Social Insurance and the Marriage Market

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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 to marriage affects the marriage market. Exploiting Sweden's elimination of survivors insurance, I demonstrate that severing this link (1) affected entry into marriage up to 50 years before expected payout, (2) raised the divorce rate by 10%, and (3) 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.

I. 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% 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 am grateful to Pierre-André Chiappori, Wojciech Kopczuk, and Navin Kartik for their generous support at all stages of this project. I also thank Ran Abramitzky, S. Anukriti, Clément de Chaisemartin, Juanna Joensen, Lisa Jönsson, Arizo Karimi, Todd Kumler, Samuel Lee, Ben Marx, David Munroe, Suresh Naidu, Torsten Persson, Giovanni Paci, Kiki Pop-Eleches, Malgorzata Poplawska, Maya Rossin-Slater, Bernard Salanié, Anna Sjögren, Reed Walker, Heidi Williams, Stephen Zeldes, Björn Öckert, and seminar participants at numerous universities and conferences for helpful discussions, suggestions, and comments. Funding from the Center for Retirement Research at Boston College, grant 5001537 from the Social Security Administration, and the Hewlett Foundation/IIE (Institute of International Education) Dissertation Fellowship in Population, Reproductive Health, and Economic Development is gratefully acknowledged. Data are provided as supplementary material online

Electronically published December 9, 2019
[*Journal of Political Economy*, 2020, vol. 128, no. 1]
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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. As many countries do today, Sweden used to provide survivors insurance through the marriage contract. A widow was granted a lifetime annuity of survivor 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 $5,000, for an average duration of 8 years (sec. II provides details). But to most couples, who entered marriage in their 20s or 30s, the insurance was not likely to pay out 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 the 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 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 III 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 depends on whether the individual, at the time of the reform's announcement, is married, matched but yet unmarried, or unmatched. For

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each group of individuals, I present precise, testable predictions about initial responses and about future behavior conditional on these initial responses. I test these by using individual-level marital and tax records, described in section IV.

In section V, I start by exploring the long-run consequences of eliminating survivors insurance from the marriage contract. First, comparing couples who initiate cohabitation well before and well after the reform suggests that the reform is associated with a reduction in entry into marriage, consistent with its 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 VI, 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, \$4,375 at the notch, about 45,000 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 they 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 retime their marriages, I show that approximately 47% 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 remained in place.[1] Further, I hypothesize that the hastened sorting process in the grandfathering period yields a lower average match quality

[1] Intuitively, the reform attaches option value to marrying fast; this causes couples to rush to marriage who otherwise would have waited. Some of these couples would, in the absence of reform, never have decided to marry.

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in marriage-boom marriages. Indeed, I show that marriages in the boom are 5 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 were underdeveloped in Sweden at the time of reform.

In section VII, I analyze the causal impact of (losing) survivors insurance on exit from marriage. I exploit the fact that, for some couples who were already married at the reform's announcement, grandfathering rules induced variation in survivors insurance coverage. Specifically, couples who married before January 1, 1985, were allowed to keep the contract they married into; for most couples who married thereafter, this contract was revoked and replaced with the new one. This change was announced in June 1988—3-1/2 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 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 10%.

Finally, section VIII analyzes how tying social insurance to marriage affects the assortativeness of matching. Because the annuity replaced household income that was lost as a result of the husband's death, payments were higher in couples with more spousal specialization in market and nonmarket 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 who marry a woman of lower skill. I show that the share of skilled men who enter assortatively matched marriages increases by 4 percentage points (11%) after the introduction of the new marriage contract. This suggests that survivors insurance linked to marriage promoted spousal specialization in market and nonmarket 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

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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 responses[3] 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 stems 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.

Third, while the previous literature has studied responses along the entry and exit margins, there is very little evidence on how taxes and benefits

[2] Existing 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 United States, Alm and Whittington (1995), Whittington and Alm (1997), and Bitler, Gelbach, and Hoynes (2006) find that a smaller marriage tax penalty increases the rate of marriage relative to divorce or nonmarriage; in contrast, Bitler et al. (2004) and Fitzgerald and Ribar (2005) fail to robustly document any statistically significant responses. See Moffitt (1998) and Alm, Dickert-Conlin, and Whittington (1999) for surveys of the literature. Outside of the United States, see, e.g., Frimmel, Halla, and Winter-Ebmer (2014), who show that the removal of cash transfers upon marriage temporarily raises the marriage rate in Austria. A smaller literature has analyzed marital responses to incentives inherent in the social security system. In the United States and Canada, respectively, Brien, Dickert-Conlin, and Weaver (1996) and Baker, Hanna, and Katarevic (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 they find little or no response to this incentive (Dickert-Conlin and Meghea 2004; Goda, Shoven, and Slavov 2007; Dillender 2016).

[3] See the discussion in n. 2.

[4] I 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.

[5] Chiappori 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.

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affect the assortativeness of matching.[6] The literature on matching has established preferences for nonmeritocratic 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, Delavande, and Vasconcelos 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.[7] I instead document responses to benefits that pay out only 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.[8] This 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.[9] 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

[6] 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 in identifying cohabiting couples in most data sets—a hurdle that I am able to overcome by leveraging detailed Swedish administrative data. My analysis of exit from marriage 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 (2015).

[7] 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; Goda, Shoven, and Slavov 2007; Dillender 2016).

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

[9] A positive correlation between risk type and demand for insurance, at given prices, suggests the presence of asymmetric information (Chiappori and Salanié 2000). The empirical evidence from annuities and life insurance markets is mixed. For example, Finkelstein and Poterba (2002, 2004) reject the null hypothesis of symmetric information in UK annuities markets, and Cawley and Philipson (1999) find no evidence of adverse selection in the US life insurance market, whereas He (2009) does.

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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 hinders private annuities markets from developing.

II. Institutional Background

A. *Survivors Insurance in Sweden*

1. Prereform 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 old at marriage and (1) they had a joint child or (2) they had been married for at least 5 years. Each married couple who satisfied one of these conditions was covered by survivors insurance during marriage. This scheme included the overwhelming majority of all married couples: among couples who 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 United States, earned benefits were proportional to lifetime earnings up to a ceiling (see app. B for details; apps. A–E are available online). 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 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 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 35,000 (~$5,000), and the average

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duration of payments was 8 years. Upon realization, the value of the average annuity, applying a zero discount rate, was SEK 280,000 (~$40,000).

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 years. Payout was thus, on average, expected to occur several decades after marriage.

2. Postreform 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 1-year "adjustment transfer" amounting to 40% of the deceased spouse's earned benefit. Thereafter, the surviving spouse received Social Security income based solely on his or her own earned benefit, just as a divorced spouse would.

3. 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 who would have been eligible for survivors insurance if the husband had died on December 31, 1989, got prereform survivors insurance; all other couples got postreform survivors insurance. I refer to the "old marriage contract" as the contract that came with prereform survivors insurance and to the "new marriage contract" as the contract that came with postreform survivors insurance (but otherwise was identical). The eligibility rules governing prereform survivors benefits, together with the transition rule, meant that couples who married before the husband turned 60 obtained the old marriage contract if they (1) had a joint child on or before December 31, 1989, and married on or before the same date or (2) had no joint child on or before December 31, 1989, but married on or before December 31, 1984.[10]

4. "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

[10] Couples where the wife was born before 1945 were exempt from condition 2 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.

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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 suggest that entry into parenthood was unaffected by the reform. Figure 1*A* plots the number of couples who have

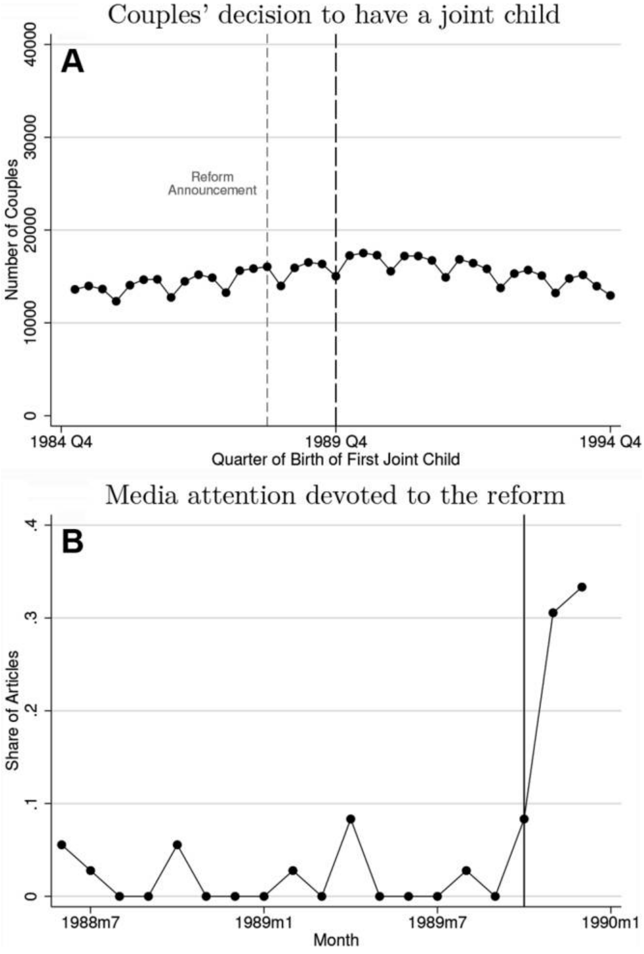

FIG. 1.—Couples' decision to have a joint child and the "effective" reform announcement date. *A*, The sample includes the universe of all children born from 1985 through 1994 who were their parents' first joint child. The figure plots the number of first births—i.e., the number of couples who have a first joint child—by quarter. The thick vertical line indicates the last quarter before the survivors insurance reform was implemented. The thin vertical line indicates the quarter of reform announcement. *B*, This plot 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 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.

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a first joint child, at a quarterly level, around the reform. The empirical distribution is smooth around the threshold.[11]

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 1*B* 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 3*A*, 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.[12] Given that childbearing was unaffected by the reform, I henceforth focus on marriage market responses.

B. 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.[13]

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.[14]

Separation or divorce.—In case of divorce, married individuals' assets are considered marital property 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.[15] Moreover, the law stipulates a right to alimony payments for the

[11] 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.

[12] Two working papers use this reform as an instrument for marriage to study the impact of marriage on child welfare (Björklund, Ginther, and Sundström 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.

[13] Most taxes and benefits were decoupled from marriage in 1971, when joint taxation of labor income was eliminated.

[14] While the marriage contract had 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 other than the spouse).

[15] Specifically, if the cohabiting couple lives in a property that was (1) acquired during the cohabitation period and (2) acquired with the intent of being used jointly, then this

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economically disadvantaged spouse upon divorce but not upon separation from cohabitation.[16]

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;[17] 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% of the cases.[18] Thus, 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 before 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 sec. VI.B). In appendix C, I examine whether two key child outcomes differ, depending on the parents' marital status: educational attainment and the share of children who live with the mother or father 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

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.

16 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.

17 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 identify the father for 96.8% of all children born in Sweden. Thus, while critical in the United States (Rossin-Slater 2017), paternity establishment is not an issue in the Swedish context.

18 My data do 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.

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causal impact of marriage. Indeed, table A1 (tables A1–A4, C5, and E1–E3 are available online) 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 the scope of this paper.

III. 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.[19]

A. 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.

B. 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

[19] 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.

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the individual, at the time of the reform's announcement, is married, matched but yet unmarried, or unmatched.

1. Transition I: Matched but Unmarried Couples

Couples who 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 who would have continued to cohabit in the absence of reform to instead enter marriage by the end of 1989.

PREDICTION MU2 (Retimed and extra marriages). The couples who 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 who 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 was, in fact, too low to warrant entering 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 intertemporal 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 and 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.

2. Transition II: Matched and Married Couples

Consider couples who were already matched and married (MM) when the reform was announced and who faced an ex post elimination of survivors insurance.

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PREDICTION MM1 (Marital instability). When insurance is removed from marriage, marital surplus falls. This induces some married couples to divorce.

3. Transition III: Unmatched and Unmarried Individuals

Consider individuals who 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.

IV. 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 who 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's and father's ID. While relationship codes link spouses together, these data are of rather poor quality. I therefore link spouses by 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 who 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 who initiate cohabitation—that is, 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

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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).[20]

V. 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 who enter cohabitation 9 years before the reform and 10 years thereafter. Summary statistics of this sample are described in table A1.[21]

A. *Prediction SS1: Steady-State Marriage Rate*

Prediction SS1 concerns the steady-state rate of entry into marriage from cohabitation. Figure 2*A* displays the shares of couples who marry within 3, 5, or 7 years of moving in together by the year of initiation of cohabitation. Intuitively, couples who initiate cohabitation in a particular year are deciding whether to enter marriage or not, trading off the costs and benefits of marriage relative to those of cohabitation. Among couples who move in together in 1981, approximately 35% 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 2*A*, couples are directly affected by the reform within 3, 5, or 7 years of 1990; 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.[22] The figure provides visual evidence suggesting that the marriage rate was declining during the prereform period but that the reform induced a long-run drop relative to this trend. Indeed, fitting a linear trend to the prereform data points that are unaffected by the reform and predicting into the postreform period suggests

[20] While I observe the address panel data from 1975 to 2009, the quality is poor before 1981. Further, the panel ends in 2000 (9 years before 2009) in order to capture the outcome "separation within 9 years." Additional details on this sample are provided in app. E.1.

[21] This table displays summary statistics for couples who initiate cohabitation between 1985 and 2000, because I observe educational attainment and income starting only in 1985.

[22] The dots in the figure illustrate marital behavior among couples who initiate cohabitation at the cusp of the reform, in 1991.

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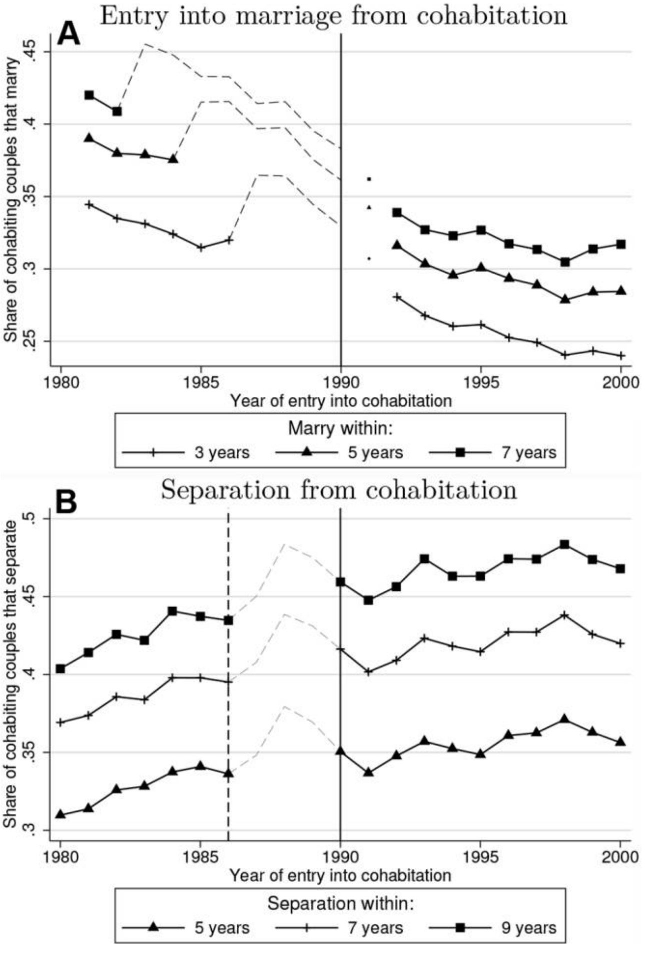

FIG. 2.—Cohabitation versus marriage and the quality of cohabiting unions. *A*, This plot is constructed from a sample of cohabiting couples who initiate cohabitation between 1981 and 2000 (inclusive), described in detail in app. E.1. The figure displays the share of couples who marry within 3, 5, or 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. *B*, The sample is further 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 who move apart, within 5, 7, or 9 years, by year of initiation of cohabitation. The dashed portion of each line represents couples who are affected by the transition between survivors insurance regimes (more specifically, those 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 who choose not to marry.

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that the 3-year marriage rate falls by an average of 6.2% over the years 1992 to 2000 relative to the postreform marriage rate predicted from the pre-reform data.[23]

This is merely a "sanity check" of prediction SS1—it relies on a simple linear prediction out of the sample;[24] moreover, the estimate is sensitive to the choice of polynomial. In section VI.B, I estimate the impact on long-run steady-state marriages by using population-level data for a larger range of years. While that analysis is restricted to couples with children, I am able to leverage a methodology that assuages these concerns and delivers a more robust estimate. Interestingly, in section VI.B, I obtain an estimate of the long-run steady-state reduction in entry into marriage of 5.6%, which is close to the one obtained from this simple "sanity check" of prediction SS1.

B. *Prediction SS2: Steady-State Quality of Cohabiting Unions*

Prediction SS2 concerns the quality of cohabiting unions and follows directly from prediction SS1: as the marriage rate declines after the reform (prediction SS1), the average quality of cohabiting unions should fall. By prediction 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 2*B*, the sample is restricted to the subset of couples who did not enter marriage within 2 years of moving in together. The figure displays the share of such couples who move apart within 5, 7, or 9 years, by the year of initiation of cohabitation. The dashed portion of each line represents couples who are affected by the transition between survivors insurance regimes (more specifically, those who 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. Prediction SS2 suggests that, after 1990, we should observe a worse average match quality among cohabiting couples, that is, a higher rate of separation. The raw data in figure 2*B* indeed offer suggestive evidence in support of this conjecture.

VI. Survivors Insurance and Selection into Marriage

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

[23] Figure A1*a* displays the 3-year marriage rate along with the linear prediction.

[24] If 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%.

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A. *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 who had at least one child on or before December 31, 1989, with an incentive to enter marriage by the deadline; other couples faced no such incentive. Importantly, as shown in section II.A above, entry into parenthood was unaffected by the reform. Nonetheless, I define the sample of couples who were incentivized to marry fast as those whose first joint child was born before 1989 (and starting in 1971, when the child data starts).[25] I refer to this as the *treated sample*.

Figure 3*A* 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 reveal 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 3*B* displays the empirical distribution of marriages among couples whose first joint child was conceived after the elimination of survivors insurance.[26] These couples faced no economic incentives to marry fast; I therefore refer to this sample as the *untreated sample*. Figure 3*B* shows no spike in marriages in the last quarter of 1989, suggesting that economic incentives were central to the boom. Interestingly, figure 3 reveals 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 VI.B below.[27]

To quantify the marriage boom in figure 3*A*, 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 unmanipulated 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

[25] Children born before 1989 were conceived before the reform announcement in June 1988.

[26] Specifically, the sample depicted in fig. 3*B* includes all couples whose first joint child was born from 1991 (and thus conceived in 1990) through 1998.

[27] In particular, as I discuss more below, part of the reason for the decrease in marriages after 1990 visible in fig. 3*A* is that the sample consists of couples who already had had their first joint child (by 1990) and who therefore were less likely to marry thereafter. Similarly, fig. 3*B* 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).

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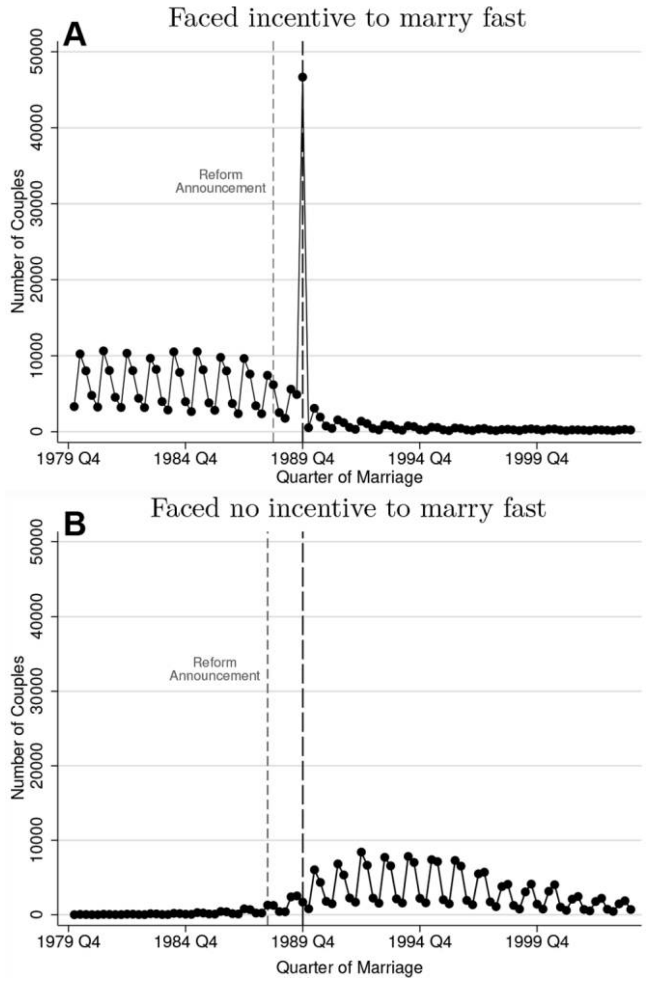

FIG. 3.—Empirical distributions of new marriages. This figure displays the empirical distribution of marriages in Sweden, by quarter, from 1980 through 2003, for two nonoverlapping samples of couples. In *A*, the sample includes all couples whose first joint child was born before January 1, 1989 (and 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 *B*, 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 short-dashed vertical line indicates the quarter of the reform announcement. The long-dashed vertical line indicates the last quarter of 1989. *A* displays a marriage boom in the last quarter of 1989; in contrast, *B* displays no such boom.

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TABLE 1
SUMMARY STATISTICS

	Analysis of Entry into Marriage			RD Design Analysis: Baseline or Placebo Sample	
	Date of Marriage				
	1980–2003 (1)	1980–88 (2)	1989 (3)	Around January 1, 1985 (4)	Around January 1, 1984 (5)
Demographic characteristics at marriage:					
Husband age (years)	31.49	29.82	35.12	36.70	36.47
Wife age (years)	28.72	27.05	32.23	28.82	28.45
Husband high school	.51	.49	.51	.40	.40
Wife high school	.54	.52	.55	.45	.42
Husband college	.23	.25	.19	.21	.20
Wife college	.23	.25	.20	.19	.21
Husband marriage number	1.12	1.11	1.11	1.44	1.42
Wife marriage number	1.11	1.11	1.09	1.30	1.29
Economic characteristics at marriage:					
Husband log labor income	9.86	9.65	11.77	11.29	11.30
Wife log labor income	9.03	8.88	10.71	10.78	10.82
Fertility behavior:					
Couple's completed fertility	2.26	2.26	2.23		
First child out of wedlock	.66	.52	1.00	0	0
Observations	306,822	220,069	58,939	17,047	15,794

NOTE.—Column 1 presents summary statistics for the sample of couples who faced an incentive to marry fast in response to the elimination of survivors insurance, used in the bunching analysis. The sample includes all couples who had a joint child between January 1, 1971 and December 31, 1988, and 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 col. 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 col. 4, I start from all individuals who entered marriage within 180 days of January 1, 1985. Because the reform affected only the subset of already-married couples who were childless, I exclude all couples who had a joint child within the first 4 years and 9 months of marriage. I further exclude women born before 1945 and men who were 60 or older at the date of marriage. This baseline sample captures individuals who were eligible for survivors insurance when the reform was announced but 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 col. 5 is defined analogously for individuals who entered marriage within 180 days of January 1, 1984. The number of observations refers to the number of couples in cols. 1–3 and to the number of husbands plus the number of wives in cols. 4–5.

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 3*A*, the density never "resumes" a pattern similar to that before the reform. I

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therefore start by using only prereform data in the treatment sample, that is, 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:

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

where N_s is the number of marriages in quarter s; $\mathbf{1}[s = s^*]$ 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 ζ_q are quarter fixed effects. Intuitively, I fit a polynomial to the counts before 1990Q1 plotted in figure 3*A*, accounting for seasonality, and include an indicator variable for the last quarter of 1989. Here, β measures the size of the marriage boom in the last quarter of 1989. All estimates are in the range of 46,000 marriages.[28]

B. Prediction MU2: Retimed and Extra Marriages

1. Approach to Decomposition

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 denoted *A*, retiming *B*, and extra marriages *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 in figure 3*A*, I can quantify

- $A + B$, the total missing mass due to both effects, and
- $B + C$, the total number of extra and retimed marriages.[29]

[28] 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 $\mathbf{1}[s = s^*]$ equal to zero) is rejected, with *t*-statistics that imply *p*-values satisfying $p < 10^{-9}$. (Results are available upon request.)

[29] Similar in spirit, Best and Kleven (2017) show that a temporary tax cut in housing transaction taxes in the United Kingdom yields both a timing effect and an effect akin

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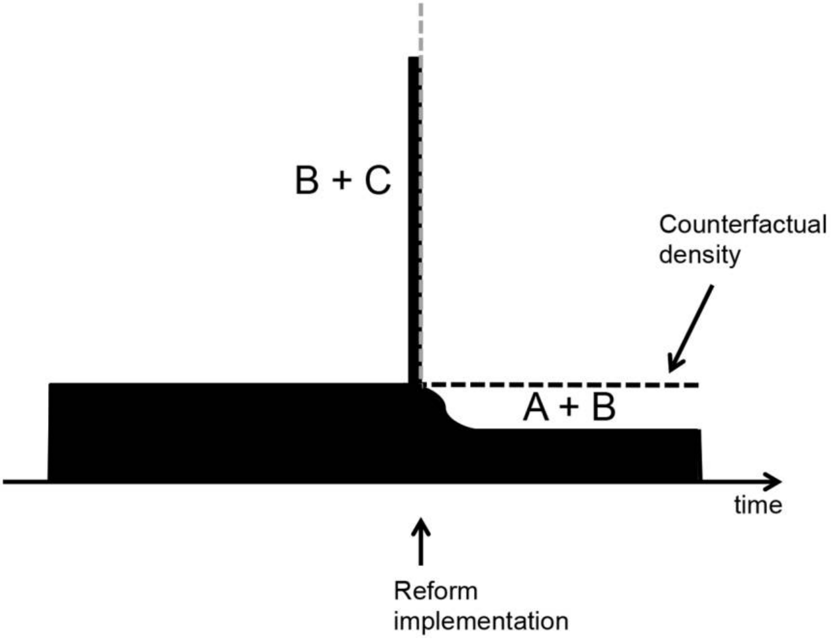

FIG. 4.—Simple sketch of the steady-state drop, retiming, and extra marriages. This figure provides a simple sketch of how the theory predicts that retimed and extra marriages (prediction MU2), as well as the steady-state reduction in entry into marriage (prediction SS1), appear in the empirical distribution. 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 denoted A, retiming B, and extra marriages C. At the cusp of the reform, the empirical distribution displays bunching, as a result of 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, because of 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 postreform period (B).

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 by using couples who 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 figure 3*B*. In the empirical framework presented next, I use both the treated and untreated samples and simultaneously estimate $A + B$, $B + C$, and A.

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).

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2. 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.[30] 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. Figures 5*A* and 5*B* plot marital behavior for couples whose first joint child was born in 1987–88 and 1983–84, respectively. In both panels, entry into marriage is concentrated around the date of first childbirth. Thus, even though I observe prereform marital behavior only until 1989 for all couples, in figure 5*B* I observe prereform 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 recentering 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 figure 5*C*. Further, I exploit the fact that, in the untreated sample, no couples were "hit" by the reform.[31]

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 32 cohorts, $c \in \{73, 104\}$. I estimate the following regression:

$$\begin{aligned} n_{cs} = \alpha &+ \mathbf{1}[s = s^*]\sum_{c=1}^{72}\beta_c + g(s) + \zeta_q + \eta_c \\ &+ \mathbf{1}[s > s^*]\sum_{c=1}^{104}\gamma_c + h(t_{\text{prebirth}}) + j(t_{\text{postbirth}}) + \varepsilon_{cs}, \end{aligned} \tag{2}$$

where n_{cs} is the natural logarithm of the number of marriages in quarter s and cohort c.[32] As in equation (1) above, β_c captures bunching at the notch, and the magnitude of this response is now allowed to be different for each cohort in the treated sample, $c \in T$. Moreover, $g(s)$ is a higher-order

[30] Section VI.A above uses prereform data in fig. 3*A* 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 eq. (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.)

[31] 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 features similar to the empirical distributions of new marriages in the baseline sample.

[32] I use the natural logarithm, $n_{cs} = \ln(N_{cs})$, because the cohort-specific distributions of new marriages exhibit nonlinearities, as illustrated in fig. 5.

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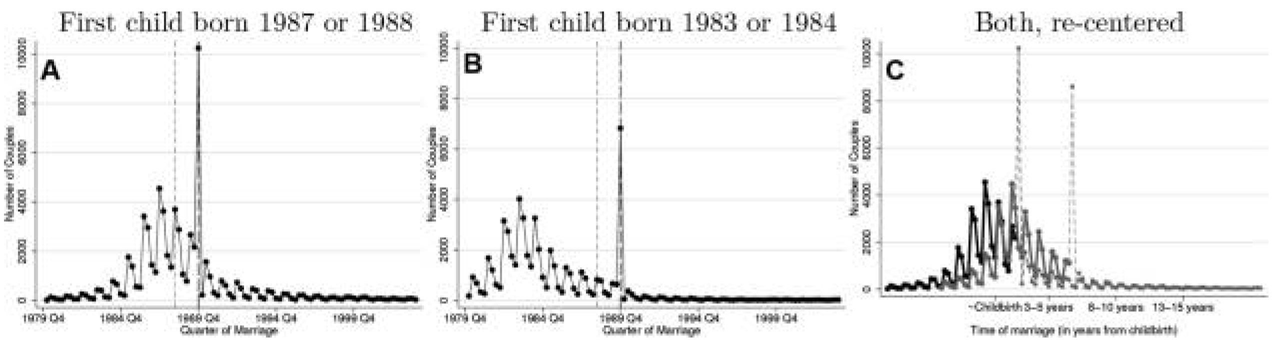

FIG. 5.—Empirical strategy: intuition. This figure replicates fig. 3*A* for subsets of the sample, described in detail in the fig. 3 legend. *A*, New marriages among couples who had their first joint child in 1987 or 1988. *B*, New marriages among couples who had their first joint child in 1983 or 1984. In both panels, entry into marriage is concentrated around the date of first childbirth. Thus, even though I observe prereform marital behavior only until 1989 for all couples, I observe prereform marital behavior for a longer period of time relative to the date of birth of a couple's first joint child in *B* than in *A*. *C* lays *A* (in black) and *B* (in gray) on top of each other, recenters the distributions around the date of childbirth, and illustrates with thin dashed lines the marital behavior that takes place after the reform and thus cannot be used to predict postreform 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 after childbirth is observable for a longer period of time before the 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 postchildbirth, prereform marital behavior.

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polynomial in time (quarter), and ζ_q are quarter fixed effects. In addition, equation (2) includes cohort fixed effects, η_c, and cohort-specific reductions in entry into marriage after the notch, γ_c.[33] The functions $h(t_{\text{prebirth}})$ and $j(t_{\text{postbirth}})$ 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 γ_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 γ_c capture only the steady-state reduction.

Recovering retimed and extra marriages.—I use the coefficients obtained in estimation of equation (2) but set $\mathbf{1}[s = s^*]$ and $\mathbf{1}[s > s^*]$ 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 $\Delta p_{s^*}/p_{s^*}$; this simply is the ratio of the estimated boom in 1989Q4 (numerator) to the estimated counterfactual number of marriages in 1989Q4 (denominator). I discuss the interpretation of this ratio in section VI.E below. I calculate standard errors for each estimated statistic, using a cluster bootstrapping procedure. Appendix E.2 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 η_c allows for vertical shifts of each cohort's recentered 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 table A2 and represent the preferred bunching estimation specification.[34] The yellow

[33] Given that n_{cs} is the natural logarithm of the number of marriages, β_c, η_c, and γ_c can be thought of as proportional.

[34] Specifically, I performed the estimation using three different higher-order polynomials, and the results that are presented graphically are the ones that minimize the Akaike information criterion (AIC) using fourth-order polynomials.

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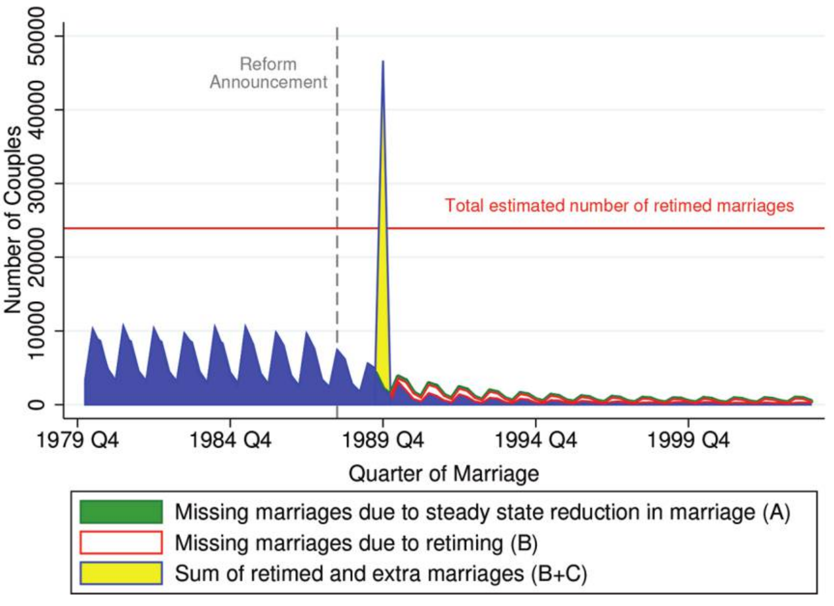

FIG. 6.—Decomposition of the marriage boom. This figure presents the results of the bunching decomposition estimation among couples who faced an incentive to marry before 1990 to secure survivors insurance. See the fig. 3 legend for more information on the definition of the analysis sample, which includes all couples included in the "treated sample" depicted in fig. 3*A* as well as all couples included in the "untreated sample" depicted in fig. 3*B*.

area illustrates the marriage boom, which is the sum of 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% decline in the number of marriages relative to the counterfactual, which is 3,066 of the missing marriages after the reform. Consequently, the boom can be decomposed into 23,855 retimed marriages (B) and 20,718 extra marriages (C). Figure A3 (figs. A1–A6, C7, E1, E2 are available online) illustrates the 5.6% steady-state reduction in the number of marriages in the sample of couples who faced no incentive to marry fast.[35] The last row of

[35] This estimate of the steady-state reduction in marriages is similar to the estimate obtained in sec. V.A above, a 6.2% reduction in the marriage rate. Note that a 5.6% 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% 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) % reduction in the marriage rate and an $(x(1/y))$ % reduction in the number of marriages.

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table A2 presents the estimated change in the probability of marriage at the eligibility threshold, $\Delta p_{s^*}/p_{s^*} = 21$. I interpret and discuss the magnitude of this estimate in section VI.E below.

C. Prediction MU3: Heterogeneous Effects and Economic Incentives

By prediction MU3, couples who were incentivized to marry should respond more strongly, the larger is their annuity's expected value. Figure 7 verifies that the raw data are consistent with this conjecture. First, because mortality increases with age, figure 7*A* 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 7*B* 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.

1. 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 sections VI.A and VI.B, I analyzed distributions of marriages; naturally, these analyses were restricted to couples who actually enter marriage. In this subsection, I start from the entire treated sample—that is, 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 as entering the risk pool for marriage 7 years (28 quarters) before the birth of its first child.[36] As I observe marital behavior starting in 1969, the sample therefore includes all couples whose marital behavior I can observe for at least 7 years before childbirth, that is, 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 by that time. The framework presented here exploits this fact by

[36] The distribution of first births itself is predetermined here by definition, as the treatment sample includes only couples whose first joint child was conceived at the time of reform.

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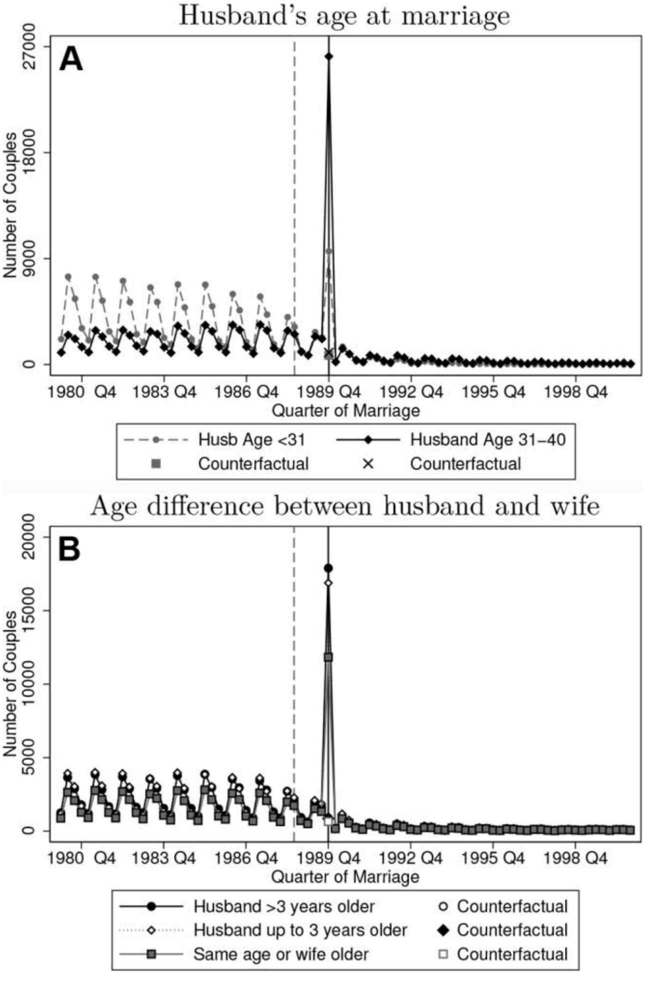

FIG. 7.—Heterogenous effects in the raw data. This figure replicates fig. 3*A* for subsets of the sample of couples who faced an incentive to marry before 1990 to secure survivors insurance (described in detail in the fig. 3 legend). *A*, Distribution of new marriages for two strict subsamples based on the husband's age at marriage: (1) younger than 31 or (2) between 31 and 40. *B*, Distribution of new marriages for three (other) strict subsamples, based on the couple's age structure, where (1) the husband is more than 3 years older than the wife, (2) the husband is strictly older than the wife but no more than 3 years older, and (3) the wife is older or the spouses are the same age. Both figures also provide the estimated counterfactual in the last quarter of 1989.

contrasting the marital behavior of couples who 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

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quarter c becomes under risk for marriage $7 \times 4 = 28$ quarters earlier, in quarter $c - 28$. For couple i, let $\mathbf{Z}_i(t)$ 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, $h(t; \mathbf{Z}_i(t)) = h_0(t)\exp(\beta\mathbf{Z}_i(t))$. The baseline hazard at time t, $h_0(t)$, is left unspecified. I estimate

$$h(t; \mathbf{Z}_i(t)) = h_0(t)\exp(\beta s_i^*(t) + \gamma \mathrm{post}_i(t) + \delta_1\mathbf{F}_i(t) + \delta_2\mathbf{D}_i(t)), \quad (3)$$

where $s_i^*(t)$ is an indicator variable that takes the value of one in the last quarter of 1989 and $\mathrm{post}_i(t)$ is an indicator variable that takes the value of one after the last quarter of 1989.[37] Importantly for the analysis of heterogeneity, $\boldsymbol{F}_i(t)$ 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). The vector $\mathbf{D}_i(t)$ captures other observable couple characteristics: the partners' educational attainment 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 SEK 175K and above in 1988 (12% of the sample); each birth year range is 4 years; and each age difference range is 2 years. I refer to the vector that includes flexible controls for male income and birth year as $\tilde{\boldsymbol{F}}_i(t)$.

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 1989Q4 relative to marriage in another quarter, given by the hazard ratio of marriage in 1989Q4, $\hat{h}_{1989Q4} = \exp(\hat{\beta})$. Intuitively, a hazard ratio of 10 means that a couple is 10 times more likely to marry in 1989Q4 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 with the delta method.

2. Results

In table 2, panel A, the top row presents results from estimation of equation (3). The estimated hazard ratio in the full sample is 14.14 when $\mathbf{F}_i(t)$,

[37] Couples with children born in different quarters experience the reform at different durations since (28 quarters before) childbirth.

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TABLE 2
IMPACT ON MARRIAGE: HAZARD RATIOS

	Financial Controls		All Observable Controls	
	Coefficient $\hat{\beta}$ (1)	Exponential $e^{\hat{\beta}}$ (2)	Coefficient $\hat{\beta}$ (3)	Exponential $e^{\hat{\beta}}$ (4)
	A. All			
Marriage in 1989Q4	2.65***	14.14***	2.71***	14.96***
	(.03)	(.48)	(.03)	(.52)
Marriage in 1989Q4 (flexible controls)	2.70***	14.81***	2.74***	15.45***
	(.04)	(.54)	(.04)	(.57)
No. of couples	960,414	960,414	764,332	764,332
	B. Male Dies within 5 Years			
Marriage in 1989Q4	2.84***	17.09***	2.97***	19.46***
	(.09)	(1.50)	(.09)	(1.73)
No. of couples	3,865	3,865	2,980	2,980
	C. Male Alive after 5 Years			
Marriage in 1989Q4	2.65***	14.14***	2.70***	14.95***
	(.03)	(.48)	(.03)	(.52)
No. of couples	956,031	956,031	760,966	760,966

NOTE.—The table reports hazard model estimates for three different samples. Columns 1 and 3 report the estimated coefficient on the indicator variable for marriage in 1989Q4, with standard errors clustered at couple cohort in parentheses. Columns 2 and 4 report the corresponding exponential. Significance levels are from a test of the null hypotheses that each regression coefficient is 0 or, equivalently, that each exponential is 1. In cols. 1 and 2, each regression includes controls for financial characteristics that influence the annuity's expected value. In cols. 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 the first row of panel A for two distinct subsamples: couples where the male dies within 5 years of January 1, 1990, and couples where the male remains alive 5 years after the reform implementation date, respectively.
*** $p < .01$.

the vector of financial characteristics that influence the annuity's expected value, is controlled for and 14.96 when other demographics are also controlled for. Thus, a couple who is unmarried at the end of 1989Q3 is, on average, 15 times more likely to marry in the next quarter than they would have been in the absence of reform.[38] The second row replicates these results including flexible controls for male income and birth year; the results remain unchanged.

[38] This estimate differs slightly from the one obtained in sec. VI.B, which suggested that the change in the probability of marriage at the eligibility threshold be given by $\Delta p_{s^*}/p_{s^*} = 21$. The sample in the current analysis includes couples who never choose to marry.

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3. Male Income and the Partners' Age Difference

I now examine how the hazard ratio of marriage in 1989Q4 varies with two different measures of the economic value of the annuity. First, I add interactions between $s_i^*(t)$ and each male labor income group l in $\tilde{\mathbf{F}}_i(t)$, and between $s_i^*(t)$ and each male birth year group b. I thus estimate the following regression model:

$$h(t;\mathbf{Z}_i(t)) = h_0(t)\exp\left(\sum_l \alpha_l s_i^*(t) + \sum_b \beta_b s_i^*(t) + \gamma \mathrm{post}_i(t) + \delta_1 \tilde{\mathbf{F}}_i(t) + \delta_2 \mathbf{D}_i(t)\right). \tag{4}$$

The estimated hazard ratio for marriage in 1989Q4 for a couple with male labor income l and birth year b is given by $e^{\alpha_l+\beta_b}$. Figure 8*A* 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 SEK 25K–50K in the year before the reform is 11 times more likely to marry in 1989Q4; the corresponding figure for men whose income instead is in the range SEK 125K–150K is 18. In this sample, SEK 150K is the 77th percentile of labor income. In next range, the hazard ratio decreases, which may reflect the fact that some males' income exceed the Social Security limit.[39] The hazard ratio is thus increasing in male income in the range where a higher husband labor income raises the annuity's value.[40]

Second, I replace the interactions between $s_i^*(t)$ and each male birth year group b in equation (4) by interactions between $s_i^*(t)$ and each age difference group a (controlling for the absolute age of the husband). Figure 8*B* plots the estimated hazard ratios for couples where the male earns income in the range SEK 50K–75K, for different ranges of the partners' age difference. The hazard ratio is increasing with the age difference. Couples where the male is 1–2 years older than the female are 11 times more likely to marry in 1989Q4; the corresponding figure for couples where the male is more than 9 years older is 13.

4. Male Mortality Risk

Even when holding constant all the observables included in $\mathbf{F}_i(t)$ that influence the value of the annuity, couples with private information suggesting

[39] This limit is calculated based on "pension rights income," which in addition to labor income includes some social insurance payments; see app. B for details. The last group includes couples who exceed this limit with certainty; this is indicated by the vertical dashed line in fig. 8*A*.

[40] This is consistent with Fadlon and Nielsen (2017), who find higher a valuation of survivors insurance by spouses with divergent levels of earned income.

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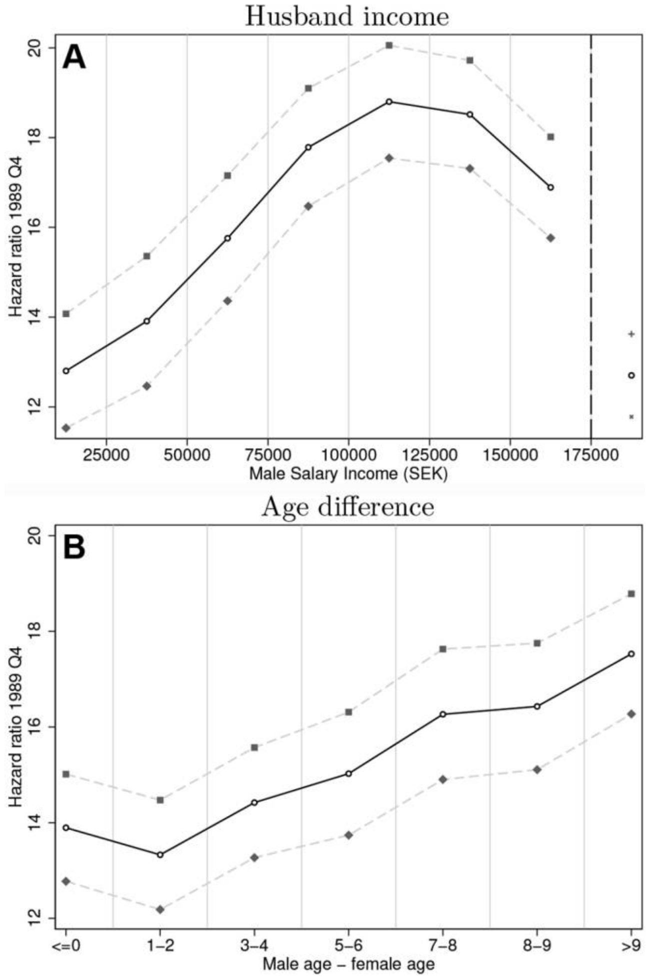

FIG. 8.—Hazard ratios for couples with different male income and age difference. The sample includes all couples whose first joint child was born from 1976 through 1988, regardless of eventual marital status. *A*, Estimated hazard ratios for marriage in 1989Q4 for couples with different male income ranges in the year before the reform. The specification includes two sets of interactions, between an indicator for marriage in 1989Q4 and indicator variables for each male income group, and between an indicator for marriage in 1989Q4 and indicator variables for each male birth year group. 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 *A* includes couples who exceed the Social Security limit (see app. B for details); this is indicated by the vertical 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. *B*, 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 1989Q4 and each age difference group instead of interactions with each male birth year group. Gray dashed lines represent 95% confidence intervals.

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that the male is likely to die sooner may respond more strongly to the reform. To examine whether 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 who died within 5 years of January 1, 1990. I then reestimate equation (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 5 years is 17.09 when $\mathbf{F}_i(t)$ is controlled for and 19.46 when other observables that a private annuity could potentially be priced on are also controlled for. The corresponding estimates in the sample of couples where the male remains alive after 5 years are 14.14 and 14.95, respectively. Thus, take-up of survivors insurance through marriage in the last quarter of 1989 is higher among couples for whom the remaining life span of the male is shorter.[41]

A positive correlation between demand for survivors insurance and risk type, controlling for prices, is consistent with adverse selection.[42] 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 $\mathbf{F}_i(t)$, which capture the financial value of survivors insurance for a given couple, is akin to controlling for individual prices in the context of private insurance. My results thus suggest that if insurance companies would observe (and hence be able to price on) all the characteristics $\mathbf{F}_i(t)$ that influence the value of the annuity—as well as all characteristics $\mathbf{D}_i(t)$ that do not directly affect the annuity's value—but no more information, then adverse selection would likely arise in such a private market. This may be one reason why private markets for annuities and life insurance were underdeveloped in Sweden at the time of reform.

[41] I also reestimate eq. (3), including an indicator variable for couples where the male dies within 5 years (capturing the fact that couples where a spouse is likely to die within 5 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 1988Q4 (capturing any extra responsiveness to the elimination of survivors insurance among these couples). When financial characteristics that influence the annuity's expected value, $\mathbf{F}_i(t)$, and all other characteristics that I observe, $\mathbf{D}_i(t)$, are controlled for, the implied hazard contribution from the interaction term is 1.26. This implies that a couple where the male dies within 5 years has a 26% higher hazard ratio than a couple where the male remains alive for at least 5 years; this difference is significant at the 5% level.

[42] 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 sec. VII allows me to explicitly examine causal impacts of survivors insurance. While sec. VII 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.

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D. *Prediction MU4: Long-Run Divorce Rate in Marriage-Boom Marriages*

By prediction MU4, rushed marriages should be more likely to end in divorce. Prediction 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 who marry in the last quarter of 1989 with that of those who marry into the same marriage contract, but earlier. I estimate the following regression using OLS (ordinary least squares):

$$\mathbf{1}[\text{Divorce}_x]_{imd} = \alpha + \beta\mathbf{1}[\text{marr} = s^*]_i + X_i'\theta + \eta_m + \zeta_d + \varepsilon_{imd}, \quad (5)$$

where the variable Divorce_x takes the value of one if couple i divorces within x years of marriage; the main explanatory variable of interest is a dummy taking the value of one if the couple married in the last quarter of 1989; η_m and ζ_d capture wedding month and day-of-week fixed effects, respectively; and X_i captures observable couple-specific characteristics: the spouses' ages, household income, and husband'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 β, which measures the difference in marital stability between marriage-boom marriages and 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

TABLE 3
Heightened Divorce Risk in Boom Marriages
(Dependent Variable: Divorce within *n* Years)

	3 years	5 years	10 years	15 years
Married in 1989Q4	.01***	.03***	.05***	.05***
	(.00)	(.00)	(.01)	(.01)
Mean, dependent variable	.03	.07	.17	.25
No. of couples	175,015	175,015	175,015	175,015

Note.—The sample includes all couples with a joint child born in 1988 or earlier who married from 1980 through 1989. The dependent variable is an indicator variable for the couple divorcing within 3, 5, 10, or 15 years. 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 the husband'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, are in parentheses.

*** $p < .01$.

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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.

E. On the Size of the Documented Responses

1. Magnitudes

Section VI 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% (and taking the sample expected duration until husband death of 42 years) yields an average expected value of the annuity at reform of approximately \$4,575, which was roughly one-third of mean annual posttax income. (See app. E.3 for calculations.)

We can interpret the ratio $\Delta p_{s^*}/p_{s^*}$ estimated in section VI.B as the numerator of an elasticity that quantifies how marital decisions respond to financial incentives. In particular, let $\epsilon = (\Delta p_{s^*}/p_{s^*})/(\Delta S_{s^*}/S_{s^*})$ relate the change in marriage probability at the threshold to the change in marital surplus stemming from the elimination of survivors insurance. To calculate this elasticity, we need to know not only the numerator ($\Delta p_{s^*}/p_{s^*} = 21.21$) and the size of the change in marital surplus at the notch ($\Delta S_{s^*} = -\$4,575$) but also the surplus from marriage relative to cohabitation, S_{s^*}. Even in the absence of an estimate of S_{s^*}, however, we can obtain a lower bound on this elasticity by assuming that elimination of survivors insurance eliminated the entire surplus. This lower bound is given by $21.21/-1 = -21.21$.[43] It is important to keep in mind that, while the elasticity ϵ captures responsiveness to financial incentives at the notch, section VI.B shows that 54% of the couples who married at

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

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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 VI.C to calculate an "ever-married" elasticity; see appendix E.4 for these calculations.

2. Interpretation

Section VI has documented responses along the margin of entry into marriage that are large, both relative to the existing evidence from other contexts[44] and relative to the long-run steady-state effects in Sweden discussed in section V.A. 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.[45] 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"—figure 3 suggests that the boom was driven by eligible couples. However, the media attention may have mattered by making it easy for all eligibles—even couples who otherwise would have been unaware of the financial gains from marriage—to figure out whether they would benefit from survivors insurance (marriage). Indeed, even couples where the husband had low IQ responded strongly to the reform.[46] Yet another

[44] Whittington and Alm (1997) exploit tax changes to examine how the marriage penalty affects the exit margin from marriage in the United States. They find that a tax change that erodes 71% of the marriage penalty raises the likelihood of divorce by 0.4 percentage points, or by 10%, 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% rise in the marriage penalty leads to a 2.3% reduction in the possibility of first marriage. See Alm, Dickert-Conlin, and Whittington (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 Sjoquist and Walker 1995 for the United States and Gelardi 1996 for Canada and the United Kingdom); the retiming responses documented here far supersede them.

[45] 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.

[46] There is, of course, substantial heterogeneity in the ability of couples to prepare for financial security in old age. Interestingly, replicating fig. 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 are 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).

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interpretation is that, even though there were substantial legal differences between cohabitation and marriage at the time of reform (as documented in sec. II), 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 underestimate their own chance of divorce (Mahar 2003).[47]

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 III 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.

VII. Survivors Insurance and Preexisting Marriage Contracts—Prediction MM1: Marital Instability

I now turn to the second group of couples in my study who 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.

A. *Sample and Descriptive Statistics*

To construct my baseline sample, I start from all individuals who entered marriage within 180 days of the eligibility threshold, January 1, 1985. I exclude all couples who had their first joint child within 4 years and 9 months of their marriage date, so that no couple had a joint child by the reform announcement (either in the baseline sample or 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.[48] Table 1 presents summary statistics for the baseline sample in column 4, as well as the analogous sample who entered marriage in a window 180 days before and after January 1, 1984, which I refer to as the placebo sample, in column 5. These groups are similar, but relative to the sample studied in section VI, 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.

[47] 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.

[48] 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 via child ID. See app. 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.

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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.[49] 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.

B. 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 VI 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 who married close to, but on opposite sides of, January 1, 1985, were treated differently in the reform implementation process.

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.[50] This raises the concern that couples on opposite sides of the threshold may be systematically different from each other. In an RD 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 that it exists—I use a difference-in-differences design that exploits the fact that couples who married around January 1 one year earlier were unaffected by the reform, and their distribution of marriages is similar.[51] 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 3-1/2 years after marriage.

[49] Section II.A shows that entry into parenthood was unaffected by the reform.

[50] Figure A4*a* plots the number of marriages in weekly bins against distance from the survivors insurance eligibility cutoff 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.

[51] Figure A4*b* shows their distribution of marriages.

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My estimation strategy follows that of Lalive (2008), who combines a difference-in-differences design with an RD design when faced with a discontinuity in the density of the assignment variable.[52] Intuitively, it captures the difference between two distinct RD estimates—one around January 1, 1985, and one around January 1, 1984. A discontinuity around January 1, 1985, reflects (1) the fact that only couples who married before this threshold get to keep survivors insurance and (2) a potential effect of "crossing" New Year's Eve. A discontinuity around January 1, 1984, reflects item 2 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 $Y_{it} = \tau_t \times I_{it} + \sigma_t \times \mathrm{NYE}_i + g(\mathrm{dom}) + U_{it}$ represent the causal relationship between whether couple i divorces within t years, Y_{it}, and survivors insurance status at time t, $I_{it} = I_{it}(\mathrm{dom})$, where dom is the couple's date of marriage and U_{it} is a random vector of predetermined and unobservable characteristics. Thus, $\mathrm{NYE}_i = \mathrm{NYE}_i(\mathrm{dom})$ 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 $t > \mathrm{June}_{1988}$, the required identifying assumptions are as follows. (1) The impacts of I_{it} and NYE_i are additively separable. (2) Conditional on NYE_i, the direct marginal impact of dom on Y_{it} is continuous. (3) Further, the interpretation of the (RD) 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. (4) 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.[53]

The first-stage and reduced-form equations are given by

$$\begin{aligned} I_{it} = {} & \alpha + \gamma 1\left[\widetilde{\mathrm{dom}}_b > 0\right]\mathbf{1}[\mathrm{Around85}] + \delta\mathbf{1}\left[\widetilde{\mathrm{dom}}_b > 0\right] \\ & + g\left(\widetilde{\mathrm{dom}}_b\right) + h\left(\widetilde{\mathrm{dom}}_b\right)\mathbf{1}[\mathrm{Around85}] + \varepsilon_{it} \end{aligned} \tag{6}$$

[52] Lalive (2008) refers to this estimate as the BD-RDD, i.e., the before-during-RD design.

[53] Some 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 (assumption 4) but 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 4 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 sec. II.A rules out such a response.

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and

$$\begin{aligned} Y_{it} = \alpha + \beta \mathbf{1}\left[\widetilde{\mathrm{dom}}_b > 0\right]\mathbf{1}[\mathrm{Around85}] + \eta \mathbf{1}\left[\widetilde{\mathrm{dom}}_b > 0\right] \\ + i\left(\widetilde{\mathrm{dom}}_b\right) + j\left(\widetilde{\mathrm{dom}}_b\right)\mathbf{1}[\mathrm{Around85}] + \nu_{it}, \end{aligned} \tag{7}$$

where i indexes couples, t indexes year after marriage, and $\widetilde{\mathrm{dom}}_b$ is the distance from New Year's Eve in 1985 or 1984. 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.[54] The RD difference-in-differences estimate is given by the ratio $\hat{\beta}/\hat{\gamma}$. 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.[55]

C. *Results*

To gain intuition for the results as well as the empirical strategy, figure 9*A* displays the empirical cumulative distributions of durations until divorce, obtained by estimating Kaplan-Meier failure functions without covariates, for couples marrying in the last 3 months of 1984 and the first 3 months of 1985. This graphical evidence suggests that the removal of survivors insurance caused divorces. During the first 3 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 who married in 1985, the failure functions begin to diverge. Figure 9*B* plots the same functions for couples who married within 3 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 figure 9*A* as the causal effect of (losing) survivors insurance.

Table 4 presents two-stage least squares (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.[56] The estimates suggest that removing survivors insurance

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

[55] Figure A5 plots these characteristics in weekly bins against distance from the eligibility cutoff.

[56] Table A3 presents OLS estimates of the first stage (eq. [6]); it is close to one, which reflects the fact that treatment is near-universal at the right side of the threshold. Among couples who married around 1985 and 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).

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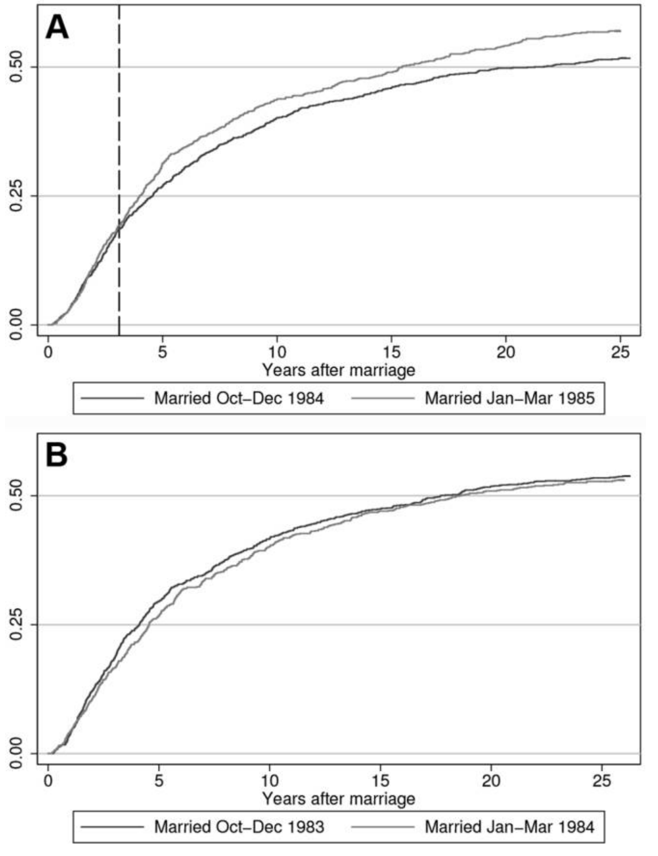

FIG. 9.—Empirical CDF (cumulative distribution function) of marriage duration around actual and placebo thresholds: *A*, married around 1985; *B*, married around 1984. To define the sample in *A*, I start from all individuals who entered marriage in a window of 180 days around the eligibility threshold, January 1, 1985. Because the reform affected only the subset of already-married couples who were childless, I exclude all couples who had a joint child within the first 4 years and 9 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 or older at the date of marriage. This sample captures individuals who were eligible for survivors insurance when the reform was announced but 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 *B* is defined analogously for individuals who 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 who married in the last 3 months before each threshold (black line) and the first 3 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 who married after January 1, 1985, depicted by the vertical line in *A*. All couples in *B* were allowed to keep the old marriage contract that they married into.

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TABLE 4
Impact on Divorce in Preexisting Marriages

	Bandwidth: 150 Days		Bandwidth: 180 Days	
Divorce within	Second-Order Polynomial (1)	Third-Order Polynomial (2)	Second-Order Polynomial (3)	Third-Order Polynomial (4)
3 years	.0313	.0243	.0173	.0238
	(.0236)	(.0257)	(.0204)	(.0226)
Mean, dependent variable	.17	.17	.16	.16
AIC	7,542.00	7,544.51	10,569.11	10,572.27
24 years	.0535*	.0455	.0429	.0518*
	(.0313)	(.0340)	(.0276)	(.0306)
Mean, dependent variable	.50	.50	.49	.49
AIC	12,671.80	12,675.36	18,739.71	18,736.43
No. of couples	9,117	9,117	13,421	13,421

Note.—The sample includes all women in the baseline and placebo samples, which are described in detail in the notes for table 1, cols. 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 the stub column. 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 are in parentheses.
* $p < .1$.

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, which represents a 10% increase.[57] Thus, the removal of survivors insurance pushed couples on the margin into divorce.

VIII. Survivors Insurance and Matching—Prediction UU1: Assortativeness of Matching

Because the survivors insurance tied to the old marriage contract was worth more for couples with highly unequal earnings (capacities), the old marriage contract subsidized "one-sided" unassortative matching, that is, matches between high-earning men and low-earning women.[58] Removing

[57] The high divorce rate reflects the fact that married couples who 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.

[58] In 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 who 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.)

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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 who marry "down," that is, marry a woman of low skill (predetermined earnings capacity).

A. *Sample and Descriptive Statistics*

To take this prediction to the data, I begin by comparing the matching patterns of couples who 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 who attended college and individuals who 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 who married from 1983 through 1999, excluding the 6% of the observations for which I have no information about educational attainment at marriage.[59] Table A4 displays summary statistics for this sample.

B. *Empirical Methodology*

I collapse the data into quarterly bins. I define the distance between a couple's quarter of marriage, $\mathrm{Vig}q_s$, and the final quarter in which marriage entails take-up of the old marriage contract by $\widetilde{\mathrm{Vig}q_s} = (\mathrm{Vig}q_s - 1989\mathrm{Q}4)$ and estimate the following regression:

$$\begin{aligned} r_s = \alpha &+ \beta\mathbf{1}\left[\widetilde{\mathrm{Vig}q_s} > 0\right] + \gamma\mathbf{1}\left[\widetilde{\mathrm{Vig}q_s} > 0\right]\left(\widetilde{\mathrm{Vig}q_s}\right) \\ &+ \delta\mathbf{1}[s = s^*] + g(\widetilde{\mathrm{Vig}q_s}) + \zeta_q + \varepsilon_c, \end{aligned} \tag{8}$$

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

$$r_s = \frac{N(\tau_h^{\mathrm{HIGH}}, \tau_w^{\mathrm{LOW}})}{N(\tau_h^{\mathrm{HIGH}}, \tau_w^{\mathrm{LOW}}) + N(\tau_h^{\mathrm{HIGH}}, \tau_w^{\mathrm{HIGH}})}, \tag{9}$$

where the function $N(\cdot)$ counts the number of marriages of the match type indicated by the arguments. The main coefficient of interest is β, which

[59] The sample is limited to couples who ever had a joint child (before or after marriage) because of the difficulty of matching married individuals into couples in the absence of joint children. I exclude couples who 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 observe educational attainment starting only in 1985, I use educational attainment in 1985 for those who marry in 1983 and 1984.

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captures a discontinuous change after the threshold s^*. Further, γ captures any change in the slope at s^*, and $g(\widetilde{\mathrm{Vig}q_s})$ is a polynomial in $\widetilde{\mathrm{Vig}q_s}$.

C. *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 these 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 1983–2000. Specifically, I plot residuals from a regression on quarter fixed effects and a dummy for the last quarter of 1989, represented by open circles. The solid 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 lines show the 95% confidence intervals. At the eligibility threshold, the figure shows a discontinuous change in the share of men marrying down.

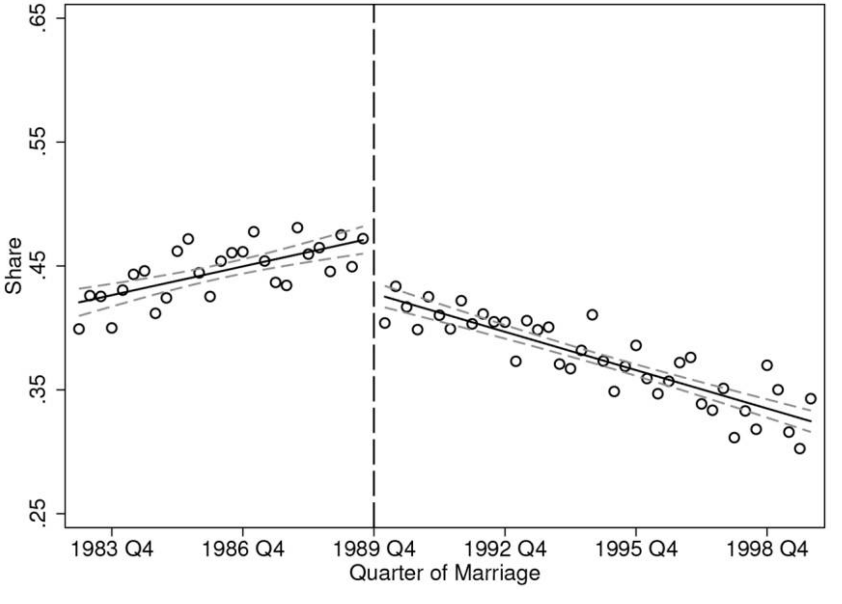

FIG. 10.—Assortativeness of matching. The sample includes all couples who had a joint child and married between 1983 and 2000 (a time period during which the definition of educational attainment at marriage is constant around the 1989 threshold), but omitting the reform implementation year, 1989. The open circles depict the share of highly skilled men marrying a woman of low skill (seasonality adjusted), at a quarterly level. The 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 cutoff. Dashed lines represent 95% confidence intervals.

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TABLE 5
MATCHING: IMPACT ON THE SHARE OF HIGHLY SKILLED MEN MARRYING LOW-SKILLED WOMEN

	POLYNOMIAL ORDER		
	1	2	3
New marriage contract	−.0447***	−.0364**	−.0492*
	(.0087)	(.0135)	(.0194)
Mean, dependent variable	.41	.41	.41
Adjusted R^2	.87	.88	.87
AIC	−353.65	−353.06	−350.20
Observations	68	68	68

NOTE.—Dependent variable is the share of highly skilled men who marry a low-skilled woman. The table reports the coefficient on the key explanatory variable in eq. (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 are in parentheses. The AIC favors the linear model.
* $p < .1$.
** $p < .05$.
*** $p < .01$.

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 equation (8), using as $g(\widetilde{\text{Vig}q_s})$ polynomials in $\widetilde{\text{Vig}q_s}$ of three different orders. The linear model, which is favored by the AIC, suggests that the share of highly skilled men who enter unassortatively matched marriages decreases by 4 percentage points (11%) after the introduction of the new marriage contract.

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 at 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 figure A6. While the trends in assortative and unassortative marriages are similar before the reform, they diverge after the 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 who had a joint child before the reform's announcement (and hence had an incentive to respond to the reform by marrying before 1990) constituted only a small share of all unassortative matches considering marriage.[60]

[60] 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 app. E7 for a discussion of the potential role of changing skill distributions of men and women in the longer run.

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IX. 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.

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