Social Insurance and the Marriage Market*

Petra Persson†

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Abstract
Social insurance is often linked to marriage. Existing evidence suggests small marital responses to financial incentives and stems from settings where benefits are realized in the near future. I analyze how linking survivors insurance (SI) to marriage affects the marriage market. Exploiting Sweden’s elimination of SI, I demonstrate that severing this link (i) affected entry into marriage up to 50 years before expected payout; (ii) raised the divorce rate by ten percent; and (iii) raised the assortativeness of matching. This suggests that marital behavior is a key component of couples’ strategies to plan for financial security in old age.

JEL classification: J12, H31, D13

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†Stanford University Department of Economics, NBER, CEPR, IFAU, and IFN. Email: perssonp@stanford.edu.
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 one: Who contracts with – that is, marries – whom?

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 this annuity, but that otherwise was legally identical.

Informed by a theoretical model that analyzes how couple formation, marital decisions, and spousal welfare depend on the link between survivors insurance and marriage, Section 3 presents predictions for how an elimination of survivors insurance affects forward-looking individuals in the marriage market. First, the elimination of survivors’ insurance from the marriage contract alters the long-run steady state marriage market equilibrium. Second, for individuals who are exposed to a transition between the two regimes, the impact of the elimination of survivors insurance depend on whether the individual, at the time of the reform’s announcement, is married, matched but yet unmarried, or unmatched. For each group of individuals, I present 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.

In Section 5, I start by exploring the long-run consequences of eliminating survivors insurance from the marriage contract. First, comparing couples that initiate cohabitation well before and well after the reform
suggests that the reform is associated with a reduction in entry into marriage, consistent with it decreasing the surplus from marriage relative to cohabitation. Second, as fewer cohabiting couples choose to enter marriage after 1990, we should observe an altered quality composition of cohabiting and married couples. Intuitively, when the (match quality) hurdle for entering marriage increases, the average match quality among cohabiting couples falls and the average match quality among married couples rises. Indeed, consistent with this prediction, the elimination of survivors insurance from the marriage contract is associated with an increase in the steady state rate of separation from cohabiting unions.

My second set of results, presented in Section 6, concerns how tying social insurance to marriage affects entry into marriage among couples who were affected by the transition. 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 45000 marriages take place in the last quarter of 1989. I estimate that a couple is on average 21 times more likely to marry in this quarter than it would have been if the reform had not taken place. This translates into an elasticity of marriage take-up with respect to financial incentives that exceeds existing estimates from other contexts. Importantly, even couples below the age of 30 exhibit significant responses, which implies financial-planning horizons as long as 50 years.

While part of the marriage boom is accounted for by couples who retim their marriages, I show that approximately 47 percent of these marriages never would have occurred in the absence of reform. The reform thus induced “extra” marriages relative to a state of the world where the old marriage contract had remained in place. Further, I hypothesize that the hastened sorting process in the grandfathering period yields a lower average match quality in marriage-boom marriages. Indeed, I show that marriages in the boom are five percentage points (20%) 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. I first document stronger responses among couples with observable characteristics that imply a higher expected value of survivors insurance. I then show that, even conditioning on the observables that determine the annuity’s value, couples with a higher husband mortality risk at the time of reform – captured in the data by ex post realized mortality – respond more strongly to the reform. The positive correlation between couples’ risk type and take-up of insurance (through marriage) may partly explain why private annuities markets are underdeveloped in Sweden at the time of reform.

In Section 7, 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 survivors insurance coverage. 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 difference-in-differences design that exploits both the eligibility cutoff and

\footnote{Intuitively, the reform attaches option value to marrying fast; this causes couples to rush to marriage whom otherwise would have waited. Some of these couples would, in the absence of reform, never have decided to marry.}
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 ten percent.

Finally, Section 8 analyzes how tying social insurance to marriage affects the assortativeness of matching. 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); consequently, I hypothesize that removing this subsidy 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 skilled men that enter assortatively matched marriages increases by four percentage points (11%) following the introduction of the new marriage contract. This suggests that survivors insurance linked to marriage promoted spousal specialization in market and non-market work.

The stated aim of legislators in creating social security systems that confer spousal benefits, both in the United States and in Sweden, was to insure constituents – notably widows with little (previous) labor force attachment – against poverty in old age. One central policy implication of the findings in this paper is that the social planner may face a trade-off between this stated aim, on the one hand, and distorting marriage market decisions, on the other. While some existing evidence suggests that such distortions may not be important, this paper highlights that tying social insurance to marriage has interconnected and economically far-reaching impacts across four key margins of behavior in the marriage market.²

This paper makes several contributions. First, even the existing studies that detect significant marital responses³ typically report elasticities that are substantially smaller than the ones that I document among couples who were incentivized to marry to secure survivors insurance. One candidate interpretation of these large responses is that they merely represent relabeling. 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, default custodial rights of children, and the division of assets in case of separation. Thus, converting a cohabiting union into marriage has substantial real economic implications.⁴

Second, the existing evidence on different margins of marriage market behavior almost exclusively stem from different studies, of different contexts.⁵ To the best of my knowledge, this paper is the first to take a holistic perspective of the marriage market by simultaneously analyzing behavioral responses across all three stages of the mating process: matching, entry into marriage, and exit from marriage.

²Existing evidence on how marital behavior responds to penalties or subsidies inherent in tax and benefit schemes is mixed, with some studies documenting statistically significant responses and others failing to do so. For example, in the context of the United States, Alm and Whittington (1995), Whittington and Alm (1997), and Bitter et al. (2006) find that a smaller marriage tax penalty increases the rate of marriage relative to divorce or non-marriage; in contrast, Bitler et al. (2004) and Fitzgerald and Ribar (2005) fail to robustly document any statistically significant responses. See Moffitt (1998) and Alm et al. (1999) for surveys of the literature. Outside of the United States, see, e.g., Frimmel et al. (2014) who show that the removal of cash transfers upon marriage temporarily raises the marriage rate in the context of Austria. A smaller literature has analyzed marital responses to incentives inherent in the social security system. In the context of the United States and Canada, respectively, Brien et al. (1996) and Baker et al. (2004) show that widows delay remarriage so as not to lose survivors insurance payments. Further, a few studies analyze whether couples’ delay divorce in response to the 10-year eligibility threshold for spousal benefits in the United States, and find little or no response to this incentive (Dickert-Conlin and Meghea, 2004; Shah Goda et al., 2007; Dillender, 2016).

³See the discussion in footnote 2.

⁴I speculate that the difference may stem from the significant media attention to the reform, which ensured that the financial reward from entering marriage was salient; moreover, couples with children (who were the ones incentivized to enter marriage fast) may be more responsive to financial incentives, as these unions are more stable on average.

⁵Chiappori et al. (2017) study a reform of alimony laws in Canada and find that it has distinct impacts on cohabiting and married couples; they do not study impacts on matching or exit from marriage, however.
Third, while the previous literature has studied responses along the entry and exit margins, there is very little evidence on how taxes and benefits affect the assortativeness of matching. The literature on matching has established preferences for non-meritocratic attributes such as race (e.g., Fisman et al. (2008) and Lee (2016)), caste (e.g., Banerjee et al. (2013)), and social status (e.g., Abramitzky et al. (2011)) in matching. This paper breaks new ground by presenting evidence suggesting 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.

Fourth, the literature has hitherto focused on documenting responses to benefits that are realized immediately or in the near future. I instead document responses to benefits that only pay out in the far future. This enables me to point to a general lesson about couples’ economic behavior, namely, that marriage market behavior constitutes one important long-term financial planning mechanism. This connects the paper to a large literature that analyzes individuals’ strategies for ascertaining financial security in old age; naturally, this literature focuses on individuals’ savings behavior. The current paper shows that marital decisions, too, are an integral 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.

Finally, and related to the above discussion, the paper contributes to the literature on adverse selection in annuities markets. 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 even if insurance companies would observe (and hence be able to price on) all the observable characteristics that influence the value of the annuity – as well as a range of demographic and socioeconomic characteristics that do not directly affect the annuity’s value – adverse selection would likely arise in such a private market. This suggests that government provision of annuities, in the form of survivors benefits, may remain in many countries partly because adverse selection hinder private annuities markets to develop.

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Further, there is little evidence on how taxes and benefits affect entry into marriage from cohabitation (as opposed to the overall marriage rate) and the assortativeness of matching. The absence of evidence on cohabitation stems from a difficulty to identify cohabiting couples in most datasets – a hurdle that I am able to overcome by leveraging detailed Swedish administrative data. One exception is the studies that show retiming of divorce around the 10-year eligibility rule in the United States to secure survivors insurance coverage; however, these studies find either no or small responses (Dickert-Conlin and Meghea, 2004; Shah Goda et al., 2007; Dillender, 2016).

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, as women younger than 36 could not collect benefits) 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 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. As in the U.S., earned benefits were proportional to lifetime earnings up to a ceiling.\(^{10}\) A widow who was between 36 and 64 years old got a (monthly) survivors benefit equal to 40% of the husband’s earned benefit. For a widow who was 65 years or older, the survivors benefit also depended on her own earned benefit. 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. 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, the benefit was increasing in the difference between the spouses’ earned benefits. Given the spouses earned benefits and the wife’s discount rate, the value of the annuity was increasing in the 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, which I take to be the reform announcement date. The transition rule specified that *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. I 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.\(^{11}\)

“Effective” reform announcement date and the absence of fertility responses  The transition rule created incentives both for couples to enter marriage and for couples to have a joint child between the reform announcement and January 1, 1990. With the reform announcement in June 1988, childless couples would in principle have time to (try to) conceive a joint child. In practice, however, the data suggests that entry into parenthood was unaffected by the reform. Figure 1a plots the number of couples who have a first joint child, at a quarterly level, around the reform. The empirical distribution is smooth around the threshold.\(^{12}\)

One potential reason for the absence of immediate fertility responses is that knowledge of the reform was not widespread immediately after June 8, 1988. Figure 1b displays the distribution of mentions of the reform in three media outlets, and shows that coverage was concentrated in the last quarter of 1989 – by then, it was too late to conceive a joint child in response to the reform announcement. This means that the “effective” reform announcement date – when the population at large obtained knowledge of the reform – may have been the last quarter of 1989, rather than the second quarter of 1988. Consistent with this interpretation, as we will see below, the empirical distribution of marriages, depicted in Figure 3a, shows that the marriage boom is concentrated in the last quarter of 1989. Nonetheless, I take a conservative approach and treat June 1988 as the reform announcement.\(^{13}\) Given that childbearing was unaffected by the reform, I henceforth focus on marriage market responses.

2.2 Differences between cohabitation and marriage in Sweden

Other than the right to social security survivors benefits, the central legal distinctions between marriage and cohabitation in Sweden at the time of the reform concern inheritance rights, the division of assets in case of separation, and the default custodial rights over children.\(^{14}\)

_Inheritance._ A surviving spouse has a right to inheritance, but a cohabiting partner does not. It is generally difficult for cohabiting couples to write a testament that fully replicates marriage in this regard.\(^{15}\)

_Separation or divorce._ In case of divorce, married individuals’ assets are considered marital property

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\(^{11}\)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. For couples where the wife was born after 1945, the value of the annuity is calculated using the husband’s benefit as it would have been calculated at the time of the reform.

\(^{12}\)I also test whether there is any discontinuity at the threshold. Consistent with the graphical evidence, I do not find any discontinuity at the threshold in the likelihood of having a first joint child, or in the number of children.

\(^{13}\)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, Hoem (1991), Andersson (1998), and Roine (1997) document the marriage spike in Sweden in 1989.

\(^{14}\)Most taxes and benefits were decoupled from marriage since 1971, when joint taxation of labor income was eliminated.

\(^{15}\)While the marriage contract remained largely unchanged since 1974, minor changes were made in 1988, in particular concerning inheritance rights among couples with children outside of marriage (with someone else than the spouse).
by default, whereas cohabiting individuals' assets are considered separate property upon separation, with the possible exception of the apartment or house that the cohabiting couple lives in.\textsuperscript{16} Moreover, the law stipulates a right to alimony payments for the economically disadvantaged spouse upon divorce, but not upon separation from cohabitation.\textsuperscript{17}

The rights over 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, paternity is established for essentially all children born outside of marriage\textsuperscript{18}; the key distinction thus concerns the default custodial arrangement. This can be altered to joint custody on the paternity establishment form, and according to Statistics Sweden, of all children who live with two unmarried but cohabiting parents, the parents de facto have legal joint custody in 98 percent of the cases.\textsuperscript{19} Thus, \textit{so long as couples live together}, their legal marital status is, in practice, immaterial for their rights over children. When it comes to the rights over children after separation, however, the parents' legal marital status before separation may matter. The law stipulates that a judge's decision regarding custody be in the "best interests of the child," and while the law makes no explicit mention of whether the parents were married or unmarried prior to separation, judges may respond to this information when determining what is in the child's best interest.

The welfare of children. While the marriage contract has legal consequences for children, it is not a priori clear whether, in practice, there are any differences between the children of cohabiting and married couples – cohabitation is, after all, socially accepted in Sweden at the time of the reform, and childbearing outside of marriage is common (a fact that I return to in Section 6.2 below). In Appendix C, I examine whether two key child outcomes differ depending on the parents' marital status: educational attainment and the share of children that live with the mother of father, respectively, after parental separation. For both sets of outcomes, the data demonstrate important differences between children of cohabiting and married parents. It is not clear, however, whether these differences reflect selection or a causal impact of marriage. Indeed, Table A1 compares cohabiting and married \textit{couples}, and shows evidence of some degree of advantageous selection into marriage; whether it can account for the entire difference between children of married and cohabiting couples is outside of the scope of this paper.

\textsuperscript{16}Specifically, if the cohabiting couple lives in a property that was (i) acquired during the cohabitation period and (ii) acquired with the intent of being used jointly, then this asset should be divided equally at separation. All other assets are considered private property for cohabiting couples. This is the default during the sample period after 1988; cohabiting couples were free to alter it through a private contract.

\textsuperscript{17}While married couples can write a prenuptial agreement specifying that all assets be considered separate property, it is hard for cohabiting partners to replicate marriage by writing a contract where one partner commits to making financial transfers to the other in case of separation, as it may not be enforced by court.

\textsuperscript{18}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. Mothers also 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, 2017), paternity establishment is not an issue in the Swedish context.

\textsuperscript{19}My data does not contain information on legal custody, but Statistics Sweden report aggregate statistics for certain years. These exact figures are from 2001, the earliest year for which I observe aggregate statistics.
3 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, presented in Appendix D. Here, I briefly summarize the model’s predictions, and the basic intuition driving them.\(^{20}\)

### 3.1 Impacts on the long-run marriage market steady state

I start by comparing two regimes, one in which all marital decisions are made in the presence of survivors insurance, and one in which all marital decisions are made without it. This illustrates the impact of an elimination of survivors insurance on the long-run steady state marriage market equilibrium (SS).

**Prediction SS1: Steady state marriage rate.** Survivors’ insurance tied to marriage raises the surplus from marriage relative to cohabitation. Consequently, the reform reduces the surplus from marriage, so a couple who is on the margin of entering marriage in a regime with survivors insurance chooses to cohabit in a regime without it. This lowers the steady state rate of entry into marriage from cohabitation.

**Prediction SS2: Steady state quality of cohabiting unions.** When the marriage rate declines, the average quality of cohabiting unions falls. Intuitively, this is because the couple who is at the margin of entering marriage in a regime without survivors insurance has a higher (expected) match quality than the couple who is at the margin of entering marriage in a regime without it.

### 3.2 Impacts stemming from the marriage market regime transition

For individuals who are exposed to a transition between the two regimes, the impact of the elimination of survivors insurance depend on whether the individual, *at the time of the reform’s announcement*, is married, matched but yet unmarried, or unmatched.

**Transition I: Matched but unmarried couples** Couples that were matched but unmarried (MU) at the reform’s announcement could take up survivors benefits within a limited grandfathering period, by entering marriage.

**Prediction MU1: Marriage boom.** The incentives to marry in the grandfathering period in order to harvest the expiring benefits generates a marriage boom. The underlying mechanism is that the price of waiting, and hence of learning more about the quality of the match before deciding whether to marry, rises discontinuously on December 31, 1989. This induces cohabiting couples that would have continued to cohabit in the absence of reform to instead enter marriage in the end of 1989.

**Prediction MU2: Retimed and extra marriages.** The couples that rush to marry in the boom can, *ex post*, be divided into two groups. First, some of them would have waited but, at some point after 1989, learned that the quality of their match was high enough to warrant marriage. *Ex post*, these marriage-boom marriages thus turn out to simply have been retimed. Second, some of the couples that choose to marry in the boom would never have chosen to marry in the absence of the reform. Intuitively, these couples would have waited and subsequently learned that the quality of the match, in fact, was too low to warrant entering marriage.

In addition to the predictions presented here, the theoretical model yields an additional prediction for matched and married couples, concerning the reform’s impact on the division of marital surplus. Appendix E.6 presents this prediction and attempts to provide some empirical evidence on it using spousal labor supply.
into (the old) marriage contract. *Ex post*, the second group of marriage-boom marriages will turn out to be “extra” in the precise sense that the couples were induced to marry by the reform even though they never would have opted to marry into this (old) marriage contract if it had remained in place after 1989. Both retimed and extra marriages thus stem from inter-temporal substitution effects.

*Prediction MU3: Heterogeneous responses and economic incentives.* Couples are more likely to enter marriage in the grandfathering period, the greater the annuity’s expected value. This expected value depends on the couple’s age structure, relative income shares, and the husband’s likelihood of death.

*Prediction MU4: Higher long-run divorce rate in marriage-boom marriages.* The strong incentives to marry in the grandfathering period hastens the sorting process. This reduces the average quality of matches in the transition, which implies a higher future divorce rate in boom marriages.

Transition II: Matched and married couples Consider couples that were already matched and married (MM) when the reform was announced, and that faced an *ex post* elimination of survivors insurance.

*Prediction MM1: Marital instability.* When insurance is removed from marriage, marital surplus falls. This induces some married couples to divorce.

Transition III: Unmatched and unmarried individuals Consider individuals that were unmatched and unmarried (UU) at the reform’s announcement.

*Prediction UU1: Assortativeness of matching.* Elimination of survivors insurance from the marriage contract raises the assortativeness of matching. This is because, 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.

4 Data

*Population-level data.* I merge administrative data from various registers compiled by Statistics Sweden. For the universe of individuals aged 16 and above, I observe month and year of birth, educational attainment, employment status, and taxable labor income for the years 1985 - 2009. For the universe of individuals that entered marriage between 1968 and 2009, I observe the complete marital history, immigration status, and exact death date. For each child born in Sweden since 1971, I observe the exact birth date and the mother and father ID. While relationship codes link spouses together, this data is of rather poor quality. I therefore link spouses using the child ID whenever couples have children.

*Cohabitation sample.* The predictions concerning steady state marital behavior should arise without conditioning on children. However, in the population-level data, there are no relationship codes that allow identification of childless couples that cohabit. I therefore create a sample of cohabiting couples that does not condition on the presence of children (or on marriage). Specifically, for each year from 1981 until 2000,
I create a sample of couples that initiate cohabitation – i.e., move in together – in that particular year. To do this, I use an address panel that allows me to manually link individuals into cohabiting couples. Because the addresses correspond to households only for individuals who live in houses (and not in, e.g., apartment buildings or elderly homes), I obtain a sample of cohabiting couples (as opposed to the universe of cohabiting couples).21

5 Survivors insurance and marriage market steady states

Predictions SS1 and SS2 concern the impact of the reform on the marriage market steady state. Because they concern all cohabiting couples, and not just couples with children, I use the cohabitation sample, which allows me to study couples that enter cohabitation nine years before the reform and ten years thereafter. Summary statistics of this sample are described in Appendix Table A1.22

5.1 Prediction SS1: Steady state marriage rate

Prediction SS1 concerns the steady state rate of entry into marriage from cohabitation. Figure 2a displays the shares of couples that marry within 3, 5, and 7 years of moving in together, respectively, by the year of initiation of cohabitation. Intuitively, couples that initiate cohabitation in a particular year are deciding whether to enter marriage or not, trading off the costs and benefits of marriage relative to cohabitation. Among couples that move in together in 1981, approximately 35 percent marry within 3 years. By prediction SS1, because the reform reduces the surplus from marriage relative to cohabitation, it should lower the steady state rate of entry into marriage. This prediction concerns behavior in the marriage market well before and well after the survivors' insurance reform, and not the transition between regimes (which I analyze in subsequent sections).

In Figure 2a, couples are directly affected by the reform within 3, 5, and 7 years of 1990, respectively; the dashed portion of each line represents these transition years. To gauge a change in steady state behavior, we should thus restrict attention to the solid portions of the lines.23 The figure provides visual evidence suggesting that the marriage rate was declining during the pre-reform period, but that the reform induced a long-run drop relative to this trend. Indeed, fitting a linear trend to the pre-reform data points that are unaffected by the reform, and predicting into the post-reform period, suggests that the three-year marriage rate falls by an average of 6.2 percent over the years 1992 to 2000 relative to the post-reform marriage rate predicted from the pre-reform data.24

This is merely a “sanity check” of SS1 – it relies on a simple linear prediction out of sample25; moreover, the estimate is sensitive to the choice of polynomial. In Section 6.2 I estimate the impact on long-run steady state marriages using population-level data for a larger range of years. While that analysis is restricted to

21While I observe the address panel data from 1975 to 2009, the quality is poor prior to 1981. Further, the panel ends in 2000 (9 years prior to 2009) in order to capture the outcome “separation within 9 years.” Additional details on this sample are provided in Online Appendix E.1.
22This table displays summary statistics for couples that initiate cohabitation between 1985 and 2000, because I only observe educational attainment and income starting in 1985.
23The dots in the figure illustrate marital behavior among couples that initiate cohabitation at the cusp of the reform, in 1991.
24Appendix Figure A1a displays the three-year marriage rate along with the linear prediction.
25If I instead use the marriage rate within other time periods (4, 5, 6, or 7 years) of initiation of cohabitation, I obtain estimates between 4 and 7 percent.
couples with children, I am able to leverage a methodology that assuages these concerns and delivers a more robust estimate. Interestingly, in Section 6.2, I obtain an estimate of the long-run steady state reduction in entry into marriage of 5.6 percent, which is close to the one obtained from this simple “sanity check” of SS1.

5.2 Prediction SS2: Steady state quality of cohabiting unions

Prediction SS2 concerns the quality of cohabiting unions, and follows directly from SS1: As the marriage rate declines post-reform (SS1), the average quality of cohabiting unions should fall. By SS2, we thus expect the elimination of survivors insurance from the marriage contract to raise the steady state rate of separation from cohabiting unions. In Figure 2b, the sample is restricted to the subset of couples who did not enter marriage within two years of moving in together. The figure displays the share of such couples that move apart, within 5, 7, and 9 years, respectively, by the year of initiation of cohabitation. The dashed portion of each line represents couples that are affected by the transition between survivors insurance regimes (more specifically, that are incentivized to marry fast); the solid portions of each line thus capture separation behavior among couples who initiate cohabitation before or after the reform. SS2 suggests that, after 1990, we should observe a worse average match quality among cohabiting couples, i.e., a higher rate of separation. The raw data in Figure 2b indeed offers suggestive evidence in support of this conjecture.

6 Survivors insurance and selection into marriage

The next four predictions concern the impact of (removal of) survivors insurance on entry into marriage.

6.1 Prediction MU1: Marriage boom

To be entitled to survivors insurance beyond 1990, a couple needed to be married and have a joint child on or before December 31, 1989. The reform thus provided all couples that had at least one child on or before December 31, 1989, with an incentive to enter marriage by the dead-line; other couples faced no such incentive. Importantly, as shown in Section 2.1 above, entry into parenthood was unaffected by the reform. Nonetheless, I define the sample of couples that were incentivized to marry fast as those whose first joint child was born before 1989 (and starting in 1971, when the child data starts).\footnote{Children born before 1989 were conceived before the reform announcement in June 1988.} I refer to this as the treated sample.

Figure 3a displays the empirical distribution of marriages in the treated sample at the quarterly level, from 1980 through 2003, and the first column of Table 1 displays summary statistics for these couples. Consistent with prediction MU1, the distribution displays substantial bunching – a marriage boom – in the last quarter of 1989. The raw data also reveals a seasonal pattern, with more marriages in spring and summer. Prediction MU1 stipulates that the marriage boom is driven by couples’ incentives to secure survivors insurance coverage. Another interpretation relates to the fact that media featured the law change prominently in the last quarter of 1989, which may have induced couples to enter marriage as a form of herding behavior. To shed light on this, Figure 3b displays the empirical distribution of marriages among
couples whose first joint child was conceived after the elimination of survivors insurance. These couples faced no economic incentives to marry fast; I therefore refer to this sample as the untreated sample. Figure 3b shows no spike in marriages in the last quarter of 1989, suggesting that economic incentives were central to the boom. Interestingly, Figures 3a and 3b reveal that couples’ tend to enter marriage close to the date of birth of their first joint child, an empirical fact that I return to, and exploit, in Section 6.2 below.

To quantify the marriage boom in Figure 3a, I estimate the extent of bunching at the notch. Bunching methodologies predict how a manipulated distribution would have looked, had there been no manipulation, by using un-manipulated parts of the distribution to help “fill in” the shape inside any manipulated regions. The key underlying assumption is that, in the absence of manipulation, the distribution would follow the polynomial that can be estimated from the unmanipulated parts of the distribution. The usual case, however, is one where manipulation is confined to a small part of the distribution. In contrast, in Figure 3a, the density never “resumes” a pattern similar to that before the reform. I therefore start by using only pre-reform data in the treatment sample, i.e., marriages that were entered before the first quarter of 1990. In the spirit of Saez (2010), Chetty et al. (2011), and Kleven and Waseem (2012), I estimate the following regression:

\[ N_s = \alpha + \beta (1 [s = s^*]) + g(s) + \zeta_q + \varepsilon_s, \]

where \( N_s \) is the number of marriages in quarter \( s \); \( 1 [s = s^*] \) is an indicator variable that takes the value of one in the last quarter of 1989, \( s = s^* = 1989q4 \); the function \( g(s) \) is a higher order polynomial in time (quarter); and \( \zeta_q \) are quarter fixed effects. Intuitively, I fit a polynomial to the counts before 1990 Q1 plotted in Figure 3a, accounting for seasonality, and include an indicator variable for the last quarter of 1989. Here, \( \beta \) measures the size of the marriage boom in the last quarter of 1989. All estimates are in the range of 46000 marriages.

6.2 Prediction MU2: Retimed and extra marriages

Next, I want to decompose the marriage boom into retimed and extra marriages. To fix ideas, Figure 4 provides a simple sketch of how the theory predicts that retimed and extra marriages (prediction MU2), as well as the steady state reduction in entry into marriage (prediction SS1), appear in the empirical distribution. The black area illustrates a hypothetical observed distribution of marriages, and the dashed line shows its counterfactual distribution in the absence of the reform. The steady state reduction is labeled by (A), retiming by (B), and extra marriages by (C). After 1989, the empirical distribution contains too few marriages relative to the counterfactual, for two reasons. First, the steady-state marriage rate falls after 1989 (A). Second, some marriages that would have occurred after 1989 in the absence of reform were retimed to the boom (B). At the cusp of the reform, the empirical distribution is characterized by a marriage boom, which consists of the retimed marriages (B), but also of the “extra” marriages (C) that would never have taken place in the absence of the reform. The figure illustrates that, by estimating a counterfactual density

\[ \text{Specifically, the sample depicted in Figure 3b includes all couples whose first joint child was born from 1991 (and thus conceived in 1990) through 1998.} \]

\[ \text{In particular, as I discuss more below, part of the reason for the decrease in marriages visible in Figure 3a after 1990 is that the sample consists of couples that already had had their first joint child (by 1990), and that therefore were less likely to marry thereafter. Similarly, Figure 3b displays few marriages before 1990, which stems from the fact that this sample of couples had not yet had their first joint child (by 1990).} \]

\[ \text{The null hypothesis that there is no excess mass at the threshold relative to the counterfactual number of marriages in the last quarter of 1989 (obtained by setting } 1 [s = s^*] \text{ equal to zero) is rejected with t-statistics that imply p-values satisfying } p < 10^{-9}. \text{ (Results are available upon request.)} \]
in Figure 3a, I can quantify\textsuperscript{30}:

- \((A + B)\) [the total missing mass due to both effects], and
- \((B + C)\) [the total number of extra and retimed marriages].

The graphical sketch also (trivially) illustrates that if there is no marriage boom (i.e., \(B = 0\) and \(C = 0\)), then total missing mass is due to \((A)\) alone. Thus, under certain assumptions, I can estimate the impact on long-run steady state marital behavior in the population-level data using couples that were unaffected by the transition dynamics but experienced the reduction in marital surplus that generates the steady state reduction – that is, by using the distribution of marriages in the untreated sample displayed in sub-figure 3b.

In the empirical framework presented next, I use both the treated and untreated samples, and simultaneously estimate \((A + B)\), \((B + C)\), and \(A\).

**Estimation of the counterfactual density** I use information on the timing of marriage around a couple’s first birth to construct a counterfactual for the probability of marriage after the reform.\textsuperscript{31} Figure 5 illustrates how my estimation strategy works. I form subsamples of couples based on the date of birth of each couple’s firstborn child. Panels 5a and 5b plot marital behavior for couples whose first joint child was born in 1987 - 1988 and 1983 - 1984, respectively. In both panels, entry into marriage is concentrated around the date of first childbirth. Thus, even though I observe pre-reform marital behavior only until 1989 for all couples, in Panel 5b I observe pre-reform marital behavior for a longer period of time *relative to the date of birth of a couple’s first joint child*. Intuitively, my estimation strategy can be thought of as re-centering the distributions of marriages around the date of the firstborn child, and then exploiting the fact that different cohorts were “hit” by reform at different distances in time from childbirth. This is illustrated in Panel 5c. Further, I exploit the fact that, in the untreated sample, no couples were “hit” by the reform.\textsuperscript{32}

**Specification.** I divide the treated sample, \(T\), into 72 cohorts. Each cohort \(c \in \{1, 72\}\) consists of couples whose first joint child was born in a given quarter, from the first quarter of 1971 until the last quarter of 1988. Because the untreated sample, \(U\), includes couples whose first joint child was born from the first quarter of 1991 until the last quarter of 1998, I similarly divide it into 36 cohorts, \(c \in \{73, 108\}\). I estimate the following regression:

\[
n_{cs} = \alpha + 1 \left[ s = s^* \right] \sum_{c=1}^{72} \beta_c + g(s) + \zeta_q + \eta_c + 1 \left[ s > s^* \right] \sum_{c=1}^{108} \gamma_c + h(t_{pre-birth}) + j(t_{post-birth}) + \varepsilon_{cs},
\]

where \(n_{cs}\) is the natural logarithm of the number of marriages in quarter \(s\) and cohort \(c\).\textsuperscript{33} As in equation (1) above, \(\beta_c\) captures bunching at the notch, and the magnitude of this response is now allowed to be

\textsuperscript{30}Similar in spirit, Best and Kleven (2017) show that a temporary tax cut in housing transaction taxes in the U.K. yields both a timing effect and an effect akin to “extra marriages” on home purchases. Further, Kopczuk and Munroe (2015) use a similar conceptual idea, comparing bunching at the notch with the missing mass beyond it. Also see Marx (2012).

\textsuperscript{31}Section 6.1 above uses pre-reform data in Figure 3a to predict how many marriages would have occurred in the last quarter of 1989 in the absence of reform. Simply using the coefficients obtained in estimation of (1) to predict a counterfactual density beyond 1990 (out of sample) is problematic, however, as the obtained counterfactuals are sensitive to the choice of polynomial. (Results are available upon request.)

\textsuperscript{32}Appendix Figure A2 plots marital behavior for subsamples of the untreated sample; these distributions do not display any bunching in the last quarter of 1989, but otherwise display similar features as the empirical distributions of new marriages in the baseline sample.

\textsuperscript{33}I use the natural logarithm, \(n_{cs} = \ln(N_{cs})\), because the cohort-specific distributions of new marriages exhibit nonlinearities, as illustrated in Figure 5.
different for each cohort in the treated sample, \( c \in T \). Moreover, \( g(s) \) is a higher order polynomial in time (quarter), and \( \zeta_q \) are quarter fixed effects. In addition, (2) includes cohort fixed effects, \( \eta_c \), and cohort-specific reductions in entry into marriage after the notch, \( \gamma_c \).\(^{34}\) 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.

For all treated cohorts \( c \in T \), the \( \gamma_c \) capture the sum of the reduction in entry that is due to retiming and the reduction in entry that is due to the steady state effect. For all untreated cohorts \( c \in U \), the \( \gamma_c \) only capture the steady state reduction.

**Recovering retimed and extra marriages.** I use the coefficients obtained in estimation of (2), but set \( 1 \{ s = s^* \} \) and \( 1 \{ s > s^* \} \) equal to zero, to predict cohort-specific frequencies in a counterfactual scenario without the survivors insurance reform. I then aggregate the cohort-specific frequencies into two sample-wide ones, one for the treated cohorts and one for the untreated cohorts. I calculate \( (A+B) \) and \( (B+C) \) from the treated cohorts’ counterfactual, and use the untreated cohorts’ counterfactual to obtain \( (A) \). I subsequently calculate \( (B) \) and \( (C) \). Finally, I calculate one estimate of the change in the probability of marriage at the threshold, which I denote by \( \frac{\partial p_{ret}}{p_{pre}} \); this simply is the ratio of the estimated boom in 1989 Q4 (numerator) to the estimated counterfactual number of marriages in 1989 Q4 (denominator). I discuss the interpretation of this ratio in Section 6.5 below. I calculate standard errors for each estimated statistic using a cluster bootstrapping procedure. Appendix E.2 and provides more details on the construction of the counterfactual frequencies and the bootstrapping procedure.

**Identifying assumption:** In the absence of reform, couples marrying after 1989Q4 would have behaved like couples marrying before 1989Q4 at the same duration since childbirth, after allowing each cohort of couples to have a separate marriage propensity (recall that \( \eta_c \) allows for vertical shifts of each cohort’s re-centered log distribution of marriages). The identifying assumption is thus akin to a “common trends” assumption with respect to how entry into marriage declines with distance from the date of childbirth. Moreover, in order to be able to apply the estimated percentage steady state reduction in the number of marriages in the untreated sample to the treated sample, I assume that the percentage steady state reduction in the untreated sample applies to the treated sample.

**Results.** Figure 6 presents the results of the decomposition of the marriage boom graphically. The estimates are also presented in Appendix Table A2, and represent the preferred bunching estimation specification.\(^{35}\) The yellow area illustrates the marriage boom, which is the sum or retimed and extra marriages \( (B+C) \), estimated to be 44 573. After the reform, I estimate the sum of all missing marriages \( (A+B) \), due to retiming and a drop in the steady state marriage rate, to be 26 921, indicated by the area between the green solid line and the blue empirical distribution. The steady state reduction in entry into marriage \( (A) \) is estimated to account for a 5.6 percent decline in the number of marriages relative to the counterfactual, which is 3 066 of the missing marriages post reform. Consequently, the boom can be decomposed into 23 855 retimed marriages \( (B) \) and 20 718 extra marriages \( (C) \). Appendix Figure A3 illustrates the 5.6 percent steady state reduction in the number of marriages in the sample of couples that faced no incentive to marry fast.\(^{36}\) The last row of Table A2 presents the estimated change in the probability of marriage at

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\(^{34}\)Given that \( \eta_{cs} \) is the natural logarithm of the number of marriages, \( \beta_c \) and \( \eta_c \) and \( \gamma_c \) can be thought of as proportional.

\(^{35}\)Specifically, I performed the estimation using three different higher order polynomials, and the results that are presented graphically are the ones that minimize the AIC (using fourth-order polynomials).

\(^{36}\)This estimate of the steady state reduction in marriages is similar to the estimate obtained in Section 5.1 above, a 6.2 percent reduction the marriage rate. Note that a 5.6 percent reduction in the number of marriages (relative to the number of marriages at the same point in time in a counterfactual scenario without a survivors insurance reform) translates into a 5.6
the eligibility threshold, \( \frac{\Delta p_s^*}{p_s^*} = 21 \). I interpret and discuss the magnitude of this estimate in Section 6.5 below.

### 6.3 Prediction MU3: Heterogeneous effects and economic incentives

By Prediction MU3, couples that were incentivized to marry should respond more strongly, the larger is their annuity’s expected value. Figure 7 verifies that the raw data is consistent with this conjecture. First, because mortality increases with age, Figure 7a 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 larger among older men. Second, the expected value of the annuity is higher, the larger is the age difference between husband and wife (holding fixed the absolute age of the husband). Figure 7b shows the distribution of marriages in three subsamples with different age differences. While the baseline rate of marriage is similar across the three groups in all other quarters, the increase in the last quarter of 1989 is more pronounced among couples where the age difference is larger.

**Sample and empirical framework.** I use a 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. In sub-sections 6.1 and 6.2, I analyzed distributions of marriages; naturally, these analyses were restricted to couples that actually enter marriage. In this sub-section, I start from the entire treated sample – i.e., couples whose first joint child was born from 1971 through 1988 – but impose one restriction that is motivated by the empirical design. In particular, the hazard analysis requires defining a point in time at which each couple becomes “at risk” for marriage. Because the probability of marriage more than 7 years (28 quarters) before childbirth is essentially zero – as shown in Figure 5 – I define a couple to enter the risk pool for marriage 7 years (28 quarters) before the birth of its first child.\(^{37}\) As I observe marital behavior starting in 1969, the sample therefore includes all couples whose marital behavior I can observe for at least seven years before childbirth, i.e., all couples whose first joint child was born from 1976 (through 1988). 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 a duration model with time varying variables (Heckman and Singer, 1984; Van den Berg, 2001). A couple whose first joint child is born in quarter \( c \) becomes under risk for marriage \( 7 \times 4 = 28 \) quarters earlier, in quarter \((c - 28)\). For couple \( i \), let \( Z_i(t) \) denote a vector of covariates at time \( t \in [0, X_i] \), where \( X_i \) is the time of marriage (measured relative to 28 quarters before childbirth). 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, \( h(t; Z_i(t)) = h_0(t) \exp(\beta Z_i(t)) \). The baseline hazard at time \( t \), \( h_0(t) \), is left unspecified. I estimate:

\[
h(t; Z_i(t)) = h_0(t) \exp(\beta s_i^* (t) + \gamma post_i (t) + \delta_1 F_i(t) + \delta_2 D_i(t)),
\]

(3)  

where \( s_i^* (t) \) is an indicator variable that takes the value of one in the last quarter of 1989 and \( post_i (t) \) percent reduction in the marriage rate (again relative to the marriage rate that would have occurred in the absence of reform). To see this, consider an \( x \) percentage point decrease in the marriage rate relative to a counterfactual marriage rate (in the absence of reform) of \( y \). This translates into an \( x/y \) percent reduction in the marriage rate and an \( x \times \frac{1}{y} \% \) percent reduction in the number of marriages.

\(^{37}\) The distribution of first births itself is predetermined here by definition, as the treatment sample only includes couples whose first joint child was conceived at the time of reform.
is an indicator variable that takes the value of one after the last quarter of 1989.\(^{38}\) Importantly for the analysis of heterogeneity, \(F_i(t)\) is a vector of potentially time-varying financial characteristics that influence the annuity’s expected value: the man’s labor income and share of household income, and each partner’s employment status and birth year (or, in some specifications, the spouses’ age difference). \(D_i(t)\) captures other observable couple characteristics: the partners’ levels of education and the couple’s completed fertility. In alternative specifications, I control more flexibly for the male’s labor income and birth year (or the spouses’ age difference) by including indicator variables for eight income ranges \(l\) and eight birth year ranges \(b\) (or eight age difference ranges \(a\)). 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; and each age difference range is two years. I refer to the vector that includes flexible controls for male income and birth year 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 probabilities for marriage in 1989 Q4 relative to marriage in another quarter, given by the hazard ratio of marriage in 1989 Q4, \(\hat{h}_{1989} = \exp(\hat{\beta})\). 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.

**Results.** In Table 2, Panel A, the top row presents results from estimation of (3). The estimated hazard ratio in the full sample is 14.14 when controlling for \(F_i(t)\), the vector of financial characteristics that influence the annuity’s expected value, and 14.96 when also controlling for other demographics. Thus, a couple that is unmarried at the end of 1989 Q3 is, on average, 15 times more likely to marry in the next quarter than it would have been in the absence of reform.\(^{39}\) The second row replicates these results including flexible controls for male income and birth year; the results remain unchanged.

**Male income and the partners’ age difference** I now examine how the hazard ratio of marriage in 1989 Q4 varies with two different measures of the economic value of the annuity. First, I add interactions between \(s_l^*(t)\) and each male labor income group \(l\) in \(\tilde{F}_i(t)\), and between \(s_l^*(t)\) and each male birth year group \(b\). I thus estimate the following regression model:

\[
h(t; Z_i(t)) = h_0(t)exp\left(\sum_{l} \alpha_l s_l^*(t) + \sum_{b} \beta_l s_b^*(t) + \gamma post_i(t) + \delta_1 \tilde{F}_i(t) + \delta_2 D_i(t)\right). \tag{4}
\]

The estimated hazard ratio for marriage in 1989 Q4 for a couple with male labor income \(l\) and birth year \(b\) is given by \(e^{\alpha_l + \beta_b}\). Figure 8a plots the estimated hazard ratios for couples in which the male was born between 1952 and 1956, for different male income ranges. A man with income in the range 25k-50k in the year before the reform is 11 times more likely to marry in 1989 Q4; the corresponding figure for men whose income instead is in the range 125k-150k is 18. 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 males’ income exceed

\(^{38}\) Couples with children born in different quarters experience the reform at different durations since (28 quarters before) childbirth.

\(^{39}\) This estimate differs slightly from the one obtained in Section 6.2, which suggested that the change in the probability of marriage at the eligibility threshold to be given by \(\frac{\Delta p_{\text{post}}}{p_{\text{pre}}} = 21\). The sample in the current analysis includes couples that never choose to marry.
the Social Security limit.\textsuperscript{40} The hazard ratio is thus increasing in male income in the range where a higher husband labor income raises the annuity’s value.\textsuperscript{41}

Second, I replace the interactions between $s_i^*(t)$ and each male birth year group $b$ in (4) by interactions between $s_i^*(t)$ and each age difference group $a$ (controlling for the absolute age of the husband). Figure 8b plots the estimated hazard ratios for couples where the male earns income in the range 50k-75k, for different ranges of the partners’ age difference. The hazard ratio is increasing with the age difference: Couples where the male is one to two years older than the female are 11 times more likely to marry in 1989 Q4; the corresponding figure for couples where the male is more than nine years older is 13.

**Male mortality risk** Even when holding constant all the observables included in $F_i(t)$ that influence the value of the annuity, couples with private information suggesting that the male is likely to die sooner may respond more strongly to the reform. To examine if couples with a high male mortality risk at the time of reform – captured by ex post realized mortality risk – anticipated this by responding more strongly to the reform, I identify all men in my sample that died within 5 years of January 1, 1990. I then reestimate (3) separately for the two male ex post mortality samples. The results are presented in Table 2, Panels B and C. The estimated hazard ratio in the sample of couples where the male dies within five years is 17.09 when controlling for $F_i(t)$, and 19.46 when also controlling for other observables that a private annuity could potentially be priced on. The corresponding estimates in the sample of couples where the male remains alive after five years are 14.14 and 14.95, respectively. Thus, take-up of survivors insurance through marriage in the last quarter of 1989 Q4 is higher among couples for whom the remaining life span of the male is shorter.\textsuperscript{42}

A positive correlation between demand for survivors insurance and risk type, controlling for prices, is consistent with adverse selection.\textsuperscript{43} In the context of this survivors insurance scheme, all types (of couples) face the same out of pocket cost, namely zero. However, couples receive insurance plans that vary in value. Hence inclusion of the variables $F_i(t)$, 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. My results thus suggest that if insurance companies would observe (and hence be able to price on) all the characteristics $F_i(t)$ that influence the value of the annuity – as well as all characteristics $D_i(t)$ that do not directly affect the annuity’s value – but no more information, then adverse selection would likely arise in such a private

\textsuperscript{40} 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.

\textsuperscript{41} This is consistent with Fadlon and Nielsen (2017) who find higher a valuation of survivors insurance by spouses with divergent levels of earned income.

\textsuperscript{42} I also reestimate (3), including an indicator variable for couples where the male dies within five years (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 this variable interacted with an indicator variable for 1988 Q4 (capturing any extra responsiveness to the elimination of survivors’ insurance among these couples). When controlling for financial characteristics that influence the annuity’s expected value, $F_i(t)$, and all other characteristics that I observe, $D_i(t)$, the implied hazard contribution from the interaction term is 1.26. This implies that a couple where the male dies within five years has a 26 percent higher hazard ratio than a couple where the male remains alive for at least five years; this difference is significant at the five percent level.

\textsuperscript{43} It is also consistent with moral hazard, i.e., that survivors insurance coverage raises the likelihood of husband death. While I cannot test for such moral hazard in this particular sample, the analysis in Section 7 allows me to explicitly examine causal impacts of survivors insurance. While Section 7 focuses on divorce, I have also used that framework to examine husband mortality, but found no effect (results available upon request). This suggests that the positive correlation is due to adverse selection.
market. This may be one reason why private markets for annuities and life insurance were underdeveloped in Sweden at the time of reform.

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

By prediction MU4, rushed marriages should be more likely to end in divorce. MU4 thus is a prediction about the nature of selection into marriage in the last quarter of 1989. 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, but earlier. I estimate the following regression using OLS:

\[
1 \text{[Divorce}_x\text{]}_{imd} = \alpha + \beta 1 [marr = s^*]_i + X_i \theta + \eta_m + \zeta_d + \epsilon_{imd},
\]

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; \(\eta_m\) and \(\zeta_d\) 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’ educational attainment and immigration status; the spouses’ marriage number; and completed fertility. The key coefficient of interest is \(\beta\), which measures the difference in marital stability of marriage-boom marriages relative to other marriages with the same marriage contract.

Table 3 presents the results, with robust standard errors clustered on marriage month*marriage day in parentheses. Consistent with the prediction, the estimates suggest that marrying in the boom is associated with a 3 percentage point (43 %) higher probability of divorce within 5 years, and a 5 percentage point (29 %) higher probability of divorce within 10 years. If this difference is driven by a higher divorce rate among extra marriages, given that such marriages constitute roughly 46% of marriage-boom marriages, the implied probability of divorce in extra marriages within 10 years of marriage is 0.3. On the one hand, this implies that some long-lasting marriages 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 induced marriages may end up in divorce.

6.5 On the size of the documented responses

Magnitudes  Section 6 has documented responses along the margin of entry into marriage. 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 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, which was roughly one third of mean annual post-tax income.\(^{44}\)

We can interpret the ratio \(\frac{\Delta p_s}{p_s^*}\) estimated in Section 6.2 as the numerator of an elasticity that quantifies how marital decisions respond to financial incentives. In particular, let \(\epsilon = \frac{\Delta p_s}{p_s^*} / \frac{\Delta S_s^*}{S_s^*}\) relate the change

\(^{44}\)See Appendix E.3 for calculations.
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 \( \frac{\Delta p^\ast}{p^\ast} = 21.21 \) and the size of the change in marital surplus at the notch \( \Delta S^\ast = -\$4575 \), but also the surplus from marriage relative to cohabitation, \( S^\ast \). Even in the absence of an estimate of \( S^\ast \), 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 \( 21.21 / -1 = -21.21 \).\(^{45}\) It is important to keep in mind that, while the elasticity \( \epsilon \) captures responsiveness to financial incentives at the notch, Section 6.2 shows that 54 percent of the couples that married at the notch retimed their marriages, and hence would have married later in the absence of reform. Even if half of the response is accounted for by retiming, however, the (other half of the) estimated response suggests that Swedish couples’ marital behavior was highly responsive to the financial incentives. It is also instructive to use the hazard framework from Section 6.3 to calculate an “ever married” elasticity; see Appendix E.4 for these calculations.

**Interpretation** Section 6 has documented responses along the margin of entry into marriage that are large, both relative to the existing evidence from other contexts\(^{46}\), and relative to the long-run steady state effects in Sweden discussed in Section 5.1. One candidate interpretation of the large responses relates to the fact that only couples with children were incentivized to enter marriage before January 1, 1990.\(^{47}\) If couples choose to have a joint child once some (positive) learning about match quality has taken place, then cohabiting unions with children will, on average, be more stable than cohabiting unions without children.

Another interpretation relates to the media attention devoted to the reform in the end of 1989. As I discuss above, this did not cause “herding” – figures 3a and 3b suggest that the boom is driven by eligible couples. However, the media attention may have mattered by making it easy for all eligibles – even couples whom otherwise would be unaware of the financial gains from marriage – to figure out whether they would benefit from survivors insurance (marriage). Indeed, even couples where the husband has low IQ respond strongly to the reform.\(^{48}\) Yet another interpretation is that, even though there were substantial legal differences between cohabitation and marriage at the time of reform (as documented in Section 2), couples who were incentivized to enter marriage may not have perceived these differences as important. These legal differences are of crucial importance after separation, and some evidence suggests that couples generally

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\(^{45}\) As discussed by Manoli and Weber (2016), who estimate an analogous elasticity of retirement take-up at a notch in retirement income, the elasticity \( \epsilon \) essentially reflects a thought experiment that compares a situation with discontinuous (marital) surplus to a counterfactual situation with a smooth marital surplus around the threshold.

\(^{46}\) Whittington and Alm (1997) 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. Alm and Whittington (1995) examine the marriage-tax elasticity in the United States between 1947 and 1988 and find it to be smaller than -0.05. Alm and Whittington (1999) use data from the Panel Study of Income Dynamics and find that a 10 percent rise in the marriage penalty leads to a 2.3 percent reduction in the possibility of first marriage. See Alm et al. (1999) for a survey of the literature on the marriage penalty. Similarly, some evidence suggests that couples may retime their marriage by one year in order to avoid the marriage penalty (see Sjöquist and Walker (1995) for the U.S., and Gelardi (1996) for Canada and the United Kingdom); the retiming responses documented here far supersede them.

\(^{47}\) In contrast, Alm and Whittington (1995) and Alm and Whittington (1999) examine the overall marriage rate in the United States and marital behavior in the Panel Survey of Income Dynamics, respectively.

\(^{48}\) There is of course substantial heterogeneity in the ability of couples to prepare for financial security in old age. Interestingly, replicating Figure 7 by quantiles of the husband IQ distribution shows that couples with a low husband IQ – who may not be financially sophisticated – respond strongly to this reform. (Results available upon request.) This suggests that couples did not need to have a high cognitive ability to understand whether they benefitted from opting into survivors insurance (marriage).
underestimate their own chance of divorce (Mahar, 2003).49

Whether the large responses stem from the fact that they are exhibited by couples with children or from the salience of the economic gains from marriage, Section 6.3 documents that couples with children strategically enter marriage because they are aware of the associated economic gains – even if these gains are realized only in the far future.

7 Survivors insurance and pre-existing marriage contracts

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

7.1 Prediction MM1: Marital instability

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 exclude all couples that had their first joint child within four years and nine months of their marriage date, so that no couple had a joint child by the reform announcement (neither in the baseline sample nor in the placebo sample, which I discuss below). I further exclude women born before 1945 and men who were 60 years or older at the date of marriage.50 Table 1 presents summary statistics for the baseline sample in Column 4, as well as an analogous sample that entered marriage in a window 180 days before and after January 1, 1984, which I’ll refer to as the placebo sample, in Column 5. These groups are similar, but relative to the sample studied in Section 6, 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.

All individuals in my baseline sample 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. In contrast, those who had married thereafter lost survivors insurance, unless the couple had a joint child before January 1, 1990.51 By prediction MM1, the removal of survivors insurance from the marriage contracts entered after January 1, 1985 induces some of the affected couples to divorce in response to the loss of marital surplus.

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 6 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 fact that couples that married close to, but on opposite sides of, January 1, 1985, were treated differently in the reform implementation process.

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49 A related interpretation is that the salience of the reform caused eligible couples to overreact relative to what likely would be predicted by a model of rational behavior.

50 The ideal sample would consist of all couples satisfying these conditions. However, matching married individuals into couples poses a challenge in the absence of joint children, when spouses cannot be linked using child ID. See Appendix E.5 for details. I use the sample of women in the analysis of divorce; all results are robust to instead using the male sample.

51 Section 2.1 shows that entry into parenthood was unaffected by the reform.
I cannot implement a standard regression discontinuity (RD) design (Angrist and Lavy (1999), Lee and Lemieux (2010)) because the assignment variable – a couple’s date of marriage – can be precisely manipulated and is not smooth around the threshold, as many couples choose to get married on New Year’s Eve. 52 This raises the concern that couples on opposite sides of the threshold may be systematically different from each other. In a regression discontinuity design, this would raise the concern that “crossing” New Year’s Eve may have a separate effect on the outcome of interest. Two features of my estimation strategy address this concern: First, to net out such an effect – provided it exists – I use a difference-in-differences design that exploits the fact that couples that married around January 1 one year earlier were unaffected by the reform, and their distribution of marriages is similar. 53 Second, the timing of the announcement of the reform 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: differences should emerge no earlier than three and a half years after marriage.

My estimation strategy follows that of Lalive (2008), who combines a difference-in-differences design with a regression discontinuity design when faced with a discontinuity in the density of the assignment variable. 54 Intuitively, it captures the difference between two distinct regression discontinuity estimates – one around January 1, 1985 and one around January 1, 1984. A discontinuity around January 1, 1985, reflects (i) the fact that only couples who married before this threshold get to keep survivors insurance, and (ii) a potential effect of “crossing” New Year’s Eve. A discontinuity around January 1, 1984, reflects (ii) only, as all couples who got married around this threshold get to keep survivors insurance. The design thus represents a version of a differences-in-differences design, but where each “difference” is obtained by zooming in close to each January 1 threshold. Let $Y_{it} = \tau_t * I_{it} + \sigma_t * NYE_i + g(dom) + U_{it}$ represent the causal relationship between whether couple $i$ divorces within $t$ years, $Y_{it}$, and survivors insurance status at time $t$, $I_{it} = I_{it}(dom)$, where $dom$ is the couple’s date of marriage and $U_{it}$ is a random vector of predetermined and unobservable characteristics. $NYE_i = NYE_i(dom)$ is an indicator variable capturing whether the couple married after New Year’s Eve.

Given the existence of a discontinuity in the (expected) survivors insurance coverage from $t > June_{1988}$, the required identifying assumptions are as follows: (i) The impacts of $I_{it}$ and $NYE_i$ are additively separable. (ii) Conditional on $NYE_i$, the direct marginal impact of $dom$ on $Y_{it}$ is continuous. (iii) Further, the interpretation of the (regression discontinuity) difference-in-differences estimate as a causal effect requires a monotonicity assumption, which is satisfied here, since getting married before January 1, 1985, did not induce anyone to lose survivors insurance eligibility. (iv) Finally, the exclusion restriction is that the impact of marrying before January 1, 1985, on outcome $Y_{it}$ runs through its effect on survivors benefits. 55

The first stage and reduced form equations are given by:

\[ Y_{it} = \tau_t * I_{it} + \sigma_t * NYE_i + g(dom) + U_{it} \]

52The first panel of Appendix Figure A4 plots the number of marriages in weekly bins against distance from the survivors insurance eligibility cut-off and shows an increase in the marriage frequency in the last week of 1984. A formal McCrary (2008) test rejects the hypothesis that the density is continuous at the threshold.

53The second panel of Appendix Figure A4 shows their distribution of marriages.

54Lalive (2008) refers to this estimate as the BD-RDD, i.e., the before-during-Regression Discontinuity Design.

55Some couples who married on or after January 1, 1985 qualified for survivors insurance by having a child between the reform announcement and January 1, 1990. This does not, in itself, invalidate the exclusion restriction (iv), it simply yields a fuzzy RD difference-in-differences design: 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, assumption (iv) would be violated if some couples in the ITT group chose to have a child in order to qualify for survivors insurance; the analysis in Section 2.1 rules out such a response.
\[ I_{it} = \alpha + \gamma 1[\tilde{d} \tilde{m}_b > 0] \mathbf{1}[\text{Around85}] + \delta 1[\tilde{d} \tilde{m}_b > 0] + g \left( \tilde{d} \tilde{m}_b \right) + h \left( \tilde{d} \tilde{m}_b \right) \mathbf{1}[\text{Around85}] + \epsilon_{it} \quad (6) \]

\[ Y_{it} = \alpha + \beta 1[\tilde{d} \tilde{m}_b > 0] \mathbf{1}[\text{Around85}] + \eta 1[\tilde{d} \tilde{m}_b > 0] + i \left( \tilde{d} \tilde{m}_b \right) + j \left( \tilde{d} \tilde{m}_b \right) \mathbf{1}[\text{Around85}] + \nu_{it} \quad (7) \]

where \( i \) indexes couples, \( t \) indexes year after marriage, and \( \tilde{d} \tilde{m}_b \) is the distance from New Year’s Eve in 1985 or 1984, respectively. I include a vector of characteristics that is not 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.56 The RD difference-in-differences 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 and find no evidence that couples have systematically different observable characteristics on different sides of the cutoff.57

Results. To gain intuition for the results as well as the empirical strategy, the left panel of Figure 9 displays the empirical cumulative distributions of durations until divorce, obtained by estimating Kaplan-Meier failure functions without covariates, for couples marrying in the last three months of 1984 and the first three months of 1985, respectively. This graphical evidence suggests that the removal of survivors insurance caused divorces: During the first three years of marriage, when both groups of couples had the same marriage contract, they display similar divorce behavior. When the reform is announced in June 1988, and survivors insurance is removed from couples that married in 1985, the failure functions begin to diverge. The right panel plots the same functions for couples that married within three months of 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. This offers support to my interpretation of the divergence in the left panel as the causal effect of (losing) survivors insurance.

Table 4 presents 2SLS (fuzzy RD difference-in-differences) estimates of the impact of survivors insurance on divorce at different durations of marriage \( t \), using different bandwidths and polynomial orders.58 The estimates suggest that removing survivors insurance raises the probability that a marriage ends in divorce within 24 years by 5.35 percentage points using the smaller bandwidth (5.18 using baseline bandwidth) in the specifications favored by the AIC criterion, which represent a 10 percent increase.59 Thus, the removal of survivors insurance pushed couples on the margin into divorce.

8 Survivors insurance and matching

8.1 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,

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56 Educational attainment (indicators for high school and college) is measured in 1985, which is the earliest year for which I observe education.

57 Appendix Figure A5 plots these characteristics in weekly bins against distance from the eligibility cut-off.

58 Appendix Table A3 presents OLS estimates of the first stage (6); it is close to one, which reflects the fact that treatment is near-universal at the right side of the threshold. Among couples that married around 1985 and that had no joint child by the reform’s announcement in June 1988, only 9% had a child before January 1, 1990 (and thus obtained the old marriage contract, with survivors insurance).

59 The high divorce rate reflects the fact that married couples that do not have children on average are less stable than couples with children (they are also more likely to be second marriages). The AIC is estimated from the corresponding OLS regression.
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. My sample includes all couples with children that married from 1983 through 1999, excluding the 6% of the observations for which I have no information about educational attainment at marriage. Appendix Table A4 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, $V_{igq}s$, and the final quarter in which marriage entails take-up of the old marriage contract by $V_{igq}s = (V_{igq} - 1989Q4)$, and estimate the following regression:

$$r_s = \alpha + \beta_s [V_{igq}s > 0] + \gamma_s [V_{igq}s > 0] (V_{igq}s) + \delta_s [s = s^*] + g(V_{igq}s) + \zeta + \epsilon_c,$$  

(8)

where $r_s$ denotes the ratio of highly skilled men that marry “down,” $r_s = \frac{N(c_{high}, q_{low})}{N(c_{high}, q_{low}) + N(c_{low}, q_{high})}$, where the function $N()$ counts the number of marriages of the match type indicated by the arguments. The main coefficient of interest is $\beta_s$, which captures a discontinuous change after the threshold $s^*$. Further, $\gamma_s$ captures any change in the slope at $s^*$, and $g(V_{igq}s)$ is a polynomial in $V_{igq}s$.

**Results.** Figure 10 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 5 presents the estimates from (8), using as $g(V_{igq}s)$ polynomials in $V_{igq}s$ of three different orders. The linear model, which is favored by the AIC criterion, suggests that the share of highly skilled men that enter assortatively matched marriages increases by four percentage points (11%) following the introduction of the new marriage contract.

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60In the same vein, the old marriage contract subsidized matches where the husband was older than the wife. I replicated the analysis in the current section, but using the share of couples where the husband is older than the wife as an outcome variable instead of the share of couples that are unassortatively matched on skill, and found suggestive evidence of a decline in the share of matches in marriage where the husband is older than the wife. (Results are available upon request.)

61The 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. I exclude couples that married after 1999 as educational attainment (which I measure at marriage) is coded differently starting in 2000. The share of the sample for which I observe educational attainment is somewhat higher starting in 1990; however, the coding of high and low educational attainment remains unchanged. As I only observe educational attainment starting in 1985, I use educational attainment in 1985 for those who marry in 1983 and 1984.
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 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 A6. 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. \(^{62}\)

9 Conclusion

This paper analyzes how linking social insurance to marriage affects the marriage market, exploiting Sweden’s elimination of survivors insurance in 1989. Its findings establish that tying social insurance to marriage has economically important impacts across all three stages of the mating process: matching, entry into marriage, and exit from marriage. Further, they suggest that marital behavior is an important component of couples’ strategies for ascertaining financial security in old age.

A number of important questions remain. Chief among them is when it is optimal to separate social insurance from marriage in modern societies. The stated aim of legislators in creating social security systems that confer spousal benefits, both in the United States and in Sweden, was to insure constituents – notably widows with little (previous) labor force attachment – against poverty in old age. In the presence of marriage market responses such as the ones documented in this paper, the social planner may face a trade-off between this stated aim, on the one hand, and generating economically important marriage market distortions, on the other.

The resolution of this trade-off may depend on the extent of female labor force participation. Intuitively, the higher the share of couples with a single (male) breadwinner, the greater the share of women who, in the absence of survivors insurance, would end up impoverished in widowhood. Thus if – for reasons exogenous to the design of survivors insurance – female labor force participation rises, the social benefits of survivors insurance may fall. This suggests that at some point, as the share of dual-earner households rises, it may become optimal to decouple social insurance from marriage; but such questions are left for future research.

\(^{62}\)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. See Appendix E.7 for a discussion of the potential role of changing skill distributions of men and women in the longer run.
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10 Figures and tables

Figure 1: Couples’ Decision to Have a Joint Child and the “Effective” Reform Announcement Date

(a) Couples’ decision to have a joint child

(b) Media attention devoted to the reform

Notes: In Figure 1a, the sample includes the universe of all children born from 1985 through 1994 who were their parents’ first joint child. The figure plots the number of first births – i.e., the number of couples who have a first joint child – by quarter. The thick long-dashed vertical line indicates the last quarter before the survivors’ insurance reform was implemented. The thin dashed vertical line indicates the quarter of reform announcement. Figure 1b displays the distribution of mentions of the reform in leading newspapers. The sample includes all articles published by Tidningarnas Telegrambyrå, Västerbottenskuriren, and Dagens Industri from June 1988 until December 1998. The solid vertical line indicates the (first month of the) last quarter of 1989. Media coverage was concentrated in the last quarter of 1989 – by then, it was too late to conceive a joint child in response to the reform.

Figure 2: Cohabitation versus Marriage and the Quality of Cohabiting Unions

(a) Entry into marriage from cohabitation

(b) Separation from cohabitation

Notes: Figure 2a is constructed using a sample of cohabiting couples that initiate cohabitation between 1981 and 2000 (inclusive), described in detail in Appendix E.1. The figure displays the share of couples that marry within 3, 5, and 7 years of moving in together, by the year of initiation of cohabitation. Couples’ marital behavior was directly affected by the reform during some years; the dashed portions of the lines represent these transition years. The solid portions of the lines capture marital behavior among couples who initiate cohabitation well before or well after the reform. In figure 2b, the sample is restricted to the subset of couples who did not enter marriage within 2 years after initiating cohabitation. The figure displays the share of such couples that move apart, within 5, 7, and 9 years, respectively, by year of initiation of cohabitation. The dashed portion of each line represents couples that are affected by the transition between survivors insurance regimes (more specifically, that are incentivized to marry fast); the solid portions of each line thus capture separation behavior among couples who initiate cohabitation well before or well after the reform. A higher rate of separation indicates a poorer average match quality among couples that choose not to marry.
Figure 3: Empirical Distributions of New Marriages

(a) Faced incentive to marry fast

(b) Faced no incentive to marry fast

Notes: This figure displays the empirical distribution of marriages in Sweden, by quarter from 1980 through 2003, for two non-overlapping samples of couples. In Figure 3a, the sample includes all couples whose first joint child was born before January 1, 1989 (and hence was likely conceived before the reform announcement in June 1988). These couples faced strong incentives to marry fast, i.e., ahead of January 1, 1990. In Figure 3b, the sample instead includes all couples whose first joint child was born after January 1, 1991 (and hence was conceived after the reform implementation, in January 1990) and through 1998. Because they were ineligible for survivors insurance regardless of their date of marriage, the reform did not provide these couples with any incentive to marry fast. The (grey) dashed vertical line indicates the quarter of reform announcement. The (red) long-dashed vertical line indicates the last quarter of 1989. Figure 3a displays a marriage boom in the last quarter of 1989; in contrast, Figure 3b displays no such boom.

Figure 4: Simple Sketch of the Steady State Drop (A), Retiming (B), and Extra Marriages (C)

Notes: This figure provides a simple sketch of how the theory predicts that retimed and extra marriages (prediction MU2), as well as the steady state reduction in entry into marriage (prediction SS1), appear in the empirical distribution. The black area illustrates a hypothetical observed distribution of marriages, and the dashed line shows its counterfactual empirical distribution in the absence of reform. The steady state reduction is labeled by (A), retiming by (B), and extra marriages by (C). At the cusp of the reform, the empirical distribution displays bunching, due to two effects. First, some marriages that would have occurred after the reform were retimed to the boom (B). Second, the reform also induced extra marriages that – in the absence of the reform – never would have been entered into (C). After the reform, the empirical distribution contains “missing” marriages relative to the counterfactual, due to two effects. First, the steady-state marriage rate falls after the reform (A). Second, the marriages that were retimed to the boom no longer occur in the post-reform period (B).
**Figure 5: Empirical Strategy: Intuition**

(a) First child born 1987 or 1988
(b) First child born 1983 or 1984
(c) Both, re-centered

**Notes:** This Figure replicates Figure 3a for subsets of the sample, described in detail in the notes of Figure 3. Panel 5a depicts new marriages among couples that had their first joint child in 1987 or 1988. Panel 5b depicts new marriages among couples that had their first joint child in 1983 or 1984. In both panels, entry into marriage is concentrated around the date of first childbirth. Thus, even though I observe pre-reform marital behavior only until 1989 for all couples, I observe pre-reform marital behavior for a longer period of time relative to the date of birth of a couple’s first joint child in Panel 5b than in Panel 5a. Panel 5c lays 5a (in black) and 5b (now in red) on top of each other, re-centers the distributions around the date of childbirth, and 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. It illustrates that different cohorts were “hit” by reform at different distances in time from childbirth. This makes it possible to use early cohorts of couples – whose marital behavior post-childbirth 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.

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**Figure 6: Decomposition of the Marriage Boom**

**Notes:** This figure presents the results of the bunching decomposition estimation among couples who faced an incentive to marry before 1990 to secure survivors insurance. See the notes of Figure 3 for more information on the definition of the analysis sample, which includes all couples included in the “treated sample” depicted in Figure 3a as well as all couples included in the “untreated sample” depicted in Figure 3b. I estimate the sum or retimed and extra marriages in 1989 Q4 to be 44 573 (the yellow area); and the sum of all missing marriages – due to retiming and due to the drop in the steady state marriage rate post reform – to be 26 921 (the area between the green solid line and the blue empirical distribution). The steady state reduction in entry into marriage is estimated to account for 3 066 of the missing marriages post reform. Consequently, the number of retimed marriages is given by 26 921 - 3 066 = 23 855, and the number of extra marriages is given by 44 573 - 23 855 = 20 718.
Figure 7: Heterogenous Effects in the Raw Data

Notes: This figure replicates Figure 3a for subsets of the sample of couples who faced an incentive to marry before 1990 to secure survivors insurance (described in detail in the notes of Figure 3). Figure 7a plots the distribution of new marriages for two strict subsamples based on the husband’s age at marriage: (i) the husband is younger than 31, or (ii) between 31 and 40, respectively. Figure 7b plots the distribution of new marriages for three (other) strict subsamples, based on the couple’s age structure, where (i) the wife is older or the spouses are the same age, (ii) the husband is strictly older than the wife, but no more than three years older, and (iii) the husband is more than three years older than the wife. Both figures also provide the estimated counterfactual in the last quarter of 1989.

Figure 8: Hazard Ratios for Couples with Different Male Income and Age Difference

Notes: The sample includes all couples whose first joint child was born from 1976 through 1988, regardless of eventual marital status. Figure 8a plots the estimated hazard ratios for marriage in 1989 Q4 for couples with different male income ranges in the year before the reform. The specification includes two sets of interactions, between an indicator for marriage in 1989 Q4 and indicator variables for eight male income groups, and between an indicator for marriage in 1989 Q4 and indicator variables for eight male birth year groups, respectively. The specification also includes controls for other observable financial characteristics that influence the annuity’s expected value; thus, a movement from left to right along the x-axis represents an increase the expected annuity value. Further, the specification includes controls for other observable demographic characteristics (see the text for exact details) that a private annuity could potentially be priced on. The last income group in Figure 8a includes couples that exceed the Social Security limit (see Section B for details); this is indicated by the green dashed line. The hazard ratio is thus increasing in male income in the range where a higher husband labor income raises the annuity’s expected value. Figure 8b plots the hazard ratio for couples in different age difference intervals. These estimates are obtained from estimation of the same specification, but including interactions between an indicator for marriage in 1989 Q4 and each age difference group instead of interactions with each male birth year group. Gray dashed lines represent 95% confidence intervals.
Figure 9: Empirical CDF of Marriage Duration Around Actual and Placebo Thresholds

(a) Married around 1985

(b) Married around 1984

Notes: To define the sample in Figure 9a, I start from all individuals that entered marriage in a window of 180 days around the eligibility threshold, January 1, 1985. Because the reform only affected the subset of already married couples that were childless, I exclude all couples that had a joint child within the first four years and nine months of marriage, so that no couple had a child at the time of the reform announcement. I further exclude women born before 1945 and men who were 60 years or older at the date of marriage. This sample captures individuals that were eligible for survivors insurance when the reform was announced, but that lost coverage of survivors insurance if the couple had married after January 1, 1985 (unless they had a child before January 1, 1990). The sample in Figure 9b is defined analogously for individuals that entered marriage within 180 days of January 1, 1984. Both panels display the empirical CDF of durations until divorce, obtained by estimating the Kaplan-Meier failure function without covariates, separately for couples that married in the last three months before each threshold (black line) and the first three months after each threshold (gray line). 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 dashed vertical line in the left panel. All couples in the right panel were allowed to keep the old marriage contract that they married into.

Figure 10: Assortativeness of Matching

Notes: 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 reform implementation year, 1989. The hollow circles depict the share of highly skilled men marrying a woman of low skill (seasonality adjusted), at a quarterly level. 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 1: Summary Statistics

<table>
<thead>
<tr>
<th>Demographic characteristics at marriage</th>
<th>Analysis of entry into marriage</th>
<th>RDD analysis: Baseline or Placebo Sample</th>
</tr>
</thead>
<tbody>
<tr>
<td>H age</td>
<td>31.49</td>
<td>29.82</td>
</tr>
<tr>
<td>W age</td>
<td>28.72</td>
<td>27.05</td>
</tr>
<tr>
<td>H high school</td>
<td>0.51</td>
<td>0.49</td>
</tr>
<tr>
<td>W high school</td>
<td>0.54</td>
<td>0.52</td>
</tr>
<tr>
<td>H college</td>
<td>0.23</td>
<td>0.25</td>
</tr>
<tr>
<td>W college</td>
<td>0.23</td>
<td>0.25</td>
</tr>
<tr>
<td>H marriage number</td>
<td>1.12</td>
<td>1.11</td>
</tr>
<tr>
<td>W marriage number</td>
<td>1.11</td>
<td>1.11</td>
</tr>
<tr>
<td>Economic characteristics at marriage</td>
<td></td>
<td></td>
</tr>
<tr>
<td>H log labor income</td>
<td>9.86</td>
<td>9.65</td>
</tr>
<tr>
<td>W log labor income</td>
<td>9.03</td>
<td>8.88</td>
</tr>
<tr>
<td>Fertility behavior</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Couple’s completed fertility</td>
<td>2.26</td>
<td>2.26</td>
</tr>
<tr>
<td>First child out of wedlock</td>
<td>0.66</td>
<td>0.52</td>
</tr>
<tr>
<td>Number of observations</td>
<td>306822</td>
<td>220069</td>
</tr>
</tbody>
</table>

Notes: Column 1 presents summary statistics for the sample of couples that faced an incentive to marry fast in response to the elimination of survivors insurance, used in the bunching analysis. 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. Columns 2 and 3 present summary statistics for two strict subsets of the sample in Column 1; couples in these subsamples married into the old marriage contract. Columns 4 and 5 present summary statistics for the baseline and placebo samples used in the analysis of the causal impact of survivors’ insurance on divorce. To define the baseline sample in Column 4, I start from all individuals that entered marriage within 180 days of January 1, 1985. Because the reform only affected the subset of already married couples that were childless, I exclude all couples that had a joint child within the first four years and nine months of marriage. I further exclude women born before 1945 and men who were 60 years or older at the date of marriage. This baseline sample captures individuals that were eligible for survivors insurance when the reform was announced, but that lost coverage of survivors insurance if the couple had married after January 1, 1985 (unless they had a child before January 1, 1990). The placebo sample in Column 5 is defined analogously for individuals that entered marriage within 180 days of January 1, 1984. I do not report completed fertility in columns 4 and 5 since only a small share of the sample has any child. The number of observations refers to the number of couples in Columns 1-3, and the number of husbands plus the number of wives in Columns 4-5.
Table 2: Impact on Marriage: Hazard Ratios

<table>
<thead>
<tr>
<th></th>
<th>Financial controls</th>
<th>All observable controls</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Coefficient, $\hat{\beta}$</td>
<td>Exponential, $e^{\hat{\beta}}$</td>
</tr>
<tr>
<td>Panel A: All</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Marriage 1989 Q4</td>
<td>2.65***</td>
<td>14.14***</td>
</tr>
<tr>
<td></td>
<td>(0.03)</td>
<td>(0.48)</td>
</tr>
<tr>
<td>Marriage 1989 Q4 (flexible controls)</td>
<td>2.70***</td>
<td>14.81***</td>
</tr>
<tr>
<td></td>
<td>(0.04)</td>
<td>(0.54)</td>
</tr>
<tr>
<td>Number of couples</td>
<td>960414</td>
<td>960414</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Panel B: Male dies within 5 years</th>
</tr>
</thead>
<tbody>
<tr>
<td>Marriage 1989 Q4</td>
</tr>
<tr>
<td></td>
</tr>
<tr>
<td>Number of couples</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Panel C: Male alive after 5 years</th>
</tr>
</thead>
<tbody>
<tr>
<td>Marriage 1989 Q4</td>
</tr>
<tr>
<td></td>
</tr>
<tr>
<td>Number of couples</td>
</tr>
</tbody>
</table>

Notes: The table reports hazard model estimates for three different samples (panels). Columns 1 and 3 report the estimated coefficient on the indicator variable for marriage in 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 columns 1 and 2, each regression includes controls for financial characteristics that influence the annuity’s expected value. In columns 3 and 4, each regression also includes controls for other observable characteristics. In Panel A, the sample includes all couples whose first joint child was born from 1976 through 1988, regardless of eventual marital status. All controls are similar in the upper and lower rows of Panel A, except that I control flexibly for male labor income and birth year in the lower row. Panels B and C report hazard model estimates corresponding to those in Panel A, row 1, for two distinct sub-samples: couples where the male dies within five years of January 1, 1990, and couples where the male remains alive five years after the reform implementation date, respectively.

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

Table 3: Hightened Divorce Risk in Boom Marriages

<table>
<thead>
<tr>
<th></th>
<th>Dependent variable: Divorce within</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>3 years</td>
</tr>
<tr>
<td>Married in 1989 Q4</td>
<td>0.01***</td>
</tr>
<tr>
<td></td>
<td>(0.00)</td>
</tr>
<tr>
<td>Mean, dept. var</td>
<td>0.03</td>
</tr>
<tr>
<td>Number of couples</td>
<td>175015</td>
</tr>
</tbody>
</table>

Notes: The sample includes all couples with a joint child born in 1988 or earlier that married from 1980 and through 1989. The dependent variable is an indicator variable for the couple divorcing within 3, 5, 10, and 15 years, respectively. The key independent variable is an indicator for marriage in the last quarter of 1989. All regressions include controls for the spouses’ ages, household income, and h’s share of household income at marriage, the spouses’ educational attainment and immigration status, the spouses’ marriage parity, completed fertility, and wedding month and day of week fixed effects. The reported coefficient measures the difference in marital stability of marriage-boom marriages relative to other marriages with the same (old) marriage contract. Standard errors clustered at wedding month*wedding day of week in parentheses.

* p < 0.10, ** p < 0.05, *** p < 0.01.
### Table 4: Impact on Divorce in Pre-Existing Marriages

<table>
<thead>
<tr>
<th>Divorce within</th>
<th>Bandwidth 150 days</th>
<th></th>
<th>Bandwidth 180 days</th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Polynomial 2</td>
<td>Polynomial 3</td>
<td>Polynomial 2</td>
<td>Polynomial 3</td>
</tr>
<tr>
<td>3 years</td>
<td>0.0313 (0.0236)</td>
<td>0.0243 (0.0257)</td>
<td>0.0173 (0.0204)</td>
<td>0.0238 (0.0226)</td>
</tr>
<tr>
<td>Mean, dept. var</td>
<td>0.17</td>
<td>0.17</td>
<td>0.16</td>
<td>0.16</td>
</tr>
<tr>
<td>AIC</td>
<td>7542.00</td>
<td>7544.51</td>
<td>10569.11</td>
<td>10572.27</td>
</tr>
<tr>
<td>24 years</td>
<td>0.0535* (0.0313)</td>
<td>0.0455 (0.0340)</td>
<td>0.0429 (0.0276)</td>
<td>0.0518* (0.0306)</td>
</tr>
<tr>
<td>Mean, dept. var</td>
<td>0.50</td>
<td>0.50</td>
<td>0.49</td>
<td>0.49</td>
</tr>
<tr>
<td>AIC</td>
<td>12671.80</td>
<td>12675.36</td>
<td>18739.71</td>
<td>18736.43</td>
</tr>
<tr>
<td>Number of couples</td>
<td>9117</td>
<td>9117</td>
<td>13421</td>
<td>13421</td>
</tr>
</tbody>
</table>

Notes: The sample includes all women in the baseline and placebo samples, which are described in detail in the notes for Table 1, Columns 4-5. The dependent variable is 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. The table reports the RD difference-in-differences estimates obtained using different orders of polynomials and different bandwidths. Each cell represents a separate regression. Robust standard errors in parentheses.

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

### Table 5: Matching: Impact on the Share of Highly Skilled Men Marrying Low-Skilled Women

<table>
<thead>
<tr>
<th>New Marriage Contract</th>
<th>Polynomial 1</th>
<th>Polynomial 2</th>
<th>Polynomial 3</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>-0.0447***</td>
<td>-0.0364**</td>
<td>-0.0492*</td>
</tr>
<tr>
<td></td>
<td>(0.0087)</td>
<td>(0.0135)</td>
<td>(0.0194)</td>
</tr>
<tr>
<td>Mean, dept. var</td>
<td>0.41</td>
<td>0.41</td>
<td>0.41</td>
</tr>
<tr>
<td>Adj. R squared</td>
<td>0.87</td>
<td>0.88</td>
<td>0.87</td>
</tr>
<tr>
<td>AIC</td>
<td>-353.65</td>
<td>-353.06</td>
<td>-350.20</td>
</tr>
<tr>
<td>Number of obs</td>
<td>68</td>
<td>68</td>
<td>68</td>
</tr>
</tbody>
</table>

Notes: Dependent variable: The share of highly skilled men that marry a low-skilled woman. The table reports the coefficient on the key explanatory variable in Equation (8), 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.
A Supplemental figures and tables

Figure A1: Cohabitation Sample: Reduction in Entry into Marriage from Cohabitation

(a) Three-year marriage rate
(b) Four-year marriage rate

Notes: The figure is constructed using a sample of cohabiting couples that initiate cohabitation between 1981 and 2000 (inclusive), described in detail in Appendix E.1. Figure A1a replicates the red line in Figure 2a, which displays the share of couples that marry within 3 years of moving in together, by the year of initiation of cohabitation. The green dashed line represents a linear prediction, obtained using the solid portion of the red line in pre-reform years. Figure A1b displays the share of couples that marry within 4 years of moving in together, by the year of initiation of cohabitation. The green dashed line represents a linear prediction, obtained using the solid portion of the red line in pre-reform years.

Figure A2: Empirical Strategy: Intuition II

(a) First joint child born 1991-92
(b) First joint child born 1993-94

Notes: This Figure replicates Figure 3b for strict subsets of the untreated sample, described in detail in the notes of Figure 3. Panel A2a depicts new marriages among couples that had their first joint child in 1991 or 1992. Panel A2b depicts new marriages among couples that had their first joint child in 1993 or 1994. Because these couples’ first joint child were conceived after the reform, they faced no incentive to marriage fast. These distributions do not display any bunching in the last quarter of 1989, but otherwise display similar features as the empirical distributions of for subsets of the treated sample, displayed in Figure 5.
Figure A3: Steady State Reduction in Entry Into Marriage Among “Untreated” Couples

Notes: This Figure presents the results of the bunching decomposition estimation in Section 6.2, for couples in the “untreated sample,” untreated sample, described in detail in the notes of Figure 3. These couples faced no incentive to marry fast in response to the reform, but experienced the decline in marital surplus after 1990 that induced a steady state reduction in entry into marriage. The Figure illustrates the 5.6 percent steady state reduction in the number of marriages in the sample of couples that faced no incentive to marry fast suggested by the preferred estimate.

Figure A4: Distribution of New Marriages around 1985 (the Eligibility Threshold) and 1984

(a) Around 1985
(b) Around 1984

Notes: This Figure displays the frequency of new marriages, in weekly bins, around January 1, 1985 (the eligibility threshold) and January 1, 1984 (the placebo threshold), respectively. The baseline sample (left graph) and placebo sample (right graph) are described in detail in the notes for Table 1, Columns 4-5. The green circles represent each year’s last week, which includes New Year’s Eve.
### Table A1: Summary Statistics for Cohabitation Sample

<table>
<thead>
<tr>
<th></th>
<th>All</th>
<th>Marry within 10 years</th>
<th>Do not marry within 10 years</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Age at marriage</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Male</td>
<td>36.92</td>
<td>36.27</td>
<td>47.22</td>
</tr>
<tr>
<td>Female</td>
<td>33.73</td>
<td>33.06</td>
<td>44.30</td>
</tr>
<tr>
<td><strong>Age at initiation of cohabitation</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Male</td>
<td>37.73</td>
<td>36.48</td>
<td>38.49</td>
</tr>
<tr>
<td>Female</td>
<td>35.18</td>
<td>33.27</td>
<td>36.36</td>
</tr>
<tr>
<td><strong>Education (4 lvls)</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Male</td>
<td>2.03</td>
<td>2.16</td>
<td>1.94</td>
</tr>
<tr>
<td>Female</td>
<td>2.09</td>
<td>2.21</td>
<td>2.00</td>
</tr>
</tbody>
</table>

#### Economic characteristics

|                                |              |                       |                               |
| Total log household labor income (He and She) | 9.00         | 9.29                  | 8.81                          |
| His income share                | 0.61         | 0.63                  | 0.59                          |

#### Fertility behavior

|                                |              |                       |                               |
| Couple has joint child          | 0.49         | 0.72                  | 0.35                          |

#### Separation

|                                |              |                       |                               |
| Couple moves apart within 10 years | 0.42 | 0.21                  | 0.56                          |

Observations: 322115 122876 199239

Notes: The sample includes couples that initiate cohabitation between 1981 and 2000 (inclusive) and is described in detail in Appendix E.1. Column 1 displays summary statistics for all couples, regardless of their eventual marital status. Columns 2 and 3 present the same statistics for couples that eventually marry and do not eventually marry, respectively, where eventually marrying is defined as marrying within ten years of moving in together.

### Table A2: The Impact on Marriage and Marriage Boom Decomposition

<table>
<thead>
<tr>
<th></th>
<th>Polynomial order = 4</th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>Induced Marriages (B+C)</td>
<td>44572.62***</td>
<td>(3856.54)</td>
<td></td>
</tr>
<tr>
<td>Missing Marriages (A+B)</td>
<td>26920.50***</td>
<td>(2920.89)</td>
<td></td>
</tr>
<tr>
<td>Missing Marriages due to (A)</td>
<td>3065.75</td>
<td>(2889.29)</td>
<td></td>
</tr>
<tr>
<td>Decomposition of marriage boom</td>
<td>Retimed Marriages (B)</td>
<td>23854.75***</td>
<td>(318.20)</td>
</tr>
<tr>
<td>Extra Marriages (C)</td>
<td>20717.87***</td>
<td>(3867.77)</td>
<td></td>
</tr>
<tr>
<td>Estimated Counterfactual</td>
<td>2101.38***</td>
<td>(337.59)</td>
<td></td>
</tr>
<tr>
<td>Δp/p</td>
<td>21.21***</td>
<td>(2.04)</td>
<td></td>
</tr>
</tbody>
</table>

Number of obs (cohort quarters): 9243  
AIC: 12781.12

Notes: This table presents the results of the bunching decomposition estimation among couples who faced an incentive to marry before 1990 to secure survivors insurance. The results are represented visually in Figure 6; see the notes of this figure for more information on the definition of the analysis sample. The dependent variable is the log number of marriages (by cohort and quarter). The results are from the preferred specification across specifications with polynomials of orders 2, 3, and 4, in the sense that these results minimize the AIC (the AIC using second and third order polynomials are 13473.51 and 12959.86, respectively). The first row reports the estimated number of induced marriages in 1989 Q4, which consists of both retimed (B) and extra marriages (C). The second row reports the total number of “missing marriages,” due to both retiming (B) and a steady state reduction in entry into marriage (A). The third row reports the number of missing marriages due to (A). The next two rows decompose the marriage 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 a cluster bootstrapping procedure with 10 000 randomly drawn panels, further described in Appendix E.2, in parentheses. AIC is obtained from the regression with the entire (non-random) sample.

* p < 0.05, ** p < 0.01, *** p < 0.001.
Figure A5: Distribution of Covariates around Eligibility Threshold (1985)

Notes: This Figure displays the distribution of covariates among couples entering marriage, in weekly bins, around January 1, 1985 (the eligibility threshold). The baseline sample is described in detail in the notes of Table 1, Column 4.
Figure A6: The Number of High-low and High-high Matches

Notes: 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 reform implementation year, 1989. The figure 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).

Table A3: RDD Results: First stage

<table>
<thead>
<tr>
<th>Polynomial</th>
<th>2</th>
<th>3</th>
</tr>
</thead>
<tbody>
<tr>
<td>Married after NYE*Married around NYE 1985</td>
<td>0.9144*** (0.0108)</td>
<td>0.9067*** (0.0103)</td>
</tr>
<tr>
<td>Mean, dept. var</td>
<td>0.23</td>
<td>0.23</td>
</tr>
<tr>
<td>AIC</td>
<td>-8638.54</td>
<td>-8643.33</td>
</tr>
<tr>
<td>Number of observations</td>
<td>13421</td>
<td>13421</td>
</tr>
</tbody>
</table>

Notes: The sample includes all women in the baseline and placebo samples, which are described in detail in the notes for Table 1, Columns 4-5. The dependent variable is the couple’s survivors insurance status after January 1, 1990. The table presents the estimate of $\gamma$ in Equation (6), 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 and the indicator for marrying around New Year’s Eve of 1985. Each cell represents a separate regression. Robust standard errors in parentheses.

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

40
Table A4: Summary statistics: Marriages of Highly Skilled Men 1983 - 2000

<table>
<thead>
<tr>
<th>Quarter of Marriage</th>
<th>Old Marriage Contract</th>
<th>New Contract</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>1983Q1-1989Q3</td>
<td>1989Q4</td>
</tr>
<tr>
<td><strong>H high skill but W low skill</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>H age at marriage</td>
<td>30.67</td>
<td>36.22</td>
</tr>
<tr>
<td>W age at marriage</td>
<td>27.70</td>
<td>33.24</td>
</tr>
<tr>
<td>W high school</td>
<td>0.85</td>
<td>0.81</td>
</tr>
<tr>
<td>W college</td>
<td>0.00</td>
<td>0.00</td>
</tr>
<tr>
<td>H cognitive capacity</td>
<td>0.75</td>
<td>0.55</td>
</tr>
<tr>
<td>Total log household labor income (H and W)</td>
<td>12.04</td>
<td>12.28</td>
</tr>
<tr>
<td>H income share</td>
<td>0.70</td>
<td>0.73</td>
</tr>
<tr>
<td>Couple's completed fertility</td>
<td>2.21</td>
<td>2.13</td>
</tr>
<tr>
<td>First child out of wedlock</td>
<td>0.39</td>
<td>0.91</td>
</tr>
<tr>
<td><strong>Observations</strong></td>
<td>21124</td>
<td>5044</td>
</tr>
</tbody>
</table>

| 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 high school           | 0.00  | 0.00  | 0.00  |
| W college               | 1.00  | 1.00  | 1.00  |
| 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 |

Notes: The sample includes all couples that had a joint child and married between 1983 and 2000. 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, where high educational attainment is defined as some college or higher.
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 6.3 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

\[
b_h = 0.6 \times (Income - BA) \text{ for } Income \in (BA, 7.5BA). \tag{9}
\]

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 6.3.

For individuals who worked in Sweden for \( N < 30 \) years prior to retirement, the supplementary social security benefit in (9) 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 Child Outcomes and Parental Marital Status

While the marriage contract has relevant consequences for children, it is not clear whether, in practice, there are any differences between the children of cohabiting and married couples – cohabitation is, after all, socially accepted in Sweden at the time of the reform, and childbearing outside of marriage is common (a fact that I explore to in Section 6.2). Here, I therefore briefly examine whether child outcomes differ depending on the parents’ marital status. Needless to say, these differences need not be causal; however, it is instructive to know whether the parents’ marital status may matters for children.
In practice, the data displays large differences in the two sets of outcomes that I observe. First, Appendix Figure C7 displays the share of children that live with the mother of father, respectively, after the parents separate. Here, separation is defined as the end of cohabitation both for couples that were married while cohabiting and for couples that were unmarried while cohabiting, and the x-axis displays the year relative to the last year before separation (year zero). The figure shows that, post separation, a child is more likely to live with the mother – and less likely to live with the father – if the parents were unmarried prior to separation. Second, Appendix Table C5 presents educational attainment at age 16 by the parents’ marital status. On a range of measures of educational attainment, children of cohabiting couples fare worse than those of married couples. In sum, the data on physical co-residence and educational attainment demonstrate important differences between the outcomes of children of cohabiting and married parents. It is not clear, however, whether these differences reflect selection or a causal impact of marriage. Indeed, Table A1 compares cohabiting and married couples, and shows evidence of some degree of advantageous selection into marriage; whether it can account for the entire difference between children of married and cohabiting couples is outside of the scope of this paper.

Figure C7: Cohabiting vs. Married Couples: Post-separation Physical Custody of First Child

Notes: The sample includes all children born in Sweden from 1970 to 1995 who were the parents’ first joint child and whose parents cease to cohabit before the child turns 10 years old. The figure plots the share of children who live with the mother and father, respectively, by the parents’ marital status during cohabitation. The x-axis displays the year relative to the last year before separation (year zero). Separation is defined as the end of cohabitation both for couples that were married while cohabiting and for couples that were unmarried while cohabiting.

63 The address register data used to construct the co-residence indicators is described in Appendix E.1. In the figure, the sample includes all children born in Sweden from 1970 to 1995 who were the parents’ first joint child and whose parents cease to cohabit before the child turns 10 years old.

64 In the year prior to the parents’ separation, all children live with both parents – this arises by definition in this graph, as we define zero to be the last year in which the parents live together.
### Table C5: Child Outcomes by Parental Marital Status

<table>
<thead>
<tr>
<th></th>
<th>Parents Married</th>
<th>Parents Unmarried</th>
<th>Parents Married</th>
<th>Parents Unmarried</th>
</tr>
</thead>
<tbody>
<tr>
<td>Share of childhood that parents are married</td>
<td>0.81</td>
<td>0.00</td>
<td>0.77</td>
<td>0.00</td>
</tr>
<tr>
<td><strong>Test scores 9th grade</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Grade, nationwide test in math (0-20)</td>
<td>11.43</td>
<td>10.10</td>
<td>11.87</td>
<td>10.28</td>
</tr>
<tr>
<td>Grade, nationwide test in Swedish (0-20)</td>
<td>12.17</td>
<td>11.04</td>
<td>12.60</td>
<td>11.21</td>
</tr>
<tr>
<td>Grade, nationwide test in English (0-20)</td>
<td>11.77</td>
<td>10.59</td>
<td>12.21</td>
<td>10.78</td>
</tr>
<tr>
<td><strong>End of 9th grade</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Completed ninth grade on time</td>
<td>0.94</td>
<td>0.89</td>
<td>0.94</td>
<td>0.89</td>
</tr>
<tr>
<td>Eligible for high school</td>
<td>0.93</td>
<td>0.87</td>
<td>0.93</td>
<td>0.87</td>
</tr>
<tr>
<td>Grade Point Average (0-320)</td>
<td>213.43</td>
<td>194.51</td>
<td>218.64</td>
<td>195.75</td>
</tr>
<tr>
<td>Final grade, math (0-20)</td>
<td>12.56</td>
<td>11.35</td>
<td>12.91</td>
<td>11.45</td>
</tr>
<tr>
<td>Final grade, English (0-20)</td>
<td>13.56</td>
<td>12.68</td>
<td>13.85</td>
<td>12.77</td>
</tr>
<tr>
<td><strong>Physical custody</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Lives in same house as father at age 2</td>
<td>0.97</td>
<td>0.80</td>
<td>0.96</td>
<td>0.75</td>
</tr>
<tr>
<td>Lives in same house as father at age 15</td>
<td>0.81</td>
<td>0.57</td>
<td>0.81</td>
<td>0.51</td>
</tr>
<tr>
<td>Lives in same house as mother at age 2</td>
<td>1.00</td>
<td>0.99</td>
<td>1.00</td>
<td>0.99</td>
</tr>
<tr>
<td>Lives in same house as mother at age 15</td>
<td>0.94</td>
<td>0.92</td>
<td>0.94</td>
<td>0.90</td>
</tr>
<tr>
<td><strong>Physical proximity</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Lives in same municipality as father at age 2</td>
<td>0.99</td>
<td>0.93</td>
<td>0.99</td>
<td>0.91</td>
</tr>
<tr>
<td>Lives in same municipality as father at age 15</td>
<td>0.93</td>
<td>0.81</td>
<td>0.93</td>
<td>0.78</td>
</tr>
<tr>
<td>Lives in same municipality as mother at age 2</td>
<td>1.00</td>
<td>1.00</td>
<td>1.00</td>
<td>1.00</td>
</tr>
<tr>
<td>Lives in same municipality as mother at age 15</td>
<td>0.98</td>
<td>0.96</td>
<td>0.98</td>
<td>0.96</td>
</tr>
<tr>
<td><strong>Observations</strong></td>
<td>729462</td>
<td>392233</td>
<td>305549</td>
<td>228751</td>
</tr>
</tbody>
</table>

**Notes:** The sample includes the universe of children born in Sweden from 1985 through 1994, except for the outcomes reflecting results on standardized tests, which are observed for children born from 1988 through 1994. We drop the children for whom the father is missing (1.02% of children) or the mother is missing (0.02 % of the remaining children). The table presents child outcomes by parental marital status. Columns 1 and 2 display summary statistics for children whose parents are married and unmarried, respectively. Parents are classified as married if they married at some point before the child’s 15th birthday. Columns 3 and 4 present the same statistics for firstborn children.
D 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 D.3.4.

D.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, $\tau_w$, are distributed according to some distribution $W$ on $[0, 1]$, and the skills of men, $\tau_m$, according to some distribution $M$ on $[0, 1]$. A match $(\tau_w, \tau_m)$ 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 $\bar{u}_w$ and $\bar{u}_m$, 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 $G_{\tilde{\theta}_3}$ on $(-\infty, +\infty)$ that is conditional on the realization of the marital surplus shock in Stage 2, $\theta_2$. Upon observing this shock, married couples decide whether to divorce or stay married, and dating couples again decide whether to marry or not (recall that not marrying yields the same utility as separating here, since the surplus from cohabitation is normalized to zero). 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 dependent: couples that are “happy” in Stage 2 are not less likely to be “happy” in Stage 3 as well. Specifically, I assume that $G_{\tilde{\theta}_2}$ weakly first-order stochastically dominates $G_{\tilde{\theta}_3}$ if $\theta_2 > \theta_2'$; $G_{\tilde{\theta}_2}(x) \leq G_{\tilde{\theta}_3}(x)$ 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 $\theta_2$, $\theta_2$ 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 $\theta_2$. Because I allow the shocks to be dependent, 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.\footnote{Such “learning by cohabitation” would also occur in an alternative model where couples, instead of experiencing changes in...}
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 \( (\tau_w, \tau_m) \) 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.

D.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 \( \theta_2 \) and \( \theta_3 \) 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

\[
(1 - p) S(\tau_w, \tau_m, \theta_3) + p U_A(\tau_w, \tau_m) \geq 0. \tag{10}
\]

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 (10) 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, \( \theta_2 \), 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 \( \beta(\tau_w, \tau_m) = 1 - G[\theta_{SB}(\tau_w, \tau_m)] \).

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 (2016) finds evidence supporting the assumption that couples experience shocks to actual marital surplus.

66 Specifically, a couple has to renegotiate marital surplus if the wife’s outside option is higher than the post-reform inside option without renegotiation, i.e., the utility that she gets if the post-reform marital surplus is divided using the couple’s pre-reform sharing rule. In this case, renegotiation must occur, in which marital surplus is reallocated from the husband to the wife. I assume that the smallest necessary reallocation of marital surplus will be implemented, that is, that the share of (post-reform) marital surplus will be increased until the wife’s post-reform inside option is exactly equal to her outside option.

67 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.
Consider Stage 2. The couple chooses to marry if and only if the following condition is satisfied:

\[ S(\tau_w, \tau_m, \theta_2) \geq 0 \iff \theta_2 \geq \theta_{NSB}(\tau_w, \tau_m) \equiv -V(\tau_w, \tau_m), \]  

(11)

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

\[
A(\tau_w, \tau_m) = \alpha(\tau_w, \tau_m) E\left[ S(\tau_w, \tau_m, \bar{\theta}_2) \mid \bar{\theta}_2 \geq \theta_{NSB}(\tau_w, \tau_m) \right]
\]

\[
B(\tau_w, \tau_m) = \beta(\tau_w, \tau_m) \left[ (1 - p) E\left[ S(\tau_w, \tau_m, \bar{\theta}_3) \mid \bar{\theta}_3 \geq \theta_{SB}(\tau_w, \tau_m) \right] + pU_A(\tau_w, \tau_m) \right]
\]

and \( \delta \) 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 (but not necessarily unique) match exists, at which the partners’ utilities satisfy** \( u(\tau_w) + v(\tau_m) = M(\tau_w, \tau_m) \).

### D.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.

#### D.3.1 Unmatched and unmarried individuals (Reform announced in Stage 1)

First consider individuals that were unmatched and unmarried (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.

**D.3.2 Matched but unmarried couples (Reform announced in Stage 2)**

Second consider couples that were matched but unmarried (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.

**Predictions MU1 and MU2: Marriage boom consisting of 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 lose 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 \( G_{\theta_2} \). 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 (11) 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 D.3.4, 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 \( \theta_2 \) than couples that marry in Stage 2 also in the absence of reform. While their \( V(\tau_m, \tau_w) \) or \( \theta_2 \) 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 \( \theta_3 \), even in the absence of reform. Others, however, would never have married because their \( \theta_3 \) 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 \( \theta_3 \), testing for existence of extra marriages also offers a test of the assumption that match quality is stochastic.

**Prediction MU3: 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 D.3.4 I also show that the option value \( \Omega_A \) increases with \( p \), the husband’s likelihood of death, which affects the likelihood of payout.

**Prediction MU4: Long-run divorce rate in marriage-boom marriages.** 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 \( \theta_2 \) – which imply a higher threshold \( \theta_3(\tau_m, \tau_w) \) or a lower expected shock \( \theta_3 \).
D.3.3 Matched and married couples (Reform announced in Stage 3)

Third, consider couples that were already matched and married (MM) when the reform was announced, and that faced an \textit{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 D.3.4 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.

D.3.4 Steady state impacts

We compare two regimes, one in which all marital decisions are made in the presence of survivors insurance, and one in which all marital decisions are made without it. This illustrates the impact of an elimination of survivors insurance on the long-run steady state marriage market equilibrium (SS).

**Prediction SS1: Steady state marriage rate.** Survivors’ insurance tied to marriage raises the surplus from marriage relative to cohabitation. Consequently, the reform reduces the surplus from marriage, so a couple who is on the margin of entering marriage in a regime with survivors insurance chooses to cohabit in a regime without it. This lowers the steady state rate of entry into marriage from cohabitation.

**Prediction SS2: Steady state quality of cohabiting unions.** When the marriage rate declines, the average quality of cohabiting unions falls. Intuitively, this is because the couple who is at the margin of entering marriage in a regime without survivors insurance has a higher (expected) match quality than the couple who is at the margin of entering marriage in a regime without it.

Mathematical proofs

**Lemma 1**

\textit{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 \( M_{\text{total}} \) 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. \( \square \)

Results in subsection D.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 \( \tau_w \), given \( \tau_h \).
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

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

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 \( \theta_2 \) and \( \theta_3 \) 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,

\[
\frac{\partial A(\tau_w, \tau_m)}{\partial \tau_w} = -[V(\tau_w, \tau_m) - V(\tau_w, \tau_m)] f(-V(\tau_w, \tau_m)) \left( -\frac{\partial V(\tau_w, \tau_m)}{\partial \tau_w} \right)
\]

\[
+ \int_{-V(\tau_w, \tau_m)}^{\infty} \left[ \frac{\partial V(\tau_w, \tau_m)}{\partial \tau_w} \right] f(\theta_2) d\theta_2
\]

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

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

By contrast, for \( U_A(\tau_w, \tau_m) \neq 0 \), \( B(\tau_w, \tau_m)/(1 - p) \) 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

\[
\frac{\partial B(\tau_w, \tau_m)}{\partial \tau_m} \geq \frac{\partial B(\tau_w, \tau_m)}{\partial \tau_m} \quad \forall \tau_w > \tau_w, \forall \tau_m.
\]

Write

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

The derivative with respect to \( \tau_m \) is

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

I now prove by example that this derivative need not satisfy (12). 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:

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

Fix $\tau_m$. 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 $\tau_w < \tau_m$, (12) 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 D.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 $\Omega_A$ increases in $p$. Write

$$\Omega_A = \delta \left[ B_{\theta_2}(\tau_w, \tau_m) - (1 - p) A_{\theta_2}(\tau_w, \tau_m) \right] \quad (13)$$

where

$$A_{\theta_2}(\tau_w, \tau_m) = \int_{\theta_{NSB}(\tau_w, \tau_m)}^{\infty} [V(\tau_w, \tau_m) + \theta_3] g_{\theta_2}(\theta_3) d\theta_3$$

$$B_{\theta_2}(\tau_w, \tau_m) = \int_{\theta_{SB}(\tau_w, \tau_m)}^{\infty} [(1 - p) [V(\tau_w, \tau_m) + \theta_3] + p U_A(\tau_w, \tau_m)] g_{\theta_2}(\theta_3) d\theta_3.$$ 

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 (13) and rearranging yields

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

where the first integral is positive and the second integral is negative (by the definitions of $\theta_{NSB}(\tau_w, \tau_m)$ in (11)).

The derivative with respect to $p$ is

$$\Omega_A = \delta \int_{\theta_{SB}(\tau_w, \tau_m)}^{\infty} U_A(\tau_w, \tau_m) g_{\theta_2}(\theta_3) d\theta_3 - \delta \int_{\theta_{SB}(\tau_w, \tau_m)}^{\theta_{NSB}(\tau_w, \tau_m)} [V(\tau_w, \tau_m) + \theta_3] g_{\theta_2}(\theta_3) d\theta_3 > 0.$$
Second, I show that the increase in marriages during the grandfathering period comprises retimed marriages and, *given uncertainty about* $\theta_3$, 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 $\theta_3$ 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. It is clear that the couples that the reform induces to marry in Stage 2 have lower $V$ or lower $\theta_2$ than those that marry even absent the reform. For a given $\theta_2$, a couple is less likely to satisfy (10) for lower $V$. And for a given $V$, a couple is less likely to satisfy (10) for lower $\theta_2$ because $G_{\theta_2} (\theta_3)$ first-order stochastically dominates $G_{\theta_2} (\theta_3)$ for all $\theta_2 > \theta_2'$.

**Results in subsection D.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 $u_3^w (\tau_m, \tau_w)$ denote spouse $i$’s utility in Stage 3 if the husband is alive under the contract entered at marriage. These utilities satisfy

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

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

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

For some shocks $\theta_3$, the reform – the abolition of survivors benefits – causes the wife’s expected utility from marriage to be smaller than her outside utility:

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

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

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

To keep the wife in the marriage, the husband would then have to agree to a new sharing rule $\hat{\gamma} > \gamma$ such that

$$(1 - p) \hat{\gamma} [V (\tau_m, \tau_w) + \theta_3] \geq (1 - p) u_w.$$
Results in subsection D.3.4  SS1 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. SS2 follows directly from SS1.

E   Additional Material

E.1   Construction of cohabitation sample

To find cohabiting couples, I use a panel of the complete address registry, for the universe of individuals living in Sweden, from 1975 until 2009. This address registry allows me to observe the exact house in which every individual lives, and hence offers the possibility of generating a definition of a cohabiting couple that does not rely on whether the couple is married or on whether the couple has a joint child.

While a unique source of data for constructing cohabitation, the data source nonetheless implies a few limitations on the sample of cohabiting couples that can be constructed from these data. First, the addresses correspond to households only for individuals who live in houses; when many individuals live at a single address (e.g., apartment buildings, elderly homes, and so on), we no longer are able to match two individuals together based on a joint address. As I describe in detail below, the process enables me to link together cohabiting couples only when they live in households with two or three adults (satisfying certain criteria specified below). While this results in a sample of cohabiting couples based on these particular selection criteria, I maintain the selection criteria throughout the time period, so that the sample (albeit not complete) is constructed in a similar fashion for all years, and, in particular, before and after the reform. This makes the samples of newly cohabiting couples before and after 1990 comparable.

Second, the quality of the address register is poorer before 1981, so I use it to form cohabiting couples only from 1981 and onwards. Below is the detailed process through which the sample of cohabiting couples is created.

I first count how many couples are cohabiting in a given year, say, 1981. To do this, we proceed in the following fashion:

- Start from the population living in Sweden in the particular year (e.g., 1981)
- Remove individuals under the age of 18 in that year
- Keep households of size <=3 (after removing individuals under age 18)
- For households of size=3, drop an individual if they are related to someone else in the household.
  - In particular, this is done through a pairwise comparison process: First, check that each person does not share parents (i.e., are siblings) or grandparents (i.e., are cousins) with someone else in the household.
  - Second, we check that the person is not the father or mother of another person in the household (i.e., households comprising of parent and adult child).
- Keep households of exactly size 2.
- Keep households consisting of one male and one female (heterosexual households)
Drop households where the age difference between the male and female is greater than 25

Second, I define newly cohabiting couples in, say, year 1982 as couples who are cohabiting in 1982 but that were not cohabiting in 1981, and so on.

E.2 Construction of counterfactual frequencies

After the estimation of (2), I take the following steps:

1. I obtain the predicted arithmetic cohort-specific frequencies, \( \hat{N}_{cs} \). \(^{68}\)

2. I aggregate the cohort-specific predicted frequencies to obtain predicted total frequencies for each quarter, for the treated and untreated samples, respectively, by calculating \( \hat{N}_T^s = \sum_{c=1}^{72} \hat{N}_{cs} \) and \( \hat{N}_U^s = \sum_{c=73}^{108} \hat{N}_{cs} \). I refer to the two sample-wide predicted frequencies in the last quarter of 1989, \( s = s^* \), as \( \hat{N}_T^{s*} \) and \( \hat{N}_U^{s*} \), respectively. I refer to the two sample-wide actual frequencies in each quarter as \( N_T^s \) and \( N_U^s \), respectively, and to the two sample-wide actual frequencies in the last quarter of 1989 as \( N_T^{s*} \) and \( N_U^{s*} \), respectively.

3. I then use the coefficients obtained in estimation of (2), but I set \( 1[s = 1989q4] \) and \( 1[s > 1989q4] \) equal to zero, to predict cohort-specific counterfactual frequencies, \( \hat{K}_{cs} \).

4. I aggregate the cohort-specific counterfactual frequencies to obtain predicted total counterfactual frequencies for each quarter, for the treated and untreated samples, respectively, by calculating \( \hat{K}_T^s = \sum_{c=1}^{72} \hat{K}_{cs} \) and \( \hat{K}_U^s = \sum_{c=73}^{108} \hat{K}_{cs} \).

5. In the language of the graphical sketch provided in Figure 4, the estimated total number of induced marriages at the eligibility threshold (due to both retiming and extra marriages), is given by \( B + C \equiv (N_T^{s*} - \hat{K}_T^{s*}) \). Moreover, the estimated sum of missing marriages post reform among treated couples (due to both retiming and a steady state reduction in entry into marriages), is given by \( A + B \equiv \sum_{s > s^*} \left( \hat{K}_T^s - N_T^s \right) \).

6. The number of marriages that are missing due to the steady state reduction among untreated couples is given by \( A_U \equiv \sum_{s > s^*} \left( \hat{K}_U^s - N_U^s \right) \). This translates into a percentage decline in the number of marriages, among untreated couples, of \( \hat{h} \equiv \frac{A_U}{\sum_{s > s^*} (K_U^s)} \).

7. I now have estimates of (A+B), (B+C) for treated couples, and an estimate of (A) for untreated couples.

8. Applying the same percentage steady state decline among treated couples as among untreated couples, I then obtain the number of marriages that are missing due to the steady state reduction among treated couples as \( \hat{h} \equiv \sum_{s > s^*} \left( \hat{h} * \hat{K}_T^s \right) \). This allows me to decompose the boom into A, B, and C.

\(^{68}\)I calculate \( \hat{N}_{cs} = \exp(\hat{n}_{cs} + \frac{\hat{\sigma}_{cs}^2}{2}) = \exp(\hat{n}_{cs})\exp\left(\frac{\hat{\sigma}_{cs}^2}{2}\right) \), where \( \hat{\sigma}_{cs}^2 \) is the squared standard error of the regression. This is because, if \( N_{cs} \) and \( n_{cs} \) are random variables satisfying \( n_{cs} \sim \mathcal{N}\left(\mu, \sigma^2\right) \) and \( N_{cs} = \exp(n_{cs}) \), then \( \mathbb{E}(N_{cs}) = \exp\left(\mu + \frac{\sigma^2}{2}\right) \). Because \( \exp\left(\frac{\hat{\sigma}_{cs}^2}{2}\right) \) is close to one, however, similar results obtain if I let \( \hat{N}_{cs} = \exp(\hat{n}_{cs}) \).
9. Finally, I calculate one estimate of the change in the probability of marriage at the threshold, given by 
\[ \frac{\Delta p_s}{p_s} = \frac{(N^T_s - K^T_s)}{K^T_s}. \]

10. I calculate standard errors for each estimated statistic using the following procedure: 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) 108 cohorts with replacement. Each of my randomly drawn samples thus corresponds to a panel with 108 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.

### E.3 Calculation of the expected value of the annuity

The expected value of the annuity 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, which was roughly one third of mean annual post-tax income.

This estimate is calculated as follows: 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. The average mean pre-tax income in Sweden was \$127000 at the time of reform. Thus, for a couple facing a 35 percent average tax rate, the expected discounted value of the annuity was roughly 30 percent of one year’s income.

### E.4 Calculating an “ever married” elasticity

To further interpret the magnitude of the results presented in Section 6, in addition to the discussion in Section 6.5 I here here use the hazard framework from Section 6.3 to put the response in relation to the share of couples that ever marry. The top panel of Appendix Figure E1 displays the empirical cumulative density function of durations until marriage, relative to the first child’s quarter of birth, in the sample analyzed in Section 6.3. It 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. The figure 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 (the bottom panel illustrates that this “hump” corresponds to the timing of the elimination of survivors insurance in other cohorts of couples as well). 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. 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 \(-0.375\).

E.5 Construction of RD diff-in-diff sample

The eligibility rules governing pre-reform survivors benefits, together with the transition rule, meant that couples that married before the husband turned 60 and were childless on January 1, 1990 obtained the old marriage contract – with survivors insurance – if they married on or before December 31, 1984. Childless couples where the wife was born before 1945 were exempt, that is, they would remain covered after December 31, 1989, even if they entered marriage after December 31, 1984.

The ideal sample would thus consist of all couples that entered marriage close to the eligibility threshold, January 1, 1985, that had no joint child by the reform announcement, June 8, 1988, where the husband was younger than 60 at marriage and the wife was born in 1945 or later. However, matching married individuals into couples poses a challenge in the absence of children since spouses cannot be linked using the dataset linking each child to its mother and father, and since comprehensive relationship codes are lacking. To construct my sample, I therefore start from all individuals that entered marriage at the eligibility threshold, January 1, 1985, or plus / minus 180 (150) days. Because the reform only affected the subset of married couples that were childless, I exclude all couples that had a joint child within the first four years (and nine months) of marriage, so that no couple in the treatment group had conceived a child at the time of the reform announcement. I further exclude women born before 1945 and men who were 60 years or older at the date of marriage.

Thus, I know with certainty that no individual who is included in my final sample had conceived a joint child with their spouse on or before the reform announcement of June 8, 1988 (even though I may not know the identity of the spouse). However, my final sample of men, which all entered marriage before the age of 60, includes (i) men who married women who were born before 1945, and (ii) men who married women who were born after 1944. If I were able to match all men and women into couples, I would remove all men who married a woman who was born before 1945, because these men (couples) were unaffected by the reform (they remained covered by survivors insurance after January 1, 1990 as well). Because I cannot exclude these men, however, I end up counting them as treated (losing survivors insurance by marrying after December 31, 1984) even though they were in couples that remained covered regardless of their date of marriage. This may 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 in the analysis of divorce; however, using the male sample leaves the results unchanged.

E.6 Prediction MM2: Division of marital surplus

In addition to prediction MM1, the theoretical model yields a second predicted impact on matched and married couples concerning the reform’s impact on the division of marital surplus. The prediction is as follows:

Prediction MM2: Division of marital surplus. 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.

Testing prediction MM2 Here I discuss this prediction further and provide some (albeit imperfect) empirical evidence on it using spousal labor supply. 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.

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. 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.

69This analysis 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), and Voena (2014).

70This 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.
Results  Husbands’ labor supply. Table E1 presents estimation results for the outcome variable $Y_{it} = \text{Husb}_{ls}it$, 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 E2. 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.

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.

Wives’ labor supply. To give a complete picture of household labor supply, Table E3 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.

E.7 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 visible in Figure 10, 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

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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.
While a detailed analysis of this is outside the scope of this paper, to illustrate that such impacts may be a possibility, Appendix Figure E2 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 (Björklund 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.

E.8 Additional Material: Figures and Tables

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\(^{72}\)For evidence that admittance to university has substantial returns in the marriage market, see, e.g., Kaufmann et al. (2013).
Figure E1: Empirical Cumulative Distribution Functions of Marriages

(a) All couples in hazard analysis sample

(b) Couples whose first joint child was born in 1985

(c) Couples whose first joint child was born in various years

Notes: This Figure presents the empirical CDF of durations until marriage relative to the first child’s quarter of birth, with each couple entering the risk pool seven years (28 quarters) earlier. They display the “raw data,” in the sense that they show Kaplan-Meier failure functions, and their 95% confidence intervals, estimated without any covariates. In Figure E1a, the sample includes all couples included in the hazard analysis in Section 6.3, i.e., all couples whose first child was born from 1975 through 1988. This sample includes couples that are “hit” by the reform at different durations from childbirth, and hence at a different point along the x-axis. In Figure E1b, the sample includes only the (four) cohorts of couples who had their first joint child in 1985, and the dashed green line indicates the timing of the removal of survivors insurance. In Figure E1c, the sample 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.
Table E1: RD Difference-in-differences Estimates: Impact on Husband’s Labor Supply (Extensive Margin)

<table>
<thead>
<tr>
<th>Husband working in</th>
<th>Bandwidth 150 days</th>
<th></th>
<th>Bandwidth 180 days</th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Polynomial</td>
<td>2</td>
<td>3</td>
<td>Polynomial</td>
</tr>
<tr>
<td>1988</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>0.0114 (0.0183)</td>
<td></td>
<td>0.0128 (0.0158)</td>
<td></td>
</tr>
<tr>
<td></td>
<td>0.0133 (0.0199)</td>
<td></td>
<td>0.0122 (0.0174)</td>
<td></td>
</tr>
<tr>
<td>Mean, dept. var</td>
<td>0.84</td>
<td>10551.45</td>
<td>0.85</td>
<td>14071.13</td>
</tr>
<tr>
<td>AIC</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>2000</td>
<td>0.0235 (0.0215)</td>
<td></td>
<td>0.0392 (0.0188)</td>
<td></td>
</tr>
<tr>
<td></td>
<td>0.0216 (0.0233)</td>
<td></td>
<td>0.0269 (0.0207)</td>
<td></td>
</tr>
<tr>
<td>Mean, dept. var</td>
<td>0.56</td>
<td>14722.79</td>
<td>0.58</td>
<td>20695.93</td>
</tr>
<tr>
<td>AIC</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>2004</td>
<td>0.0456** (0.0215)</td>
<td></td>
<td>0.0449** (0.0189)</td>
<td></td>
</tr>
<tr>
<td></td>
<td>0.0406* (0.0233)</td>
<td></td>
<td>0.0475** (0.0209)</td>
<td></td>
</tr>
<tr>
<td>Mean, dept. var</td>
<td>0.51</td>
<td>14737.32</td>
<td>0.53</td>
<td>20906.15</td>
</tr>
<tr>
<td>AIC</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>2008</td>
<td>0.0364* (0.0207)</td>
<td></td>
<td>0.0369** (0.0183)</td>
<td></td>
</tr>
<tr>
<td></td>
<td>0.0256 (0.0224)</td>
<td></td>
<td>0.0343* (0.0202)</td>
<td></td>
</tr>
<tr>
<td>Mean, dept. var</td>
<td>0.44</td>
<td>13707.26</td>
<td>0.47</td>
<td>19618.41</td>
</tr>
<tr>
<td>AIC</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Number of couples (husbands)</td>
<td>13153</td>
<td>13153</td>
<td>18928</td>
<td>18928</td>
</tr>
</tbody>
</table>

Notes: The sample includes all men in the baseline and placebo samples, which are described in detail in the notes of Table 1, Columns 4-5. The dependent variable is an indicator variable taking the value one if the husband was in the labor force in year \( x \), for the values of \( x \) indicated in column 1. The table reports the RD difference-in-differences estimates obtained using different orders of polynomials and different bandwidths. Each cell represents a separate 2SLS regression. Robust standard errors in parentheses.

* \( p < 0.10 \), ** \( p < 0.05 \), *** \( p < 0.01 \).
### Table E2: Impacts on Husband Labor Supply Driven by Men Close to Retirement

<table>
<thead>
<tr>
<th>Subsample of couples: Husband age</th>
<th>All couples</th>
<th>H younger than 63</th>
<th>H older than 62</th>
</tr>
</thead>
<tbody>
<tr>
<td>Husband working in 2008</td>
<td>0.0369**</td>
<td>0.0140</td>
<td>0.0651***</td>
</tr>
<tr>
<td></td>
<td>(0.0183)</td>
<td>(0.0274)</td>
<td>(0.0210)</td>
</tr>
<tr>
<td>Mean, dept. var</td>
<td>0.47</td>
<td>0.69</td>
<td>0.14</td>
</tr>
<tr>
<td>Number of couples (husbands)</td>
<td>18928</td>
<td>11158</td>
<td>7770</td>
</tr>
</tbody>
</table>

**Notes:** In the first column, the sample includes all men in the baseline and placebo samples, which are described in detail in the notes of Table 1, Columns 4-5. The second and third columns present separate results for two strict subsets of this sample, based on husband age in 2008. The dependent variable is an indicator variable taking the value one if the husband was in the labor force in year 2008. The table reports the RD difference-in-differences estimates obtained using a second order polynomial and a bandwidth of 180. Robust standard errors in parentheses.

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

### Table E3: RD Difference-in-differences Estimates: Impact on Wife’s Labor Supply (extensive margin)

<table>
<thead>
<tr>
<th>Wife working in</th>
<th>Bandwidth 150 days</th>
<th>Bandwidth 180 days</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Polynomial</td>
<td>Polynomial</td>
</tr>
<tr>
<td></td>
<td>2</td>
<td>3</td>
</tr>
<tr>
<td>1988</td>
<td>-0.0277</td>
<td>-0.0331</td>
</tr>
<tr>
<td></td>
<td>(0.0254)</td>
<td>(0.0276)</td>
</tr>
<tr>
<td>Mean, dept. var</td>
<td>0.79</td>
<td>0.79</td>
</tr>
<tr>
<td>AIC</td>
<td>8836.07</td>
<td>8839.43</td>
</tr>
<tr>
<td>2000</td>
<td>0.0143</td>
<td>0.0135</td>
</tr>
<tr>
<td></td>
<td>(0.0296)</td>
<td>(0.0322)</td>
</tr>
<tr>
<td>Mean, dept. var</td>
<td>0.66</td>
<td>0.66</td>
</tr>
<tr>
<td>AIC</td>
<td>11646.21</td>
<td>11650.04</td>
</tr>
<tr>
<td>2004</td>
<td>0.0276</td>
<td>0.0238</td>
</tr>
<tr>
<td></td>
<td>(0.0297)</td>
<td>(0.0329)</td>
</tr>
<tr>
<td>Mean, dept. var</td>
<td>0.64</td>
<td>0.64</td>
</tr>
<tr>
<td>AIC</td>
<td>11718.52</td>
<td>11721.06</td>
</tr>
<tr>
<td>2008</td>
<td>0.0267</td>
<td>0.0241</td>
</tr>
<tr>
<td></td>
<td>(0.0299)</td>
<td>(0.0326)</td>
</tr>
<tr>
<td>Mean, dept. var</td>
<td>0.61</td>
<td>0.61</td>
</tr>
<tr>
<td>AIC</td>
<td>11831.99</td>
<td>11835.90</td>
</tr>
<tr>
<td>Number of couples (wives)</td>
<td>9117</td>
<td>9117</td>
</tr>
</tbody>
</table>

**Notes:** The sample includes all women in the baseline and placebo samples, which are described in detail in the notes of Table 1, Columns 4-5. The dependent variable is an indicator variable taking the value one if the wife was in the labor force in year \( x \), for the values of \( x \) indicated in column 1. The table reports the RD difference-in-differences estimates obtained using different orders of polynomials and different bandwidths. Each cell represents a separate 2SLS regression. Robust standard errors in parentheses.

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