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Wolfgang Frimmel, University of Linz
Martin Halla, University of Linz
Rudolf Winter-Ebmer, University of Linz and CEPR

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Centre for Economic Policy Research
77 Bastwick Street, London EC1V 3PZ, UK
Tel: (44 20) 7183 8801, Fax: (44 20) 7183 8820
Email: cepr@cepr.org, Website: www.cepr.org

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ABSTRACT

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

Policies to promote marriage are controversial, and it is unclear whether they are successful. To analyze such policies, it is essential to 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). In this paper, 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 this suspension led to an enormous marriage boom (plus 350 percent) among eligible couples that allows us to identify marginal marriages. Applying a difference-in-differences approach, we show that marginal marriages are surprisingly as stable as average marriages, but have fewer children and have them later in marriage. Notably, the children born to marginal marriages are similar in terms of health at birth.

JEL Classification: H24, H53, I38 and J12
Keywords: divorce, fertility, marital instability, marriage subsidies and marriage-promoting policies

Wolfgang Frimmel
Department of Economics
University of Linz
Altenberger Strasse 69
4040 Linz-Auhof
AUSTRIA

Email: wolfgang.frimmel@jku.at

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Martin Halla
Department of Economics
University of Linz
Altenberger Strasse 69
4040 Linz-Auhof
AUSTRIA

Email: martin.halla@jku.at

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Rudolf Winter-Ebmer
Department of Economics
University of Linz
Altenbergerstraße 69
A-4040 Linz-Auhof
AUSTRIA

Email: rudolf.winterebmer@jku.at

For further Discussion Papers by this author see:
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1 Introduction

Policies to promote marriage are controversial (McLanahan, 2007; Amato, 2007a,b; Furstenberg, 2007a,b; Struening, 2007). While there is extensive empirical literature (Waite and Gallagher, 2000) documenting a strong correlation between being married and better family outcomes, scholars do not agree whether this is a causal relationship. A host of 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 they are indefensible since 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).

In this paper, we solve neither the statistical nor the political debate, but we do add yet another important (and so 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 our 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 a state intervention. In contrast, a marginal marriage is given by spouses who would not have married without the state intervention.[2] It is possible

[1] In theory, legal marriage may increase well-being (as compared to cohabitation) if marriage acts as a commitment device that fosters co-operation 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 as compared to ending cohabitation.

[2] In the terminology of the literature on *local treatment effects*, one could term marginal marriages

that marriage improves the well-being of average marriages but is not (as) beneficial to marginal couples. Loosely speaking, 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, the most 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 a lower match quality (as compared to average marriages), to be 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 how selective marginal marriages are in terms of marital stability and fertility behavior, is of particular interest to researchers and policy-makers alike. Answering this question involves the econometric challenge identifying average and marginal marriages and estimating their differential behavior. To our knowledge, no work has yet attempted to provide such an empirical analysis.

To answer this question, we propose to use the suspension of a straightforward cash-on-hand marriage-promoting policy in Austria. Since the early seventies, two Austrian citizens, both marrying for the first time, received approximately EUR 4,250 or USD 5,680 (values are adjusted for inflation). At the end of August 1987, the suspension of this marriage subsidy was announced to be effective as of January 1, 1988. This led to an enormous marriage boom in the three months from October to December 1987 (see Figure 1). Compared to the period from October to December 1986 with 7,844 marriages,

compliers and average marriages *always-takers*.

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

in 1987 (within the same period) 35,847 couples decided to marry. This is an increase of more than 350 percent. Clearly, part of the marriage boom was simply due to timing. However, using individual-level data on the entirety of Austrian marriages, we show that approximately half of the couples who married between October and December 1987 were motivated by the cash transfer and thus constitute marginal marriages.

[Insert Figure 1 around here]

For our estimation analysis, we exploit the eligibility criteria to set up a difference-in-differences framework. This allows us to estimate the differential divorce and fertility behavior of marginal couples. Quite surprisingly, we find hardly any evidence of a lower marital stability of marginal marriages. We do find, however, that marginal marriages have fewer children and have them later in marriage. Notably, the children born to marginal marriages are similar in terms of health at birth.

Our findings contribute to different strands of economic literature and hold important implications for public policy-makers. First, there is a strand of literature that asks the fundamental question of whether the state can effectively encourage people to marry or to stay married. While 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., Whittington and Alm, 1997; Alm, Dickert-Conlin and Whittington, 1999). The empirical evidence on behavioral effects created by transfer programs is less consistent. However, Moffitt (1998) concludes based on a comprehensive survey of the literature from the last three decades that transfer programs do affect marital decisions as well. As argued by Blank (2002), it is typically difficult to identify effects of tax and welfare reforms on family formation. 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 as compared to labor market behavior. In contrast, the reform studied in our paper was straightforward and had an obvious and enormous effect on marriage behavior.

Second, our paper relates to the literature interested in the effects of marriage. Only a small number of studies offer a credible research design to identify a causal effect of marriage. Almost all of these papers exploit exogenous variation in marital status due to some kind of policy intervention. Two papers use a marriage boom in Sweden — created by the Swedish widow's pension reform in 1989 — to estimate the corresponding treatment effect of marriage on children's school outcomes (Björklund, Ginther and Sundström, 2007) and on spouses' labor market outcomes (Ginther and Sundström, 2010). The first paper does not find any effect of marriage on children's school performance. The second finds a small marriage premium for men and a small marriage penalty for women, where both effects seem to be the result of increased specialization of married couples. Most recently, Fisher (2010) uses differences in U.S. marriage tax penalties or subsidies to instrument for marital status. She finds 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 counterpart. However, there is some evidence that complying men with low education benefit from marriage, while complying women with higher education report lower health if married.[4]

Finally, the results should be of considerable interest to policy-makers. In most OECD member countries, different marriage-promoting policies are in place, and we are not aware of any systematic evaluation of these.[5] One of the most prominent marriage-promoting policies is the *Temporary Assistance for Needy Families* (TANF).[6] This U.S. federal assistance program provides states with block grants that can be used for a wide range of activities to end welfare dependency by encouraging work, marriage, and two-parent families.[7] In the fiscal year 2009, the TANF block grant amounted to $16.5 billion. Examples of other U.S. policies to increase marriage rates and stabilize existing marriages

[4]Other papers (Finlay and Neumark, 2009; Dahl, 2010) concentrate on sub-populations (namely prison inmates and teenagers) that are typically not the target of a pro-marriage policy.

[5]For a comprehensive overview of U.S. policies to promote marriage, see Gardiner, Fishman, Nikolov, Glosser and Laud (2002); Brotherson and Duncan (2004).

[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]In particular, the four purposes set out in federal law are (i) to provide assistance to needy families so that children may be cared for in their own homes or in the homes of relatives, (ii) to end the dependence of needy parents on government benefits by promoting job preparation, work, and marriage, (iii) to prevent and reduce the incidence of out-of-wedlock pregnancies and establish annual numerical goals for preventing and reducing the incidence of these pregnancies, and (iv) to encourage the formation and maintenance of two-parent families.

are the introduction of covenant marriages (Brinig, 1999) and the removal of marriage penalties in tax codes (Alm, Dickert-Conlin and Whittington, 1999), pension systems (Baker, Hanna and Kantarevic, 2004) and Medicaid programs (Yelowitz, 1998). Similar policies can be observed in many other OECD member countries.

The remainder of this paper is structured as follows. The next section outlines the development of marriage-promoting polices in Austria and describes the circumstances of the (announcement of the) suspension of the marriage subsidy in 1987. In Section 3, we present the data, discuss how we identify marginal marriages, and present our difference-in-differences estimation strategy. Section 4 provides the estimation results on differential divorce and fertility behavior of marginal marriages, as well as results on their marital offspring's health. The final section concludes the paper with a discussion of potential policy implications.

2 Institutional setting

In Austria, newlywed couples had been traditionally subsidized via tax deductions. In particular, since 1967 a newlywed couple could deduct home furnishings and articles of daily use up to 70,000 Austrian Schillings within the first five years after the establishment of their new household.[8] Starting from 1972, the Austrian government switched to a more straightforward marriage-promoting policy and provided instead cash on hand, no strings attached. Every person with unrestricted tax liability in Austria who had never been married before received 7,500 Austrian Schilling upon marriage.[9] This corresponds to approximately EUR 2,125 or USD 2,840 in 2011. Thus, two Austrian citizens, both marrying for the first time, received a total of EUR 4,250. While the old tax deductability 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. The cash on hand marriage subsidy had been a heavily discussed election pledge of the *Social Democratic Party of Austria* in its 1971 election campaign, which they adhered to after gaining

[8] *Bundesgesetzblatt* (BGBl.) 161/196.

[9] BGBl. 460/1971. For foreigners it is not always clear, whether they are tax liable in Austria in such a sense; therefore, we eliminated foreign citizens from our analysis completely.

the majority in the Austrian Parliament in 1971. Over time, the regulations of this marriage subsidy did not change, and the transfer had not been adjusted for inflation. Almost sixteen 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 compensatory schemes.[10]

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 Figure 1). Compared to the same time period in 1986 (with 7,844 marriages), we observe an increase of more than 350 percent to 35,847 marriages in 1987. Clearly, part of the marriage boom might be simply due to timing; however, even based on theoretical grounds, we expect an increase in marriage rates 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. First, search costs increase sharply due to the time constraint introduced by the announcement of the suspension; second, the value of a continued search (for a better match) is reduced as there are no subsidies after the suspension. Thus, the observed 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 given by those matches that only became acceptable due to the reduction in the reservation match quality caused by the announcement of the suspension. Consequently, a marginal marriage should be of lower match quality than average marriages, whose match quality would be sufficient even without state intervention. In our empirical analysis, we are precisely interested in 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 given by what we term the

[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 in the press, nor was there a parliamentary debate before August 1987.

transfer effect. The transfer effect describes the behavioral response due to additional resources on family outcomes (divorce likelihood and fertility) in the absence of selection: the true causal effect of the reform.[11] Here, one has to keep in mind 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 our empirical analysis below is on the selection effects; nevertheless, our estimation strategy also enables us to identify any transfer effects.

3 Estimation strategy and data

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 to 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 since a certain level of marital stability and marital offspring is a necessary condition for pro-marriage policies to succeed.

In our empirical analysis, a marginal marriage is defined as a couple who has married because of the *announcement* of the suspension of the marriage subsidy. For two reasons, we focus on the suspension of the subsidy rather than on its introduction. First, prior to 1972, some financial incentives to marry already existed due to the aforementioned tax deduction. Second, the marriage subsidy had been introduced following a heavy discussion in the 1971 election campaign, which probably resulted in (potentially heterogenous) anticipation effects. In contrast, the suspension by January 1, 1988 had been implemented without any compensatory measures; it had been announced abruptly by the Minister of Finance (without any prior discussions) at the end of August 1987. The suspension thus

[11] The transfer effect can be highlighted by the following thought experiment. Imagine a situation where the existence of a marriage subsidy is not publicly announced, but marrying couples (or a sub-group of them) still receive a subsidy upon marriage. Here, the transfer effect is given by the difference in the counterfactual outcomes (with and without subsidy).

provides a clear break.

3.1 Data

For our empirical analysis, we combine information from different administrative data sources. Most importantly, we use data from the *Austrian Marriage Register*. This covers the entirety of marriages and includes the date of marriage, the spouses' marital histories, their place of residence, their ages at marriage, their religious denominations and their citizenships. Since 1984, information on the spouses' countries of birth and on the number, age and sex of any premarital children is also recorded. For further specifications with an enlarged set of covariates, we extend our 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*. Our base sample consists of all 550,294 marriages that took place between 1981 and 1993; thus, we include approximately six years of data before and after the reform. From these marriages, 150,767 divorced until the end of 2007. To obtain information on mortality and out-migration, we matched information from the *Austrian Death Register* and the ASSD.[12] This results in 36,893 right-censored observations due to death and 5,484 due to out-migration. Finally, for our 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.[13] This includes the entity of births in Austria with individual-level information on socio-economic characteristics and birth outcomes, such as gestation length, birth weight, and Apgar scores. Approximately 68 percent of the 401,314 marriages in this sample had marital offspring by 2007.

[12] We presume that if a person is still alive but has no records in the ASSD anymore that s/he left Austria.

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

3.2 Locating marginal marriages

To estimate the selection effect, we need to identify marginal and average marriages. While this is impossible at an individual level, our 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 different groups of couples: a control group, comprising couples where no spouse is eligible; a treatment group 1 (T^1), comprising all couples where one spouse is eligible; and a treatment group 2 (T^2), comprising couples where both spouses are eligible. That means, spouses from T^2 couples — where both partners have never been married before — faced the highest incentive to marry; their marriage had been subsidized in sum with $15,000$ Austrian schillings. T^1 couples comprise one spouse who had been married before; they received only $7,500$ Austrian schillings. The control group couples consist of spouses who had both been previously married; they were 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), we can see a fairly uniform seasonal pattern for each group, with a peak in May. For the control group, the patterns overlap in all three years. However, for T^1 and T^2 marriages, we observe in 1987 a clear divergence of the normal seasonal pattern starting in October. 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, whereas in September there is no artificial increase. It seems that couples needed at least one month (September) to plan their weddings. In 1988, we observe somewhat smaller numbers of T^1 and T^2 marriages in the first quarter of the year, which is most likely due to some couples who married in advance of the suspension of the transfer.

[Insert Figures 2 and 3 around here]

Figure 3 shows the annual number of marriages of T^2 couples from 1981 through 2007. It seems that the long-run trend of this series — .that is, the trend that would have been observed without the suspension of the marriages subsidy — can be approximated well by a linear interpolation between 1986 and 1990.[14] This is illustrated by the dashed line. 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: (i) couples who had planned to marry (in the near future) and decided to marry earlier to cash the subsidy and (ii) couples who had no plans to marry, but married just to receive the cash. We refer to the former group as *early average marriages*, and the latter group constitutes the marginal marriages in our 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 in the period between 1988 and 1989; these two shortfalls are equal to 8,621 and 2,676 (see the vertical red bars).[15] 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. Since, by definition, these marginal marriages can only be formed after the announcement of the suspension (and before January 1, 1988) we can relate this number 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 the County Clerk's office or at City Hall. Figure 2 indicates that the marriage boom began in October, suggesting that approximately one month of wedding planning was necessary. If we relate 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, we find that approximately 51 percent of these were marginal marriages, 36 percent were early average marriages, and the remaining 13 percent were average marriages. If we apply an equivalent procedure to T^1 marriages, we find a comparably lower share of marginal marriages of 44 percent (see the upper panel of Table 1).

[Insert Table 1 around here]

Table 1 compares the average characteristics of spouses from the two treatment groups

[14] This assumption is not crucial for our estimation analysis below.

[15] This is equivalent to assuming that couples did not advance their planned weddings more than 26 months (i. e. from December 1989 to October 1987).

and the control group (who married between October and December) for 1986, 1987, and 1988. This comparison highlights several things. First we can see that there are baseline differences 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 control group marriages), the older the spouses are, the higher is their age difference, the less likely they are both Catholic, and the lower is their number of premarital children. Second, as expected, there is little variation in the composition of the control group over time. The only exception is given by 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 half of the T^1 and T^2 marriage in 1987 were marginal marriages, this comparison should show observable differences between average and marginal marriages. However, quite surprisingly, these numbers suggest that average and marginal marriages are similar along measurable characteristics documented in the data. The only notable difference is the higher incidence of premarital children among T^1 marriages.

3.3 Difference-in-differences estimation strategy

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

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 non-parametric baseline hazard $h_0(t)$ that is augmented due to 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.[16] To estimate

[16] All our 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 $\mathbf{X}$, $\frac{h(t|\mathbf{X}^*)}{h(t|\mathbf{X})}$. Figure 4 plots the hazard function by group for marriages formed between October and December in 1986, 1987, and 1988. For all groups (and years) we can see that given a marriage that has survived until its third year, the divorce hazard is actually decreasing. In the case of the control and the treatment group 1, there is no statistically significant

the selection and the transfer effect, we exploit the control group of non-eligible couples. Consequently, we implement a difference-in-differences (DiD) estimation strategy, where the treatment is given by the announcement of the suspension of the marriage-subsidy. Our estimation strategy deviates in some aspects from the conventional DiD framework and specifics $\mathbf{X}\beta$ as follows:

$$\begin{aligned} \mathbf{X}\beta = \beta_0 + \beta_1 T^1 + \beta_2 T^2 + \beta_3 TP + \beta_4 postTP + \beta_5 T^1 * TP + \beta_6 T^2 * TP \\ + \beta_7 T^1 * postTP + \beta_8 T^2 * postTP + \gamma * X_i + u_i. \end{aligned} \quad (2)$$

First, we have more than one treatment group. As introduced above, we distinguish between spouses from the two treatment groups (T^1 and T^2) and the control group (C). The specification therefore allows for a different baseline hazard of T^1 and T^2 marriages (i. e., β_1 and β_2 compare to control group marriages). Second, we do not only distinguish between before- and after-treatment periods but we also define three different time periods. We have a pre-treatment period (captured by $preTP$) starting with our sample in 1981 and running through September 30, 1987. The treatment period (TP) is given by the period between October 1, 1987 through December 31, 1987. Thereafter, the post-treatment period ($postTP$) starts. Consequently, we allow marriages formed in these three different time periods to have a different divorce hazard (see β_3 and β_4).[17]

We also deviate somewhat from the conventional DiD framework with respect to the identifying assumptions. Typically, one assumes that the trends in the outcome variables would have been the same for the treatment and the control 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 will relax this assumption to some degree by allowing for group-specific linear trends (see below). In contrast, we do not rule out

difference between the hazard functions of 1986, 1987, and 1988; similar results hold for treatment group 2 with the exception of the very first periods.

[17] Another way to think about this specification is to refer not only to the announcement of the suspension as a treatment, but also to the actual abolishment as another treatment, and to denote the post-treatment period as a treatment period 2.

compositional changes in the treatment groups during the treatment period. We rather aim to quantify these effects since they allow us to infer on the selection effects. In other words, we expect the composition of treated couples to change during the treatment period since a large share of these are marginal marriages. We assume, however, the control group to be free of compositional effects over time.

The coefficients on the interaction terms between the two treatment group indicators and the treatment period dummy (β_5 and β_6) provide the estimates for the compositional changes of T^1 and T^2 marriages. Given that we know the approximate shares of marginal marriages and (early) average marriages during the treatment period TP, we can calculate the respective selection effects. As marginal marriages contain approximately half of the population in this period, we should multiply β_5 and β_6 by two to arrive at 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. Since β_7 and β_8 are based on a comparison of the post-treatment period and the treatment 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, Figure 5 provides a graphical presentation of the setup.

[Insert Figures 4 and 5 around here]

In each of our 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. Our 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 percent), no religious denomination (12.0 percent), and others (14.4 percent) (*Austrian Census* from 2001). 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 for which we also control for the spouses labor market statuses and occupations (measured one quarter before marriage) and the number of joint pre-marital children, where the latter information is only available starting from 1984.[18] The results do not change much after including further covariates.[19]

An equivalent set of specifications, but 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 (see below).

4 Estimation results

At first, we present our estimation results on marital instability. Section 4.2 provides our estimates on differential fertility behavior, and Section 4.3 reports results on marital offspring's health at birth.

4.1 Marital instability

Table 2 summarizes our 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 to average marriages. This finding is very consistent across different specifications. In the baseline specification in column (I), we include all marriages. In the second and the third specification, we restrict our sample, to exclude potentially selected marriages from our control group, which may bias our estimates of the composition (and selection) effect downward. In particular, in specification (II) we exclude marriages formed in 1983. Marriages formed in this year may include marginal marriages with respect to a different policy intervention. In this year the Austrian government announced the abolishment of the tax deductibility of dowry per January 1, 1984. Thus, our control group marriages in 1983 may comprise couples

[18] Frimmel, Halla and Winter-Ebmer (2009) show 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.

[19] Clearly, we do not want to control for any post-marriage events. It can be argued that all other factors that might also have an important impact on divorce risk — such as the number of post-marital children, the labour supply of either partner and marital satisfaction — are endogenous with respect to the viability of the marriage, and therefore all coefficient estimates might be biased.

who married to save taxes and who would not have married (at that time) without this reform. In specification (III) 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 specification, we extend the set of socio-demographic control variables. Specification (IV) also includes information on the spouses' labor market statuses and occupations (measured in the quarter before marriage). Finally, Specification (V) also controls for the number of pre-marital children.

[Insert Table 2 around here]

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 higher 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 percent. However, the lowest p-value (see T^2 in specification II) is 0.17 and, therefore, far above conventional levels of statistical significance.

Given that during the treatment period TP the groups of T^1 and T^2 marriages consisted approximately half of marginal marriages — and half of (early) average marriages — we can multiply our estimates of the compositional effects by two 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 one out of our ten estimates in Table 2 would reach significance levels close to conventional levels (8.6 in specification II).

To sum up, a conservative interpretation of the estimation of the compositional effects is that there is only little evidence that marginal marriages are a selected group in terms of marital stability. This leaves us with the somewhat surprising result that marriage-promoting policies indeed have the potential to create stable marriages.

Less surprisingly, there is also little evidence for transfer effects. Only in the case of specification (V) we do find a statistically significant transfer effect for T^2 marriages. The point estimate suggests that their divorce likelihood decreased by 5.4 percent due to the marriage subsidy. The effect is, however, not statistically significant at the five percent level.

The remaining control variables from our DiD specification show that our treated couples — basically 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), which is well known from the literature. More importantly, our controls for the treatment period (β_3) and the post-treatment period (β_4) are always statistically indistinguishable from one showing that there are no other time trends that might interfere with our compositional effects.

4.2 Marital fertility

In this section, we report estimation results on fertility behavior. Table 3 summarizes DiD estimation results for which we consider the number of marital children born by 2007 as an outcome variable.[20] While not all women in our sample have reached the end of their reproductive life by 2007, our estimation results will most likely resemble the effect on completed fertility since the vast majority of women are born before 1968.[21] We only list results for our most extensive specifications — resembling Specifications (IV) and (V) from Table 2 — since the results do not change much across other specifications.

[Insert Table 3 and Figure 6 around here]

In contrast to the results on marital instability, we find statistically significant compositional effects with respect to fertility behavior. Specification (I) suggests that T^2 marriages formed during the treatment period have less marital offspring (minus 0.15 children). For T^1 marriages, we observe a comparable smaller effect of minus 0.06 children. Thus, the selection effects for T^2 and T^1 marriages are approximately minus 0.30 and minus 0.12

[20] 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.

[21] Thus, by 2007 approximately 80 percent of the women in our sample are 40 years of age or older.

children. This is equivalent to 25 and 10 percent fewer marital offspring for T^2 and T^1 marriages, respectively.

Part of these effects, however, might be due to the fact that marginal marriages tend to have more pre-marital children. Specification (II) introduces the number of pre-marital 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 marginal marriages are more likely to have some of them born out of wedlock. In the case of T^2 marriages, the estimated effect stays statistically significant, but shrinks somewhat in size. This results in a reduced selection effect of minus 0.21 children or 17 percent fewer marital offspring. In other words, marginal marriages of T^2 are statistically significantly different compared to average marriages in terms of their overall number of children.

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

Figure 6 provides further results to explore potentially differential timing of marital fertility. The bars summarize estimates of compositional effects in terms of the number of marital children by marriage duration, and they reveal a diverging timing for marriages formed during the treatment period. This translates into the following estimates of selection effects. For marginal marriages from both treatment groups, we observe statistically significant fewer marital offspring in the first two years of marriage (T^1: minus 0.1 children, T^2: minus 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, in the case of 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 two years of marriage and fifteen years of marriage is very small — which can be seen by comparing the bar on the far left and the one on the far right.[22]

In sum, these results suggest that marginal marriages (of T^2) have fewer children and have them later in marriage (this applies to T^1 and T^2 couples).

4.3 Children's health at birth

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.[23] These are the most common measures of health at birth. Gestation periods are classified as premature if they are below 37 weeks. Weight at birth is typically considered as low if it is below 2500 grams, and very low below 1500 grams. Both a premature gestation length and a low birth weight are related to higher likelihood of infant mortality, but may also have long lasting effects on health, education, and labor market outcomes later in life (see, for instance, Behrman and Rosenzweig, 2004; Black, Devereux and Salvanes, 2007; Almond and Currie, 2011). The Apgar scores assess after one, five, and ten minutes quickly and summarily the health of newborn babies based on five criteria (appearance, pulse, grimace, activity, and respiration) and range from zero ("good") to ten ("bad"). Finally, the sex-ratio (or 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).[24]

[Insert Table 4 around here]

The estimation results from a DiD estimation are summarized in Table 4. With one exception we do not find any statistically significant composition effects; the same result is obtained for more parsimonious specifications. Only in the case of weight at birth we do find statistically significant negative composition effects. The point estimates for both treatment groups suggest that a newborn from a marginal marriage weighs approximately

[22]In a further estimation, we examined the extensive marital fertility margin. We find that marginal marriages are approximately four (T^1) and six (T^2) percent more likely to have no marital offspring at all (measured in the year 2007).

[23]It has to be noted that 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.

[24]See also Almond and Edlund (2007).

ninety gram less. This is equivalent to approximately minus 2.8 percent or approximately one sixth of a sample standard deviation. To get a sense of these magnitudes, it useful to consider the associations between birth weight and later outcomes. Black *et al.* (2007) show that a 2.5 percent increase in birth weight leads to approximately one sixth of a centimeter increase in height, a 0.3 percent increase in full-time earnings, and a 0.4 percent increase in the birth weight of their children. This suggests, that selection effects — even if we assume them twice as large — are quantitatively of little importance. The finding that children born to marginal marriages are similar in terms on health at birth is also supported by the lack of statistically significant composition effects among the other outcomes, such as gestation length, the Apgar scores after 10 minutes, and the likelihood of a male birth.

The remaining variables from the DiD specification are all statistically insignificant. Children born to parents where one spouse (see β_1) or two spouses (see β_2) had been married before are as healthy as children born to parents in their first marriage. Further, children born to control parents in the treatment period (see β_3) and in the post-treatment period (see β_4) are indistinguishable from those control children born in the pre-treatment period. Finally, we do not find any evidence for transfer effects on children's health at birth. The (untabulated) estimated effects of the socio-economic controls variables are very comparable to those found in other papers (e. g., Frimmel and Pruckner, 2011).

4.4 Robustness checks

We ran several robustness checks to test the sensitivity of our results. For instance, we excluded the group-specific time trends from all our specifications. Or, we extended our sample period and used all marriage cohorts from 1974 through 2000. Overall, we do not find any significant changes in the estimated compositional and transfer effects due to theses modifications. This applies to all outcomes under consideration.

5 Conclusions and policy implications

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 by 350 percent just before this suspension. This allows us to identify couples who married just because of the suspension. We examine the selectivity of these marginal marriages — couples who would have not married in the counterfactual situation without the suspension — within a difference-in-differences framework along the outcome dimensions of marital stability, fertility behavior, and marital offspring's health. In particular, the estimation of compositional effects of the treated population due to the announcement of the suspension allows us to quantify the degree of selectivity. Contrary to expectations, we find that those who married just because of the subsidy are not much different from the crowd of regular marriages: their unions are practically as stable as an average marriages, but they have somewhat fewer children and have them later in their marriage. Moreover, the children born into these marriages are also similar in terms of health at birth.

Thus it seems that pro-marriage policies can work. Financial incentives significantly influence marriage behavior, and those who marry because of the 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. Whether it is worthwhile — from a taxpayer's point of view — to invest money into inducing people to get married is another issue. The existing evidence indicates that causal effects of marriage are quite mixed. In particular, such 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 further evaluate pro-marriage policies, estimates of local average treatment effects precisely for the population responding to pro-marriage policies (i. e., compliers) are needed. We hope further evidence from such instrumental variable approaches will be available soon. Our results — which are based on a subsidy that induced a relatively large

[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.

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 since marginal marriages (compliers) are similar to average marriages (always-takers).

Why are marginal marriages as stable as average marriages? It seems that the match quality of marginal marriages is almost sufficient to warrant a regular marriage. One might expect then that a substantially higher subsidy would reduce the marginal reservation match quality further and result in a higher degree of negative selection. Consequently, pro-marriage policies should not incorporate too high incentives, after all. Furthermore, policy makers could try not to simply subsidize marriage, but to facilitate stable marriage by, for instance, subsidizing marital-specific investment.

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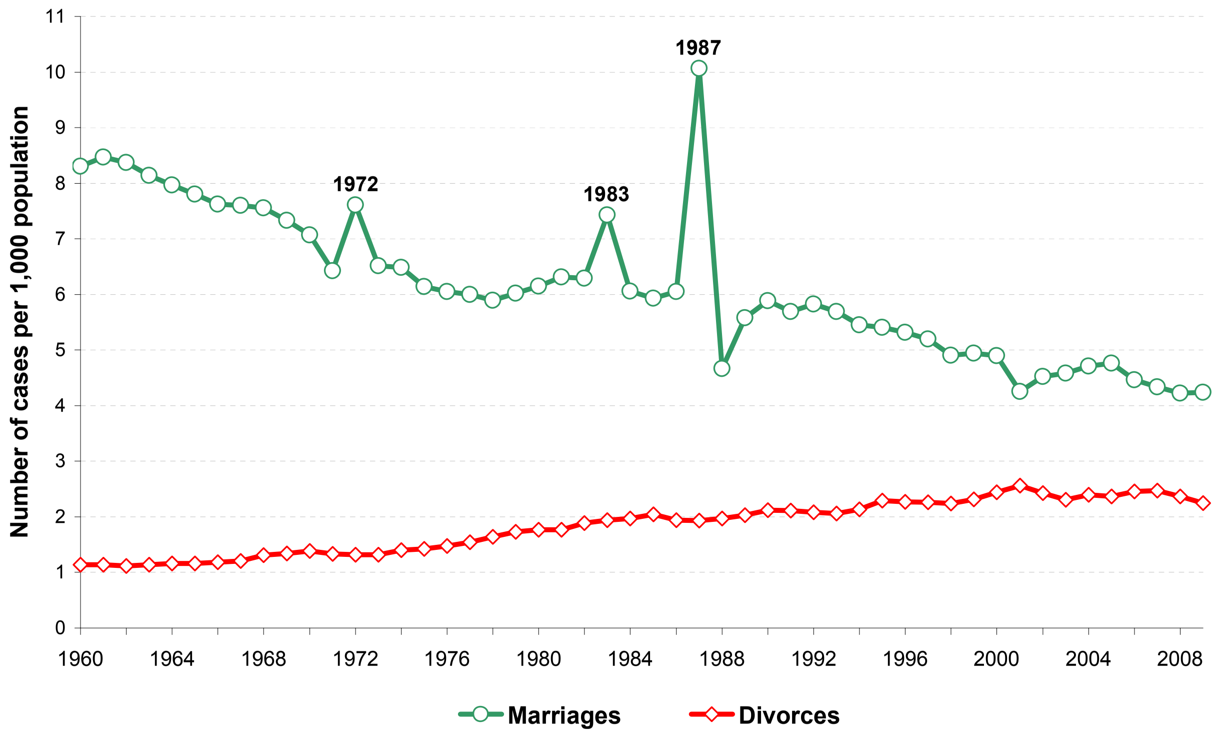
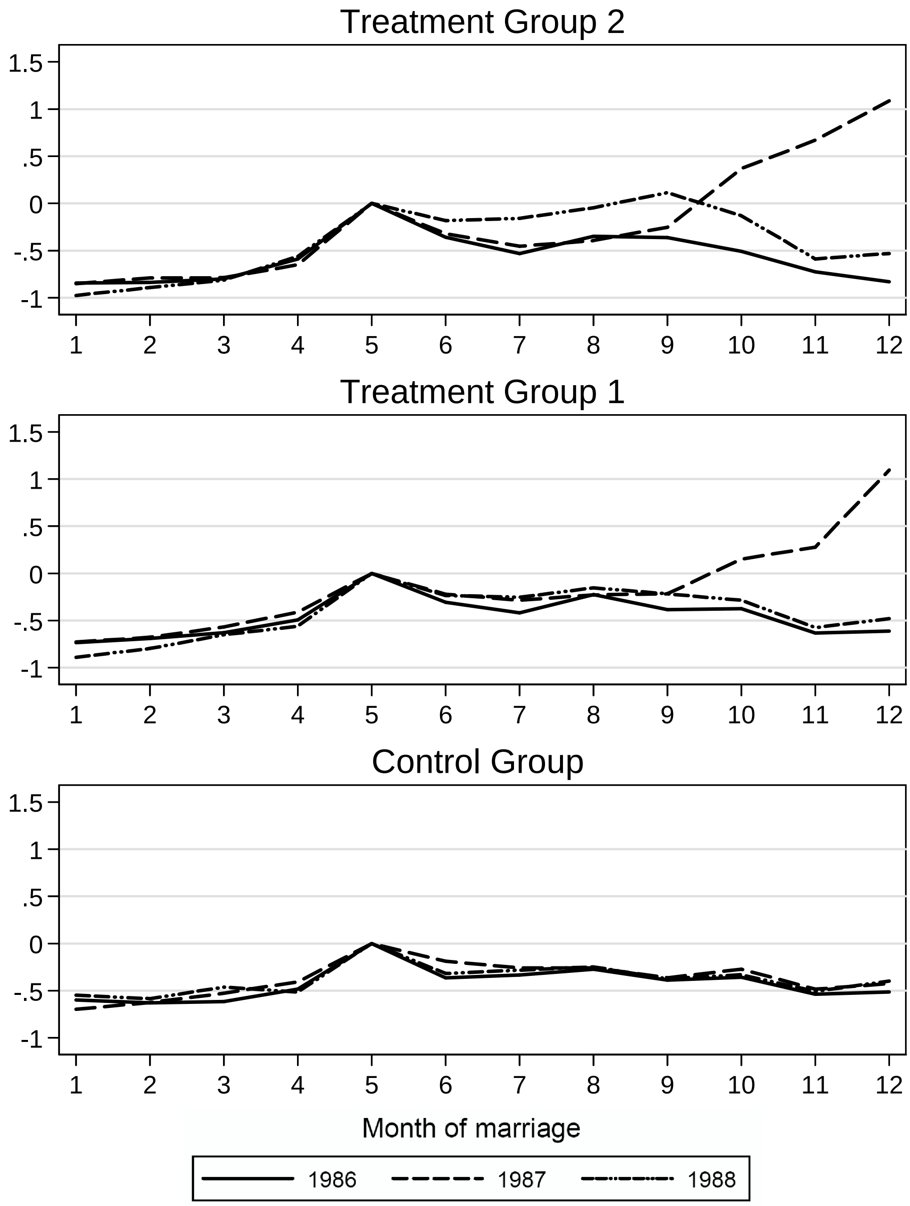
6 Tables & figures

Figure 1: Annual number of marriages and divorces per 1,000 of population, Austria 1960 through 2009[a]



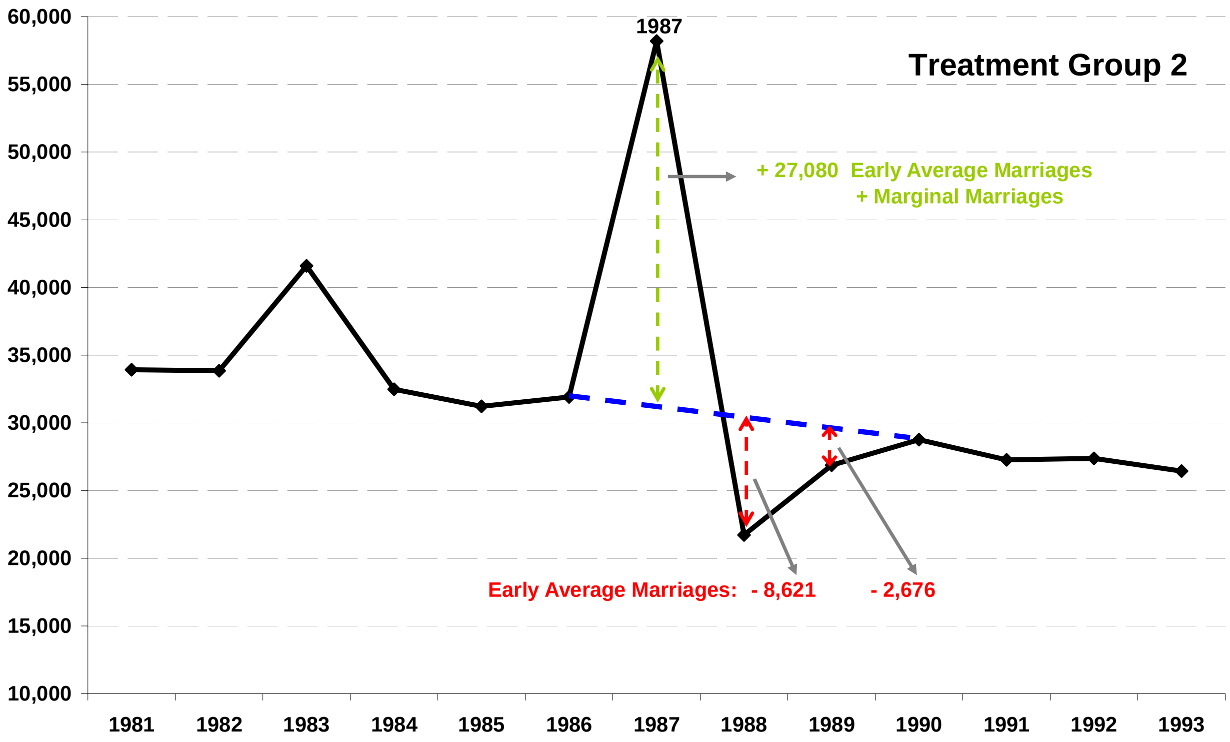
[a] Own calculations based on data from *Statistics Austria*; details are available upon request. Note, per December 31, 1971 the deductability of furnishings and articles of daily use up to 70,000 Austrian schilling within the first five years after the establishment of a new household by newlyweds was abolished. However, per January 1, 1972 a marriage subsidy for every person with unrestricted tax liability in Austrian who had never been married of 7,500 Austrian schilling was introduced. That means, two Austrian citizens, both marrying the first time, received a total of 15,00 Austrian schilling (2010: EUR 4,250 or USD 5,680). Per January 1, 1984 the tax deductibility of dowry was abolished. Per December 31, 1987 the marriage subsidy was suspended with any replacement. This was announced on August 26, 1987.

Figure 2: Monthly number of marriages by group in the years 1986 to 1988[a]



[a] Own calculations based on data from the *Austrian Marriage Register*. These graphs show the number of monthly marriages for three groups (see below) 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 where each spouse has never been married before. Treatment group 1 consists of couples where only one spouse has been married before. The control group covers couples where both spouse had been married before.

Figure 3: Quantification of (early) average marriages and marginal marriages[a]



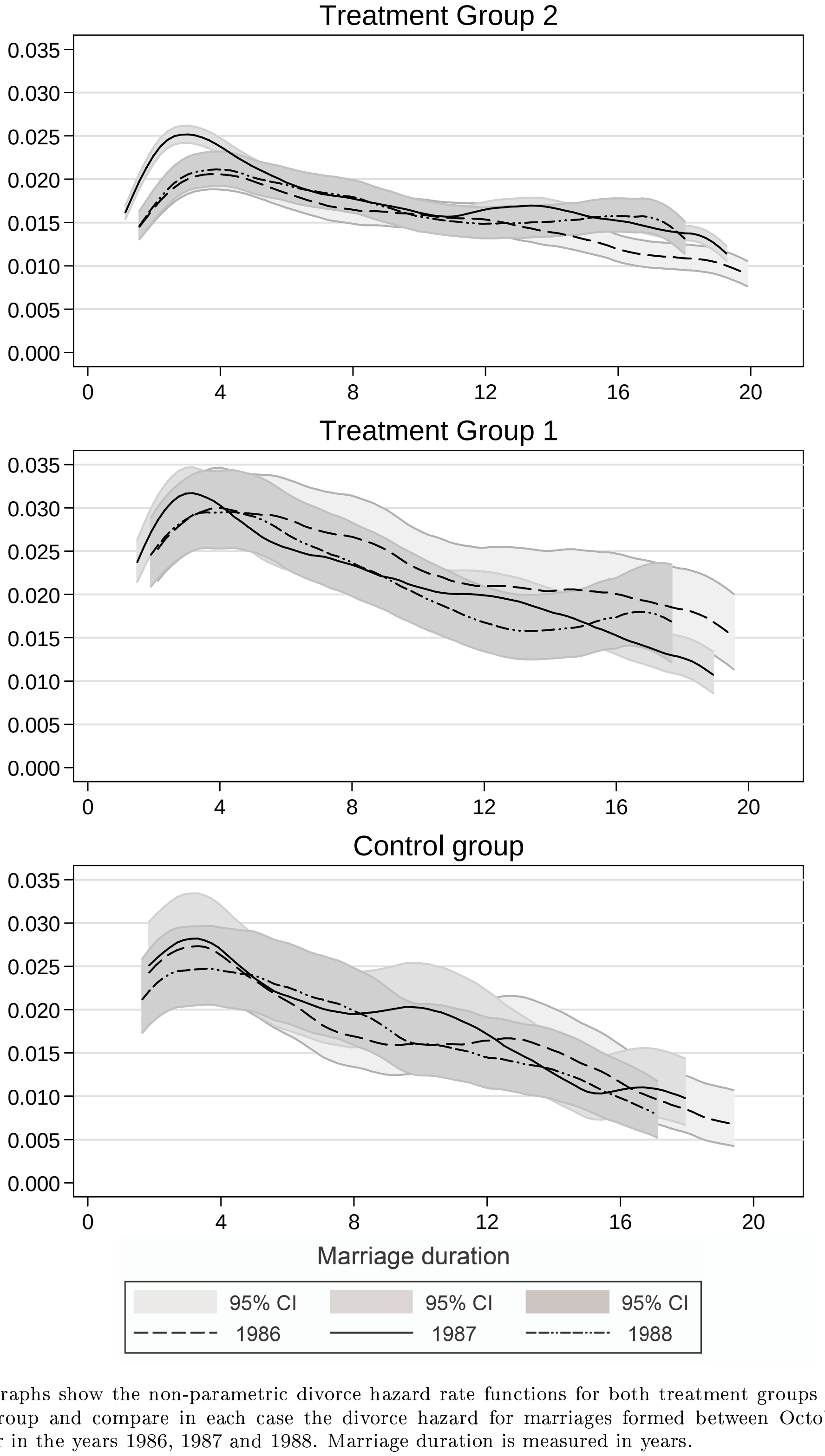
[a] Own calculations based on data from the *Austrian Marriage Register*. This graph shows the number of yearly of marriages of treatment group 2 couples (i. e. neither spouse has been married before) from 1981 through 1993. See also notes to Figures 1 and 2.

Table 1: **Characteristics of average and marginal marriages**

	Treatment group 2			Treatment group 1			Control 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
No. of premarital kids	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 undenominational	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, undenominational	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, undenominational	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	23.2	24.1	20.3	18.2	18.6	18.0	17.0	15.5	17.2
White collar	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	43.0	43.9	38.7	29.6	30.4	27.9	22.1	21.0	23.1
White collar	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
No. of observations	5,658	31,005	5,258	1,280	3,884	1,229	906	958	967

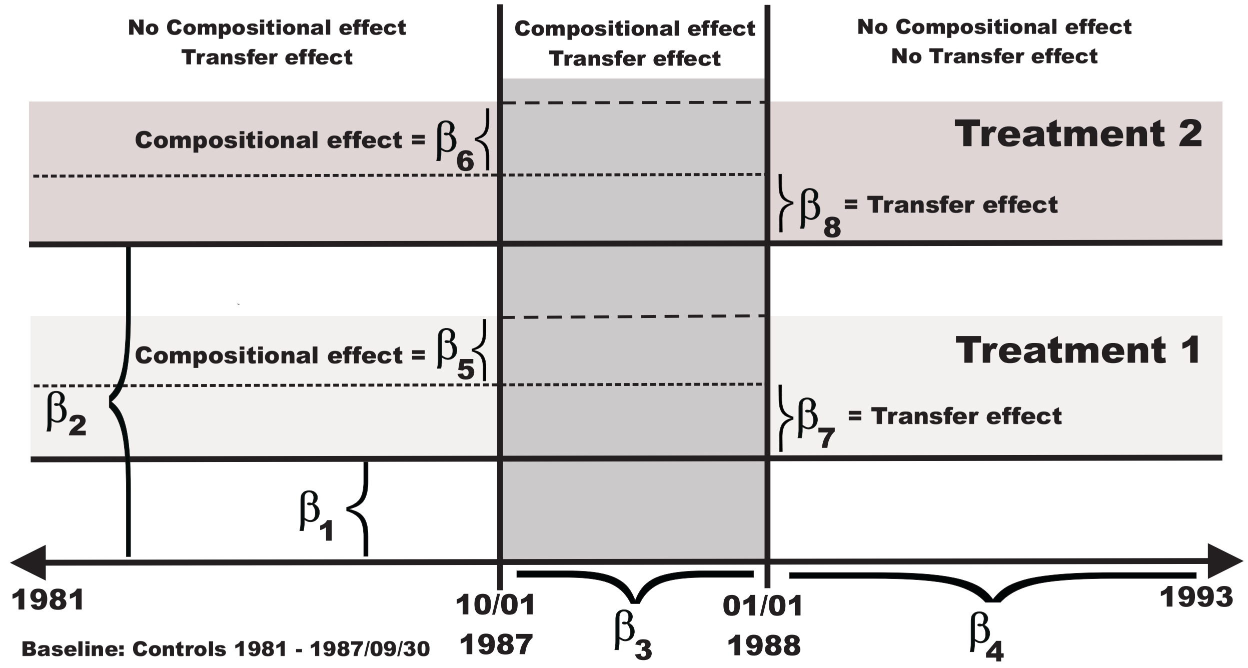
Own calculations based on data from the *Austrian Marriage Register* and the *Austrian Social Security Database* (ASSD). In each column only marriages between two Austrian citizens formed between October and December are included. Note, from January 1, 1972 through December 31, 1987 every person with unrestricted tax liability in Austria who had never been married before received 7,500 Austrian schilling (2010: EUR 2,125 or USD 2,840) upon marriage. The suspension of this marriage subsidy has been announced on August 26, 1987. Treatment group 2 comprises couples where each spouse has never been married before. Treatment group 1 consists of couples where only one spouse has been married before. The control group covers couples where 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 (around 36%) we used the average labor market states of all so-found matches.

Figure 4: Hazard function by group for the years 1986, 1987 and 1988[a]



[a] These graphs show the non-parametric divorce hazard rate functions for both treatment groups and the control group and compare in each case the divorce hazard for marriages formed between October and December in the years 1986, 1987 and 1988. Marriage duration is measured in years.

Figure 5: Research design



[a] This graph depicts our research design. We have two treatment groups and one control group: treatment group 1 (β_1; only one spouse eligible), treatment group 2 (β_2; both spouses are eligible), and control group (base group; no spouse is eligible). We have three different time periods: Pre-treatment period (1981 through September 1987, no compositional effect, transfer effect due to existence of marriage subsidy), treatment period (β_3; October through December 1987, compositional effect due to marginal marriages and transfer effect), and a post-treatment period (β_4; 1988 through 1993, no compositional effect, no transfer effect). The compositional effects for treatment 1 and 2 are given by β_5 and β_6, respectively. The transfer effect for treatment 1 and 2 are given β_7 and β_8, respectively

Table 2: Marital instability[a]

	(I) 1981-1993		(II) without 1983		(III) w/o 1983 & h1-1988		(IV) + Labor		(V) + Kids	
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 & 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	
Pre-marital children[e]	no		no		no		no		yes	
No. of observations	550,295		498,654		486,876		486,876		400,381	

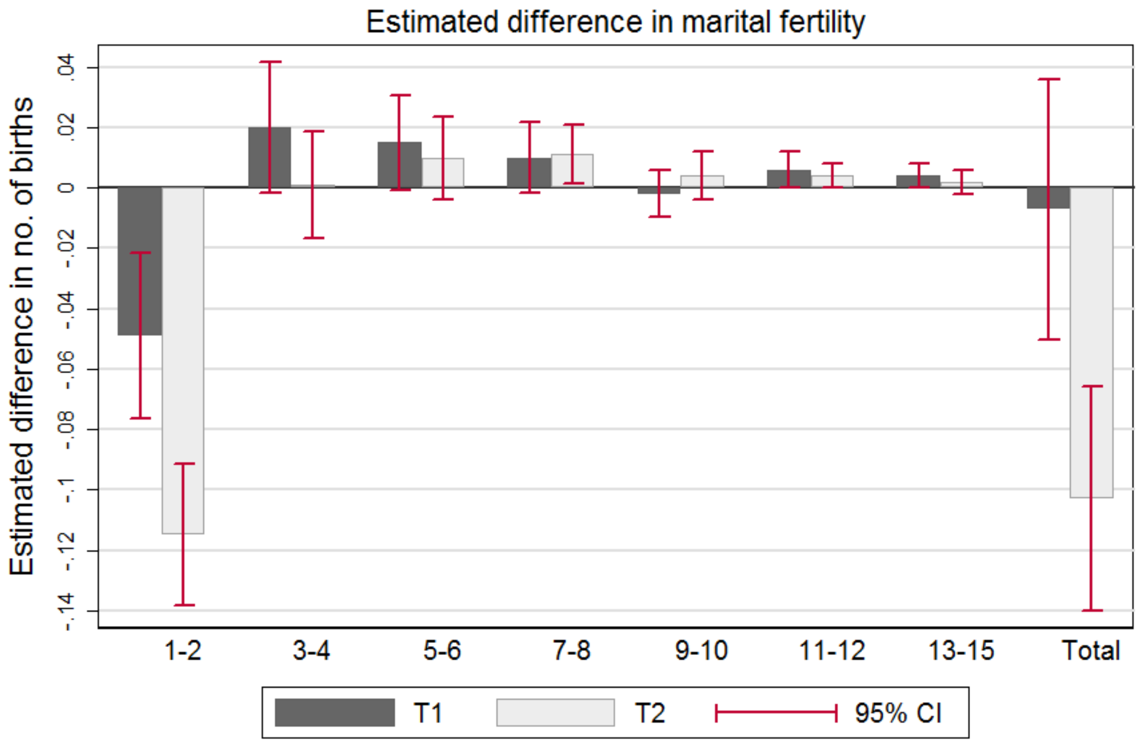
[a] Estimation method: Cox proportional hazards model. Hazard ratios with p-values (based on heteroskedasticity-robust standard errors) in parentheses. *, ** and *** indicate statistical significance at the 10-percent, 5-percent and 1-percent level respectively. Interaction terms recomputed according to Ai & 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 & other denomination, catholic & no denomination, other denomination & 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 blue-collar worker, employed as white-collar worker, other employment (e. g. self-employed), unemployed, and out of labor force. [e] The estimation includes a cardinal variable capturing the number of joint pre-marital children.

Table 3: Marital fertility[a]

	(I) w/o pre-marital children		(II) with pre-marital 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 & age difference[b]	yes		yes	
Religious denomination[c]	yes		yes	
Labor market status[d]	yes		yes	
Pre-marital children[e]	no		yes	
Mean of dep. var.	1.195			
S.d. of dep. var.	1.060			

[a] Dependent variable is the number of marital children born by 2007. Estimation method: ordinary least squares. Coefficients with p-values (based on heteroskedasticity-robust standard errors) in parentheses. *, ** and *** indicate statistical significance at the 10-percent, 5-percent and 1-percent level respectively. 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 & other denomination, catholic & no denomination, other denomination & 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 blue-collar worker, employed as white-collar worker, other employment (e.g. self-employed), unemployed, and out of labor force. [e] The estimation includes a cardinal variable capturing the number of joint pre-marital children.

Figure 6: Timing of marital fertility



[a] This figure summarizes estimated compositional effects in the number of marital children equivalent to those presented in Specification (II) of Table table-fertility, however, separated by marriage duration.

Table 4: Health at birth[a]

	Gestation length[b]		Birth weight[c]		Apgar score 10[d]		Male birth	
Selection effects:								
$\beta_5 : T_1 \cdot TP$	−0.243	(0.152)	−95.62*	(0.054)	−0.046	(0.187)	0.036	(0.466)
$\beta_6 : T_2 \cdot TP$	−0.217	(0.175)	−86.57*	(0.044)	−0.031	(0.318)	0.022	(0.640)
Transfer effects (inverse):								
$\beta_7 : T_1 \cdot postTP$	0.022	(0.862)	−12.68	(0.734)	−0.002	(0.954)	−0.001	(0.977)
$\beta_8 : T_2 \cdot postTP$	−0.069	(0.551)	5.62	(0.871)	−0.014	(0.656)	−0.001	(0.978)
$\beta_1 : T_1$	0.235	(0.492)	32.41	(0.744)	−0.030	(0.749)	0.009	(0.918)
$\beta_2 : T_2$	0.034	(0.914)	57.21	(0.532)	−0.060	(0.498)	0.016	(0.841)
$\beta_3 : TP$	0.208	(0.194)	79.73	(0.102)	0.023	(0.450)	−0.023	(0.628)
$\beta_4 : postTP$	0.075	(0.517)	1.19	(0.972)	0.003	(0.931)	0.002	(0.941)
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	
Observations	230,168		230,168		227,482		230,168	
Mean of dep. var.	39.684		3,255.02		9.879		0.513	
S.d. of dep. var.	1.773		516.07		0.535		-	

[a] Estimation method: ordinary least squares. Coefficients with p-values in parentheses. *, ** and *** indicate statistical significance at the 10-percent, 5-percent and 1-percent level respectively. 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 2,686 observations. [e] The estimation includes binary variables capturing the following combinations of spouses' religious denominations: catholic & other denomination, catholic & no denomination, other denomination & 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 blue-collar worker, employed as white-collar worker, other employment (e. g. self-employed) & not employed. [g] The estimation includes a cardinal variable capturing the number of pre-marital children.