Marriage Subsidies and Divorce: An Analysis of Marginal Marriages*

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incomplete draft,
December 31, 2010

Abstract

Policies to promote marriage are controversial. It is essential to distinguish between a marriage which is created by a marriage promoting policy (*marginal marriage*), and a marriage which 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. Based on a difference-in-differences approach we show that marginal marriages are at least 26 percent less likely to have children and 17 percent more likely to divorce.

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

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For helpful discussion and comments we would like to thank Raj Chetty, Enrico Moretti, Helmut Rainer, Mario Schnalzenberger, Andrea Weber, participants at the Annual Conference 2010 of the European Society for Population Economics in Essen, of the Friday Seminar at the Austrian National Bank, the Departmental Seminar at the University of Strathclyde, the Economic Research Seminar at the National Taiwan University, the Economic Research Seminar at the Vienna University of Economics and Business, the KOF Research Seminar at the ETH Zurich, and the Economic Research Seminar at the University of St. Gallen. This paper was partly written during Martin Halla's and Rudolf Winter-Ebmer's visiting scholarship at the Center for Labor Economics at the University of California at Berkeley. They would like to thank for the stimulating academic environment and hospitality there. Financial support from the Austrian FWF (NRN Labor Economics and the Welfare State) is gratefully acknowledged.

1 Introduction

Policies to promote marriage are controversial (McLanahan, 2007; Amato, 2007*b*; Furstenberg, 2007*b*; Amato, 2007*a*; Furstenberg, 2007*a*; Struening, 2007). While there is an extensive empirical literature (Waite and Gallagher, 2000) documenting a strong correlation between being married and better health, longer life, higher wages, greater wealth and better child outcomes, scholars do not agree whether marriage has a causal effect on family outcomes; confounding factors that further marriage may also be beneficial to the outcomes under consideration. 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).

The most prominent marriage promoting policy is the *Temporary Assistance for Needy Families* (TANF).[1] 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.[2] 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 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).[3] Similar policies can be observed in many other OECD member countries.

The debate on policies to promote marriage can be linked to a strand of literature that analyzes the impact of (dis)incentives created by the tax and transfer programs to marry or to stay married. This literature asks the fundamental question whether the state is capable to influence individuals' marital decisions. Empirical work consistently shows that individuals respond to tax incentives in their marital decisions as predicted by theory, however, the magnitude of these effects is typically small (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 (1997) concludes based on

[1] 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.

[2] 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.

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

a comprehensive survey of the literature from the last three decades that transfer programs do affect marital decisions.

Given that we know that the state is potentially able to affect individuals' marital decisions, and we are willing to assume that marriage has a true causal effect on families' well being, it is still unclear whether marriage promoting policies are welfare-enhancing. The state intervention has two potentially important effects, which have to be considered. First, and probably most important, the policy intervention will change the selection into marriage (*selection effect*). Here 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 and without a state intervention. A marginal marriage is given by spouses who would not have married without the state intervention.[4] In other words, marginal marriages are created by the state intervention. In order to evaluate the efficiency of marriage promoting policies it is crucial to understand and quantify this selection effect along all dimensions of marital outcomes. A second potential effect of the policy intervention is given by a *transfer effect*. The transfer effect describes the behavioral response due to additional resources on family outcomes in the absence of selection; the true causal effect of the reform. We argue that a key dimension in such an evaluation is clearly marital stability. Based on theoretical grounds (e. g. Becker, 1973, 1974) marginal marriages should have a lower (match) quality compared to average marriages, and should exhibit a higher baseline divorce hazard. That means, it would be revealing to know by how much marginal marriages are more likely to divorce compared to average marriages. Moreover, many beneficial effects of marriage are related with children; it is, therefore, be important to study fertility decisions of such marginal marriages.

To be concrete, in this paper we study marginal marriages created by a unique natural experiment in Austria. Traditionally, the Austrian government subsidized newlywed couples via tax exemptions. In the year 1972 the government switched to a more straightforward marriage promoting policy, and provided cash transfers. Two Austrian citizens, both marrying the first time, got 15,000 Schilling cash on hand with no strings attached. This is equal to about EUR 3,800 or USD 5,300 in the year 2009. However, at the end of August 1987 the Austrian Minister of Finance unexpectedly announced the suspension of this marriage subsidy (without any replacement) by January, 1 1988. This led to an enormous marriage boom in the three months from September to December 1987, see Figure 1.[5] Part of the marriage boom might be due to timing. Some couples could simply have married in advance. However the spike in 1987 is much more pronounced than the slump in 1988, which means, that the announcement to stop subsidizing marriages created additional marriages as well. We will show that about half of the couples who married

[4] In the terminology of the literature on *local average treatment effects* one could term marginal marriages *compliers* and average marriages *always-takers*.

[5] Compared to the time period from October to December 1986 with 7,844 marriages, 35,849 couples decided to marry within the same period in 1987. This is an increase of more than 350 percent. It should be noted, that during the period under consideration another policy intervention affected the incentive to marry. Per January, 1 1984 the tax deductibility of dowry was abolished, and increased the incentive for couples (supposedly from wealthy families) to marry before 1984.

between September and December 1987 where motivated by the cash transfer and, thus, constitute marginal marriages.

The announcement of the suspension of a marriage subsidy provides a clear incentive to marry. In a standard family matching model with frictions (see, for instance, 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, and second the value of a continued search (for a better match) is reduced by not getting the subsidy after the suspension. That means, 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 – i. e. in the minimum acceptable match quality sufficient for a marriage. Marginal marriages are precisely given by those matches, which became only 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 an average marriages; whose match quality would be sufficient even without state intervention.

In the presence of information asymmetries about the match quality (see Mortensen (p. 233f, 1988)), the effect of the announcement of the suspension on the (reservation) match quality should be even more pronounced. The true match quality is *ex ante* unknown to the partners, and is only revealed after some time of experience. If the quality turns out to be less than reservation quality, the pair separates because both benefit more as singles. This should be particularly true for marginal marriages, because the marriage subsidy forces people to decide within a few months, shortening the time for learning about the true quality. Therefore, marginal marriages are matches where the learning process is less advanced than for average marriages or the expectations about the true match quality are distorted by the announcement. In other words, marginal marriages should make more mistakes in expectations than average marriages leading to lower match quality and higher marital instability.

We use the universe of all Austrian marriages and a difference-in-differences estimation strategy to show that marginal marriages have a likelihood of divorce which is about 17 percent higher than average marriages. Thus, the selection effect erodes the initial policy purpose to a large extent. Potential beneficial effects of marriage – as far as they exist (for marginal marriages) – do not have the chance to materialize. Correspondingly, these couples have less marital offspring.

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 the year 1987. In Section 3, we present the data and our difference-in-differences estimation strategy. Section 4 provides the results of our estimations. 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 newlywed couples could deduct nome furnishings and articles of daily use up to 70,000 Austrian schilling within the first five years after the establishment of their new household.[6] Starting from 1972, the Austrian government switched to a more straight-forward marriage promoting policy and provided 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.[7] This corresponds to approximately EUR 1,900 or USD 2,650 in the year 2009. That means, two Austrian citizens, both marrying the first time, received a total of EUR 3,800. 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, 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 their 1971 election campaign; which they redeemed after they gained majority in the Austrian Parliament on October, 10 1971. Over time, the regulations of this marriage subsidy did not change, and the transfer had not been adjusted for inflation. However, on August 26, 1987 the Minister of Finance quite unexpectedly announced the suspension of the marriage subsidy per December, 31 1987 without any compensatory schemes.[8] The suspension was justified by necessary budget cuts and was quickly enacted without any further parliamentary discussion on October 21, 1987.[9] Between 1972 and 1987 1,282,137 eligible individuals received a total of EUR 2,436,060,300 (in 2009 value), i. e. the Austrian government spent on average 0.75 percent of gross domestic product per year to subsidize marriages.

3 Estimation strategy and data

Our empirical analysis concentrates on the selection effect. We are interested in the differential divorce likelihood 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 likely to divorce, compared to a couple who would have married even without this intervention. We argue that this divorce gradient is a parameter which should actually be taken into account before adopting (costly) marriage promoting policies.

[6] See, Bundesgesetzblatt (BGBl.) 161/196 (`http://www.ris.bka.gv.at/Dokumente/BgblPdf/1966_161_0/1966_161_0.pdf`).

[7] See, BGBl. 460/1971 (`http://www.ris.bka.gv.at/Dokumente/BgblPdf/1971_460_0/1971_460_0.pdf`). 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.

[8] See, for instance, *Kronen Zeitung* on August 27, 1987 (`http://www.econ.jku.at/members/Halla/files/research/marriage-subs/Announcement_of_suspension_Kronen_Zeitung_1987-08-27.pdf`).

[9] There was no prior discussion of such a suspension in the press, nor was there a parliamentary debate before August 1987.

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. We focus on the suspension of the subsidy, rather than on its introduction, for two reasons. First, prior to 1972, some incentives to marry 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 measurements, it had been announced abruptly by the Minister of Finance (without any prior discussions) end of August 1987. The suspension, thus, provides a very clear break.

The selection effect to be estimated, quantifies, therefore, the differential divorce likelihood of such a marginal marriage compared to a couple who would have also married in absence of this announcement. Note, our estimation analysis will also identify the true causal effect of the marriage subsidy, which is equal to the causal effect of the additional cash transfer on the divorce likelihood in the absence of any selection effect. We refer to this effect as the *transfer effect*.

We combine data from the *Austrian Marriage Register*, covering all marriages from 1974 to 2007, with the *Austrian Divorce Register*, covering all divorces from 1974 to 2007.[10] Our sample consists of all $1,270,206$ marriages which took place between 1974 and 2007. From these marriages, $277,175$ divorced until the end of 2007.

3.1 Locating marginal marriages

In order to estimate the selection effect we actually have to be able to identify marginal and average marriages. While this is impossible at an individual level, our research design allows to quantify this number. 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): (i) a control group, comprising couples where no spouse is eligible, (ii) a treatment group 1 (T^1), consisting of all couples where one spouse is eligible, and (iii) a treatment group 2 (T^2), equal to couples where both spouses are eligible. That means, spouses from T^2 couples – where both partners have never been married before – face the highest incentive to marry; their marriage had been subsidized in sum with $15,000$ Austrian schillings. T^1 couples comprise one spouse which had been married before; they received only $7,500$ Austrian schillings. The control group couples consist of spouses, where both of them have been married before; they were not eligible for any marriage subsidy.

Figure 2 shows the number of monthly marriages by group for the years 1986, 1987 and 1988. In the year 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,

[10] The *Austrian Marriage Register* includes information on the date of marriage, the spouses' former family status, their place of residence, their age at marriage, their religious denomination and on their citizenship. Since 1984 information on the spouses' country of birth and on the number, age and sex of any premarital children is also recorded. From the available information in the the *Austrian Divorce Register*, we use the date of divorce.

the patterns overlap in all three years. In the year 1987, we observe for T^1 and T^2 marriages a clear divergence of the normal seasonal pattern starting in October. The announcement of the suspension of the marriage subsidy 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 need at least one month (September) to plan their weddings. In the year 1988 we observe somewhat smaller numbers of T^1 and T^2 marriages in the first quarter of the year, which might be due to some couples who married in advance of the suspension of the transfer.

Figure 3 shows the annual number of marriages of treatment group 2 couples from 1974 through 2007. It seems that the long-run trend of this series – i. e. the trend that would have been observed without the suspension of the marriages subsidy – can be approximated very well by a linear interpolation between 1986 and 1990. This is illustrated by the dashed line. The additional marriages in the year 1987, i. e. the number of marriages that exceed the interpolated long-run trend in the marriage rate, are equal to 27,082 (see the vertical green bars), and can be attributed to two groups: (i) Couples who had planned to marry (in the near future) and decided to marry earlier in order to cash the subsidy, and (ii) couples who had actually no plans to marry, however, 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).[11] Consequently, the number of marginal marriages is equal to 15,786 – the difference between the surplus from the year 1987 and the sum of the shortfalls from the years 1988 and 1989. Since these marginal marriages by definition 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. At least, one has to make an appointment at the County Clerk's office or at City Hall. Figure 2 indicates that the marriage boom kicked in October, suggesting that about one month of wedding planning has been necessary. If we relate the 15,786 marginal marriages (and the 11,297 early average marriages) to all 31,006 T^2 marriages formed between October and December 1987, we find that 51 percent of these were marginal marriages, 36 percent were early average marriages, and the remaining 13 were average marriages. If we apply an equivalent procedure to T^1 marriages, we find a comparably lower share of marginal marriages (44 percent), see Figure 4.

Table 1 compares the average characteristics of spouses from the two treatment groups and the control group (who married between October and December) for the years 1986, 1987 and 1988.[12] Given that roughly every second T^1 and T^2 marriage in 1987 constitutes

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

[12] **?** show for Austria that divorce risk is related to a higher age at marriage, a lower likelihood of having any religious denomination and a higher incidence of joint premarital children.

a marginal marriage, this comparison should show observable differences between average and marginal marriages. Somewhat surprisingly, these numbers suggest that average and marginal marriages are very comparable along measurable characteristics documented in the data. The only notable difference is the higher incidence of premarital children among T^1 marriages.

3.2 Difference-in-differences estimation strategy

To estimate the duration of a marriage we use a Cox proportional hazard models (Cox, 1972). In such a model, the hazard rate at time t – i. e. the risk that a marriage dissolves at time t, provided it lasted that long – is explained by a non-parametric baseline hazard $h_0(t)$ which 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.[13] In order to estimate 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. As introduced above, we distinguish between spouses i from the two treatment groups (T_i^1 and T_i^2) and the control group (C_i).[14] Further, we have three time periods. There is a pre-treatment period (captured by $preTP_t$) starting with our sample in 1974 and running through September, 30 in 1987. The treatment period (TP_t) is given by the period between October, 1 1987 through December, 31 1987. Thereafter, the post-treatment period ($postTP_t$) starts. This gives rise to the following specification of $\mathbf{X}\beta$ (see also Figure 6):

$$\begin{aligned}\mathbf{X}\beta = \beta_0 + \beta_1 T_i^1 + \beta_2 T_i^2 + \beta_3 TP_t + \beta_4 postTP_i + \beta_5 T_i^1 * TP_i + \beta_6 T_i^2 * TP_i \\ + \beta_7 T_i^1 * postTP_i + \beta_8 T_i^2 * postTP_i + \gamma * X_i + u_i\end{aligned} \tag{2}$$

This specification 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), as well as for marriages formed in the three different time periods (see β_3 and β_4). The coefficients of the interaction terms between the two treatment group indicators and the treatment period dummy gives us a lower bound estimate of the selection effect for T^1 and T^2marriages because in the treatment period TP_t around half of the marriages are composed of marginal marriages and half of

[13] 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 $\mathbf{X}$, $\frac{h(t|\mathbf{X}^*)}{h(t|\mathbf{X})}$.

[14] Figure 5 plots the hazard function by group for marriages formed between October and December in the years 1986, 1987 and 1988. For all groups (and years) we can see that given a marriage 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 difference between the hazard functions of the years 1986, 1987 and 1988. However, in the case of treatment group 2 one can see that marriages formed in 1987 have a statistically significant higher divorce hazard compared to those formed in either 1986 or 1988.

average ones. The estimates of the transfer effect for T^1 and T^2 marriages are given by β_7 and β_8, respectively. To be precise, 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 in order to learn the causal effect of the additional resources on the divorce hazard.

As additional control variables we only include exogenous factors (i. e. pre-determined at the time of marriage), such as the wife's age, the spouses' age difference (squared), and the spouses' religious denomination[15] at the time of marriage. In further specifications, we control in addition for calendar month and 115 district fixed-effects. It can be argued that all other factors which might also have an important impact on divorce risk are endogenous with respect to the viability of the marriage: e. g. the number of post-marital children, labor supply of either partner or marital satisfaction. If the quality of the marriage is bad and the risk of divorce is relatively high, it can be expected that both partners might invest less in marriage-specific capital, like joint children or joint enterprises of any kind, and that specialization in the household might be different with corresponding changes in labor supply. For these reasons we refrain from using such potentially endogenous regressors.

4 Estimation results

4.1 Marital stability

Our results are consistent with the theoretical prediction that marginal marriages have a lower match quality. In Table 2 Col. (1) presents our baseline results, in Col. (2) fixed effects for marriage months and districts are included; the results are qualitatively very similar. We find significant selection effects for the suspension of the cash transfer: While there is no effect for the small treatment T^1, couples who got the full transfer T^2 who married in anticipation of the suspension of the transfer face an 8 percentage point higher divorce hazard. In Col. (3) we exclude marriages formed in the year 1983 because in that year, a suspension of the marriage premium was discussed but, finally, not enacted. The results do not change sizeably. We have to consider that in our treatment period only around half of the couples really were marginal marriages, while half of the couples advanced their marriage plan to profit from the cash transfer. A simple calculation would, therefore, consider a selection effect of a $16 - 17$ percent higher hazard rate for marginal marriages, which is quite substantial.[16]

Likewise, we do find positive transfer effects: hazard rates for couples receiving a cash transfer have significantly lower hazard rates; the divorce hazard is between 2.1 and 3.6

[15] 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 the marriage between two Catholics will serve as the base group.

[16] The results do not change if we control for the presence of premarital children or use a different sample period.

percent lower, depending on the specification. Note that this transfer effect is in all cases, considerably smaller then the selection effect.

Our difference-in-differences setup shows that our treated groups - basically individuals in their first marriages - have significantly lower hazard rates, which is known from the literature. More importantly, our controls for the treatment period (β_3) and the post-treatment period (β_4) are always insignificantly different from one, which shows that apart from the effect on the treated couples, the considered time periods are indistinguishable from the rest.

What is the quantitative importance of this selection effect? A simple back-of-the-envelope calculation would run as follows: From $31,006$ T^2 marriages between Oct. - Dec. 1987, approx. $15,700$ of these are marginal marriages. Approximately $9,000$ among all these T^2 marriages got divorced until the end of 2007. With a 17 percent higher divorce risk, approximately $5,300$ of the marginal marriages got divorced which leaves us with some 10,400 stable additional marriages - bought with a transfer program of €60 Mio. in today's value.

4.2 Marital fertility- still incomplete!

An important dimension of marital outcomes is the presence of children. Most supposed welfare effects of marrying and in particular in divorcing are concerned with the welfare of the offspring. If giving financial marriage premia would increase the number of children in marginal marriages while, at the same time, these marginal marriages are less stable, we would end up with more children of divorced parents. To investigate this issue we look at fertility patterns following the same difference-in-differences strategy as before.

Table 3 reports the results from a linear probability model for the number of children within two years after marriage. We find that in marginal marriages - on the contrary - less children are born, compared to average marriages (selection effect): Couples marrying before the suspension of the transfer and receiving the small cash transfer (T^1) have on average 6 percentage points less children, those receiving the large transfer (T^2) have 13 percentage points less; which is a relative large effect, given an average fertility rate over these two years of 45 percent. On the other hand, the pure transfer effect is negative: those couples receiving a cash transfer have between 3 and 5 percentage points less children. [17]

Compared to average marriages, marginal marriages tend to have less children. This is not the only comparison, though. We have to compare the number of children in marginal marriages with the number of children, potential spouses would have had, had they not married. Still needs to be done, after we can obtain data on all unmarried women.

[17] The results do not change if we control for the presence of premarital children.

5 Conclusions and policy implications

We have shown that couples who marry because of a marriage promoting policy are a highly selected group. While it is well known by empirical researchers, that individuals reacting to policy instruments (compliers) might be quite different from the average in the population (always-takers in the local-average-treatment-effect language), the implications of such phenomena are not always well understood. If public policy wants to increase the number of marriages with financial premia, potential spouses reacting to such a cash transfer are different from average spouses: they have lower match quality and, therefore, their match is less viable. Our estimates show that marginal marriages have a divorce hazard, which is up to 17 percent higher - compared to average marriages.

By giving financial marriage premia the policy maker can only buy less stable marriages. Additional consequences of such premia on children and in particular on the welfare of children are still to be explored in more detail.

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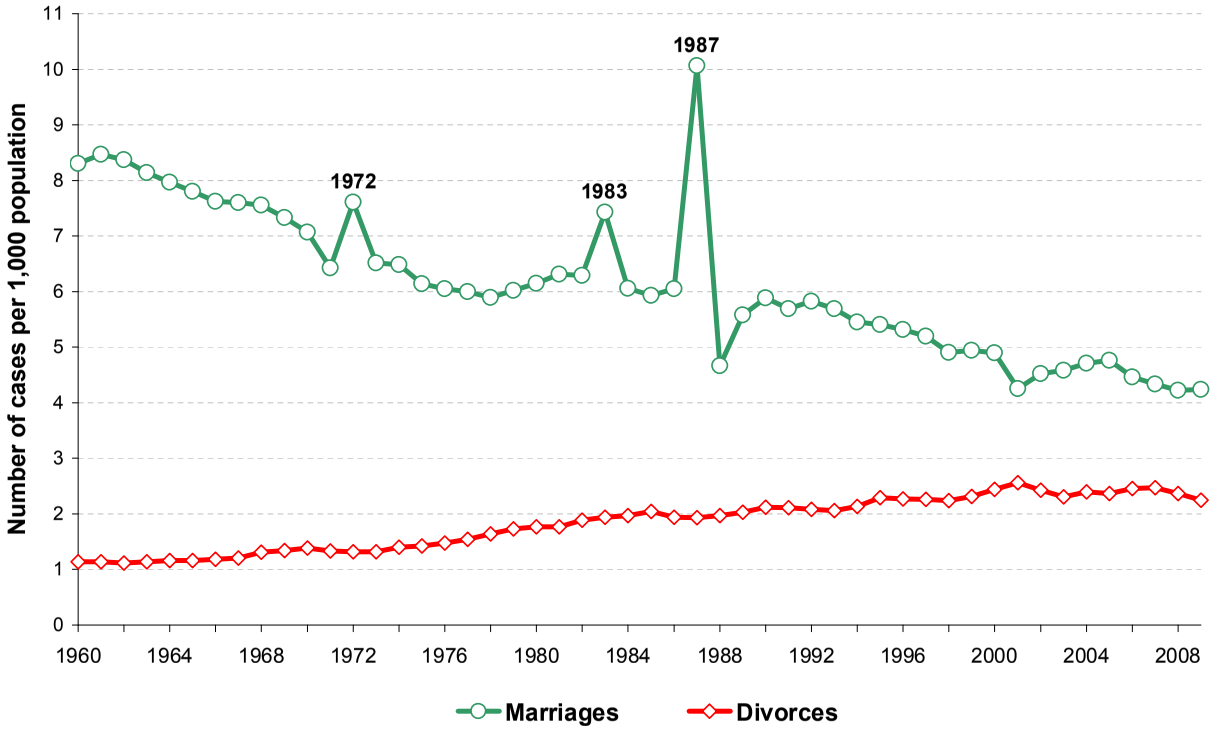
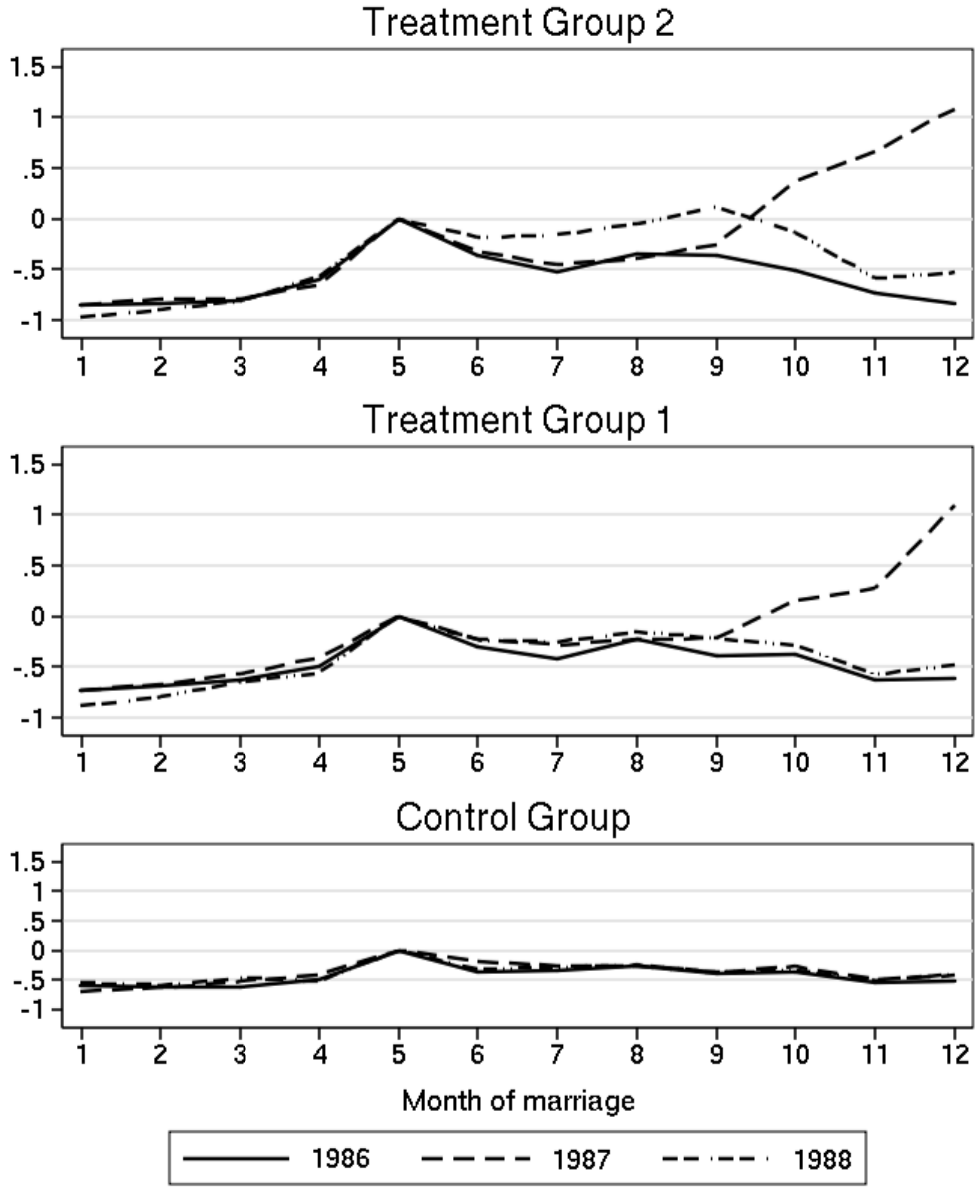
6 Appendix

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



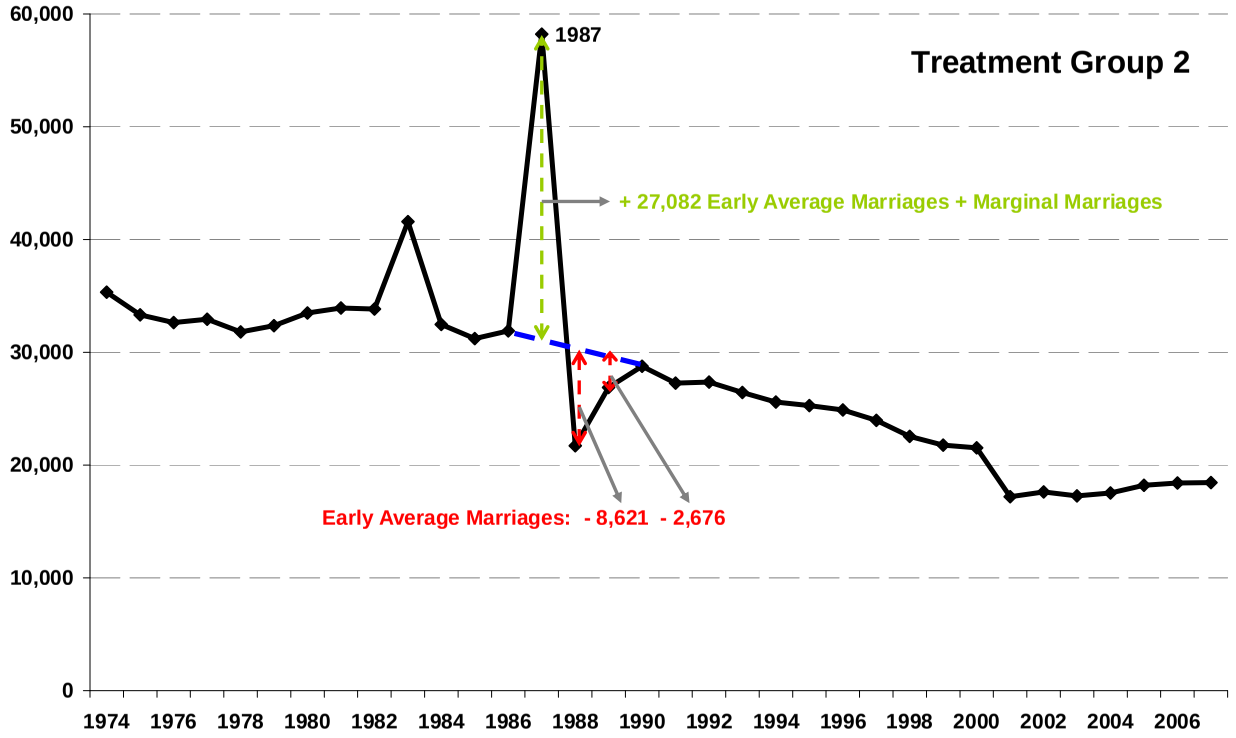
[a] Own calculations based on data from *Statistics Austria*; details are available upon request. Note, per December 31, 1971 the deductability of furnishings and articles of daily use up to 70,000 Austrian schilling within the first five years after the establishment of a new household by newlyweds 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 (2009: EUR 3,800 or USD 5,300). 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 through 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). 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 (2009: EUR 1,900 or USD 2,650) 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 spouse have 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 (see below) from 1974 through 2007. 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 (2009: EUR 1,900 or USD 2,650) 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 spouse have been married before.)

Figure 4: Decomposition of the treatment groups (new)

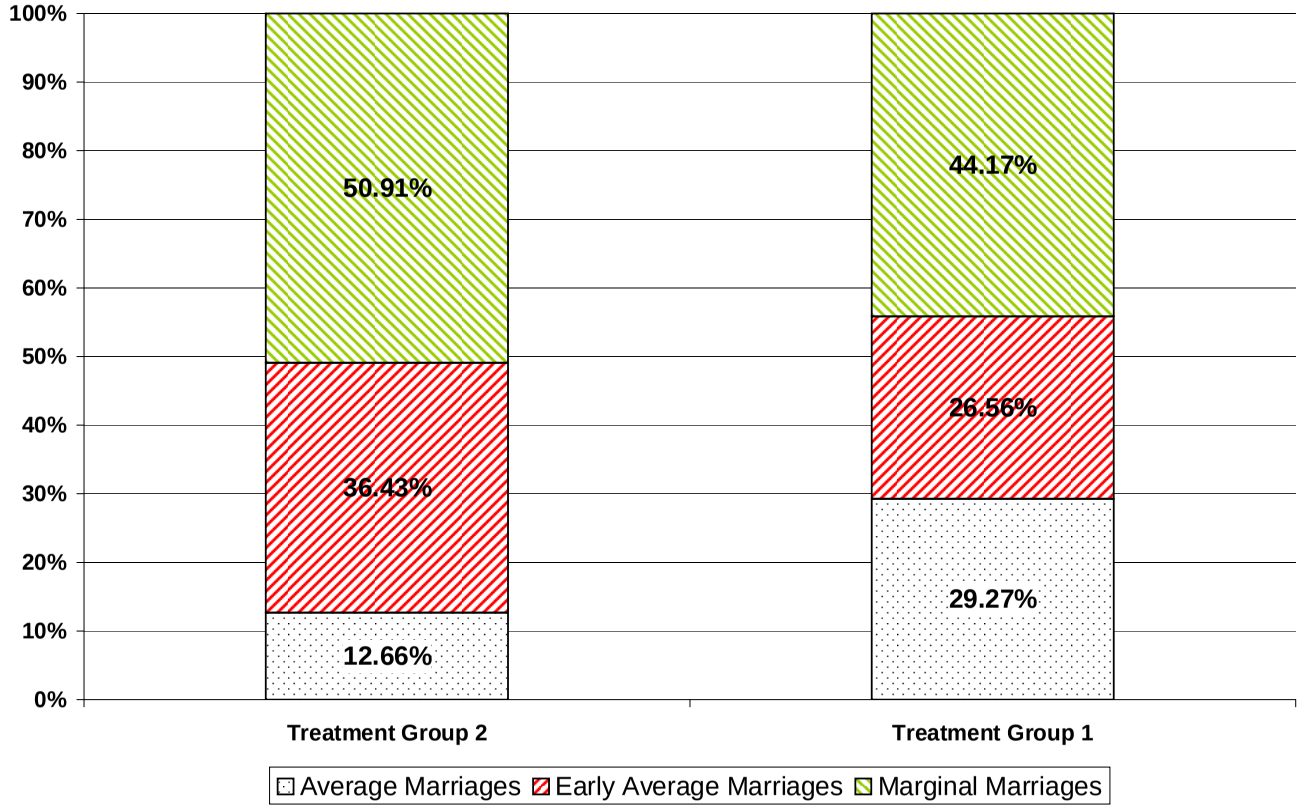
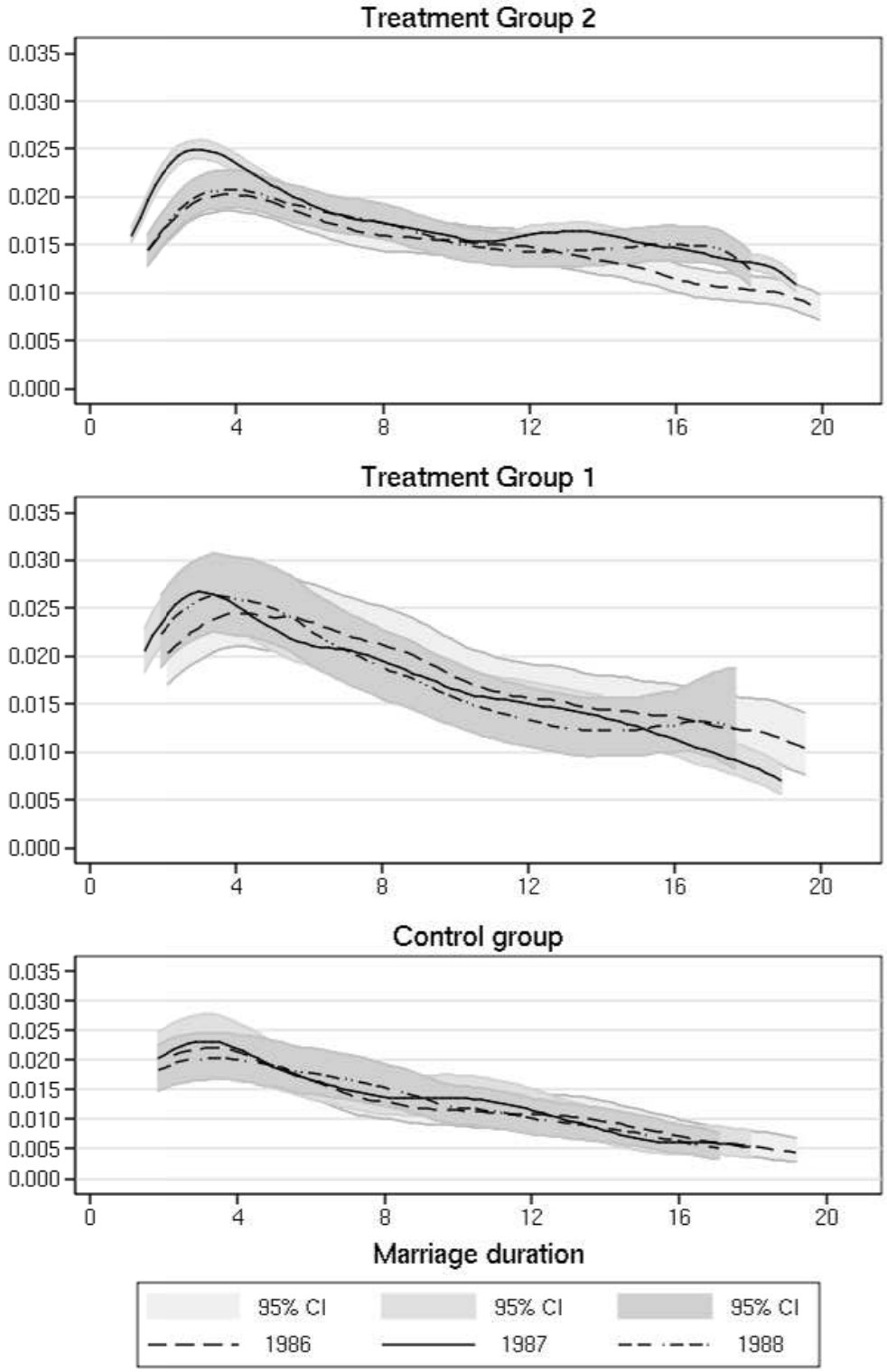


Table 1: Characteristics of average marriages and marginal marriages[a]

	Treatment group 2			Treatment group 1			Controls		
	1986	1987	1988	1986	1997	1988	1986	1987	1988
Age of wife	23.82	24.09	24.26	30.39	31.26	30.64	40.24	40.33	40.25
Age of husband	26.53	26.61	26.77	34.78	35.81	35.00	45.50	45.57	45.44
Age difference	2.70	2.52	2.51	4.39	4.55	4.36	5.26	5.24	5.19
Both catholic	0.862	0.844	0.849	0.672	0.667	0.645	0.535	0.558	0.531
Both undenominational	0.014	0.019	0.017	0.039	0.049	0.062	0.111	0.098	0.119
Both other denomination	0.011	0.009	0.011	0.009	0.011	0.018	0.015	0.016	0.012
Catholic, undenominational	0.041	0.053	0.047	0.149	0.154	0.164	0.192	0.208	0.216
Catholic, other denomination	0.068	0.070	0.072	0.108	0.104	0.093	0.124	0.094	0.092
Other, undenominational	0.005	0.006	0.004	0.023	0.015	0.019	0.022	0.026	0.030
No. of premarital children	0.337	0.339	0.283	0.213	0.307	0.222	0.100	0.109	0.107

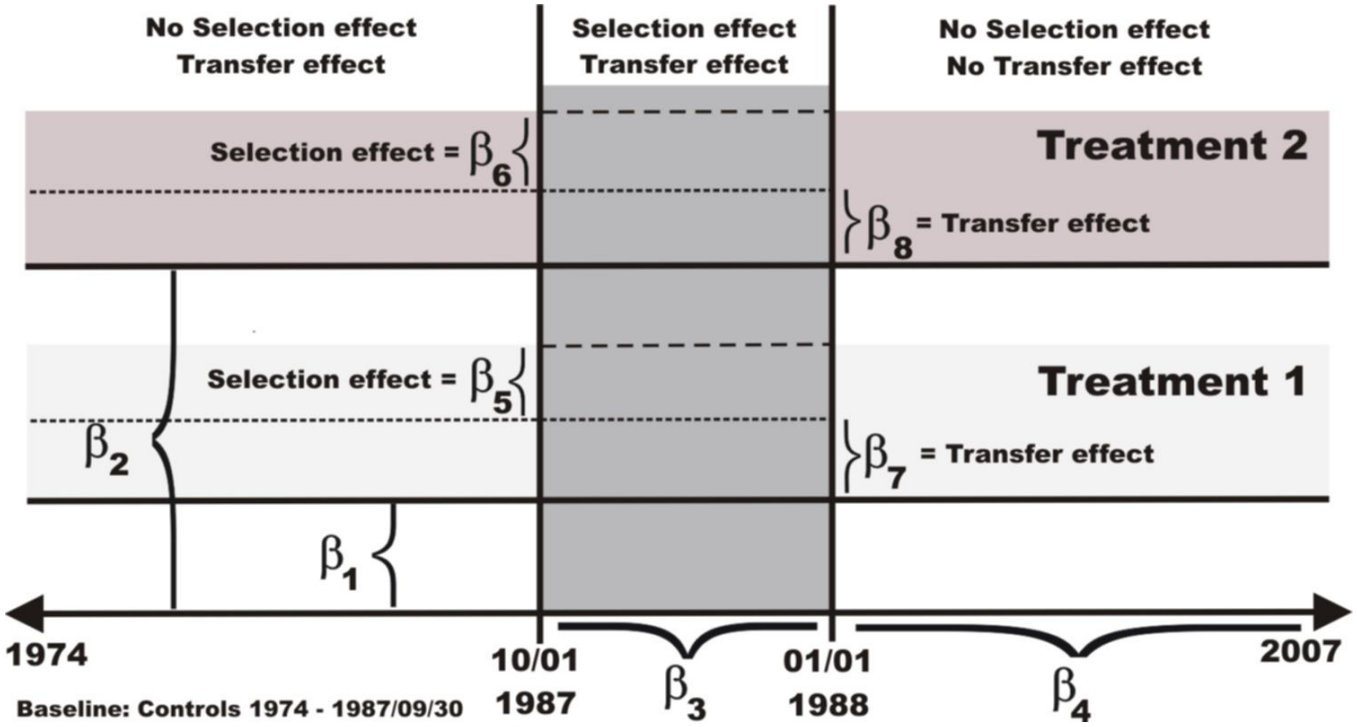
[a] Own calculations based on data from the *Austrian Marriage Register*. In each column only marriages formed between October and December are included. Note, from January 1, 1972 through December 31, 1987 every person with unrestricted tax liability in Austria who had never been married before received 7,500 Austrian schilling (2009: EUR 1,900 or USD 2,650) 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 spouse have been married before. Age and age difference are measured in years.

Figure 5: 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. The shaded areas represent the 95%-confidence interval. Marriage duration is measured in years. before.)

Figure 6: Our 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 (1974 through September 1987, no selection effect, transfer effect due to existence of marriage subsidy), treatment period (β_3; October through December 1987, selection effect due to marginal marriages and transfer effect), and a post-treatment period (β_4; 1988 through 2007, no selection effect, no transfer effect). The selection effect is given by β_5 and β_6. The transfer effect is given by β_7 and β_8.

Table 2: Determinants of divorce risk[a]

	(I) Baseline		(II) I + FE		(III) II w/o 1983	
Selection effects:						
$\beta_5 : T_1 \cdot TP$	1.006	(0.890)	1.002	(0.966)	1.009	(0.865)
$\beta_6 : T_2 \cdot TP$	1.084***	(0.000)	1.080***	(0.007)	1.083***	(0.006)
Transfer effects (inverse):						
$\beta_7 : T_1 \cdot postTP$	0.991	(0.335)	1.036*	(0.087)	1.045**	(0.038)
$\beta_8 : T_2 \cdot postTP$	1.026***	(0.000)	1.021**	(0.028)	1.025**	(0.011)
$\beta_1 : T_1$	0.582***	(0.000)	0.660***	(0.000)	0.659***	(0.000)
$\beta_2 : T_2$	0.268***	(0.000)	0.351***	(0.000)	0.351***	(0.000)
$\beta_3 : TP$	0.954	(0.478)	0.913	(0.180)	0.913	(0.180)
$\beta_4 : postTP$	1.000	(0.984)	1.008	(0.729)	1.003	(0.893)
Control variables included						
Age & age difference[b]	yes		yes		yes	
Religious denomination[c]	yes		yes		yes	
Trend	yes		yes		yes	
Group-specific trend	yes		yes		yes	
Month fixed-effects	no		yes		yes	
District fixed-effects	no		yes		yes	
No. of observations	1,270,206		1,270,206		1,218,469	

[a] Sample 1974-2007. Estimation method: Cox proportional hazards model. Hazard ratios with p-values 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.

Table 3: Probability of marital fertility within 2 years after marriage[a]

	(I) Baseline		(II) I + FE		(III) II w/o 1983	
Selection effects:						
$\beta_5 : T_1 \cdot TP$	-0.096***	(0.000)	-0.095***	(0.000)	-0.097***	(0.000)
$\beta_6 : T_2 \cdot TP$	-0.136***	(0.000)	-0.137***	(0.000)	-0.143***	(0.000)
Transfer effects (inverse):						
$\beta_7 : T_1 \cdot postTP$	0.002	(0.865)	0.005	(0.575)	0.004	(0.681)
$\beta_8 : T_2 \cdot postTP$	0.021***	(0.005)	0.026***	(0.001)	0.021***	(0.006)
$\beta_1 : T_1$	0.053***	(0.000)	0.052***	(0.000)	0.052***	(0.000)
$\beta_2 : T_2$	0.151***	(0.000)	0.127***	(0.000)	0.127***	(0.000)
$\beta_3 : TP$	0.023	(0.143)	0.010	(0.538)	0.001	(0.930)
$\beta_4 : postTP$	0.002	(0.779)	-0.000	(0.948)	-0.002	(0.727)
Control variables included						
Age & age difference[b]	yes		yes		yes	
Religious denomination[c]	yes		yes		yes	
Trend	yes		yes		yes	
Group-specific trend	yes		yes		yes	
Month fixed-effects	no		yes		yes	
District fixed-effects	no		yes		yes	
No. of observations	700,510		700,510		648,773	

[a] Sample 1974-1989. Estimation method: Linear probability model. Dependent variable: Dummy indicating a birth within 2 years after marriage. P-values in parentheses, *, ** and *** indicate statistical significance at the 10-percent, 5-percent and 1-percent level respectively. [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.

Table 4: Population 15 years of age and older by marital status and sex[a]

	1951	1961	1971	1981	1991	2001
All						
Single	28.8	26.8	24.2	27.6	29.4	30.8
Married	57.2	58.5	60.5	57.0	54.9	52.8
Widowed	11.3	11.7	11.9	11.0	9.7	8.6
Divorced	2.7	3.0	3.4	4.5	6.0	7.8
Male						
Single	30.3	29.1	27.0	31.1	33.5	34.9
Married	63.0	64.4	66.5	61.9	58.4	55.6
Widowed	4.4	4.0	3.8	3.3	2.9	2.7
Divorced	2.3	2.4	2.7	3.7	5.2	6.8
Female						
Single	27.5	24.9	21.9	24.5	25.7	27.1
Married	52.5	53.5	55.4	52.8	51.7	50.3
Widowed	17.0	18.1	18.7	17.6	15.9	14.0
Divorced	3.0	3.5	4.0	5.1	6.7	8.6

[a] Source: Own calculations based on data from the decennial Austrian Census 1951 to 2001.