Primary education	4.24	3.03
Secondary vocational	16.83	12.95
General secondary	6.83	7.10
Senior vocational	39.88	39.85
Higher professional	23.16	28.39
College	9.05	8.67
Ethnicity		
Natives	83.11	82.85
Western immigrants	7.71	7.80
Turks/Moroccans	3.21	3.43
Surinamese/Arubans	3.01	3.26
Other non-Western immigrants	2.97	2.66
Residence in urban area at entry into sample	62.86	63.79
Four largest cities		
Residence at entry into sample	10.23	10.55
Percent of total marriages	12.11	12.03
Percent of residents marrying	20.99	24.64
Bible belt		
Residence at entry into sample	4.35	4.35
Percent of total marriages	6.93	6.66
Percent of residents marrying	43.13	52.44
Number of individuals	70,717	53,799

Notes: Never-married individuals aged 18-22 (men) or 18–24 (women) in 1995 or who turned 18 years of age between 1996–2005. All statistics are weighted using sample weights.

Table 3: Discrete-time duration model for age at first marriage

	Men (N = 70,717)		Women (N = 53,799)	
	(1)	(2)	(3)	(4)
Period 1 (1998–2000)	-0.016	0.032	-0.038*	0.054*
	(0.012)	(0.014)	(0.013)	(0.014)
Period 2 (2001–2005)	-0.048*	0.002	-0.180*	-0.078*
	(0.018)	(0.019)	(0.018)	(0.020)
Linear trend (1995=0)	-0.039*	-0.040*	-0.028*	-0.028*
	(0.003)	(0.003)	(0.003)	(0.004)
Education (omitted category: Senior vocational)				
Primary education	-0.264*	-0.121*	0.009	0.532*
	(0.011)	(0.018)	(0.012)	(0.022)
Secondary vocational	-0.060*	0.037*	0.092*	0.379*
	(0.006)	(0.009)	(0.007)	(0.011)
General secondary	-0.348*	-0.509*	-0.272*	-0.427*
	(0.010)	(0.013)	(0.009)	(0.013)
Higher professional	-0.166*	-0.333*	-0.395*	-0.688*
	(0.006)	(0.008)	(0.006)	(0.008)
University	-0.171*	-0.393*	-0.642*	-1.096*
	(0.008)	(0.011)	(0.010)	(0.013)
Ethnicity (omitted category: Natives)				
Western immigrants	-0.148*	-0.165*	-0.192*	-0.161*
	(0.009)	(0.012)	(0.009)	(0.013)
Turks/Moroccans	1.278*	2.312*	1.616*	2.867*
	(0.010)	(0.017)	(0.010)	(0.018)
Surinamese/Arubans	-0.145*	-0.069*	-0.321*	-0.152*
	(0.014)	(0.021)	(0.016)	(0.024)
Other non-Western	0.144*	0.238*	-0.005	0.177*
	(0.014)	(0.019)	(0.016)	(0.023)
Unemployment rate	-0.027*	-0.027*	-0.017*	-0.016*
	(0.002)	(0.002)	(0.002)	(0.002)
Urban indicator	-0.154*	-0.178*	-0.227*	-0.311*
	(0.005)	(0.006)	(0.005)	(0.007)
Bible belt	0.767*	1.106*	0.705*	1.080*
	(0.009)	(0.013)	(0.009)	(0.014)
Four largest cities	-0.236*	-0.361*	-0.259*	-0.355*
	(0.007)	(0.009)	(0.007)	(0.010)
Unobserved heterogeneity	no	yes	no	yes
log L / 1000	-983.130	-978.405	-872.522	-863.108

Notes: Sample of never-married individuals aged 18-24 (men) or 18–22 (women) in 1995 or who turned 18 years of age between 1996–2005. All specifications include five-year birth cohort dummies and a fourth-degree polynomial in $ln(age - 17)$ and are weighted using sample weights. The unobserved heterogeneity term is drawn from a discrete distribution with 2 mass points. Starred coefficients are significant at the 1 percent level.

Table 4: Discrete-time duration model for age at first marriage, by location

	Men (N = 70,717, log L / 1000 = -978.205)		
	Bible belt	Four largest cities	Rest of the Netherlands
	(1)	(2)	(3)
Main effect	1.041*	-0.130*	–
	(0.023)	(0.022)	
Period 1 (1998–2000)	0.250*	-0.101*	0.080*
	(0.024)	(0.029)	(0.014)
Period 2 (2001–2005)	0.308*	-0.114*	0.034
	(0.025)	(0.043)	(0.020)
Linear trend (1995=0)	-0.033*	-0.055*	-0.040*
	(0.005)	(0.006)	(0.003)
	Women (N = 53,799, log L / 1000 = -862.592)		
	Bible belt	Four largest cities	Rest of the Netherlands
Main effect	1.086*	0.004	–
	(0.023)	(0.022)	
Period 1 (1998--2000)	0.388*	-0.037	0.085*
	(0.026)	(0.030)	(0.015)
Period 2 (2001--2005)	0.440*	-0.359*	-0.030
	(0.028)	(0.046)	(0.021)
Linear trend (1995=0)	-0.051*	-0.051*	-0.027*
	(0.005)	(0.006)	(0.004)

Notes: Discrete-time duration model with unobserved heterogeneity for age at first marriage using a sample of never-married individuals aged 18-24 (men) or 18–22 (women) in 1995 or who turned 18 years of age between 1996–2005. All specifications include a fourth-degree polynomial in $ln(age - 17)$, the regional unemployment rate, and dummies for five-year birth cohort, ethnicity, education and residence in an urban area. All specifications are weighted using sample weights. The unobserved heterogeneity term is drawn from a discrete distribution with 2 mass points. Starred coefficients are significant at the 1 percent level.

Table 5: Discrete-time duration model for age at first marriage, by ethnicity

	Men (N = 70,717, log L / 1000 = -977.941)				
	Natives	Western immigrants	Turks/ Moroccans	Surinamese/ Arubans	Other Non-Western
	(1)	(2)	(3)	(4)	(5)
Main effect	–	-0.040	2.648*	0.373*	0.151*
		(0.029)	(0.028)	(0.046)	(0.051)
Period 1 (1998--2000)	-0.018	0.133*	0.446*	0.418*	0.346*
	(0.014)	(0.038)	(0.040)	(0.062)	(0.065)
Period 2 (2001--2005)	-0.054*	-0.047	0.621*	0.671*	0.292*
	(0.020)	(0.059)	(0.067)	(0.096)	(0.097)
Linear trend (1995=0)	-0.025*	-0.056*	-0.185*	-0.194*	-0.063*
	(0.003)	(0.008)	(0.010)	(0.013)	(0.012)
	Women (N = 53,799, log L / 1000 = -861.873)				
	Natives	Western immigrants	Turks/ Moroccans	Surinamese/ Arubans	Other Non-Western
Main effect	–	0.087*	3.131*	0.976*	0.886*
		(0.030)	(0.028)	(0.042)	(0.045)
Period 1 (1998--2000)	-0.003	0.281*	0.461*	0.237*	0.091
	(0.015)	(0.040)	(0.040)	(0.062)	(0.064)
Period 2 (2001--2005)	-0.141*	0.275*	0.247*	0.226	-0.267
	(0.021)	(0.061)	(0.071)	(0.103)	(0.107)
Linear trend (1995=0)	-0.007	-0.108*	-0.128*	-0.260*	-0.133*
	(0.004)	(0.008)	(0.010)	(0.014)	(0.014)

Notes: Discrete-time duration model with unobserved heterogeneity for age at first marriage using a sample of never-married individuals aged 18-24 (men) or 18–22 (women) in 1995 or who turned 18 years of age between 1996–2005. All specifications include a fourth-degree polynomial in $ln(age - 17)$, the regional unemployment rate, and dummies for five-year birth cohort, ethnicity, education and residence in an urban area. All specifications are weighted using sample weights. The unobserved heterogeneity term is drawn from a discrete distribution with 2 mass points. Starred coefficients are significant at the 1 percent level.

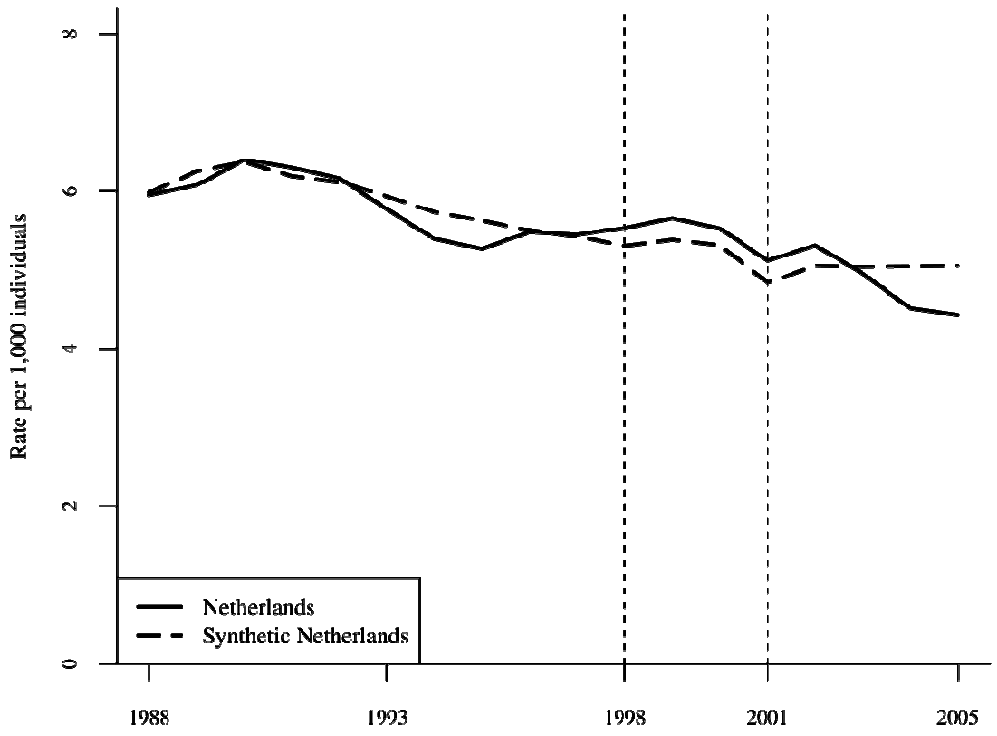
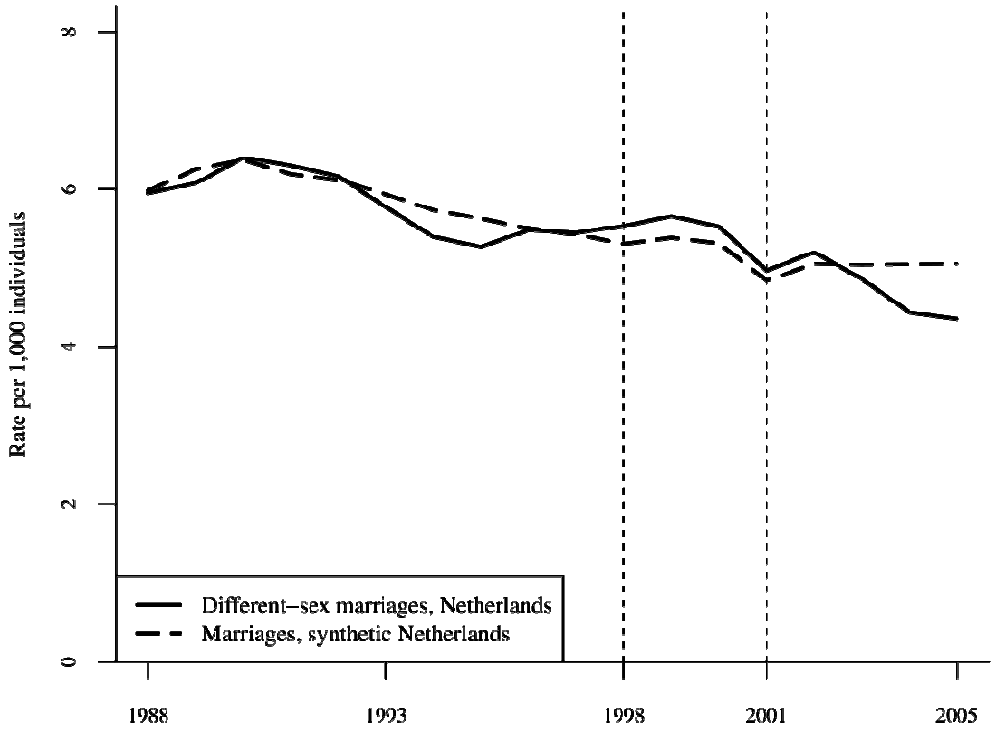
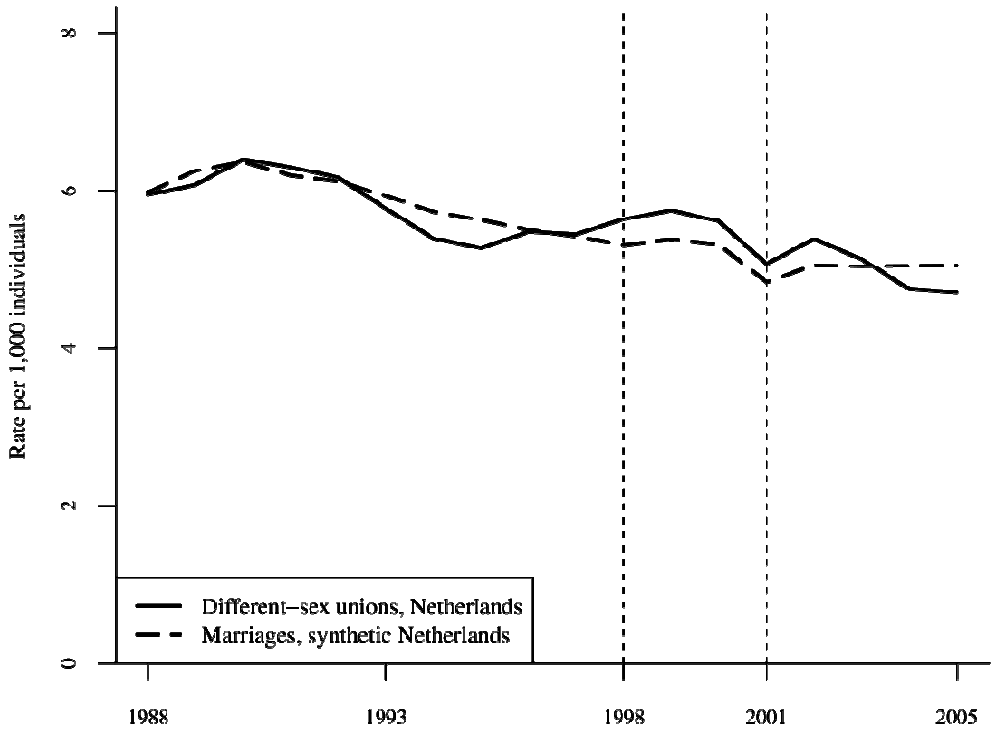


Figure 1: Evolution of marriage rate in the Netherlands and in the synthetic control

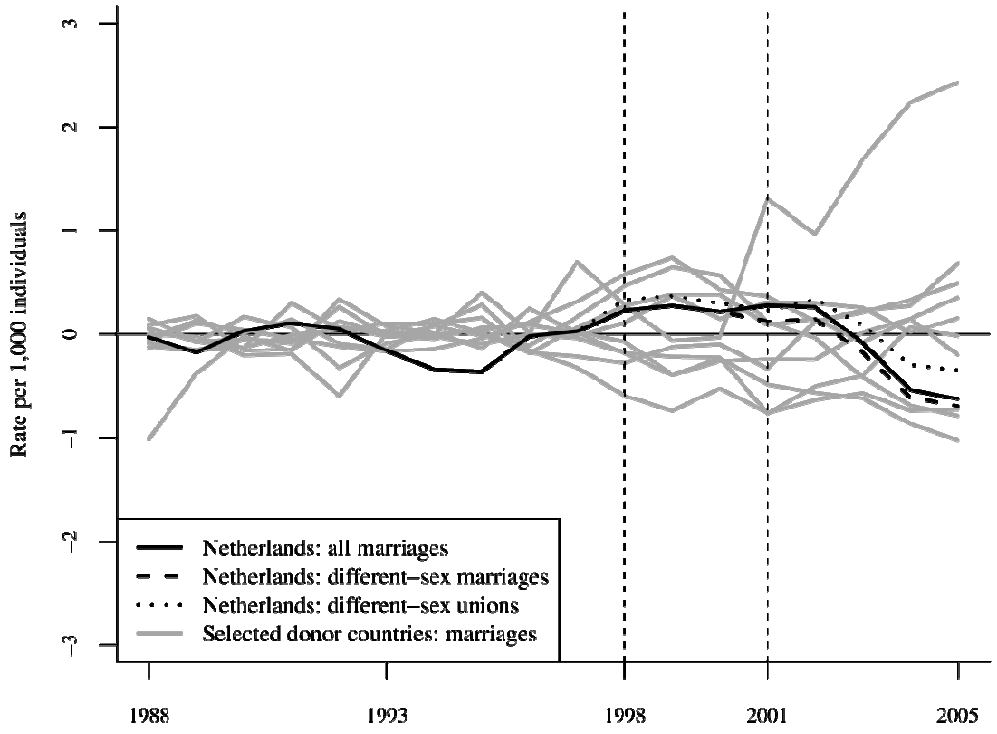


(a) Different-sex marriage rate

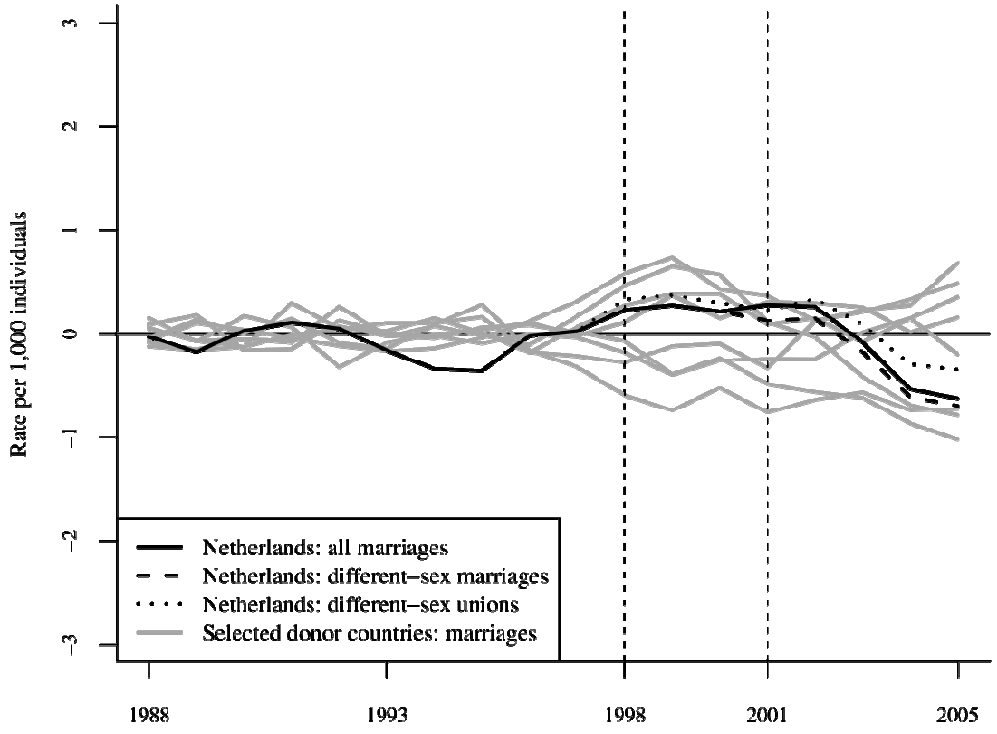


(b) Different-sex unions rate

Figure 2: Alternative measures of different-sex union formation



(a) At most 5 times the MSPE of the Netherlands



(b) At most 2 times the MSPE of the Netherlands

Figure 3: Comparison of actual-synthetic marriage rate differences