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Social insurance and the marriage market

Petra Persson

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Abstract

Social insurance is often linked to marriage. I model how such linkage affects the marriage market, and exploit Sweden's elimination of survivors insurance to demonstrate economically important responses along several behavioral margins in this market. Entry into marriage reflects a demand for survivors insurance up to 50 years before expected payout, especially among couples with high husband mortality risks. Further, elimination of survivors insurance induces divorces and intra-household redistribution towards wives in pre-existing marriages. Because survivors insurance subsidizes couples with highly unequal earnings (capacities), its elimination also raises the long-run assortativeness of matching. These findings demonstrate that when social insurance is linked to marriage, marital behavior is an integral component of couples' strategies to plan for financial security in old age. The magnitude of these marriage market responses influences the optimality of linking social insurance to marriage.

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Contents

1 Introduction

A major function of governments in many developed countries is to provide social insurance. The two largest social insurance programs in the United States, Social Security and Medicare, together account for more than 30 percent of federal spending. It is well recognized that the provision of social insurance to protect against adverse income or health shocks distorts markets that offer private insurance against such shocks.

I instead focus on responses in the marriage market. I do this because social insurance often represents a *twofold* intervention, both into private insurance markets and into the marriage market. This occurs whenever marital status influences eligibility for social insurance. In the United States, for example, both Social Security and Medicare fit into this category. I first ask how a link between social insurance and marriage affects the marriage market, and then discuss the implications of my findings for when it is optimal to separate social insurance from marriage.

Specifically, in the context of Sweden, I study a particular type of social insurance, *survivors insurance*. Survivors insurance replaces part of the income that a household loses upon the death of one household member. Like many countries do today, Sweden used to provide survivors insurance through the marriage contract. A widow was granted a lifetime annuity of survivors benefits upon her husband's death, but cohabiting partners or divorcees were not eligible. The value of this insurance was significant; beneficiaries' average annual payout was $5000, for an average duration of eight years (Section 2 provides details). But to most couples, who entered marriage in their 20s or 30s, the insurance was not likely to payout until far in the future. Marriage market responses to survivors insurance thus necessitate that couples have a long financial-planning horizon. I ask how this twofold intervention, into annuities/life insurance markets and into the marriage market, affected the *volume* and *nature* of private contracting in the marriage market in Sweden. The volume of contracting is determined by entry into and exit from marriage. The nature of private contracts has a range of dimensions, of which I study two: Who contracts with – that is, marries – whom? And, which contracting party gets what share of joint realized gains?

In my empirical analysis, I exploit a 1989 reform that changed how survivors insurance was tied to the marriage contract in Sweden. The reform essentially eliminated survivors insurance, replacing the promise of a lifetime annuity with a promise of one (small) "adjustment transfer." Thus, the old marriage contract, which came with a government-provided annuity that was expected to pay out in old age, was replaced with a new marriage contract that came without it, but that otherwise was identical.

Section 3 theoretically analyzes how this affects the marriage market when individuals are forward-looking. My framework captures typical relationship dynamics. Specifically, single individuals form couples; then, in each period, each couple gets new information about (shocks to) its match-specific marital surplus and updates its marital decision. Formally, I introduce matching into a collective household framework and model match quality as a stochastic process. In this framework, the impact of a change in survivors insurance tied to marriage depends on whether an individual, *at the time of the reform's announcement*, is married, matched but yet unmarried, or single (to be matched in the future). For each group, the theory delivers precise, testable predictions about initial responses and about future behavior conditional on these initial responses. I test these using individual-level marital and tax records, described in Section 4.

My first set of results, presented in Section 5, concerns how tying social insurance to marriage affects entry into marriage. Couples with a joint child who were not yet married at the reform's announcement in June 1988 were allowed to take up survivors insurance by marrying by December 31, 1989. These couples thus faced a "time notch," where marital surplus fell discontinuously by the expected discounted present value of the annuity. By analyzing bunching in the distribution of new marriages, I study how selection into marriage responded to a demand for survivors insurance. The distribution displays substantial bunching: in response to the loss in expected marital surplus of, on average, $4375 at the notch, about 46000 marriages take place in the last quarter of 1989. I estimate that a couple is on average 17 times more likely to marry in this quarter relative to the counterfactual scenario. This translates into an elasticity of marriage take-up with respect to financial incentives that exceeds existing estimates from other contexts (see, e.g., Whittington and Alm (2005)). Importantly, these responses demonstrate a high degree of forward-looking behavior on the part of Swedish couples, and imply financial-planning horizons as long as 50 years.

When match quality is stochastic, the theory suggests that the marriage boom should consist of both "retimed" marriages, that would have eventually taken place even in the absence of reform, and "extra" marriages that would never have occurred if the old marriage contract had remained available. Intuitively, announcing a replacement of the existing marriage contract with a less desirable one attaches option value to marrying into the desirable contract. Couples then rush to marriage even though they, in the absence of reform, would wait to see if the relationship improves. To test whether extra marriages arose in response to the reform, I develop a novel empirical framework that allows me to decompose the excess mass in the bunching region into responses along these two margins. I find that extra marriages accounted for more than a quarter of the response. Inherent in the theoretical option value explanation for this is a prediction that marriages in the boom are of a lower average match quality. Indeed, I show that such marriages are four percentage points more likely to end within 15 years than other marriages with the *same* contract. Nevertheless, a sizable portion of extra marriages survives in the long run.

Next, I explore response heterogeneity. Consistent with the theoretical prediction that couples in which the male is more likely to die should exhibit stronger responses, I document larger increases in take-up of marriage – and thus, of survivors insurance – among couples for which the husband's *ex post* mortality is higher. This remains true when controlling for a range of observable factors that determine the government's expected cost of extending survivors insurance to a given couple, including the husband's age at marriage. The positive correlation between couples' take-up of insurance through marriage and couples' expected cost of coverage suggests the possible presence of asymmetric information, which may partly explain why private annuities markets are underdeveloped in Sweden.[1] Further, I find that the reform's impact on take-up of marriage is positive but several times *lower* in a sample of couples in which one spouse reveals a same-sex preference years later, by entering a same-sex union after they were legalized in 1995. If sexual preferences exhibit positive autocorrelation, this is consistent with the theoretical prediction that responses would be weaker in couples with weaker *ex ante* beliefs of staying together for life – the only circumstance in which the annuity would pay out.

One candidate interpretation of these large responses in entry into marriage is that they merely represent relabeling, as many responding couples previously cohabit. But at the time of the reform, entry into marriage has major legal implications that cannot be replicated by cohabiting couples though private contracting – concerning inheritance rights, custodial rights of children, and the division of assets in case of separation. Thus, converting a cohabiting union into marriage has far-reaching real economic implications. Nonetheless, the next sections of the paper present further evidence of real responses along several other margins, including exit from marriage and intra-household bargaining.

In Section 6, I analyze the causal impact of (losing) survivors insurance on exit from marriage. I exploit the fact that, for some couples that were already married at the reform's announcement, grandfathering rules induced variation in insurance tied to marriage. Specifically, couples that married before January 1, 1985 were allowed to keep the contract they married into; for most couples that married thereafter, this contract was revoked and replaced with the new one. This change was announced in June 1988 – three and a half years after entry into marriage, rendering impossible any manipulation in response to a demand for survivors insurance. Using a regression discontinuity design that exploits both the eligibility cutoff and the timing of the reform's announcement, I show that the removal of survivors insurance from preexisting marriage contracts raised the long-run divorce rate among these couples by five percentage points.

Next, I analyze how tying social insurance to marriage affects the nature of contracting in the marriage market; specifically, the division of joint marital gains, and matching. I begin by studying couples' division of surplus. Among couples that married into the old contract but

[1] This argument builds on the literature on adverse selection in private insurance markets. Akerlof (1970) and Rothschild and Stiglitz (1976) develop the theoretical argument. Chiappori and Salanié (2000) begin to examine its key theoretical prediction, a positive correlation between demand and risk type. The empirical evidence, which I discuss in Section 5.2, is mixed. The novelty in my paper is that I focus not on a product offered in a private insurance market, but on a government-provided scheme that is supplied indirectly through the marriage contract.

then lost insurance coverage, the theory predicts a renegotiation of marital surplus in favor of the wife in surviving unions. Intuitively, the wife's expected utility from marriage is the sum of her utilities in the states of the world where her husband is alive and where he is dead. The reform reduces her utility in the latter state; as compensation for this loss, her share of surplus in the former increases. I test this empirically by studying spousal labor supply, using a regression discontinuity design similar to the one described above. My results suggest that revocation of survivors insurance delayed elderly husbands' retirement – even though it affected household wealth only in a state of the world in which the husband would not be alive. This suggests that, while the reform induced a statutory loss solely on the wife, intra-household bargaining resulted in the economic incidence partly being borne by the husband, who gave up utility – that is, leisure – when he was alive.

Finally, Section 7 analyzes long-term impacts on matching behavior. Because the annuity replaced household income that was lost due to the husband's death, payments were higher in couples with more spousal specialization in market and non-market work. Survivors insurance thus constituted a public subsidy on matches with highly unequal earnings (capacities). I theoretically show that under a standard supermodularity assumption on marital surplus, assortative matching arises in the absence of survivors insurance; however, when survivors benefits favor household specialization, assortativeness may break down. In a nutshell, removing survivors insurance should induce a larger share of skilled men to match with skilled women. To test this, I study the density of the share of highly skilled men that marry a woman of lower skill. I show that the share of newly married couples that are assortatively matched on education increases by four percentage points following the introduction of the new marriage contract. This suggests that the old survivors insurance program promoted spousal specialization in market and non-market work.

These findings establish that tying social insurance to marriage has economically important impacts across various margins of behavior in the marriage market. In Section 8, I formally examine the implications of these findings for a social planner who wishes to alleviate old age poverty. In the presence of marriage market responses, the social planner faces a trade-off between, on the one hand, protecting women who do not participate in the labor market against poverty at the end of life, and distorting marriage market behavior, on the other. The magnitude of the marriage market responses influences the optimality of linking social insurance to marriage.

My study builds on and contributes to several strands of the literature. The existing evidence on how marital behavior responds to penalties or subsidies inherent in tax and benefit schemes is mixed.[2] To the best of my knowledge, this paper is the first to take a holistic

[2] Whittington and Alm (2005) and Bitler et al. (2006) find that a smaller marriage penalty increases the rate of marriage relative to divorce or cohabitation, whereas Bitler et al. (2004) and Fitzgerald and Ribar (2005) reach the opposite conclusion. In the context of Austria, Frimmel et al. (2012) show that the removal of cash transfers upon marriage raises the marriage rate. In the context of the US and Canada, respectively, Brien et al. (1996) and Baker et al. (2004) show that individuals who collect survivors benefits delay entry into

perspective of the marriage market by simultaneously analyzing behavioral responses across all three stages of the mating process: matching, entry into marriage, and exit/bargaining.[3] Moreover, it is the first to document responses to benefits that only pay out in the far future. By virtue of these features, the paper yields three important general lessons about the economic behavior of couples.

The first lesson concerns how couples prepare for financial security in old age. An important literature studies individuals' investments into retirement accounts and other long-term savings vehicles.[4] This paper shows that marital decisions, too, are an integral and complementary part of couples' long-term financial planning strategies. Indeed, the responses that I document among young couples reveal financial-planning horizons of up to 50 years, with implied discount rates reflecting a substantial degree of forward-looking behavior. Considering a household's asset allocation alone may thus yield too gloomy a picture of its capacity to plan for financial security in retirement.

The second lesson concerns a largely open question about cohabitation, namely, whether cohabitation is a learning process in transition to marriage or an "end-state" substitute for marriage. One of the predictions that I test relies crucially on the assumption that the shocks to a couple's marital surplus are correlated over time. With this assumption, the model embodies "learning by doing" during cohabitation in the following sense: the match-specific surplus experienced during a period of cohabitation is predictive of the surplus in future periods. My model and context offer a rare test of such learning, and the results support the view that it plays a key role in cohabitation.[5]

The third lesson concerns the origins of positive assortative matching. It could result from preferences for marrying a like (Becker, 1973).[6] It could arise even if preferences are uniform over partner types, however, if search frictions matter and individuals simply are more likely to meet likes (men who attend university are more likely to meet women who attend university than women who do not, individuals of the same caste meet more often, etc.). The elimination of survivors insurance reduces the penalty for marrying assortatively, but it does not immediately alter search frictions. My results thus suggest that preferences for

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remarriage; they thus focus on elderly individuals' responses to immediate benefits. See Moffitt (1998) and Alm et al. (1999) for surveys of other earlier contributions.

[3] My results for cohabiting couples relate to Lafortune et al. (2012), who study a reform of alimony laws in Canada and find that it has distinct impacts on cohabiting and married couples. My analysis of spousal bargaining relates to the large literature that analyzes how couples' decisions depend on factors that influence each spouse's relative bargaining position. See, e.g., Angrist (2002), Wolfers (2006), Stevenson (2008), Voena (2014), and Vardardottir and Thornqvist (2014). Studies of the link between marriage and social security in the U.S., albeit with different focuses, include Casanova (2010) who studies the incentives for joint retirement, and Henriques (2012) who shows that husbands' claiming behaviors do not maximize total household benefits.

[4] See, e.g., Laibson (1997), Benartzi and Thaler (2004), Carroll et al. (2009), and Beshears et al. (2010, 2011, 2012).

[5] Learning has been proposed as a key mechanism not only driving cohabitation, but also divorce. Alternative theories attribute dissolutions to changes in the spouses' outside options (Weiss and Willis, 1997) or to failure of consumption insurance (Hess, 2004).

[6] In particular, Becker (1973) shows that matching is positively assortative if types are complements.

assortativeness play an important role in explaining the observed matching patterns. While the literature has established preferences for non-meritocratic attributes such as race, caste, and social status in matching[7], this paper breaks new ground by showing that institutional features that directly affect the economic gains from household specialization influence the degree of assortativeness. This lesson may be applicable more broadly to institutional features that encourage specialization, notably joint taxation.

In addition to these broader lessons, the paper contributes to the literature on bunching estimation strategies that has emerged from the work of Saez (2010) and Chetty et al. (2011). Like the setting analyzed by Manoli and Weber (2014), the one analyzed here represents a time-notch faced by individuals who are making a discrete choice (in my case whether to enter marriage; in theirs, whether to enter retirement in the context of Austria). The key difference is that, given that virtually all individuals do retire, all responses in the Austrian setting operate along the intertemporal margin. I also must account for extra marriages, which were induced by the elimination of survivors insurance *even though these couples never would have entered marriage in the counterfactual scenario* with continued survivors insurance. This response is distinct from a "standard" extensive margin response (a reaction to the price). Theoretically, the underlying force driving the extra marriages is the removal of the option to wait – and learn more about the quality of the match – beyond December 31, 1989. To the best of my knowledge, this paper is the first to develop a bunching methodology that captures such an extra effect in response to a permanent change in financial incentives.[8] Moreover, I face a novel challenge: the responses that I document are not limited to the proximity of the notch, but extend across the entire distribution. To address this, I develop a framework that not only exploits the shape of the density, but also a second dimension of information. More broadly, this methodology enables estimation of a counterfactual density in contexts where the region of missing mass extends far beyond the notch/kink.

2 Institutional background

2.1 Survivors insurance in Sweden

Pre-reform survivors insurance *Eligibility.* Before the reform, survivors insurance was tied to the marriage contract. A divorced woman received no survivors benefits upon the death of her former husband. A widow, in contrast, could collect survivors benefits from the date of her husband's death (or her 36th birthday) given that the husband was less than 60 years at marriage and (i) they had a joint child, or (ii) they had been married for at

[7]See, e.g. Fisman et al. (2008), Lee (2009) and Hitsch et al. (2010) on race; Dugar et al. (2012) and Banerjee et al. (2013) on caste; and Almenberg and Dreber (2009) and Abramitzky et al. (2011) on social status.

[8]Similar in spirit, the concurrent work of Best and Kleven (2014) show that a temporary tax cut in housing transaction taxes in the U.K. yield both a timing effect and an extensive margin effect on home purchases, using a diff-in-diff strategy.

least five years. Each married couple that satisfied one of these conditions was covered by survivors insurance during marriage. This scheme included the overwhelming majority of all married couples: Among couples that married in 1980, for example, 86% satisfied one of the two criteria, and were thus covered. While marriage entitled a woman to survivors benefits, no other Social Security benefits were tied to marriage. Men were not eligible for survivors insurance.

Size of annuity. Survivors insurance replaced part of the husband's earned Social Security benefit, b_h. As in the U.S., earned benefits were proportional to lifetime earnings up to a ceiling; see Appendix B for details. A widow who was between 36 and 64 years old got a (monthly) survivors benefit equal to 40% of the husband's earned benefit, $s^{wife<65} = 0.4 * b_h$. For a widow who was 65 years or older, the survivors benefit also depended on her own earned benefit, b_w. Specifically, survivors insurance guaranteed that the wife got 50% of the Social Security income that the household would have received had the husband been alive. Her benefit was given by

$$s^{wife \geq 65} = 0.5^* \left(b_h + b_w\right) - b_w = 0.5^* \left(b_h - b_w\right), \tag{1}$$

so long as this did not exceed her pre-65 benefit, $s^{wife<65}$. For widows aged 65 or above, survivors benefits thus increased with the husband's earned benefit, but decreased with her own earned benefit, and were equal to zero if the wife's earned benefit exceeded the husband's earned benefit. Put differently, (1) was increasing in the difference between the spouses' earned benefits. Widows who had little labor market participation thus relied heavily on survivors benefits.

Denoting the date of the husband's death by $t = 0$, the wife's 65th birthday by t_{65}, and her date of death by t_{death}, the annuity's value *upon realization* was thus given by

$$A = \sum_{t=0}^{t_{65}-1} \delta^t s^{wife<65} + \sum_{t_{65}}^{t_{death}} \delta^t min \left\{ s^{wife<65}, s^{wife \geq 65} \right\}, \tag{2}$$

where δ is the wife's discount rate. Given the spouses' earned benefits, (2) illustrates that A was increasing in the annuity's (expected) duration, that is, the number of years that the wife outlived her husband. In 2002, the average realized payout to survivors insurance beneficiaries was SEK 35000 (~$ 5000), and the average duration of payments was eight years. Upon realization, the value of the average annuity, applying a zero discount rate, was SEK 280000 ($40000).

Marital decisions are often made long before a spouse dies. The average age at marriage in Sweden between 1980 and 1988 was 32.94 years for men and 29.98 years for women, and the average age of entry into widowhood was 74.7. Payout was thus, on average, expected to occur several decades after marriage.

Post-reform survivors benefits The reform eliminated the gender difference in survivors benefits in a manner that drastically reduced survivors benefits for women, while increasing them modestly (from zero) for men. In particular, a surviving spouse – regardless of gender – got a *one year* "adjustment transfer," amounting to 40% of the deceased spouse's earned benefit. Thereafter, the surviving spouse received Social Security income solely based on his or her own earned benefit, just like a divorced spouse would.

Transition The social security reform was discussed for the first time in the Parliament of Sweden on June 8, 1988. While it is unlikely that the entire population obtained knowledge of the reform upon its first mention in Parliament, I take a conservative approach and treat this date as the reform announcement.[9]

Transition rule. All couples that would have been eligible for survivors insurance if the husband had died on December 31, 1989 got pre-reform survivors insurance. All other couples got post-reform survivors insurance. Henceforth I will refer to the "old marriage contract" as the contract that came with pre-reform survivors insurance and to the "new marriage contract" as the contract that came with post-reform survivors insurance (but that otherwise was identical). The eligibility rules governing pre-reform survivors benefits, together with the transition rule, meant that couples that married before the husband turned 60 obtained the old marriage contract if they (i) had a joint child together on or before December 31, 1989, and married on or before the same date; or (ii) had no joint child together on or before December 31, 1989, but married on or before December 31, 1984.[10] This transition rule implies that couples that had a joint child at the time of the reform's announcement were given an option to enter the old marriage contract by marrying on or before December 31, 1989. In contrast, childless couples that were already married at the reform's announcement but that had entered marriage after December 31, 1984, faced the prospect of having survivors insurance revoked on January 1, 1990.

2.2 Legal aspects of the marriage contract in Sweden

Other than the right to social security survivors benefits, the central legal and economic distinctions between marriage and other relationship statuses in Sweden at the time of the reform concern inheritance rights, the division of assets in case of separation, and the custody rights of children.

Inheritance. A surviving spouse has a right to inheritance, but a cohabiting partner does not. It is generally not possible for cohabiting spouses to write a testament that fully replicates

[9]Two working papers use this reform as an instrument for marriage to study the impact of marriage on child welfare (Björklund et al., 2007) and labor supply (Ginther and Sundström, 2010). These studies use 1989 as the reform year. Also see Roine (1997).

[10]Couples where the wife was born before 1945 were exempt from (ii), and remained insured after December 31, 1989, even if they entered marriage after December 31, 1984.

marriage in this regard.

Separation. In case of separation, married individuals' assets are considered marital property by default, whereas cohabiting individuals' assets are considered separate property. Moreover, the law stipulates a right to alimony payments for the economically disadvantaged spouse upon separation from marriage, but not upon separation from cohabitation. While married couples can write a prenuptial agreement specifying that all assets should be considered separate property, it is hard for cohabiting partners to replicate marriage by writing a contract stipulating that their assets should be considered joint property, or where one partner commits to making financial transfers to the other in case of separation (in particular, it is doubtable whether such a contract would be enforced by court).

Custody of children. The law presumes that a husband is the legal father of his wife's children, and spouses have joint custody by default. Outside of marriage, paternity must be established after birth, and the mother has sole custody by default. In practice, the identity of the father is established for essentially all children[11]; the key distinction thus concerns the default custodial arrangement.

Other features of the marriage contract. While the economic and legal differences between marriage and cohabitation are important at the time of reform, most taxes and benefits are largely decoupled from marriage since 1971, when joint taxation of labor income was eliminated.[12] Hospital visitation rights and (medical) proxy rights for incapacitated partners are independent of marriage.

3 Conceptual framework and hypotheses

To examine how couple formation, marital decisions, and spousal welfare depend on the link between survivors insurance and marriage, I build a model of dating, marriage, and divorce. It shows that behavioral responses to a change in survivors insurance tied to marriage depend on whether an individual is married, cohabiting, or single (to be matched in the future) when this change is announced. I derive precise, testable predictions for individuals in each relationship stage. Proofs are in Appendix C.

[11] If the parents have not notified the authorities of the identity of the father within a certain time frame of the child's birth, the social services automatically conduct a paternity investigation. Moreover, mothers are given strong financial incentives to report the identity of the father. Consistent with essentially full reporting of paternity, my data identifies the father for 96.8 percent of all children born in Sweden. Thus, while critical in the U.S. (Rossin-Slater, 2014), paternity establishment is not an issue in the Swedish context.

[12] Naturally, some changes are implemented with respect to inheritance rules and the precise benefit structure tied to marriage during the approximately 40 years that I analyze, 1971 to 2009. In the empirical analyses, however, I always compare groups of couples whose marriage contracts differ only with respect to whether marriage comes with survivors insurance, but where all other features of the marriage contracts are similar.

3.1 Model

Consider a continuum of unmarried men of measure one and a continuum of unmarried women of measure one. The population plays a three-stage game. In Stage 1, singles match into heterosexual couples. Each man and woman is endowed with a characteristic that I call skill (e.g., educational attainment or IQ) and assume to be positively related with income. The skills of women, τ_w, are distributed according to some distribution W on $[0,1]$, and the skills of men, τ_m, according to some distribution M on $[0,1]$. A match (τ_w, τ_m) derives a deterministic surplus $V(\tau_w, \tau_m)$ in each period of marriage, where $V(\tau_w, \tau_m)$ is continuously differentiable and supermodular. This means intuitively that, all else equal, a highly skilled man places a higher value on marrying a highly skilled woman than does a lower skilled man, and vice versa. In Stage 1, couples do not make marital decisions; they only match into couples who "start dating," by proceeding to stage 2 together.

In the beginning of Stage 2, each couple that matched in Stage 1 experiences a stochastic marital surplus shock, $\tilde{\theta}_2$, drawn from some distribution F on $(-\infty, +\infty)$. After observing this shock, the couple decides whether to marry or continue dating (wait, cohabit, etc.). Marriage entails a total marital surplus $S(\tau_w, \tau_m, \tilde{\theta}_2) = V(\tau_w, \tau_m) + \tilde{\theta}_2$ in Stage 2; if they instead continue dating, they obtain exogenous per-period utilities $\underline{u}_w$, and $\underline{u}_m$, which are normalized to zero. That is, $S(\tau_w, \tau_m, \tilde{\theta}_2)$ represents the value of marriage over and above cohabitation. All couples then proceed to Stage 3.

In the beginning of Stage 3, a new marital surplus shock, $\tilde{\theta}_3$, is drawn from some distribution G_{θ_2} on $(-\infty, +\infty)$ that is conditional on the realization of the marital surplus shock in Stage 2, θ_2. Upon observing this shock, married couples decide whether to divorce or stay married, and dating couples again decide whether to marry or not. Marriage entails the total marital surplus $S(\tau_w, \tau_m, \tilde{\theta}_3) = V(\tau_w, \tau_m) + \tilde{\theta}_3$ in Stage 3; if they instead divorce or remain unmarried, they obtain their exogenous utilities in Stage 3. The shocks $\tilde{\theta}_2$ and $\tilde{\theta}_3$ are non-negatively correlated: couples that are "happy" in Stage 2 are not less likely to be "happy" in Stage 3 as well. Specifically, I assume that G_{θ_2} weakly first-order stochastically dominates $G_{\theta'_2}$ if $\theta_2 > \theta'_2$: $G_{\theta_2}(x) \leq G_{\theta'_2}(x)$ for all x if $\theta_2 > \theta'_2$. Let G denote the *unconditional* (from the perspective of Stage 1) distribution of $\tilde{\theta}_3$.

Because the distribution of $\tilde{\theta}_3$ depends on θ_2, θ_2 conveys information to the couple about its expected total marital surplus in period 3. If $\tilde{\theta}_2$ and $\tilde{\theta}_3$ were uncorrelated, however, then a couple's belief in Stage 2 about marital surplus in Stage 3 would remain unaffected by θ_2. Because I allow the shocks to be correlated, this model thus embodies "learning by doing" in the following sense: the process of cohabitation generates extra information to the couple, above and beyond the information that the couple had upon matching.[13]

[13]Such "learning by cohabitation" would also occur in an alternative model where couples, instead of experiencing changes in actual marital surplus, have some underlying fundamental match quality that they get noisy signals about during cohabitation (marriage). In a sample of married couples in the U.S., Marinescu (2014) finds evidence supporting the assumption that couples experience shocks to actual marital surplus.

So far, Stage 2 is isomorphic to Stage 3: Each couple experiences either a good or bad period, can alter its marital status in response to this, and then gets payoffs that depend on the marital decision. To analyze the impact of government-provided old-age support, I further assume that the man dies with probability p after the marital decision in Stage 3. If the couple is married at the time of his death, the government may transfer an annuity to the wife that renders her utility $U_A(\tau_w, \tau_m)$. Social insurance is thus tied to marriage. Further, the dependence of U_A on (τ_w, τ_m) captures that the value of the annuity varies depending on the match.

Utility is transferable and the spouses' interaction is efficient (Shapley and Shubik, 1971). I assume that the intertemporal allocation of utility in a match between a man and a woman is contracted upon in Stage 1. The man and the woman do not renegotiate this contract unless one of them credibly threatens to divorce the other. If renegotiation does occur, I assume that it results in the minimal change needed for a marriage to continue, provided that divorce is inefficient.

3.2 Solution

I use backward induction. First, consider a couple in Stage 3 with the deterministic marital surplus $V(\tau_w, \tau_m)$ and realizations θ_2 and θ_3 of the marital shocks $\tilde{\theta}_2$ and $\tilde{\theta}_3$. If the couple is married upon entry into Stage 3, the spouses remain married if and only if

$$(1-p)\, S(\tau_w, \tau_m, \theta_3) + pU_A(\tau_w, \tau_m) \geq 0. \tag{3}$$

Intuitively, the expected surplus from marriage in Stage 3 – a weighted sum of the joint surplus from marriage when the husband is alive, $S(\tau_w, \tau_m, \theta_3)$, and the wife's utility from the social security benefits when he is dead, $U_A(\tau_w, \tau_m)$ – must exceed the sum of their expected outside utilities. If the couple is unmarried at the start of Stage 3, their decision problem is identical: they choose to marry if and only if (3) is satisfied. Rearranging yields that the couple chooses marriage if and only if $\theta_3 \geq \theta_{SB}(\tau_w, \tau_m) \equiv -V(\tau_w, \tau_m) - \frac{p}{1-p} U_A(\tau_w, \tau_m)$, where SB indicates the presence of survivors benefits. Couples that are sufficiently happy – get a sufficiently high shock – in Stage 3 choose to be married; otherwise, couples that enter the period married choose to divorce, and couples that enter unmarried choose not to marry. A couple's payoffs in Stage 3 are thus independent of the marital decision in Stage 2. The probability of being married in stage 3 conditional on the realization of the marital surplus shock in stage 2, θ_2, is $1 - G_{\theta_2}[\theta_{SB}(\tau_w, \tau_m)]$. I also define the unconditional probability of being married in stage 3, (from the perspective of stage 1) as $\beta(\tau_w, \tau_m) = 1 - G[\theta_{SB}(\tau_w, \tau_m)]$.[14]

Consider Stage 2. The couple chooses to marry if and only if the following condition is

[14] When the shocks are correlated across periods 2 and 3, the marital decisions are correlated as well. However, it remains true that the marital *decision* in period 2 per se has no causal influence on the payoffs in period 3.

satisfied:

$$S(\tau_w, \tau_m, \theta_2) \geq 0 \;\Leftrightarrow\; \theta_2 \geq \theta_{NSB}(\tau_w, \tau_m) \equiv -V(\tau_w, \tau_m), \tag{4}$$

where NSB indicates "non-presence" of survivors benefits. The unconditional probability of being married in stage 2 is $\alpha(\tau_w, \tau_m) = 1 - F[\theta_{NSB}(\tau_w, \tau_m)]$.

Consider Stage 1, where individuals match, that is, form couples that start dating. By the law of iterated expectations, the expected value, in Stage 1, of a match with $V(\tau_w, \tau_m)$ can be written as $M(\tau_w, \tau_m) = \delta A(\tau_w, \tau_m) + \delta^2 B(\tau_w, \tau_m)$ where

$$A(\tau_w, \tau_m) = \alpha(\tau_w, \tau_m) E\left[S(\tau_w, \tau_m, \tilde{\theta}_2) \middle| \tilde{\theta}_2 \geq \theta_{NSB}(\tau_w, \tau_m) \right]$$
$$B(\tau_w, \tau_m) = \beta(\tau_w, \tau_m) \left[(1-p) E\left[S(\tau_w, \tau_m, \tilde{\theta}_3) \middle| \tilde{\theta}_3 \geq \theta_{SB}(\tau_w, \tau_m) \right] + pU_A(\tau_w, \tau_m) \right]$$

and δ is the time discount factor.

Given transferable utility, the total expected surplus from a given match, $M(\tau_w, \tau_m)$, can be distributed between w and m. I search for a solution to the matching problem. Specifically, I search for a stable match as well as the (endogenous) utilities of all men and all women at the stable match(es). Denoting the utility of a woman and a man by $u(\tau_w)$ and $v(\tau_m)$, respectively, a match is stable if, for any $(\tau_w, \tau_m) \in W \times M$, the following two conditions are met: (i) $u(\tau_w) + v(\tau_m) \geq M(\tau_w, \tau_m)$ and (ii) $u(\tau_w) \geq 0$ and $v(\tau_m) \geq 0$. In words, (i) implies that no two individuals who are not matched with each other prefer to instead be matched with each other, and (ii) says that no matched individual would be better off unmatched.

Lemma 1. *A stable match exists, at which the partners' utilities satisfy* $u(\tau_w) + v(\tau_m) = M(\tau_w, \tau_m)$.

3.3 Impact of a change in survivors insurance tied to marriage

The reform affected couples at different relationship stages. Some couples had formed but not yet married at the reform's announcement, and were allowed to take up survivors insurance by marrying within a limited time period. Others were already married when the reform was announced. Finally, many couples were not yet formed. To derive testable predictions for the reform's impact on individuals in each relationship stage, I analyze the impact of an *unexpected* reform announcement at each of the three stages of the game.

3.3.1 Unmatched and unmarried individuals (Reform announced in Stage 1)

First consider individuals that were **u**nmatched and **u**nmarried (UU) at the reform's announcement.

Prediction UU1: Assortativeness of matching. Elimination of survivors insurance from the marriage contract induces a larger share of highly skilled men to match with highly skilled women.

In the absence of survivors benefits, the match that maximizes joint marital surplus is characterized by assortativeness: high-skilled men match with high-skilled women. In the presence of a government-provided annuity to widows that is higher for couples in which the husband earns more than the wife, however, assortative matching may fail. Intuitively, such an annuity *de facto* constitutes a subsidy to unassortatively matched couples in which the husband is of high skill. If the additional surplus from the subsidy more than outweighs the premium a skilled man puts on matching with a skilled woman, some high-skilled men prefer to match with less skilled women, and assortativeness breaks down.

3.3.2 Matched but unmarried couples (Reform announced in Stage 2)

Second consider couples that were **m**atched but **u**nmarried (MU) at the reform's announcement, and that could take up survivors benefits by marrying within a limited time period, that is, within Stage 2.

Prediction MU1: Retimed and extra marriages. The reform induces a marriage boom. This comprises "retimed" marriages and, given that match quality is stochastic, "extra" marriages that would never have occurred if the old marriage contract had remained available.

The mechanism driving the existence of a marriage boom is as follows. In Stage 2, unmarried couples that loose the survivors insurance if they wait to marry until Stage 3 choose to marry if and only if

$$V(\tau_m, \tau_w) + \theta_2 + \delta B_{\theta_2}(\tau_w, \tau_m) \geq \delta(1-p) A_{\theta_2}(\tau_w, \tau_m),$$

where $B_{\theta_2}(\tau_w, \tau_m)$ is isomorphic to $B(\tau_w, \tau_m)$ and $A_{\theta_2}(\tau_w, \tau_m)$ is isomorphic to $A(\tau_w, \tau_m)$, respectively, except that the expectations are formed over G_{θ_2}. This can be written as $\theta_2 \geq -V(\tau_m, \tau_w) - \Omega_A(\tau_m, \tau_w, \theta_2, p)$, where $\Omega_A(\tau_m, \tau_w, \theta_2, p) > 0$ represents the value of the option on a claim to survivors benefits in Stage 3. A comparison with (4) shows that the threshold for marriage in Stage 2 decreases and is no longer independent of the marital decision in Stage 3. Thus, selection into marriage increases.

In Appendix C, I formally show that this increase can be decomposed into retimed and extra marriages. Intuitively, couples that marry *because* of this option have a lower $V(\tau_m, \tau_w)$ or θ_2 than couples that marry in Stage 2 also in the absence of reform. While their $V(\tau_m, \tau_w)$ or θ_2 are too low to warrant a marriage in Stage 2 without the option, they are not too low with the option, because their likelihood of marrying in the future warrants keeping the option alive. Of these couples, some would have married eventually, after observing a sufficiently high θ_3, even in the absence of reform. Others, however, would never have married because their θ_3 would have turned out too low. *Ex post*, these marriages thus turn out to be "extra." Because the marriage boom would consist of only retimed marriages in the absence of uncertainty about θ_3, testing for existence of extra marriages also offers a test of the assumption that match quality is stochastic.

Prediction MU2: Heterogeneous responses and economic incentives. The marriage boom's magnitude reflects the value of $\Omega_A(\tau_m, \tau_w, \theta_2, p)$ in the population of unmarried couples. Responses thus increase with the annuity's value at payout, $U_A(\tau_w, \tau_m)$. In Appendix C I also show that the option value Ω_A increases with p, the husband's likelihood of death, which affects the likelihood of payout.

Prediction MU3: Heterogeneous responses and expectations of lifelong commitment. Responses increase with θ_2. Intuitively, a higher happiness signal imply a higher expected happiness in the future, and thus a lower probability of divorce, which indirectly raises the likelihood of payout. Importantly, this prediction relies on the assumption that match quality shocks are positively correlated over time. Testing this prediction thus offers a test of this assumption, which is inherent in any theory of cohabitation as learning.

Prediction MU4: Long-run divorce rate in marriage-boom marriages. Finally, couples that marry in the grace period have a higher future divorce rate. This is because couples that marry because of the reform have lower $V(\tau_m, \tau_w)$ or lower θ_2 – which imply a higher threshold $\theta_3(\tau_m, \tau_w)$ or a lower expected shock θ_3.

3.3.3 Matched and married couples (Reform announced in Stage 3)

Third, consider couples that were already **m**atched and **m**arried (MM) when the reform was announced, and that faced an *ex post* elimination of survivors insurance.

Prediction MM1: Marital instability. When the insurance provision is decoupled from marriage, a married couple's marital surplus falls. This induces some married couples to divorce.

Prediction MM2: Division of marital surplus. I show in Appendix C that in the marriages that survive, the wife's share of marital surplus (weakly) increases. This is because her expected utility from marriage is a weighted sum of her utility when her husband is alive and her utility when he is dead. The reform reduces her utility in the latter case. If this loss violates her participation constraint under the existing sharing rule in the household, her share of household utility, and hence her utility while the husband is alive, must increase for the marriage to continue, if that is indeed optimal. While the statutory loss induced by the reform is borne by the wife, intra-household bargaining results in the economic loss partly being borne by the husband.

4 Data

I merge administrative data from various registers compiled by Statistics Sweden. For the universe of individuals that entered marriage between 1968 and 2009, I observe the complete marital history, month and year of birth, education level, immigration status, exact death date, and taxable labor income for the years 1985-2009. For each child born in Sweden since 1971, I observe the exact birth date and the mother and father ID.[15] For couples that have joint children, I link partners (spouses) using the joint child ID. Finally, for each male born between 1951 and 1988, I obtain the cognitive ability score from military enlistment.[16]

5 Survivors insurance and selection into marriage

Couples that were both married and had a joint child on or before December 31, 1989 were entitled to survivors insurance. Here I analyze the impact of (removal of) survivors insurance on their marriage decision.

Sample and descriptive statistics My sample includes all couples that had a joint child before January 1, 1989 (and from January 1, 1971, when the child data starts).[17] For these couples, survivors insurance eligibility was determined by the marriage decision alone. Those who married before January 1, 1990, obtained survivors insurance; those who married thereafter did not.

Many of the couples in this sample had already married by June 1988, and consequently the reform did not alter their incentives to enter marriage. Intuitively, my empirical analysis exploits this fact by, in various ways, contrasting the marital behaviors of couples that married before and after the reform, starting one decade before the eligibility threshold. Table 1 displays summary statistics.

Graphical evidence Figure 1 plots the empirical distribution of new marriages at a quarterly frequency. This raw data exhibits a seasonal pattern, with more marriages in the spring and summer. At the eligibility threshold for survivors insurance, indicated by the red dashed line, there is a marriage boom. Even though the reform was announced in the second quarter of 1988, indicated by the gray dashed line, the excess mass is concentrated at the eligibility threshold.[18] After the threshold, the distribution displays missing mass.

[15] For children born in Sweden, I can identify the mother (father) in 100 (98) percent of the cases.

[16] The test consists of four subtests: logical ability, verbal ability, technological comprehension and metal folding. For each part $p \in \{1, 4\}$, each individual i is given a score $s_{pi} \in \{0, 9\}$. Following Lindqvist and Vestman (2011), I sum the scores from each part, percentile rank the sum within enlistment year, and transform these scores using the inverse normal function. The resulting variable q_i is normally distributed with a mean of zero and a standard deviation of one.

[17] Live children born on January 1, 1989, were likely conceived before the reform announcement in June 1988.

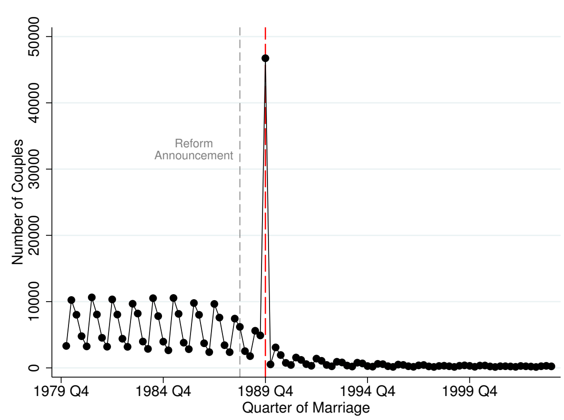
[18] This is consistent with the theoretical framework because waiting to marry is costly only when it entails giving up the option to get the old marriage contract, that is, in the last period. Another potential reason for this seemingly late response may be that information about the reform was not widespread in June 1988, which is the date when it was first discussed in Parliament. Appendix Figure A1 shows that media reporting about the reform was heavily concentrated in the last three months of 1989, suggesting that it then may have been more salient.

Table 1: Summary statistics for matched but not married sample

	Main Sample	Old Marriage Contract	
Year of marriage	1980-2003	1980-1988	1989
Demographic characteristics at marriage			
H age at marriage	31.49	29.82	35.12
W age at marriage	28.72	27.05	32.24
H education (4 lvls)	2.08	2.13	1.97
W education (4 lvls)	2.10	2.13	2.02
H cognitive capacity	0.03	0.09	-0.13
H marriage number	1.12	1.12	1.11
W marriage number	1.11	1.11	1.09
H is immigrant	0.07	0.08	0.05
W is immigrant	0.08	0.08	0.04
Economic characteristics at marriage			
Total log household labor income (H and W)	10.26	10.09	12.08
H income share	0.66	0.66	0.70
Fertility behavior			
Couple's completed fertility	2.26	2.26	2.22
First child out of wedlock	0.66	0.52	1.00
Number of couples	306823	220112	59012

Note: The sample includes all couples that had a joint child between January 1, 1971 and December 31, 1988, that married between 1980 and 2003. By marrying before January 1, 1990, these couples opted into the old marriage contract, to which survivors benefits were tied.

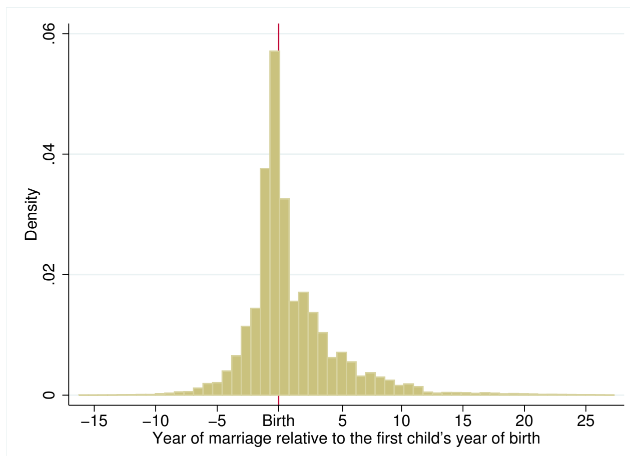
Figure 1: Empirical distribution of new marriages



Note: The sample includes all couples that had a joint child between January 1, 1971 and January 1, 1989. The black connected line depicts the number of couples entering marriage (i.e., the number of marriages) at a quarterly frequency, from 1980 to 2003. The red dashed vertical line indicates the last quarter during which marriage entailed survivors insurance. The grey dashed vertical line indicates the quarter of reform announcement.

The boom is accounted for by couples that had already conceived their first child and who were given an opportunity to take up survivors insurance by marrying before the eligibility threshold. In Sweden, childbirth often precedes marriage; in fact, around half of the ever married couples with children enter marriage after the birth of the first joint child: Figure 2 plots the distribution of marriages relative to the date of birth of the couple's first joint child and illustrates that entry into marriage is concentrated around this date, with approximately half of the density thereafter.

Figure 2: Distribution of marriages relative to the birth of the first joint child



Note: The figure plots the density of year of marriage relative to the first child's year of birth. Marriage is concentrated around the year of birth of the first joint child. The sample includes all couples that had their first joint child between January 1, 1976 and January 1, 1989. I observe marital behavior from 1969. For couples that had their first joint child in January 1976, I thus observe marital histories for seven years before childbirth. Restricting the sample to couples that had their first joint child from 1980 yields a similar graph; this is because the probability of marriage more than seven years before childbirth is close to zero.

5.1 Prediction MU1: Retimed and extra marriages

In Figure 1, the marriage boom, coupled with the missing mass after the eligibility threshold, suggests that couples that would have otherwise married after the threshold retimed their entries into marriage. This is a response along the intertemporal margin. The theoretical framework suggests that, in the presence of uncertainty about future match quality, the reform also generates extra marriages. These can be thought of as an extensive margin response, although it is important to note that this effect is distinct from a "standard" reaction to the price: Theoretically, the underlying force driving the extra marriages is that the option to wait, and hence to learn more about the quality of the match, is removed on December 31, 1989.

Testing for the presence of extra marriages requires (i) estimation of a counterfactual that approximates the density in a scenario without the reform; and (ii) estimation of the reduction in entry into marriage that occurred after 1990 precisely because the reform reduced the benefit from marriage.[19] In Appendix D.1 I use my empirical framework (presented below) to simultaneously estimate (i) and (ii) and find that (ii) is negligible; here I therefore focus on (i).

Conceptually, I test for the presence of extra marriages by comparing the extent of bunching at the notch with the missing mass thereafter. If this bunching exceeds the missing mass, then the difference constitutes extra marriages.[20] I can estimate the extent of bunching at the notch by constructing a counterfactual distribution based on data to the left of the threshold. To estimate the missing mass, however, I face a novel challenge: the density in Figure 1 entirely "vanishes" after the eligibility threshold. Because the threshold represents a notch in marital surplus, marrying shortly after the threshold should indeed be strictly dominated; however, my setting is particular in that the density never "resumes" a shape similar to its shape before the announcement. This suggests that I cannot rely on data to the right of the threshold to construct my counterfactual density.

For this reason, I start by using only pre-reform data, including only marriages that were entered before the first quarter of 1990. Intuitively, this treats the entire post-reform period as one large omitted region. On this sample, I start by estimating the following regression, in the spirit of Saez (2010), Chetty et al. (2011), and Manoli and Weber (2014):

$$N_s = \alpha + \beta \left(\mathbf{1} \left[s = s^* \right] \right) + g(s) + \zeta_q + \varepsilon_s, \tag{5}$$

where N_s is the number of marriages in quarter s; $\mathbf{1}\left[s = s^*\right]$ is an indicator variable that takes the value of one at the eligibility threshold, $s = s^* = 1989q4$; the function $g(s)$ is a higher order polynomial; and ζ_q are quarter fixed effects. Intuitively, I fit a polynomial to the counts plotted in the figure before 1990 Q1, accounting for seasonality. Because the excess mass is concentrated at s^*, β measures the size of the marriage boom, that is, the number of marriages that would not have occurred in 1989 Q4 in the absence of reform. Appendix Table A1 presents estimates of β from specifications with varying polynomial degrees. All estimates are in the range of 46000 induced marriages. The null hypothesis that there is no excess mass at the threshold relative to the counterfactual distribution (obtained by setting

[19] In the theoretical framework above, (ii) trivially arises in Stage 3. Intuitively, the loss in marital surplus that drives prediction MM1, by pushing existing marriages on the margin of divorce into divorce, also affects couples that consider entry into marriage post reform. This is akin to the standard labor force participation decision in the context of income taxes (Marx, 2012).

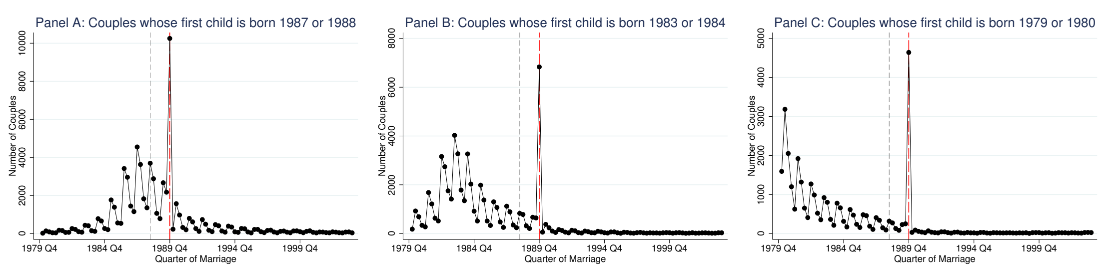
[20] Studying a notch in transfer taxes in the real estate market, Kopczuk and Munroe (2014) use a similar conceptual idea, comparing bunching at the notch with the missing mass beyond it. They find that, in that context, the missing mass exceeds the transactions at the notch.

$\mathbf{1}[s = s^*]$ equal to zero) is rejected with t-statistics that imply p-values satisfying $p < 10^{-9}$.

Appendix Figure A2 displays attempts to use the coefficients obtained in estimation of (5) to predict a counterfactual density (out of sample). The counterfactuals are sensitive to the choice of polynomial. While the functional form assumptions made here can clearly be improved upon, this exercise illustrates that I cannot solve the problem of a lack of post-reform observations by using data to the left of the threshold only, since any functional form assumption will require projection far out of sample (and be arbitrary).

Empirical framework using two dimensions of information To address this, I develop an empirical framework that (i) uses a second dimension of information to decompose my sample into subsamples, and (ii) combines this with specific assumptions about the impact of this second dimension of information on marital behavior. Specifically, I exploit the empirical fact illustrated in Figure 2, namely, that entry into marriage is concentrated around the birth of a couple's first child. To illustrate this further, Panel A of Figure 3 replicates Figure 1 for the subsample of couples whose first joint child was born in 1987 or 1988. Panels B and C, respectively, depict other "cohorts" of couples, whose firstborns were born in earlier time periods.

Figure 3: Empirical distribution of new marriages - specific couple cohorts



Note: This Figure replicates Figure 1 for subsets of the sample used in this figure. Panel A depicts new marriages among couples that had their first joint child in 1987 or 1988, the last "cohort" included in my sample. Panels B and C depict new marriages among couples that had their first joint child at earlier time intervals. The panels illustrate that entry into marriage is concentrated around the date of birth of a couple's first joint child. Note that the scales on the three y-axes differ.

The panels show that entry into marriage decreases within a year of childbirth. This, in turn, suggests that the *decrease* in the number of new marriages around the reform announcement in Figure 1 can be explained by the fact that the last cohort of couples included in my sample has their first joint child before or within six months of the reform announcement. Intuitively, my estimation strategy exploits this second dimension of information – the date of birth of a couple's first child – by using early cohorts, whose marital behavior is observable for a longer period of time pre-reform, to help predict how the marital behavior of late cohorts would have evolved in the absence of reform. For the earliest cohort included in my sample, I observe 19 years of post-childbirth, pre-reform marital behavior. Importantly, the distribution of first births itself is predetermined in the sample, as I include only couples whose first joint child was conceived at the time of the reform announcement.

Specification. Because the sample includes couples whose first joint child was born during 72 quarters, from the first quarter of 1971 until the last quarter of 1988, I divide the sample into 72 cohorts. Each cohort $c \in \{1, 72\}$ consists of couples whose first joint child was born in a given quarter, where $c = 1$ represents the first quarter of 1971, $c = 2$ the second quarter of 1971, and so on. For each of these cohorts, I observe the marital behavior at a quarterly frequency. I estimate the following regression:

$$n_{cs} = \alpha + \eta_c + \zeta_q + \beta_c \left(\mathbf{1} \left[s = s^* \right] \right) + \gamma_c \left(\mathbf{1} \left[s > s^* \right] \right) + g(s) + h(t_{pre-birth}) + j(t_{post-birth}) + \varepsilon_{cs}, \quad (6)$$

where n_{cs} is the natural logarithm of N_{cs}, the number of marriages in quarter s in cohort c, $n_{cs} = ln(N_{cs})$. I use the natural logarithm because the distribution of new marriages exhibits nonlinearities; I show the distribution of n_{cs} in Appendix Figure A3. As before, $g(s)$ is a higher order polynomial in time (quarter), ζ_q capture seasonality. Further, η_c are cohort fixed effects. The β_c capture the cohort-specific increases in entry into marriage at the eligibility threshold, $s = s^*$, and the γ_c capture the corresponding reductions in entry after this threshold. These effects can be thought of as proportional, given that n_{cs} is the natural logarithm of the number of marriages. They are allowed to be cohort-specific because each cohort experiences the reform at different durations since birth of the first joint child, and hence have different baseline levels of entry into marriage pre-reform. The functions $h(t_{pre-birth})$ and $j(t_{post-birth})$ are higher order polynomials in the number of quarters before and after the first child's birth, respectively. Inclusion of these time trends allows me to use early cohorts, whose marital behavior is observable for a longer period of time *post-childbirth* before the reform, to predict how the trend in marital behavior of late cohorts would have evolved in the reform's absence. Intuitively, as illustrated in Appendix Figure A4, inclusion of the functions $h(t_{pre-birth})$ and $j(t_{post-birth})$ can be thought of as recentering the distributions around the birth of a couple's first joint child, and then exploiting the fact that different cohorts were "hit" by reform at different distances in time from childbirth.

Inference about the impact of the reform on take-up of marriage relies on the following identifying assumption: In the absence of reform, couples marrying at the threshold would behave like couples marrying before the threshold at the same duration since childbirth, after allowing each cohort of couples to have a separate marriage propensity (that is, after allowing for proportional, vertical shifts of each cohort's recentered distribution, to account for the declining propensity to marry over time). This is akin to a "common trends" assumption with respect to how the rate of marriage declines with distance from the date of childbirth.[21]

After estimating this regression, I obtain the predicted arithmetic cohort-specific frequencies, $\hat{N}_{cs}$.[22] To predict cohort-specific counterfactual frequencies, $\hat{K}_{cs}$, I set $\mathbf{1}[s = 1989q4]$ and $\mathbf{1}[s > 1989q4]$ equal to zero, and I then aggregate the cohort-specific frequencies into sample-wide ones by calculating $\hat{K}_s = \sum_c \hat{K}_{cs}$. I use the estimated counterfactual density to decompose the marriage boom into intensive (retimed) and extensive (extra) margin responses. Specifically, I denote by A the estimated number of induced marriages at the eligibility threshold, $A \equiv \left(N_{s^*} - \hat{K}_{s^*}\right)$, and by B the estimated sum of missing marriages post reform, $B \equiv \sum_{s>s^*} \left(\hat{K}_s - N_s\right)$. If all induced marriages represent retimed marriages, then $A = B$. If $A > B$, then the difference $(A - B)$ represents extra marriages. Thus, rather than imposing that the excess mass at the threshold equals the missing mass (Kleven and Waseem (2012); Manoli and Weber (2014)), this estimation strategy allows me to decompose the excess mass into retimed and extra marriages.

Finally, I calculate one estimate of the change in the probability of marriage at the threshold, given by $\frac{\triangle p_{s^*}}{p_{s^*}} = \frac{\left(N_{s^*} - \hat{K}_{s^*}\right)}{\hat{K}_{s^*}}$. As I discuss further below, $\frac{\triangle p_{s^*}}{p_{s^*}}$ can be thought of as the numerator of an elasticity that quantifies how marital decisions respond to financial incentives, where the denominator captures the change in marital surplus at the threshold. I calculate standard errors for each estimated statistic using a cluster bootstrapping procedure described in Appendix D.2.

Results Panel A of Appendix Figure A5 displays the empirical distribution and the arithmetic sample-wide counterfactual. Their seasonally adjusted counterparts are displayed in Panel B. In both panels, relative to the empirical distribution, the predicted frequency distribution entails "excess mass" in the last quarter of eligibility for survivors insurance, but displays a persistent "missing mass" after the marital surplus notch.

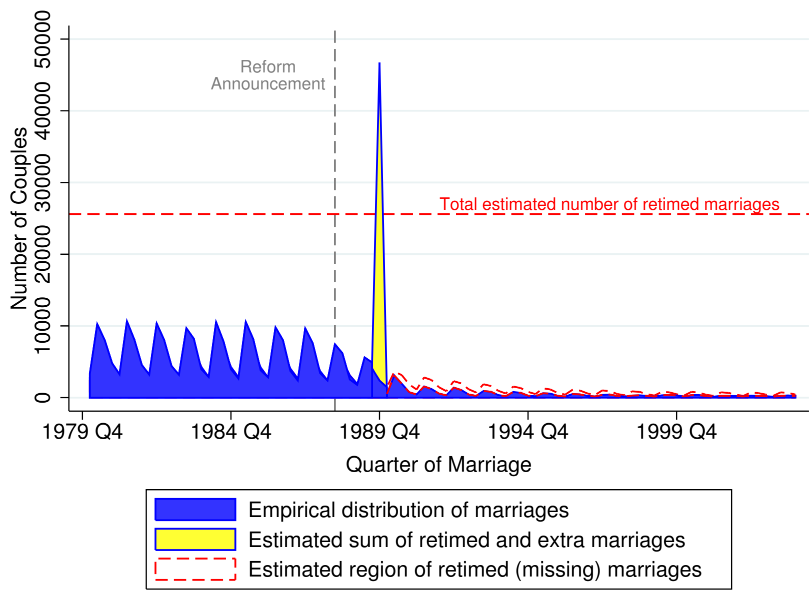
Figure 4 replicates Panel A in Figure A5, adding indications of the areas A and B. Table 2 presents the estimated sizes of these areas, using three different higher order polynomials. The yellow area in Figure 4 depicts the marriage boom induced by the reform, an estimated $A = 44305$ marriages at the eligibility threshold (using the specification that minimizes the AIC). The white area between the red dashed and blue solid lines depicts the missing marriages post reform. These retimed (missing) marriages are estimated to sum to $B = 25600$. Retiming of marriages can thus explain only parts of the extra mass in the bunching region. The

[21] To gauge the plausibility of this assumption, I have performed "placebo" tests, assuming that the reform occurred earlier, and using my framework to predict marital behavior. I am able to capture 97 percent of actual marital behavior, suggesting that the assumption is appropriate.

[22] I calculate $\hat{N}_{cs} = exp(\hat{n}_{cs} + \frac{\hat{\sigma}^2_{cs}}{2}) = exp(\hat{n}_{cs})exp\left(\frac{\hat{\sigma}^2_{cs}}{2}\right)$, where $\hat{\sigma}^2_{cs}$ is the squared standard error of the regression. This is because, if N_{cs} and n_{cs} are random variables satisfying $n_{cs} \sim N\left(\mu, \sigma^2\right)$ and $N_{cs} = exp\left(n_{cs}\right)$, then $E\left(N_{cs}\right) = exp\left(\mu + \frac{\sigma^2}{2}\right)$. Because $exp\left(\frac{\hat{\sigma}^2_{cs}}{2}\right)$ is close to one, however, similar results obtain if I let $\hat{N}_{cs} = exp(\hat{n}_{cs})$.

difference, $(A-B) = 18705$, is the estimated extra marriages that would not have materialized in the absence of reform, which accounts for 42 percent of the response.

Figure 4: "Extra" and "retimed" marriages



Note: I estimate the sum or retimed and extra marriages in 1989 Q4 to be 44 305 (the yellow area). Of these marriages, 25 600 are estimated to be retimed (the white area enclosed by red dashed lines; "intensive margin response"). The remainder, 18 705, are estimated to be "extra marriages" that, in the absence of reform, would not have been entered into.

Row 4 of Table 2 presents the estimated change in the probability of marriage at the eligibility threshold, $\frac{\triangle p_{s^*}}{p_{s^*}} = 18.25$. The excess mass at the threshold is 1825% of the counterfactual frequency in that quarter. This suggests that, on average, a couple that is unmarried at the start of the last quarter of 1989 is *18 times* more likely to marry in this quarter than it would have been in the absence of reform. The hazard model estimation presented in the next subsection, 5.2, yields comparable estimates of the average change in the probability of marriage in the last quarter of 1989 relative to this quarter in a counterfactual scenario of no reform, given that a couple had not yet married in the beginning of this quarter: my estimate of this hazard ratio is 17.04 (details are presented below). This suggests that, even though the bunching technique developed here relies on a sample of married couples alone, it adequately accounts for the changing risk set of entry into marriage over time.

Magnitudes and interpretation To interpret these estimates, it is instructive to put them in relation to the size of the incentive at the time of reform. The expected value of the annuity

Table 2: Impact on marriage

	Polynomial		
	2	3	4
Induced Marriages	44065***	44396***	44305***
	(3261)	(3238)	(3229)
Which consists of			
Retimed Marriages	23113***	21646***	25600***
	(4496)	(2748)	(3176)
Extra Marriages	20951***	22750***	18705***
	(2685)	(1931)	(1629)
Estimated Counterfactual	2668***	2337***	2428***
	(385)	(327)	(323)
$\Delta p/p$	16.51***	19.00***	18.25***
	(2.0029)	(2.1566)	(1.8862)
Number of obs (cohort quarters)	6473	6473	6473
AIC	9186	8928	8880

Note: Dependent variable: log number of marriages (by cohort*quarter). Each column represents statistics calculated from estimates from a separate regression, with a polynomial of the indicated degree. "Induced marriages" reports the estimated number of induced marriages in 1989 Q4, calculated from the estimates of cohort-specific indicator variables for 1989 Q4. The next two rows decomposes this boom into retimed and extra marriages, and the final two rows report additional statistics defined in the text. Each regression also includes cohort fixed effects, (four) quarter fixed effects, and cohort-specific increases (decreases) in entry into marriage at (after) the eligibility threshold. Standard errors obtained from cluster bootstraping procedure with 10 000 randomly drawn panels, further described in Appendix D.2, in parentheses. AIC is obtained from the regression with the entire (non-random) sample.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

in 1989 is given by the average expected annuity value at payout, multiplied by the probability of the wife being widowed (i.e., still married and still alive at the time of husband death), and discounted from the expected year of death of the husband, back to 1989. Applying an annual discount rate of 3 percent, and taking the sample expected duration until husband death of 42 years, yields an average expected value of the annuity at reform of approximately \$4575.[23]

We can interpret the ratio $\frac{\triangle p_{s*}}{p_{s*}}$ as the numerator of an elasticity that quantifies how marital decisions respond to financial incentives. In particular, let $\epsilon = \frac{\triangle p_{s*}}{p_{s*}} / \frac{\triangle S_{s*}}{S_{s*}}$ relate the change in marriage probability at the threshold to the change in marital surplus stemming from the elimination of survivors insurance. To calculate this elasticity, we not only need to know the numerator ($\frac{\triangle p_{s*}}{p_{s*}} = 18.25$) and the size of the change in marital surplus at the notch ($\triangle S_{s^*} = -\$4575$), but also the surplus from marriage relative to cohabitation, S_{s^*}. Even in the absence of an estimate of S_{s^*}, however, we can obtain a lower bound on this elasticity by assuming that elimination of survivors insurance eliminated the entire surplus. This lower bound is given by $18.25/-1 = -18.25$, which again suggests that Swedish couples' marital behavior was highly responsive to the financial incentives.

As discussed by Manoli and Weber (2014), who estimate an analogous elasticity of retirement take-up at a notch in retirement income, the elasticity ϵ essentially reflects a thought experiment that compares a situation with discontinuous marital surplus (like the one induced by the reform) to a counterfactual situation with a smooth marital surplus around the threshold. However, while all responses in retirement take-up occur along the intertemporal margin, I have showed that Swedish couples also respond by entering extra marriages. For this reason, the estimated elasticity at the notch cannot immediately be generalized to reflect behavior away from the notch. Nonetheless, even if we disregard the 42 percent of the boom that is accounted for by extra marriages, the extent of retiming still implies a high responsiveness of marital decisions to marital surplus when compared to estimates from other contexts. In a sample of couples with a similar age structure as the couples studied here, Whittington and Alm (2005) exploit tax changes to examine how the marriage penalty affects the exit margin from marriage in the context of the US. They find that a tax change that erodes 71 percent of the marriage penalty raises the likelihood of divorce by 0.4 percentage points, or by 10 percent, which translates into an elasticity of divorce with respect to the marriage penalty of -0.005.

It is also important to keep in mind that, while the elasticity ϵ captures responsiveness

[23]The average husband age at marriage of 35 among couples who marry in the boom, and an expected male life span of 77 years, translates into a 42-year discounting horizon. In the sample of couples who entered marriage in 1989, the probability of remaining married in 2013 is 0.694, and as the cumulative divorce hazard is essentially flat in this sample 24 years after marriage, I use this figure as the probability of not ever divorcing. Conditional on staying together until the end of life, I assume that the wife outlives the husband with probability 0.65, which corresponds to the ratio of widows to widowers in this sample by 2013. I use the average payout of \$5000 and the average duration of transfers of 8 years (reflecting a the average age difference and longer life span of women). I discuss the choice of discount rate further below.

to financial incentives at the notch, it does not capture responsiveness in couples' decisions to *ever* marry, since many couples that married at the notch would have married later in the absence of reform. The roughly 44 300 couples who entered marriage in the last quarter of 1989 corresponds to approximately 20 percent of *all* couples who faced an incentive to enter marriage in response to the reform, i.e., who had a joint child but who were not yet married at the start of this quarter. Among couples who had conceived their first joint child just shortly before the reform, the share entering was higher. To illustrate this, the three panels of Figure 5 depicts the empirical cumulative density function of durations until marriage, relative to the first child's quarter of birth, for different subsamples. Since Figure 2 shows that the probability of marriage more than 7 years before childbirth is essentially zero, I here define a couple to enter the risk pool for marriage 28 quarters before the birth of its first child. (The distribution of first births itself is predetermined here by definition, as the sample only includes couples whose first joint child was conceived at the time of reform.)

The three panels of Figure 5 display the "raw data," in the sense that they show Kaplan-Meier failure functions, and their 95 % confidence intervals, estimated without any covariates. In the upper panel, the sample includes all couples whose marital behavior I can observe for at least seven years before childbirth, i.e., all couples whose first child was born after 1975 (but before 1989). The graph shows that roughly 63 percent of all couples with a joint child eventually marry, with an inflection point around the date of birth of the first joint child. This upper panel displays no visible response to the elimination of survivors insurance, because different couples experience the elimination of survivors insurance at different durations from childbirth, and hence at a different point on the x-axis.

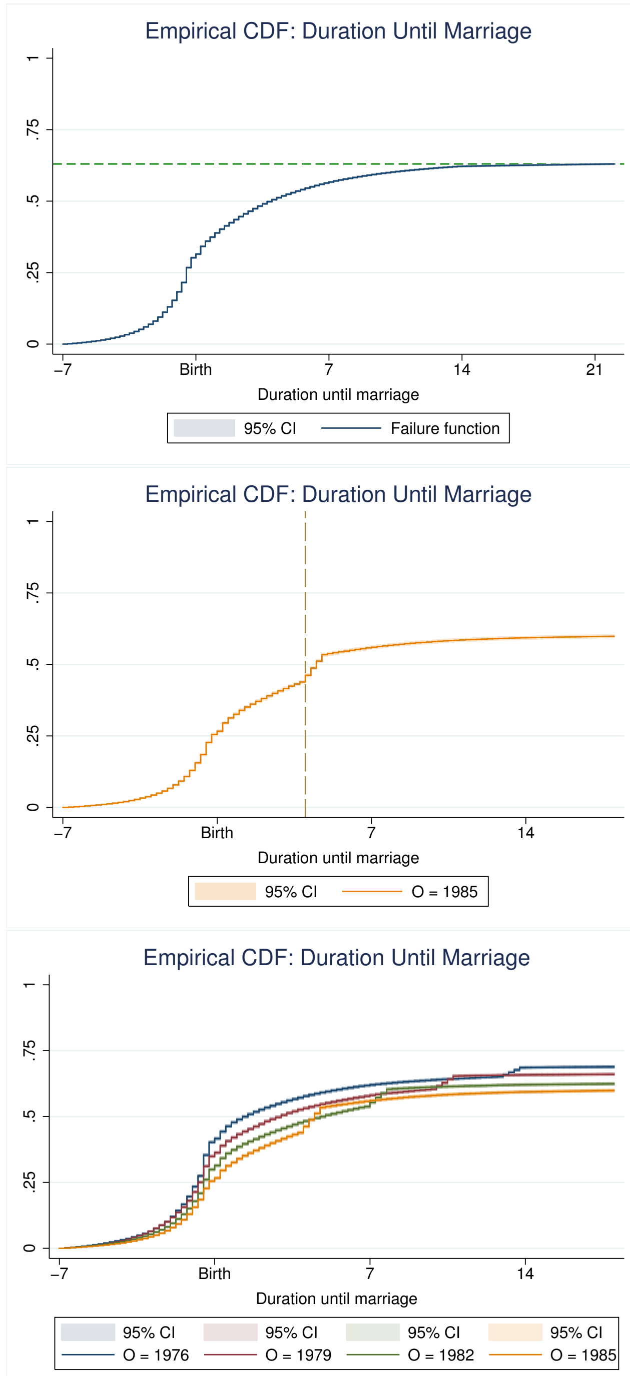
To illustrate this, in the middle panel, the sample includes only couples whose first joint child was born in 1985. Here, we see a visible "hump" in the cumulative density function at the dashed green line, which indicates the last quarter of 1989.[24] By the beginning of the last quarter of 1989, approximately 40 percent of these couples had married; at the end of this quarter, 55 percent had married. This suggests that the reform induced a percentage change in the share of ever married couples of $\frac{15}{40} = 0.375$, and hence a bound on an "ever married" elasticity of -0.375. This elasticity, too, implies a high responsiveness relative to e.g. Whittington and Alm (2005)'s estimated elasticity of -0.005.[25]

The lower panel displays separate functions for the cohorts of couples who had their first joint child in 1976, 1979, 1982, and 1985, respectively. For each cohort, the visible "hump" occurs at the elimination of survivors insurance. In the next subsection, use this hazard framework to analyze how the reform's impact on a couple's probability of marriage varies with the financial characteristics that determine the value of survivors insurance.

[24] Specifically, the dashed green line indicates the last quarter of 1989 for couples whose first joint child was born in the first quarter of 1985. Because the sample includes couples whose first joint child was born during all four quarters of 1985, the last quarter of 1989 occurs, in the Figure, in a "staggered" fashion.

[25] Whittington and Alm (2005) find that the "ever divorced" elasticity and the elasticity reflecting temporary changes in marital status are largely similar.

Figure 5: Empirical cumulative distribution function of marriages

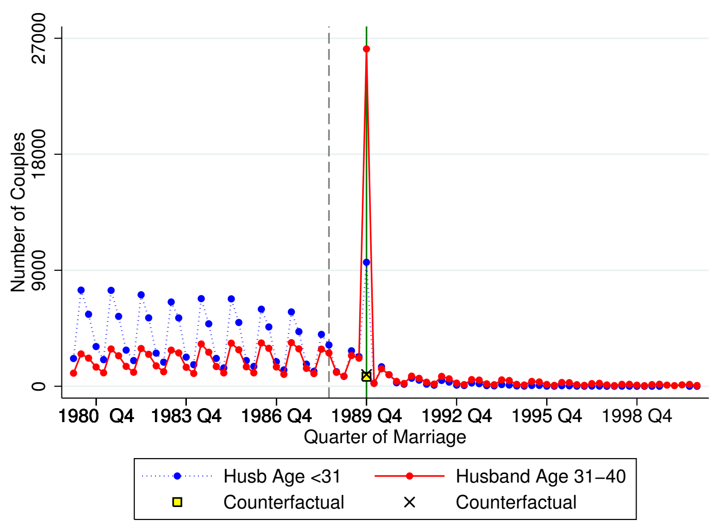


Note: The graph depicts the empirical CDF of durations until marriage relative to the first child's date of birth, with each couple entering the risk pool seven years (28 quarters) earlier. In the upper panel, the sample includes all couples that had their first joint child between January 1, 1976 and January 1, 1989; in the middle panel, it includes only the cohorts of couples who had their first joint child in 1985, and the dashed line indicates the removal of survivors insurance. The lower panel includes the cohorts of couples who had their first joint child in 1976, 1979, 1982, and 1985, respectively. For each cohort, the visible "hump" in the cdf corresponds to the timing of the elimination of survivors insurance.

5.2 Prediction MU2: Heterogeneous effects and economic incentives

By Prediction MU2, the response in entry into marriage should be larger, the larger is the annuity's expected value. One of it's determinants is the husband's likelihood of death, that is, the likelihood of payout. Because mortality increases with age, Figure 6 shows the distribution of marriages in two subsamples with different husband ages. While the baseline rate of marriage is higher among men who are younger at marriage, the increase in the last quarter of 1989 is higher among older men. This raw data thus constitutes graphical evidence of heterogeneity in responsiveness with respect to husband age.

Figure 6: Heterogenous effects w.r.t. husband age at marriage



Note: The Figure plots the distribution of new marriages at a quarterly frequency. Each point captures the number of couples that enter marriage, for each "Husband Age At Marriage" group, each quarter. The sample includes all couples who had (conceived) a joint child at the reform announcement.

Two challenges arise in interpreting these results, however. First, to isolate the impact of age, I should hold constant other couple-specific characteristics, especially other variables than age that influence the annuity's expected value at payout. Second, I show in Section 5.1 above that the reform's impact on entry into marriage is not accounted for by retiming alone. Given this, an analysis of the distribution of marriages – and hence of only married couples – introduces selection when analyzing impacts on the probability to marry. Specifically, the relevant set of couples that were "at risk" of entry into marriage in the beginning of the last quarter of 1989 not only included the couples that indeed choose to marry in this quarter, but all couples who were still unmarried at the start of this quarter (and whose first joint child was conceived at the time of reform). The theoretical framework thus corresponds to a hazard model.

Empirical framework I use the sample employed when creating Panel A of Figure 5. It includes all couples whose first joint child was born before 1989 (and not only those who ever married), with the restriction that I can observe their marital behavior for at least seven years before childbirth. Thus, it includes all couples whose first child was born after 1975 (but before 1989).[26] Of course, not all of these couples were at risk of marriage in the last quarter of 1989, as many of them had already married at that time. The framework presented here exploits this fact by contrasting the marital behavior of couples that married before, and at the time of, the reform.

I estimate an extended Cox model (Fisher and Lin, 1999), where covariates are time dependent with fixed functions of time. For couple i, I define an "ever married indicator" κ_i; the time of marriage (measured from 28 quarters before childbirth), X_i; and a covariate path, $Z_i(t), t \in [0, X_i]$, of potentially time-varying covariates while the couple is at risk for marriage. Specifically, in keeping with the formulation above, I define two time-varying covariates, $s_i^*(t)$ and $post_i(t)$, where $s_i^*(t) = 1$ when $t_c \equiv s^* - (c - 28)$, that is, in the last quarter of 1989. Each cohort of couples, defined by the quarter of birth of their first joint child, thus experiences the reform at different durations since childbirth. Similarly, I let $post_i(t) = 1$ when $t > t_c$. I assume that conditional on a couple's covariate history, the hazard for marriage at time t depends only on the value of the covariates at that time, $h(t; \mathbf{Z}_i(t)) = h_0(t) exp(\beta \mathbf{Z}_i(t))$. The baseline hazard at time t, $h_0(t)$, is left unspecified. I estimate:

$$h(t; \mathbf{Z}_i(t)) = h_0(t) exp(\beta s_i^*(t) + \gamma post_i(t) + \delta_1 \mathbf{F}_i(t) + \delta_2 \mathbf{D}_i(t)), \tag{7}$$

where $F_i(t)$ is a vector of potentially time-varying financial characteristics that influence the annuity's expected value as defined in (2): the man's labor income and share of household income, and each partner's employment status and birth year. $\mathbf{D}_i(t)$ captures other observable couple characteristics: the partners' levels of education, marriage parities, and completed fertility. In alternative specifications, I control more flexibly for the man's labor income and birth year by including indicator variables for eight income ranges l and eight birth year ranges b. Each income range is SEK 25k, with the highest range including incomes of $175k$ and above in 1988 (12% of the sample); each birth year range is four years. I refer to the vector which includes these flexible controls as $\tilde{F}_i(t)$.

The hazard rate at t is the predicted probability that couple i in cohort c marries t quarters after $(c - 28)$, given that they are unmarried until then. I calculate the ratio of these predicted

[26] As discussed above, I define a couple whose first joint child is born in quarter s to become under risk for marriage 7 years earlier, in quarter $(s - 28)$.

Table 3: Impact on marriage: estimated hazard ratios

	Financial controls		All observable controls	
	Coefficient, $\hat{\beta}$	Exponential, $e^{\hat{\beta}}$	Coefficient, $\hat{\beta}$	Exponential, $e^{\hat{\beta}}$
1989 Q4	2.83***	16.88***	2.84***	17.04***
	(0.03)	(0.52)	(0.03)	(0.55)
Flexible controls	NO	NO	NO	NO
1989 Q4	2.84***	17.05***	2.84***	17.08***
	(0.03)	(0.53)	(0.03)	(0.55)
Flexible controls	YES	YES	YES	YES
Number of couples	247194	247194	218278	218278

Note: Columns 1 and 3 report the estimated coefficient on the indicator variable for 1989 Q4, with standard errors clustered at couple cohort in parentheses. Columns 2 and 4 report the corresponding exponential. Significance levels from a test of the null hypotheses that each regression coefficient is 0 or, equivalently, that each exponential is 1. In the upper panel, all regressions include controls for financial characteristics $\mathbf{F}_i(t)$ that influence the annuity's expected value: Each partner's employment status, the man's total labor income and share of household labor income, and the partners' birth years. In columns 3 and 4, each regression also includes controls for other characteristics $\mathbf{D}_i(t)$ that I observe: The partners' education levels, their number of children, and the spouses' marriage parities. In the lower panel, all controls are similar except that I control flexibly for male labor income and birth year, including $\tilde{\mathbf{F}}_i(t)$ instead of $\mathbf{F}_i(t)$.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

probabilities for marriage in 1989 $Q4$ relative to marriage in another quarter, given by the hazard ratio of marriage in 1989 Q4, $\hat{h}_{1989\ Q4} = exp\left(\hat{\beta}\right)$. Intuitively, a hazard ratio of 10 means that a couple is 10 times more likely to marry in 1989 Q4 relative to the counterfactual scenario, *given that the couple was not yet married in the beginning of that quarter*, holding constant couple characteristics. Standard errors are clustered on the child's quarter of birth, and standard errors of the hazard ratio are calculated using the delta method.[27]

Results The upper panel of Table 3 presents results from estimation of (7). The estimated hazard ratio in the full sample is 16.88 when controlling for $F_i(t)$, the vector of financial characteristics that influence the annuity's expected value, and 17.04 when also controlling for $\mathbf{D}_i(t)$, the vector of demographic and other observable couple characteristics. This means that a couple that is unmarried at the end of 1989 $Q3$ is, on average, 17 times more likely to marry in the next quarter than it would have been in the absence of reform. The lower panel replicates these results including flexible controls for male income and birth year; the results remain unchanged. These estimates are very similar to the estimates of presented in Section 5.1 above.

I then study how this hazard ratio varies with two different measures of the economic value of the annuity, by adding interactions between $s_i^*(t)$ and each male labor income group

[27] Another approach to addressing potential unobserved heterogeneity that causes correlation across observations in cohorts is to model such correlation explicitly as shared frailty, using a random effects model. Because I do not know the correlation structure, I use clustered standard errors in my baseline specification, which assumes no particular model of correlation. If anything, failure to model frailty will underestimate the true response.

l in $\tilde{\mathbf{F}}_i(t)$, and between $s_i^*(t)$ and each male birth year group b. I thus estimate the following regression model:

$$h(t;\mathbf{Z}_i(t)) = h_0(t)exp(\sum_l \alpha_l s_i^*(t) + \sum_b \beta_b s_i^*(t) + \gamma post_i(t) + \delta_1 \tilde{\mathbf{F}}_i(t) + \delta_2 \mathbf{D}_i(t)), \qquad (8)$$

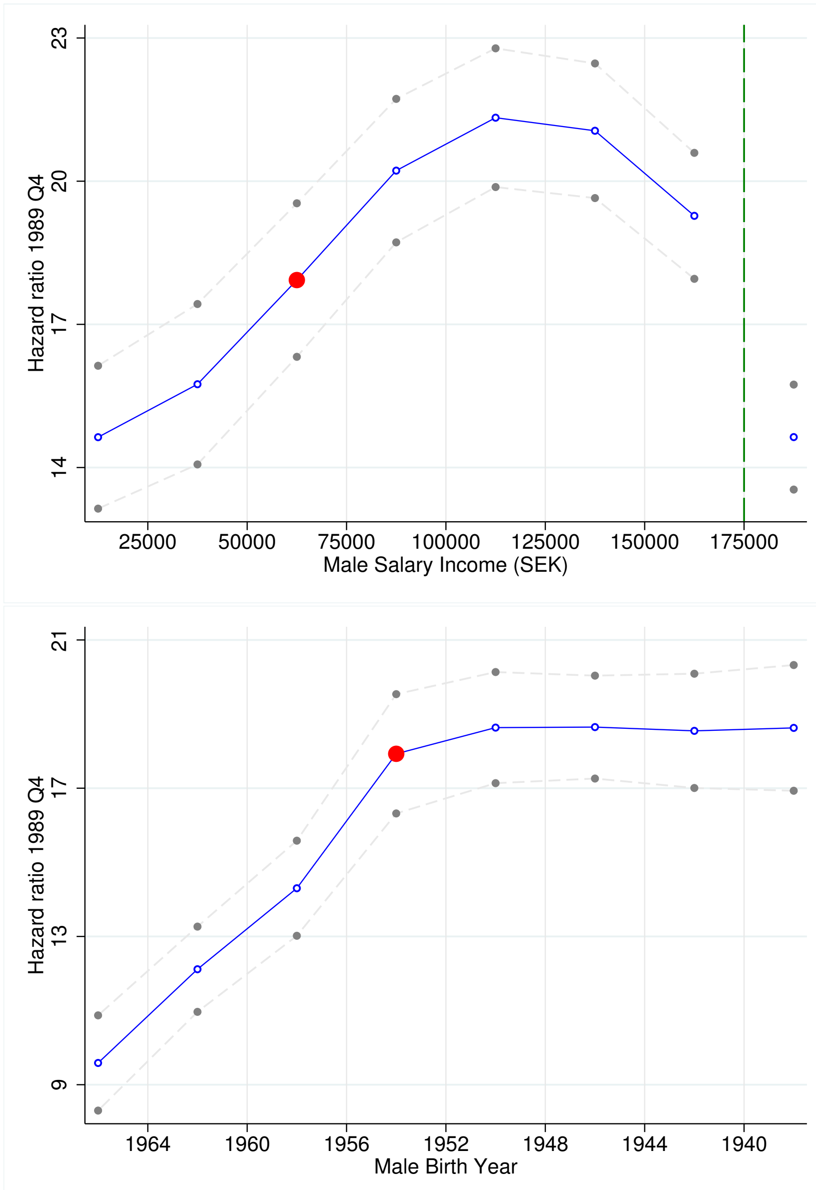
Here, the estimated hazard ratio for marriage in 1989 $Q4$ for a couple with male labor income l and birth year b is given by $e^{\alpha_l+\beta_b}$.

The upper panel of Figure 7 plots the estimated hazard ratios for couples in which the male was born between 1952 and 1956, for different male income ranges. The hazard ratio increases with male income, and thus with the annuity's expected value: A man with income in the range 25k-50k is 16 times more likely to marry in 1989 $Q4$; the corresponding figure for men whose income instead is in the range 125k-150k is 21. In this sample, 150k is the 77th percentile of labor income. In next range, the the hazard ratio decreases, which may reflect the fact that some husbands exceed the Social Security limit. (This limit is calculated based on "pension rights income," which in addition to labor income includes some social insurance payments; see Appendix B for details). The last group includes couples that exceed this limit with certainty; this is indicated by the green dashed line. The hazard ratio is thus increasing in husband income in the range where a higher husband labor income raises the annuity's value. Above the Social Security limit, the annuity's value remains constant. The fact that couples in the highest bracket, above the 91th income percentile in this sample, are less responsive may suggest that the annuity is less important in the highest earning households.

The lower panel of Figure 7 plots the estimated hazard ratios for couples where the male earns income in the range 50k-75k, for different male birth year ranges. The hazard ratio is increasing with male age: A man born after 1964, and who hence is younger than 25 in 1989 $Q4$, is 9 times more likely to marry in 1989 $Q4$; the corresponding figure for men born between 1952 and 1948 is 18. For yet older cohorts, the ratio remains constant. The red dot indicates the same group of couples in both panels (and hence the same hazard ratio).

Husband mortality risk and adverse selection into survivors insurance To the extent that husband age captures mortality risk, it is an observable characteristic that could potentially be priced into a private annuity. I therefore turn to investigating whether responses to the change in marriage contract vary with husbands' *ex post* mortality. Using death records, I identify all men in my sample that died within five years of January 1, 1990. I then reestimate (7), including an indicator variable for such couples, $1[H\ dies\ within\ 5years]_i$ (capturing the fact that couples where a spouse is likely to die within five years may be more likely to marry in general, for reasons relating to inheritance, etc), as well as the interaction term $1[H\ dies\ within\ 5years]_i^* s_i^*(t)$ (capturing any extra responsiveness to the elimination of

Figure 7: Hazard ratios for couples with different male income and birth year ranges



Note: The Figure plots the estimated hazard ratios of marriage in 1989 Q4, for each male labor income group l in the upper panel and for each male birth year group b in the lower panel, obtained from estimation of (8). The upper panel plots the hazard ratio for couples in different male income intervals, for males born between 1952 and 1956. The last income group includes couples that exceed the Social Security limit (see Section B for details); this is indicated by the green dashed line. The lower panel plots the hazard ratio for couples in different male birth intervals, for couples with male incomes in the range SEK 50k-75k. The red dot indicates the same group, couples where the male earns labor income in the range SEK 50k-75k and was born between 1952 and 1956, captured in both panels. Gray dashed lines represent 95% confidence intervals.

Table 4: Impact on marriage: heterogenous effects w.r.t. male *ex post* mortality

	Financial controls		All observable controls	
Estimates from sample	Coefficient, $\hat{\beta}$	Exponential, $e^{\hat{\beta}}$	Coefficient, $\hat{\beta}$	Exponential, $e^{\hat{\beta}}$
1989 Q4	2.83***	16.88***	2.84***	17.04***
	(0.03)	(0.52)	(0.03)	(0.55)
Interaction	0.12**	1.13**	0.13**	1.14**
	(0.06)	(0.07)	(0.06)	(0.07)
Number of couples	247046	247046	218147	218147

	Financial controls		All observable controls	
Estimates from sample	Coefficient, $\hat{\beta}$	Exponential, $e^{\hat{\beta}}$	Coefficient, $\hat{\beta}$	Exponential, $e^{\hat{\beta}}$
Husband dies within 5 years	3.00***	20.12***	3.06***	21.28***
	(0.10)	(1.93)	(0.11)	(2.24)
Number of couples	1214	1214	1051	1051
Husband alive after 5 years	2.83***	16.88***	2.84***	17.04***
	(0.03)	(0.52)	(0.03)	(0.55)
Number of couples	245832	245832	217096	217096

Note: Columns 1 and 3 report the estimated coefficient on the indicator variable for 1989 Q4, with standard errors clustered at couple cohort in parentheses. Columns 2 and 4 report the corresponding exponentials, i.e. the hazard ratios plus one. Significance levels from a test of the null hypotheses that each regression coefficient is 0 or, equivalently, that each exponential is 1. The upper panel reports results from a regression including $s_i^*(t)$, an indicator variable taking the value one if the man dies within five years of January 1, 1990, and their interaction term. The lower panel reports results from separate estimations of the baseline regression equation in the two male *ex post* mortality samples. All regressions include controls for financial characteristics $\mathbf{F}_i(t)$ that influence the annuity's expected value: Each partner's employment status, the man's total labor income and share of household labor income, and the partners' birth years. In columns 3 and 4, each regression also includes controls for other characteristics $\mathbf{D}_i(t)$ that I observe, and that the price of a (private) annuity potentially could be made contingent on: The partners' education levels, their number of children, and the spouses' marriage parities.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

survivors' insurance among these couples). The upper panel in Table 4 presents the results. When controlling for financial characteristics that influence the annuity's expected value, $\mathbf{F}_i(t)$, and all other characteristics that I observe, $\mathbf{D}_i(t)$, the implied hazard contribution from the interaction term is 1.14. Thus, a couple where the husband dies within five years has a 14 percent higher hazard ratio than a couple where the husband remains alive for at least five years; this difference is significant at the 5 percent level. The lower panel presents results from separate estimations of (7) in the sample where the husband dies within five years, and the in the sample where he lives for at least five years. In the mortality sample, the estimated hazard ratio is 21.28; in the live sample, it is 17.04. The implied difference in hazard ratios is similar to the estimate presented in the upper panel, but estimated with lower precision due to the small size of the mortality sample.

This may suggest the presence of adverse selection into survivors insurance. In particular, a positive correlation between demand for insurance and risk type, at given prices, is consistent with adverse selection (and/or moral hazard, which I return to below). In the context of the Swedish survivors insurance scheme, all types (of couples) face the same out of pocket cost, namely zero. However, upon entering marriage, couples receive insurance plans that vary in value. Hence inclusion of the variables $\mathbf{F}_i(t)$, which capture the financial value of survivors insurance for a given couple, is akin to controlling for individual prices in the context of private insurance.

To investigate this further, I replicate the same analysis in subsamples with different *ex post* mortality. I estimate (7) for each distinct *ex post* mortality sample and plot the estimated hazard ratios for marriage in 1989 Q4 in Appendix Figure A6 as points along the red line (these estimates are also reported in Appendix Table A2, both with and without controlling for demographic observables, $\mathbf{D}_i(t)$). Take-up of marriage in the last quarter of 1989 Q4 is higher, the shorter is the time span until death of the husband. I thus observe a positive correlation between couples' take-up of insurance – through marriage – and couples' expected cost of coverage, *at given benefits* (implicit prices, captured by $\mathbf{F}_i(t)$). Further, this correlation persists when controlling for variables that are unrelated to the value of survivors insurance, but that could be used (by a private insurer) to price a corresponding annuities plan, $\mathbf{D}_i(t)$.

While this correlation is consistent with adverse selection, coverage by survivors insurance could, in principle, also raise the likelihood of husband death.[28] While I cannot test for such moral hazard in this particular sample, the regression discontinuity analysis in Section 6 allows me to explicitly examine whether survivors insurance has a causal impact on husband mortality. I find no evidence of moral hazard, which suggests that the positive correlation presented here is due to adverse selection.[29]

These results contribute to the literature on adverse selection in insurance markets. Existing evidence from private markets for annuities or life insurance is mixed.[30] My innovation lies in focusing not on a product provided in a private insurance market, but on a government-provided scheme that is provided indirectly, through the marriage contract. My results suggest that if insurance companies would observe all the information that I capture by $\mathbf{D}_i(t)$ – the spouses' educational attainment, marriage parities, and completed fertility – but no more information, then adverse selection would likely arise in such a private market. This may be one reason why private markets for annuities and life insurance were underdeveloped in Sweden at the time of reform.

More broadly, while we typically cannot analyze the premise of government intervention in presence of government intervention, these results suggest that government provision of annuities, in the form of survivors benefits, may remain in many countries precisely because adverse selection hinder private annuities markets to develop.

[28] For evidence that marriage may affect health, see, e.g., Lillard and Panis (1996).

[29] These results are omitted to save space.

[30] For example, Finkelstein and Poterba (2002, 2004) reject the null hypothesis of symmetric information in U.K. annuities markets. Cawley and Philipson (1999) find no evidence of adverse selection in the US life insurance market, whereas He (2009) does.

5.3 Prediction MU3: Heterogenous effects and expected lifelong commitment

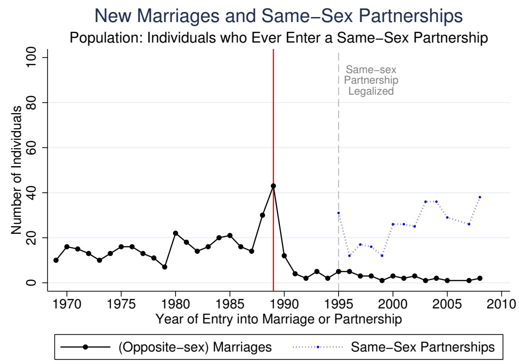
Because survivors insurance only accrued to widows and never to divorcees, the annuity's *realized* value was de facto zero for couples that eventually divorced. Couples that, at announcement, attach a smaller probability to the event that they will remain together for life thus have a weaker incentive to respond to the reform by taking up marriage. This is captured in the model by the prediction that the response is lower, the lower is the realized stochastic component of marital surplus in period 2, θ_2. Importantly, this prediction relies on the assumption that match quality shocks are positively correlated over time (so that θ_2 conveys information to the couple about its expected total marital surplus in period 3). Because this assumption is necessary for cohabitation to constitute a "learning by doing" process as defined in Section 3, testing this prediction thus, more broadly, speaks to the role of learning in cohabitation.

Testing this prediction poses a challenge, however. A precise measure of couples' ex ante beliefs about the strength of their union is not observable in my administrative data. To obtain a proxy for having a weak belief at the time of reform in remaining together for life, I turn to a 1995 law that legalized same-sex marriage in Sweden. Because my data set contains all marriages that were entered into between 1969 and 2009, including same sex marriages from 1995 onward, I can create a sample consisting of all individuals who entered into a same-sex marriage between 1995 and 2008, and hence revealed a same-sex preference. Now consider the following two assumptions: (i) Sexual preferences at two distinct points in time t and $t+s$, $s>0$, are positively correlated; and (ii) a stronger same-sex preference reduces the expected duration of a heterosexual marriage. Under assumption (i), an individual who in 1995 or later reveals a same-sex preference already had, on average, a weaker opposite-sex preference in 1988. Under assumption (ii), this translates into a weaker average belief about the longevity of heterosexual marriage. Under these two potentially restrictive assumptions, the response in marriage take-up would be smaller in this "same-sex sample" than in the entire (baseline) sample.

In Figure 8, the number of same-sex unions is depicted by a blue line, starting in 1995. At legalization, a spike in same-sex marriages occurs (potentially reflecting latent demand), after which the number of marriages first falls, and then begins to rise. The black solid line depicts the opposite-sex marriages among the same individuals, starting in 1970. While the figure displays bunching in the distribution of opposite-sex marriages in response to the reform, estimates of the increase in the probability of heterosexual marriage, relative to the counterfactual, are in the range of $\frac{\triangle p_{s^*}}{p_{s^*}}=2$. This estimate is several times smaller than the estimated impact in the baseline sample.[31] Under the assumptions postulated above,

[31] Because the analysis presented in Section 5.1 uses quarterly data, whereas the current analysis uses yearly data due to the small sample size, these estimates are not directly comparable. When remaking the analysis

Figure 8: Heterosexual marriage and same-sex preference: ex ante beliefs in marital decisions



Note: The sample includes the universe of individuals who ever entered a same-sex partnership since 1995, when partnerships were legalized, and had a child before 1989. The blue dotted line plots the number of new same-sex partnerships at a yearly frequency. The black solid line plots, for the same population of individuals, the number of new (opposite-sex) marriages at a yearly frequency since 1970.

this suggests that the response was smaller among couples with weaker *ex ante* beliefs in the union's survival. Together with the theoretical framework, this result is informative about the distribution of marital surplus shocks in this empirical context, suggesting that the shocks were correlated over time. This supports the interpretation of cohabitation as a learning process.

5.4 Prediction MU4: Long-run divorce rate in marriage-boom marriages

When marital quality evolves over time, replacing the existing marriage contract with a less desirable one attaches option value to marrying into the desirable contract, which induces couples to rush to marriage even though they, in the absence of reform, would wait to see whether the relationship improves. When match quality exhibits persistence, rushed marriages should be more likely to end in divorce. Indeed, this is why, in the absence of reform, such marriages would never materialize. I thus have a prediction about *the nature of selection* into marriage in the last quarter of 1989, when rushed marriages were entered into.

To examine this, I compare the incidence of divorce among couples that marry in the last quarter of 1989 with those that marry into the *same* marriage contract earlier. Summary statistics for this subsample are presented in Columns 2 and 3 of Table 1. I first estimate the following regression using OLS:

$$1\left[Divorce_x\right]_{imd} = \alpha + \beta \mathbf{1}\left[marr = s^*\right]_i + X_i'\theta + \eta_m + \zeta_d + \epsilon_{imd}, \tag{9}$$

presented in Section 5.1 using yearly data, the estimated response in the same sex sample remains several times smaller (results omitted to save space).

Table 5: Hightened divorce risk in rushed marriages: OLS results

	Dependent variable: Divorce within			
	3 years	5 years	10 years	15 years
Married in 1989Q4	0.0075**	0.0228***	0.0423***	0.0393***
	(0.0027)	(0.0048)	(0.0088)	(0.0091)
Number of obs	94681	94681	94681	94681

Note: Each column represents a regression with a different dependent variable. All regressions include the controls described in the text, as well as wedding month fixed effects, wedding day of week fixed effects, and four educational level indicators for each spouse. Standard errors clustered at (wedding month*wedding day of week) in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

where the variable $Divorce_x$ takes the value of one if couple i divorces within x years of marriage; the main explanatory variable of interest is a dummy taking the value of one if the couple married in the last quarter of 1989; η_m and ζ_d capture wedding month and day of week fixed effects, respectively; and X_i captures observable couple-specific characteristics: the spouses' ages, household income, and h's share of household income at marriage; the spouses' levels of education and immigration status; h's IQ level; the spouses' marriage number; and completed fertility. The key coefficient of interest is β, which measures the difference in marital stability of marriage-boom marriages relative to other marriages with the same marriage contract. Robust standard errors are clustered on the (marriage month*marriage day of week) level.

Table 5 presents the results. Consistent with the prediction, the estimates suggest that marrying in the boom is associated with a 2.28 percentage point higher probability of divorce within 5 years, and a 4.23 percentage point higher probability of divorce within 10 years, at the sample mean. The average 10-year divorce rate is 0.16. Appendix Table A3 reports average marginal effects from Probit estimation; the results are similar.

Because the couples that married in the last quarter of 1989 on average are older and have a higher household income – as is illustrated in Table 1 – and thus have characteristics for whom divorce rates are generally lower, it is crucial to control for these demographic and economic factors. Appendix Table A4 illustrates this by omitting the couple-specific characteristics. This implies that the marginal effect should be evaluated at the mean characteristics of couples marrying in the last quarter of 1989. Probit estimation yields $Pr\left(Div_{10}\left|X_{1989q4}, s^*=1\right.\right) = 0.148$ and $Pr\left(Div_{10}\left|X_{1989q4}, s^*=0\right.\right) = 0.093$; the estimated discrete increase in the probability of divorce if marrying in the last quarter of 1989 is thus 0.055 (with a standard error of 0.0257). If this difference is driven by a higher divorce rate among rushed marriages, given that such marriages constitute roughly 40% of marriage-boom marriages, the implied probability of divorce in rushed marriages is 0.229. Of the 18000 rushed marriages, only around 13800 last for 10 years.

On the one hand, this implies that some long-lasting unions were prompted by the reform.

On the other hand, policy-induced marriages are more likely to dissolve, underscoring that the effectiveness of policies that aim to promote lasting commitment in unions should not be evaluated solely on the policy's impact on marriage take-up, because a large share of the "complier marriages" may end up in divorce.

5.5 On the size of the documented responses

Section 5 has documented responses along the margin of entry into marriage that are large relative to the value of the annuity, especially in light of the existing evidence discussed above that has documented small responses to the wedge between marital surplus and the surplus from cohabitation or divorce.

One candidate interpretation of these large responses in entry into marriage is that they merely represent relabeling, as many responding couples previously cohabit. Indeed, the difference between cohabitation and marriage likely is smaller in Sweden at the time of the reform than it is in the U.S. today, e.g. when it comes to the social acceptance of cohabitation. However, as discussed in Section 2 above, at the time of reform, entry into marriage has major legal implications that cannot be replicated by cohabiting couples though private contracting – concerning inheritance rights, custodial rights of children, and the division of assets in case of separation. Thus, converting a cohabiting union into marriage has far-reaching real economic implications during the time period that I study. (The next sections of the paper present further evidence of real responses along several other margins, including exit from marriage and intra-household bargaining.)

Another interpretation of the large responses relates to the fact that media featured the law change prominently in the last quarter of 1989, as demonstrated in Figure A1. This may have induced couples to enter marriage *not* for economic reasons, but as a form of herding behavior. Indeed, other settings suggest that significant media attention to an impeding event may cause overreactions relative to what likely would be predicted by a model of rational behavior.[32] To try to get at this, Figure A7 displays marital behavior for couples who had their first joint child just after the elimination of survivors insurance. As such, these couples remained ineligible for survivors' insurance, and thus faced no economic incentives to enter marriage by the threshold; however, they were subject to the same media frenzy. Interestingly, no spike in marriages is visible in panels D, E, and F (labeled to reflect the fact that this Figure essentially replicates panels A, B, and C in Figure 3, but for cohorts that were "untreated" by the reform). The absence of spillovers to cohorts that did not face any economic incentives to enter marriage, but for whom the media reporting likely raised the salience of marriage, suggests that economic incentives, rather than salience, drove the documented responses.

The responses that I document are large precisely because the reform only altered economic

[32] One such event, for example, is the anticipated computer shutdown at the entry into the new millennium, referred to as the "Y2K bug." See, e.g., http://history1900s.about.com/od/1990s/qt/Y2K.htm

transfers to the household in states of the world that were expected to occur, on average, 42 years into the future. Hence, it only created moderate economic incentives in present value terms: As stated above, applying an annual discount rate of 3 percent yields an average expected value of the annuity at reform of approximately $4575. Instead applying a discount rate of 7 percent, the thirty year risk free bond yield at the time of reform[33], yields an average value of $785; and a discount rate of 10 percent values the lost marital surplus at a mere $219. The marital responses documented here thus constitute compelling evidence of a substantial degree of forward-looking behavior. More generally, this suggests that marital decisions are an integral part of couples' strategies to plan for financial security in the far future.

6 Survivors insurance and pre-existing marriage contracts

I now turn to the second group of couples that I study, those who were already married at the announcement of reform, to analyze the causal impact of survivors insurance on family well-being.

Sample and descriptive statistics To construct my baseline sample, I start from all individuals that entered marriage within 180 days of the eligibility threshold, January 1, 1985. I then exclude all couples that had a joint child before the reform announcement, June 8, 1988. I further exclude women born before 1945 and men who were 60 years or older at the date of marriage. Throughout the analysis, I also present results for a narrower window of 150 days around the eligibility threshold.

All of these individuals married into the same marriage contract, with survivors insurance, in 1984 or 1985. When the reform was announced in 1988, those who had married before January 1, 1985 were allowed to keep this contract. For those who married thereafter, the survivors insurance reform revoked the old marriage contract and replaced it with the new contract without survivors insurance, unless the couple had a joint child before January 1, 1990. Table 6 presents summary statistics for the baseline sample, as well as for the analogous sample that entered marriage within 180 days of January 1, 1984. These groups are similar, but relative to the sample studied in Section 5, the spouses studied here are more likely to be in their second marriages. This is consistent with the fact that second marriages are more likely to be childless (for three and a half years after marriage) in my data.[34]

[33] Retrieved from https://research.stlouisfed.org/fred2/data/GS10.txt.

[34] Note that the sample includes more men than women – due to the difficulty to match individuals into couples in the absence of children, my sample includes some men who were younger than 60 at the date of marriage, but who married a woman born before 1945. I thus count some men as losing survivors insurance by marrying after December 31, 1984, even though they were in couples that remained covered regardless of their date of marriage. If anything, this should bias my results toward zero. In a similar vein, my final sample of women may also include some who (were born after 1944 but) married a man who was 60 or older at marriage. However, the fact that the final sample of men is larger than the final sample of women suggests that my sample of women contains fewer "mistakenly included" individuals. For this reason, I use the sample of women

Table 6: Summary statistics: sample of already married couples

	Sample and placebo sample	
	Married around 1985	Married around 1984
Demographic characteristics		
H age at marriage	36.99	36.74
W age at marriage	29.18	28.82
H birth year	1948	1947
W birth year	1955	1954
H education (4 lvls)	2.06	2.05
W education (4 lvls)	2.05	2.08
H marriage number	1.45	1.44
W marriage number	1.30	1.29
Financial characteristics		
H log labor income at marriage	11.45	11.45
W log labor income at marriage	10.99	11.00
Observations	17009	15722

Note: The sample includes all individuals that married within 180 days of January 1, 1985 and within 180 days of January 1, 1984.

Empirical Framework An evaluation of the causal impact of survivors insurance on family well-being requires a comparison of couples who have such insurance with couples who do not. Section 5 illustrates, and the theory predicts, that couples strategically influence entry into marriage in order to take-up survivors insurance. This margin can thus not be exploited to identify causal effects.

Instead, the ideal experiment would be to randomly allocate survivors insurance to some couples but not to others. To mimic this, I take advantage of the relationship between survivors insurance, the date of marriage among childless, and the timing of the reform announcement. Specifically, I identify the impact of survivors insurance on family well-being exploiting variation induced by the discontinuity in time for couples that get survivors insurance if and only if they married on or before December 31, 1984. This variation allows me to use a regression discontinuity difference-in-difference estimation (RDD). The estimator identifies the average treatment effect for couples near the eligibility cut-off under specific conditions.

RDD design A regression discontinuity (difference-in-difference) design allows for identification of the impact of an endogenous regressor that is a known function of an observable assignment variable, where the assignment variable cannot be precisely manipulated (Angrist and Lavy (1999), Lee and Lemieux (2010)). In my setting, the date of marriage (*dom*) is the assignment variable and can be precisely manipulated, but the endogenous regressor – survivors insurance eligibility – is an unknown function of the observable assignment variable.

in the analysis of divorce; however, using the male sample leaves the results unchanged.

Intuitively, while couples could precisely manipulate their date of marriage, it was impossible for them to manipulate the timing of marriage in response to the reform, which was announced three and a half years after these couples married. Nevertheless, the fact that the assignment variable could be precisely manipulated implies that couples on one side of the cutoff could be systematically different from those on the other.

The first panel of Appendix Figure A8 plots the number of marriages in each weekly interval against distance from the survivors insurance eligibility cut-off. It shows an increase in the marriage frequency in the last week of 1984. To test for continuity in this distribution, I implement the McCrary (2008) test by collapsing the data into weekly bins and running the following regression:

$$N_b = \alpha + \beta \mathbf{1}\left[\tilde{dom}_b > 0\right] + g\left(\tilde{dom}_b\right) + \varepsilon_b, \tag{10}$$

where b indexes bins, N_b is the number of marriages in bin b, $\tilde{dom}_b$ indexes distance from the threshold in weeks, and $g\left(\tilde{dom}_b\right)$ is a polynomial. A test of $\beta = 0$ estimates whether the density is smooth. Indeed, I reject the hypothesis that the density is continuous at the threshold (p-value: 0.004). This raises the concern that "crossing" New Year's Eve has a separate effect on the outcome(s) of interest. Two features of my estimation strategy address this concern: First, to net out such an effect – provided it exists – I use an RDD design, which exploits the fact that couples that married around New Year's Eve one year earlier were unaffected by the reform. The second panel of Appendix Figure A8 shows their distribution of marriages, which is similar. Second, the timing of the announcement of the assignment rule gives me precise predictions about *when* differences in outcomes should emerge between the couples marrying close to, but on opposite sides of, January 1, 1985. Specifically, differences should emerge no earlier than three and a half years after marriage.

My estimation strategy follows that of Lalive (2008), who implements an RDD design when faced with a similar discontinuity in the density of the assignment variable. Let $Y_t = \tau_t * I_t + \sigma_t * NYE + g_t\left(dom\right) + U$ represent the causal relationship between the outcome of interest in time period t, Y_t, and survivors insurance status, $I_t = I_t(dom)$, where dom is the couple's date of marriage and U is a random vector of predetermined and unobservable characteristics. $NYE = NYE(dom)$ captures a potential impact of marrying after ("crossing") New Year's Eve. Given the existence of a discontinuity in insurance allocation from $t > June_{1988}$, the required identifying assumptions are that the impacts of I_t and NYE are additively separable (as above), and that, conditional on NYE, the direct marginal impact of dom on Y_t is continuous. Further, the interpretation of the RDD estimate as a causal effect requires a "monotonicity" assumption, which is satisfied here, since getting married before January 1, 1985, cannot have induced anyone to lose survivors insurance eligibility.

Finally, this fuzzy design requires the following exclusion restriction: the only impact of marrying before January 1, 1985, on outcome Y runs through its effect on survivors benefits. This assumption could be violated. Indeed, there are couples that married on or after January

1, 1985, but that qualified for continued survivors insurance coverage by conceiving a child in the time period between the reform announcement date in June 1988 and the reform implementation date on January 1, 1990.

The fact that some couples qualified by having a child does not, in itself, invalidate the exclusion restriction, as this is accounted for by the fuzzy RDD: all couples who married after December 31, 1984 are in the intent to treat (ITT) group, and a subset of these – the couples who did not have a joint child before January 1, 1990 – are treated. However, the exclusion restriction would be violated if some couples in the ITT group chose to have a child *in order to qualify* for survivors insurance (as there likely is an effect of the child on the outcome Y).

The key question, then, is how many couples can be expected to have their first joint child between four and five years after the date of their marriage.[35] This is precisely what Figure 2 tells us something about, as it displays the distribution of first births around the date of marriage in the population of couples whose first joint child was conceived at the reform announcement. The figure shows that, while some couples indeed have their first joint child between four and five years after marriage, the overwhelming mass of births occur before this point in time. In particular, approximately seven percent of the density lies in the relevant interval. In my sample, Figure 9 shows that 8% of the couples in the ITT group had a child by January 1, 1990; thus, this number is precisely in line with what is suggested by (the undistorted choices in) Figure 2.[36] Why, then, does fertility not respond? One potential reason for this is that knowledge of the reform was not widespread immediately after the reform's announcement. As shown in Appendix Figure A1, media reporting about the reform was heavily concentrated in the last three months of 1989, when any responses along the conception margin are impossible. Nonetheless, these results suggest that the exclusion restriction holds.

The first stage and reduced form equations are given by:

$$I_{it} = \alpha + \gamma 1\left[\tilde{dom_b} > 0\right]\mathbf{1}[Around85] + \delta\mathbf{1}\left[\tilde{dom_b} > 0\right] + g\left(\tilde{dom_b}\right) + h\left(\tilde{dom_b}\right)\mathbf{1}[Around85] + \epsilon_{it} \tag{11}$$

$$Y_{it} = \alpha + \beta\mathbf{1}\left[\tilde{dom_b} > 0\right]\mathbf{1}[Around85] + \eta 1\left[\tilde{dom_b} > 0\right] + i\left(\tilde{dom_b}\right) + j\left(\tilde{dom_b}\right)\mathbf{1}[Around85] + \nu_{it} \tag{12}$$

where i indexes couples, t indexes year after marriage, and $\tilde{dom_b}$ is the distance from New Year's Eve in 1985 or 1984, respectively. I include a vector of characteristics that is not

[35] A child conceived in or later than June 1988 is born in 1989 or later; hence, the potentially problematic births are those occurring from Jaunary 1, 1989 to January 1, 1990. This corresponds to the time interval between four and five years after their date of marriage (which occurred around 1985).

[36] I also explicitly test whether there is any discontinuity in the timing or number of children born at the threshold. Consistent with the graphical evidence, I do not find any impact on fertility in this sample. I omit these null results to save space.

Figure 9: Distribution of treatment (=change in marriage contract) around eligibility threshold

Note: The figure displays the share of entered marriages that experienced a switch in marriage contract due to the reform. Though my estimation strategy in practice involves a fuzzy RDD, treatment is near-universal at the right side of the threshold. Among couples that married around 1985 and that had not conceived a joint child by the reform's announcement in June 1988, only 8% had a child before January 1, 1990 (and thus obtained the old marriage contract, with survivors insurance).

necessary for identification but that reduces the standard errors: wedding day of week fixed effects and the spouse's educational attainment, age at marriage, age at marriage squared, and marriage parity. The RDD estimate is given by the ratio $\frac{\hat{\beta}}{\hat{\gamma}}$.

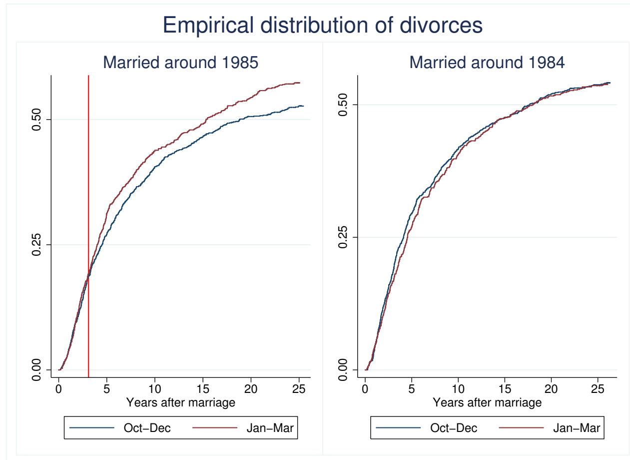
I test for continuity in the distributions of predetermined couple characteristics around the survivors insurance cutoff. Appendix Figure A9 plots these characteristics in each weekly interval against distance from the eligibility cut-off. I find no evidence that couples are systematically different on different sides of the cutoff.

6.1 Prediction MM1: Marital instability

The first prediction for matched and married couples is that removing survivors insurance from pre-existing marriage contracts induces some couples to divorce in response to the loss of marital surplus.

Results The left panel of Figure 10 displays the empirical cumulative distribution of divorces in two subsamples: couples (women) that entered marriage in the last three months of 1984 and in the first three months of 1985. The graph depicts the empirical estimates of the Kaplan-Meier failure (of marriage) functions, that is, the probabilities of divorce, conditional on being married, at any duration of marriage. Because I include no covariates, this corresponds to the empirical distribution. Graphing the data in this way captures the average differences between couples on different sides of the cutoff at different points in time. The panel offers

Figure 10: Empirical CDF of marriage duration around eligibility and placebo thresholds



Note: The graph depicts the empirical CDF of durations until divorce, obtained by estimating the Kaplan-Meier failure function without covariates, for couples marrying around 1985 (left panel) and around 1984 (right panel), respectively. Until the announcement of the reform in June 1988, all four groups of couples had the same marriage contract. Upon the reform announcement in June 1988, the old marriage contract was replaced by the new contract – without survivors benefits – for couples that married after January 1, 1985, depicted by the red solid line in the left panel. All couples in the right panel were allowed to keep the old marriage contract that they married into.

graphical evidence that the removal of survivors insurance caused divorces. During the first three years of marriage, when all couples had the same marriage contract, no difference can be discerned between the functions. Upon the reform announcement in June 1988, when survivors insurance was removed for couples that married after December 31, 1984, the failure functions begin to diverge. The figure also provides an indication of the magnitude of the difference in divorce outcomes for each time span after the reform announcement, and suggests that the difference widens over time.

The right panel plots the same functions for couples that married within three months of another cutoff, January 1, 1984. Because both groups were unaffected by the reform, the reform should not induce a wedge between the two failure functions; indeed, the figure confirms this prediction. I find no unexpected comparable divergence in the right panel, which offers support to my interpretation of the divergence in the left panel as the causal effect of survivors insurance. Put differently, this suggests that the observed differences in the cohorts that married on opposite sides of January 1, 1985, are not driven by differences between couples that marry in different months, but that they instead represent the causal impact of revoking survivors insurance.

Adding 95% confidence intervals to Figure 10, however, indicates that the difference in the left panel is statistically significant only two decades after marriage. Results from my

Table 7: Results: first stage

	Polynomial	
	2	3
Married after NYE*Married around NYE 1985	0.9142***	0.9065***
	(0.0108)	(0.0103)
AIC	-8608.54	-8613.40
Number of observations	13400	13400

Note: The dependent variable is the couple's survivors insurance status after January 1, 1990. The table presents the estimate of γ in Equation 11, the coefficient on the key independent variable in the first stage regression. This variable is an interaction between the indicator variable for marrying after New Year's Eve (abbreviated NYE) and the indicator for marrying around New Year's Eve of 1985. Robust standard errors in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

RDD design largely support this conjecture. Table 7 presents OLS estimates of the first stage (11), and Table 8 presents 2SLS (fuzzy RDD) estimates of the impact of survivors insurance on divorce at different durations of marriage t, and using polynomials $g(\tilde{dom}_b)$ of different orders and bandwidths. The estimates suggests that removing survivors insurance raises the probability that a marriage ends in divorce within 24 years by 5.27 percentage points using the smaller bandwidth (5.08 using baseline bandwidth) in the specifications favored by the AIC criterion. The picture that emerges from these results is that the removal of survivors insurance pushed couples on the margin into divorce, and predominantly so closer to the annuity's expected date of realization. Indeed, with discounting, the opportunity cost of forgoing the annuity is higher, the sooner it is expected to pay out.

6.2 Prediction MM2: Division of marital surplus

The second prediction for matched and married couples is that on average, the wife's share of household utility increases in marriages that exist at the reform announcement and survive the reform, as compensation for the loss imposed on her in the event of her husband's death. Under the assumption that leisure is a normal good, spouses' division of market labor provides one (inverse) measure of their division of intra-household utility (see, e.g., Chiappori (1992)). Interpreting labor supply responses as an indication of a transfer of utility requires caution in this context, however. This is because labor supply may also respond to the reform for other reasons. Chief among them is that the loss of survivors insurance is a negative income shock. I exploit a non-standard feature of this shock: Contrary to any shock that affects income that both spouses' *can* consume, and hence can give up when income decreases, the survivors insurance reform only affects transfers to the household in states of the world where the husband is dead. If the spouses' labor supplies and consumption shares were to remain constant, the husband's well-being would thus be unaffected (barring altruism) but the wife's well-being would deteriorate.

Table 8: IV (RDD) estimates: impact on divorce

	Bandwidth 150 days		Bandwidth 180 days	
	Polynomial		Polynomial	
Divorce within	2	3	2	3
3 years	0.0312	0.0246	0.0168	0.0236
	(0.0237)	(0.0257)	(0.0204)	(0.0227)
AIC	7546.38	7549.05	10574.03	10577.20
9 years	0.0504*	0.0413	0.0254	0.0285
	(0.0301)	(0.0328)	(0.0264)	(0.0292)
AIC	11961.37	11963.45	17392.55	17387.72
12 years	0.0296	0.0222	0.0162	0.0189
	(0.0308)	(0.0335)	(0.0270)	(0.0300)
AIC	12359.77	12362.95	18070.91	18068.44
21 years	0.0564*	0.0493	0.0414	0.0478
	(0.0313)	(0.0340)	(0.0276)	(0.0306)
AIC	12649.32	12652.96	18693.89	18690.54
24 years	0.0527*	0.0456	0.0418	0.0508*
	(0.0313)	(0.0341)	(0.0277)	(0.0307)
AIC	12656.42	12660.06	18714.88	18711.91
Number of observations	9104	9104	13400	13400

Note: Dependent variable: An indicator variable taking the value one if the couple divorced within x years of marriage, for the values of x indicated in column 1. Each cell represents a separate 2SLS regression. Results from regressions with divorce within 15 and 18 years as dependent variables, respectively, are omitted to save space; the effects are similar to those reported from the impact on divorce within 12 years. Robust standard errors in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

This suggests that while changes in wives' labor supply in response to the reform cannot readily be interpreted as a transfer of resources within the household, an increase in husbands' labor supply can: If he works more, he gives up utility from leisure, and he does this in response to a reform that imposes a statutory loss only on the wife.[37] Note, however, that while an increase in husband labor supply is consistent with the theoretical prediction operating through intra-household bargaining, it is also consistent with altruism (with or without bargaining); the data does not permit me to distinguish between the two.

Results *Husbands' labor supply.* Table 9 presents estimation results for the outcome variable $Y_{it} = Husb\_ls_{it}$, an indicator variable taking the value of one if the husband is working in year t, and zero otherwise. On average, I find no impacts immediately upon the reform's announcement. In the longer run, however, the estimates suggest that removing survivors insurance raises the average probability that the husband is in the labor force by circa 4 percentage points in 2004. More careful investigation suggests that these responses need not, however, reflect an extensive margin response in the traditional sense.

Specifically, these responses are largely driven by men who are very close to "retirement age." Today, there is no fixed age of retirement in Sweden, but the majority of men retires between the ages of 63 and 65. I decompose my sample of husbands into those that are older than 62 and younger than 63 in 2008 and present results for each subsample in Table 10. The left column again presents the estimated average increase in the full sample (using a second order polynomial), according to which the loss of survivors insurance raises the probability that a husband is working in 2008 by 3.58 percentage points. Columns two and three show that the estimated increase is smaller – and insignificant – among men who are younger than 63 in 2008; in contrast, among husbands who are older than 62, the estimated impact is larger and highly significant. These results suggest that in response to the loss of survivors insurance, men delay their entry into retirement, which may be thought of as an intensive response along the timing-of-retirement margin.[38]

Coming back to the hypothesis, the fact that husbands respond at all suggests that husbands behave as if the reform induced an income shock on the household – even though the reform only affected the household in states of the world where he would be dead but left the surplus unchanged while he would be alive. Although the statutory loss of the reform is borne by the wife alone, the economic incidence is thus partly being borne by the husband, who gives up leisure – forgoes utility – in states where he is alive.

[37] This interpretation is consistent with, for example, the household spending resources that previously were available for joint consumption on buying a private life insurance or annuity to replace the lost annuity, and the husband working more to compensate for this. Put differently, the household responds as if this were a standard income shock, and thus the husband bears part of its economic incidence.

[38] Interestingly, however, I do not observe any responses on intensive margin labor supply, i.e., husband's (log) earnings top coded at the social security maximum. The absence of intensive margin responses may reflect rigidities in the labor market such as union-negotiated wages and working hours. Responses thus seem to operate through the timing of retirement margin only.

Table 9: IV (RDD) estimates: impact on husband's labor supply (extensive margin)

	Bandwidth 150 days		Bandwidth 180 days	
	Polynomial		Polynomial	
Husband working in	2	3	2	3
1988	0.0109 (0.0182)	0.0125 (0.0197)	0.0123 (0.0157)	0.0087 (0.0173)
AIC	10281.15	10284.58	13693.01	13695.18
2000	0.0221 (0.0216)	0.0201 (0.0233)	0.0290 (0.0189)	0.0242 (0.0208)
AIC	14668.67	14670.58	20617.14	20620.43
2004	0.0443** (0.0216)	0.0390* (0.0234)	0.0441** (0.0190)	0.0455** (0.0209)
AIC	14687.58	14691.14	20830.89	20832.65
2008	0.0348* (0.0208)	0.0234 (0.0225)	0.0358* (0.0184)	0.0322 (0.0202)
AIC	13679.73	13680.80	19563.14	19566.92
Number of observations	13085	13085	18839	18839

Note: Dependent variable: Husband labor force participation in the year indicated in column 1. Each cell represents a separate 2SLS regression. Results from regressions with labor force participation in 1992 and 1996, respectively, are omitted to save space; these effects are insignificant. Robust standard errors in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 10: Impacts on husband labor supply driven by men close to retirement

		Subsample of couples: Husband age	
	All couples	H younger than 63	H older than 62
Husband working in 2008	0.0358* (0.0184)	0.0136 (0.0274)	0.0641*** (0.0212)
Number of observations	18839	11127	7712

Note: Each cell represents a separate 2SLS regression using a second order polynomial and a bandwidth of 180. The left column presents results for the whole male sample. Columns two and three present results from estimation on two subsamples. Robust standard errors in parentheses.

Wives' labor supply. To give a complete picture of household labor supply, Appendix Table A5 presents results for wives' labor market participation, which show no significant responses. Given that the reform raises a wife's marginal incentive to work for several (unmodeled) reasons - she becomes more reliant on her own earned benefits, experiences a negative income shock, and faces a lower effective marginal tax on labor - the absence of significant responses suggest significant barriers to re-entry. Indeed, the women who were hit the hardest by this reform were those who had worked the least beforehand, whom likely were the least able to make labor supply adjustments.

7 Survivors insurance and matching

I now turn to an analysis of impacts on the assortativeness of matching.

7.1 Prediction UU1: Assortativeness of matching

Because the survivors insurance tied to the old marriage contract was worth more for couples with highly unequal earnings (capacities), the old marriage contract subsidized "one-sided" unassortative matching, that is, matches between high-earning men and low-earning women. Removing the survivors insurance provision from the marriage contract is therefore predicted to have long-term impacts on matching patterns between men and women. In particular, the precise prediction concerns the *density of the share of highly skilled men that marry "down,"* that is, that marry a woman of low skill (pre-determined earnings capacity).

Sample and descriptive statistics To take this prediction to the data, I begin by comparing the matching patterns of couples that choose to marry into the old and new marriage contracts. As a measure of skill, I use educational attainment *at marriage*, and distinguish between individuals that attended college and individuals that did not. I refer to those who attended college as having "higher education," a category that comprises roughly 25% of all men who marry, and to high school or less as "lower education." My sample includes all couples with children that married between 1983 and 1999, a period during which the definition of educational attainment remains constant.[39] I exclude the 6% of the observations for which I have no information about educational attainment at marriage. Appendix Table A6 displays summary statistics for this sample.

Empirical methodology I collapse the data into quarterly bins. I define the distance between a couple's quarter of marriage, $Vigq_s$, and the final quarter in which marriage entails

[39]The sample is limited to couples who ever had a joint child (before or after marriage) due to the difficulty to match married individuals into couples in the absence of joint children, when spouses cannot be linked using child ID, since comprehensive relationship codes are lacking.

take-up of the old marriage contract by $\tilde{Vigq_s} = (Vigq_s - 1989Q4)$, and estimate the following regression:[40]

$$r_s = \alpha + \beta\mathbf{1}\left[\tilde{Vigq_s} > 0\right] + \gamma\mathbf{1}\left[\tilde{Vigq_s} > 0\right]\left(\tilde{Vigq_s}\right) + \delta\mathbf{1}\left[s = s^*\right] + g(\tilde{Vigq_s}) + \zeta_q + \varepsilon_c, \quad (13)$$

where r_s denotes the ratio of highly skilled men that marry "down,"

$$r_s = \frac{N\left(\tau_h^{HIGH}, \tau_w^{LOW}\right)}{N\left(\tau_h^{HIGH}, \tau_w^{LOW}\right) + N\left(\tau_h^{HIGH}, \tau_w^{HIGH}\right)}, \quad (14)$$

where the function $N()$ counts the number of marriages of the match type indicated by the arguments. The main coefficient of interest is β, which captures a discontinuous change after the threshold s^*. Further, γ captures any change in the slope at s^*, and $g(\tilde{Vigq_s})$ is a polynomial in $\tilde{Vigq_s}$.

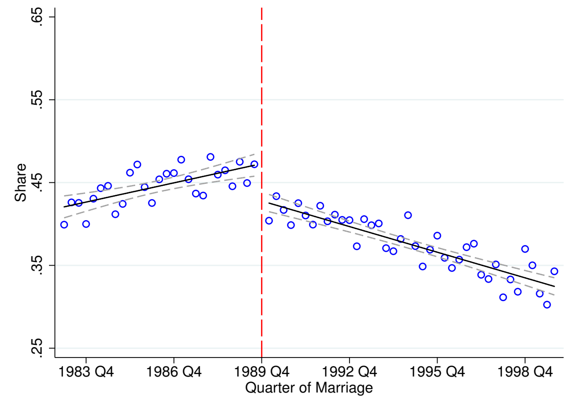
Results Figure 11 previews the results. I use observations from all couples with children in which the husband has high educational attainment at marriage, collapse this data into quarterly bins, and calculate the share of marriages in which the husband married a woman of low skill ("married down"), for each quarter. I plot the relationship between this share and the date of marriage during the years 1983 to 2000. Specifically, I plot residuals from a regression on quarter fixed effects and a dummy for the last quarter of 1989, represented by hollow circles. The black lines represent the linear fit of the share of men marrying down on the quarter of marriage, estimated separately on either side of the eligibility threshold. Finally, the dashed gray lines show the 95 percent confidence intervals. At the eligibility threshold, the figure shows a discontinuous change in the share of men marrying down. Consistent with the prediction, a larger share of highly skilled men marry down in the group that marries into the old marriage contract.

Table 11 presents the estimates from (13), using as $g(\tilde{Vigq_s})$ three different polynomials in $\tilde{Vigq_s}$. The linear model, which is favored by the AIC criterion, suggests that highly skilled men that marry into the old marriage contract are 4.47% more likely to marry a woman of low skill.

This begs the question whether the increase in assortativeness is driven by the fact that the reform induced more unassortative than assortative couples to marry in the end of 1989. Indeed, if no unassortative matches remain unmarried, the result obtains mechanically; not by

[40]Note that in the previous section, $\tilde{dom_b}$ represents the date of marriage relative to January 1, 1985, whereas $\tilde{Vigq_s}$ represents the quarter of marriage relative to the last quarter of 1989.

Figure 11: Marriages: impact on the share of highly skilled men marrying a low skill woman



Note: Quarterly bins. The hollow circles depict the share of highly skilled men marrying a woman of low skill (seasonality adjusted). The black solid lines represent linear fits of the share of highly skilled men marrying a woman of low skill on quarter of marriage, estimated separately on each side of the cut-off. Gray dashed lines represent 95 percent confidence intervals.

Table 11: Matching: Impact on the share of highly skilled men marrying low skilled women

	Polynomial		
	1	2	3
New Marriage Contract	-0.0447***	-0.0364**	-0.0492*
	(0.0087)	(0.0135)	(0.0194)
Adj. R squared	0.87	0.88	0.87
AIC	-353.65	-353.06	-350.20
Number of obs	68	68	68

Note: Dependent variable: The ratio of highly skilled men that marry a low-skilled woman, as defined in Equation 14. The table reports the coefficient on the key explanatory variable in Equation 13, an indicator variable for the marriage occurring after the last quarter of 1989, into the new marriage contract. Each column represents a separate regression with different orders of polynomials in time. All regressions include quarter fixed effects. Robust standard errors in parentheses. The AIC criterion favors the linear model.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

an increase in assortative matching, but by a decrease in new unassortative unions. To examine this, I plot the frequencies of new assortative and unassortative marriages in Appendix Figure A10. While the trends in assortative and unassortative marriages are similar pre-reform, they diverge post-reform. Specifically, the frequency of assortative matches increases, whereas the frequency of unassortative matches slowly declines over time. Intuitively, the reform did not crowd out all new unassortative marriages, because unassortatively matched couples that had a joint child before the reform's announcement (and hence that had an incentive to respond to the reform by marrying before 1990) constituted only a small share of all unassortative matches considering marriage.

Pre-marital investments and the returns to education While the theory offers a precise prediction for the change in the *level* of assortativeness in highly skilled men's unions, it does not offer any prediction for the change in the time trend. One potential reason for this is that, over time, the change in marriage patterns affected the composition of high-skilled and low-skilled men and women in the marriage market. In the theoretical framework, this would correspond to changes in the skill distributions for men and women. Indeed, if I were to add to the model a pre-matching stage where men and women can invest in education, one prediction that would obtain is that the reform induces a greater increase in women's marginal benefit from education than in men's.[41]

While a detailed analysis of this is outside the scope of this paper, to illustrate that such impacts may be a possibility, Appendix Figure A11 shows the simple aggregate numbers of individuals that enter university, by year, in Sweden in the top panel. The bottom panel splits this up by gender, with the black line depicting women and the red line men. The upper panel shows a large increase in overall university enrollment after 1990, which corresponded to a nationwide expansion of higher education (Bjorklund et al., 2010). The lower panel shows that the enrollment increase is greater among women. This change in enrollment is consistent with the fact that the relative returns to education were changing in the mating market. This is only suggestive evidence, however; it is also possible that the higher influx of women into higher education reflected the fact that fields of study that were popular among women expanded faster than those popular among men.

8 Tying survivors insurance to marriage

My empirical findings paint a consistent picture: In Sweden, tying survivors insurance to marriage promoted marriage, deterred divorce, and subsidized unequal matches, which in turn subsidized spousal specialization in market and domestic work. The theoretical framework in Section 3 provides insight into the mechanisms that drive these responses.

[41] For evidence that admittance to university has substantial returns in the marriage market, see, e.g., Kaufmann et al. (2013).

However, when survivors insurance was created in Sweden in 1948 – and in the U.S. in 1939 – the schemes were not put in place to affect behavior in the marriage market. Instead, they aimed to solve a concrete problem, namely that women often ended up impoverished in widowhood.[42] If survivors benefits serve an insurance purpose, then how does the presence of marriage market responses – documented in this paper – affect the optimal design of such benefits?

Here I address this question in a stylized framework that serves to highlight the key mechanisms at play in the decision to link social insurance to marital status – insurance benefits and marriage market distortions. I intentionally abstract from classic issues in the social insurance literature that pertain to why the government may need to provide insurance in the first place, moral hazard, etc; instead, to mimic as closely as possible my empirical setting, I take the existence of social insurance as given, and ask when the social planner wants to *link it to marriage* through survivors benefits.

As before, I consider men and women with heterogenous income-earning abilities τ_w and τ_m. I now add a few more specifics. Outside marriage, an individual gets utility only from his or her labor income, $u(\tau_s)$, which I assume to be increasing and strictly concave, with the normalization $u(0) = 0$. Inside marriage, *gross* marital production is a function of total household labor income, $V(\tau_w + \tau_m)$. *Net* marital surplus thus is $S(\tau_w, \tau_m) = V(\tau_w + \tau_m) - \sum_{w,m} u(\tau_s)$, which I assume to be positive, as before.[43]

The social planner's insurance objective. Following previous work on the design of social insurance against poverty (Besley and Coate, 1992, 1995; Kleven and Kopczuk, 2011), I assume that the social planner is concerned with ensuring that each individual has income above a "poverty line," denoted by z, which I normalize to 1. This *income maintenance* objective induces the social planner to transfer sufficiently much from the "rich" to the "poor" to lift the latter out of poverty. Notice that a total income of at least 4 keeps two individuals out of poverty over two periods.

Sequence of events. To focus on marriage and divorce decisions, I here abstract from matching and bargaining and consider a population of couples exogenously matched in Stage 1. In Stage 2, couples observe a stochastic marital surplus shock $\tilde{\theta}_2$ and decide whether to marry. Then, labor income is earned. In Stage 3, couples observe another stochastic marital surplus shock $\tilde{\theta}_3$, upon which married couples decide whether to divorce, and unmarried couples decide whether to marry. After this decision, men die with probability p, in which case survivors benefits may be paid to the wife. Marital surplus is generated in every period

[42] In the U.S., Social Security played a key role in reducing absolute poverty among the elderly from 1950 to 2000 (Engelhardt and Gruber, 2004) and and in Sweden, Social Security income constituted 74% of the income of individuals above age 65 in 1994 (Palme and Svensson, 1997).

[43] Letting $S(\tau_w, \tau_m) = V(\tau_w + \tau_m) - u(\tau_w) - u(\tau_m)$ does not restrict $S(\tau_w, \tau_m)$ to be a function of total household income; as in Section 3, $S(\tau_w, \tau_m)$ is a function of both τ_m and τ_w. Supermodularity of $S(\cdot, \cdot)$ is preserved so long as $V(\cdot)$ is supermodular: ${}^{\partial S}/_{\partial \tau_w \partial \tau_m} = {}^{\partial V}/_{\partial \tau_w \partial \tau_m}$. Supermodularity will, however, not matter in this section, where I abstract from matching and focus on already formed couples.

that married spouses are both alive.

Social security. The social planner uses payroll taxes to fund a social security program that may or may not include survivors benefits. For simplicity, consider a payroll tax that collects *half* of an individual's income during his or her working life (Stage 1). Without survivors insurance, each retiree has a claim in Stage 2 to the equivalent of his or her accumulated payroll taxes. Because some men die without collecting this claim, the scheme runs a surplus, and the budget is balanced by a universal lump-sum benefit b in Stage 1. With survivors insurance, I assume that a widow receives $0.5\tau_w + \max\{0.25(\tau_m - \tau_w), 0\}$ in social security benefits, whereof $\max\{0.25(\tau_m - \tau_w), 0\}$ are survivors benefits. Notice that $0.25(\tau_m - \tau_w) = 0.25(\tau_m + \tau_w) - 0.5\tau_w$ is the difference between her own social security benefits and half of the joint social security benefits that the couple would receive if the husband were alive – exactly the scheme Sweden implemented pre-reform (see equation (1) in Section 2 above). With survivors benefits, a widow's utility is thus $U_A(\tau_w, \tau_m) = u(0.5\tau_w + \max\{0.25(\tau_m - \tau_w), 0\})$. When making its marital decisions, each atomistic couple takes into account any survivors benefits the woman may receive from the social planner, but the couple does not consider the impact of its marital decisions on the universal lump-sum benefits b, which are determined by the social planner's budget constraint $b = T - E(B)$, where T is the sum of all payroll taxes and $E(B)$ is the expected social security benefits, which, by the law of large numbers, I treat as deterministic.

Traditional society. I capture a "traditional" society, where women do not participate in the labor market, by letting $\tau_w = 0$ and $\tau_m = \bar{\tau} > 4$. One can think of $\tau_m - \tau_w$ as the gender income gap. First suppose there are no survivors benefits. The couple marries in Stage 2 if $S\left(1/2\tau_w, 1/2\tau_m\right) + \theta_2 \geq 0$ and in Stage 3 if $pV\left(0.5\tau_w, 0.5\tau_m\right) + (1-p)u(0.5\tau_w) + \theta_2 \geq u(0.5\tau_w) + pu(0.5\tau_m)$. In the traditional society, these conditions can be written

$$\theta_t \geq \underline{\theta}^{td} \equiv u\left(0.5\bar{\tau}\right) - V\left(0.5\bar{\tau}\right) \tag{15}$$

for $t = 2, 3$. Introducing survivors benefits changes the condition for marriage in Stage 3 to

$$\theta_3 \geq \underline{\theta}_s^{td} \equiv u(0.5\bar{\tau}) - V\left(0.5\bar{\tau}\right) - \frac{(1-p)u(0.25\bar{\tau})}{p}. \tag{16}$$

Not surprisingly, survivors benefits make marriage more attractive: $\underline{\theta}_s^{td} < \underline{\theta}^{td}$. At the same time, survivors benefits eliminate old-age poverty because widows now receive social security benefits in the amount of $0.25\bar{\tau} > 1$. In regard to the social planner's budget constraint in this society, note that $T = 0.5\bar{\tau}$ and

$$E(B) = \begin{cases} (1-p)0.5\bar{\tau} & \text{without survivors benefits} \\ (1-p)0.5\bar{\tau} + p\left[1 - G_{\theta_3}(\underline{\theta}_s^{td})\right]0.25\bar{\tau} & \text{with survivors benefits} \end{cases},$$

where $1 - G_{\theta_3}(\underline{\theta}_s^{td})$ is the share of the population that is married in Stage 3 given the existence of a survivors insurance scheme. So, the universal lump-sum benefits b are smaller in the presence of survivors benefits because benefits are being shifted to widows.

Modern society. I capture a "modern" society, in which men and women encounter more equal opportunities in the labor market but survivors benefits still matter, by letting $\tau_m > \tau_w > 2$ and $\tau_m + \tau_w = \bar{\tau}$, thus keeping total household income the same as in the traditional society. Intuitively, women earn a significant portion of the household income in this society, albeit still less than men. Without survivors benefits, the condition for marriage is

$$\theta_t \geq \underline{\theta}^{md} \equiv u(0.5\tau_w) + u(0.5\tau_m) - V(0.5\bar{\tau}) \tag{17}$$

for $t = 2, 3$. With survivors benefits, the condition in Stage 3 changes to

$$\theta_3 \geq \underline{\theta}_s^{md} \equiv u(0.5\tau_w) + u(0.5\tau_m) - V(0.5\bar{\tau}) - \frac{(1-p)u(0.25\lambda\bar{\tau})}{p} \tag{18}$$

where $\lambda\bar{\tau} = \tau_m - \tau_w$ and thus $\lambda \in (0, 1)$. Survivors benefits subsidize marriage, as before. But in this society, survivors benefits are superfluous as social insurance: the social security benefits tied to a woman's own labor income, $0.5\tau_w > 1$, are sufficient to keep her out of poverty in old age, even as a widow. The social planner's budget constraint is the same as in the traditional society, $T = \bar{\tau}$, but the expected social security benefits are now

$$E(B) = \begin{cases} (1-p)0.5\tau_m + 0.5\tau_w & \text{without surv. ben.} \\ (1-p)0.5\tau_m + 0.5\tau_w + p\left[1 - G_{\theta_3}(\underline{\theta}_s^{md})\right]0.25\lambda\bar{\tau} & \text{with surv. ben.} \end{cases}$$

where $1 - G_{\theta_3}(\underline{\theta}_s^{md})$ is the share of the population that is married in Stage 3 given the existence of a survivors insurance scheme. As before, b must be smaller in the presence of survivors benefits.

Comparison. Survivors benefits always tend to distort marriage decisions, but their social insurance benefits are larger when the spouses' incomes are so unequal that one spouse is cast into poverty if the other dies. Survivors benefits therefore serve a greater purpose in traditional societies. A comparison of (15)-(16) with (17)-(18) flushes out these differences more clearly. First, note in (16) and (18) that, since $\lambda\bar{\tau} < \bar{\tau}$, survivors benefits increase the value of marriage on the margin more in traditional societies. But that is precisely because, in view of the large gender income gap, the social insurance is so valuable. Second, note in (15) and (17) that by Jensen's inequality, $u(0.5\tau_w) + u(0.5\tau_m) > u(0.5\bar{\tau})$ for any $\tau_w = \bar{\tau} - \tau_m > 0$, so that $\underline{\theta}^{md} > \underline{\theta}^{td}$. This means that even in the *absence* of survivors benefits, couples are much more likely to get and stay married in traditional societies than in modern societies. If most people in society marry anyway, survivors benefits have *no meaningful* impact on the marriage margin.

To see this more clearly, suppose that "happiness" shocks are bounded below so that

$\tilde{\theta}_t \in (\underline{\theta}^{td}, \infty)$. In this case, all couples in the traditional society marry and none ever divorce. This provides a clean benchmark and captures a simple intuition: Women, who are not given the possibility to earn their own living in the labor market, prefer marriage – even "unhappy" ones – over living alone in poverty (on an income of $\tau_w = 0$). Nevertheless, without social insurance, a measure p of women become impoverished widows in Stage 2. This provides the rationale for survivors benefits. Moreover, as marriage is universal, survivors benefits introduce no distortion in the marriage market.

By contrast, couples in the modern society find marriage unattractive for $\tilde{\theta}_t \in (\underline{\theta}^{td}, \underline{\theta}^{md})$. Thus, not everyone marries, and some married couples divorce. This means that some couples' marital decisions are affected by survivors benefits – even though widowhood does not entail poverty. In other words, on the margin, couples that are "unhappy" choose to marry or remain married because of the financial implications of a social insurance system that no longer serves its income maintenance objective. In this modern society, survivors benefits are thus a purely distortive redistribution scheme that moves marital decisions away from the non-intervention optimum, promoting too many unions and discouraging desirable divorces. The social planner is better off abolishing survivors benefits.

Intermediate society and the social planner's trade-off. Last, I consider a society "in transition," where the population is divided into "modern" and "traditional" couples. Specifically, suppose the spouses in a fraction ϕ of couples earn "modern" wages $\tau_w^{md}, \tau_m^{md} > 2$, whereas the spouses in the remaining $1-\phi$ couples earn "traditional" wages $\tau_w^{td} = 0$ and $\tau_m^{td} > 4$. Further assume $\tilde{\theta}_t \in (\underline{\theta}^{td}, \infty)$ such that traditional couples are always married. The social planner now faces a trade-off: weighing the social insurance benefits in one part of the population against marriage market distortions in the other.

To obtain a policy rule, I need to be more specific about the social planner's preferences. Given that income maintenance is not a strictly welfarist objective, I consider a stylized objective function that weighs "income maintenance benefits" against "marriage market distortions": Suppose the social planner places a value z on each widow "saved from poverty" and weighs these benefits against losses in "happiness" in married couples. On one hand, the social benefit of survivors insurance is then $SB = (1-\phi)\,pz$, where $(1-\phi)\,p$ is the measure of impoverished widows that survivors benefits would lift out of poverty. On the other hand, the social cost of survivors insurance is

$$SC = \phi \Pr[\theta_3 \in (\theta_s^{md}, \theta^{md})] \left| \left[S(\tau_w^{md}, \tau_m^{md}) - E[\theta_3 | \theta_3 \in (\theta_s^{md}, \theta^{md}) \right]] \right|,$$

where $\Pr[\theta_3 \in \left(\theta_s^{md}, \theta^{md}\right)]$ is the share of modern couples whose marital decision is distorted by survivors benefits and $S(\tau_w^{md}, \tau_m^{md}) - E[\theta_3 | \theta_3 \in \left(\theta_s^{md}, \theta^{md}\right)] < 0$ is the loss of "happiness" these couples suffer from this distortion *net of survivors benefits.* The social planner prefers to abolish survivors benefits when $SC > SB$, which can be written

$$\phi > \phi^* \equiv \frac{pz}{pz + \Pr[\theta_3 \in (\theta_s^{md}, \theta^{md})] \left\{ S(\tau_w^{md}, \tau_m^{md}) - E[\theta_3 | \theta_3 \in (\theta_s^{md}, \theta^{md})] \right\}}.$$

The social benefit of survivors insurance is to protect $p\,(1-\phi)$ widows from poverty. The social cost, created by the wedge between (17) and (18), is to keep $\phi \Pr[\theta_3 \in (\theta_s^{md}, \theta^{md})]$ couples together that are "unhappy" enough to opt out of marriage in the absence survivors benefits. The higher is ϕ, the smaller are the social benefits and the larger the social costs; consequently, for high enough ϕ, survivors insurance is suboptimal. (This is only true *when* the marriage market responds; otherwise, the survivors insurance scheme entails social benefits, but no costs.)

Two main points emerge. First, whether marriage markets respond is an important factor in the optimal design of social insurance. Second, *in the presence of such marriage market responses*, the extent of female labor force participation is a key determinant for whether it is optimal, in a given society, to separate social insurance from marriage. Intuitively, the greater the share of women who do not work or earn enough in the labor market, the greater the share of women who, in the absence of survivors benefits, may end up impoverished in widowhood. Thus if – for reasons exogenous to the design of survivors insurance – the gender income gap decreases in society, the social benefits of survivors insurance gradually vanish. Moreover, when women work outside of the household and have their own claims to retirement income, couples no longer stay together for financial reasons when "happiness" dictates that they should part ways. As female labor force participation increases, not only do the social benefits from survivors insurance decrease, but the costs from marriage market distortions gradually increase. At some point, as societies transition from "traditional" to "modern," it becomes optimal to decouple social insurance from marriage.

9 Conclusion

Conditioning social insurance on a private contract can alter the economic behavior in this market. I model how linking social insurance to the marriage contract affects the marriage market, and provide empirical evidence exploiting Sweden's elimination of survivors insurance. First, I analyze bunching in the distribution of marriages and show that, by affecting the wedge between marriage and cohabitation, survivors insurance alters the composition of married couples up to 50 years before the annuity's expected payout. Insurance take-up is larger in couples with higher *ex post* male mortality, holding constant the policy's value at realization and all demographics that I observe, suggesting adverse selection into government-provided insurance. Second, I use a regression discontinuity design to show that removal of survivors insurance from existing marriage contracts caused divorces and an increase in husband labor supply, suggesting renegotiation of marital surplus in favor of the wife in surviving unions.

Third, I show that the removal of survivors insurance from the marriage contract increased the assortativeness of matching of highly skilled men, consistent with the fact that survivors insurance subsidized couples with highly unequal earnings (capacities). Such marriage market responses to social insurance design have important implications for when it is optimal to separate social insurance from marriage in modern societies.

These results yield three important general lessons about the economic behavior of couples. First, couples' marital behavior constitutes an important long-term financial strategy, complementary to asset accumulation, for ascertaining financial security in old age. In particular, the couples that I study display a substantial degree of forward-looking behavior, and apply financial-planning horizons of up to 50 years. Second, my results speak to the long-standing debate about whether pre-marital cohabitation is a learning process, and offer evidence in support of this interpretation. Third, my results suggest that institutions that encourage household specialization, such as survivors insurance, or even joint taxation, may actively counteract assortativeness of matching in society, which potentially has consequences for upward mobility and intergenerational persistence of inequality.

A number of important questions remain. Chief among them is how couples' fertility choices interact with their decisions along the three marriage market margins analyzed in this paper. While abstracting from fertility responses is natural in the context that I analyze – indeed, the data suggests that the elimination of survivors insurance left fertility decisions unaffected – decisions concerning fertility are a key aspect of couples' interaction, and as such an important avenue for future work in other contexts.

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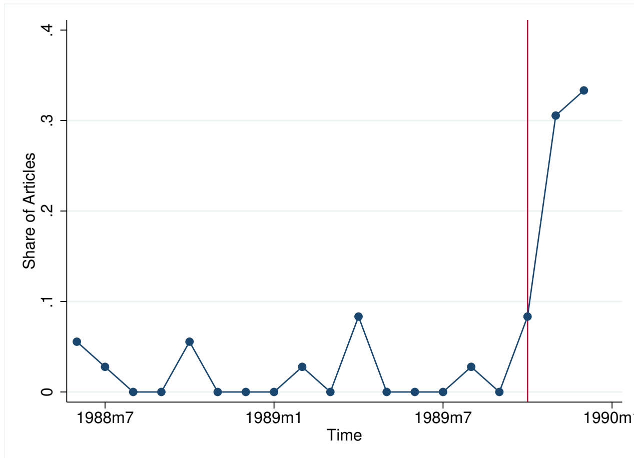
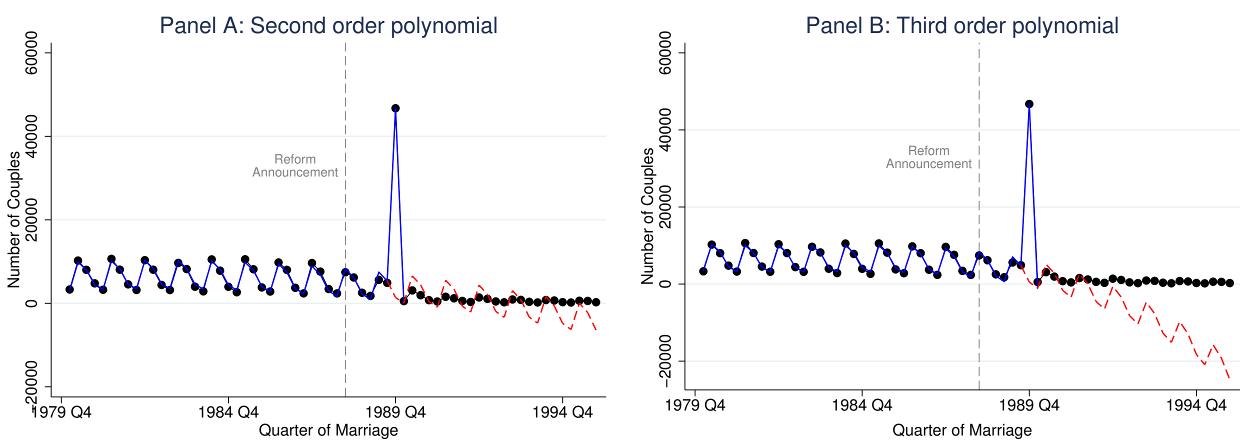
A Supplemental figures and tables

Figure A1: Media attention and reform salience: newspaper articles mentioning the reform



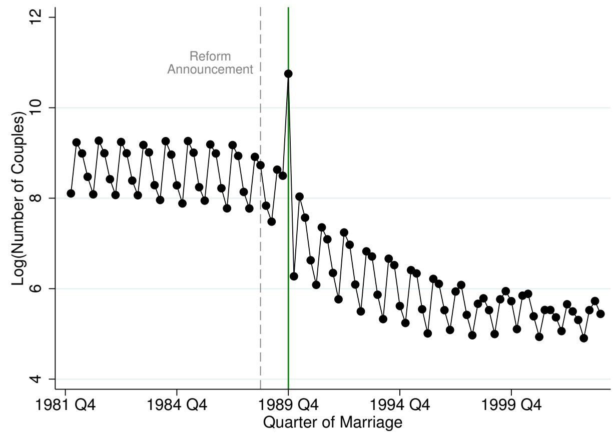
Note: The sample includes all articles published by Tidningarnas Telegrambyrå, Västerbottenskuriren, and Dagens Industri from June 1988 until December 1998. Media coverage, and hence salience of the reform, was concentrated in the last quarter of 1989.

Figure A2: Predicted and counterfactual marriage frequencies



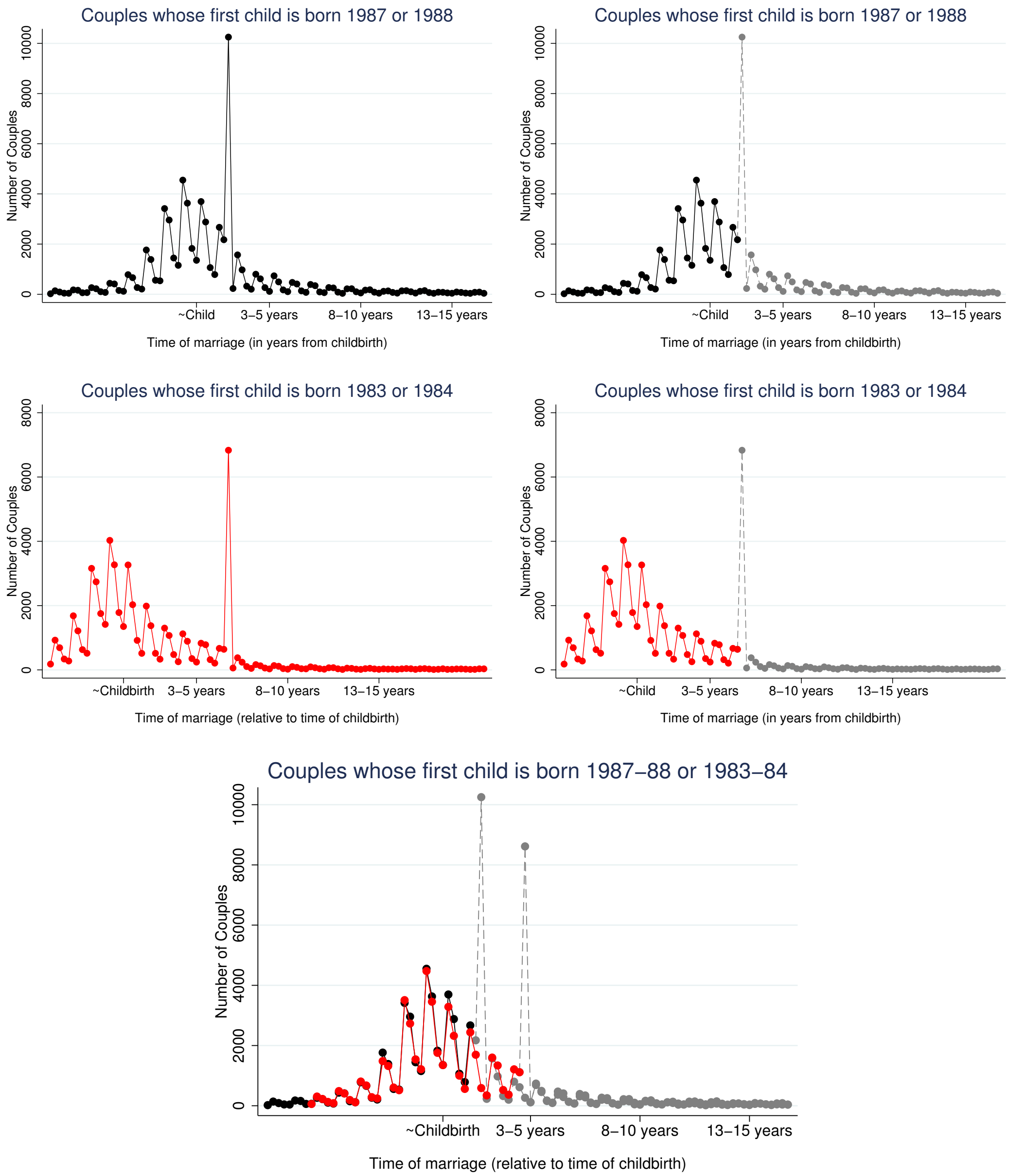
Note: The black connected line displays the empirical distribution of marriages at a quarterly frequency. The blue solid line depicts the predicted frequencies within the estimation sample, which includes all quarters before the new marriage contract was launched in 1990 Q1. The red dotted line depicts the predicted counterfactual marriage frequency (out of sample).

Figure A3: Empirical distribution of log (new marriages)



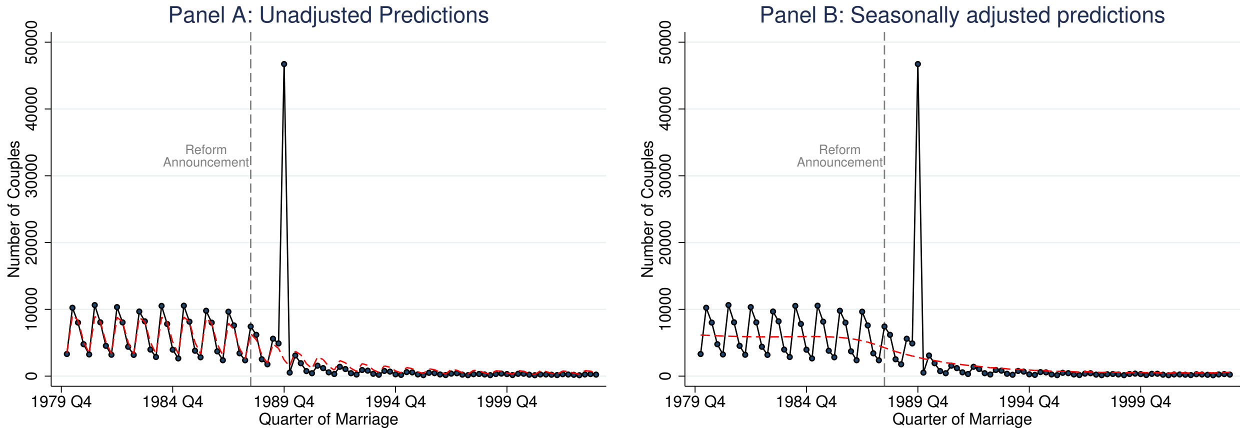
Note: The sample includes all couples that had a joint child between January 1, 1971 and January 1, 1989. The black connected dots depict the natural logarithm of the number of marriages at a quarterly frequency, from 1980 to 2003. The green solid vertical line indicates the last quarter during which marriage entailed survivors insurance. The grey dashed vertical line indicates the quarter of reform announcement.

Figure A4: Empirical strategy - intuition



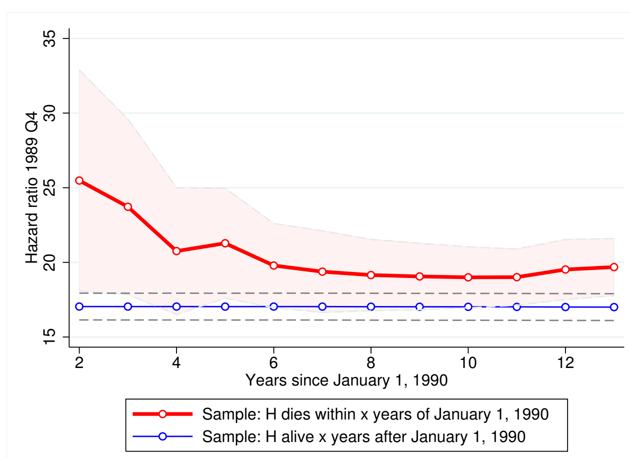
Note: The upper row replicates Panel A of Figure 3, which displays the empirical distribution of marriages among couples that had their first joint child in 1987 or 1988, but with the x-axis labeled in time since the birth of a couple's first joint child (instead of calendar time). The rightmost graph grays out marital behavior that takes place after the reform, and that thus cannot be used to predict post-reform marital behavior in the absence of reform. The middle row replicates Panel B of Figure 3, which displays the empirical distribution of marriages among couples that had their first joint child in 1983 or 1984. The third row lays the rightmost figures in rows one and two on top of each other, which illustrates how the estimation strategy exploits this second dimension of information – the date of birth of a couple's first child – by using early cohorts (row 2), whose marital behavior is observable for a longer period of time pre-reform, to help predict how the marital behavior of late cohorts (row 1) would have evolved in the absence of reform.

Figure A5: Predicted and counterfactual marriage frequencies



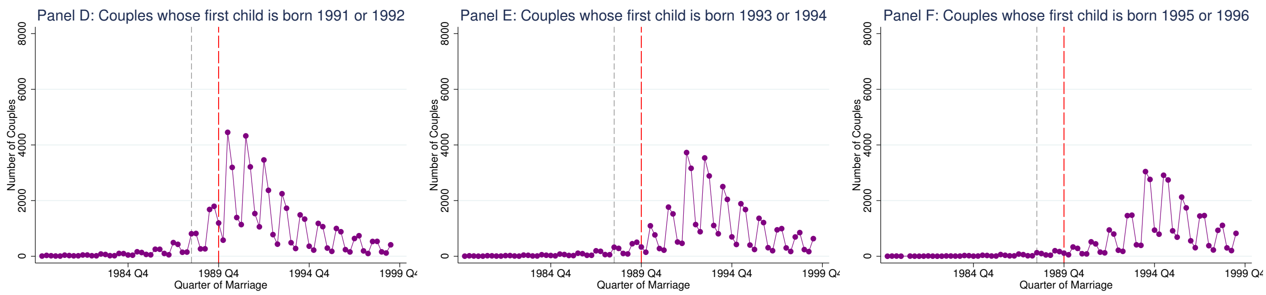
Note: The black connected line displays the empirical distribution of the marriage frequency at a quarterly frequency. The red dashed line depicts the estimated (unadjusted or seasonally adjusted) counterfactual marriage frequencies estimated using the specification that minimizes the Akaike Information Criterion (AIC). Panel A is reproduced in Figure 4.

Figure A6: Hazard ratio for different husband *ex post* mortality



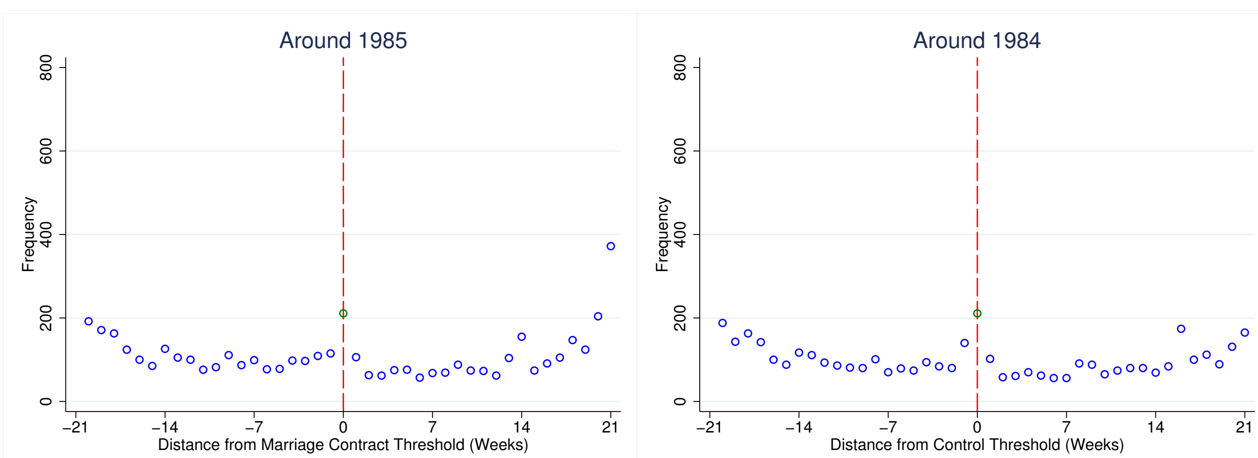
Note: The Figure plots the estimated hazard ratio for marriage in Q4 1989 for different samples, where each sample represents couples with a specific observed husband *ex post* mortality. Each point represents a separate regression. Each point on the red line displays the hazard ratio for marriage in Q4 1989 in a sample where the husband died within a certain time frame (number of years, indicated on the x-axis) of January 1, 1990. Each point on the blue line displays the hazard ratio in a sample where the husband did not die within the same time frame (but died, or will die, later). Coefficients generating this table are reported in Appendix Table A2. Areas represent 95% confidence intervals.

Figure A7: Empirical distribution of new marriages - specific “untreated” couple cohorts



Note: This Figure replicates panels A, B, and C in Figure 3, but for cohorts of couples that were “untreated” by the reform, i.e., for couples that had no incentive to rush to marry, because their first joint child was born after the threshold. To reflect the fact that this Figure essentially replicates panels A, B, and C in Figure 3, the panels are labeled D, E, and F. Panel D depicts new marriages among couples that had their first joint child in 1991 or 1992, and Panels E and F depicts new marriages among couples that had their first joint couples at later time intervals. The panels clearly illustrate that entry into marriage is concentrated around the date of birth of a couple’s first joint child. Importantly, there is no spike in marriages at the threshold, suggesting that responses were absent in groups without economic incentives, but who nonetheless likely experienced the media reporting of the reform.

Figure A8: Distribution of new marriages around 1985 (eligibility threshold) and 1984



Note: The figure displays the frequency of new marriages, in weekly bins, around January 1, 1985 and January 1, 1984. The green circles represents each year’s last week.

Figure A9: Distribution of covariates around eligibility threshold (1985)

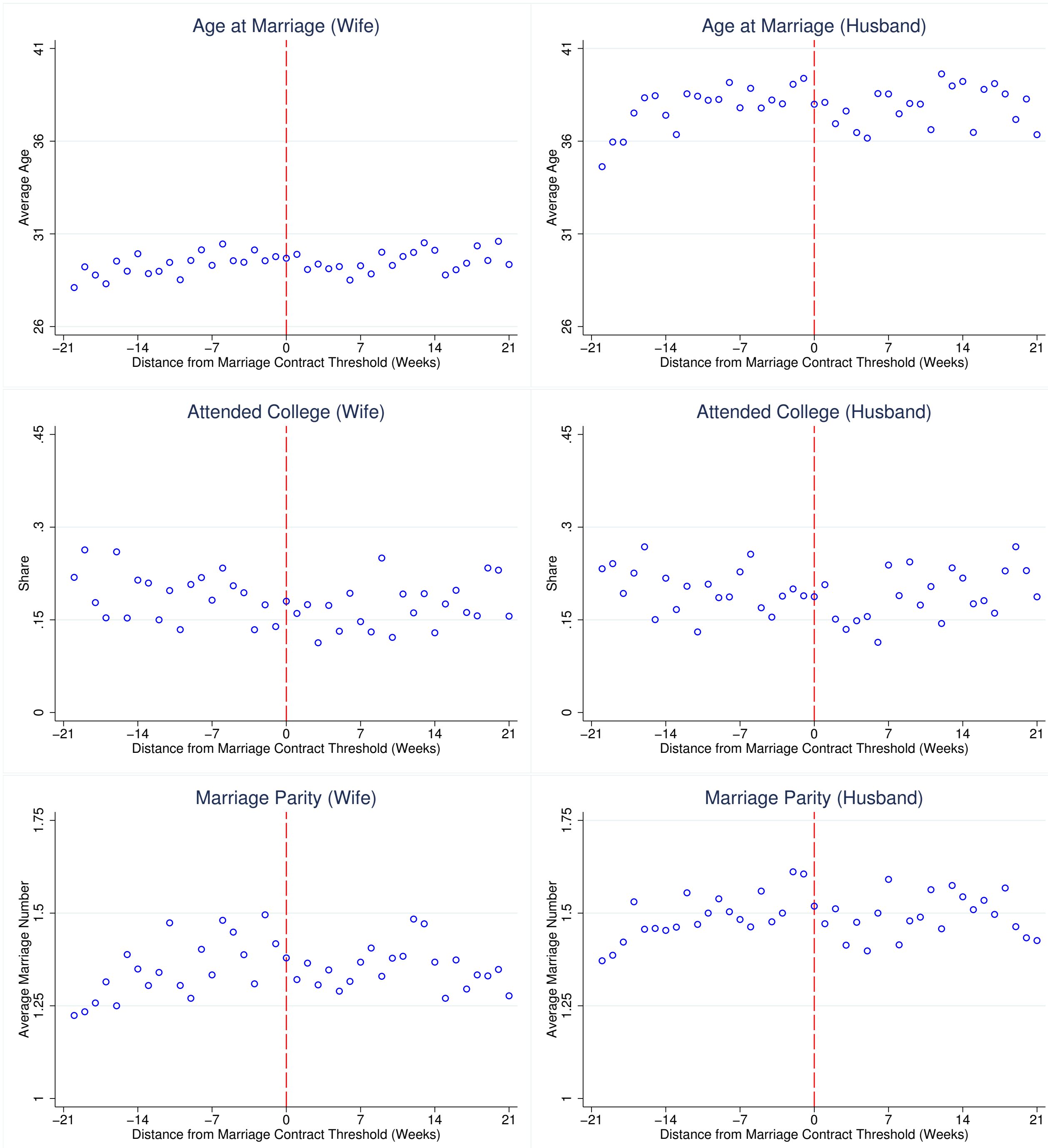
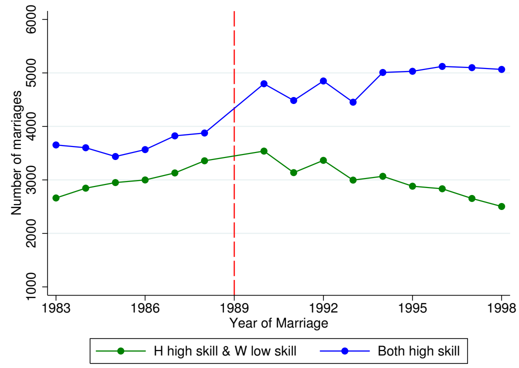
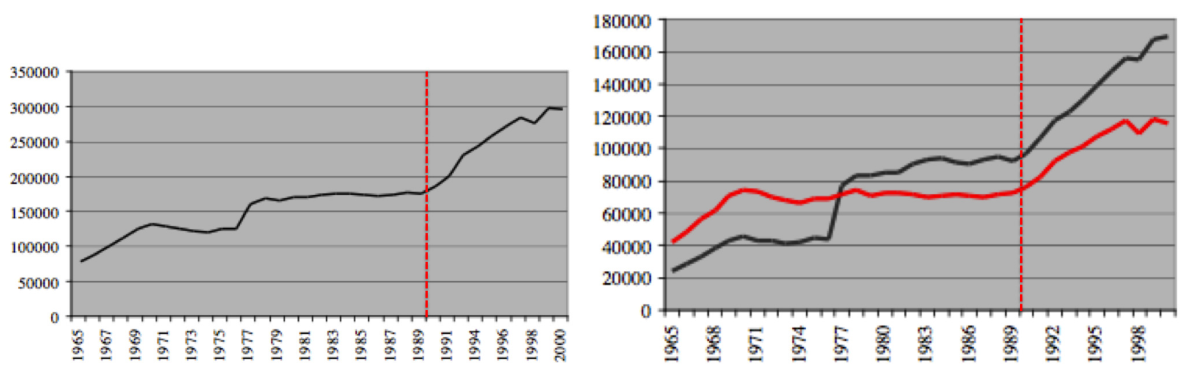


Figure A10: Marriages: The number of high-low and high-high matches



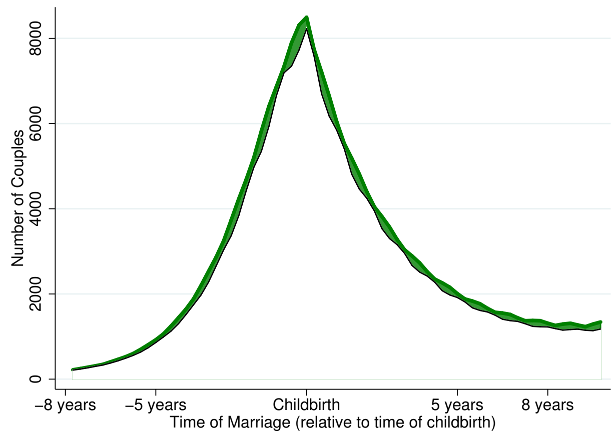
Note: The sample includes all couples that had a joint child and married between 1983 and 2000 (a time period during which the definition of educational attainment at marriage is constant around the 1989 threshold), but omitting the 1989, when marital behavior responded to the reform. The table displays the number of couples where the husband has a high educational attainment at marriage and the wife has either a low educational attainment at marriage (green solid line) or a high educational attainment at marriage (blue solid line).

Figure A11: The number of university enrollees (total and by gender)



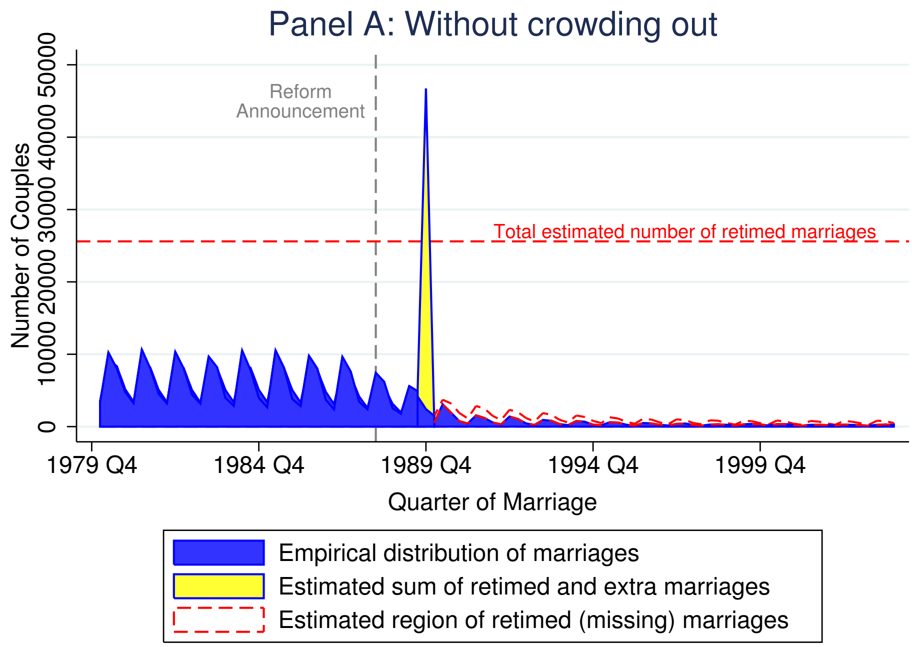
Note: The upper panel depicts the number of university enrollees by year. The lower panel depicts the gender breakdown, with the black solid line representing women and the red solid line representing men. Source: Högskoleverket. (The National Board of Higher Education.)

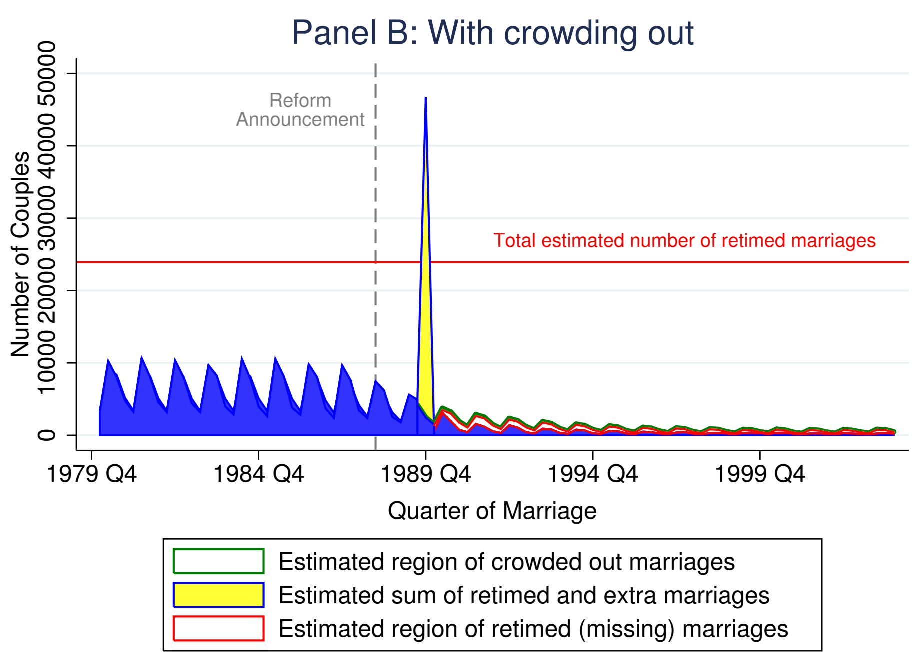
Figure A12: Crowding out



Note: The figure plots both the empirical distribution (black solid line) and the sample-wide counterfactual (green solid line) against distance from childbirth in the crowdout sample using a fourth order polynomial. The green area between the two solid lines represents crowding out due to the reform reducing the benefit from marriage.

Figure A13: Adding crowding out to baseline estimates of retiming and extra marriages





Note: Panel A replicates Figure 4. Panel B also depicts crowding out, the solid green area. Crowding out accounts for 1646 of the marriages that are counted as retimed in Panel A. Consequently, the number of extra marriages rises from 18705 to 20351 in Panel B.

Table A1: Marriage boom estimates

	Polynomial					
	2	3	4	5	6	7
Induced Marriages	45348	46195	46751	46634	46641	46745
Std error	(667)	(630)	(688)	(801)	(963)	(1192)
T-statistic	(68)	(73)	(68)	(58)	(48)	(39)
AIC	627.5	617.0	615.3	617.2	619.2	621.1
Number of obs (pre-reform quarters)	40	40	40	40	40	40
Number of couples	279124	279124	279124	279124	279124	279124

Note: Each column represents a separate regression, with a polynomial of the indicated degree. Estimates are obtained using only pre-reform data (all quarters before 1990 Q1). "Induced marriages" reports the estimated coefficient on $\mathbf{1}[s = s^*]$, which takes the value of one in 1989 Q4, with robust standard errors in parentheses. Each regression includes four quarter fixed effects. Instead of indicating significance with stars, I report standard errors and, for the number of induced marriages, t-statistics, which imply $p < 10^{-9}$ for all induced marriage estimates.

Table A2: Impact on marriage: heterogenous effects w.r.t. male *ex post* mortality

	Financial controls		All observable controls	
Estimates from sample	Coefficient, $\hat{\beta}$	Exponential, $e^{\hat{\beta}}$	Coefficient, $\hat{\beta}$	Exponential, $e^{\hat{\beta}}$
Husband dies within 2 years	3.08***	21.80***	3.24***	25.48***
	(0.16)	(3.50)	(0.18)	(4.51)
Number of couples	410	410	359	359
Husband alive after 2 years	2.83***	16.88***	2.84***	17.03***
	(0.03)	(0.52)	(0.03)	(0.55)
Number of couples	246636	246636	217788	217788
Husband dies within 3 years	2.93***	18.74***	3.17***	23.72***
	(0.14)	(2.55)	(0.15)	(3.58)
Number of couples	651	651	563	563
Husband alive after 3 years	2.83***	16.88***	2.84***	17.03***
	(0.03)	(0.52)	(0.03)	(0.55)
Number of couples	246395	246395	217584	217584
Husband dies within 4 years	2.93***	18.72***	3.03***	20.76***
	(0.12)	(2.20)	(0.12)	(2.59)
Number of couples	921	921	797	797
Husband alive after 4 years	2.83***	16.88***	2.84***	17.03***
	(0.03)	(0.52)	(0.03)	(0.55)
Number of couples	246125	246125	217350	217350
Husband dies within 5 years	3.00***	20.12***	3.06***	21.28***
	(0.10)	(1.93)	(0.11)	(2.24)
Number of couples	1214	1214	1051	1051
Husband alive after 5 years	2.83***	16.88***	2.83***	17.03***
	(0.03)	(0.52)	(0.03)	(0.55)
Number of couples	245832	245832	217096	217096
Husband dies within 12 years	2.94***	18.86***	2.97***	19.52***
	(0.06)	(1.10)	(0.06)	(1.23)
Number of couples	3901	3901	3385	3385
Husband alive after 12 years	2.82***	16.86***	2.83***	17.00***
	(0.03)	(0.52)	(0.03)	(0.55)
Number of couples	243145	243145	214762	214762

Note: This table replicates the lower panel of Table 4 for different male mortality samples. See the notes to Table 4.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A3: Hightened divorce risk in rushed marriages: probit results

	Dependent variable: Divorce within			
	3 years	5 years	10 years	15 years
Married in 1989Q4	0.0095***	0.0250***	0.0411***	0.0367***
	(0.0021)	(0.0042)	(0.0078)	(0.0086)
Demographic characteristics at marriage				
H age at marriage	-0.0005**	-0.0008**	-0.0019***	-0.0026***
	(0.0002)	(0.0003)	(0.0004)	(0.0005)
W age at marriage	-0.0024***	-0.0056***	-0.0099***	-0.0115***
	(0.0001)	(0.0002)	(0.0003)	(0.0005)
H education (4 lvls)	-0.0019**	-0.0055***	-0.0111***	-0.0116***
	(0.0007)	(0.0011)	(0.0027)	(0.0026)
W education (4 lvls)	-0.0082***	-0.0170***	-0.0318***	-0.0394***
	(0.0008)	(0.0013)	(0.0019)	(0.0021)
H cognitive capacity	-0.0058***	-0.0104***	-0.0173***	-0.0192***
	(0.0008)	(0.0009)	(0.0013)	(0.0014)
H second marriage	0.0119***	0.0262***	0.0715***	0.0889***
	(0.0021)	(0.0031)	(0.0056)	(0.0066)
W second marriage	0.0090***	0.0237***	0.0649***	0.0984***
	(0.0019)	(0.0035)	(0.0058)	(0.0060)
H is immigrant	0.0071*	0.0123*	0.0202**	0.0306**
	(0.0032)	(0.0057)	(0.0076)	(0.0097)
W is immigrant	0.0047*	0.0079	0.0044	0.0036
	(0.0021)	(0.0042)	(0.0048)	(0.0062)
Economic characteristics at marriage				
Total household labor income (H and W)	-0.0070***	-0.0175***	-0.0304***	-0.0355***
	(0.0007)	(0.0013)	(0.0022)	(0.0025)
H income share	0.0078	0.0348***	0.0673***	0.0723***
	(0.0040)	(0.0077)	(0.0103)	(0.0138)
W earns much more	0.0033	0.0110**	0.0257***	0.0277***
	(0.0022)	(0.0035)	(0.0052)	(0.0057)
H earns much more	0.0031	0.0064*	0.0084	0.0118*
	(0.0017)	(0.0032)	(0.0046)	(0.0050)
Total joint number of children				
Couple's completed fertility	-0.0337***	-0.0692***	-0.0991***	-0.0905***
	(0.0023)	(0.0047)	(0.0070)	(0.0075)
Number of obs	94681	94681	94681	94681

Note: The table presents average marginal effects. Each column represents a Probit regression with a different dependent variable. All regressions include wedding month fixed effects and wedding day of week fixed effects. Robust standard errors clustered on the (wedding month*wedding day of week) in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A4: Hightened divorce risk in rushed marriages: probit results

	Dependent variable: Divorce within			
	5 years	5 years	15 years	15 years
Married in 1989Q4	-0.0144***	0.0250***	-0.0329***	0.0367***
	(0.0041)	(0.0042)	(0.0094)	(0.0086)
Demographic characteristics at marriage				
H age at marriage		-0.0008**		-0.0026***
		(0.0003)		(0.0005)
W age at marriage		-0.0056***		-0.0115***
		(0.0002)		(0.0005)
H education (4 lvls)		-0.0055***		-0.0116***
		(0.0011)		(0.0026)
W education (4 lvls)		-0.0170***		-0.0394***
		(0.0013)		(0.0021)
H cognitive capacity		-0.0104***		-0.0192***
		(0.0009)		(0.0014)
H second marriage		0.0262***		0.0889***
		(0.0031)		(0.0066)
W second marriage		0.0237***		0.0984***
		(0.0035)		(0.0060)
H is immigrant		0.0123*		0.0306**
		(0.0057)		(0.0097)
W is immigrant		0.0079		0.0036
		(0.0042)		(0.0062)
Economic characteristics at marriage				
Total household labor income (H and W)		-0.0175***		-0.0355***
		(0.0013)		(0.0025)
H income share		0.0348***		0.0723***
		(0.0077)		(0.0138)
W earns much more		0.0110**		0.0277***
		(0.0035)		(0.0057)
H earns much more		0.0064*		0.0118*
		(0.0032)		(0.0050)
Total joint number of children				
Couple's completed fertility		-0.0692***		-0.0905***
		(0.0047)		(0.0075)
Number of obs	94681	94681	94681	94681

Note: The table presents average marginal effects. Each column represents a regression with a different dependent variable. All regressions include wedding month fixed effects and wedding day of week fixed effects. Robust standard errors clustered on the (wedding month*wedding day of week) level in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A5: IV (RDD) estimates: impact on wife's labor supply (extensive margin)

	Bandwidth 150 days		Bandwidth 180 days	
	Polynomial		Polynomial	
Wife working in	2	3	2	3
1988	-0.0290	-0.0338	-0.0223	-0.0307
	(0.0254)	(0.0276)	(0.0217)	(0.0240)
AIC	8791.09	8794.32	12128.59	12127.08
2000	0.0126	0.0122	0.0155	0.0157
	(0.0296)	(0.0322)	(0.0256)	(0.0284)
AIC	11624.61	11628.38	16590.63	16594.27
2004	0.0258	0.0223	0.0226	0.0208
	(0.0298)	(0.0324)	(0.0258)	(0.0286)
AIC	11702.42	11704.82	16766.94	16768.57
2008	0.0248	0.0226	0.0252	0.0329
	(0.0300)	(0.0326)	(0.0260)	(0.0288)
AIC	11821.48	11825.39	16967.25	16970.83
Number of observations	9104	9104	13400	13400

Note: Dependent variable: Wife labor force participation in the year indicated in column 1. Each cell represents a separate 2SLS regression. Results from regressions with labor force participation in 1992 and 1996, respectively, are omitted to save space; these effects are insignificant. Robust standard errors in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A6: Summary statistics: marriages of highly skilled men 1983 - 2000

	Old Marriage Contract		New Contract
Quarter of Marriage	1983Q1-1989Q3	1989Q4	1990Q1-1999Q4
H high skill but W low skill			
H age at marriage	30.67	36.22	31.43
W age at marriage	27.71	33.24	28.52
W education (4 lvls)	1.85	1.81	1.91
H cognitive capacity	0.75	0.55	0.67
Total log household labor income (H and W)	12.04	12.28	7.82
H income share	0.70	0.73	0.68
Couple's completed fertility	2.21	2.13	2.09
First child out of wedlock	0.39	0.91	0.44
Observations	21124	5044	29752
Both H and W high skill			
H age at marriage	31.77	37.78	31.88
W age at marriage	29.46	35.29	29.61
W education (4 lvls)	3.51	3.49	3.51
H cognitive capacity	0.87	0.64	0.86
Total log household labor income (H and W)	12.21	12.43	8.05
H income share	0.63	0.68	0.61
Couple's completed fertility	2.27	2.20	2.16
First child out of wedlock	0.32	0.91	0.30
Observations	25659	4936	49879

Note: The sample includes all couples that had a joint child and married between 1983 and 2000 (a time period during which the definition of educational attainment at marriage is constant around the 1989 threshold). The table displays summary statistics for couples where the husband has a high educational attainment at marriage, separately depending on whether the husband married a woman of low (upper panel) or high (lower panel) skill.

B Social Security in Sweden

This information is largely obtained from Palme and Svensson (1997). In Sweden, 74% of the income of individuals above the age of 65 constitutes Social Security income (this exact figure is from 1994). Only a small share is from private pension insurance; the remainder, instead, is mostly income from employer-provided pensions, centrally negotiated by unions.

All persons living in Sweden were entitled to a *basic pension* from the Social Security system, which was not tied to earnings history (but to the time of residence in Sweden). The size of the basic pension is linked to the "basic amount" (BA), which in turn is linked to the consumer price index. In 1988, the BA was SEK 25800, and the annual salary income among men in my sample used in Section 5.2 was SEK 110968.

In addition, *supplementary pension* (ATP) payments from Social Security were linked to earned income. For an individual who had obtained labor income in Sweden for 30 years prior to retirement, the supplementary social security benefit, b_h, was given by

$$b_h = 0.6 * (Income - BA) \text{ for } Income \in (BA, 7.5BA) . \tag{19}$$

Here, pension rights $Income$ is earnings and self-employment income recorded in the annual tax return, which I know for each year from 1985 to 2009. This is a lower bound of the pension rights income, however, which also includes income from sickness and unemployment insurance, parental leave benefits, and the partial retirement pension, neither of which I observe. The lower limit ascertains that supplementary pension is positive; the upper limit, 7.5 BA, is the social security ceiling. In 1988, the BA was SEK 25800, and the social security ceiling thus SEK 193500. In 1988, this corresponded to the 91th percentile in the distribution of salary income in the sample that I use in Section 5.2.

For individuals who worked in Sweden for $N < 30$ years prior to retirement, the supplementary social security benefit in (19) was multiplied by the function $min\left\{\frac{N}{30}, 1\right\}$. That is, the supplementary pension was reduced by $\frac{1}{30}$ for each year of work experience below 30. Finally, three years of positive pension-rights income between the ages of 16 to 65 were required to be eligible for supplementary pension.

The Social Security system is financed by employer contributions levied on wages. The level of all social contributions was 31.36 percentage points on gross earnings in 1994. The level of the contribution for the national basic pension was 5.86, for the supplementary pension (ATP) 13.00, and for the part-time pension 0.20 percentage points. General tax revenues partially finance the national basic pension. The payments from these systems (basic and supplementary) amounted to 42.4 percent and 55.3 percent of total pension payments in 1994.

C Mathematical proofs

Lemma 1

Proof. Under transferable utility, a matching is stable if and only if it maximizes total surplus. A solution to this maximization problem thus exists by the Bolzano Weierstrass theorem, because (i) total surplus function M_{total} is continuous in $\{\tau_w,\tau_m\}$, and (ii) the function is maximized over the set of matching allocations $\{\tau_w, \tau_m\}$, which is compact. □

Results in subsection 3.3.1 The formal result driving this prediction is as follows:

Lemma 2. *Supermodularity of $V(\tau_w, \tau_m)$ implies supermodularity of $M(\tau_w, \tau_m)$ if $U_A(\tau_w, \tau_m) = 0$; but $M(\tau_w, \tau_m)$ may fail to be supermodular if $U_A(\tau_w, \tau_m)$ is decreasing in τ_w, given τ_h.*

Proof. In Stage 1, the expected value of match is $M(\tau_w, \tau_m) = \delta A(\tau_w, \tau_m) + \delta^2 B(\tau_w, \tau_m)$ where

$$A(\tau_w, \tau_m) = \alpha(\tau_w, \tau_m)\,[V(\tau_w, \tau_m) + E[\theta_2|\theta_2 \geq \theta_{NSB}(\tau_w, \tau_m)]]$$

and

$$B(\tau_w, \tau_m) = \beta(\tau_w, \tau_m)\,[(1-p)\,[V(\tau_w, \tau_m) + E[\theta_3|\theta_3 \geq \theta_{SB}(\tau_w, \tau_m)]] + pU_A(\tau_w, \tau_m)]$$

For $U_A(\tau_w, \tau_m) = 0$, we get $\theta_{SB}(\tau_w, \tau_m) = -V(\tau_w, \tau_m) = \theta_{NSB}(\tau_w, \tau_m)$, so that $\alpha(\tau_w, \tau_m) = \beta(\tau_w, \tau_m)$ and $A(\tau_w, \tau_m)$ becomes identical to $B(\tau_w, \tau_m)/(1-p)$ except that the shocks $\tilde{\theta}_2$ and $\tilde{\theta}_3$ are drawn from different distributions. Showing that $A(\tau_w, \tau_m)$ is supermodular, or $\partial A(\tau_w, \tau_m)/\partial\tau_w\partial\tau_m \geq 0$, for an arbitrary distribution F would then imply that $B(\tau_w, \tau_m)/(1-p)$ is also supermodular. This in turn would imply that $M = \delta A(\tau_w, \tau_m) + \delta^2 B(\tau_w, \tau_m)$ is supermodular, since positively linear combinations of supermodular functions are supermodular. Now write

$$A(\tau_w, \tau_m) = \int_{-V(\tau_w,\tau_m)}^{\infty} [V(\tau_w, \tau_m) + \theta_2]\, f(\theta_2) d\theta_2.$$

Then,

$$\begin{aligned}\frac{\partial A(\tau_w, \tau_m)}{\partial \tau_w} &= -[V(\tau_w, \tau_m) - V(\tau_w, \tau_m)]\, f(-V(\tau_w, \tau_m)) \left(\frac{-\partial V(\tau_w, \tau_m)}{\partial \tau_w}\right) \\ &\quad + \int_{-V(\tau_w,\tau_m)}^{\infty} \left[\frac{\partial V(\tau_w, \tau_m)}{\partial \tau_w}\right] f(\theta_2) d\theta_2\end{aligned}$$

Simplifying yields

$$\frac{\partial A(\tau_w, \tau_m)}{\partial \tau_w} = \int_{-V(\tau_w,\tau_m)}^{\infty} \frac{\partial V(\tau_w, \tau_m)}{\partial \tau_w} f(\theta_2) d\theta_2.$$

The second derivative is thus given by

$$\frac{\partial^2 A\left(\tau_w,\tau_m\right)}{\partial\tau_w\partial\tau_m}=\frac{\partial V\left(\tau_w,\tau_m\right)}{\partial\tau_w}f\left[-V\left(\tau_w,\tau_m\right)\right]\frac{\partial V\left(\tau_w,\tau_m\right)}{\partial\tau_m}+\int_{-V(\tau_w,\tau_m)}^{\infty}\frac{\partial V\left(\tau_w,\tau_m\right)}{\partial\tau_w\partial\tau_m}f(\theta_2)d\theta_2>0.$$

By contrast, for $U_A\left(\tau_w,\tau_m\right)\neq 0$, $B(\tau_w,\tau_m)/(1-p)$ is not isomorphic to $A(\tau_w,\tau_m)$ and need not be supermodular. To show this, I use the fact that, for functions on R^2, supermodularity is equivalent to *increasing differences*. Function $B(\tau_w,\tau_m)$ has increasing differences if

$$\frac{\partial B(\tau_w',\tau_m)}{\partial\tau_m}\geq\frac{\partial B(\tau_w,\tau_m)}{\partial\tau_m}\quad\forall\tau_w'>\tau_w,\forall\tau_m. \tag{20}$$

Write

$$B(\tau_w,\tau_m)=\int_{\theta_{SB}(\tau_w,\tau_m)}^{\infty}g(\theta_3)\left[(1-p)\left[V(\tau_w,\tau_m)+\theta_3\right]+pU_A\left(\tau_w,\tau_m\right)\right]d\theta_3.$$

The derivative with respect to τ_m is

$$\frac{\partial B(\tau_w,\tau_m)}{\partial\tau_m}=\int_{\theta_{SB}(\tau_w,\tau_m)}^{\infty}g(\theta_3)\left[(1-p)\frac{\partial V(\tau_w,\tau_m)}{\partial\tau_m}+p\frac{\partial U_A\left(\tau_w,\tau_m\right)}{\partial\tau_m}\right]d\theta_3.$$

I now prove by example that this derivative need not satisfy (20). Consider a large p or a V-function where $\partial V/\partial\tau_m$ is very small everywhere, such that the first term in the bracket, $(1-p)\partial V/\partial\tau_m$, is negligible. In this case, the derivative is largely determined by the second term:

$$\frac{\partial B(\tau_w,\tau_m)}{\partial\tau_m}\approx\int_{\theta_{SB}(\tau_w,\tau_m)}^{\infty}g(\theta_3)p\frac{\partial U_A\left(\tau_w,\tau_m\right)}{\partial\tau_m}d\theta_3.$$

Fix τ_m. Because $\frac{\partial U_A(\tau_w',\tau_m)}{\partial\tau_m}=0$ for $\tau_w'>\tau_m$ and $\frac{\partial U_A(\tau_w,\tau_m)}{\partial\tau_m}>0$ for $\tau_w<\tau_m$, (20) is violated:

$$\frac{\partial B(\tau_w',\tau_m)}{\partial\tau_m}\approx 0<\frac{\partial B(\tau_w,\tau_m)}{\partial\tau_m}\quad\text{for }\tau_w'>\tau_w,\tau_m\in(\tau_w,\tau_w')$$

Thus, $B(\tau_w,\tau_m)$ does not have increasing differences everywhere and is hence not supermodular. □

Results in subsection 3.3.2 I formulate the results regarding couples that were matched but unmarried at the reform's announcement in the following Proposition:

Proposition 1 (Reform announced in Stage 2.)**.** *Consider all couples not yet married at the reform announcement that become eligible for survivors benefits if and only if they marry during a limited time period, that is, within Stage 2. First, selection into marriage in Stage 2 is stronger when the man is more likely to die in Stage 3 (higher p). Second, the reform induces some marriages in Stage 2 that would otherwise occur in Stage 3, but also some* *marriages that would never occur without the reform. Third, couples that the reform induces to marry are more likely to divorce.*

Proof. First, I show that selection into marriage increases with p. For this, I need only show that the option value Ω_A increases in p. Write

$$\Omega_A = \delta \left[B_{\theta_2}(\tau_w, \tau_m) - (1-p) A_{\theta_2}(\tau_w, \tau_m) \right] \tag{21}$$

where

$$\begin{aligned} A_{\theta_2}(\tau_w, \tau_m) &= \int_{\theta_{NSB}(\tau_w, \tau_m)}^{\infty} \left[V(\tau_w, \tau_m) + \theta_3 \right] g_{\theta_2}(\theta_3) d\theta_3 \\ B_{\theta_2}(\tau_w, \tau_m) &= \int_{\theta_{SB}(\tau_w, \tau_m)}^{\infty} \left[(1-p) \left[V(\tau_w, \tau_m) + \theta_3 \right] + p U_A(\tau_w, \tau_m) \right] g_{\theta_2}(\theta_3) d\theta_3. \end{aligned}$$

Recall that $\theta_{NSB}(\tau_w, \tau_m) > \theta_{SB}(\tau_w, \tau_m)$. Substituting $A_{\theta_2}(\tau_w, \tau_m)$ and $B_{\theta_2}(\tau_w, \tau_m)$ into (21) and rearranging yields

$$\Omega_A = \delta p \int_{\theta_{SB}(\tau_w, \tau_m)}^{\infty} U_A(\tau_w, \tau_m) g_{\theta_2}(\theta_3) d\theta_3 + \delta (1-p) \int_{\theta_{SB}(\tau_w, \tau_m)}^{\theta_{NSB}(\tau_w, \tau_m)} \left[V(\tau_w, \tau_m) + \theta_3 \right] g_{\theta_2}(\theta_3) d\theta_3,$$

where the first integral is positive and the second integral is negative (by the definitions of $\theta_{NSB}(\tau_w, \tau_m)$ in (4)).

The derivative with respect to p is

$$\Omega_A = \delta \int_{\theta_{SB}(\tau_w, \tau_m)}^{\infty} U_A(\tau_w, \tau_m) g_{\theta_2}(\theta_3) d\theta_3 - \delta \int_{\theta_{SB}(\tau_w, \tau_m)}^{\theta_{NSB}(\tau_w, \tau_m)} \left[V(\tau_w, \tau_m) + \theta_3 \right] g_{\theta_2}(\theta_3) d\theta_3 > 0.$$

Second, I show that the increase in marriages during the grandfathering period comprises retimed marriages and, *given uncertainty about* θ_3, extra marriages. Among those couples that the reform *causes* to marry in Stage 2, some will get shocks $\theta_3 \geq \theta_{SB}$ in Stage 3 and hence would have married in Stage 3 in the absence of the reform. However, some will get shocks $\theta_3 < \theta_{SB}$, and hence would not have married in Stage 3. Now suppose that θ_3 is known already at the beginning of Stage 2. In that case, couples with $\theta_3 < \theta_{SB}$ do not want to marry in Stage 2 only to keep the option on survivors benefits alive. This is because they will certainly not exercise the option, since they already know that they do not want to be married in Stage 3, even when eligible for survivors benefits. The aforementioned "extra marriages" during the grandfathering period would thus not happen.

Third, I show that the divorce rate increases. From inspection of (**??**), it is clear that the couples that the reform induces to marry in Stage 2 have lower V or lower θ_2 than those that marry even absent the reform. For a given θ_2, a couple is less likely to satisfy (3) for lower V. And for a given V, a couple is less likely to satisfy (3) for lower θ_2 because $G_{\theta_2}(\theta_3)$ first-order stochastically dominates $G_{\theta_2'}(\theta_3)$ for all $\theta_2 > \theta_2'$. $\square$

Results in subsection 3.3.3

I formulate the results regarding couples that were unmarried at the reform's announcement in the following Proposition:

Proposition 2 (Reform announced in Stage 3.)**.** *Consider all couples already married at the reform announcement. In couples that remain married, the wife's share of household utility (weakly) increases.*

Proof. In married couples, let $u_3^i(\tau_m, \tau_w)$ denote spouse i's utility in Stage 3 if the husband is alive under the contract entered at marriage. These utilities satisfy

$$E\left[u_3^w(\tau_m, \tau_w)\right] = (1-p)\gamma\left[V\left(\tau_m, \tau_w\right) + \theta_3\right] + pU_A\left(\tau_m, \tau_w\right) \geq 0$$

and the husband wants to remain in marriage if and only if

$$E\left[u_3^m(\tau_m, \tau_w)\right] = (1-p)\left[1-\gamma\right]\left[V\left(\tau_m, \tau_w\right) + \theta_3\right] \geq 0.$$

For some shocks θ_3, the reform – the abolition of survivors benefits – causes the wife's expected utility from marriage to be smaller than her outside utility:

$$(1-p)\gamma\left[V\left(\tau_m, \tau_w\right) + \theta_3\right] + pU_A\left(\tau_m, \tau_w\right) \geq 0 > (1-p)\gamma\left[V\left(\tau_m, \tau_w\right) + \theta_3\right].$$

By contrast, the husband's expected utility is unaffected by the reform under any contract. Thus, if anyone, the wife may seek a separation after the reform. In such a state, there is scope for efficient renegotiation if

$$(1-p)\left[V\left(\tau_m, \tau_w\right) + \theta_3\right] \geq 0 > (1-p)\gamma\left[V\left(\tau_m, \tau_w\right) + \theta_3\right].$$

To keep the wife in the marriage, the husband would then have to agree to a new sharing rule $\hat{\gamma} > \gamma$ such that $(1-p)\hat{\gamma}\left[V\left(\tau_m, \tau_w\right) + \theta_3\right] \geq (1-p)\underline{u}_w$. $\square$

D Additional material

D.1 Incorporating crowding out

In this section, I augment my empirical framework presented in Section 5.1 to also allow for estimation of the reduction in entry into marriage that occurred after 1990 precisely because the reform reduced the benefit from marriage. I here refer to this effect as "crowding out," to distinguish it effect from the (non-standard) extensive margin effect that I refer to as "extra marriages" throughout the paper.

Sample. In addition to the baseline sample, I now add a "crowdout sample," which consists of couples who had their first joint child after 1990 (but before 1999). These couples could not respond to the reform on the marriage margin – they were ineligible for survivors insurance from 1990 and onwards regardless of whether they married before or after January 1, 1990. In the absence of spillovers, we would therefore not expect any bunching in the last quarter of 1989 in these cohorts. By extension, we would not expect any retiming. However, these couples were affected by the reform in the sense that it reduced the surplus from marriage after 1990: for all these couples, marriage came without survivors insurance from January 1, 1990.[44] As I explain below, this sample – combined with the baseline sample – allows for estimation of crowding out.

Figure A7 plots the empirical distribution of new marriages at a quarterly frequency for three different subsets of the crowdout sample. Panel A depicts entry into marriage in the subsample of couples whose first joint child was born in 1991 or 1992, and Panels B and C depict couples whose firstborns were born in later time periods. Two features are noteworthy. First, the distributions do not display any bunching in the last quarter of 1989. Second, other than the absence of bunching, the distributions display similar features as the empirical distributions of new marriages in the baseline sample depicted in Figure 3. In particular, they exhibit a seasonal pattern with more marriages in the spring and summer. And importantly, entry into marriage is concentrated around childbirth, and the distributions display similar changes relative to the time of childbirth as we observe in the baseline sample. This suggests that we can use cohorts of couples whose first joint child is born after 1990 as a comparison group, and exploit the fact that they had no incentives to retime their marriages in response to the reform.

Specification. As before, I divide my baseline sample into 72 cohorts, where each cohort $c \in \{1, 72\}$ consists of couples whose first joint child was born in a given quarter, where $c = 1$ represents the first quarter of 1971, $c = 2$ the second quarter of 1971, and so on. In addition, I now add the crowdout sample. Because it includes couples whose first joint child was born during 32 quarters, from the first quarter of 1991 until the last quarter of 1998, I divide it into 32 cohorts, $c \in \{73, 104\}$. Because the baseline sample was "treated" by the reform, I now refer to the set of cohorts in the baseline sample as $T = \{1, 72\}$, whereas I refer to the

[44]This is true so long as the couples entered marriage after January 1, 1985, when the five-year rule that I exploit in the RDD analysis of existing marriages in Section 6 starts to apply. (This five-year rule guarantees that couples that entered marriage before January 1, 1985 remain covered by survivors insurance after 1990.) As Panel A of Appendix Figure A7 shows, entry into marriage before 1985 was negligible in among couples that had their first joint child after 1990. Nonetheless, results are unchanged if I predict crowding out using only marriages in the crowdout sample that took place after 1985.

crowdout sample as untreated cohorts. I estimate the following regression:

$$n_{cs} = \alpha + \eta_c + \zeta_q + g(s) + h(t_{pre-birth}) + j(t_{post-birth}) + \mathbf{1}_{c\in T}\left[\beta_c\left(\mathbf{1}\left[s = s^*\right]\right) + \right.$$
$$\left.\gamma_c\left(\mathbf{1}\left[s > s^*\right]\right) + \mathbf{1}_{c\notin T}\left(\theta_c\left(\mathbf{1}\left[s > s^*\right]\right)\right) + \varepsilon_{cs},\right.$$

where n_{cs} is the natural logarithm of N_{cs}, the number of marriages in quarter s in cohort $c \in \{1, 104\}$, $n_{cs} = ln(N_{cs})$, η_c are cohort fixed effects for $c \in \{1, 104\}$; ζ_q capture seasonality; $g(s)$ is a higher order polynomial in time (quarter); and the functions $h(t_{pre-birth})$ and $j(t_{post-birth})$ are higher order polynomials in the number of quarters before and after the first child's birth, respectively.

As in (6), for all treated cohorts $c \in T$, the β_c capture the cohort-specific increases in entry into marriage at the eligibility threshold, $s = s^*$, and the γ_c capture the corresponding reductions in entry after this threshold. In the presence of crowding out, the γ_c thus capture the sum of the reduction in entry that is due to retiming and the reduction in entry that is due to crowding out. For all untreated cohorts $c \notin T$, however, the reductions in entry after s^*, captured by θ_c, only reflect crowding out. Put differently, the cohorts $c \notin T$ entered marriage at the "old marriage contract"-rate before s^*; thereafter, they entered at a lower rate, and the decline fully reflects the crowding out.

After estimating this regression, I obtain the predicted arithmetic cohort-specific frequencies, $\hat{N}_{cs}$, as in Section 5.1. To predict cohort-specific counterfactual frequencies, $\hat{K}_{cs}$, I set $\mathbf{1}\left[s = 1989q4\right]$ and $\mathbf{1}\left[s > 1989q4\right]$ equal to zero. For each cohort c, each point in calendar time – i.e., each quarter s – corresponds to a certain distance (in quarters) from childbirth:

$$d(s; c) = s - a^c, \tag{22}$$

where a^c denotes the quarter of childbirth for couples in cohort c. Thus, the counterfactual frequencies $\hat{K}_{cs}$ can be redefined as $\hat{K}_{cd}$, which captures the cohort-specific predicted counterfactual frequencies at distance d from childbirth. I then aggregate the $\hat{K}_{cd}$ into sample-wide counterfactual frequencies, separately for the treated and untreated cohorts, by distance to childbirth, $\hat{K}_d^{c\notin T} = \sum_{c\notin T} \hat{K}_{cd}$ and $K_d^{c\in T} = \sum_{c\in T} \hat{K}_{cd}$. Similarly, I redefine N_{cs}, the number of marriages in quarter s in cohort c, as N_{cd}, and aggregate these.

For each distance from childbirth d, pure crowdout (i.e., without any retiming) is given by $\left(\hat{K}_d - N_d\right)$ in the crowdout sample. Because crowding out per se is relative to the actual marriage frequency in a cohort, and because entry into marriage has been declining over time in Sweden (which is captured by the cohort-fixed effects), it is suitable to express crowding out as a share of the predicted counterfactual (pre-reform) marriages at distance d from childbirth. For a range D of distances from childbirth, crowding out as a share of pre-reform marriages is given by $C\left(D\right) = \frac{\sum_{d\in D}^{c\notin T}\left(\hat{K}_d - N_d\right)}{\sum_{d\in D}^{c\notin T}\hat{K}_d}$. To obtain standard errors, I again use the cluster bootstrapping procedure described in Section D.2.

Results. Figure A12 plots both the empirical distribution (black solid line) and the sample-wide counterfactual (green solid line) against distance from childbirth in the crowdout sample using a fourth order polynomial. Relative to the empirical distribution, the predicted frequency distribution entails "excess mass" after 1990, which represents crowding out. It is noteworthy that the shape of the distribution around childbirth displayed in Figure A12 is tighter around childbirth than the distributions displayed in Figure A7. This is purely graphical: Figure A7 displays marital frequencies for subsets of couples that had their first joint child during a two year period, that is, for eight cohorts (quarters) at a time. Moreover, the distribution displayed in Figure A12 shows a higher absolute number of births because the 32 cohort-specific frequencies have been aggregated (instead of 8 cohort-specific frequencies at a time as in Figure A7).

The green area between the two solid lines represents crowding out, $C(D)$, for distances to birth in the interval $D \in \{-32, 40\}$. For this range of distances, crowding out is estimated to be 0.0645, or 6.45 percent. If I instead calculate crowding out using only positive distances from childbirth – motivated by the fact that all treated cohorts experience crowding out only after childbirth – I obtain a similar estimate of crowdout, 6.43 percent. These estimates suggest that, because the reform reduced the benefit from marriage, it crowded out approximately six percent of all marriages.

Panel A of Appendix Figure A13 replicates Figure 4. Panel B of Appendix Figure A13 also depicts crowding out, the solid green area. Crowding out accounts for 1646 of the marriages that are counted as retimed in Panel A. Consequently, the number of extra marriages rises from 18705 to 20351 in Panel B.

D.2 Bootstrap procedure

I use the following bootstrapping procedure to obtain standard errors. Because I use panel data (where cohort c is the panel variable and quarter s the time variable), I use a cluster bootstrapping procedure, with 10000 samples drawn with replacement. Specifically, starting from the collapsed data, I create each new sample by drawing (each of the s observations for) 72 cohorts with replacement. Each of my randomly drawn samples thus corresponds to a panel with 72 cohorts (with seasonality preserved). If an entire panel is drawn twice, the two draws are treated as different panels (samples). For each of the 10000 samples, I estimate each of the statistics. The standard error for each of the statistics is estimated by computing the standard deviation of the 10000 estimates of this statistic.

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