Demography (2014) 51:1357–1379
DOI 10.1007/s13524-014-0312-y

Can Pro-Marriage Policies Work? An Analysis of Marginal Marriages

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Published online: 1 July 2014

Abstract Policies to promote marriage are controversial, and it is unclear whether they are successful. To analyze such policies, one must distinguish between a marriage that is created by a marriage-promoting policy (*marginal marriage*) and a marriage that would have been formed even in the absence of a state intervention (*average marriage*). We exploit the suspension of a cash-on-hand marriage subsidy in Austria to examine the differential behavior of marginal and average marriages. The announcement of an impending suspension of this subsidy led to an enormous marriage boom among eligible couples that allows us to locate marginal marriages. Applying a difference-in-differences approach, we show that marginal marriages are surprisingly as stable as average marriages but produce fewer children, children later in marriage, and children who are less healthy at birth.

Keywords Marriage-promoting policies · Marriage subsidies · Marital instability · Divorce · Fertility

Introduction

Policies to promote marriage are controversial (Amato 2007a, b; Furstenberg 2007a, b; McLanahan 2007; Struening 2007). Although an extensive empirical literature (Waite and Gallagher 2000) has documented a strong correlation between being married and better family outcomes, scholars do not agree whether this is a causal relationship.

Electronic supplementary material The online version of this article (doi:10.1007/s13524-014-0312-y) contains supplementary material, which is available to authorized users.

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Confounding factors that further marriage may also be beneficial to the outcomes under consideration, and the debate seems far from settled.

This statistical debate is accompanied by a political debate, which reflects a basic disagreement about whether the state should intervene in the private sphere. Liberal activists believe that unmarried relationships deserve the same acceptance and support as marriage. The feminist movement argues that existing policies to encourage marriage reinforce traditional gender roles, and homosexual rights groups object that the policies are indefensible because they exclude same-sex couples. On the other side, the marriage movement—a loose group of conservatives and religious leaders—favors public policies that strengthen the institution of marriage (Cherlin 2003).

This study solves neither the statistical nor the political debate, but it does add yet another important (and thus far neglected) aspect to this controversy. Supporters of marriage promotion contend that couples (and especially their children) should be better off within a marriage.[1] However, even under the assumption that marriage on average causally improves family outcomes, it is *a priori* unclear whether the state should pursue a pro-marriage agenda. The right question to ask is whether marriage improves the well-being of the couples who marry because of a marriage-promoting policy.

For this argument, it is essential to distinguish between an *average marriage* and a *marginal marriage*. We use the term "average marriage" to describe a couple who would marry with or without state intervention. In contrast, a "marginal marriage" is one that would not have occurred without state intervention.[2]

To account for the possibility that a policy affects the timing of marriage, we introduce a third type of marriage: *early average marriage*, defined by spouses who would have married in the counterfactual situation (i.e., in absence of the policy suspension) but would have married later. That means, in total, we distinguish between three different types of marriages, depending on their behavior in the absence of the policy suspension:

- Average marriage: spouses who would have married on the same date
- Early average marriage: spouses who would have married, but would have married later
- Marginal marriage: spouses who would not have married in the absence of the policy

The distinction between the first and the second type of marriage introduces a conceptual consideration of the difference between selection and timing. We assume that early average marriages and average marriages are not substantially different with respect to other dimensions apart from timing.

[1] In theory, legal marriage may increase well-being (compared with cohabitation) if marriage acts as a commitment device that fosters cooperation and/or induces partners to make more relationship-specific investments (Matouschek and Rasul 2008); this argument presumes that it is more costly to exit a marriage than to end cohabitation.

[2] Conceptually, we relate here to the treatment effect literature and employ a framework of potential outcomes (counterfactual reasoning). In the terminology of this literature, one could term marginal marriages "compliers" and average marriages "always-takers" (Imbens and Angrist 1994).

It is possible that marriage improves the well-being of average marriages but is not (as) beneficial to marginal couples. Therefore, it is important to know how different these two types of marriages are. Given that the benefits of marriage require a certain level of marital stability to materialize, an important question is whether marginal marriages are as stable as average marriages. Moreover, expected or actual stability is a prerequisite for marital investment. If children are the targeted beneficiaries of pro-marriage policies, a successful state intervention also requires that stable marginal marriages will have offspring. We think of these conditions as necessary (but not sufficient) conditions for marriage-promoting policies to work.

Based on theoretical grounds (Becker 1973, 1974), however, we expect marginal marriages to have lower match quality (compared with average marriages); to have partners who are less willing to make marriage-specific investments, such as children; and to exhibit a comparably higher baseline divorce risk. If these gradients predicted by theory turn out to be empirically relevant, a marriage-promoting policy is bound to fail because marginal marriages may be short-lived and may not produce children.[3] Thus, understanding selective marginal marriages in terms of marital stability and fertility behavior is of particular interest to researchers and policy-makers alike. Answering this question is empirically challenging because an individual classification of average, early average, and marginal marriage is impossible. We use a research design wherein an approximation of the shares of these three groups is sufficient to estimate the selection effect.

We use the suspension of a cash-on-hand marriage-promoting policy in Austria. Starting in the early 1970s, two Austrian citizens, both marrying for the first time, would receive €4,250 (values adjusted for inflation; 2010). At the end of August 1987, however, the suspension of this marriage subsidy was announced, effective as of January 1, 1988. This led to an enormous marriage boom of more than 350 % (see Fig. 1). Clearly, part of the marriage boom was due to timing (i.e., early average marriages). However, using individual-level data on the entirety of Austrian marriages, we show that approximately one-half of the couples who married between October and December 1987 were motivated by the cash transfer and thus constitute marginal marriages.

We exploit the eligibility criteria to set up a difference-in-differences framework, which allows us to estimate the differential divorce and fertility behavior of marginal couples. Surprisingly, we find hardly any evidence of lower marital stability of marginal marriages. We do find, however, that marginal marriages have fewer children and have them later in marriage. The children born to marginal marriages exhibit a lower health at birth.

These findings contribute to different strands of economic literature and hold important implications for policy-makers. First, studies investigate whether the state can effectively encourage people to marry or to stay married. Although empirical work consistently shows that individuals respond to tax incentives in their marital decisions, as predicted by theory, the magnitudes of these effects are typically small or short-lived (e.g., Alm et al. 1999; Whittington and Alm 1997). The empirical evidence on behavioral effects created by transfers is less consistent. Based on a comprehensive

[3] In the worst case, the state may create unstable marriages with additional children—that is, children who would have not been conceived in the counterfactual without policy intervention.

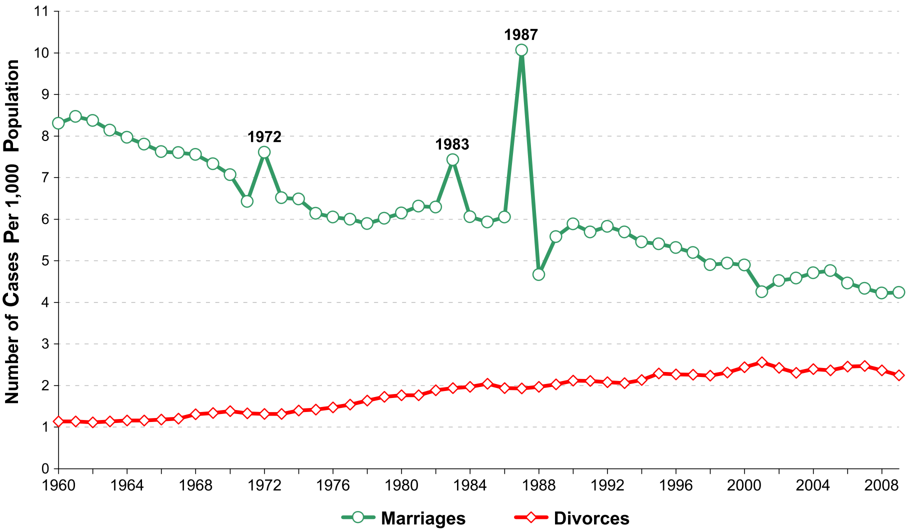

Fig. 1 Annual number of marriages and divorces per 1,000 of population, Austria 1960 through 2009. Own calculations based on data from Statistics Austria; details are available upon request. As of December 31, 1971, the deductibility of furnishings and articles of daily use up to 70,000 Austrian schillings within the first five years after the establishment of a new household by newlyweds was abolished. However, per January 1, 1972, a marriage subsidy for every person with unrestricted tax liability in Austria who had never been married of 7,500 Austrian schillings was introduced. Thus, two Austrian citizens, both marrying the first time, received a total of 15,000 Austrian schillings (2010: € 4,250 or $5,680 US). As of January 1, 1984, the tax deductibility of dowry was abolished. As of December 31, 1987, the marriage subsidy was suspended without any replacement. This was announced August 26, 1987

survey of the literature from the last three decades, Moffitt (1998) concluded that transfer programs do affect marital decisions as well. As argued by Blank (2002), identifying effects of tax and welfare reforms on family formation is difficult. These reforms are often complicated, only a relatively small share of the population gets married in any given year, and family behavior seems to be much more sluggish and resistant compared with labor market behavior. In contrast, the reform studied here was straightforward and had an enormous effect on marriage behavior.

Second, this article relates to the literature interested in the effects of marriage. Only a small number of studies have offered a credible research design to identify a causal effect. Almost all these studies exploited exogenous variation in marital status resulting from policy interventions. Two studies used a marriage boom in Sweden—created by the widow's pension reform in 1989—to estimate the corresponding treatment effect of marriage on children's school outcomes (Björklund et al. 2007) and on spouses' labor market outcomes (Ginther and Sundström 2010). The former did not find any effect of marriage on children's school performance. The latter found a small marriage premium for men and a small penalty for women, wherein both effects seem to be the result of increased specialization of married couples. More recently, Fisher (2010) used differences in U.S. marriage tax penalties or subsidies to instrument for marital status. She found that the average married couple—whose marital status is determined by (dis)incentives created by tax law—does not have health outcomes that differ from those of their unmarried counterparts. However, some evidence suggests that complying men with low education benefit from marriage and that complying women with higher education report lower health if married.[4]

Finally, the results should be of interest to policy-makers. In most Organisation for Economic Co-operation and Development (OECD) countries, different marriage-promoting policies are in place, and we are not aware of any systematic evaluation of these.[5] The U.S. federal assistance program Temporary Assistance for Needy Families (TANF)—although being primarily a cash-assistance program—has also explicit marriage-promoting components.[6] This program provides states with block grants that can be used for a wide range of activities to end welfare dependency by encouraging not only work but also marriage and two-parent families. Examples of other U.S. policies implemented to help increase marriage rates and stabilize existing marriages are the introduction of covenant marriages (Brinig 1999) as well as the removal of marriage penalties in tax codes (Alm et al. 1999), pension systems (Baker et al. 2004), and Medicaid programs (Yelowitz 1998). Similar policies can be observed in many other OECD countries.

Institutional Setting

The Austrian marital landscape could be characterized as being situated between two extremes defined by the United States and Scandinavia.[7] As discussed by Stevenson and Wolfers (2007), Americans marry, divorce, and remarry at rates higher than in any other developed country. Only a comparably small share of the population believes that marriage is an outdated institution, and cohabitation is still not as widespread. Consequently, although nonmarital fertility is rising, the United States has still a comparably low share of out-of-wedlock births. In contrast, in Sweden, marriage rates are low; cohabitation rates are high; and since the 1990s more than one-half of Swedish children are born out of wedlock. Divorce is a socially widely accepted option to exit bad marriages, more so than in the United States, and a higher stock of people are currently divorced. Austrians marry less than Americans but more than Swedes. A corresponding intermediate share of Austrians think that marriage is an outdated institution. The share of cohabitating persons in Austria is somewhat larger than in the United States but substantially lower than in Sweden.[8] Similarly, divorce is more accepted in the Austrian society than in the United States but not as accepted as in Sweden. The stock of divorced people is, however, very comparable in Austria and the

[4] In a recent working paper, Persson (2013) revisited the analysis of the Swedish reform. Other papers (Dahl 2010; Finlay and Neumark 2009) have concentrated on subpopulations (prison inmates and teenagers) that are typically not the target of pro-marriage policy.

[5] For a comprehensive overview of U.S. policies promoting marriage, see Gardiner et al. (2002) and Brotherson and Duncan (2004). Wood et al. (2012) evaluated relationship skills education programs serving unmarried parents.

[6] TANF was created by the Personal Responsibility and Work Opportunity Reconciliation Act instituted in 1996. It replaced the welfare programs known as Aid to Families with Dependent Children (AFDC), the Job Opportunities and Basic Skills Training (JOBS) program, and the Emergency Assistance (EA) program.

[7] See the summary of key demographic trends for Austria and some selected countries in Table A.1 in Online Resource 1.

[8] Zeman (2003) found that cohabitation (versus marriage) in Austria is basically determined by education and religious denomination, which are variables that we can control for in the empirical analysis.

United States. Americans have substantially more children, but the share of children born out of wedlock is very similar between the two countries.

In Austria, newlywed couples had been traditionally subsidized via tax deductions. Starting from 1972, the Austrian government switched to a more straightforward marriage-promoting policy and provided a monetary premium. Every person with unrestricted tax liability in Austria who had never been married before received 7,500 Austrian schillings upon marriage.[9] This corresponds to approximately €2,125 in 2010. Thus, two Austrian citizens, both marrying for the first time, received a total of €4,250. Although the old tax deductibility scheme was heavily income-dependent, the new scheme offered a flat-rate transfer, which might be more visible and thus be a stronger incentive to marry. This marriage subsidy had been a heavily discussed election pledge of the Social Democratic Party of Austria in its 1971 election campaign, which it adhered to after gaining the majority in Parliament in 1971. Over time, the regulations of this marriage subsidy did not change, and the transfer had not been adjusted for inflation. Almost 16 years later, on August 26, 1987, the Minister of Finance quite unexpectedly announced the suspension of this marriage subsidy as of December 31, 1987, without any compensation.[10] The focus of this article is on the (announcement of the) suspension of the marriage subsidy.

The announcement of the suspension of the marriage subsidy provided a clear incentive to marry. Indeed, this led to an enormous marriage boom in the three months from October to December 1987 (see Fig. 1). Compared with the same time period in 1986 (with 7,844 marriages), marriages increased by more than 350 % to 35,847 in 1987. Clearly, part of the marriage boom might be simply due to timing; however, even based on theoretical grounds, an increase in marriage rates would be expected to result in a different selection into marriage.

In a standard family matching model with frictions (Mortensen 1988), such an unexpected announcement decreases the expected present value of a continued search. Search costs increase sharply because of the time constraint introduced by the announcement of the suspension; the value of a continued search (for a better match) is reduced because there are no subsidies after the suspension. Thus, the increase in the incidence of marriage in the last quarter of 1987 can be explained by a reduction in the reservation match quality—that is, in the minimum acceptable match quality sufficient for a marriage. Marginal marriages are precisely defined as those matches that became acceptable only because of the reduction in the reservation match quality caused by the announcement of the suspension. Consequently, marginal marriages should be of lower match quality than average marriages, whose match quality would be sufficient even without state intervention. This empirical analysis focuses on a quantification of this selectivity with respect to marital stability, fertility behavior, and marital offspring's health: we refer to this as the "selection effect."

A second potential effect of the policy intervention is what we term the "transfer effect," which describes the behavioral response resulting from additional resources on

[9] See Austrian Law: BGBl. 460/1971. Because it is not always clear whether foreigners are tax liable in Austria in such a sense, we eliminated foreign citizens from the analysis.

[10] See, for instance, *Kronen Zeitung* on August 27, 1987. The suspension was argued with a necessity of budget cuts and was quickly enacted without any further parliamentary discussion on October 21, 1987. Detailed research of the daily press archives shows that there was also no prior discussion of such a suspension, nor was there a parliamentary debate before August 1987.

family outcomes (divorce likelihood and fertility) in the absence of selection: the true causal effect of the reform.[11] Remember that the transfer was just a one-time payment, and the amount (while not negligible) was probably not significant enough to have long-lasting effects on behavior over time. Therefore, the focus of the empirical analysis is on the selection effects; nevertheless, the estimation strategy also enables an identification of any transfer effects by comparing the period before the announcement of the suspension with the time period after actual suspension.

Estimation Strategy

We are interested in the differential divorce likelihood and fertility behavior between a marginal marriage and an average marriage. In other words, we want to learn by how much a couple who has married just because of a state intervention is on average more (or less) likely to divorce or to have offspring, compared with a couple who would have married even without this intervention. We argue that this divorce and fertility gradient is a parameter that should be taken into account before adopting (costly) marriage-promoting policies because a certain level of marital stability and marital offspring is a necessary condition for pro-marriage policies to succeed.

In this empirical analysis, a marginal marriage is defined as a couple who has married because of the announcement of the suspension of the marriage subsidy. The Austrian subsidy suspension by January 1, 1988, had been implemented without any compensatory measures; as mentioned earlier, it was announced abruptly by the Minister of Finance (without any prior discussions) at the end of August 1987. The suspension thus provides a clear break. The introduction of the subsidy was not as unexpected, because it had been introduced following a heavy discussion in the 1971 election campaign. Nevertheless, an examination of the introduction provides consistency checks of the main estimation results.

Data

We combine different administrative data sources. Most importantly, we use data from the Austrian Marriage Register, which covers the entirety of marriages and includes the date of marriage as well as the spouses' marital histories, place of residence, ages at marriage, religious denominations, and citizenships. Information on the spouses' countries of birth and on the number, age, and sex of any premarital children has also been recorded since 1984.[12] For further specifications, we extend the data set with information on the spouses' labor market statuses and occupations from the Austrian Social Security Database (ASSD) (see Zweimüller et al. 2009). To obtain information on marriage duration, we merge the Austrian Divorce Register. The base sample consists of all 550,294 marriages that took place between 1981 and 1993; thus, we include

[11] The transfer effect can be highlighted by the following thought experiment. Imagine the existence of a marriage subsidy that is not publicly announced, but couples who marry (or a subgroup of them) still receive a subsidy upon marriage. Here, the transfer effect is the difference in the counterfactual outcomes (with and without subsidy).

[12] In the mid-1980s, about every fifth child was born out of wedlock. This number had increased to every fourth child by 1995.

approximately six years of data before and after the reform. From these marriages, 150,767 had ended in divorce by the end of 2007.[13] To obtain information on mortality and out-migration, we match information from the Austrian Death Register and the ASSD,[14] resulting in 36,893 right-censored observations owing to death and 5,484 owing to out-migration. Finally, for the analysis of fertility behavior and children's health at birth, we use data from the Austrian Birth Register on children born to mothers who married between 1984 and 1993.[15] This includes all births in Austria with individual-level information on socioeconomic characteristics and different birth outcomes. Approximately 68 % of the 401,314 marriages in this sample had marital offspring by 2007.

Locating Marginal Marriages

To estimate the selection effect, we need to identify average, early average, and marginal marriages. Although such identification is impossible at an individual level, the research design allows us to quantify their aggregate number (over a period of three months). First, we exploit the fact that only a subset of the population had been eligible for the marriage subsidy, and we distinguish between three groups of couples: a comparison group, comprising couples in which no spouse is eligible; treatment group 1 (T^1), comprising couples in which one spouse is eligible; and treatment group 2 (T^2), comprising couples in which both spouses are eligible. Thus, spouses from T^2 couples—in which neither partner had ever been married before—faced the highest incentive to marry; these marriages were subsidized in sum with 15,000 Austrian schillings. T^1 couples comprise those in which one spouse had been married before; those couples received only 7,500 Austrian schillings. The comparison group couples consist of spouses who had both been previously married, and were thus not eligible for any marriage subsidy.

Figure 2 shows the number of monthly marriages by group for 1986, 1987, and 1988. In 1986 (the year before the announcement of the suspension), the seasonal pattern for each group is fairly uniform, peaking in May. For the comparison group, the patterns overlap in all three years. However, T^1 and T^2 marriages show a clear divergence of the normal seasonal pattern starting in October 1987. The announcement of the suspension of the marriage subsidy at the end of August led to a exceptionally high number of T^1 and T^2 marriages from October through December; in September, there is no artificial increase. It seems that couples needed at least one month to plan their weddings. The first quarter of 1988 saw somewhat smaller numbers of T^1 and T^2 marriages, which is most likely due to some couples who married in advance of the suspension of the transfer.

Figure 3 shows the annual number of marriages of T^2 couples from 1981 through 1993. It seems that the long-run trend of this series—that is, the trend that would have been observed without the suspension of the marriage subsidy—can be approximated

[13] No major divorce law reform took place through the sample period. Divorce by mutual consent and unilateral divorce have been available since 1978. Divorce by mutual consent is possible after at least six months of separation, and unilateral divorce is available after three years apart.

[14] We presume that a person with no ongoing records in the ASSD has left Austria.

[15] The reduced sample period is a result of the limited possibility to link the Austrian Marriage Register with the Austrian Birth Register before 1984.

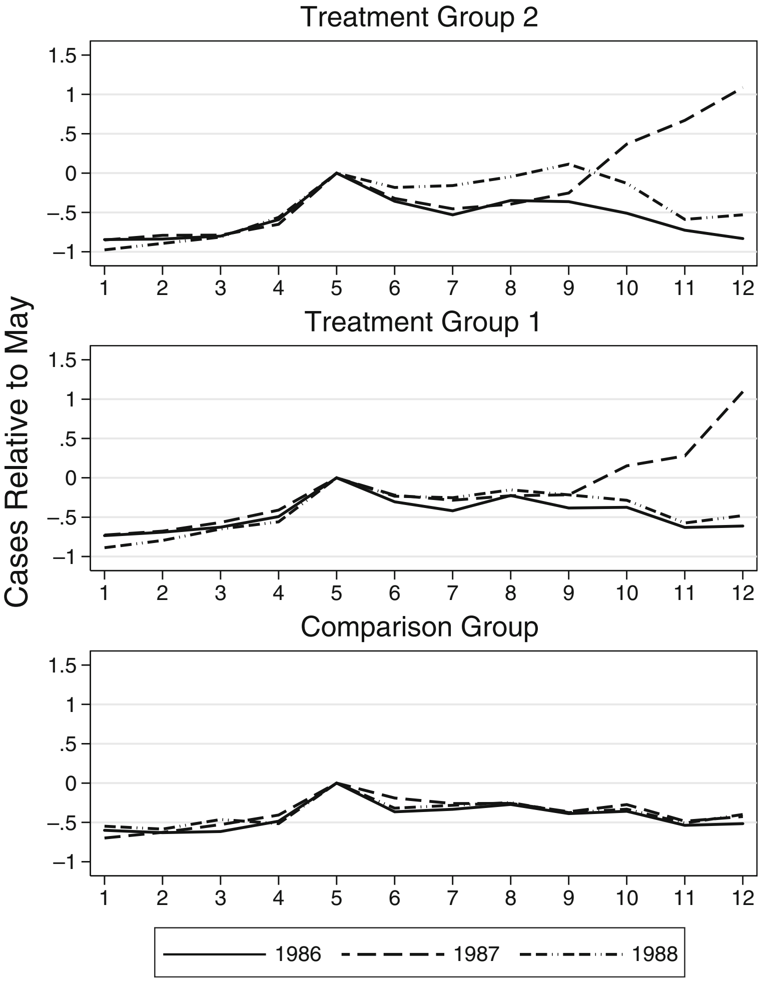

Fig. 2 Monthly number of marriages by group in the years 1986 to 1988. Own calculations based on data from the Austrian Marriage Register. These graphs show the number of monthly marriages for three groups in the years in 1986, 1987, and 1988. The monthly number of marriages is normalized to May of each year (and group). Treatment group 2 comprises couples in which neither spouse has ever been married before. Treatment group 1 consists of couples in which only one spouse has been married before. The comparison group covers couples in which both spouses had been married before

well by a linear interpolation between 1986 and 1990. This is illustrated by the dashed horizontal line. This approximation assumes that couples did not advance their planned weddings by more than 26 months (i.e., from December 1989 to October 1987).[16] The additional marriages in 1987—that is, the number of marriages that exceed the interpolated long-run trend in the marriage rate—is equal to 27,080 and can be attributed to two groups: (1) couples who had planned to marry (in the near future) and decided to marry earlier to take advantage of the cash subsidy and (2) couples who had no plans to marry but married just to receive the cash subsidy. The former group represents early

[16] This assumption is not crucial for the estimation analysis. Moreover, we will discuss alternative (more elaborated) interpolation strategies.

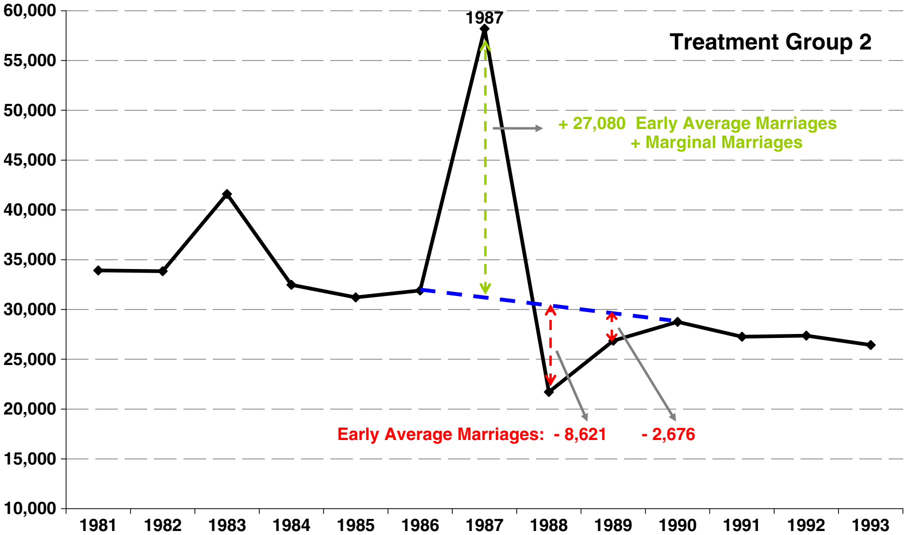

Fig. 3 Quantification of (early) average marriages and marginal marriages. Own calculations based on data from the Austrian Marriage Register. This graph shows the number of yearly marriages of treatment group 2 couples (i.e., neither spouse has been married before) from 1981 through 1993. See also notes to Figs. 1 and 2. Alternative approximations of the number of early average, average, and marginal marriages are discussed in Online Resource 2

average marriages, and the latter group constitutes the marginal marriages in the research design.

We argue that the number of early average marriages can be quantified by the difference between the interpolated long-run trend in the marriage rate and the actual number of marriages between 1988 and 1989; these two shortfalls are equal to 8,621 and 2,676, respectively (see the vertical arrows). Consequently, the number of marginal marriages is equal to 15,785—the difference between the surplus from 1987 and the sum of the shortfalls from 1988 and 1989. Because, by definition, these marginal marriages can be formed only after the announcement of the suspension (and before January 1, 1988), this number can be attributed to marriages formed after August 26, 1987. Clearly, the planning of a wedding requires some time. One has to at least make an appointment at City Hall. Figure 2 indicates that the marriage boom began in October, suggesting that approximately one month of wedding planning was necessary. Relating the 15,785 marginal marriages (and the 11,297 early average marriages) to all 31,005 T^2 marriages formed between October and December 1987 reveals that approximately 51 % of these were marginal marriages, 36 % were early average marriages, and the remaining 13 % were average marriages. Applying an equivalent procedure to T^1 marriages shows a comparably lower share of marginal marriages of 44 % (see the upper panel of Table 1).

Clearly, this is not the only possibility to approximate the shares of different types of marriages. However, alternative procedures produce very comparable estimates. In Online Resource 2, we present two alternatives in more detail. First, we discuss an alternative linear approximation based on the period before the announcement of the suspension. This results in an estimate of 46 % for marginal marriages and 41 % for

Table 1 Characteristics of average and marginal marriages

	Treatment Group 2			Treatment Group 1			Comparison Group		
	1986	1987	1988	1986	1987	1988	1986	1987	1988
Approximate Shares									
Marginal marriages	0.0	50.9	0.0	0.0	44.2	0.0	0.0	0.0	0.0
Early average marriages	0.0	36.4	0.0	0.0	26.6	0.0	0.0	0.0	0.0
Average marriages	100.0	12.7	100.0	100.0	29.3	100.0	100.0	100.0	100.0
Spouses' Age and Age Difference									
Age of wife	23.8	24.1	24.3	30.4	31.3	30.6	40.2	40.3	40.3
Age of husband	26.5	26.6	26.8	34.8	35.8	35.0	45.5	45.6	45.4
Age difference	2.0	2.5	2.5	4.4	4.6	4.4	5.3	5.2	5.2
Number of premarital children	0.3	0.3	0.3	0.2	0.3	0.2	0.1	0.1	0.1
Distribution of Spouses' Religious Denomination									
Both Catholic	86.2	84.4	84.9	67.2	66.7	64.5	53.5	55.8	53.1
Both nondenominational	1.4	1.9	1.7	3.9	4.9	6.2	11.1	9.8	11.9
Both other denomination	1.1	0.9	1.1	0.9	1.1	1.8	1.5	1.6	1.2
Catholic, nondenominational	4.1	5.3	4.7	14.9	15.4	16.4	19.2	20.8	21.6
Catholic, other denomination	6.7	7.0	7.1	10.8	10.4	9.3	12.4	9.4	9.2
Other, nondenominational	0.5	0.6	0.4	2.3	1.5	1.9	2.2	2.6	3.0
Wife's Labor Market Status									
Employed	60.5	61.2	62.5	51.3	48.2	52.3	44.7	44.4	49.1
Blue-collar worker	23.2	24.1	20.3	18.2	18.6	18.0	17.0	15.5	17.2
White-collar worker	33.3	34.2	37.5	27.8	25.2	27.1	21.5	22.9	23.9
Other employment	4.0	2.9	4.7	5.3	4.4	7.2	6.2	6.0	8.0
Unemployed	8.2	7.3	9.7	7.7	7.4	8.2	5.4	6.4	5.8
Out of labor force	31.3	31.5	27.9	41.0	44.4	39.5	49.9	49.2	45.2
Husband's Labor Market Status									
Employed	71.9	70.1	76.7	59.8	58.8	65.3	52.7	51.1	56.9
Blue-collar worker	43.0	43.9	38.7	29.6	30.4	27.9	22.1	21.0	23.1
White-collar worker	20.3	19.9	25.1	20.0	19.9	22.4	19.0	18.2	16.9
Other employment	8.6	6.3	12.9	10.2	8.5	15.0	11.6	11.9	16.9
Unemployed	1.9	2.3	1.7	3.2	2.7	3.3	3.0	3.6	2.7
Out of labor force	26.2	27.6	21.6	36.9	38.5	31.5	44.3	45.3	40.5
Number of Observations	5,658	31,005	5,258	1,280	3,884	1,229	906	958	967

Notes: Authors' calculations based on data from the Austrian Marriage Register and the Austrian Social Security Database (ASSD). In each column, only marriages between two Austrian citizens formed between October and December are included. From January 1, 1972, through December 31, 1987, every person with unrestricted tax liability in Austria who had never been married before received 7,500 Austrian schillings (2010: €2,125 or $2,840 US) upon marriage. The suspension of this marriage subsidy was announced on August 26, 1987. Treatment group 2 comprises couples in which neither spouse has ever been married before. Treatment group 1 consists of couples in which only one spouse has been married before. The comparison group is couples in which both spouses had been married before. Age and age difference are measured in years. Labor market status is constructed by matching data from marriage and divorce registers with those from the ASSD, using birth dates of both spouses. In case of ambiguous matches (about 36 %), we use the average labor market states of all such matches.

early average marriages. A more elaborate regression-based approach leads to similar shares of marginal (45 %) and early average marriages (38 %).

Table 1 compares the average characteristics of spouses from the two treatment groups and the comparison group (who married between October and December) for 1986, 1987, and 1988, highlighting three notable findings. First, baseline differences exist between the three groups. As expected, the higher the divorce experience of the couples is (i.e., moving from T^2 to T^1 and to comparison group marriages), the older the spouses are, the higher their age difference, the less likely they are both Catholic, and the lower their number of premarital children. Second, as expected, the composition of the comparison group varies little over time. The only exception is the spouses' labor market status, which is affected by the business cycle; in 1987, the unemployment rate was higher than in the other two years. Third, given that approximately one-half of the T^1 and T^2 marriages in 1987 were marginal marriages (see the upper panel of Table 1), this comparison should show observable differences between average and marginal marriages. However, somewhat surprisingly, these numbers suggest that average and marginal marriages are quite similar along measurable characteristics documented in the data. For instance, spouses from both groups do not differ significantly in their age or religious denominations. The only notable difference is the higher incidence of premarital children among T^1 marriages.

Difference-in-Differences Estimation Strategy

For the different outcome variables, we use the same specification but different estimation methods. To estimate the duration of a marriage, we use Cox proportional hazard models (Cox 1972); for the analysis of fertility behavior and marital children's health at birth, we use ordinary least squares (OLS).

In the Cox model, the hazard rate at marriage duration t—that is, the risk that a marriage dissolves at time t, provided it lasted that long—is explained by a nonparametric baseline hazard $h_0(t)$ that is augmented because of the influence of covariates $\mathbf{X}$:

$$h(t|\mathbf{X}) = h_0(t)\exp(\mathbf{X}\beta). \tag{1}$$

A Cox model is flexible because the baseline hazard remains unspecified.[17] To estimate the selection and the transfer effect, we exploit the comparison group of non-eligible couples. Consequently, we implement a difference-in-differences (DiD) estimation strategy, where the treatment is the announcement of the suspension of the marriage subsidy. The estimation strategy

[17] All the results are presented as hazard ratios—that is, the hazard rate of spouses with characteristics $\mathbf{X}^*$ relative to the hazard rate of the base group $\mathbf{X}$, $h(t\,|\,\mathbf{X}^*)/h(t\,|\,\mathbf{X})$. Figure D.1 in Online Resource 4 plots the hazard function by group for marriages formed between October and December in 1986, 1987, and 1988. For all groups (and years), given a marriage that has survived until its third year, the divorce hazard actually decreased over time. In the case of the control and the treatment group 1, there are no statistically significant differences between the hazard functions of 1986, 1987, and 1988; similar results hold for treatment group 2, with the exception of the very first periods.

deviates in some aspects from the conventional DiD-framework and specifies $\mathbf{X}\beta$ as follows:

$$\mathbf{X}\beta = \beta_0 + \beta_1 T^1 + \beta_2 T^2 + \beta_3 TP + \beta_4 postTP + \beta_5 T^1 \times TP + \beta_6 T^2 \times TP \\ + \beta_7 T^1 \times postTP + \beta_8 T^2 \times postTP + \gamma \times X_i + u_i. \tag{2}$$

First, we have more than one treatment group. As introduced earlier, we distinguish between spouses from the two treatment groups (T^1 and T^2) and the comparison group. The specification therefore allows for a different baseline hazard of T^1 and T^2 marriages (i.e., β_1 and β_2 compared with comparison group marriages). Second, we distinguish not only before- and after-treatment periods but also three time periods. The pretreatment period starts with the sample in 1981 and runs through September 30, 1987. The treatment period (*TP*) is October 1, 1987, through December 31, 1987. Thereafter, the post-treatment period (*postTP*) starts. Consequently, we allow marriages formed in these three periods to have a different divorce hazard (see β_3 and β_4).

The strategy used here deviates somewhat from the conventional DiD framework with respect to the identifying assumptions. First, one typically assumes that the trends in the outcome variables would have been the same for the treatment and the comparison group in the absence of the treatment. Second, the composition of the two groups is usually assumed to be unchanged over the course of the treatment. In principle, we also assume that the trend in the outcome variables would have been the same across all groups in the counterfactual situation without treatment; however, we relax this assumption to some degree by allowing for group-specific linear trends. In contrast, we do not rule out compositional changes in the treatment groups during the treatment period. Instead, we aim to quantify these effects because they allow us to infer the selection effects. In other words, we expect the composition of treated couples to change during the treatment period because a large share of these are marginal marriages.

The coefficients on the interaction terms between the two treatment group indicators and the treatment period dummy variable (β_5 and β_6) provide the estimates for the compositional changes of T^1 and T^2 marriages: they should yield the difference in divorce risk between average and marginal marriages. Unfortunately, the treatment group does not consist solely of marginal marriages. As shown earlier, approximately one-half of the treatment group is composed of (early) average marriages. The measured coefficients β_5 and β_6 are therefore underestimating the true selection effect. Given the approximate composition of half average and half marginal marriages, the coefficients should be multiplied by two to produce an estimate of the respective selection effects.

The estimates of the transfer effects for T^1 and T^2 marriages are given by β_7 and β_8, respectively. Because β_7 and β_8 are based on a comparison of the post-treatment period and the pretreatment period, they measure the effect of the suspension of the subsidy, and we have to flip their signs to learn the causal effect of the additional resources on the divorce hazard. For clarification, Fig. 4 provides a graphical presentation of the setup.

Importantly, for the clean identification of these transfer effects, we have to assume the absence of any compositional effects prior to the announcement of its suspension. In order to verify the plausibility of this assumption, we examine the introduction of the marriage subsidy in the year 1972. Based on a comparable DiD framework, we do not find any

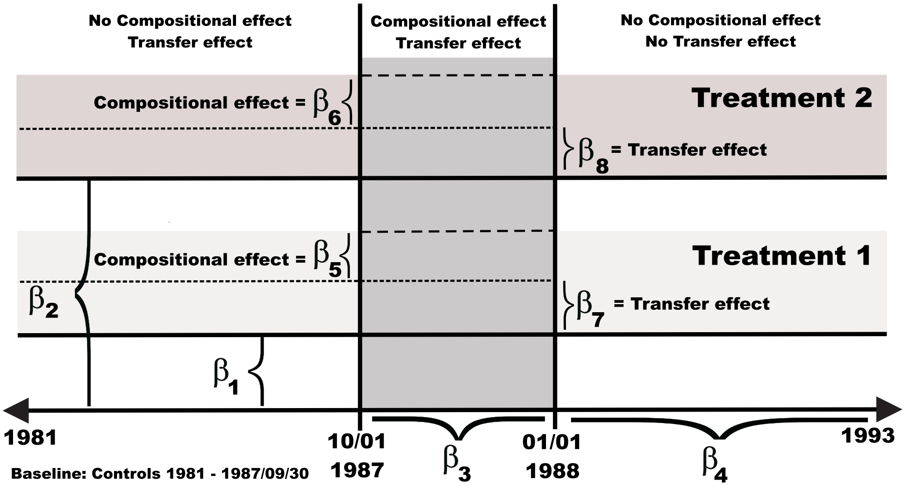

Fig. 4 Research design. This graph depicts the research design. We have two treatment groups and one comparison group: treatment group 1 (β_1; only one spouse eligible), treatment group 2 (β_2; both spouses are eligible), and comparison group (base group; no spouse is eligible). We have three periods: pretreatment period (1981 through September 1987, no compositional effect, transfer effect from the existence of marriage subsidy), treatment period (β_3; October through December 1987, compositional effect from marginal marriages and transfer effect), and post-treatment period (β_4; 1988 through 1993, no compositional effect, no transfer effect). The compositional effects for treatments 1 and 2 are given by β_5 and β_6, respectively. The transfer effects for treatments 1 and 2 are given by β_7 and β_8, respectively

evidence for compositional effects induced by the introduction of the subsidy. A detailed discussion and estimation output is provided in Online Resource 3. This finding seems quite intuitive. Until 1986, Austrians were used to ongoing marriage-promoting policies, and there was not a strong incentive to risk a bad marriage if one could also have waited for the right spouse to arrive and to cash the subsidy later. In contrast, after the announcement of the subsidy suspension, the incentives changed substantially, and one would expect compositional effects during the defined treatment period.

In each of the specifications, we control for quarter fixed effects, district fixed effects, and group-specific time trends. The latter relax to some degree the parallel trend assumption. The baseline specification also includes the wife's age, the spouses' age difference (squared), and the spouses' religious denominations at the time of marriage as covariates. With respect to religious denomination, we differentiate between the three quantitatively most important religious affiliations in Austria: Catholic (73.6 %), no religious denomination (12.0 %), and others (14.4 %) (Austrian 2001 census). This gives rise to six possible combinations, where a marriage between two Catholics will serve as the base group. Given that we are interested in the estimation of compositional effects, more control variables are not necessarily better; they may partial out some of these effects. Still, we present a further specification in which we also control for the spouses' labor market statuses and occupations (measured one quarter before marriage) and the number of joint premarital children, where the latter information is available only starting from 1984.[18] The results do not change much after additional covariates are included.

[18] Frimmel et al. (2013) showed for Austria that a lower age at marriage, different religious denominations, and the presence of premarital children are associated with a higher risk of divorce.

An equivalent set of specifications using least squares regression is used for the estimation of marital fertility behavior and marital offspring's health at birth. In the latter case, the set of covariates is adjusted somewhat.

Estimation Results

Marital Instability

Table 2 summarizes the main estimation results on marital stability using different specifications. In contrast to theoretical predictions, we find practically no evidence for a higher divorce risk of marginal marriages compared with average marriages. This finding is consistent across different specifications. In the baseline specification in column 1 of Table 2, we include all marriages. In the second and the third specification, we restrict the sample to exclude potentially selected marriages from the comparison group, which may bias the estimates of the composition (and selection) effect downward. In particular, in specification 2 of Table 2, we exclude marriages formed in 1983. In this year, the Austrian government announced the abolishment of the tax deductibility of dowry per January 1, 1984. Thus, the comparison group in 1983 may comprise couples who married to save taxes and who would not have married (at that time) without this reform (see the spike in Fig. 1). In specification 3, we further exclude marriages formed immediately after the reform (i.e., in the first half-year of 1988). Given that a sizable number of spouses have brought forward their wedding day to cash the subsidy (the early average marriages), the pool of marriages formed in early 1988 might also be selective. In the fourth and in the fifth specifications, we extend the set of sociodemographic control variables. Specification 4 of Table 2 also includes information on the spouses' labor market statuses and occupations (measured in the quarter before marriage). Specification 5 also controls for the number of premarital children.

Across specifications, we consistently find no statistically significant composition effects. The point estimates (for both groups) are quite small and insensitive to modifications of the sample and the covariates included. Even leaving statistical significance aside, the point estimates of the composition effects provide little to no evidence for a different marital instability of marginal marriages. In the case of T^1, the point estimates even suggest a lower divorce likelihood for marginal marriages. For T^2, we find positive composition effects between 2.8 % and 3.6 %. However, the lowest p value (see T^2 in specification 2) is .17—far above conventional levels of statistical significance.

Given that during the treatment period *TP*, the groups of T^1 and T^2 marriages made up approximately one-half of marginal marriages—and one-half of (early) average marriages—we can multiply the estimates of the compositional effects by 2 to arrive at an appropriate estimate of the selection effect. Assuming point estimates that are twice as large as the ones we have estimated, only 1 of the 10 estimates in Table 2 would reach significance levels close to conventional levels (8.6 in specification 2).

In sum, a conservative interpretation of the estimation of the compositional effects is that there is little evidence that marginal marriages are a selected group in terms of

Table 2 Marital instability: Hazard ratios, with p values (based on heteroskedasticity-robust standard errors) in parentheses[a]

	(1) 1981–1993		(2) Without 1983		(3) Without 1983 and First Half of 1988		(4) + Labor		(5) + Children	
Compositional Effects										
$\beta_5 : T_1 \cdot TP$	0.987	(0.773)	0.990	(0.829)	0.985	(0.728)	0.969	(0.471)	0.960	(0.449)
$\beta_6 : T_2 \cdot TP$	1.035	(0.211)	1.036	(0.172)	1.032	(0.208)	1.028	(0.337)	1.035	(0.341)
Transfer Effects (inverse)										
$\beta_7 : T_1 \cdot postTP$	1.038	(0.255)	1.037	(0.252)	1.015	(0.614)	1.027	(0.399)	1.064	(0.215)
$\beta_8 : T_2 \cdot postTP$	1.025	(0.126)	1.024	(0.126)	1.014	(0.361)	1.022	(0.222)	1.054†	(0.079)
$\beta_1 : T_1$	0.676**	(0.000)	0.657**	(0.000)	0.626**	(0.000)	0.649**	(0.000)	0.784†	(0.075)
$\beta_2 : T_2$	0.382**	(0.000)	0.365**	(0.000)	0.351**	(0.000)	0.410**	(0.000)	0.514**	(0.000)
$\beta_3 : TP$	0.996	(0.945)	0.996	(0.945)	1.002	(0.972)	0.984	(0.802)	0.987	(0.844)
$\beta_4 : postTP$	0.948	(0.123)	0.948	(0.122)	0.970	(0.402)	0.980	(0.584)	0.915*	(0.035)
Quarter Fixed Effects	Yes		Yes		Yes		Yes		Yes	
District Fixed Effects	Yes		Yes		Yes		Yes		Yes	
Group-Specific Time Trends	Yes		Yes		Yes		Yes		Yes	
Age and Age Difference[b]	Yes		Yes		Yes		Yes		Yes	
Religious Denomination[c]	Yes		Yes		Yes		Yes		Yes	
Labor Market Status[d]	No		No		No		Yes		Yes	
Premarital Children[e]	No		No		No		No		Yes	
Number of Observations	550,295		498,654		486,876		486,876		400,381	

[a] The estimation method is a Cox proportional hazards model. Interaction terms are recomputed according to Ai and Norton (2003).

[b] The estimation controls for the wife's age and the spouses age difference (squared).

[c] The estimation includes binary variables capturing the following combinations of spouses' religious denominations: Catholic and other denomination, Catholic and no denomination, other denomination and no denomination, both other denominations, and both without denomination.

[d] The estimation includes binary variables capturing the following labor market status of wife and husband (measured one quarter before marriage): employed as a blue-collar worker, employed as a white-collar worker, other employment (e.g., self-employed), unemployed, and out of the labor force.

[e] The estimation includes a cardinal variable capturing the number of joint premarital children.

†$p < .10$; *$p < .05$; **$p < .01$

marital stability. Somewhat surprising, the results suggest that marriage-promoting policies indeed have the potential to create stable marriages.

Less surprising is the scant evidence for transfer effects. Only in the case of specification 5 we do find a statistically significant transfer effect for T^2 marriages. The point estimate suggests that their divorce likelihood decreased by 5.4 % because of the marriage subsidy. The effect is, however, not statistically significant at the 5 % level.

The remaining control variables from the DiD specification show that the treated couples—individuals in their first marriages—have significantly lower hazard rates. The lowest divorce risk is observed for spouses who are both in their first marriage (see β_2). More importantly, the controls for the treatment period (β_3) and the post-treatment period (β_4) are always statistically indistinguishable from 1, suggesting that there are no other time trends that might interfere with the compositional effects.

Marital Fertility

Table 3 summarizes DiD estimation results on fertility behavior. We consider the number of marital children born by 2007 as an outcome variable.[19] Although not all women in the sample reached the end of their reproductive life by 2007, the estimation results will most likely resemble the effect on completed fertility given that the vast majority of women were born before 1968.[20] We list results for only the most extensive specifications—resembling specifications 4 and 5 from Table 2—because the results do not change much across other specifications.

In contrast to results on marital instability, we find statistically significant compositional effects with respect to fertility behavior. Specification 1 suggests that T^2 marriages formed during the treatment period have fewer marital offspring (–0.15 children). For T^1 marriages, we observe a comparably smaller effect of –0.06 children. Thus, the selection effects for T^2 and T^1 marriages are approximately –0.30 and –0.12 children. This is equivalent to 25 % and 10 % fewer marital offspring for T^2 and T^1 marriages, respectively.

These effects might be partly due to the fact that marginal marriages have more premarital children. Specification 2 introduces the number of premarital children as an additional control variable. Indeed, the statistical significance of the compositional effect for T^1 marriages vanishes, and the point estimate is essentially zero. This suggests that marginal marriages from T^1 have the same number of overall children (as average marriages); but in marginal marriages, some of them are born out of wedlock. For T^2 marriages, the estimated effect remains statistically significant but shrinks somewhat in size. This results in a reduced selection effect of –0.21 children, or 17 % fewer marital offspring. In other words, marginal marriages of T^2 are statistically significantly different compared with average marriages in terms of overall number of children.

Again, there is only limited evidence for transfer effects. Although β_8 is statistically significant in the first specification, all transfer effects in the second specification are statistically insignificant.

Table D.1 in Online Resource 4 provides further results to explore potentially differential timing of marital fertility. It summarizes estimates of compositional effects in terms of the number of marital children by marriage duration. They reveal a diverging timing for marriages formed during the treatment period. This translates into the following estimates of selection effects. Marginal marriages from both treatment groups produce fewer marital offspring in the first two years of marriage (T^1: –0.1 children, T^2: –0.24 children). In the case of T^1 couples, we observe positive selection effects thereafter. In sum, after 15 years of marriage, marginal marriages from T^1 have the same number of marital offspring as average marriages. In contrast, for T^2 couples, we find little evidence for a catching-up process, and the difference prevails over 15 years of marriage. In particular, the difference after 2 years of marriage and 15 years of marriage is very small.[21]

[19] We use the definition of marital children from the Austrian Birth Register, where a child is coded as a marital child if the mother was married at any time during pregnancy.

[20] Thus, by 2007, approximately 80 % of the women in our sample are aged 40 or older.

[21] Looking at the extensive marital fertility margin, marginal marriages are approximately 4 % (T^1) and 6 % (T^2) more likely to have no marital offspring at all (measured in 2007).

Table 3 Marital fertility: Coefficients, with p values (based on heteroskedasticity-robust standard errors) in parentheses[a]

	(1) Without Premarital Children		(2) With Premarital Children	
Compositional Effects				
$\beta_5 : T_1 \cdot TP$	–0.062**	(0.005)	–0.007	(0.745)
$\beta_6 : T_2 \cdot TP$	–0.149**	(0.000)	–0.103**	(0.000)
Transfer Effects (inverse)				
$\beta_7 : T_1 \cdot postTP$	0.009	(0.594)	0.002	(0.915)
$\beta_8 : T_2 \cdot postTP$	0.032*	(0.023)	0.008	(0.563)
$\beta_1 : T_1$	0.083†	(0.064)	0.069	(0.118)
$\beta_2 : T_2$	0.401**	(0.000)	0.373**	(0.000)
$\beta_3 : TP$	0.015	(0.410)	–0.014	(0.433)
$\beta_4 : postTP$	–0.003	(0.808)	–0.007	(0.550)
Quarter Fixed Effects	Yes		Yes	
District Fixed Effects	Yes		Yes	
Group-Specific Time Trends	Yes		Yes	
Age and Age Difference[b]	Yes		Yes	
Religious Denomination[c]	Yes		Yes	
Labor Market Status[d]	Yes		Yes	
Premarital Children[e]	No		Yes	
Mean of Dependent Variable	1.195			
SD of Dependent Variable	1.060			

[a] Dependent variable is the number of marital children born by 2007. The estimation method is ordinary least squares. The number of observations is in each estimation equal to 401,314.

[b] The estimation controls for the wife's age and the spouses age difference (squared).

[c] The estimation includes binary variables capturing the following combinations of spouses' religious denominations: Catholic and other denomination, Catholic and no denomination, other denomination and no denomination, both other denominations, and both without denomination.

[d] The estimation includes binary variables capturing the following labor market status of wife and husband (measured one quarter before marriage): employed as a blue-collar worker, employed as a white-collar worker, other employment (e.g., self-employed), unemployed, and out of the labor force.

[e] The estimation includes a cardinal variable capturing the number of joint premarital children.

†$p < .10$; *$p < .05$; **$p < .01$

In sum, these results suggest that marginal marriages have fewer children and have them later in marriage. The former effect applies only to T^2 couples.

Children's Health at Birth

Austria has a Bismarckian (social) health insurance system with almost universal access to high-quality healthcare. Although Austria has had a no-fee mother-child healthcare examination program that covers prenatal and postnatal check-ups since 1974, infant mortality was still quite high in the 1980s. It amounted to 11 deaths of infants under the age of 1 year per 1,000 live births, which was comparable with the United States. Since then, infant mortality rates have declined but are still significantly higher compared with Scandinavian countries (see Table A.1 in Online Resource 1).

To compare the health of marital children born to marginal and average marriages, we use data provided in the Austrian Birth Register on the gestation length, birth weight, APGAR scores, and sex of the first marital child.[22] These are the most common measures of health at birth. Gestation periods are classified as premature if they are less than 37 weeks. Weight at birth is typically considered as low if less than 2,500 g. Both premature gestation length and low birth weight are related to higher likelihood of infant mortality and may also have long-lasting effects (see, e.g., Almond and Currie 2011; Behrman and Rosenzweig 2004; Black et al. 2007). The APGAR scores quickly and summarily assess after 1, 5, and 10 minutes the health of newborn babies based on five criteria (appearance, pulse, grimace, activity, and respiration) and range from 0 ("good") to 10 ("bad"). Finally, the likelihood of a male birth serves as a metric of fetal death. This indicator exploits the fact that males are more sensitive than females to negative health shocks *in utero* (Sanders and Stoecker 2011).[23]

The estimation results from a DiD estimation are summarized in Table 4. We do not find any statistically significant composition effects based on gestational length, APGAR scores, or the sex indicator.[24] However, we find significant evidence in the case of birth weight. The point estimates for both treatment groups suggest compositional effects of approximately –90g. Given potential misclassifications in the marginal marriages (as discussed earlier), we might multiply this effect by the factor 2. The resulting selection effect is equivalent to approximately –5.5 %, or approximately one-third of a sample standard deviation. The quantitative importance of this effect is moderate. However, if we use an indicator for low birth weight (equal to 1 for less than 2,500 g, and 0 otherwise) as an alternative outcome variable, we find substantially larger effects. Untabulated regressions show that newborns from a marginal marriage are at least between 3.8 (T^1) and 5.0 (T^2) percentage points more likely to have a low birth weight. The fact that the estimated effects are quantitatively more important based on the indicator variables (as compared with the birth weight) shows that the composition effects are located in the lower tail of the birth weight distribution. Put differently, among the marginal marriages, some couples have offspring with very low birth endowment. Equivalent results are obtained for more parsimonious specifications.

The remaining variables from the DiD specification are almost all statistically insignificant. Children born to couples in which one (β_1) or both spouses (β_2) had been married before are as healthy as children born to parents in their first marriage. Children born to control parents in the treatment period (β_3) and in the post-treatment period (β_4) are indistinguishable from those born in the pretreatment period. Finally, we find no evidence for transfer effects on children's health at birth. The untabulated estimated effects of the socioeconomic controls variables are very comparable to those found in other studies (e.g., Frimmel and Pruckner 2014).

[22] Marginal marriages have somewhat fewer children and have them later in life. We take the latter fact into account by including mother's age at birth as a control variable.

[23] The exact mechanism behind this culling process remains unclear. Still, researchers in different fields agree that the sex ratio is a useful proxy for early spontaneous abortions (Almond and Edlund 2007; Catalano and Bruckner 2006).

[24] The same is true for a binary indicator capturing premature birth.

Table 4 Marital offspring's health at birth: Coefficients, with p values (based on heteroskedasticity-robust standard errors) in parentheses[a]

	Gestation Length[b]		Birth Weight[c]		APGAR Score 10[d]		Male Birth	
Compositional Effects								
$\beta_5 : T_1 \cdot TP$	–0.232	(0.174)	$-97.31^{\dagger}$	(0.060)	–0.051	(0.154)	0.033	(0.511)
$\beta_6 : T_2 \cdot TP$	–0.211	(0.192)	$-90.00^{\dagger}$	(0.067)	–0.043	(0.172)	0.018	(0.706)
Transfer Effects (inverse)								
$\beta_7 : T_1 \cdot postTP$	0.031	(0.807)	–11.56	(0.758)	–0.005	(0.895)	–0.002	(0.956)
$\beta_8 : T_2 \cdot postTP$	–0.066	(0.571)	–6.43	(0.852)	–0.022	(0.500)	–0.001	(0.967)
$\beta_1 : T_1$	0.231	(0.501)	34.64	(0.728)	–0.045	(0.667)	0.010	(0.905)
$\beta_2 : T_2$	0.037	(0.907)	57.63	(0.530)	–0.090	(0.360)	0.015	(0.848)
$\beta_3 : TP$	0.196	(0.223)	$82.45^{\dagger}$	(0.092)	0.033	(0.290)	–0.018	(0.699)
$\beta_4 : postTP$	0.069	(0.548)	1.38	(0.968)	0.011	(0.741)	0.003	(0.926)
Quarter Fixed Effects	Yes		Yes		Yes		Yes	
District Fixed Effects	Yes		Yes		Yes		Yes	
Group-Specific Time Trends	Yes		Yes		Yes		Yes	
Birth Quarter Fixed Effects	Yes		Yes		Yes		Yes	
Age of Mother at Birth	Yes		Yes		Yes		Yes	
Religious Denomination[e]	Yes		Yes		Yes		Yes	
Labor Market Status[f]	Yes		Yes		Yes		Yes	
Pre-marital Children[g]	Yes		Yes		Yes		Yes	
Number of Observations	229,089		229,089		228,995		229,089	
Mean of Dependent Variable	39.684		3,255.02		9.879		0.513	
SD of Dependent Variable	1.773		516.07		0.535		—	

[a] Estimation method: ordinary least squares. Health outcomes refer to the first marital child.

[b] The gestation length is measured in weeks.

[c] The weight at birth is measured in grams.

[d] Missing information on APGAR scores for 94 observations.

[e] The estimation includes binary variables capturing the following combinations of spouses' religious denominations: Catholic and other denomination, Catholic and no denomination, other denomination and no denomination, both other denominations, and both without denomination.

[f] The estimation includes binary variables capturing the following labor market status of wife (measured at the time of birth): employed as a blue-collar worker, employed as a white-collar worker, other employment (e.g., self-employed) and not employed.

[g] The estimation includes a cardinal variable capturing the number of premarital children.

$^{\dagger}p < .10$

Robustness Checks

We ran several robustness checks to test the sensitivity of the results; these robustness checks are summarized in Online Resource 4. For instance, we excluded the group-specific time trends from all the specifications (see Tables D.2–D.4 in Online Resource 4). We also extended the sample period and used all marriage cohorts from 1974 through 2000 (see Tables D.5–D.7 in Online Resource 4). Overall, we find no significant changes in the estimated compositional and transfer effects resulting from these modifications. This applies to all outcomes under consideration.

Conclusions

We exploit a unique policy episode in Austria, where a suspension of a relatively large marriage subsidy was announced and the number of marriages was rapidly increasing just before this suspension. This allows us to locate couples who married just because of the impending subsidy suspension. We examine the selectivity of these marginal marriages—that is, couples who would not have married in the counterfactual situation without the subsidy suspension—within a DiD framework along the outcome dimensions of marital stability, fertility behavior, and marital offspring's health. The estimation of compositional effects of the treated population resulting from this announcement allows us to quantify the degree of selectivity. Contrary to expectations, we find that those who married just because of the subsidy are not different from the average marriages in terms of marital stability. However, they have somewhat fewer children and have them later in their marriage. Also, their offspring are less healthy at birth.

Thus, it seems that in this case, pro-marriage policies can work. Financial incentives significantly influence marriage behavior, and those who marry because of a marriage subsidy are not much different from an average marriage. The concern that marginal marriages are less stable—and may even generate additional children affected by parental divorce—proves to be unfounded. However, evidence also shows that simply motivating couples to marry does not improve all their family outcomes; health outcomes of children born to marginal marriages are still worse compared with those of average marriages.

These results must be interpreted in the light of the Austrian institutions and the country's specific marital landscape. Austria might be thought of as having attitudes toward marriage and divorce that are midway between the United States and, say, Scandinavia. Moreover, Austria is representative of a Central European welfare state. In countries with a less pronounced social insurance system, marginal and average marriages may be more distinct, and the generalizability of our results may be limited.

Whether it is worthwhile from a taxpayer's point of view to invest money in inducing people to get married is another issue. The existing evidence indicates that causal effects of marriage are quite mixed. In particular, instrumental variables estimates of local average treatment effects may vary substantially across different groups of compliers and, therefore, across different groups of persons induced into marriage.[25] To evaluate pro-marriage policies further, estimates of local average treatment effects precisely for the population responding to pro-marriage policies (i.e., compliers) are needed. Our results, which are based on a subsidy that induced a relatively large shift in marriage behavior, suggest that the local average treatment effects provided by such instrumental variables approaches may also be good approximations for the average treatment effects given that marginal marriages are quite comparable to average marriages.

[25] See, for instance Ichino and Winter-Ebmer (1999) for a study in which different instruments shift different populations and therefore lead to different conclusions.

The results presented here suggest that the match quality of marginal marriages is almost sufficient to warrant a stable marriage. One might expect that substantially higher subsidies would reduce the marginal reservation match quality further and result in a higher degree of negative selection. Consequently, pro-marriage policies should not incorporate incentives that are too high, after all. Furthermore, policy-makers should try not to simply subsidize marriage, but to facilitate stable marriage by, for example, subsidizing marital-specific investment.

Acknowledgments For their helpful comments, we would like to thank Raj Chetty, Jungmin Lee, Daniel S. Hamermesh, Shelly Lundberg, Enrico Moretti, Helmut Rainer, Mario Schnalzenberger, Betsey Stevenson, Andrea Weber, Josef Zweimüller, and participants of several seminars and conferences. We are also grateful for very helpful comments provided by the Editor and two anonymous referees. The usual disclaimer applies. This research was funded by the Austrian Science Fund: National Research Network S103, The Austrian Center for Labor Economics and the Analysis of the Welfare State.

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