

Legal recognition of same-sex couples and family formation

Mircea Trandafir*

University of Southern Denmark

This version: June 2014

Abstract

It has long been debated how legalizing same-sex marriage would impact (different-sex) family formation. In this paper, I use data on OECD member countries for the period 1980–2009 to examine the effects of the legal recognition of same-sex couples (through marriage or an alternative institution) on different-sex marriage, divorce, and extramarital births. Estimates from difference-in-difference models indicate that the introduction of same-sex marriage or of alternative institutions has no negative effects on family formation. These findings are robust to a multitude of specification checks, including the construction of counterfactuals using the synthetic control method. In addition, the country-by-country case studies provide evidence of homogeneity of the estimated effects.

Keywords: Same-sex marriage, divorce, extramarital births, synthetic control.

JEL Classification: J12, J15.

*University of Southern Denmark and COHERE, Campusvej 55, DK-5230, Odense, Denmark, mircea.trandafir@sam.sdu.dk. I am grateful to Meltem Daysal, Dan Hamermesh, Judy Hellerstein, Seth Sanders, John Wallis, and multiple anonymous referees for their helpful comments and suggestions. All remaining errors are my own.

1 Introduction

An issue often discussed in the debates on the legal recognition of same-sex couples, either via same-sex marriage or civil unions, is whether there would be an effect on the value of marriage and generally on family formation. One concern is that same-sex marriage and possibly any other form of legal recognition of same-sex couples (such as domestic partnerships or civil unions) would encourage alternative family forms, such as cohabitation or single parenthood. This in turn would lead to less marriages, more extramarital births and possibly more divorces. Several recent laws were at least partly justified using this argument, the most prominent ones being state constitution amendments such as Proposition 8 in California or the Defense of Marriage Acts, laws preventing federal or state governments from recognizing same-sex marriages.¹

However, the effect of legalizing same-sex marriage (SSM) or same-sex civil unions or registered partnerships (SSRP) on family formation is theoretically unclear. On the one hand, SSM and SSRP laws could lead to “less traditional” family forms if they change social norms toward a “deinstitutionalization of marriage,” effectively reducing the social stigma associated to cohabitation and other alternative family forms (Cherlin, 2004; Kurtz, 2004). On the other hand, legal recognition of same-sex couples could induce the formation of more “traditional” families if it reignites the interest in marriage or if it reduces the pressure on government and employers to provide benefits to cohabiting couples (Lauer and Yodanis, 2010; Safire, 2003; Rauch, 2004).² In this paper, I attempt to provide causal estimates of the effects of the introduction of SSM and of SSRP on family formation.

¹For Proposition 8, see document 678 from case number 09-CV-2292, or the transcript of David Blankenhorn’s testimony, available online at <http://www.afer.org/our-work/hearing-transcripts/perry-trial-day-11-transcript/>, last accessed on January 27, 2014. For the Federal Defense of Marriage Act, see Rep. Henry Hyde’s intervention in House of Representatives Report 104-664, 1996. Finally, according to the National Conference of State Legislatures, as of January 2014 there were 33 states with constitutional or statutory provisions that effectively prohibit same-sex marriage (see <http://www.ncsl.org/issues-research/human-services/same-sex-marriage-overview.aspx>, last accessed on January 27, 2014).

²Throughout the paper, “traditional family form” refers to different-sex married couples and to children born in these marriages.

Recently, a small but growing body of academic research aims to assess some of the presumed consequences of legally recognizing same-sex couples, including claims made in the same-sex marriage debate. For instance, [Alm et al. \(2000\)](#) estimate the increase in federal income tax revenue due to legalizing same-sex marriage. A few studies examine the welfare of children raised by same-sex couples as compared to children raised by heterosexual couples: [Rosenfeld \(2010\)](#) finds similar progress in school for both types of family structures, while [Allen et al. \(2013\)](#) and [Allen \(2013\)](#) argue that children in same-sex families fare worse. Most related to the current research, there are only three attempts to estimate the causal effects of SSM or SSRP laws on family formation.

[Langbein and Yost \(2009\)](#) use state-level data from 1990, 2000 and 2004 on several indicators of family formation: the marriage rate, the divorce rate, the abortion rate, the extramarital birth rate, and the percentage of female-headed households. In all cases, they find no negative effects of granting marriage-like rights for same-sex couples on family formation and no positive effects of SSM bans. A limitation of this study is that the effects are identified by a small number of observations because of the lack of variation in SSM and SSRP laws during this period. In addition, this prevents the authors from separating the effects of SSM from the effects of SSRP.

[Trandafir \(2014\)](#) uses the synthetic control method to construct a counterfactual marriage rate for the Netherlands, the first country to legalize SSM. He finds no significant differences between the actual and the synthetic marriage rates after the introduction of SSRP or after the legalization of SSM. However, there is some heterogeneity in the marriage behavior of various groups depending on their degree of religiosity or conservatism. This suggests that different countries could experience different responses to the legalization of SSM or SSRP based on their demographics. Moreover, the close spacing of the SSRP and SSM laws made it difficult to separately identify the effects of each law.

Finally, [Dillender \(2014\)](#) examines the effect of legalizing SSRP or SSM on US national and state-level marriage rates between 1995–2010. Estimates from difference-in-difference

models show that the legal recognition of same-sex couples does not have negative effects on the overall or on different-sex marriage rates, as well as on the stock of married people.

The current study contributes to the extant literature on several dimensions. First, it includes almost all the countries that enacted an SSM or SSRP law. In addition, there are more countries legalizing SSRP and SSM in my sample (14 and 4, respectively) than US states (8 and 5, respectively) over the study period. Taken together, these lend more credibility to the external validity of my results as compared to previous studies, all of which focus on the experience of only one country (either the US or the Netherlands). Second, many of these countries enacted SSRP or SSM laws much earlier than US states (e.g., 8 countries had introduced SSRP and 2 had introduced SSM by the time the first US state enacted a corresponding law), allowing me to examine long-term effects. Third, I consider the separate impact of SSRP and SSM laws on several indicators of family formation, yielding a more complete picture of the effects on family formation. Finally, I consider several scenarios under which the legalization of SSRP or SSM could affect family formation without an observable (short-term) change in the indicators considered.

I use data for the period 1980–2009 on 28 OECD member countries, 14 of which introduced some form of same-sex registered partnership (SSRP-adopters) and 3 same-sex marriage (SSM-adopters) during the sample period. I focus on three indicators of family formation: the different-sex marriage rate, the divorce rate, and the extramarital birth rate. The results from difference-in-difference models that allow for country-specific trends indicate no significant negative effects on family formation following either SSRP laws or SSM laws. Indeed, although imprecisely estimated, the estimated effects are generally small and insignificant and in most cases point to slightly more marriages, less divorces and lower rates of extramarital births. These results are robust to several sensitivity checks, including the use of the synthetic control method to construct counterfactuals for each of the three indicators and for each adopting country. Finally, I investigate a number of potential scenarios under which the estimated effects could be confounded by concurrent changes in behavior, at

least in the short run. In all cases, I find no evidence of any offsetting behavior, suggesting that SSRP and SSM laws are unlikely to have any negative effects on family formation.

2 Background

2.1 Short history of SSM and SSRP laws in the OECD

Several developed countries currently grant same-sex couples marriage-like rights. The first country to do so was Denmark in 1989 with the introduction of *registered partnership*. This institution was designed to be a close equivalent to marriage but open only to same-sex couples.³ The Danish registered partnership was used as a model by several other countries, starting with the other Nordic countries: Norway in 1993, Sweden in 1995, and Iceland in 1996. The next country to introduce register partnership was the Netherlands in 1998, but in a departure from the Danish model, this institution is open to both same-sex and different-sex couples. In another departure from the Danish model, France introduced in 1999 a different type of partnership, called *pacte civil de solidarité* or *pacs*. This contract is open to both same-sex and different-sex couples but offers significantly less benefits or obligations than marriage.⁴ Belgium and Germany followed more closely the French example and introduced “weaker” institutions than the Danish-style partnership in 2000 and 2001, respectively.⁵ In the following years, several other countries adopted either Danish-style “strong” versions of registered partnership, such as Finland (2002), New Zealand (2005), the United Kingdom (2005) and Switzerland (2007), or “weaker” versions, such as Luxembourg (2004) and the Czech Republic (2006).⁶

³Waldijk (2004) compares the rights and obligations stemming from the two institutions and finds that 84% of the rights of different-sex marriage are offered by the Danish registered partnership.

⁴ Using the same type of evaluation as before, Waldijk (2004) estimates that *pacs*-ed couples have only 55–63% of the benefits offered to married couples.

⁵German partnership is only open to same-sex couples and was extended in October 2009 to cover all the rights and obligations of marriage.

⁶Several other countries adopted weak or strong version of registered partnership in recent years, but are considered “non-adopters” in this paper due to their late enactment of the laws: Hungary (weak version in July 2009), Austria (strong version, January 2010) and Ireland (strong, January 2011). The United States is

Far fewer countries allow same-sex marriage. In 2001, the Netherlands became the first country to officially open the institution of marriage to same-sex couples, and Belgium followed in 2003. In both countries the decision was made by the legislature. In contrast, the judicial branch in Canada ruled against discrimination against same-sex couples in marriage and forced several provinces to legalize same-sex marriage starting from 2003, leading to recognition at the federal level in 2005 (Wright, 2006). Finally, the newly-elected socialist government of Spain pushed for and obtained the opening of marriage to same-sex couples in 2005, again by means of the legislature.⁷ However, the Spanish government also liberalized divorce at the same time, making it significantly easier for couples to divorce. Given the potential two-way relationship between divorce and marriage (Allen et al., 2006; Rasul, 2006), the main analyses exclude Spain and focus on the other three countries that legalized SSM.

All the SSRP and the SSM laws were subject to relatively heated debates (Merin, 2002). In most cases, it was unclear whether the final result would be the *status quo*, the opening of marriage, or the introduction of an alternative institution. For example, the Netherlands and Belgium enacted an SSRP law only to have the debate flare up again and lead to the legalization of SSM several years later. In other cases, the fate of the legislation hinged on election results or on court decisions. In conclusion, the laws and their timing can be interpreted as plausibly exogenous.⁸

2.2 Theoretical background

In the standard economic marriage model (Becker, 1973, 1974), individuals choose between two states: being in a marriage and not being in a relationship. The evolution of family structure over the past few decades suggests that the model has to be extended to include also considered “non-adopter,” although a few states introduced same-sex marriage or civil unions, because these states represent a minority and because marriages and civil unions conducted in these states are not granted the federal rights and obligations of marriage.

⁷Same-sex marriage is also legal in South Africa since 2006, in Norway and Sweden since 2009, in Argentina, Iceland and Portugal since 2010, and in a few US states starting with Massachusetts in 2005. South Africa and Argentina are not included in the analysis, and the other countries are considered “non-adopters” because of the period under study (see section 4).

⁸See also Dee (2008) for a similar argument.

alternative family forms such as cohabitation and/or registered partnerships. In such a model, any change in the value of marriage could affect family formation in the sense of shifting the preference of some couples from marriage to an alternative arrangement. If the different-sex and same-sex marriage markets are completely segregated, then the legalization of SSM or SSRP would not have any effects on different-sex family formation. However, there would be effects in the presence of any kind of spillovers between the two marriage markets.

The two types of laws may have different effects on family formation because of their specific features. SSRP laws introduce a separate institution, effectively segregating the market for legal unions: marriage for different-sex couples, civil union/registered partnership for same-sex couples.⁹ In contrast, SSM laws change the definition of marriage to include same-sex couples.

Legal recognition of same-sex couples through SSM could impact family formation through several channels. First, a line of research in sociology argues that the Western world experienced a “deinstitutionalization of marriage” starting with the later part of the twentieth century (e.g., [Cherlin, 2004](#)). This development is characterized by changing social norms toward an increased acceptance of alternative family forms such as cohabitation, single parenthood, divorced couples, etc. In particular, [Cherlin \(2004\)](#) identifies SSM as an indicator of the change in social norms, which suggests that the legalization of SSM could accelerate this trend of shifting preferences away from marriage. As a result, some couples on the margin could choose alternative family forms, leading to fewer (different-sex) marriages. To the extent that preferences for offspring do not change, this would also lead to more extramarital births ([Kurtz, 2004](#)). The effect on divorce is less clear, since the response of already-married couples to changes in the value of marriage is theoretically and empirically ambiguous (see, for example, the discussion in [Stevenson and Wolfers, 2007](#)).

⁹As mentioned earlier, some of the registered partnership laws allow for both same-sex and different-sex partnerships. In this case, the law could have an effect on (different-sex) family formation just by creating a competing institution to marriage. I do not explicitly consider this effect because it is not directly related to the legal recognition of same-sex couples, but the estimated effects from models controlling for this feature (available upon request) are virtually identical to the main results reported in the paper.

Second, there might be some individuals on the margin between a same-sex and a different-sex relationship. The legalization of SSM could induce these individuals to choose an SSM instead of a (different-sex) marriage. This would cause fewer different-sex marriages, potentially more divorces (if these individuals are currently in a different-sex marriage), but no effect on extramarital births. Third, some individuals may hold strong beliefs about the exclusive access of different-sex couples to marriage. [Akerlof and Kranton's \(2000\)](#) identity theory suggests that SSM laws could lead to a loss of identity for these individuals. In response, they may act in ways to support the “traditional view” of marriage, which could result in more different-sex marriage, less divorces and less extramarital births. Fourth, the legalization of SSM could be interpreted as a move toward the institutionalization of same-sex relationships ([Lauer and Yodanis, 2010](#)). This could reignite the interest of different-sex couples in marriage ([Mello, 2004](#); [Cahill, 2004](#); [Safire, 2003](#)), resulting in more different-sex marriages, less divorces and less out-of-wedlock births. Finally, when same-sex couples are granted benefits associated with marriage via SSM, gay rights organizations would presumably reduce the pressure on governments and employers to provide these rights to cohabiting couples ([Rauch, 2004](#)). The decline in the value of marriage relative to cohabitation for different-sex couples could then slow down, leading to relatively more different-sex marriages and less extramarital births. For the same reasons mentioned above, the response of divorces would be theoretically uncertain.

The introduction of SSRP can affect family formation through some of the same channels, albeit with potentially different results. First, SSRP can also be seen as accelerating the trend in the deinstitutionalization of marriage, causing a decline in (different-sex) marriage, a potential rise in extramarital births, and no clear response in divorce. Second, individuals on the margin between same-sex and different-sex relationships could choose SSRP once it becomes legal, leading again to fewer different-sex marriages, potentially more divorces, and no effects on extramarital births. Third, there is less pressure to have marriage-like rights granted to cohabiting couples when same-sex couples can have these rights through

SSRP. This would slow down the decline in the value of marriage relative to cohabitation and spur more (different-sex) marriages and less out-of-wedlock births, with an uncertain change in divorce. Finally, the fact that SSRP and (different-sex) marriage are distinct institutions can make marriage a “purer” institution and encourage more family formation among different-sex couples.

In conclusion, the effect of SSRP or SSM laws on family formation is theoretically ambiguous and remains an empirical question. In addition, note that the theories discussed above imply that the enactment of these laws changes the value of marriage regardless of the number of couples entering either type of institution.¹⁰

3 Empirical strategy

The main approach is based on a difference-in-difference model exploiting the cross-country variation both in the type of legal recognition of same-sex couples introduced and in the timing of the law. The estimating equation can be written as:

$$\ln(y_{it}) = \beta_0 + \beta_1 SSRP_{it} + \beta_2 SSM_{it} + f_i(t) + \mu_i + \nu_t + \epsilon_{it}, \quad (1)$$

where y_{it} is an indicator of family formation in country i during year t , $SSRP_{it}$ is a variable equal to the fraction of the year t during which country i had an SSRP law in effect, and SSM_{it} is a similar variable for SSM laws.¹¹

Country fixed effects μ_i are used to capture time-invariant factors that may affect family formation in distinct ways in each country i . In addition, equation (1) includes country-specific time trends $f_i(t)$ that account for general trends in family formation separately for each country. It is important to include these terms since attitudes toward “traditional”

¹⁰The number of couples entering SSM or SSRP is much smaller than the number of different-sex couples who marry. Over the study period, on average, same-sex marriages and same-sex registered partnerships were equivalent to about 2 percent and 1.6 percent of different-sex marriages, respectively.

¹¹Results with variables that take the value 1 if there was an SSM/SSRP law in effect for any fraction of the year (available upon request) are virtually identical.

family forms may evolve differently across countries. I also control for temporary shocks to family formation that are common across countries (e.g., global economic conditions, the discovery of new contraceptives or fertility treatments etc.) through year fixed effects ν_t . Some specifications also include a set of covariates X_{it} that can influence family formation. To the extent that the SSM/SSRP laws are exogenous, the inclusion of these observable characteristics should not alter significantly the value of the estimates but it should improve their precision. Finally, the standard errors are clustered at the country-level to allow for within-country correlations.

The parameters of interest, β_1 and β_2 , capture the percentage change in the indicators of family formation following the introduction of SSRP and SSM, respectively, interpreted as an “intercept shift” around the long-term trend.¹² Their sign indicates the direction of the effect on family formation depending on the indicator analyzed. For instance, a negative sign indicates negative effects on family formation if the dependent variable is an indicator of “traditional” family forms (such as the marriage rate) and positive effects if the dependent variable is related to alternative family forms (e.g., the extramarital birth rate). These effects can be interpreted as causal if two identification assumptions are satisfied: (1) the only factor that influences family formation after the legalization of SSRP or SSM is the law itself, and (2) the countries that did not introduce a particular type of institution for same-sex couples provide a good counterfactual for the countries that did. I investigate several scenarios leading to violations of these assumptions in section 5.

4 Data

I restrict the analysis to OECD member countries, which are presumably more similar to each other, in order to obtain a more homogeneous sample. I use data for the period 1980–2009 and exclude Israel, Mexico, Slovenia and the Slovak Republic because of data

¹²It is possible that what matters is if a law recognizing same-sex couples is enacted, regardless of whether it introduces SSRP or SSM. A specification replacing the two law dummies with an indicator for any type of law produces qualitatively identical results (available upon request).

availability, and Spain because the legalization of SSM coincides with the liberalization of divorce (see section 2). The countries in the sample can be divided into mutually-exclusive four groups. The first group is the “SSM-adopters” and includes the three countries that enacted an SSM law during this period: Belgium, Canada and the Netherlands. The second group, “strong SSRP-adopters,” is comprised of the countries that enacted a Danish-type SSRP law: Denmark, Finland, Iceland, New Zealand, Norway, Sweden, Switzerland, and the United Kingdom. The third group, “weak SSRP-adopters,” counts the countries that legalized weaker versions of SSRP: the Czech Republic, France, Germany and Luxembourg. In most of the analysis, the last two groups are combined into “SSRP-adopters.” Finally, “never-adopters” are the countries that did not adopt a SSM or a SSRP law during the study period: Australia, Austria, Greece, Hungary, Ireland, Italy, Japan, Korea, Poland, Portugal, Turkey, and the United States.¹³

I examine three indicators commonly used in both the economic and the sociological literature on family formation: the marriage rate, the divorce rate, and the extramarital birth rate (in economics, see the review article of [Lundberg and Pollak, 2007](#); in sociology, see [Cherlin, 2004](#), or [Coontz, 2004](#)). Since most of the debate in the media and in the political arena focuses on different-sex family formation, I use the different-sex marriage rate defined as the number of different-sex marriages per 1,000 individuals.¹⁴ The divorce rate is calculated as the number of divorces per 1,000 individuals. Although there is no distinction between same-sex and different-sex divorces in the data from SSM-adopting countries, the short time since the introduction of SSM and the relatively small number of same-sex marriages ensures that nearly all divorces are by different-sex couples. Finally, the extramarital

¹³Norway legalized same-sex marriage in January 2009. Given that the law was introduced at the very end of the sample period, I consider Norway a strong SSRP-adopter. In addition, as mentioned before, I abstract from the fact that a small number of US states legalized same-sex marriage or civil unions/domestic partnerships.

¹⁴Canada does not provide separate information on same-sex and different-sex marriages. The data from the three SSM-adopting countries that distinguish between same-sex and different-sex marriages (the Netherlands, Belgium and Spain) show very small differences between the two marriage rates. This is not surprising since the two measures are identical by construction before the introduction of SSM, and same-sex marriages represent a very small fraction in all marriages (less than 3 percent). Therefore, the results using the overall marriage rate (available upon request) are virtually identical.

birth rate is calculated as the fraction of births to unmarried mothers among all live births, without any distinction between same-sex and different-sex couples. The data sources for all the variables and the date of enactment of the SSM and SSRP laws for each country are listed in Appendix Table A1.

Figure 1 plots the evolution of the three indicators.¹⁵ To emphasize pre-intervention trends, the year when SSM or SSRP were introduced is normalized to zero. For never-adopters, year 0 corresponds to 2000, the median year of adoption among adopting countries. Panels 1(a) and 1(b) show that the marriage rate and the divorce rate are rather stable for the two SSRP-adopting groups prior to the enactment of the laws. On the other hand, these two indicators are almost parallel for SSM-adopters and never-adopters. Finally, panel 1(c) shows that all groups experienced an increase in extramarital births prior to the legalization of SSM/SSRP. These graphs underline the importance of controlling for country-specific trends in the regression analysis, and they suggest that linear trends should generally be appropriate.

As mentioned in section 3, some specifications include a set of country-year controls. These variables represent factors that can influence family formation and can be classified into three groups. The first group includes variables measuring the thickness of the partner market and includes the share of the population in the 25–44 age group and the sex ratio (the ratio of men to women in the population). The second group of variable describes the attractiveness of potential partners: the labor force participation rate and the unemployment rate in the 25–34 year-old age group, separately by gender. These variables are similar to those used in previous studies of marriage behavior (see, for example, Gould and Paserman, 2003). The last group describes the general state of the economy and comprises the overall labor force participation rate and unemployment rate, both separately by gender, and the real GDP per capita, as previous studies found that marriage, divorce and fertility vary

¹⁵The Swedish National Widow’s Pension Scheme extended certain pension benefits to married couples on January 1, 1990. This led to an abnormally large number of weddings in Sweden in 1989 (Hoem, 1991). In the rest of the analysis, I replace this observation with the average of the Swedish marriage rate in 1988 and 1990.

over the business cycle (e.g., [Hellerstein and Morrill, 2011](#); [Schaller, 2013](#) for marriage and divorce, and [Mocan, 1990](#); [Adsera, 2005](#) for fertility). Finally, total population is used to weigh observations in some specifications.¹⁶

5 Results

5.1 Baseline results

Table 1 presents the main results. The estimates in the first column, obtained from a specification including only country and year fixed effects in addition to the two law indicators, indicate that SSM laws are followed by significant declines in different-sex marriage and significant increases in extramarital births. On the other hand, SSRP laws seem to have been followed by more family formation (more different-sex marriages, less divorces and less births outside marriage). For instance, the estimate in the first column of panel A suggests that the different-sex marriage rate increased by approximately 10.5 percent ($\exp(0.100) - 1$) after the introduction of SSRP and fell by a statistically significant 17.1 percent ($\exp(0.158) - 1$) after the enactment of an SSM law.

As mentioned in the previous section, one potential concern is that these estimates might be driven by diverging long-term trends in the indicators. For example, a potential drop in the marriage rate in SSRP-adopting countries after the enactment of the SSRP law would be underestimated because the marriage rate is declining in never-adopting countries but is relatively stable in SSRP-adopting countries. The specification in the second column of Table 1 adds country-specific linear trends to eliminate this potential bias. Although the precision of the estimates is vastly improved, the results are much smaller in absolute value and almost always insignificant. For comparison, the numbers in panel A now suggest that the different-sex marriage rate increased by approximately 2.8 percent after the introduction

¹⁶The data is obtained from the World Development Indicators (sex ratio and total population) and from OECD (all other control variables). Note that some control variables are not available for the entire period, leading to some variation in sample size depending on the specification.

of SSRP and fell by 5.1 percent after the introduction of SSM, both relative to the long-term trend. Interestingly, the only coefficient that changes sign is that of the effect of SSM laws on extramarital births, which now indicates a reduction in the fraction of births outside marriage. Overall, the results from the specification including country-specific linear trends point to no significant negative effects of SSRP or SSM laws on family formation.

These estimates represent the average change in the indicators of family formation over the entire period after the introduction of SSRP or SSM. This average can mask anticipatory responses or lagged effects. For example, the estimated post-pre difference in the marriage rate may be underestimated in absolute value if couples start to marry less before the enactment of the SSM/SSRP law. The same holds if a negative effect of the laws is felt only several years after enactment because of already-planned marriages, divorces or pregnancies. In order to examine this issue in more detail, I replace the law indicators with a set of dummies for each of the 10 years prior and 9 years after the enactment of the corresponding law.¹⁷

The estimated coefficients and their 95-percent confidence intervals are plotted in Figure 2, separately for each indicator of family formation and for each law type. Despite the relatively large confidence intervals, there are several lessons to learn from these graphs. First, the coefficient estimates vary closely around zero in the period before the SSRP/SSM laws, suggesting that the identifying assumption of similar pre-treatment trends is satisfied (the case of extramarital births is one exception to which I return in section 5.3). Second, the coefficients exhibit similar patterns before and after the date of enactment, confirming the lack of anticipatory reactions to the laws as well as the lack of a significant impact on family formation. Finally, the confidence intervals become wider over time, particularly in the case of SSM. This suggests that the lack of precision in the estimates is due to the lower

¹⁷In order to further improve the precision of the estimates, I also include country-year controls. Figures plotting coefficient estimates from the specification without controls (available upon request) are qualitatively similar (see also the discussion on the similarity between the model with and without controls in the next section). Note that this strategy can also be interpreted as a test of the underlying identification assumption of similar pre-intervention trends in adopting and never-adopting countries.

number of observations given the recent enactment of these laws and not to features in the data. Overall, the figure confirms the previous finding of no significant negative effects on family formation.

5.2 Robustness checks

As mentioned in section 3, difference-in-difference models are based on two identifying assumptions: first, conditional on the control variables included, the only factor determining the outcome variable is the intervention, and second, the control observations provide an appropriate counterfactual for the treated observations. In the next two sections I examine the validity of these two assumptions.

I start by providing evidence on the exogeneity of the two law dummies. As discussed in section 2, anecdotal evidence suggests that the introduction of SSRP/SSM and the timing of the laws were not related to the evolution of family formation in adopting countries. However, this is not the only source of endogeneity. For example, migration from adopting to non-adopting countries could compensate for changes in family formation in adopting countries. In order for the coefficients to be upward biased in the case of different-sex marriage and downward biased in the case of divorce and extramarital births, individuals with lower preference for “traditional” family forms would need to migrate from adopting to non-adopting countries and/or individuals with strong preference for “traditional” family forms would need to migrate the other way. Both of these patterns are counter-intuitive if the real effect of the SSRP/SSM laws is to lower the value of marriage.¹⁸

A related concern is that the results might be driven by omitted variables correlated both with the two law dummies and with the trends in family formation. I examine several such scenarios. First, it could be that the estimates are driven by the evolution of other determinants of family formation. For example, the estimates would be biased if these

¹⁸One scenario in which this could occur is if couples in non-adopting countries would want to “reclaim” the institution of marriage in adopting countries. This would imply that the benefits from reclaiming the institution in a different country are high enough to offset the relocation costs, which is unlikely, e.g. because of the linguistic heterogeneity among OECD countries.

determinants are correlated with the adoption of an SSRP/SSM law or with the timing of the enactment. Column 3 of Table 1 shows estimates from a specification including country-year controls. The results are very similar to those in column 2, with all the coefficients indicating insignificant deviations from the long-term trends in family formation following the introduction of SSRP or SSM. Moreover, the signs of all the coefficients point to positive effects of the two laws on family formation. Given the drop in sample size and the similarity of the results when including the control variables, in what follows I consider the specification in column 2 as the baseline.

Second, it is possible that non-linearities in the long-term country-specific trends mask the true effects of the legalization of SSRP or of SSM. Column 4 of Table 1 presents the results from a specification including country-specific quadratic trends. A comparison with the baseline results reveals that the estimates are generally similar: small, insignificant, and pointing to no negative effects on family formation. The only exception is again a marginally significant increase in extramarital births after an SSM law, an issue to which I return in the next section.

Next, I examine in more detail the assumption that the “control” observations provide a good counterfactual for the “treated” observations, which could be partly or completely invalidated in several situations. Since all countries receive equal weights, it is possible that the results are driven by the evolution of family formation in smaller control countries. If these countries are “more liberal” and thus have a more similar evolution to adopting countries, they might push the overall average in the control group closer to that of the treated group and bias the results toward zero. To test for this possibility, I reestimate the baseline specification while weighing each country by its total population in that particular year. The results, listed in column 5 of Table 1, show little change as compared to the baseline results and point again to the absence of negative effects on family formation.

Second, I consider the possibility that excluding Spain from the sample could bias the results toward finding no negative effects on family formation. Column 6 of Table 1 reports

the results from the baseline specification when Spain is included in the sample. Not surprisingly, SSM laws are now followed by slightly more divorces and extramarital births and by slightly fewer marriages, but the effects are still relatively small and insignificant.

Next, I study the sensitivity of the estimates to the set of comparison countries. In the baseline specification, the effect of SSRP laws is identified through a comparison of SSRP-adopting countries to both never-adopting countries and SSM-adopting countries. Similarly, the effect of SSM laws is identified by comparing SSM-adopting countries to both never-adopting countries and SSRP-adopting countries. The estimated effects are then biased if either of these two comparison groups is not appropriate. Some suggestive evidence against this potential bias can be provided by restricting the control observations to the “most similar” observations. Columns 2 through 5 in Table 2 present the results of several such exercises (column 1 repeats the baseline results for comparison). The control group in columns 2–4 is always the never-adopters. The treated group in column 2 includes only countries enacting a strict SSRP law (obviously, only the effect of a strict SSRP law can be identified in this sample). The estimated effects are all similar to the baseline results and point to even larger “benefits” from SSRP laws on family formation. The treated group considered in column 3 comprises all SSRP-adopters and again only the effect of SSRP laws can be identified. The results are closer to the baseline estimates, still slightly larger and still indicating no negative effects on family formation. Column 4 restricts the treated group to SSM-adopters.¹⁹ Compared to the baseline, the estimated effects of SSM laws are closer to zero in the case of different-sex marriage and larger in absolute value for the other two indicators, suggesting yet again no significant negative effects on family formation. Finally, the last column shows the estimates when the sample includes only SSRP-adopters and SSM-adopters. In this case, SSM-adopters act as the control group for SSRP-adopters in the estimation of the effect of SSRP laws, and vice-versa for SSM laws. To the extent that adopting countries are more similar to each other than to never-adopters, this sample

¹⁹Note that the effect of SSRP laws in this case is identified from the years that Belgium and the Netherlands had only an SSRP law, approximately three years each.

yields the most accurate results. The estimates are again small, insignificant, and generally pointing to no negative effects on family formation. One possible exception is again the rate of extramarital births, to which I return in the next section.

5.3 A synthetic control approach

Although the analysis in the previous section revealed little sensitivity of the results to the choice of treated and control observations, one might be concerned that none of the samples used provides in its entirety a valid counterfactual for adopting countries. In situations when an obvious control group is not available, [Abadie and Gardeazabal \(2003\)](#) and [Abadie et al. \(2010\)](#) suggest creating an artificial one called “synthetic control.”²⁰

The synthetic control is constructed for each indicator of family formation in each adopting country using never-adopting countries as potential “donors.” The synthetic control is the set of weights assigned to the donors such that the weighted average of the outcome analyzed and of a given set of determining variables closely match the corresponding values for the adopting country during the period before the introduction of SSRP or SSM (the “intervention”).

Formally, let X be a vector of variables influencing family formation, y the outcome studied, subscript 1 represent the particular adopting country, subscript 0 the set of never-adopting countries, and $Z_1 = (\overline{X}'_1, \overline{y}_1)'$ and $Z_0 = (\overline{X}'_0, \overline{y}_0)'$, where the overline represents means over the pre-intervention period. The synthetic control is the set of weights W that minimize the weighted distance between pre-intervention averages of the variables in Z for the adopting country and its synthetic control:

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

where V is an arbitrary diagonal matrix of variable loadings. [Abadie et al. \(2010\)](#) suggest

²⁰Other studies using the synthetic control method in a cross-country framework include [Lee \(2011\)](#), [Nannicini and Billmeier \(2011\)](#), [Billmeier and Nannicini \(2013\)](#), and [Cavallo et al. \(2013\)](#).

choosing the loading matrix which minimizes the root mean squared predicted error of the synthetic outcome in the pre-intervention period:

$$V^* = \operatorname{argmin} \sqrt{[y_1 - y_0 W^*(V)]' [y_1 - y_0 W^*(V)]} = \operatorname{argmin} \left\{ \frac{1}{T_0} \sum_{t=1}^{T_0} (y_{1t}^* - y_{1t})^2 \right\}^{1/2},$$

where $y_{1t}^* = y_0 W^*(V)$ is the synthetic outcome in period t . The weights are restricted to lie in the unit interval and to sum up to one in order to avoid out-of-sample inference, resulting in synthetic controls that are unlikely to match perfectly the pre-intervention trend in the outcome. In practice, these weights are calculated using a two-step maximization procedure. In the first step, each variable is assigned a loading and the set of country weights which minimizes the weighted distance between the synthetic control and the adopting country is calculated as a function of these loadings. In the second step, the variable loadings are chosen such that the synthetic outcome matches as closely as possible the actual outcome in the adopting country, and the two steps are repeated until convergence is achieved. [Abadie et al. \(2010\)](#) show that the synthetic control takes into account both the observable and the unobservable determinants of the dependent variable, producing an appropriate counterfactual for the evolution of the outcome in the absence of the SSRP or SSM law.

The synthetic control method does not lend itself directly to statistical inference. In order to determine whether the actual and synthetic rates are significantly different after the intervention, [Abadie et al. \(2010\)](#) suggest conducting permutation (placebo) experiments. In these experiments, the adopting country for each adopter-outcome pair is assigned to the donor pool, each never-adopting country in turn is considered “adopting” on the same date as the true adopting country, and a synthetic control for this new adopting country is constructed. The gaps between each of the actual and synthetic outcomes produced by the placebo tests can then be plotted and compared to the initial actual-synthetic gap. In order for these graphs to be meaningful, I restrict them to the placebo tests in which the synthetic outcomes match relatively well the actual outcomes in the pre-intervention period in terms

of having a mean square prediction error in the pre-intervention period at most 5 times that of the adopting country (Abadie et al., 2010). The interpretation of the graph is that if the gap for the adopting country during the post-intervention period lies in the “cloud” produced by placebo gaps, then the difference between the actual and synthetic outcomes for the adopting country is “insignificant.” Conversely, if the gap for the adopting country is mostly outside of the “cloud,” then the actual-synthetic difference is “significant.”²¹

The data used is similar to that used in the previous section. I also include Spain given that there is no “tainting” of the results for the other countries. For each country, the pre-intervention period consists of the 10 years before the enactment of the SSRP or SSM law.²² The outcomes analyzed are the same as in the previous sections: the marriage rate (separately for different-sex couples and overall, where possible), the divorce rate and the fraction of births outside marriage. The variables included in the vector of determinants X are similar to those in the previous sections: the share of the population in the 25–44 age group, the unemployment rate of men and of women in the 25–34 age group, the sex ratio, and the real GDP per capita. In addition, since the procedure uses averages over the pre-intervention period, I can also make use of several variables for which only a few years of data are available: the share of women in tertiary education (from the World Bank Education Statistics) and several variables from the World Values Survey that capture attitudes toward divorce, marriage, single parenthood, religion and abortion.²³

²¹A second type of test plots the distribution of the ratio of post- to pre-intervention $MSPE$ for all the placebo tests and places the $MSPE$ for the adopting country in this distribution. An $MSPE$ ratio at the right tail of the distribution can be interpreted as evidence toward “significance,” while an $MSPE$ ratio at the lower tail of the distribution implies “insignificance.” The two approaches lead to the same conclusion (figures available upon request).

²²Unfortunately, there is not enough data on the vector of determinants to construct a counterfactual for Denmark and Norway, the earliest SSRP-adopting countries. Since the first ruling against discrimination in marriage in Canada was made in 2001 and several provinces started offering SSM as early as 2003, I consider the period 1992–2001 as the pre-intervention period for Canada. In the case of Spain, several provinces introduced domestic partnerships starting from 1998, so the pre-intervention period is 1988–1997. In both cases, the figures include vertical lines at both dates.

²³These variables are: the fraction of respondents who agreed with the statement “marriage is an outdated institution,” the average score provided to the question asking when divorce is justifiable (1 = never, 10 = always), the fraction of respondents approving of the situation “a woman wants to have a child as a single parent but she doesn’t want to have a stable relationship with a man,” the average score provided to the question asking when abortion is justifiable (1 = never, 10 = always), and the fraction of respondents

Figures 3, 4 and 5 plot the corresponding actual-synthetic gaps for each adopting country (solid black line) and placebo tests (gray lines). The vertical lines indicate the date of enactment of SSRP and/or SSM laws (Appendix Figures A1, A2 and A3 plot the synthetic and actual indicators of family formation for all adopting countries, with the solid line representing the actual and the dotted line the synthetic indicator).²⁴

The figures show that the marriage rate and the divorce rate in adopting countries have similar evolutions to the counterfactual after the introduction of SSRP or SSM. In many cases, the actual-synthetic differences indicate positive effects on family formation (i.e., higher marriage rates and lower divorce rates), same as in the difference-in-difference approach. In the case of extramarital births, never-adopting countries do not always seem to provide a good counterfactual as the actual rate of births outside marriage is consistently higher than the synthetic rate during the entire pre-intervention period for several countries (France, Iceland, New Zealand, Sweden, United Kingdom). This can also explain the sensitivity of the difference-in-difference results pertaining to extramarital births. However, for the countries where a reasonably good synthetic control can be constructed, the actual extramarital birth rate is largely within the cloud of the placebo tests, even if marginally so in some cases (e.g., Belgium and the Netherlands).

This exercise also provides evidence on effect heterogeneity. The difference-in-difference estimates in the previous section represent average effects over the entire post-intervention period and over all the adopting countries. This could potentially mask heterogeneity in the results, with some countries experiencing negative effects and others positive effects that cancel each other out. The comparative case studies conducted in this section show that the finding of no significant negative effects of SSRP/SSM laws on family formation apply to

answering “not at all important” or “not very important” to the question “how important is religion in your life.” The first three variables are used when the outcome is the marriage rate or the divorce rate, and the last three when the outcome is the fraction of extramarital births.

²⁴As mentioned before, Belgium, the Netherlands and Spain provide data on same-sex and different-sex marriages separately. The corresponding graphs in Figures A1 and 3 include both the overall and the different-sex marriage rate. In addition, the Netherlands provides information on different-sex registered partnerships and its graphs also include the difference-sex union rate (i.e., marriages and partnerships).

each of the adopting country and not just on average.

5.4 Additional evidence

The results in the previous sections show no evidence that the introduction of SSRP or SSM had negative effects on the three indicators analyzed. However, previous research argues that these indicators might not perfectly capture individual behavior in response to changes in norms or in their environment (Lauer and Yodanis, 2010). For example, it is possible that preferences for marriage are indeed negatively affected by the enactment of an SSRP or SSM law, but the marriage rate is almost unchanged (at least in the short run) because of a concurrent offsetting change in behavior. In this section, I study several scenarios under which changes in individual preferences for “traditional” family forms brought on by SSRP or SSM laws may be counteracted by some other behavior.

First, suppose that the legalization of SSRP/SSM makes people less likely to marry, but it also makes them prefer to marry younger. In this case, individuals older than the new desired age at (first) marriage would choose to marry, potentially leading to an unchanged marriage rate in the short run and a decline only in the longer run.²⁵ If fertility decisions are related to the timing of marriage, the observed effect of the laws on extramarital marriages could also be impacted, and a similar argument could be made in the case of divorces. Panels A and B of Table 3 provide the estimates from the baseline specification using age at first marriage of men and of women as the dependent variable, respectively (column 1). Several specification tests similar to the ones in sections 5.1 and 5.2 are shown in columns 2–5 (see the discussion of the tests in the previous sections). Regardless of the specification, the results are always small and rather precisely estimated, providing no evidence of any significant change in the timing of first marriages after the legalization of SSRP or SSM.

²⁵For example, suppose people can be of only two types: those who prefer to marry young and those who prefer to marry late. The introduction of SSRP or SSM could push the second type to the extreme of wanting to never marry. In this case, some of these individuals will indeed never marry, while others will switch to marrying young. In the short run, this will lead to more marriages until all the “switchers” have married. After that, the marriage rate will be lower as the only people who marry are those who want to marry young.

An alternative scenario is that SSRP/SSM laws change individual preferences toward offspring. If people decide to have more children and if fertility and marriage/divorce decisions are tied, then any negative effect of SSRP/SSM laws on marriage and divorce could be counteracted by an increase in marriages and a decline in divorces due to the higher fertility. This scenario could also explain the positive effects seen on extramarital births. Panel C of Table 3 shows the estimates when the dependent variable is the crude birth rate, defined as the total number of births per 1,000 individuals. While there is some relatively weak evidence of an increase in fertility following SSM laws, the estimates are generally small and insignificant.²⁶ In conclusion, I do not find any strong evidence that the main results are driven by a shift in the preferences for children.

Finally, the main results might not capture the effect of SSRP/SSM laws if the laws are associated with changes in the economic environment that also affected family formation. For example, previous research found that marriage, divorce and fertility all seem to fluctuate with the state of the economy as measured by unemployment rates or real GDP (Hellerstein and Morrill, 2011; Schaller, 2013; Mocan, 1990; Adsera, 2005). Some evidence against this scenario is already provided in section 5.2 as the main estimates are robust to the inclusion of country-year controls. In addition, in panels D–F of Table 3 I present results from models where the dependent variable is the unemployment rate of young men and women or the real GDP per capita. In general, the estimates are relatively small and insignificant. One exception is that SSM laws seem to be countercyclical (positively correlated with unemployment and negatively correlated with real GDP per capita). However, downturns are generally associated with less marriage and less divorce (Hellerstein and Morrill, 2011; Schaller, 2013), which could explain the main results for the divorce rate but not for the different-sex marriage rate. In addition, the estimates in panels D and E suggest that the unemployment rate of women increases more than that of men following an SSM law and previous research found that an increase in women’s unemployment (relative to men’s) leads

²⁶I find similar results using the fertility rate (the predicted number of children a women will have over her lifetime) instead of the crude birth rate.

to a decline in fertility (Mocan, 1990; Adsera, 2005), in contrast with the previous results in panel C. Therefore, these results do not support the idea that the economic environment dampened a potentially negative effect of SSRP and SSM laws on family formation.²⁷

5.5 Discussion

The results in the previous sections suggest that the legalization of both SSM and SSRP is followed by small and generally insignificant fluctuations in different-sex marriages, divorces and extramarital births. These results are not consistent with any of the theories predicting that SSM or SSRP affect the value of marriage (see section 2.2), suggesting the absence of spillovers of any kind between the same-sex and the different-sex partner markets.

There are several caveats to my findings. First, it is possible that some aspects of family formation are not well represented by the three indicators used in this study. For example, some researchers question whether the weakening of social norms and the deinstitutionalization of marriage can be captured by these indicators (Lauer and Yodanis, 2010). However, their evolution is still informative to the extent that they represent measures of revealed preference for family types. Second, only 14 of the 28 countries in the sample adopted SSRP and even fewer (3) legalized SSM. As such, generalizations to other contexts should be performed with caution. Finally, the number of years after the introduction of SSM (and, to some extent, SSRP) is relatively short as most laws were enacted only recently. While it is possible that some of the effects of these laws may be observed with a lag longer than what is available in current data, previous studies find that public policies that change the value of marriage can affect family formation over time spans similar to or even shorter than those in the current study (see, for example Allen, 1992; Bitler et al., 2004).

Despite these limitations, this study adds to the literature by being the first to provide causal estimates of the effects of legalizing SSM and SSRP on family formation in almost

²⁷I find similar results when using as dependent variable other measures of the state of the economy: the overall unemployment rate of men and of women, the labor force participation rate of young men and young women, and the overall labor force participation rate of men and women (results available upon request).

all the countries that introduced these arrangements. The results provide a starting point for an evidence-based debate on the legalization of SSM or SSRP and contribute to our understanding of marriage and fertility behavior in general.

6 Conclusions

The same-sex marriage debate has seen starkly different claims with respect to what effects the legalization of SSRP or SSM could have on the institution of marriage and on family formation in general. In this paper, I provide empirical evidence on this issue. My estimates suggest that both the introduction of SSRP and the legalization of SSM are not followed by significant negative effects on family formation. In fact, in many cases the effects on the three indicators analyzed (different-sex marriage rate, divorce rate, and extramarital birth rate) indicate more family formation. These results are inconsistent with theories arguing that SSM or SSRP laws reduce the value of marriage as compared to alternative arrangements. Given the prevalence of these arguments in the debate on the legal recognition of same-sex couples, these findings provide much needed foundation for evidence-based public policies.

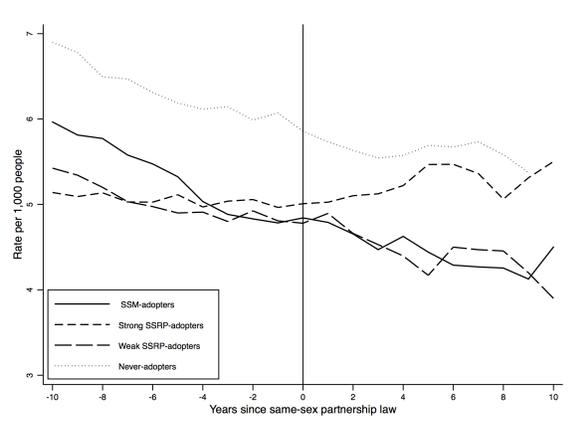
References

- Abadie, Alberto, Alexis Diamond, and Jens Hainmueller (2010), “Synthetic control methods for comparative case studies: Estimating the effect of California’s tobacco control program,” *Journal of the American Statistical Association* 105(490), 493–505.
- Abadie, Alberto, and Javier Gardeazabal (2003), “The economic costs of conflict: A case study of the Basque Country,” *American Economic Review* 93(1), 113–132.
- Adsera, Alicia (2005), “Vanishing children: From high unemployment to low fertility in developed countries,” *American Economic Review* 95(2), 189–193.
- Akerlof, George, and Rachel Kranton (2000), “Economics and identity,” *Quarterly Journal of Economics* 115(3), 715–753.
- Allen, Douglas W. (1992), “Marriage and divorce: Comment,” *American Economic Review* 82(3), 679–685.
- (2013), “High school graduation rates among children of same-sex households,” *Review of Economics of the Household* 11(4), 635–658.
- Allen, Douglas W., Catherine Pakaluk, and Joseph Price (2013), “Nontraditional families and childhood progress through school: A comment on Rosenfeld,” *Demography* 50(3), 955–961.
- Allen, Douglas W., Krishna Pendakur, and Wing Suen (2006), “No-fault divorce and the compression of marriage ages,” *Economic Inquiry* 44(3), 547–558.
- Alm, James, M.V. Lee Badgett, and Leslie A. Whittington (2000), “Wedding bell blues: The income tax consequences of legalizing same-sex marriage,” *National Tax Journal* 53(2), 201–215.
- Becker, Gary (1973), “A theory of marriage: Part I,” *Journal of Political Economy* 81(4), 813–846.

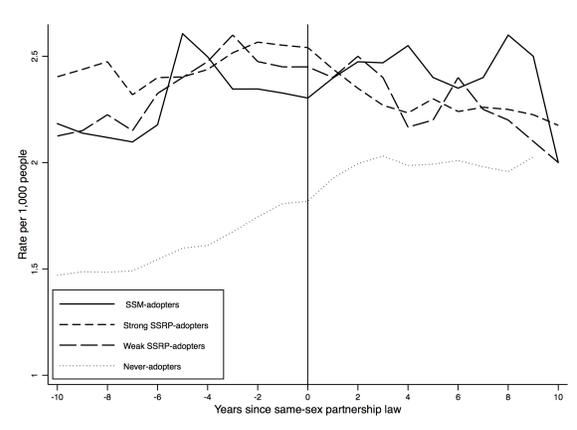
- (1974), “A theory of marriage: Part II,” *Journal of Political Economy* 82(2), S11–S26.
- Billmeier, Andreas, and Tommaso Nannicini (2013), “Assessing economic liberalization episodes: A synthetic control approach,” *Review of Economics and Statistics* 95(3), 983–1001.
- Bitler, Marianne P., Jonah B. Gelbach, Hilary W. Hoynes, and Madeline Zavodny (2004), “The impact of welfare reform on marriage and divorce,” *Demography* 41(2), 213–236.
- Cahill, Sean (2004), *Same-Sex Marriage in the United States: Focus on the Facts*, Lanham, MD: Lexington Books.
- Cavallo, Eduardo, Sebastián Galiani, Ilan Noy, and Juan Pantano (2013), “Catastrophic natural disasters and economic growth,” *Review of Economics and Statistics* 95(5), 1549–1561.
- Cherlin, Andrew (2004), “The deinstitutionalization of American marriage,” *Journal of Marriage and Family* 66(4), 848–861.
- Coontz, Stephanie (2004), “The world historical transformation of marriage,” *Journal of Marriage and Family* 66(4), 974–979.
- Dee, Thomas (2008), “Forsaking all others? The effects of same-sex partnership laws on risky sex,” *Economic Journal* 118(530), 1055–1078.
- Dillender, Marcus (2014), “The death of marriage? The effects of new forms of legal recognition on marriage rates in the United States,” *Demography* 51(2), 563–585.
- Gould, Eric, and Daniele Paserman (2003), “Waiting for Mr. Right: Rising inequality and declining marriage rates,” *Journal of Urban Economics* 53(2), 257–281.
- Hellerstein, Judith, and Melinda Sandler Morrill (2011), “Booms, busts, and divorce,” *B.E. Journal of Economic Analysis & Policy* 11(1), 1–26.

- Hoem, Jan (1991), "To marry, just in case...: the Swedish widow's-pension reform and the peak in marriages in December 1989," *Acta Sociologica* 34(2), 127–135.
- Kurtz, Stanley (2004), "The end of marriage in Scandinavia," *The Weekly Standard* 9(20), 26.
- Langbein, Laura, and Mark Yost (2009), "Same-sex marriage and negative externalities," *Social Science Quarterly* 90(2), 292–308.
- Lauer, Sean, and Carrie Yodanis (2010), "The deinstitutionalization of marriage revisited: A new institutional approach to marriage," *Journal of Family Theory & Review* 2(1), 58–72.
- Lee, Wang-Sheng (2011), "Comparative case studies of the effects of inflation targeting in emerging economies," *Oxford Economic Papers* 63(2), 375–397.
- Lundberg, Shelly, and Robert A Pollak (2007), "The American family and family economics," *Journal of Economic Perspectives* 21(2), 3–26.
- Mello, Michael (2004), *Legalizing Gay Marriage*, Philadelphia, PA: Temple University Press.
- Merin, Yuval (2002), *Equality for Same-Sex Couples: The Legal Recognition of Gay Partnerships in Europe and the United States*, Chicago, IL: University of Chicago Press.
- Mocan, Naci (1990), "Business cycles and fertility dynamics in the United States: A vector autoregressive model," *Journal of Population Economics* 3(2), 125–146.
- Nannicini, Tommaso, and Andreas Billmeier (2011), "Economies in transition: How important is trade openness for growth?" *Oxford Bulletin of Economics and Statistics* 73(3), 287–314.
- Rasul, Imran (2006), "Marriage markets and divorce laws," *Journal of Law, Economics, and Organization* 22(1), 30–69.

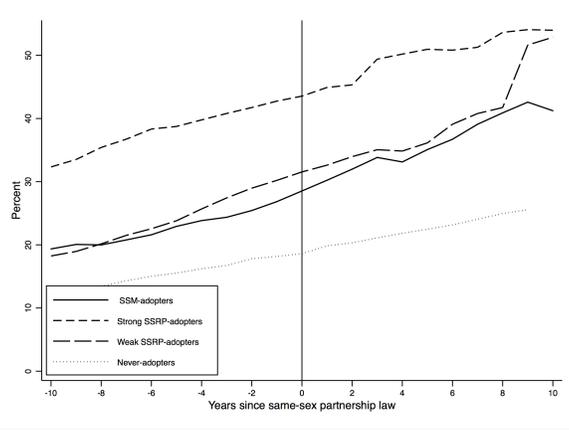
- Rauch, Jonathan (2004), *Gay Marriage: Why It Is Good for Gays, Good for Straights, and Good for America*, New York, NY: Times Books.
- Rosenfeld, Michael (2010), “Nontraditional families and childhood progress through school,” *Demography* 47(3), 755–775.
- Safire, William (2003), “The bedroom door,” *The New York Times* .
- Schaller, Jessamyn (2013), “For richer, if not for poorer? Marriage and divorce over the business cycle,” *Journal of Population Economics* 26(3), 1007–1033.
- Stevenson, Betsey, and Justin Wolfers (2007), “Marriage and divorce: Changes and their driving forces,” *Journal of Economic Perspectives* 21(2), 27–52.
- Trandafir, Mircea (2014), “The effect of same-sex marriage laws on different-sex marriage: Evidence from the Netherlands,” *Demography* 51(1), 317–340.
- Waaldijk, Kees (2004), “Levels of legal consequences of marriage, cohabitation and registered partnership for different-sex and same-sex partners: Comparative overview,” in Kees Waaldijk, ed., *More or Less Together: Levels of Legal Consequences of Marriage, Cohabitation and Registered Partnerships for Different-Sex and Same-Sex Partners. A Comparative Study of Nine European Countries*, 7–36, Institut National d’Études Démographiques, Document de Travail No. 125.
- Wright, Wade (2006), “The tide in favour of equality: Same-sex marriage in Canada and England and Wales,” *International Journal of Law, Policy & the Family* 20(3), 249–285.



(a) Different-sex marriage rate

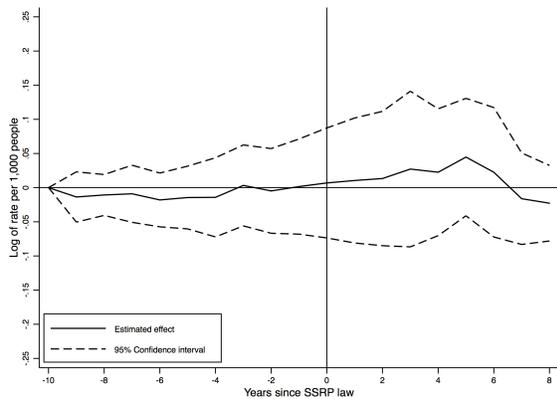


(b) Divorce rate

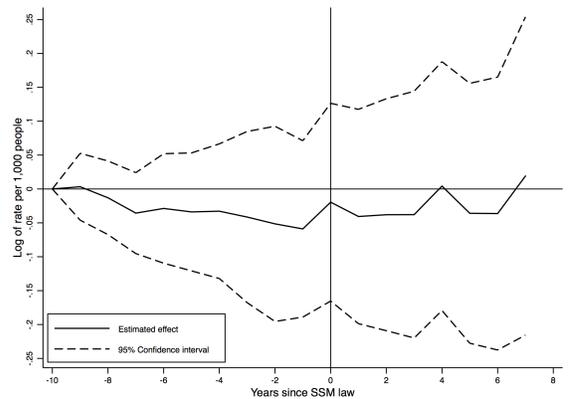


(c) Extramarital births rate

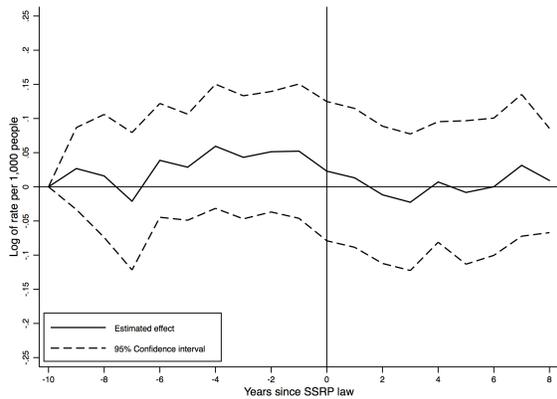
Figure 1: Evolution of family formation indicators relative to the enactment of SSM or SSRP laws



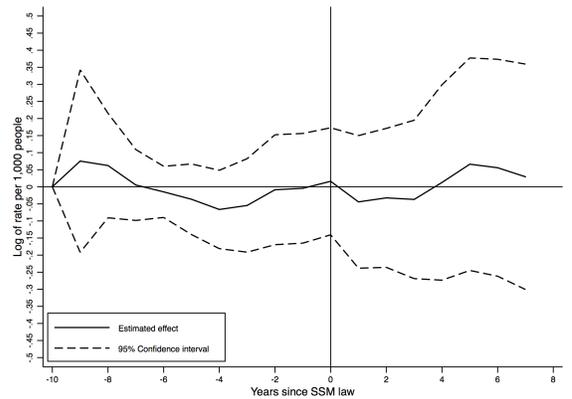
(a) Different-sex marriage rate, effect of *SSRP*



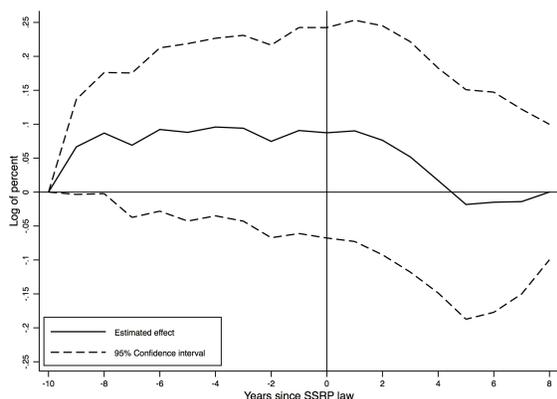
(b) Different-sex marriage rate, effect of *SSM*



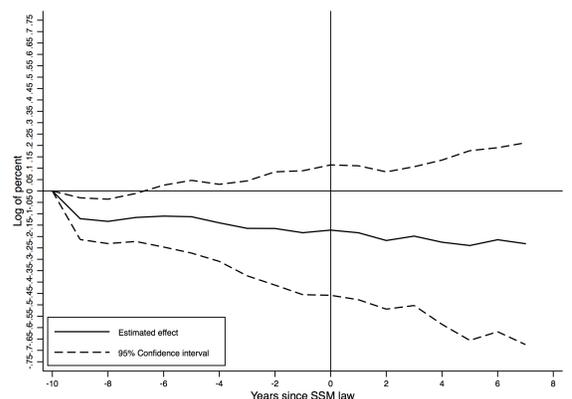
(c) Divorce rate, effect of *SSRP*



(d) Divorce rate, effect of *SSM*



(e) Extramarital births rate, effect of *SSRP*



(f) Extramarital births rate, effect of *SSM*

Figure 2: Effects of SSM and SSRP laws on family formation indicators

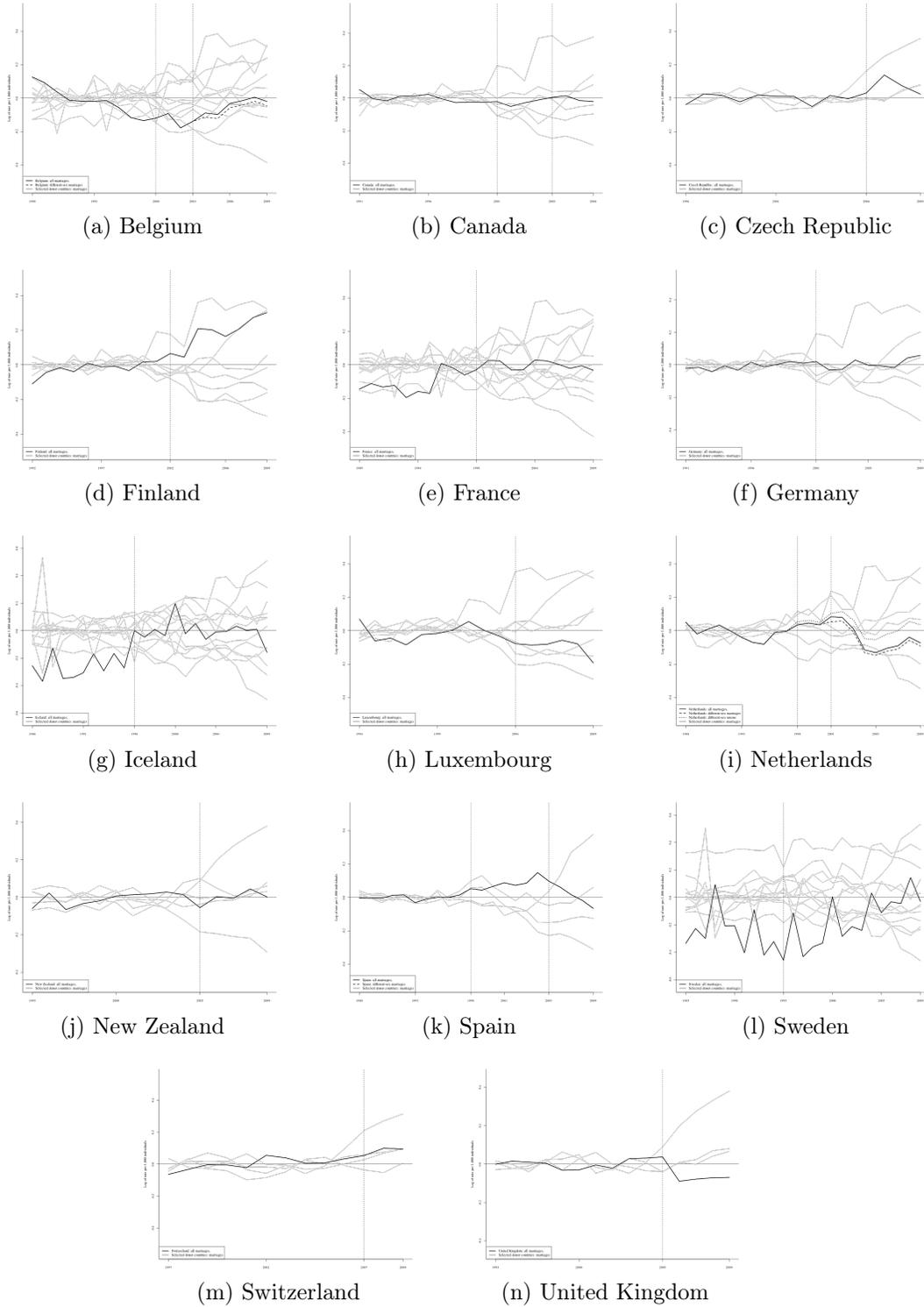


Figure 3: Actual-synthetic different-sex marriage rate gap, pre-intervention $MSPE \leq 5 \times$ adopter

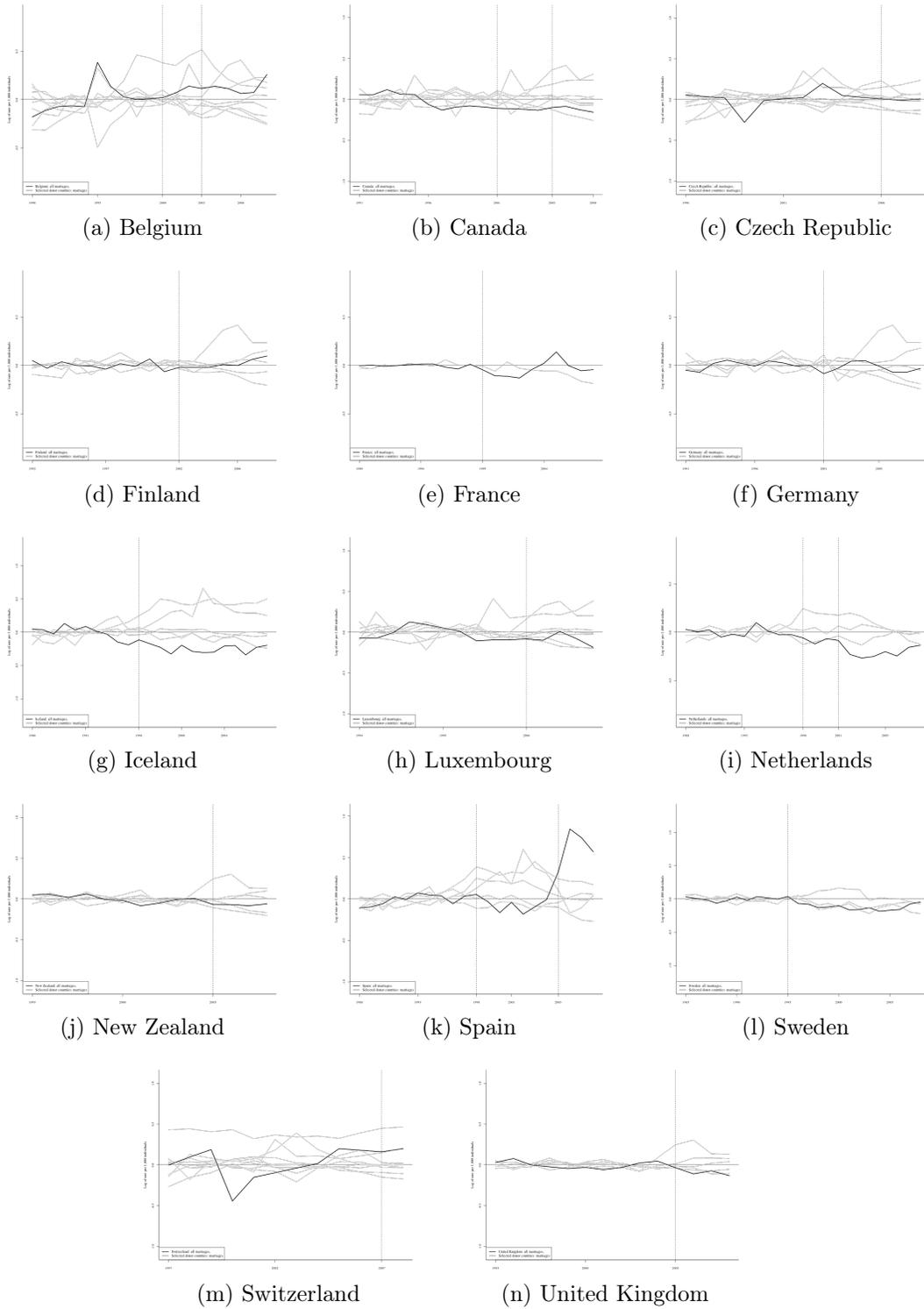


Figure 4: Actual-synthetic divorce rate gap, pre-intervention $MSPE \leq 5 \times$ adopter

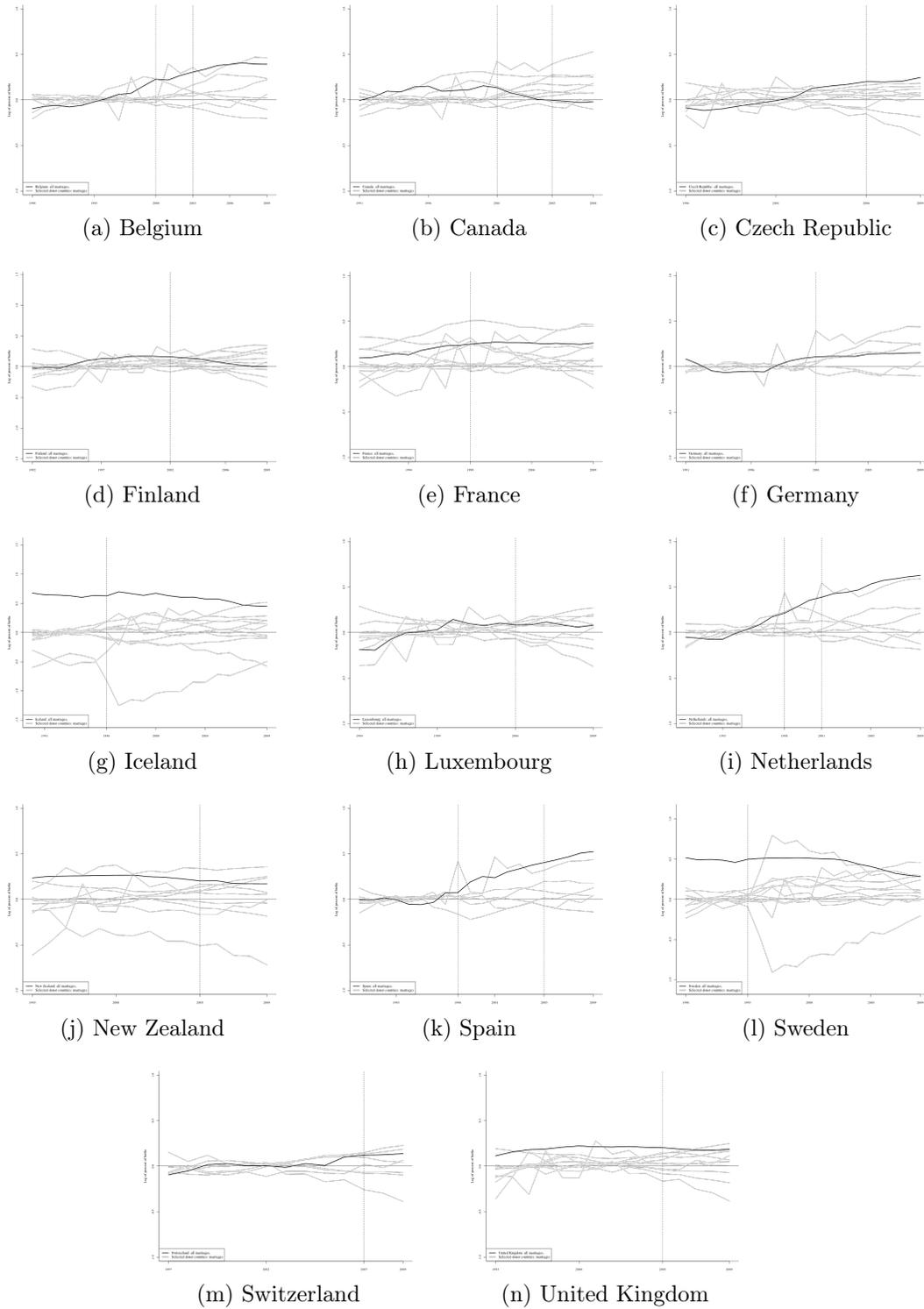


Figure 5: Actual-synthetic extramarital birth rate gap, pre-intervention $MSPE \leq 5 \times$ adopter

Table 1: Effect of same-sex partnership laws on family formation

	Analysis sample (excluding Spain)					Including Spain, unweighted (6)
	(1)	(2)	(3)	(4)	(5)	
A. Different-sex marriage rate						
<i>SSRP</i>	0.100** (0.045)	0.028 (0.038)	0.031 (0.027)	0.042 (0.038)	0.013 (0.035)	0.030 (0.037)
<i>SSM</i>	-0.158*** (0.039)	-0.050 (0.062)	0.039 (0.041)	0.020 (0.050)	-0.005 (0.063)	-0.087 (0.060)
Observations	809	809	694	809	809	839
B. Divorce rate						
<i>SSRP</i>	-0.217** (0.087)	-0.080** (0.031)	-0.040 (0.028)	-0.046*** (0.014)	-0.070 (0.066)	-0.089** (0.032)
<i>SSM</i>	-0.081 (0.141)	-0.073 (0.056)	-0.038 (0.086)	0.002 (0.114)	-0.042 (0.075)	0.111 (0.175)
Observations	790	790	677	790	790	819
C. Extramarital births rate						
<i>SSRP</i>	-0.113 (0.090)	-0.020 (0.048)	-0.048 (0.041)	-0.013 (0.027)	-0.103 (0.104)	-0.023 (0.048)
<i>SSM</i>	0.476*** (0.186)	-0.041 (0.067)	-0.085 (0.065)	0.068* (0.036)	-0.099 (0.068)	0.002 (0.071)
Observations	730	730	632	730	730	760
Country-specific linear trend	No	Yes	Yes	Yes	Yes	Yes
Country-year controls	No	No	Yes	No	No	No
Country-specific quadratic trend	No	No	No	Yes	No	No

Notes: The dependent variable in each panel is the natural logarithm of the indicator. All specifications include country and year effects. Country-year controls are the share of population in the 25–44 age group, the sex ratio, the labor force participation rates of men and of women in the 25–34 age group, the unemployment rates of men and of women in the 25–34 age group, the overall unemployment rate of men and of women, the overall labor force participation rates of men and of women, and the real GDP per capita. Robust standard errors clustered at the country level are reported in brackets. * significant at 10%, ** at 5%, *** at 1%.

Table 2: Effects of same-sex partnership laws on family formation in different samples

	Baseline (1)	Strong SSRP and never (2)	All SSRP and never (3)	SSM and never (4)	All SSRP and SSM (5)
A. Different-sex marriage rate					
<i>SSRP</i>	0.028 (0.038)	0.063 (0.052)	0.039 (0.044)	-0.034 (0.027)	0.034 (0.039)
<i>SSM</i>	-0.050 (0.062)			-0.033 (0.069)	-0.046 (0.066)
Observations	809	600	720	449	449
B. Divorce rate					
<i>SSRP</i>	-0.080** (0.031)	-0.093** (0.038)	-0.080** (0.036)	-0.127*** (0.042)	-0.049* (0.027)
<i>SSM</i>	-0.073 (0.056)			-0.101 (0.091)	-0.030 (0.044)
Observations	790	581	701	430	449
C. Extramarital births rate					
<i>SSRP</i>	-0.020 (0.048)	-0.048 (0.061)	-0.028 (0.057)	-0.034 (0.042)	0.021 (0.036)
<i>SSM</i>	-0.041 (0.067)			-0.104 (0.090)	0.016 (0.072)
Observations	730	532	652	371	437

Notes: The dependent variable is the natural log of the corresponding indicator. All specifications include country and year effects and country-specific trends. Data on Spain is excluded from all regressions. The “Strong SSRP and never” sample includes strong SSRP-adopting and never-adopting countries. The “All SSRP and never” includes all SSRP-adopting and never-adopting countries. The “SSM and never” sample includes SSM-adopting and never-adopting countries. The “All SSRP and SSM” sample includes all SSRP-adopting and SSM-adopting countries. Robust standard errors clustered at the country level are reported in brackets. * significant at 10%, ** at 5%, *** at 1%.

Table 3: Additional evidence on the effects of SSRP and SSM laws

	Baseline (1)	Including controls (2)	Quadratic trend (3)	Population weighted (4)	All SSRP SSM and (5)
A. Age at first marriage: men					
<i>SSRP</i>	-0.003 (0.006)	0.005 (0.003)	0.001 (0.002)	0.001 (0.005)	-0.002 (0.002)
<i>SSM</i>	-0.001 (0.008)	0.012*** (0.004)	0.014** (0.006)	0.002 (0.006)	0.010 (0.006)
Observations	488	419	488	488	231
B. Age at first marriage: women					
<i>SSRP</i>	-0.003 (0.008)	0.004 (0.003)	0.001 (0.002)	-0.003 (0.007)	-0.001 (0.004)
<i>SSM</i>	-0.006 (0.011)	0.012*** (0.004)	0.011*** (0.003)	0.004 (0.010)	0.004 (0.007)
Observations	478	419	478	478	221
C. Crude birth rate					
<i>SSRP</i>	-0.005 (0.040)	0.012 (0.032)	0.005 (0.037)	0.004 (0.038)	0.034 (0.036)
<i>SSM</i>	-0.001 (0.042)	0.084*** (0.024)	0.073** (0.033)	0.018 (0.046)	0.045 (0.043)
Observations	804	695	804	804	444
D. Unemployment rate among 25–34 year-old: men					
<i>SSRP</i>	-0.070 (0.126)	-0.031 (0.060)	0.041 (0.106)	-0.107 (0.179)	-0.008 (0.126)
<i>SSM</i>	0.210 (0.155)	-0.035 (0.053)	0.188* (0.094)	-0.060 (0.198)	0.329* (0.174)
Observations	695	695	695	695	386
E. Unemployment rate among 25–34 year-old: women					
<i>SSRP</i>	0.008 (0.127)	0.049 (0.037)	0.044 (0.116)	-0.013 (0.187)	0.051 (0.115)
<i>SSM</i>	0.271 (0.181)	-0.017 (0.064)	0.208** (0.091)	0.048 (0.199)	0.379* (0.197)
Observations	695	695	695	695	386
F. GDP per capita					
<i>SSRP</i>	0.021 (0.018)	0.021 (0.018)	0.003 (0.012)	0.023 (0.024)	0.009 (0.017)
<i>SSM</i>	0.001 (0.023)	0.001 (0.023)	-0.034*** (0.011)	0.043** (0.020)	-0.022 (0.023)
Observations	779	779	779	779	440

Notes: The dependent variable is the natural log of the corresponding indicator. All specifications include country and year effects. In addition, column 2 includes the full set of country-year controls (see the notes in Table 1), while column 3 includes a country-specific quadratic trend. The sample in column 5 is restricted only to SSRP-adopting and SSM-adopting countries. Robust standard errors clustered at the country level are reported in brackets. * significant at 10%, ** at 5%, *** at 1%.

Appendix

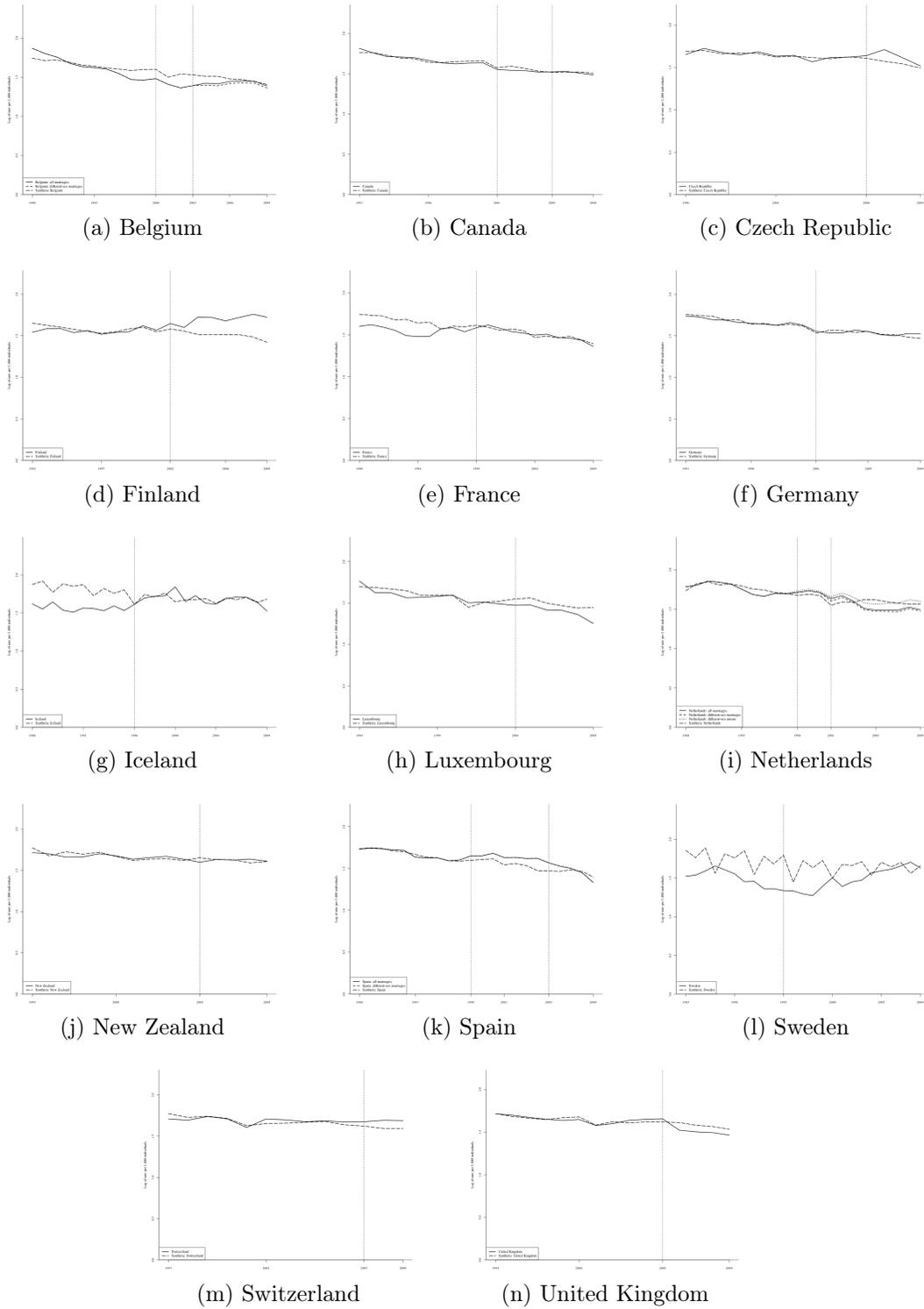


Figure A1: Evolution of actual and synthetic different-sex marriage rate

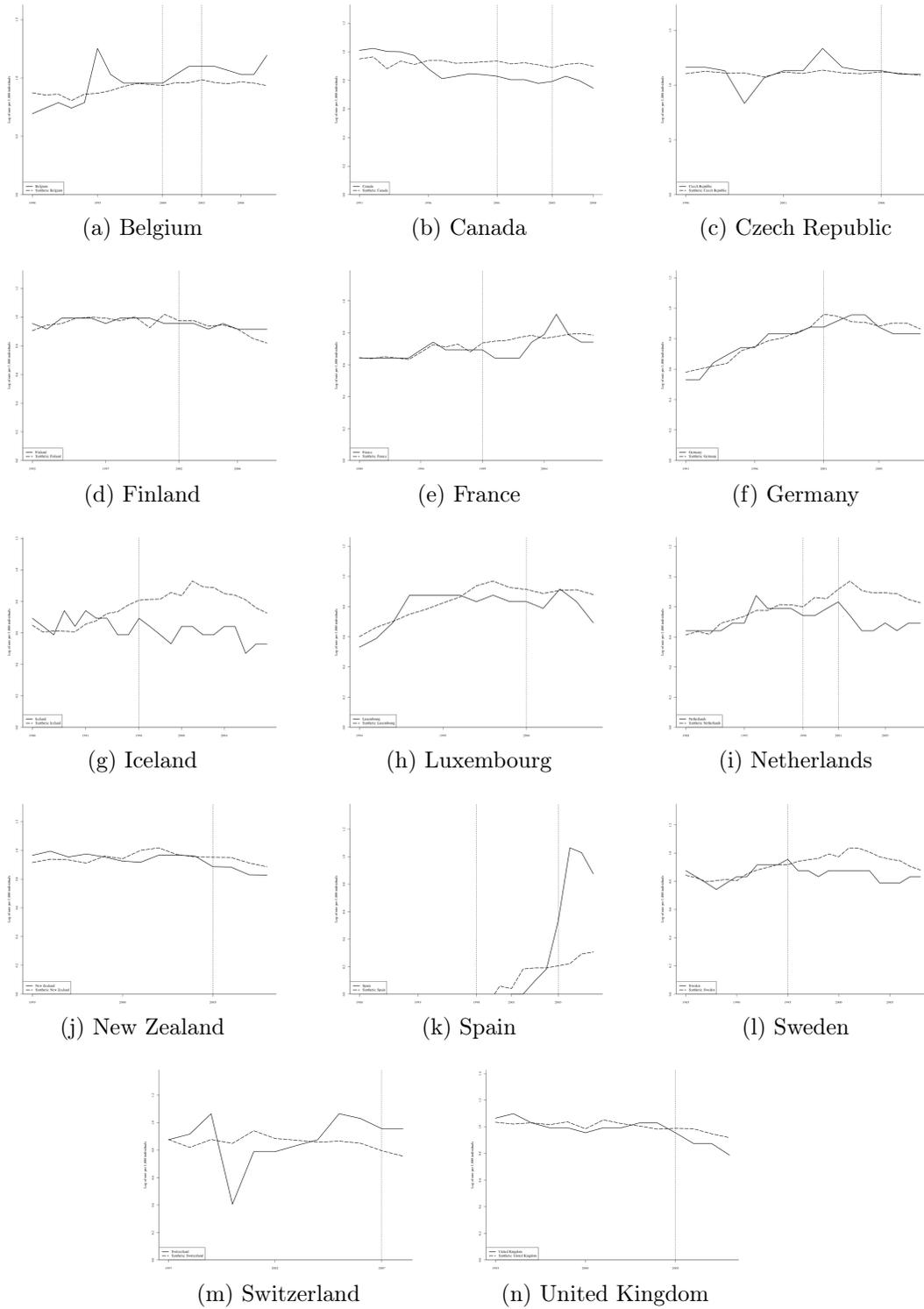


Figure A2: Evolution of actual and synthetic divorce rate

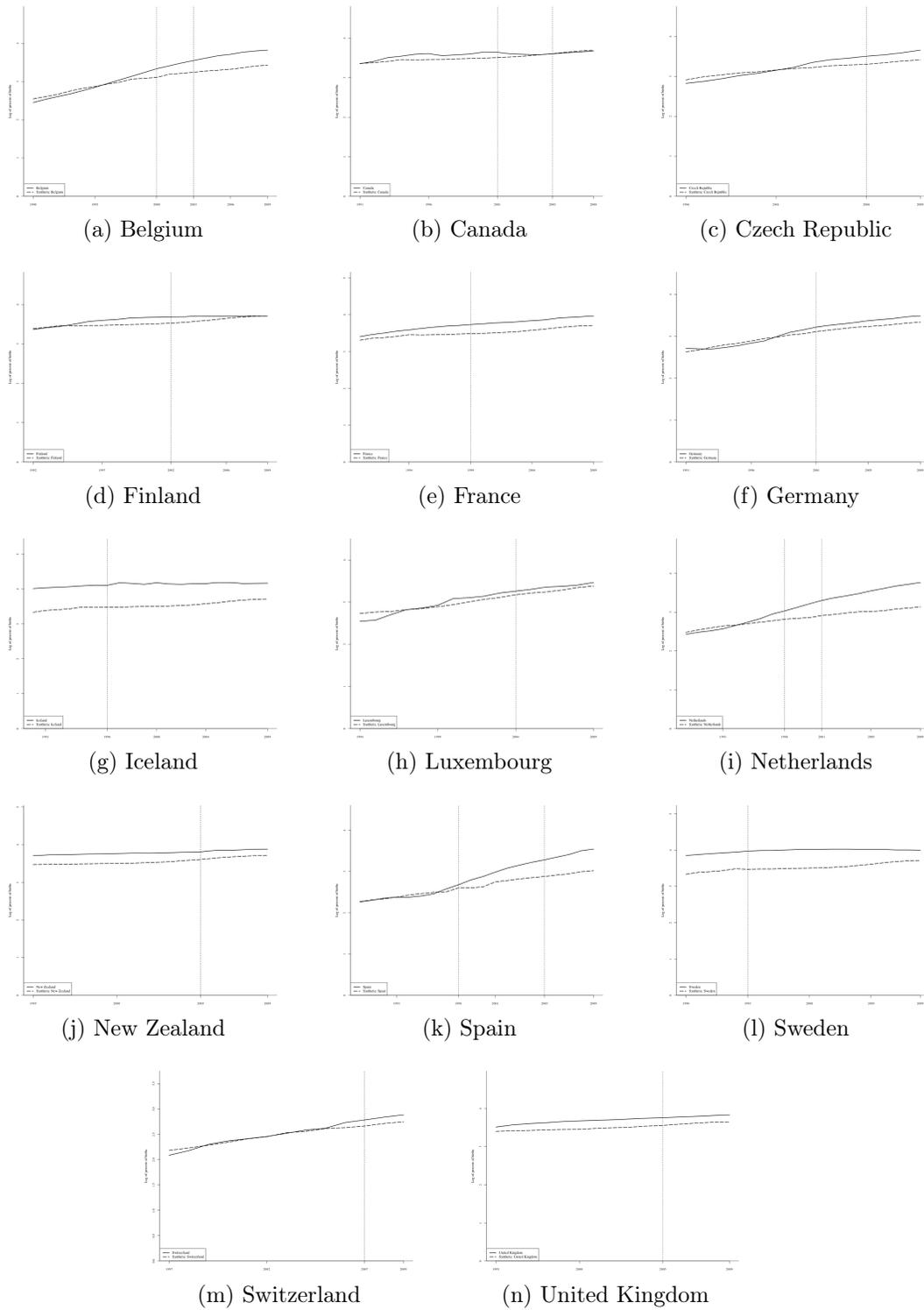


Figure A3: Evolution of actual and synthetic extramarital birth rate

Table A1: Data sources

Country	Year RP	Year SSM	Marriage rate	Divorce rate	Extramarital birth rate
Australia	—	—	ABS	ABS	UN, ABS
Austria	2010	—	Eurostat	Eurostat	Eurostat
Belgium	2000	2003	StatBel	Eurostat	Eurostat, StatBel
Canada	—	2005	HRSDC	StatCan	StatCan
Czech Republic	<i>2006</i>	—	Eurostat	Eurostat	Eurostat
Denmark	1989	—	Eurostat	Eurostat	Eurostat
Finland	2002	—	Eurostat	Eurostat	Eurostat
France	<i>1999</i>	—	Eurostat	Eurostat, INSEE	Eurostat, INSEE
Germany	<i>2001</i>	—	Eurostat	Eurostat	Eurostat
Greece	—	—	Eurostat	Eurostat	Eurostat
Hungary	2009	—	Eurostat	Eurostat	Eurostat
Iceland	1996	—	Eurostat	Eurostat	Eurostat
Ireland	2011	—	Eurostat, CSO	Eurostat	Eurostat
Italy	—	—	Eurostat	Eurostat	Eurostat
Japan	—	—	MH, JS	JSB	UN, MH
Korea	—	—	KNS	KNS	KNS
Luxembourg	<i>2004</i>	—	Eurostat	Eurostat	Eurostat
Netherlands	1998	2001	CBS	Eurostat	Eurostat
New Zealand	2005	—	SNZ	SNZ	SNZ
Norway	1993	—	Eurostat	Eurostat	Eurostat
Poland	—	—	Eurostat	Eurostat	Eurostat
Portugal	—	—	Eurostat	Eurostat, INE	Eurostat
Spain	—	2005	Eurostat	Eurostat	Eurostat
Sweden	1995	—	Eurostat	Eurostat	Eurostat
Switzerland	2007	—	Eurostat	Eurostat	Eurostat
Turkey	—	—	TSI	TSI	—
United Kingdom	2005	—	Eurostat, ONS	Eurostat	Eurostat
United States	—	—	SA, VS	VS, HS	CDC

Notes: Italics denote “weak RP” laws. All acronyms are official national or international statistical institutes except: HS = Historical Statistics of the United States, ICE = Intercensal estimates, JS = Japan Statistical Yearbook, JSB = Statistics Bureau Japan, KNS = Korea National Statistical Office, MH = Ministry of Health, Labour and Welfare of Japan, SA = Statistical Abstract of the US, SNZ = Statistics New Zealand, TSI = Statistical Indicators 1923-2009 of Turkey, UN = UN Demographic Yearbook, VS = Vital Statistics of the US.