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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.
JEL Classification: J12, J15.

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1 Introduction

Economists have long been interested in the effects of various policies on individuals' decision to marry. 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 in all these studies 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.[1] Indeed, on May 31, 1996, during a House subcommittee meeting on the issue, Rep. Henry Hyde stated that "[t]he institution of marriage is trivialized by same-sex marriage" (Mohr, 1997). However, the effect of same-sex marriage on the institution of marriage is theoretically ambiguous. On the one hand, same-sex marriage could reduce the incentives to marry if it also changes social norms toward cohabitation, if it disentangles the link between child bearing and rearing and marriage, or if it reduces the feeling of belonging to an "exclusive club" (Stewart, 2008; Kurtz, 2004b). On the other hand, the legalization of same-sex

[1] By the end of 2008, Congress and forty states had enacted such acts (Stateline.org, 2009). Thirty states also had constitutional amendments that specifically defined marriage as the union between a man and a woman, with voters in Arizona, California and Florida approving constitutional bans on same-sex marriage in November 2008.

marriage could reignite the interest in the institution of marriage or reduce the pressure on government and employers to provide marriage-like benefits to cohabiting couples, actually leading to more different-sex marriages (Safire, 2003; Rauch, 2004).

The empirical evidence on the effect of opening marriage to same-sex couples 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.[2] 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 the fact that they cannot provide an answer to the question of whether same-sex marriage would induce different-sex couples to marry less. Langbein and Yost (2009) estimate fixed-effects 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 inn The Netherlands. There are several reasons for this choice. While not the first country to create a legal arrangement for same-sex couples,[3] The Netherlands is the first country to allow same-sex couples to marry (in 2001), offering the longest time-series of same-sex

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

[3]The first country to extend marriage-like rights to same-sex couples is Denmark with the creation of registered partnership, only for same-sex couples, in 1989.

marriages. Second, the Dutch legislature introduced in 1998 registered partnership, an institution identical to marriage in almost every respect but name and tradition 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. It also allows for a partial distinction between the effects of same-sex marriage versus granting same-sex couples the same rights and benefits 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 or 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 percent of the Dutch population. I focus on the first marriages of young Dutch and use a discrete-time duration model for age at first marriage to look into the effect

[4]In personal correspondence with the author, Boris Dittrich, former member and floor leader of the Dutch Parliament and a supporter of the same-sex marriage bill, recalls: "I distinctly remember my former colleague, Kees van der Staaij from the Orthodox Christian Party SGP, using those arguments. He even said that God would punish those who are destroying the institution of marriage between a man and a woman. [...] That night of the debate (we were debating same-sex marriage for two full days) I drove home very carefully. I thought: if I will get into a car accident tonight, people will think God has punished me."

of the registered partnership law and of the same-sex marriage law on the first-marriage rate. My estimates from specifications with unobserved heterogeneity replicate the findings using aggregate data of an increase in the marriage rate after the introduction of registered partnership and a decline after the same-sex marriage law, both insignificant in the sample of men.

Even if the average effect of the two laws is insignificant, there may be differential responses across various groups in the population. In particular, I find statistically significant and different effects in samples stratified by region of residence and by ethnicity, two potential indicators of conservative views. Individuals living in more conservative municipalities (the Dutch Bible belt) and those from more conservative ethnicities (Turks, Moroccans, and other non-Westerns immigrants) tend to marry more after each of the two laws, a pattern consistent with "reclaiming the institution of marriage." In contrast, individuals residing in more liberal municipalities (the four largest cities) tend to marry less after each law, consistent with them learning over time about the alternative institution.

My findings indicate that neither the legalization of same-sex marriage nor the introduction of registered partnership have a significant negative effect on the Dutch different-sex marriage rate. These results should be taken with a grain of salt because of several limitations to the present study. First, I can only estimate the short-term effects of the two laws given how recently they were enacted. Second, the fact that the same-sex marriage law was introduced only 3 years after the registered partnership law prevents the separate identification of the short-term effect of the same-sex marriage law from the longer-term effect of the registered partnership law. 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 any social and insti-

tutional differences with The Netherlands. Despite these limitations, I believe this paper makes an important contribution to our understanding of marriage behavior in general, and to the same-sex marriage debate in particular, by providing the first causal estimates of the short-term effects of same-sex marriage laws on marriage behavior.

The rest of the paper proceeds as follows. The next section discusses some possible reasons for a link between same-sex marriage laws and different-sex marriage and the following section introduces the evolution of the legal background in The Netherlands. The aggregate-level analysis is presented in section 4 and the individual-level analysis, including the separate analysis by residence and ethnicity, is included in section 5. Section 6 concludes.

2 Conceptual framework

A priori, it would seem that economic theory cannot provide a link between the legalization of same-sex marriage to a change in the different-sex marriage rate. Since the different-sex and same-sex partner markets are presumably separate, it would be reasonable to assume that changes in one market would not affect the other one. A closer look however reveals that the link between the two markets lies in the fact that they make use of the same institutions once same-sex marriage is legalized.

The standard marriage model (Becker, 1973, 1974) compares the utility from marriage to the utility from being single, ignoring the possibility of cohabitation. Cohabitation has certain advantages over marriage, such as generally lower dissolution costs, but also disadvantages, e.g. fewer benefits provided by government or employers. Any policy or societal change that reduces the net benefits of marriage with respect to cohabitation would presumably lead to fewer couples getting married.

The possibility that same-sex marriage affects the relative value of marriage was raised at

various times in the same-sex marriage debate.[5] Several arguments can be used to justify this claim. First, marriage is an institution that produces certain social goods centered around childbearing and rearing and "genderless marriage" breaks this link, reducing and eventually eliminating these social goods (Stewart, 2008; Witherspoon Institute, 2008). As a result, the value of marriage would be lower for some different-sex couples, since marriage can be interpreted at least partly as a contract that allows men access to offspring (Edlund, 2005). Second, the legalization of same-sex marriage can shift social norms towards the acceptance of family arrangements other than marriage, removing the potential social stigma of cohabitation (Kurtz, 2004b). This leads to the cohabitation of couples "at the margin," those who prefer to cohabit but would marry in the presence of social stigma. Finally, marriage can be seen as an exclusive club, to which only certain couples (specifically, different-sex) have access. When other couples are allowed access, the "value of membership" (value of marriage) is reduced, leading certain couples to cohabit rather than marry.

It can also be argued that the legalization of same-sex marriage can have beneficial effects on the institution of marriage. For instance, it could reignite the interest in the institution of marriage, including by different-sex couples, leading to more marriages (Mello, 2004; Cahill, 2004; Safire, 2003). Rauch (2004) suggests that the relative value of cohabitation increased over time because of the pressure from cohabiting same-sex couples on government and on employers to provide them at least some of the same rights as marriage. Granting same-sex couples the right to marriage would then reduce the pressure on government and on employers

[5]For instance, Proposition 8 proponents contended that the legalization of same-sex marriage would reduce the value of marriage, resulting in fewer different-sex marriages. See "Defendant-intervenors Dennis Hollingsworth, Gail J. Knight, Martin F. Gutierrez, Mark A. Jansson, and Protectmarriage.com's answers to questions for closing arguments", available online at http://protectmarriage.com/files/DefendantsResponsestoClosingArgumentQuestions.pdf, and the transcripts of David Blakenhorn's testimony on January 26-27, 2010, available online at http://www.afer.org/our-work/hearing-transcripts/perry-trial-day-11-transcript/, both last accessed on February 14, 2012.

to provide these rights to cohabiting couples and lead to a falling value of cohabitation, implicitly changing the incentives of different-sex couples toward more marriage.

All these arguments suggest that the effect of same-sex marriage laws (if any) should be mostly observed on new marriages, particularly first marriages, but not necessarily on existing marriages.[6] This is due to the fact that most of the marriage-specific costs are already sunk at marriage. The arguments above also suggest that registered partnership laws should have similar effects, albeit of lower magnitude, as same-sex marriage laws.

3 The Dutch legal environment

The road to same-sex marriage in The Netherlands was long and bumpy.[7] At the beginning of the 1990s, gay rights organizations in The Netherlands tried to build on the success of their Danish counterparts, who had obtained the enactment of a registered partnership law in 1989, and started to push for the legal recognition of same-sex couples. The first move was to suggest in 1991 the creation of a symbolic registry, which could potentially evolve into an alternative to the marriage registry and to which municipalities would participate voluntarily. This suggestion was not against the general feelings of the population, as opinion polls showed that almost 53% of the population supported same-sex marriage in 1990, a share that increased over time to around 63% in 1991 to about 73% in 1995 (van Velde, 2005). More than 100 of the 650 Dutch municipalities decided voluntarily to participate within the first year, thus paving the way for the introduction of the registered partnership system.

The government set up a committee of legal advisers (the First Kortmann Committee) to

[6]There is an additional reason why it might be misleading to look at the evolution of divorces in the aftermath of same-sex marriage laws. If there is indeed an effect on different-sex marriage, then all the arguments above except for the one present by Rauch (2004) imply that only couples who have relatively stronger reasons to marry would choose to do so, thus influencing the divorce rate.

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

inquire into the legal effects and the desirability of the legal recognition of same-sex couples. After the committee recommended the introduction of a Danish-style partnership, a bill for the new institution was introduced in Parliament in 1993, but was held up because of the 1994 elections. The new governing coalition did not include the Christian Democrats, the largest party opposing same-sex marriage, and the new cabinet presented in 1995 a white paper suggesting the introduction of registered partnerships for same-sex couples and, in a departure from the Danish model, for different-sex couples. The reasoning was that the new institution should not discriminate based on sexual orientation and that different-sex couples not willing to marry should have access to this alternative contract (Merin, 2002).

Therefore, registered partnership was designed to be an almost perfect substitute to marriage. Waaldijk (2004) compares the two institutions based on their "levels of legal consequences," i.e., the rights and obligations derived from each contract, and finds only three differences for different-sex couples. First, a married man is automatically acknowledged as the father of a child born to his wife, whereas a man in a registered partnership has to explicitly claim the child before or after birth (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 are prohibited from engaging in international adoption. This restriction can be circumvented by one partner adopting the child as a single individual and the other partner subsequently adopting the child as the partner of the adoptive parent.

Since the planned legislation granted same-sex couples access only to registered partner-

[8]Starting from 2001, married couples can change their marriage to a registered partnership. Statistics Netherlands reports that in more than 90% of the cases this is part of a two-step procedure commonly called "flash divorce," with the second step a partnership dissolution by mutual agreement. This is a cheaper alternative to divorce when the parties agree on the division of common property. To avoid this issue, I only consider new partnerships in the empirical analysis.

ship, there was an argument that same-sex couples still face discrimination and a motion calling for the opening of civil marriage to same-sex couples was introduced in 1996. While the registered partnership bill was making its way through Parliament's two chambers, the government acknowledged the request to open up marriage and appointed 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. In particular, it addressed the issue of a possibly 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) Still, the government opposed same-sex marriage in its February 1998 answer, which prompted another request in the second chamber of Parliament for new legislation to allow same-sex couples to marry. The 1998 elections kept the same coalition in power and the ensuing negotiations for a new cabinet led to an agreement on the introduction of a same-sex marriage bill during that term. Finally, in 2000, the bill legalizing same-sex marriage was introduced in Parliament and was approved in September by the lower chamber and in December by the upper chamber (Merin, 2002). On April 1, 2001, The Netherlands became the first country in the world to allow same-sex couples to marry.

In conclusion, the road to same-sex marriage was long and uncertain: starting from 1998, both same-sex and different-sex couples could form registered partnerships, and from 2001 same-sex couples are allowed to enter marriage. Some scholars argue that the success of the gay rights movement in The Netherlands was due to this small-steps approach (Waaldijk,

2001). For the purpose of this paper, the uncertainty related to the timing of the laws makes them "exogenous" to individual marriage decisions, as individuals could not perfectly anticipate the enaction date of each law and marriages would have already been 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 can be used to construct a counterfactual marriage behavior of Dutch residents in the absence of the laws. At the same time, using a different country as a counterfactual could be problematic because of differences in the determinants of marriage behavior.[9] In the absence of an obvious control group, Abadie and Gardeazabal (2003) and Abadie et al. (2010) suggest a method for creating an artificial control. This synthetic control a weighted average of potential "donor" countries such that the average marriage rate and the averages of a set of determinant variables closely match the corresponding numbers for The Netherlands during the period before 1998 (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 $Z_1 = (\overline{X}_1', \overline{m}_1)'$ and $Z_0 = (\overline{X}_0', \overline{m}_0)'$, where the overline represents means over the pre-intervention period.

[9] For geographic and demographic reasons, Belgium would have been the obvious choice for a control country. However, Belgium followed closely in the footsteps of The Netherlands, enacting a registered partnership law in 2000 and a same-sex marriage law in 2003.

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

The synthetic control is the set of weights that minimize the weighted distance between the pre-intervention averages of the marriage rate and its determinants for The Netherlands and for synthetic Netherlands:

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

where V is an arbitrary diagonal matrix of variable loadings. Similar to Abadie et al. (2010), the loading matrix is chosen such as to minimize the mean squared error of the synthetic marriage rate in the pre-intervention period:

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

The weights are restricted to lie in the unit interval and to sum up to one, resulting in a synthetic control that is unlikely to match perfectly the trend in the marriage rate before the two laws. More details on the procedure are provided in appendix A1.

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 their determinant variables closely match the corresponding variables in The Netherlands during the pre-intervention period. These weights are the result of a two-step maximization procedure. In the first step, each variable is assigned a loading and the country weights are calculated as a function of these loadings so as to minimize the weighted distance between the synthetic control and The Netherlands. In the second stage, the variable loadings are chosen such that the synthetic marriage rate matches the actual marriage rate in The Netherlands as closely as possible, and the two steps are repeated until convergence is achieved. The synthetic control created in this manner takes into account both the observable and

the unobservable determinants of the dependent variable, thus producing an appropriate counterfactual for the evolution of the marriage rate in the absence of the two laws.

4.2 Data

The list of potential donors includes all the OECD member countries that did not enact a registered partnership or same-sex marriage law during the period 1988–2005. Despite the likely differences in the rights offered to couples in non-marital arrangements between these countries and The Netherlands, this is arguably the set of most comparable countries to The Netherlands. The data on marriage rates and their determinants come from OECD, Eurostat or national statistical offices. After excluding Mexico and the Slovak Republic due to data availability, the sample of potential donors consists of 16 countries: Australia, Austria, Czech Republic, Greece, Hungary, Ireland, Italy, Japan, Korea, New Zealand, Poland, Portugal, Switzerland, Turkey, the United Kingdom, and the United States.[11]

The ideal measure of the marriage rate would use only the population at risk, i.e., single individuals legally allowed to marry. However, statistics agencies commonly report a different measure, the crude marriage rate, defined as the total number of marriages per 1,000 inhabitants. However, as figure A1 in the appendix shows, both measures follow similar patterns in The Netherlands, even though the "correct" marriage rate presents smaller relative increases and larger relative declines. Thus, the crude marriage rate seems to be an reliable indicator of the evolution of the "correct" marriage rate.

The construction of the synthetic control is based on averages of the determinant variables and of the marriage rate over the period 1988–1997. The variables included in the vector of determinants X can be classified in three groups. The first group gathers variables that

[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.

describe 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 (the ratio of men to women in the population), the age at first marriage of both men and women, and the life expectancy of both men and women. The second set of variables describes the attractiveness of individuals in the marriage market: the share of girls in total enrollment in secondary and in tertiary education, and the total fertility rate (the average number of children that would be born to a women of bearing age). 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 fraction of people who agreed with the statement "Marriage is an out-dated institution" in the World Values Survey to account for the long-term trend in the attitude toward the institution of marriage.[12] A list of the data sources for each country is provided in table A1 in the appendix.

The first two columns in table 1 list the mean of each variable for The Netherlands and for the potential donors as a group (the unweighted average) for the period between 1988 and 1997. The relative differences in these means range from relatively small (sex ratio or urban population) to relatively large (GDP per capita, attitude toward marriage), suggesting that the unweighted average of the potential donors might not be an appropriate control group and hence the need for the synthetic control.

[12]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 average number of babies born per 1,000 inhabitants), the share of girls among students enrolled in primary education, the labor force participation rate of both men and women in the 25–34 age group, the difference in life expectancy between men and women, the inflation rate, and the rate of GDP growth.

4.3 Results

The means of all the variables for the synthetic control are listed in column 3 of table 1. 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, supporting the idea that synthetic Netherlands is a more appropriate control group. In order to assess the relative importance of each variable in the construction of the synthetic control, column 4 lists the loadings rescaled so as to sum up to one. The table indicates that the variables with the most predictive power are the life expectancy of men and the share of girls among tertiary education students, followed by the unemployment rate of 25–34 year-old men and the age at first marriage of men.[13]

Two additional measures of marriage behavior are worth studying. First, it is interesting to look separately at the different-sex marriage rate since the arguments in section 2 refer to the behavior of different-sex couples. Second, if registered partnership is perceived as a near-perfect substitute for marriage, then some different-sex couples would choose it over marriage. It could then be argued that the rate of unions (marriages and registered partnerships) should matter rather than only marriages. Note that the three 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 crude (overall) marriage rate in The Netherlands and synthetic Netherlands, while figure 2 plots the different-sex marriage rate (panel a) and 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 calculate the weights for the

[13] In particular, the marriage views variable, which has the largest difference between The Netherlands and synthetic Netherlands, does not seem to contribute much to the matching procedure.

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 whether the actual and synthetic rates are significantly different after 1998, I conduct permutation (placebo) experiments (Abadie et al., 2010). In these experiments, 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. In order for the placebo tests to be meaningful, I restrict the analysis to the cases where the actual and synthetic marriage rates are reasonably close in the pre-intervention period based on a comparison of mean square prediction errors ($MSPE$).[14] Figure 3 plots the gaps between the three measures of union formation and the synthetic marriage rates for The Netherlands (the dark lines), and between the actual and synthetic marriage rates for donors with $MSPE$ at most 5 times (panel a) or 2 times (panel b) as large as for The Netherlands (the shaded lines). As it can be seen from these graphs, the gap for The Netherlands is always within the range produced by the placebo tests. In other words, if a random country were picked from these restricted donor pools, the chance of finding an actual-synthetic gap in the post-1998 marriage rate that is as large as the one in The Netherlands is 10% (corresponding to the 10 solid lines in panel a) or 16.7% (corresponding to the 6 solid lines in panel b), levels similar to the ones used in common statistical tests. Therefore, we can conclude that the evolution of the Dutch marriage rate, whether overall or only for different-sex couples, was not statistically different after the enactment of each law from its evolution in the absence of these laws.

[14]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:

$$MSPE = \frac{1}{T_0}\sum_{t=1}^{T_0}(m_{1t}^{*} - m_{1t})^2.$$

A second way to gauge the statistical significance of the actual-synthetic difference is to use the ratio of post- to pre-intervention MSPE for the full sample of donors. Figure 4 plots the distribution of this ratio when the post-intervention is 1998–2000, after the introduction of registered partnership (panel a) or 2001–2005, after the legalization of same-sex marriage (panel b). In both cases, the pre-intervention period is 1988–1997. The Netherlands finds itself in the middle of the distribution in both graphs. The interpretation of these graphs is that if the intervention, registered partnership law or same-sex marriage law, were assigned randomly to a country in the sample, the probability of observing a pre-post relative difference in the marriage rate at least as large as in The Netherlands would be approximately 35%, corresponding to 6 countries (5 donors and The Netherlands) out of 16 having a MSPE ratio as high as The Netherlands, both in the case of the registered partnership law and in the case of the same-sex marriage law.[15] This probability is higher than the standard significance levels used in statistical tests, suggesting once again that the evolution of the Dutch marriage rate after the enactment of the two laws was not statistically different from its evolution in their absence, whether for all or only different-sex couples.

The particular setup of the two laws and the fact that they apply to the entire Dutch population make it impossible to separate the long-term effects of the registered partnership law (beyond the first three years) from the short-term and long-term effects of the same-sex marriage law. However, if there are effects from the introduction of registered partnership or same-sex marriage on the institution of marriage, it is likely that they act in the same direction (see section 2). In this case, the insignificant difference between the actual and synthetic marriage rates after 2001 suggests that each of the two effects is insignificant.

[15] In the case of the different-sex marriage rate, the probability is the same, 35%, while in the case of different-sex unions, the probability is 23% for the period 1998–2000 and 41% for the period 2001–2005. If the post-intervention period is the entire 1998–2005, the probability is approximately 41% for all three measures.

5 Individual-level analysis

5.1 Empirical strategy

The individual-level analysis can mirror the aggregate analysis with an estimating equation of the type:

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

where m_{is} is an indicator for individual i marrying during year s, X_{is} is a vector of observable and potentially time-varying characteristics, θ_i represents the time-invariant unobserved characteristics of the individual, and the main variables of interest are RP_s and $RPSM_s$, two dummy variables for the periods following the each law: 1998–2000 for the registered partnership law (RP_s) and 2001–2005 for the same-sex marriage law ($RPSM_s$).

In particular, I confine the analysis to never-married individuals, and thus first marriages, because previously-married individuals might already have their own evaluation of the value of marriage that might not be influenced by changes in its definition. This is not restrictive, as figure A2 in the appendix shows that the variation in the total number of marriages is driven by the variation in the number of first marriages for either spouse. The baseline specification can then be written in terms of the probability of marriage conditional on the individual having never married by the beginning of the year:

$$P(m_{is} = 1 | m_{is-1} = 0) = h(X_{is}, p_{1s}, p_{2s}; \theta_i), \tag{1}$$

where individuals are included in the sample for as long as they are not married and they leave the sample once marriage occurs.

The model in equation (1) has the structure of a discrete-time duration model with

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; Eberwein et al., 2002), I assume that the hazard function $h(\cdot)$ has the logit form:

$$h(X_{it}, p_{1t}, p_{2t}; \theta_i) = P(T_i = t | T_i \geq t) = h_i(t; \theta_i) = \frac{1}{1 + \exp\{-y_i(t; \theta_i)\}}, \tag{2}$$

where

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

s is a linear trend, θ_i follows a discrete distribution with two mass points (Heckman and Singer, 1984), and the function $\gamma(\cdot)$ represents the form of duration dependence, i.e., the common way age influences the probability of marriage for any given person. Since Eberwein et al. (2002) argue that the actual functional form does not seem to influence the results as long as it is flexible, I use the highest degree polynomial in $\ln(t)$ supported by the data.[16]

The first coefficient of interest, λ_1, captures the short-term effect of the registered partnership law on the age-specific (conditional) probability of marriage during the 1998–2000 period as compared to the period before the enactment of either law (before 1998).[17] The

[16] The most flexible form of duration dependence is a step function represented by an exhaustive set of age dummies, but previous studies found that this type of duration dependence leads to identification problems when unobserved heterogeneity is also modeled non-parametrically (Narendranathan and Stewart, 1993). The degree of the polynomial $\gamma(\cdot)$ is driven by the data in the sense that, as suggested by Eberwein et al. (2002), I add higher order terms until they become insignificant. This procedure yields a fourth-order polynomial in $\ln(t)$, where $t = age - 17$. In a model without unobserved heterogeneity, a specification with this type of duration dependence yield almost identical estimates to a specification with a full set of age dummies (available upon request).

[17] Technically, these coefficients represent effects on the age-specific first-marriage rate. However, 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, in the rest of the paper I will use the loose interpretation of the coefficients in equation (3) as effects on the marriage rate.

second coefficient, λ_2, includes both the long-term effect of the registered partnership law and the short-term effect of the same-sex marriage law as compared to the same 1995–1998 period. The inclusion of a linear trend in the specification implies that the effects are measured as deviations from the long-term trend in the marriage rate.

The identifying assumption is that the two laws have only *level* effects (that is, they do not change the trend in the marriage rate) and thus the marriage behavior during the period 1995–1997 is an appropriate counterfactual for the marriage behavior in the absence of the two laws once the long-term trend is accounted for.[18] This assumption is inherently untestable, but two arguments can be brought 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, some suggestive evidence can be provided using the synthetic marriage rate constructed in section 4. If the pre-1998 marriage behavior is an appropriate counterfactual for the post-1998 marriage behavior in the absence of the two laws, then the actual pre-1998 marriage rate and the synthetic post-1998 marriage rate should have a similar evolution. This is confirmed by figure A3 in the appendix, 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:

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

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

[18] This specification implies a piece-wise linear trend with the same slope over two periods of 3 years (1995–1997 and 1998–2000) and one of 5 years (2001–2005). While a more flexible approach would include a non-linear trend or allow for different slopes in each period, it would also eliminate most of the variation in the marriage rate given the short period of time.

age of the individual, and

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

This likelihood is maximized in a flow sample of individuals, i.e., a sample of individuals who become at risk of marriage (turn 18) during the sample period, since a random sample of unmarried individuals (a stock sample) oversamples individuals who prefer to marry late and produces biased estimates (the initial conditions problem).[19]

5.2 Data

I construct the data using ten waves of the restricted version of the Dutch Labor Force Survey (1996–2005) and the January 2006 snapshot of the confidential Dutch Municipal Records. The Labor Force Survey (*Enquête Beroepsbevolking*, EBB) is an annual cross-sectional random survey of the population 15 years of age and older. It includes information on educational attainment, ethnicity, employment and other demographic and labor market characteristics at the time of the interview. In addition, the restricted version provides a unique identification number that can be used to match the individuals to other data sets maintained by Statistics Netherlands. The combined ten waves of the survey yield information on almost 950,000 individuals, or approximately 6 percent of the average Dutch population over the period 1995–2005.

The Dutch Municipal Records (*Gemeentelijke Basis Administratie*, GBA) include de-

[19] 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 decline in the marriage propensity of individuals of a certain age before and after the enactment of each law.

tailed information on changes in the marital status and residence of the entire resident population for the period between January 1, 1995 and January 1, 2006. Using the unique identification number, the individuals included in the ten waves of the Labor Force Survey are matched to their marriage and residence history for the entire 1995–2005 period. The resulting longitudinal data set includes information on ethnicity, marital status and residence over the whole sample period, and on educational attainment and school enrollment at the time of the Labor Force Survey interview. Finally, the data is augmented with the yearly unemployment rate at the regional level provided by Statistics Netherlands.[20]

As in the aggregate-level analysis, the control variables included 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 long-term evolution of the view on the institution of marriage (linear trend). To increase the probability that the highest educational level reported, completed or in progress, does not change over the sample period, I restrict the sample to individuals at least 20 years old during the year of the interview.[21]

Similar to the aggregate-level analysis, time is measured in calendar years because it is practically impossible to measure certain variables such as the regional unemployment rate on a different scale than the calendar year. Moreover, the strong seasonal pattern in marriages within a calendar year suggests that marriage decisions are commonly based on the calendar year. I measure age in full years at the end of the calendar year, so that individuals who

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

[21]About 15 percent of the sample was still enrolled in some form of education at the time of the survey (approximately 9 percent full-time and 6 percent part-time). A small fraction of the individuals still in school were enrolled in a lower educational level than their highest level completed (for example, persons with a college degree in science enrolled in professional economics or business courses). The highest of the two educational levels was used in these cases. The Dutch education system, the flows among different educational attainments and the grouping of educational attainments used are shown in the appendix figure A6.

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 average over the calendar year.

Using an administrative data set reduces or eliminates measurement error but also presents several challenges. 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 Labor Force Survey, including the spouses of individuals in the sample. Finally, the coding of addresses changed over time and was aggregated at the street number level since 2003. As a result, it is virtually impossible to identify the spouse of all individuals, even if the information for that person were available, and I am unable to distinguish between same-sex marriages and different-sex marriages. This induces an small upward bias in the estimate of the different-sex marriage rate after 2001.[22]

The empirical analysis requires a flow sample because of the initial conditions problem (see section 5.1). However, the sample cannot be restricted only to individuals who turn 18 between 1995 and 2005 because of the high average age at first marriage in The Netherlands (from 29.6 in 1995 to 32.4 in 2005 for men, and from 27.4 to 29.7 for women over the same period). I include all individuals who are first observed within an age interval with a probability of still being single close to one. Based on the aggregate distribution of Dutch marriages by age, these intervals are 18–24 years old for men and 18–22 years old for women, accounting for about 10 percent of first marriages between 1995 and 2005, respectively.

I conduct the analysis separately by gender because of different marriage behaviors. Specifically, men tend to marry later than women by approximately 2 years and women are more likely to marry previously-married opposite-sex partners than men. The final sample

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

includes 70,718 men and 53,883 women. Note that, because of the reasons mentioned above, there is not a one-to-one relationship between marriages in the sample of men and in the sample of women. In addition, since women tend to marry younger, there will be more never-married men than women in every age group. Descriptive statistics for each sample are listed in table 2. All the statistics and the subsequent analysis use the sample weights provided in the Labor Force Survey.[23]

Not surprisingly, the average person in the sample is much younger than the average person in the population. The average age at first marriage is around 27 years for men and 25 years for women, significantly lower than the number in table 1. However, a majority of the sample does not marry before the end of the sample period, as evidenced by the Kaplan-Meier estimates in appendix figure A5. Overall, only 26.29 percent of men and 33.30 percent of women marry during the sample period.

The distribution of education is skewed toward higher levels of education, mostly professional degrees, but seems consistent with the shares of girls in secondary and tertiary education in table 1, which suggest that men are relatively more likely to be university graduates than women. Approximately 83 percent of the sample are natives and almost 8 percent are Western immigrants, i.e. people from Europe (except Turkey), North America, Oceania (including Australia and New Zealand), Japan, and Indonesia. Immigrants from potentially more conservative areas such as the predominantly Muslim countries Turkey and Morocco, or Aruba and Suriname, account for about 6 percent of both men and women.

The fraction of the sample living in an urban area at entry into the sample is slightly lower than in the whole population. One explanation is that single people tend to move to

[23]The sample weights refer to the year of the interview. Under the assumption that the structure of the population does not change significantly during the study period, I rescale the weights to represent the probability of interview relative to the entire sample of ten waves of the Labor Force Survey.

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 advantage of the individual-level data is that I can exploit the geographic heterogeneity with respect to attitudes toward marriage and cohabitation. One area of interest groups the four largest cities (Amsterdam, the Hague, Rotterdam and Utrecht) and exhibits low rates of fertility and marriage and low church attendance, as well as high rates of divorce and non-marital birth. A second area is the so-called Dutch Bible belt (*De Bijbelgordel*), a set of municipalities characterized by relatively high church participation and fertility rates, together with low rates of cohabitation, divorce and non-marital birth (Sobotka and Adigüzel, 2002; de Jong, 2003). I identify the municipalities in the Bible belt as those 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, based on data from the Dutch Electoral Council (*de Kiesraad*).[24] The 32 municipalities in the Bible belt are listed in the appendix table A2 and are shown together with the four largest cities on the map in appendix figure A7.

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

[24]The four conservative Christian 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). These parties obtained 5.17% of the votes at the national level and won 8 out of 150 seats.

include indicators for residence in one of these two areas among the control variables.

5.3 Results

I first estimate a discrete-time duration model without unobserved heterogeneity, in which the term θ_i is constant. The estimation results, separately for men and for women, are listed in columns 1 and 3 of table 3. I then estimate models including an unobserved heterogeneity term drawn from a discrete distribution with two mass points, shown in columns 2 and 4. Note that only coefficient ratios can be compared across specifications, not their levels, since changing the distribution of unobserved heterogeneity from a constant to a discrete distribution changes the variance normalization of coefficients in logit models (Mroz and Zayats, 2008; Nicoletti and Rondinelli, 2010).

As expected, not controlling for unobserved heterogeneity (columns 1 and 3) attributes the decline in the marriage rate to the two laws. The estimates show a drop in the marriage probability in each of the two periods for both men and women. Once the selectivity of the sample is taken into account (columns 2 and 4), the estimates suggest that there is no negative change in the marriage behavior of individuals after the enactment of each of the two laws, 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 in section 4, particularly in the case of women. If most of the variation in the overall marriage rate is due to first-marriage behavior, the relative change in the first-marriage rate of young Dutch will be larger simply because the denominator is smaller (in this case, the never-married youth). It should not be surprising that the increase

in the marriage rate after the registered partnership law is insignificant in the aggregate, but significant in the individual data. Still, it is apparent that young women seem to be more responsive to changes in their environment, with larger relative increase and decline in their marriage hazard (relative to the long-term trend) than men.

Some other results from these regressions are worth noting. As expected, most of the patterns in the explanatory variables do not change with the inclusion of unobserved heterogeneity. Not surprisingly, the estimates point to a negative long-term trend in the marriage rate for both men and women and to a negative effect of economic downturns. The relationship between education and marriage varies by sex.[25] For men, there is an almost inverted U-shaped pattern, with the least and the most educated having lower propensities to marry. For women, the relationship is almost linear and decreasing, as lower educated women have higher propensities to marry and higher educated women are much less likely to marry. These patterns are consistent with female hypergamy (women "marry up") and male hypogamy (men "marry down"), such that lower educated women have the best chance and lower educated men have the worst chance of finding a spouse.

Finally, the estimates suggest that certain groups in the population have higher or lower propensities to marry irrespective of gender. For instance, compared to natives, people from the more conservative Turkey and Morocco or from non-Western countries of origin are more likely to marry. Similarly, individuals residing in the Bible belt have significantly higher marriage probabilities. On the other hand, individuals from a Western background or from Suriname and Aruba are less likely to marry, as are individuals living in urban areas, particularly in one of the four largest cities. These different patterns suggest that these groups could have different views on the institution of marriage and, as a result,

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

their behavior could be impacted differently by the two laws. In the next section, I study separately how their marriage decisions evolved after each of the two laws.

5.4 Heterogeneous effects

To take into account the geographic differences in marriage behavior in response to the enactment of the two laws, I modify the hazard function in equation (3) as follows:

$$y_i(t;\bar{\theta}_k) = \bar{\theta}_k + X'_{it}\beta + \gamma(t) + \sum_j \left(p_{1t}D^j_{it}\lambda^j_1 + p_{2t}D^j_{it}\lambda^j_2 + s_t D^j_{it}\lambda^j_3\right), \tag{4}$$

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

The results are listed in table 4, separately by sex, and they indicate that individuals living in the Bible belt have a higher tendency to marry than those in the rest of the country, who in turn are more likely to marry than people residing in the four largest cities. A similar pattern holds with respect to the long-term trend in the marriage rate, which falls the fastest in the four largest cities and the slowest in the Bible belt.

The response to the two laws was markedly different across the three regions. The marriage rate increased in the Bible belt after the registered partnership law and then even

more after the same-sex marriage law (recall that λ_1 and λ_2 measure the difference with respect to the period before 1998). In contrast, individuals living in the four largest cities married less after the registered partnership law and even less after the legalization of same-sex marriage. Finally, individuals living in the rest of the country seem to 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. The hazard function is of 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, and other non-Western immigrants) and D_i^j is a dummy for individual i being of ethnicity j. The coefficients of interest, λ_1^j and λ_2^j, represent the change in the propensity to marry of individuals of ethnicity j between 1998–2000 and between 2001–2005, respectively, compared to the period 1995–1997 for the same ethnic group. Finally, λ_3^j represents the long-term decline in the propensity to marry for ethnicity j.

Table 5 presents the results separately by sex. Men who are presumably more conservative than the average native Dutch person (Turks/Morrocans and other non-Western immigrants) and Surinamese/Arubans seem to marry more after the registered partnership law and then at least as much after the same-sex marriage law. Dutch native and Western immigrant men, on the other hand, tend to have a behavior more similar to the majority of the Dutch population, with a marriage rate that is around or higher than its long-term trend after the registered partnership law and then falls after the same-sex marriage law. For women, all immigrant ethnic groups experience an increase in their propensity to marry after the registered partnership law and then a larger or smaller decline after the 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 support the idea that there is significant variation in the response to the enactment of the two laws. Individuals who can be considered more conservative, such as those residing in the more conservative municipalities included in the Bible belt and those of non-Western ethnicities, seem to be affected by each law, having marriage probabilities above their long-term trend in each period after 1998. This type of behavior would be consistent with them reclaiming the institution of marriage, along the lines of Akerlof and Kranton's (2000) identity theory. On the other hand, presumably more liberal individuals such as those living in the four largest cities marry less after the introduction of each law, consistent with the idea that they learn over time about the availability of an alternative institution.[26] Overall, I do not find evidence of negative effects of the legalization of same-sex marriage or of the introduction of registered partnership for the majority of the Dutch population.

6 Conclusions

This paper contributes to the debate over the legalization of same-sex marriage by providing the first causal estimates to answer a question that has been raised numerous times: would opening the institution of marriage to same-sex couples have negative consequences for the institution of marriage, and in particular for different-sex couples? To answer this question, I focus on the Netherlands, the first country to legalize same-sex marriage in 2001, a country that also introduced an alternative institution in 1998, identical in almost all ways to marriage—registered partnership. I first analyze the evolution of the marriage rate at the aggregate level using the synthetic control method developed by Abadie and Gardeazabal

[26] 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.

(2003), separately for all marriages and different-sex marriages. I then use individual-level data to study the first-marriage behavior of young Dutch. In both cases, I find that neither the introduction of registered partnership nor the legalization of same-sex marriage had a (statistically significant) negative impact on the different-sex marriage rate. The response to the two laws varied across regions and ethnicity within the Dutch population. Individuals that are potentially more conservative than the average Dutch person, such as those residing in the Dutch Bible belt or those of Turkish/Moroccan or other non-Western ethnicities, tend to marry more in the period following each of the two laws, consistent with a reclaiming of the institution of marriage. On the other hand, individuals presumably more liberal than the average Dutch, such as those residing in the four largest cities, tend to marry increasingly less after each of the two laws, a behavior consistent with learning over time about the availability of the alternative institution of registered partnership. Overall, my results indicate that there was no negative effect from the legalization of same-sex marriage in the Netherlands, whether one focuses on the overall or different-sex marriage rate.

There are three caveats to the present study. First, any analysis can inform us only on the short-term effects of the two laws because they were only recently enacted. 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, since the same-sex marriage law was introduced only 3 years after the registered partnership law. To the extent that these two effects have the same sign, my results suggest that both are statistically insignificant. Finally, one would need to take into account the social and institutional differences between other countries and the Netherlands if they want to extrapolate the conclusions of this study. Despite these limitations, I believe these results make an important contribution to our understanding of marriage behavior and to the same-sex marriage debate.

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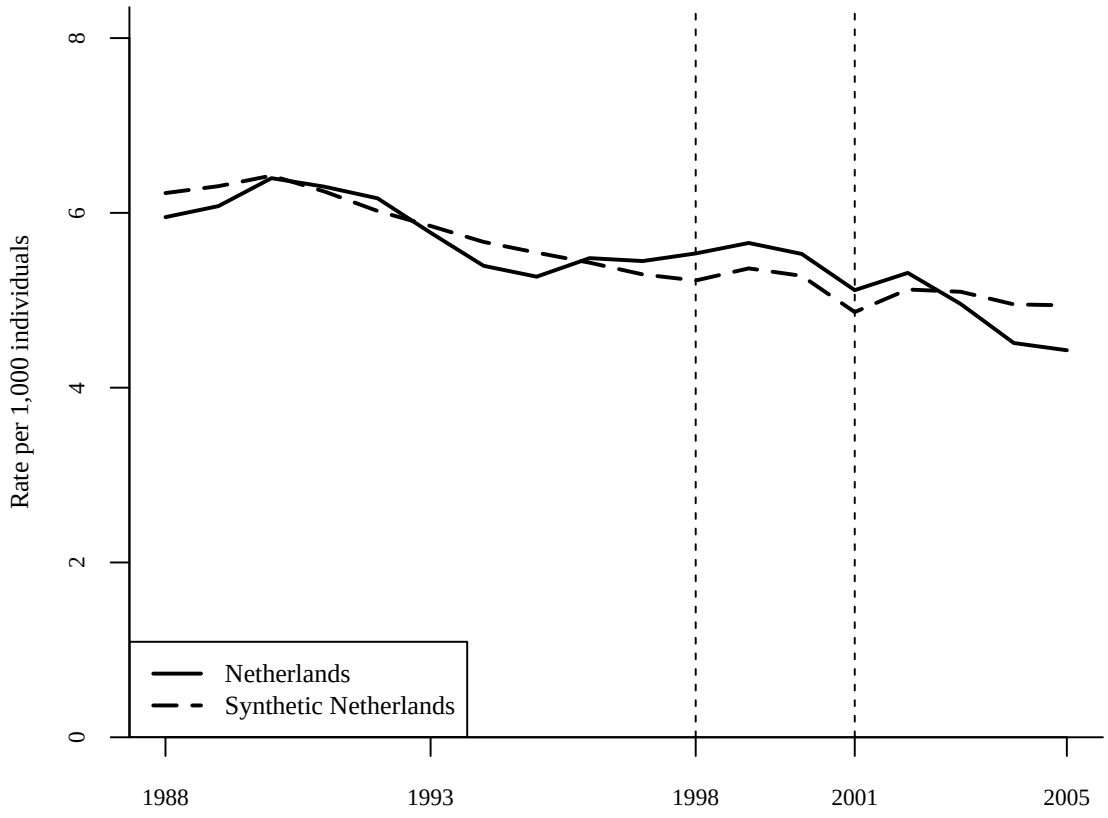
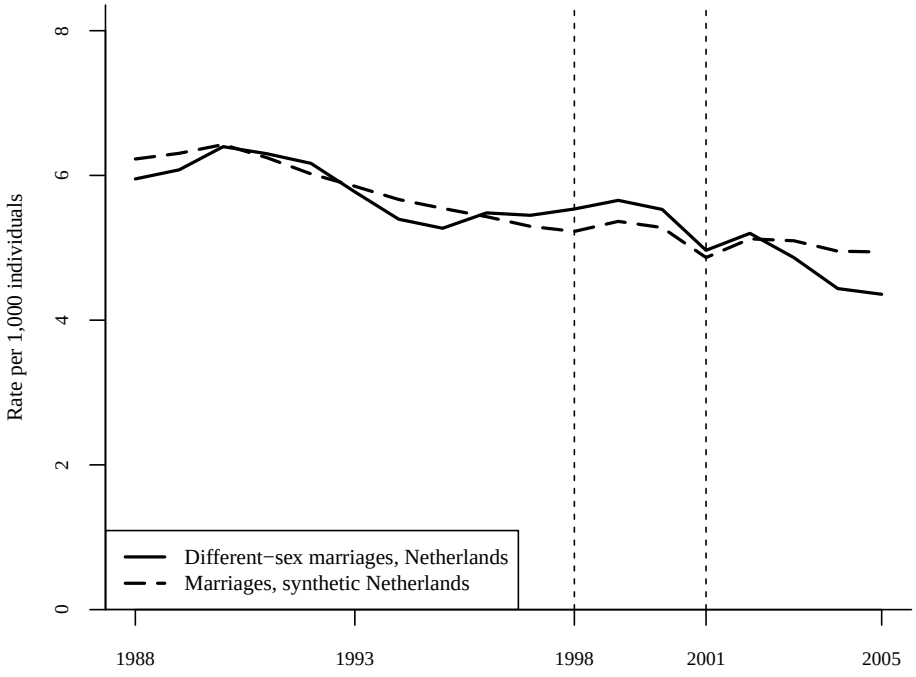
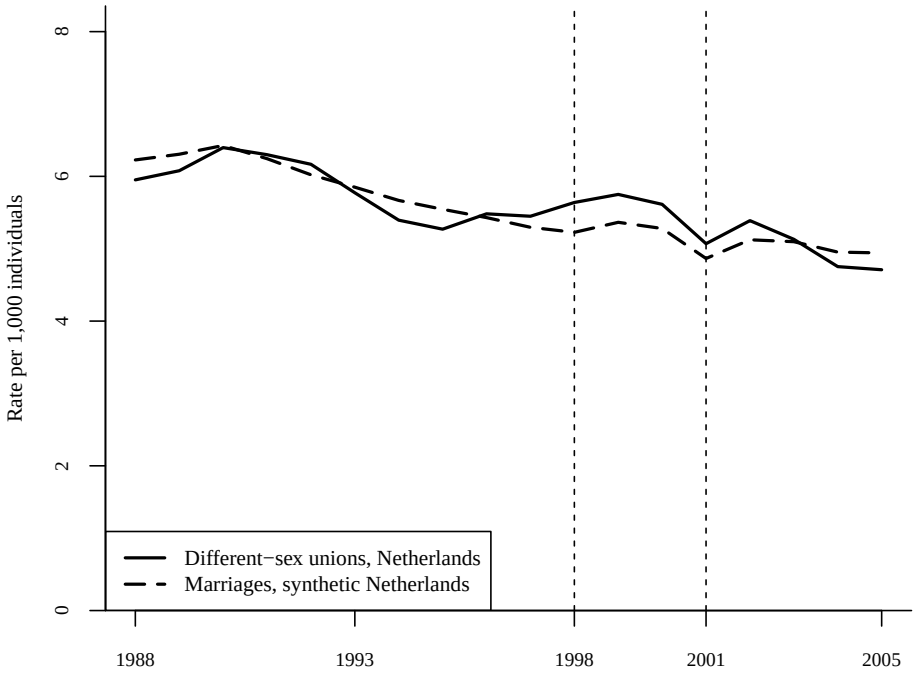


Figure 1: Evolution of marriage rate in The Netherlands and in the synthetic control

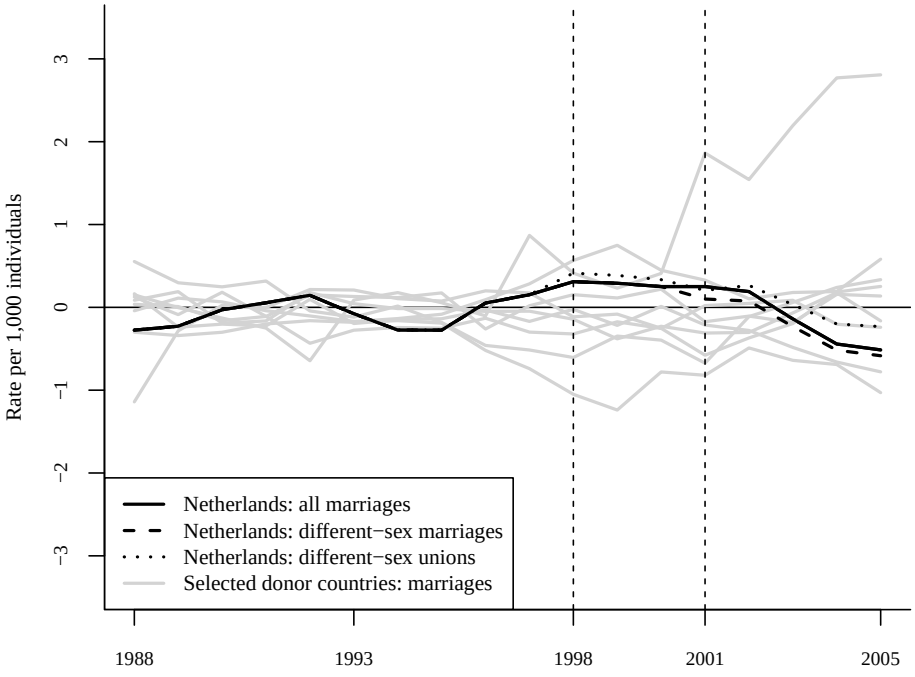


(a) Different-sex marriage rate

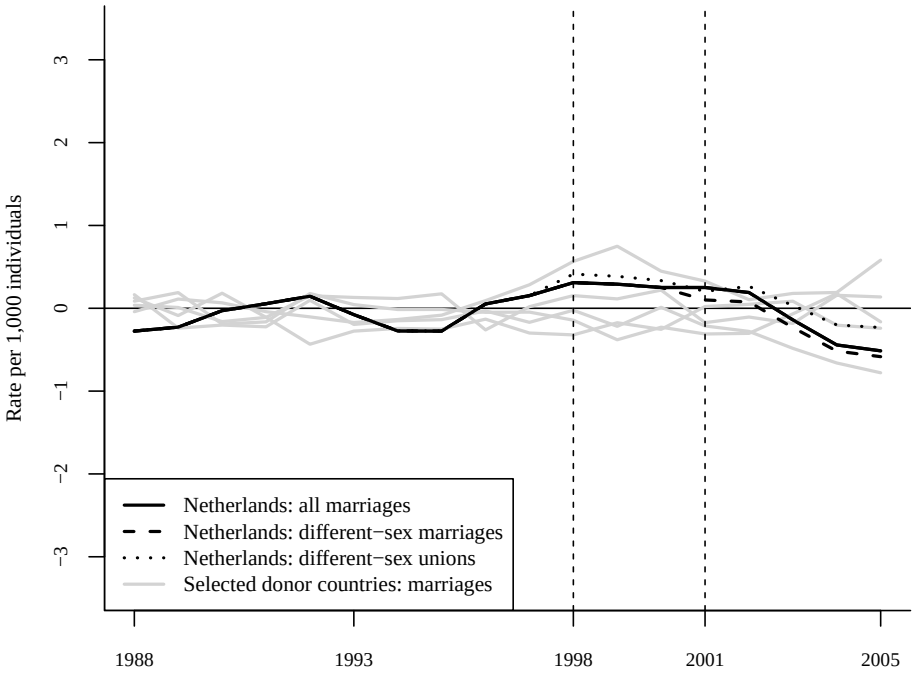


(b) Different-sex union rate

Figure 2: Alternative measures of different-sex union formation

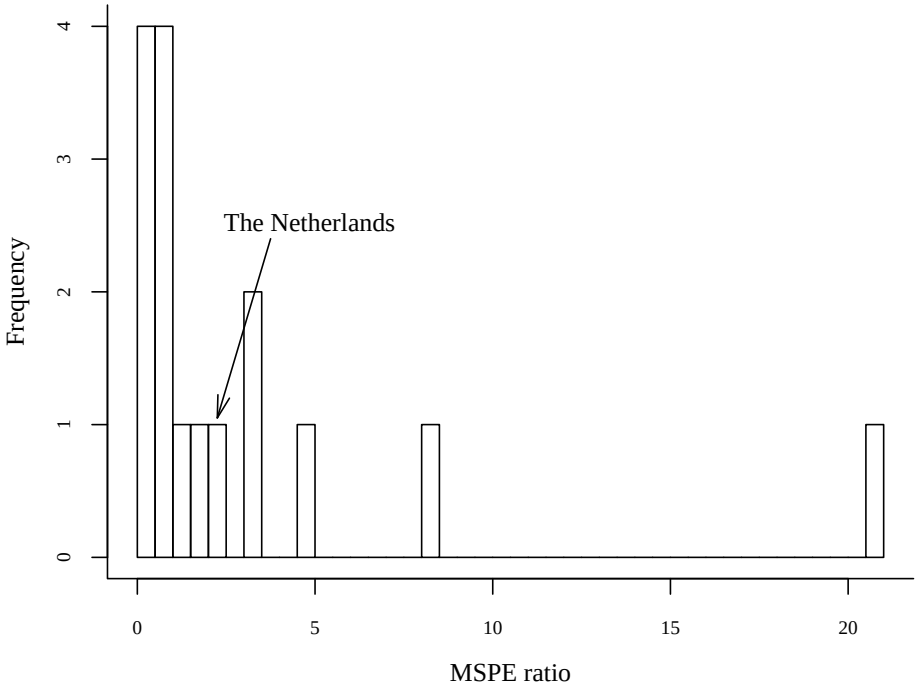


(a) At most 5 times the MSPE of The Netherlands

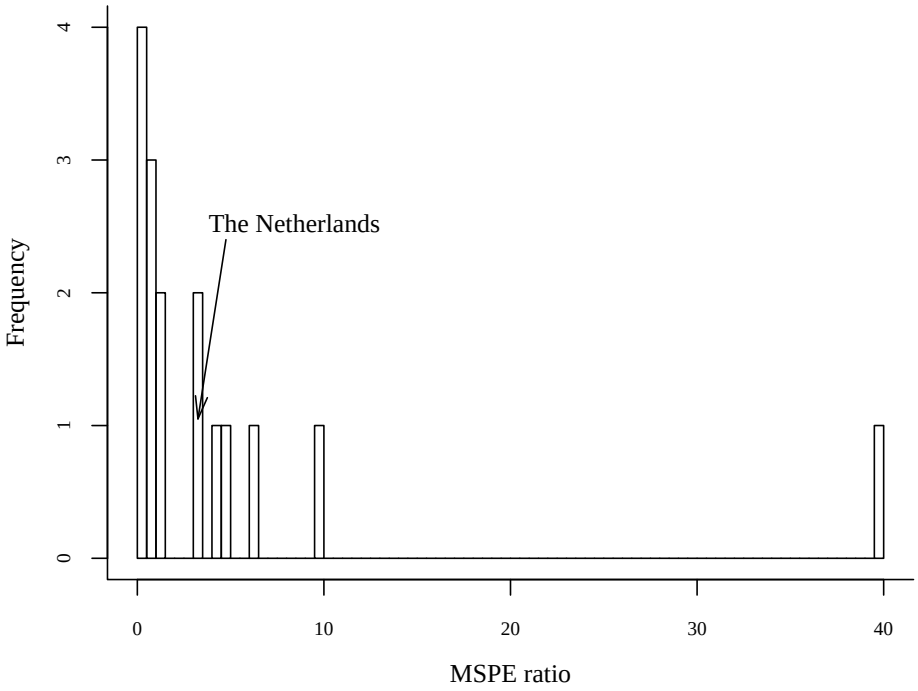


(b) At most 2 times the MSPE of The Netherlands

Figure 3: Comparison of actual-synthetic marriage rate differences



(a) Post-intervention period: 1998–2000 (after the registered partnership law)



(b) Post-intervention period: 2001–2005 (after the same-sex marriage law)

Figure 4: Ratio of post/pre-intervention MSPE, Netherlands and full sample of donors

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

Variable	Mean, 1988–1997			Loading
	Netherlands	Potential donors	Synthetic Netherlands	
	(1)	(2)	(3)	(4)
Crude marriage rate	5.83	6.45	5.90	
Population 25–44 y.o. (%)	32.32	29.57	30.39	0.007
Urban population (%)	70.87	69.08	71.16	0.063
Sex ratio	0.99	0.97	0.97	0.001
Average age at first marriage, men (years)	28.51	26.91	28.08	0.093
Average age at first marriage, women (years)	26.28	24.59	26.00	0.057
Life expectancy, men (years)	74.26	71.58	74.09	0.323
Life expectancy, women (years)	80.21	78.13	80.47	0.005
Girls share in secondary education (%)	47.54	48.58	48.07	0.038
Girls share in tertiary education (%)	46.56	47.99	45.96	0.216
Fertility rate	1.57	1.75	1.57	0.040
Unemployment rate, men 25–34 y.o. (%)	5.67	6.98	5.81	0.101
Unemployment rate, women 25–34 y.o. (%)	8.17	8.94	8.65	0.046
GDP per capita	19,583.48	15,017.01	21,942.82	0.010
Agree with "marriage is an out-dated institution" (%)	21.20	13.53	15.43	0.000

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 16 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
1975–79	39.54	51.19
1980–84	18.43	23.79
1985–89	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)
RP only (1998–2000)	−0.016	0.032	−0.038	0.054*
	(0.048)	(0.014)	(0.049)	(0.014)
RP and SSM (2001–2005)	−0.048	0.002	−0.180	−0.078*
	(0.068)	(0.019)	(0.071)	(0.020)
Linear trend (1995=0)	−0.039*	−0.040*	−0.028	−0.028*
	(0.011)	(0.003)	(0.012)	(0.004)
Education (omitted category: Senior vocational)				
Primary education	−0.264*	−0.121*	0.009	0.532*
	(0.045)	(0.018)	(0.052)	(0.022)
Secondary vocational	−0.060	0.037*	0.092*	0.379*
	(0.024)	(0.009)	(0.026)	(0.011)
General secondary	−0.348*	−0.509*	−0.272*	−0.427*
	(0.040)	(0.013)	(0.040)	(0.013)
Higher professional	−0.166*	−0.333*	−0.395*	−0.688*
	(0.023)	(0.008)	(0.023)	(0.008)
University	−0.171*	−0.393*	−0.642*	−1.096*
	(0.035)	(0.011)	(0.042)	(0.013)
Ethnicity (omitted category: Natives)				
Western immigrants	−0.148*	−0.165*	−0.192*	−0.161*
	(0.039)	(0.012)	(0.040)	(0.013)
Turks/Moroccans	1.278*	2.312*	1.616*	2.867*
	(0.047)	(0.017)	(0.048)	(0.018)
Surinamese/Arubans	−0.145	−0.069*	−0.321*	−0.152*
	(0.070)	(0.021)	(0.070)	(0.024)
Other non-Western	0.144	0.238*	−0.005	0.177*
	(0.067)	(0.019)	(0.069)	(0.023)
Unemployment rate	−0.027*	−0.027*	−0.017	−0.016*
	(0.007)	(0.002)	(0.007)	(0.002)
Urban indicator	−0.154*	−0.178*	−0.227*	−0.311*
	(0.018)	(0.006)	(0.019)	(0.007)
Bible belt	0.767*	1.106*	0.705*	1.080*
	(0.035)	(0.013)	(0.036)	(0.014)
Four largest cities	−0.236*	−0.361*	−0.259*	−0.355*
	(0.033)	(0.009)	(0.036)	(0.010)
Unobs. heterogeneity	no	yes	no	yes
log L / 1000	−74.753	−978.405	−68.225	−863.108

Notes: Discrete-time duration model for age at first marriage using a sample of never-married individuals 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)	
RP only (1998–2000)	0.250*	−0.101*	0.080*
	(0.024)	(0.029)	(0.014)
RP and SSM (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)	
RP only (1998–2000)	0.388*	−0.037	0.085*
	(0.026)	(0.030)	(0.015)
RP and SSM (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 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	Non-Western immigrants
Main effect	—	−0.040	2.648*	0.373*	0.151*
		(0.029)	(0.028)	(0.046)	(0.051)
RP only (1998–2000)	−0.018	0.133*	0.446*	0.418*	0.346*
	(0.014)	(0.038)	(0.040)	(0.062)	(0.065)
RP and SSM (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	Non-Western immigrants
Main effect	—	0.087*	3.131*	0.976*	0.886*
		(0.030)	(0.028)	(0.042)	(0.045)
RP only (1998–2000)	−0.003	0.281*	0.461*	0.237*	0.091
	(0.015)	(0.040)	(0.040)	(0.062)	(0.064)
RP and SSM (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 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.

Appendix — Not for publication

A1 The synthetic control method

As in Abadie et al. (2010), let subscript 1 indicate The Netherlands and $W = (w_2, \ldots, w_{J+1})$ be the vector of weights assigned to the J potential donor countries. Without any restrictions on the weights, a sufficiently large number of potential donor countries and of determinant variables will lead to a synthetic control that matches perfectly the evolution of the marriage rate in The Netherlands prior to the introduction of the two laws. However, weights outside the $[0, 1]$ interval would be difficult to interpret and would imply out-of-sample inference. Hence, the weights are restricted to lie in the unit interval ($0 \leq w_j \leq 1$ for all j) and to sum up to one ($\sum_{j=2}^{J+1} w_j = 1$), which results in a synthetic control that will likely not match perfectly the trend in the marriage rate before the two laws.

For the synthetic control, the marriage rate m_1^* and its determinants X_1^* are calculated as weighted averages of the corresponding variables in the donor countries:

$$m_{1t}^* = \sum_{j=2}^{J+1} w_j m_{jt}, \qquad X_{jt}^* = \sum_{j=2}^{J+1} w_j X_{jt}.$$

Let T_0 be the number of available periods before 1998 and let the vector $K = (k_1, \ldots, k_{T_0})$ define a linear combination of the pre-1998 marriage rates for any country i:

$$\overline{m}_i^K = \sum_{t=1}^{T_0} k_t m_{it}.$$

Now consider M such linear combinations for The Netherlands: $\overline{m}_1^{K_1}, \ldots, \overline{m}_1^{K_M}$, and define $Z_1 = \left(X_1', \overline{m}_1^{K_1}, \ldots, \overline{m}_1^{K_M}\right)'$ as the vector obtained by combining the determinants of the marriage rate prior to 1998 and these M linear combinations of the pre-1998 marriage rate in The Netherlands. Next, consider the matrix Z_0 constructed by combining similar vectors for the J potential donors, such that the j-th column of Z_0 is $\left(X_j', \overline{m}_j^{K_1}, \ldots, \overline{m}_j^{K_M}\right)'$, where X_j is the set of determinants of the marriage rate prior to 1998 in country j.

In principle, the linear combinations $(K_1, \ldots, K_M)$ are arbitrary. In practice, Abadie et al. (2010) suggest choosing $M = 1$ and $k_t = \frac{1}{T_0}$, which produce average marriage rates

over the period before the intervention:

$$\overline{m}_i = \frac{1}{T_0} \sum_{t=1}^{T_0} m_{it}$$

The vector of data for The Netherlands becomes $Z_1 = (X_1', \overline{m}_1)'$ and the corresponding matrix Z_0 for the donor countries has columns of the form $\left(X_j', \overline{m}_j\right)'$ for the j-th donor country.

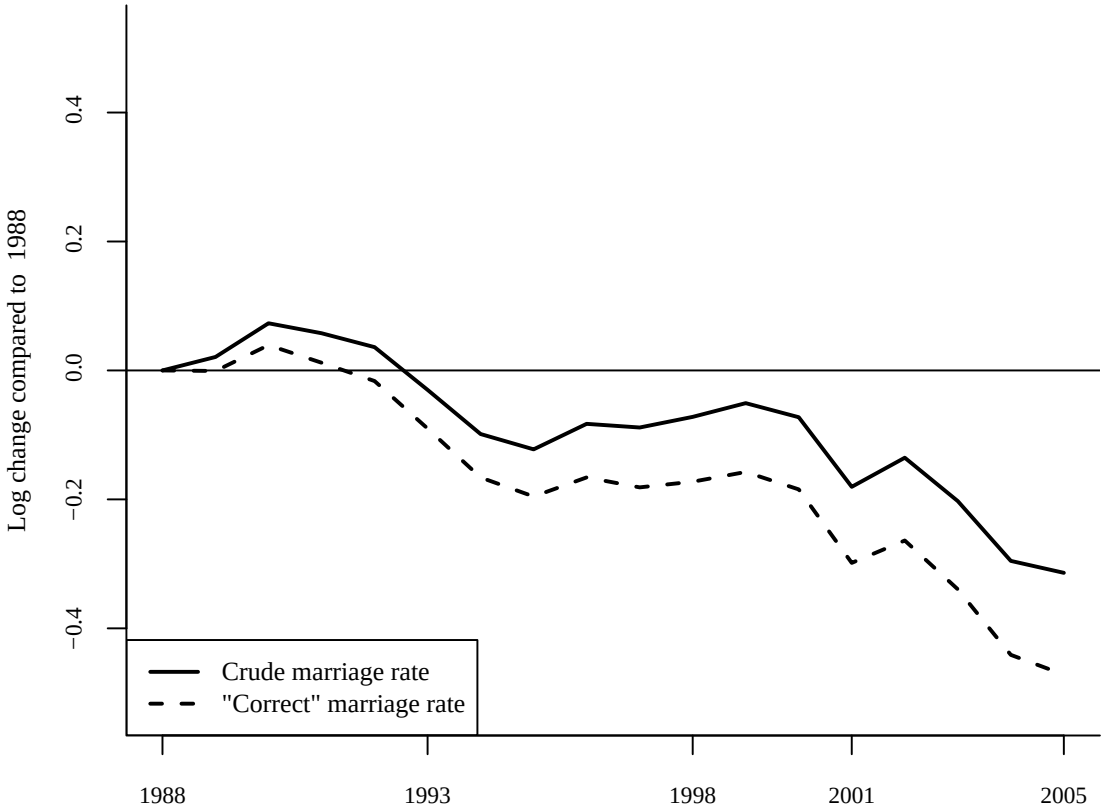
Given this structure of the Z matrices, let V be a diagonal matrix of loadings corresponding to all the variables (both the determinants X and the marriage rate m). The optimal set of weights is the one that minimizes the weighted distance between Z_1 and Z_0:

$$W^*(V) = \text{argmin} \sqrt{(Z_1 - Z_0 W)' V (Z_1 - Z_0 W)}.$$

The matrix V can be arbitrary, but a natural choice is the one that minimizes the mean squared error of the marriage rate in the synthetic control relative to the actual marriage rate in The Netherlands (Abadie et al., 2010):

$$V^* = \text{argmin} \sqrt{[m_1 - m_0 W^*(V)]' [m_1 - m_0 W^*(V)]},$$

where m_1 is the $(T_0 \times 1)$ vector containing the marriage rate in The Netherlands and m_0 is the $(T_0 \times J)$ matrix of marriage rates of the potential donors in the pre-intervention period. This ensures that the marriage rate in the synthetic control constructed using the resulting weights $W^*(V^*)$ is the best match to the marriage rate in The Netherlands in the period before 1998.



Notes: The crude marriage rate is defined as the number of different-sex marriages per 1,000 individuals. The "correct" marriage rate is measured as the number of different-sex marriages per 1,000 single individuals 18 years-old or older. The lines represent the change in each indicator with respect to 1988 on a logarithmic scale, using data on different-sex marriages from Statistics Netherlands over the period between 1988–2005.

Figure A1: Evolution of two measures of the marriage rate in The Netherlands

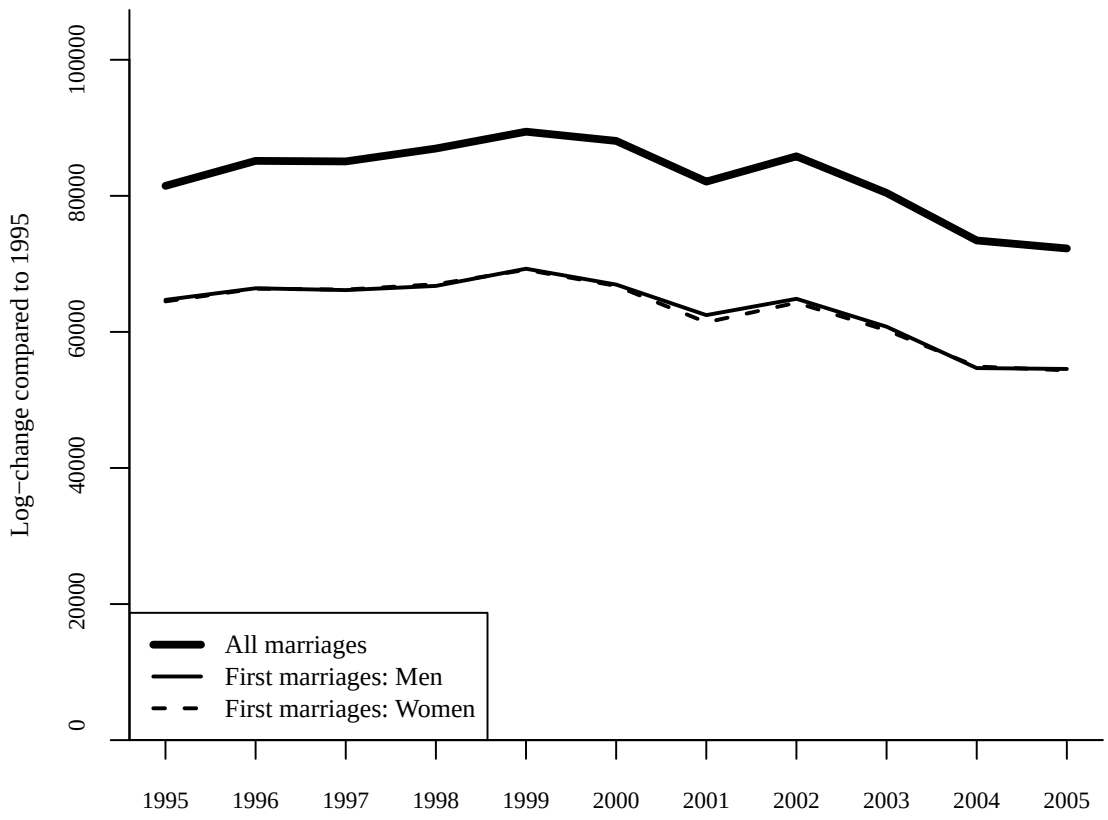


Figure A2: The evolution of all marriages and first marriages for one of the spouses

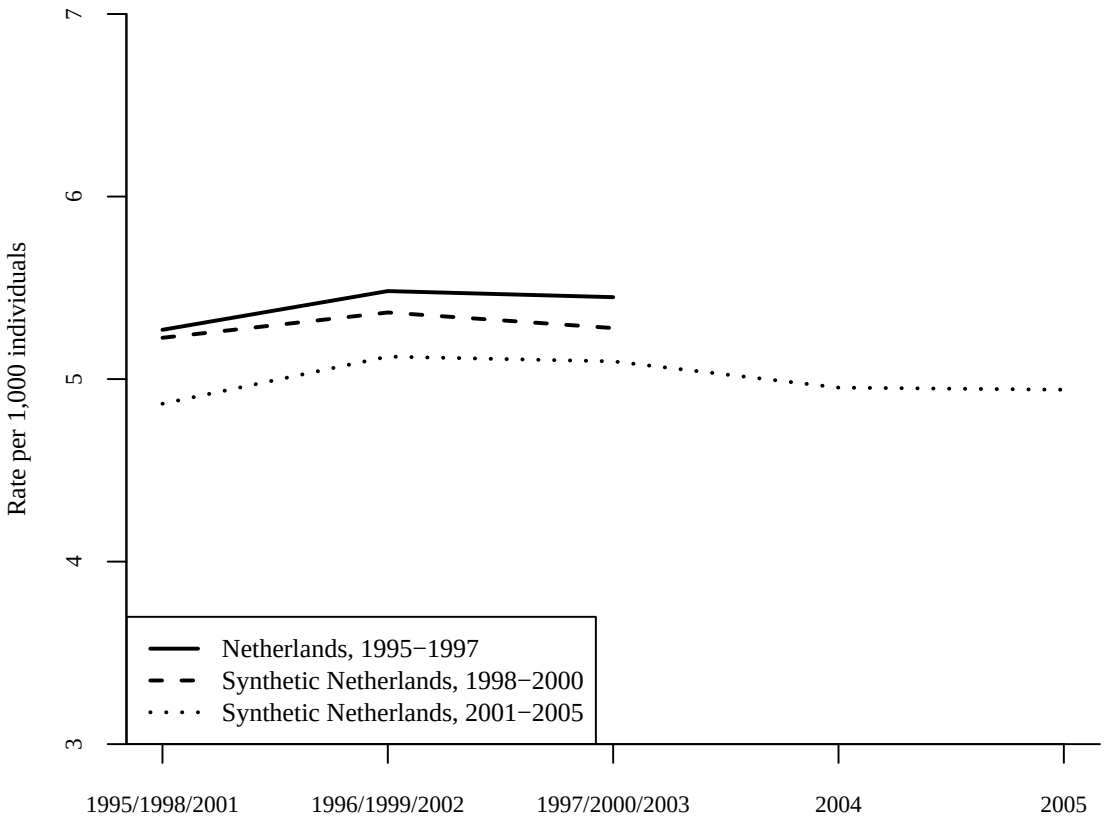


Figure A3: Comparison of counterfactuals, pre- and post-interventions

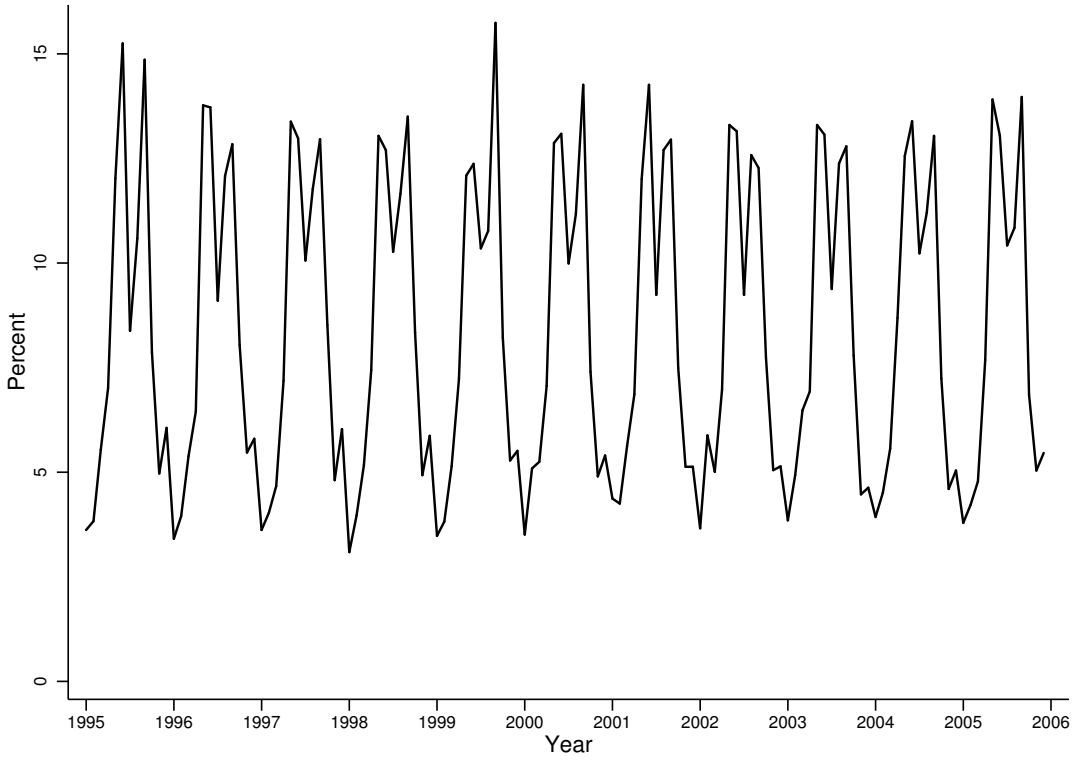


Figure A4: The seasonal pattern of marriages in the Netherlands

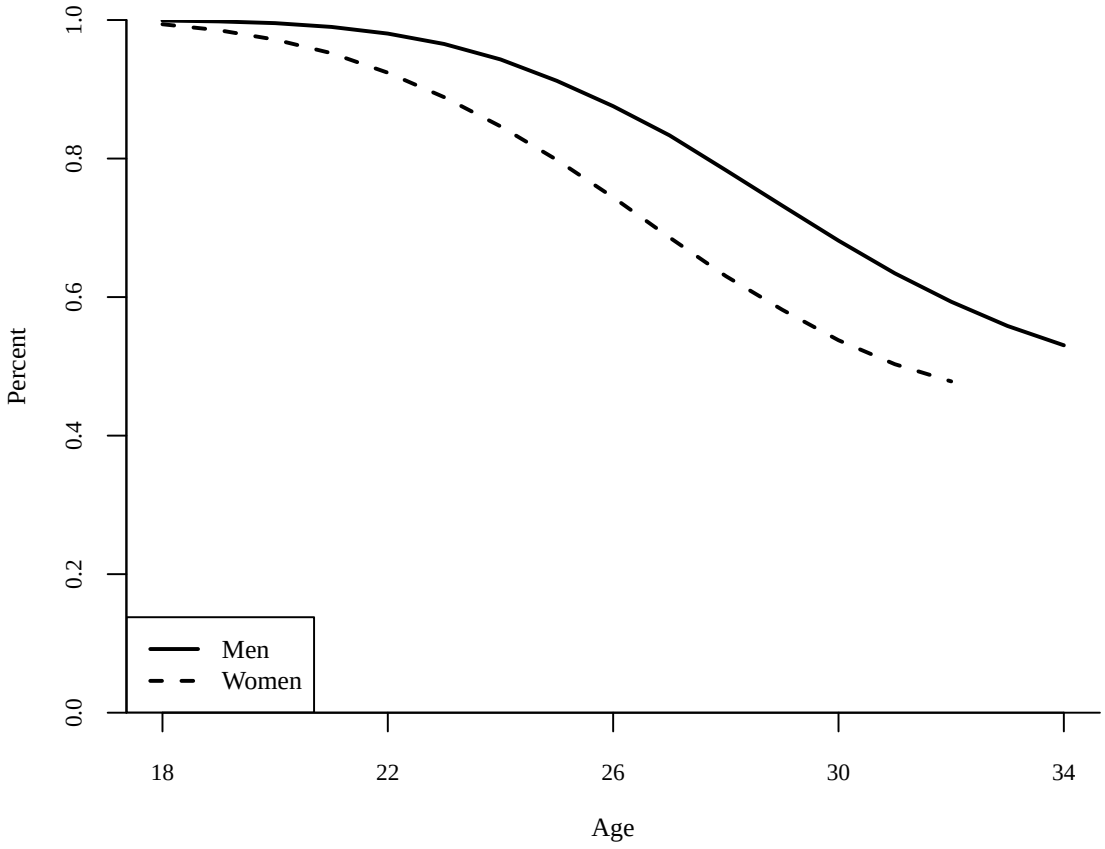
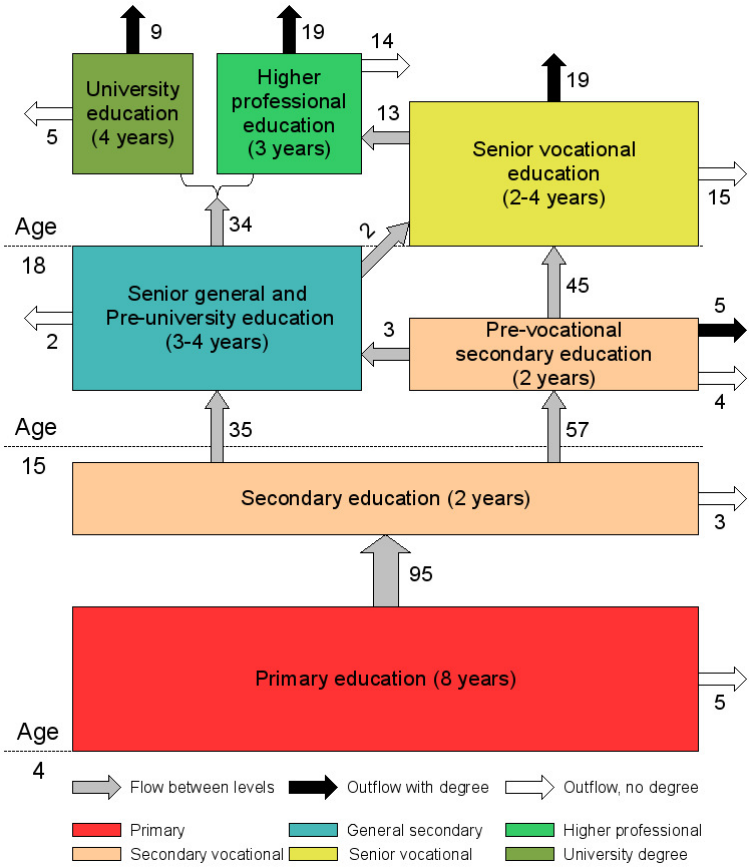


Figure A5: Kaplan-Meier estimates of the survival function (the probability of being single, by age)



Note: Numbers next to arrows represent percentages of a cohort.

Source: Dutch Ministry of Education and Science (2003)

Figure A6: The education system in the Netherlands and the definition of different levels of educational attainment

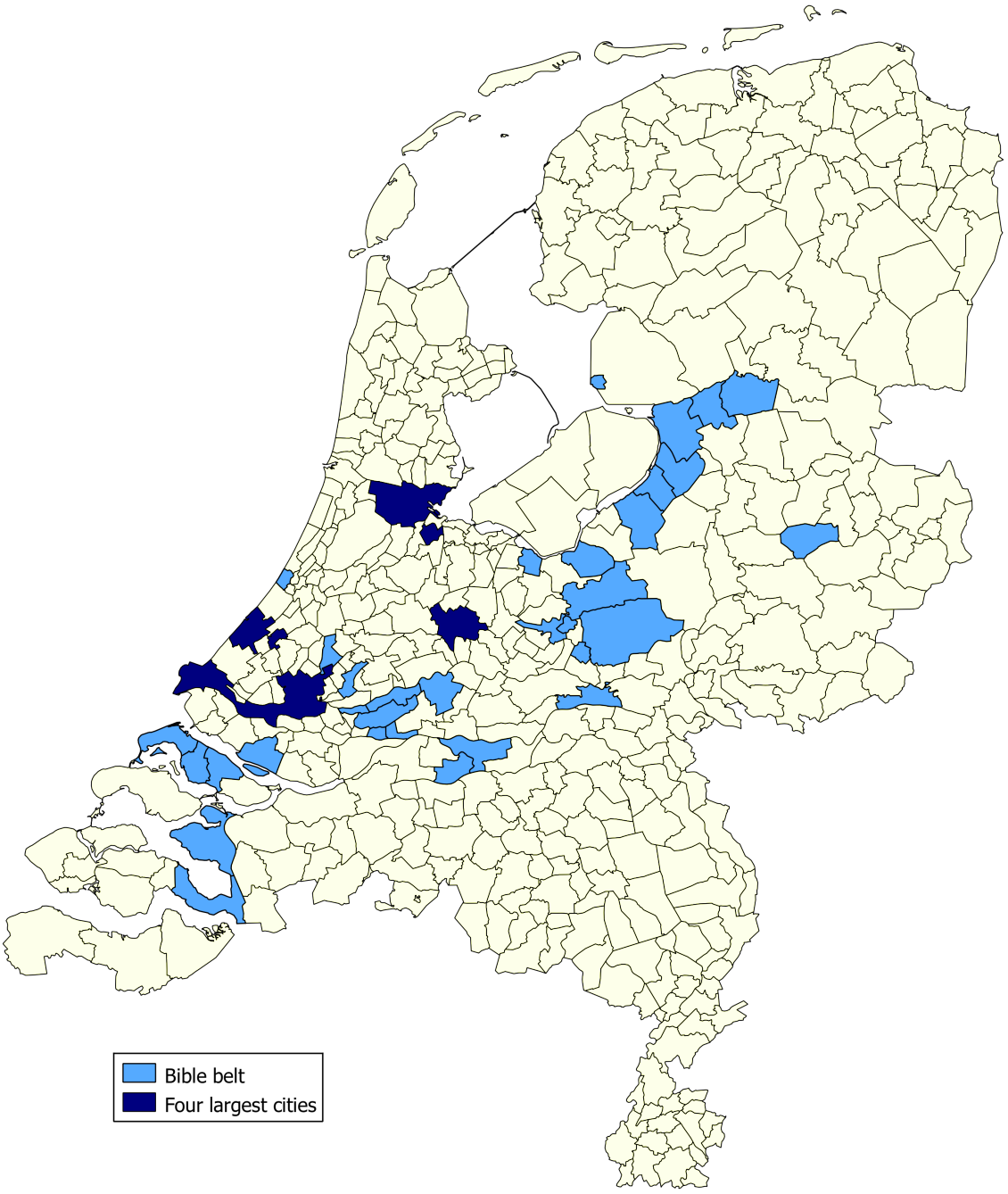


Figure A7: The four largest cities and the Bible-belt municipalities

Table A1: Data sources, aggregate analysis

	Marriage rate[1]	Pop. 25–44	Urban pop.	Sex ratio[2]	Age at first marriage[3]	Life expect.	Share of girls in education	Fertility rate	Unemp. rate	GDP per capita[4]	Marriage views[5]
Netherlands	CBS	E	WDI	WDI	E	OECD	WBES	OECD	OECD	OECD	WVS
Australia	ABS	ABS	WDI	WDI	ABS	OECD	WBES	OECD	OECD	OECD	WVS
Austria	E	E	WDI	WDI	E	OECD	WBES	OECD	OECD	OECD	WVS
Czech Republic	E	E	WDI	WDI	E	OECD	WBES	OECD	OECD	OECD	WVS
Greece	E	E	WDI	WDI	E	OECD	WBES	OECD	OECD	OECD	WVS
Hungary	E	E	WDI	WDI	E	OECD	WBES	OECD	OECD	OECD	WVS
Ireland	E, CSO	E	WDI	WDI	E	OECD	WBES	OECD	OECD	OECD	WVS
Italy	E	E	WDI	WDI	E	OECD	WBES	OECD	OECD	OECD	WVS
Japan	MH, JS	JSB	WDI	WDI	MH	OECD	WBES	OECD	OECD	OECD	WVS
Korea	KNS	KNS	WDI	WDI	KNS	OECD	WBES	OECD	OECD	OECD	WVS
New Zealand	SNZ	SNZ	WDI	WDI	SNZ	OECD	WBES	OECD	OECD	OECD	WVS
Poland	E	E	WDI	WDI	E	OECD	WBES	OECD	OECD	OECD	WVS
Portugal	E	E	WDI	WDI	E	OECD	WBES	OECD	OECD	OECD	WVS
Switzerland	E	E	WDI	WDI	E	OECD	WBES	OECD	OECD	OECD	WVS
Turkey	TSI	E	WDI	WDI	E	OECD	WBES	OECD	OECD	OECD	WVS
United Kingdom	E, ONS	E	WDI	WDI	ONS	OECD	WBES	OECD	OECD	OECD	WVS
United States	SA, VS	ICE	WDI	WDI	CPS	OECD	WBES	OECD	OECD	OECD	WVS

Notes: 1. Number of marriages per 1,000 residents, average population. 2. Constructed from the fraction of female population to total population. 3. Average age at first marriage in the 18–65 year-old population (with the exception of Australia, Austria and the United States, who provide the median age at first marriage). 4. Expenditure approach, US $, constant prices, constant PPPs, OECD base year 1995. 5. Fraction of people who agreed with the statement "marriage is an out-dated institution:" the World Values Survey was run in several waves across different years, so in order to ensure the most coverage, I use data from the first two waves of the survey (1989–1991 and 1995–1998), with the exception of Greece, for which data is only available in the third wave (1999). Abbreviations: E = Eurostat, OECD = OECD database, WDI = World Development Indicators, WBES = World Bank Education Statistics, CBS = Statistics Netherlands, ABS = Australian Bureau of Statistics, CSO = Irish Central Statistics Office, MH = Ministry of Health, Labour and Welfare of Japan, JS = Japan Statistical Yearbook, JSB = Statistics Bureau Japan, KNS = Korea National Statistical Office, SNZ = Statistics New Zealand, TSI = Statistical Indicators 1923-2009 of Turkey, ONS = UK Office for National Statistics, SA = Statistical Abstract of the US, VS = Vital Statistics of the US (CDC), CPS = Census Bureau estimates from the Current Population Survey, ICE = Intercensal estimates.

Table A2: Municipalities included in the Bible belt

Municipality	Votes for conservative parties (%)	Municipality	Votes for conservative parties (%)
Urk	66.50	Brakel[6]	31.74
Genemuiden[1]	52.52	Tholen	31.70
Staphorst	50.07	Barneveld	31.09
Rijssen[2]	48.58	Hasselt[1]	28.95
Bunschoten	43.10	Liesveld	28.12
Kesteren[3]	39.09	Middelharnis	27.99
IJsselmuiden[4]	38.39	Sliedrecht	25.34
Oldebroek	37.92	Katwijk	25.32
Nieuw-Lekkerland	37.54	Zederik	24.03
Hardinxveld-Giessendam	37.37	Scherpenzeel	23.99
Kerkwijk[5]	36.79	Ouderkerk	23.93
Nunspeet	36.47	Veenendaal	23.71
Aalburg	34.96	Woudenberg	23.55
Goedereede	34.72	Putten	22.18
Elburg	33.99	Korendijk	21.50
Reimerswaal	33.67	Echteld[7]	21.33
Graafstroom	32.31	Zevenhuizen-Moerkapelle	20.71
Dirksland	31.75	Ede	20.63

Notes: Share of votes in the 1998 election to the lower-chamber of the Dutch Parliament received by the four conservative Christian parties (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). 1. Included in Zwartewaterland starting from 1/1/2001. 2. Included in Rijssen-Holten starting from 3/15/2003. 3. Included in Neder-Betuwe starting from 4/1/2003. 4. Included in Kampen starting from 1/1/2001. 5. Included in Zaltbommel starting from 01/01/1999. 6. Included in Zaltbommel starting from 1/1/1999. 7. Included in Neder-Betuwe starting from 01/01/2002.