Divorce law reform, family stability, and children's long-term outcomes

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Abstract

While divorce laws are known to influence family behavior, empirical evidence of their effects on children remains scarce. I shed more light on this by evaluating the Swedish divorce law reform of 1974, which **i)** liberalized the existing divorce laws and **ii)** implemented a 6-month parental reconsideration period for divorce. I exploit quasi-experimental policy variation and population-wide register data of 1.17 million Swedish children born 1952–1964 to evaluate the implications of family (in)stability on children's long-term human capital outcomes. The evaluation suggests that exposure to more liberal divorce laws decreases the likelihood of graduating from upper secondary school by 5.6%. Evaluating the reconsideration period, I find that families with greater exposure to this reform element are 18.8% less likely to divorce. The exposed children are also 1.8% more likely to graduate from upper secondary school and have more stable marriage market outcomes as adults. The findings highlight a trade-off between parental freedom of choice related to divorce and externalities on children's outcomes.

Keywords: Divorce law reform; Children's outcomes; Family behavior

JEL: J12; J13; D13; I24

Contents

1. Introduction

Marriage is an institution which allows for individuals to share risk and partake in joint investments over the life cycle, specialize in different activities, and commit to a loving relationship. From the 1960s and onward, the marked increase in divorces and concurrent divorce law reforms in the Western world have affected the stability aspects of marriage (Stevenson & Wolfers, 2007). In response, sociologists have termed this period the "divorce revolution" (Weitzman, 1985).[1] While subsequent policy reforms have generally liberalized the divorce process in many countries, it remains common to retain a reconsideration period before allowing a couple to divorce. This precautionary measure aims to protect spouses and children from impetuous marital dissolution.

In this study, I improve our understanding of this societal change by studying the effects of divorce law reform on children's long-term outcomes. Specifically, I delve into the Swedish divorce law reform of 1974, which provides a rare possibility to investigate how different institutional factors governing marriage stability affect families and contribute to shaping children's life trajectories. The reform involved replacing the previous stipulation, which required divorcing spouses to wait for 1–3 years, with unrestricted no-fault divorce. Moreover, the divorce liberalization was paired with the introduction of a 6-month reconsideration period for spouses with a child under the age of 16 living in the household. The design of the new law allows me to empirically distinguish between the two dimensions of the reform, and while the main focus is to evaluate the effects of the divorce restriction, I also present evidence of the effects of the liberalization element of the reform.[2]

Marriage status is intricately linked to parental time investments (Le Forner, 2020), financial resources (Kearney, 2023), and social stigma (Gerstel, 1987). Consequently, marriage instability during crucial periods of human capital formation could significantly impact children's development (Heckman, 2000; Cunha et al., 2006; Heckman, 2011). Relating to this, a growing body of research presents evidence that divorce laws play an important role in shaping family outcomes (Gruber,

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[1]50% of U.S. children experience parental divorce during childhood, and 27% of U.S. children reside with a single parent as of 2018 (Lansford, 2009; OECD, 2018). 30% of the children born in Sweden in the year 2000 experienced separation by age 18 (Statistics Sweden, 2018). Lately, some countries and U.S. states have even started to reverse the liberalization of their divorce laws and re-imposed or increased divorce restrictions. Some examples include Louisiana (2007), South Carolina (2013), Washington (2013), Denmark (2019), China (2020), and South Korea (2008).

[2]I focus my attention on the divorce restriction element due to this part of the reform having a clearer policy relevance and better control group, i.e. for external and internal validity reasons.

2004; Heggeness, 2020; Corradini & Buccione, 2023). Estimating the effects of divorce laws on children's outcomes is, however, difficult. Not at least because it requires data tracking children over extended periods of time, but also because of challenges in accounting for correlated unobservables. The Swedish divorce law reform of 1974 constitutes an ideal setting since the existing data, combined with a suitable identification strategy, allow for such evaluation and analysis of related mechanisms.

According to marriage market theory (e.g. Chiappori et al., 2002), a parental reconsideration period for divorce affects spouses through **i)** decreased marital instability, with fewer couples divorcing than if the restriction had not been in place, **ii)** changes to intra-household bargaining between spouses due to less credible exit from the union, and **iii)** an increase in relation-specific investments due to lower divorce risk and stronger marital commitment. This means that a divorce law reform affects both divorces and intra-household behavior for spouses remaining married. The inability to separate between these two channels implies that empirical estimates from these type of reforms should be seen as capturing the combined reduced-form effects related to divorce and changes to family behavior (Gruber, 2004).

The empirical analysis draws on rich administrative data, allowing me to track 1.17 million Swedish children born 1952–1964 over six decades. With these data, I link the universe of Swedish families from 1932–2014, observe all civil status changes from 1969, and add information on the children's outcomes later in life. I also supplement the data with information from the Swedish military conscription tests, providing information on a range of cognitive and non-cognitive abilities for almost the full population of Swedish men at around age 18. Given that the divorce law reform could directly impact marriage and fertility behavior crucial for identifying causal effects, I focus on mature families with children aged 10–22 at the time of the reform.[3]

The evaluation of both elements of the reform is based on a differences-in-differences approach, where the birth cohort and family situation of the child determines differential exposure to the reform elements. In order to capture the effects of the divorce liberalization element, I compare the outcomes of children with married parents to children with unmarried parents. The evaluation of the divorce restriction element instead narrows the focus to children of married parents and uses the mandatory 6-month reconsideration period when a child under the age of 16 is living in the

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[3]However, I show treatment effects split by birth cohort to give an indication of how the reform affects younger cohorts with more years of exposure during childhood than the ones included in my analysis.

household. Since the age of the youngest child determines whether the family is subject to the reconsideration period after the reform, sibling age spacing to the youngest child in the family is used for identification. In my preferred empirical specification to evaluate the reconsideration period, I categorize children into two comparison groups based on sibling age spacing. Specifically, those with an age spacing of three years or more to their youngest sibling are compared against children with less than three years of age spacing. The evaluation of both reform elements also leverages older "placebo" cohorts, with the same family background, who are adults when the reform takes effect. These cohorts are used to account for any pre-policy differences based on family situation.

I start by decomposing the immediate divorce responses to the liberalization element of the reform, which reveals that the main respondents were older couples and those without children. I also document that the 6-month reconsideration period creates a sharp negative discontinuity, at the threshold of the the youngest child in the family turning age 16, in the probability to divorce. Moving on to the causal analysis, the evaluation of the divorce liberalization indicates that exposure to the liberalized legislation reduces the affected children's upper secondary school graduation likelihood by 5.6%, relative to children with unmarried parents and placebo cohorts graduating before the reform was implemented. I also find negative effects on the children's long-term university graduation propensity, employment probability, and cognitive ability outcomes at age 18.

Evaluating the divorce restriction element, I show that the 6-month reconsideration period decreases parental marriage instability notably. Greater exposure to the reconsideration period after age 16, defined in my preferred empirical specification as the five-year average difference in sibling age spacing between the two comparison groups, reduces the risk of experiencing parental divorce during childhood by 18.8%. This effect becomes more pronounced with each additional year of exposure. I then show that the divorce restriction has significant and positive effects on children's long-term outcomes. Greater exposure to the reconsideration period significantly increases the likelihood of graduating from upper secondary school, the main outcome of interest, by 1.8%. Statistically significant effects are also found for the children's labor market outcomes later in life and for their family outcomes as adults.

The treatment effects of the divorce restriction on children's educational attainment are significantly stronger for sons, those with parents who have at most upper secondary schooling, and

children whose mothers have weaker attachment to the labor market. Moreover, the treatment effects are small and not statistically significant for the children with a low pre-determined risk of experiencing parental divorce. Weighing up the results, the divorce law reform is found to affect children's long-term outcomes by sizable magnitudes.

The rich data allow me to shed some light on the mechanisms at play by studying several measures that have been proposed in the literature: **i)** mothers with greater exposure to the reconsideration period reduce their hours worked, while fathers' labor supply remains unchanged. This suggests changes to within-household behavior. **ii)** Concurrently, the intergenerational correlation in educational attainment between mothers and their children significantly strengthens post-reform, providing suggestive evidence of greater transmission of human capital and parental investments. **iii)** Findings from military conscription tests show that the reconsideration period positively impacts (male) cognitive and non-cognitive abilities, indicating that the effects run deep in affecting children's development. **iv)** Lastly, the affected children defer their fertility decisions away from teen parenthood, which is indicative of a more stable family environment during childhood.

While part of the effects of divorce laws on children's outcomes likely work through parental divorce and separation, the suggestive mechanisms, the magnitude of the estimates, and a mediation analysis collectively indicate that the policy effects mainly run through families with intact marriages. These findings provide evidence that divorce laws can affect children through both direct and indirect channels, highlighting the importance of additional research to better understand the effects of family policy on children's outcomes.

Relating to the previous literature, existing studies on the effects of divorce and divorce law reform on children tend to find null or negative effects linked to divorce or more liberal divorce laws (e.g. Piketty, 2003; Gruber, 2004; Björklund & Sundström, 2006; Amato, 2010; Cáceres-Delpiano & Giolito, 2012; Frimmel et al., 2016; González & Viitanen, 2018; Chen et al., 2019; Gould et al., 2020).[4] An exception to the prevailing null and negative effects in the literature is found in two studies, set in developing countries, that use divorce liberalization, divorce legalization, and court congestion for divorce cases as identifying variation for the effects of divorce laws. Both studies

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[4]Although, it should be noted that these studies all vary in the type of outcomes studied and the potential for their respective research design to deal with correlated unobservables. Examples include using **i)** observables as controls, **ii)** fixed effects relying on sibling difference in age at the time of divorce or cohort exposure to divorce law changes, and **iii)** instrumenting for divorce using husbands' exposure to women at the workplace (Frimmel et al., 2016).

find positive effects on children related to either exposure to more liberal divorce laws or less court congestion (Heggeness, 2020; Corradini & Buccione, 2023). In contrast to the latter two papers, this study evaluates a reform targeting marginal divorces with the aim of reconciling divorcing spouses or reducing their level of conflict. The conflicting evidence, differing marginal respondents, and the respective challenges to each identification strategy further underscore the need for more empirical studies related to divorce policies and children's outcomes.[5]

Understanding the the mechanisms linking divorce laws to within-household bargaining and family behavior is challenging due to a lack of detailed information on household dynamics, as noted by Gruber (2004) and others. Various studies have attempted to fill this gap by investigating how changes to divorce laws impact mechanisms related to within-household bargaining, including domestic violence, spousal labor supply, and savings behavior (e.g., Stevenson & Wolfers, 2006; Stevenson, 2007; González & Özcan, 2013; Fernández & Wong, 2014; Voena, 2015).[6] Notably, this body of work suggests that reforms targeting negatively selected families may exacerbate violence against women. In contrast, the Swedish reconsideration period for divorce specifically addresses marginal divorces, posing less risk of increasing domestic violence or abuse. Additionally, the Swedish divorce law does not mandate cohabitation during the waiting period, which further reduces the risk of affecting separation decisions in marriages characterized by substantial discord.

Recent evidence has underscored the pivotal role of parental investments in shaping children's human capital and laying the foundation for future outcomes (e.g. Le Forner, 2020; Gould et al., 2020; Ringdal & Sjursen, 2021). Moreover, changes to family policy have been demonstrated to prompt substantial responses to within-household behavior (Persson, 2020). Other studies have investigated mechanisms related to parental divorce and children's mental health and showed that the negative psychological effects associated with divorce can be substantial, even when such shocks occur during adolescence (Chen et al., 2019; Kravdal & Grundy, 2019). There is also clear evidence that the timing of parental divorce just before important events (e.g. final exams in high school) substantially hurts children's educational outcomes (Gould et al., 2020). This indicates that divorce

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[5]See Appendix B for a theoretical framework based on the Swedish divorce law reform. The empirical predictions based on this framework imply that the introduction of a divorce restriction should lead to fewer divorces being observed. Also, I expect to observe changes to the behavior and within-household bargaining of parents remaining married, to the benefit of the affected children. These implications will be tested later on in the empirical analysis.

[6]This underscores the potential trade-off between divorce laws and parental well-being, with examples of more liberal divorce laws showing both decreases and increases in domestic violence (Brassiolo, 2016; García-Ramos, 2021).

laws targeting children in adolescence may have profound impacts on educational outcomes.

Taking stock of the existing literature, previous studies indicate that a divorce law reform may bring about different treatment effects depending on the context, equilibrium effects, and the type of family affected. This study seeks to bring cohesion to these results by evaluating a comprehensive divorce law reform and investigate mechanisms related to family behavior.

The nature of the 1974 reform in Sweden, coupled with access to rich administrative data, allow me to advance the literature in several ways. First, previous research has shown that formal "cool-down" (reconsideration) periods for divorce can prevent marginal divorces (Lee, 2013; Fallesen, 2021), but to the best of my knowledge no study has used a similar identification strategy to show its effects on children's long-term outcomes. Second, the data allow me to study the effects of the reform on a wider set of outcomes than the previous literature, within the same sample, spanning from childhood until adulthood. This comprehensive approach provides a more holistic understanding of the reform's implications. Third, the setting and the data provide a rare opportunity to separate between different mechanisms to learn more about how family behavior affects children's outcomes. This enables a more nuanced exploration of the underlying dynamics at play.

My findings indicate that divorce laws wield significant influence on family behavior by shaping the institutional environment for marital stability. Notably, the causal effects of divorce law reform on children's long-term outcomes are substantial and of mixed sign depending on the nature of the reform. However, it is also important to note that the restrictiveness of divorce laws can bring about a fundamental trade-off between spousal freedom of choice and potential externalities. Learning from the divorce law reform of Sweden in 1974 could shed important light on the effects of such reforms and demographic transition, particularly for countries with similar institutional setting and demographic trajectory. The findings also highlight the importance for policy makers to consider externalities when designing public policies related to marriage stability, specifically to consider the long-term effects these policies may have on children.

The following structure outlines the paper: Section 2 presents a background to the institutional context, the educational system in Sweden, and the Swedish divorce law reform of 1974. Section 3 outlines the data sources and empirical strategy. Section 4 presents estimation results, and Section 5 discusses the findings. Finally, Section 6 concludes.

2. Background

This sections outlines relevant details of the Swedish divorce law reform of 1974 and the institutional context around the time of the reform.

2.1 Institutional context

The 1960s and 1970s witnessed a substantial surge in divorces across the Western world, in many cases coinciding with divorce law liberalizations. In 1974, Sweden implemented one of the most comprehensive overhauls, significantly simplifying the existing divorce process. This involved the elimination of all fault-based reasons for divorce, establishing unrestricted divorce as the new norm. At the time, the new law was considered the most progressive divorce legislation in the Western world (Jänterä-Jareborg, 2014).[7]

Divorce laws in Sweden 1915–1973. Before the reform, during 1915–1973, divorce in Sweden could be granted based on three principles: divorce by mutual consent (82% of all cases), unilateral divorce (4%), and fault-based reasons (adultery 12%, other 2%). Under mutual consent, the couple were required to undergo mandatory counselling with the stated aim of attempting to salvage the marriage. Should the counsellor find the marriage beyond salvaging, the couple was permitted to initiate a year-long separation period, after which they could proceed to finalize the divorce.[8]

Under unilateral divorce decisions before 1974, the divorcing spouse originally had to prove the breakdown of the marriage through "long and irreconcilable marital differences". Typically, this involved providing evidence that the couple had been separated for at least three years. After this, the divorce could be granted by the courts without any reconsideration period.[9]

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[7]Examples of divorce liberalization reforms include the United Kingdom in 1969, Denmark in 1970, the U.S. in 1970–, Australia in 1975. Similar reforms have since then taken place around the world, but many countries still share similarities with the institutional setting of Sweden before 1974. For instance, some U.S. states, the United Kingdom, Germany, and Canada still retain fault-based reasons as a primary way to divorce and restrict unilateral divorces.

[8]The restrictive divorce laws at the time were motivated with the belief that families constituted the fundamental building blocks of society, and marriages were deemed intrinsically important for society at large (SOU 1972:41, 1972).

[9]Additionally, there were a number of fault-based reasons that could be used as grounds for divorce without any reconsideration period. For more detailed information on the divorce laws in Sweden 1915–1973, see Appendix B.

The divorce law reform of 1974. The divorce law reform of 1974, implemented on January 1, constituted a complete overhaul of the existing divorce legislation by removing all fault-based reasons and making unrestricted divorce the new norm. This shift implies that divorcing spouses no longer need to wait before finalizing their divorce, nor to disclose any reason to the courts for instigating divorce. The motivating reason behind the new policy was changing views on family life and its value to society.[10] Especially women's growing economic freedom and lessened reliance on their husbands was a key reason behind the new policy.

Despite the emphasis on individual freedom of choice, the policy makers decided to implement a 6-month reconsideration period for divorce under unilateral divorce, and when a child under age 16 is living in the household. The restriction was meant to act as protection for children and spouses against impetuous divorces and the adverse effects these may have (Inger, 2011). This divorce restriction still remains in place as of 2024. There is no conclusive evidence that the reconsideration period impacts divorces more than through postponements, but data from the Swedish Courts reveals that during 2000–2010, 11% of joint applications for divorce and 21% of unilateral divorce decisions were retracted before being finalized (Swedish Courts, 2014).[11]

The institutional context around the time of the reform. The 1970s marked a transformative period in Sweden, introducing new family policies such as the abolition of joint marital taxation in 1971, the establishment of the parental leave system in 1974, and a revision of abortion laws during the same year. Universal healthcare and the education system had long since been free of charge, and women were entering the labor market in never-before-seen numbers.[12] In terms

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[10]The dominant view at the time was that marriage is to be a private and voluntary commitment. Any stabilizing effect of considerable divorce restrictions on marriages was not deemed to outweigh the costs (SOU 1972:41, 1972).

[11]Using the research strategy later outlined in this paper, I show that parents with greater exposure to the divorce restriction were more likely to be married 16 years after the reform was enacted (see Table A1). This is in line with previous research indicating that divorce laws can affect divorce behavior (González & Viitanen, 2009; Lee, 2013). However, these findings contradict work from the U.S. indicating the neutrality of divorce laws on divorce rates (Wolfers, 2006). I also show that parents with greater exposure to the reconsideration period are more likely to be alive in the long run, indicating that there are no inherent trade-offs between spousal freedom to divorce and mortality.

[12]In the empirical evaluation of the divorce law reform, I include cohort effects and parent cohort effects in all estimations to ensure that any cross-cohort effects of other policies are accounted for. I also rule out that the main coinciding reforms do not drive the results by **i)** showing that the joint marital taxation reform is unlikely to affect things since controlling for parental earnings in 1970, 1971, and 1973 does not affect the estimates, **ii)** using the family structure from 1970 in the estimations before the abortion laws were revised, **iii)** verifying that the contraceptive pill in the 1960s did not differentially affect the age spacing of siblings in any economically significant way.

of marriage patterns, society differed from today when the majority of children are born out of wedlock. Marriage was the pre-dominant form of civil status at the time of the reform in 1974, with 88% of cohabiting couples being married. Cohabitation without marriage was rare around this time, particularly among couples with children.

The progressive new divorce laws in 1974 were were complemented by equally progressive existing laws governing post-divorce arrangements. The Swedish legal framework is built on the principle that spouses bear a financial obligation to support each other during marriage, but economic ties are to be severed after a divorce. Typically, marital assets are divided equally between spouses during the divorce process. Alimony for the financially weaker party is uncommon, except during transitional periods or when one parent assumes the majority of custody. Children alternating their residence after a divorce is relatively common today, but it was not so in 1974.[13]

The education system in Sweden underwent significant changes around the time of the divorce law reform. Following a comprehensive restructuring in 1962–1963, the Swedish schooling system is comprised of nine years of compulsory education. After completing compulsory schooling, students in Sweden are, if they choose to do so, able to enrol in upper secondary education. The share of children enrolling in and graduating from upper secondary school had been increasing over time starting in the 1960s.[14] This upward trend, and other contemporaneous policy reforms, imply that any policy evaluation for these cohorts needs to rely on within-cohort variation to account for the striking increase over time.

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[13]The median child support per month for one child at the time of the reform was 1,500 SEK (roughly $170), and for two children 2,400 SEK ($280) in 2020 value (SOU 1975:24, 1975). Reportedly, only 10% of divorces in 1974 led to a court-mandated alimony for the financially weaker spouse. In 1975, the mother received full custody in 84% of cases with a custody dispute (SOU 1975:24, 1975). In the mid 1980s, only 1% of children had alternating residence following parental separation (Statistics Sweden, 2019).

[14]In 1971, the different upper secondary school systems in Sweden were replaced by a unitary system with vocational and academic tracks. The vocational tracks tended to be two years long and consisted mainly of vocational training, not granting the student access to university studies. The academic tracks typically lasted three years and typically led to university eligibility. The adult education system in Sweden allows for those who lack any upper secondary education and those who dropped out before graduating to finalize a degree. It is also possible to supplement a vocational degree to obtain a three-year degree within the adult education system. (Hall, 2012). In 1960, 10% of the cohort graduated from upper secondary school. By 1980, 85% completed upper secondary school, and by the end of the 1980s almost 90 percent of the children continued directly to upper secondary school.

3. Data & empirical method

3.1 Data

The primary data source used in the project consists of full-population data from Statistics Sweden based on Swedish taxation registers (RTB - Registret över totalbefolkningen) and other linked registers. The data include information on civil status, family linkages, educational attainment, and labor market outcomes. With these data, I am able to construct a panel of the full universe of parents and their children from 1932–2014, including information on all civil status changes from 1969, parent-child linkages through the Multi-Generation Register for those born 1932 and later, place of residence, earnings, and educational attainment for select years from the censuses (FoB - Folk- och bostadsräkningen), and other demographic information. This panel allows me to track civil status changes over time, and enables for a detailed analysis of family marital stability.

Siblings are linked together using the mother's ID from the Multi-Generation Register. Households are created by assigning children to their joint birth mother.[15] I restrict attention to outcomes for the years 1970–2000, since this time span is consistent with the data material and align with the education information in the censuses in 1970 and 1990. For some additional outcomes related to family formation, I add information up to the year 2014.

I supplement the existing data by including information from the Swedish War Archive (Krigsarkivet) on eight dimensions of non-cognitive ability and cognitive ability. Roughly 90% of the Swedish men in the cohorts I study performed these mandatory tests around age 18.[16] These abilities have previously been shown to strongly correlate with outcomes later in life, such as labor market outcomes (Lindqvist & Vestman, 2011), and are determined around the age when the divorce law reform is expected to affect the children.

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[15]This assignment of children to households implicitly assumes that in the case of separated birth parents the children live with the mother, an assumption which is consistent with most child custody arrangements at the time.

[16]The conscript's cognitive ability measures consist of four subtests of logical, verbal, and spatial abilities, as well as a test of technical comprehension. The measures of non-cognitive abilities are based on a standardized psychological evaluation aimed at determining the conscripts' capacity to fulfill the requirements of military service. The non-cognitive ability evaluation consists of a battery of survey questions and a 20–30-minute interview with an armed-forces psychologist. The interview allows the psychologist to grade the different answers on a range of topics related to leadership and coping under pressure. The interviewer gives a high score if the conscript is considered to be socially mature, persistent, willing to assume responsibility, able to take initiative, and emotionally stable (Black et al., 2018). The non-cognitive abilities are graded by the psychologist in four subscores measured on a 1 to 5 scale. I standardize all measures of ability to be mean-zero, standard deviation one by cohort.

The Swedish divorce law reform of 1974 had broad implications for all married families, potentially influencing future fertility decisions. Since this may directly affect the outcomes of existing children, I narrow my scope to families with older children (ages 10–22 in 1974) who were most likely unaffected by these considerations. However, it should be acknowledge that the reform might have distinct effects on younger children than I consider in my evaluation. I attempt to look into this in the analysis by showing the evolution of the treatment effects disaggregated by birth cohort. The primary sample for analyzing the reconsideration period comprises 1,168,874 children born between 1952 and 1964 with married parents. Additionally, for the analysis of the liberalization element, I include 9,805 children born in the same cohorts with unmarried parents.

3.2 Identification strategy

The evaluations of the two reform elements are based on the same differences-in-differences (DiD) approach, where the interaction of exposure to the specific reform element and cohort group captures the coefficients of interest. While the two evaluations and their respective identification strategies are highly similar, they differ in terms of exposure definition (age spacing or marriage status). I start by outlining the empirical strategy for evaluating the divorce restriction element, since this is the main evaluation of the paper. Lastly, I outline the strategy for evaluating the divorce liberalization.

The divorce restriction element. In order to evaluate the effects of the divorce restriction element on children's outcomes, I focus on children of married parents and exploit the fact that the age of the youngest child determines exposure to the divorce restrictions for the entire family. Based on this, I can compare children of the same birth cohort with varying age spacing to the youngest child in their respective family. Marriage status and age spacing are defined in year 1970, well before the new divorce law became known to the general public.[17] This strategy allows me to net out cohort-specific shocks, such as the divorce liberalization and the rapid schooling expansion for these cohorts, that risk confounding the effects of the divorce restriction element.

The children in the study are split into two groups based on their age spacing. The first group

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[17]Setting marriage status and age spacing in 1970 gives me stable comparison groups but does not take any new siblings or changes to marital status into account. This reduces the risk of capturing selection induced by the divorce law reform, but may also attenuate the effects. I test these concerns in Table A4 by assigning marriage status and age spacing status in 1973 and show that the results remain qualitatively unchanged.

comprises those with small to no age spacing, which are the youngest children themselves and those with a sibling 0–2 years younger than they are. The second group includes children with greater age spacing, specifically those with a younger sibling 3–8 years younger. The first group is deemed to have weak to no extra insulation against parental divorce after age 16, while the second group has greater insulation by virtue of their age spacing. The cutoff is set at three years of spacing since this corresponds to the 3-year duration of Swedish academic-track upper secondary school up to age 18.

This split means that the treatment effects can be interpreted as capturing additional years of exposure for a child in a given family. The average difference in sibling age spacing between the two comparison groups is 4.8 years, which means that the treated families have close to five additional years of exposure to the reconsideration period's direct and indirect effects.[18]

Given previous evidence of the effects of divorce shocks during childhood (e.g. Chen et al., 2019; Gould et al., 2020) and the nature of the Swedish reform, I expect the reconsideration period to affect children's outcomes positively. This is expected due to a larger age spacing insulating against shocks related to parental marital instability during adolescence and indirectly increasing parental investments. In addition, the reconsideration period could change the level of conflict associated with the divorce, even if it does not directly influence the final decision to divorce.

A key concern with comparing children of differing age spacing is that this spacing is potentially endogenous and may directly affect children's outcomes.[19] To address these concerns, I include older "placebo" cohorts of children born 1952–1955 with the same age spacing as the treated children. The idea is that the policy's impact is expected to be minor for the placebo cohorts, given that they were old enough for most to have completed upper secondary school when the policy took effect. Hence, the cohorts born 1952–1955 are unexposed to the reform during childhood. This approach allows for the identification of any main effects of age spacing on children's outcomes for these cohorts and provides a basis for accounting for such effects in the analysis.[20]

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[18]This age spacing cutoff means that all children in the reference group are exposed to unrestricted parental divorce for at least one more year. Bounding the age spacing at 0–8 years means that 90% of children in each cohort are included. I verify that this is not a concern by testing a specification including all children with more than 8 years of age spacing, and by excluding the children where the birth of a sibling 1971–1973 changes their age spacing status to more than 8 years (3.5% of the sample). This gives unchanged effect sizes on educational outcomes (see Table A4).

[19]Previous research has indicated, through correlational evidence and when instrumenting with miscarriages, that extra age spacing between siblings may affect children positively, and that close age spacing of siblings is negatively associated with parental investments (Belmont et al., 1978; Buckles & Munnich, 2012).

[20]The children born in 1955 are a borderline case, since some of them will be affected during their final 6 months in

From this, I define the treatment as the interaction between having large age spacing to the youngest sibling in the family and being born 1956–1964, i.e. as being a child aged 10–18 in 1974 with their youngest sibling 3–8 years younger than they are. As reference group, I use children of the same cohorts with 0–2 years of age spacing. The results of this difference is compared to the same definition for cohorts born 1952–1955, which are the first cohorts where I can follow parental marriage status year-by-year during childhood in the DiD-specification outlined below. The effects of exposure to the reconsideration period is estimated through the following regression equation:

$$y_i = \beta_0 + \beta_1 \underbrace{Insulation_i \times \mathbb{1}[Cohort_i \geq 1956]}_{Treat \times Post} + \beta_2 \underbrace{Insulation_i}_{Treat} + \underbrace{\theta_j}_{Post} + p_i + \boldsymbol{X_i'\delta} + \varepsilon_i$$

The indicator $Insulation_i$ takes the value one for individual i if the age spacing to the youngest sibling is between 3–8 years, and zero for the reference category with age spacing 0–2 years. This indicator corresponds to treatment assignment in the DiD terminology. $\mathbb{1}[Cohort_i \geq 1956]$ is an indicator function taking the value one for cohorts born 1956–1964 (ages 10–18 in 1974), and zero for cohorts born 1952–1955 (ages 19–22 in 1974). Cohort indicators θ_j capture cohort fixed effects, and correspond to controlling for differences between the pre and post groups. The p_i fixed effects flexibly capturing the mother and father's cohorts, which are known in advance to be imbalanced across age spacing groups. $\boldsymbol{X_i}$ is a vector of controls, which captures pre-determined characteristics of the child and the parents.[21] Missing values of control variables are included as separate indicators. Along with parental cohort effects, these controls are included to ensure that the children are as comparable as possible, and to potentially improve the precision of the estimates.

The identifying assumption for the estimation strategy to hold is that direct effects of age spacing on children are constant across cohort groups 1952–1955 and 1956–1964.[22] This assumption corresponds to the parallel trends assumption in DiD terminology (Angrist & Pischke, 2008). In the

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upper secondary school. They could also have been affected by anticipation effects in 1973 given the media search results and falling separation rates observed already at this point in time (see Panels B1b & B1c). The cohorts born 1956–1957 are only affected by the direct effects of the policy for 1–2 years, while the 1958–1964 cohorts are exposed for three years or more. I collapse these cohort groups into one in the estimation of the effects. Including the partially exposed cohorts attenuates the results slightly, but provides a better picture of the effects of the reform at large.

[21]Combining parental cohort effects and child cohort effects means that I effectively control for the age at birth for both parents. The controls include parental labor market outcomes in 1970, educational attainment, municipality of residence, the child's birth month, and sex.

[22]Alternatively, that differential parental investments w.r.t. age spacing does not change over time due to other factors unrelated to the divorce law reform, or that any such changes did not affect children's outcomes.

robustness section, I show that varying the cutoff around 3 years of age spacing slightly changes the magnitude of the estimates, but not the sign and significance of the results. Since the specification has fixed treatment and control groups, and the outcome is measured at a single point in time for each individual, this method does not need the modern adjustments designed for staggered DiD specifications (Callaway & Sant'Anna, 2021).

This specification allows β_1 to capture the average treatment effect of the reconsideration period for cohorts born 1956–1964 against those born 1952–1955, effectively netting out any pre-existing effects of age spacing on children's outcomes, and cohort effects. The pre-existing effect of age spacing is instead captured by β_2, which estimates the effect for the older cohorts born 1952–1955. By construction, roughly half of all children are the youngest in their family, which means that the reference group is heavily tilted toward the youngest children or those with no siblings. The advantage of this empirical approach is that it allows me to include cohort effects in the estimation and rely exclusively on within-cohort variation.

The divorce liberalization element. The evaluation of the divorce liberalization element of the reform consists of comparing the children of married parents to the children of the same cohorts with unmarried parents. The sample of children with unmarried parents amount to 9,805, while the children with married parents number 1,168,874. The children with unmarried parents are thus likely to be different and are very few compared to the sample of children with married parents, but serve as a valid counterfactual under the assumption of no composition changes for married and unmarried parents between the cohort groups. The relatively few unmarried parents will, however, make the comparison more unstable than a similar evaluation taking place today when the majority of children are born to unmarried parents. However, this is the best possible counterfactual available to evaluate a reform which affected the entire population of married families.[23]

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[23]Unmarried parents are defined as both parents having never been married before in 1970. The number of children with unmarried parents in 1970 are few and increasing by each cohort (roughly 1,300 children born 1952–1955, and 8,500 born 1956–1964). A concern is that defining marriage status in 1970 at different ages for the children mechanically makes the comparison groups more similar for the younger cohorts. Under the caveats of conditioning on an outcome, I run specifications where I condition on the parents remaining unmarried by 1975 and 1980 separately and show that the main result remains qualitatively unchanged (results available upon request). The effects also risk being attenuated by unmarried parents marrying after 1970 and becoming directly affected by the policy. It should be noted that the cohorts with the greatest risk of contamination from marriage are also the ones where the children are exposed during the most years, which is where the greatest effects of the policy are to be expected. One year after the reform, 89% of the unmarried parents had remained so, while 10% had married and 1% had divorced since 1970.

To evaluate the divorce liberalization, I run the same regression as with the divorce restriction, except for replacing the $Insulation_i$ indicator with the indicator $Married_i$, which takes the value one for children with married parents, and zero for the reference category consisting of children with unmarried parents:

$$y_i = \phi_0 + \phi_1 \underbrace{Married_i \times \mathbb{1}[Cohort_i \geq 1956]}_{Treat \times Post} + \phi_2 \underbrace{Married_i}_{Treat} + \underbrace{\theta_j}_{Post} + p_i + \boldsymbol{X_i'}\boldsymbol{\gamma} + \varepsilon_i$$

Similarly to the previous specification related to the divorce restriction, ϕ_1 captures the average difference in outcome between children of married and unmarried parents for the cohort groups born 1956–1964 and 1952–1955, effectively netting out any pre-existing effects of marriage status between the cohort groups. The pre-existing differences between children of married and unmarried parents are instead captured by ϕ_2, which estimates the difference for the older cohorts born 1952–1955.

3.3   Descriptive statistics

Descriptive statistics for the main sample for the first year of the panel in 1970 are presented in Table 1. The table is meant to visualize the identification strategy and serve as an initial balancing test. The average characteristics of the children and their families are presented in columns (1), (2), (4), and (5) corresponding to their cohort group and reform element exposure group. The additional columns (3), (6), and (7) display the differences in average characteristics, and the final column presents the p-value of the double difference (DiD) related to the identification strategy.[24]

The table shows that most characteristics vary substantially for children of the same cohorts with different age spacing. Reassuringly, the differences in characteristics are similar for the older placebo cohort group compared to the younger cohorts. In general, the double difference between cohort groups greatly reduces the magnitude and economical significance of the observed differences, but they still remain statistically significant for some characteristics. For the comparison of families with married and unmarried parents, the strategy serves to reduce the differences between the marriage status groups. However, the differences always remain statistically significant.

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[24]Due to the age spacing of children affecting many observables for the parents in 1970 directly (e.g. labor market outcomes), grandparental characteristics in 1970 are used as the predominant descriptive statistics.

Table 1: Descriptive statistics 1970, by cohort, age spacing, and marriage status group.

Panel A	Cohort 1956–1964			Cohort 1952–1955			Cohort 1952–1964	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Spacing group, diff. (·)–(·), [p-val.]	Sp. 3–8	0–2	d. (1)–(2)	3–8	0–2	(4)–(5)	(3)–(6)	p-val.
Age 1970 - avg., (diff.), [p-val.]	9.791	9.681	(.111)	16.479	16.498	(−.019)	(.130)	[.000]
Age spacing*	5.100	.287	(4.813)	5.237	.407	(4.830)	(−.017)	[.007]
Share female	.484	.488	(−.003)	.486	.487	(−.001)	(−.002)	[.251]
Share foreign born	.069	.057	(.013)	.064	.050	(.014)	(−.001)	[.273]
Age mother 1970	34.729	39.256	(−4.527)	42.588	46.995	(−4.407)	(−.120)	[.000]
Age father 1970	37.463	42.122	(−4.659)	45.064	49.528	(−4.464)	(−.196)	[.000]
Mother's education in years	8.831	8.641	(.190)	8.404	8.206	(.198)	(−.008)	[.466]
Father's education in years	9.435	9.246	(.189)	9.105	8.877	(.227)	(−.039)	[.005]
Upper sec. education mother	.310	.286	(.024)	.243	.216	(.027)	(−.003)	[.181]
Upper sec. education father	.435	.409	(.026)	.376	.346	(.030)	(−.005)	[.058]
Earnings mat. grandfather 1970	212.386	204.353	(8.033)	164.144	160.427	(3.717)	(4.316)	[.114]
Hours worked grandfather	23.103	22.361	(.742)	17.374	16.817	(.557)	(.185)	[.615]
Upper sec. educ. grandfather	.056	.051	(.005)	.014	.012	(.003)	(.002)	[.305]
Share married grandfather	.856	.850	(.006)	.818	.819	(−.001)	(.007)	[.330]
Earnings mat. grandmother 1970	69.756	69.582	(.174)	61.633	61.735	(−.102)	(.276)	[.822]
Hours worked grandmother	9.947	9.664	(.284)	7.329	7.027	(.302)	(−.018)	[.938]
Upper sec. educ. grandmother	.042	.037	(.005)	.013	.016	(−.003)	(.008)	[.000]
Share married grandmother	.730	.717	(.014)	.677	.670	(.007)	(.007)	[.397]
Obs.	366,648	487,252	853,900	125,307	189,667	314,974	1,168,874	
Panel B	Cohort 1956–1964			Cohort 1952–1955			Cohort 1952–1964	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Marr. status, diff. (·)–(·), [p-val.]	Married	Unmarr.	d. (1)–(2)	Married	Unmarr.	(4)–(5)	(3)–(6)	p-val.
Age mother 1970	37.533	31.53	(6.003)	44.803	43.149	(1.654)	(4.349)	[.000]
Age father 1970	40.939	35.481	(5.458)	48.226	47.417	(.808)	(4.650)	[.000]
US educ. mother	.291	.206	(.085)	.216	.081	(.136)	(−.051)	[.000]
US. educ. father	.417	.205	(.212)	.350	.108	(.242)	(−.030)	[.007]
Obs.	853,225	8,506	861,731	354,451	1,299	355,750	1,217,481	

Note: Panel A presents descriptive statistics for the main sample used to evaluate the divorce restriction, and Panel B for the sample used to evaluate the divorce liberalization. Column (3) and (6) displays the difference in characteristics across column pairs. Column (7) displays the double difference between the column pairs. Grandparental characteristics are shown for maternal grandparents. p-values in column (8) for the double differences are calculated with standard errors clustered at the household level. *Age spacing is measured in 1973 to ensure that the birth cohorts 1963–1964 also have the same potential range of age spacing values (0–8).

4. Results

This section presents the results from the empirical analysis. After outlining the direct impact of the reform on family behavior (divorces and marriages), I estimate the effects of the two reform elements on children's long-term outcomes. I then probe the robustness of the findings. The section concludes by providing additional analyses of the underlying mechanisms.

4.1 The direct impact of the reform on family behavior

The divorce law reform directly affected the entire population of married couples and is widely believed to have created the massive spike of divorces observed in 1974. A crude analysis of this spike in Figure 1 shows that the primary respondents were older couples and couples without children, with no apparent immediate heterogeneity by earnings and education. All subgroups in terms of age, child status, earnings, and education show an increased number of divorces following the reform, indicating that there was pent up demand for the reform in all echelons of society.[25]

Delving deeper into the divorce restriction element of the reform, I investigate whether the new law hinders divorce for spouses when their youngest child is below age 16. Following the implementation, the reconsideration period should affect these parents disproportionately starting in 1974. This is shown in Figure 2, where parental divorce incidence is regressed on age of the youngest child in the family for the years 1973 and 1974 separately. The figure shows a clear discontinuity in the parental divorce rates at ages below 16 starting in year 1974, indicating that the policy had large effects on short-run divorce incidence (-30%).

At the same time, the general equilibrium effects of the new divorce law became evident as the number of marriages in Sweden reversed its declining trend and increased by 17% in 1974 compared to the previous year. While the majority of these new marriages involved previously unmarried individuals, marriages between spouses previously married to other people increased by 60% over the following three years relative to the year before the divorce law reform (see Figure 3).[26]

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[25]After a few years, the heterogeneity pattern indicates that divorce is more prevalent relative to before the reform among spouses with some higher education and those earning below the median.

[26]This is consistent with evidence that reductions in waiting time for divorce increases remarriages (Wong, 2018).
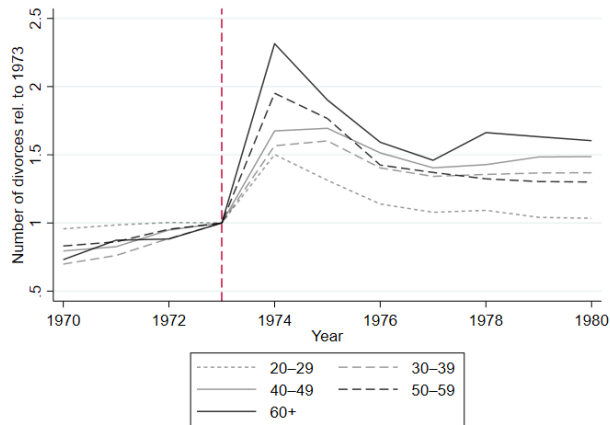
(a) Divorces by age group relative to 1973.
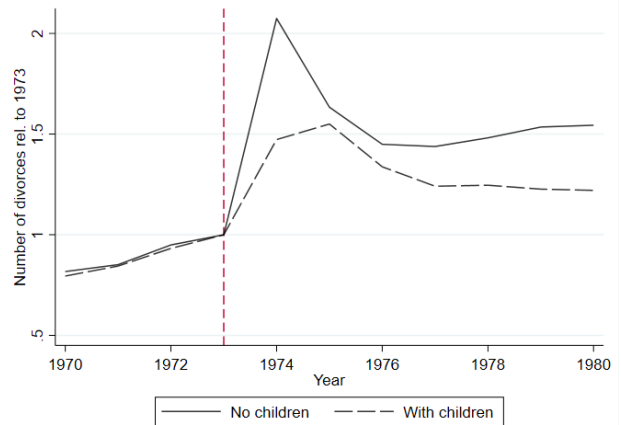
(b) Divorces by child status relative to 1973.

(c) Divorces above and below median earnings in 1970, men and women separately rel. to 1973.
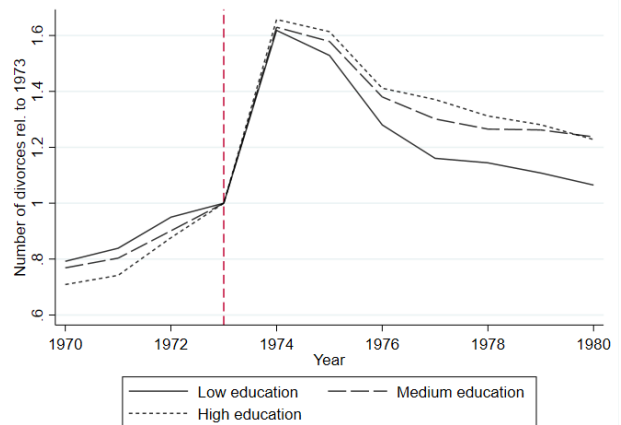
(d) Divorces by educational attainment 1970 relative to 1973.

Figure 1: Reform responses by pre-characteristics: Age, child status, earnings, and education. The red line marks the last year before the new divorce policy. All changes are relative to 1973 before the new policy. Child status is defined as having a child age 0–18. "Low education" is defined as primary school education, "Medium education" is defined as upper secondary school education, and "High education" includes university education.
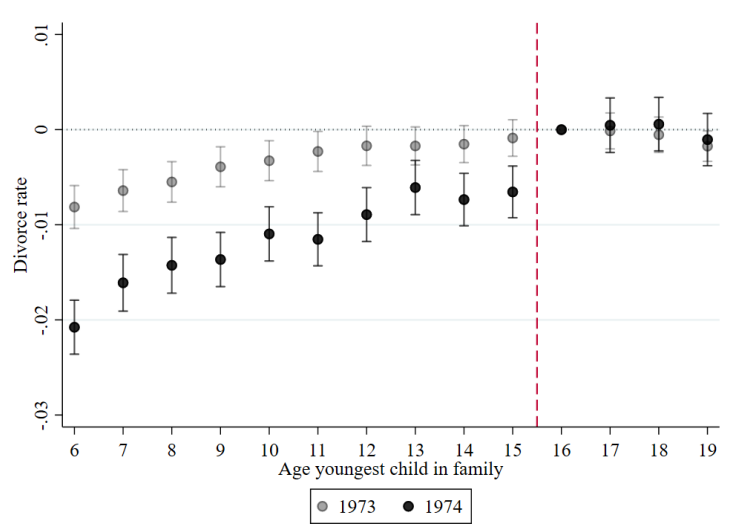
Figure 2: Parental divorce incidence 1973 and 1974 separately, by age of the youngest child.
The reference age 16 is indicated by the dashed red line for both years. Average baseline divorce risk is 1.3% in 1973 and 2.1% in 1974. Estimations include parental age fixed effects and an indicator for sex. CI95 are indicated in black, and standard errors are clustered at the household level.

(a) Number of divorces and marriages in Sweden 1968–1988.
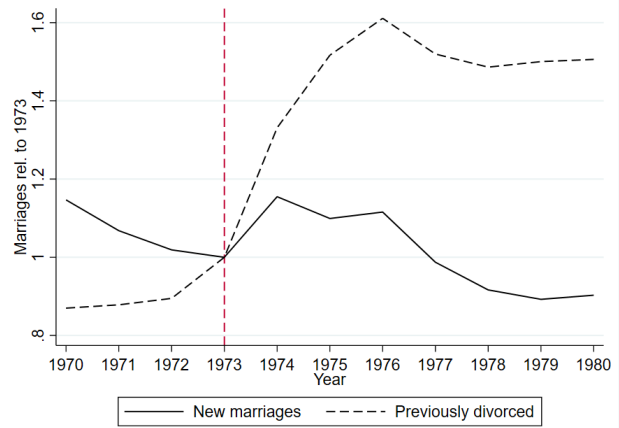
(b) Number of new marriages and marriages between divorced individuals in 1970–1980, rel. to 1973.

Figure 3: Divorces and marriages in Sweden around the time of the divorce law reform.
Panel 3a shows number of divorces and marriages over time in levels. Panel 3b shows marriages relative to 1973, split by previous civil status. "New marriages" are defined as both spouses being unmarried before entering the union. "Previously divorced" are defined as one spouse having previously been married before entering the new union. The red line marks the last year before the new divorce policy.

4.2 Evaluating the divorce liberalization

Moving on to the causal analysis, I first proceed by investigating the effects of the divorce liberalization reform element. As noted, the liberalization element affected the entire population of married families, making the causal effects difficult to disentangle. However, a relatively small number of children (9,805) had parents that were unmarried in 1970, which means that these children can be used as a plausible counterfactual for the direct effects of the reform.

Effects on educational outcomes, labor market outcomes, and ability outcomes. I presents results on educational outcomes, labor market outcomes in year 1990, and ability outcomes outcomes for the affected children in Table 2. The table shows that the difference in upper secondary school completion rate between children with married and unmarried parents decreased significantly by 4.6 pp. (-0.046, s.e. 0.016) following the reform, which amounts to -5.6% relative to the mean outcome for the reference group. The partial convergence in schooling outcomes corresponds to closing the educational gap between children of married and unmarried parents by more than a third. The effect on university graduation rate is smaller in absolute magnitude (-0.031, s.e. 0.006), but translates into a relative effect of -27%.

The estimates for labor market outcomes in year 1990 and the conscription measures of ability are somewhat noisy and do not translate into any significant effects on log earnings (-0.029, s.e. 0.024) or standardized non-cognitive ability (-0.053 SD, s.e. 0.044), even though the point estimates are negative. The effects on employment (-0.041, s.e. 0.011) and standardized cognitive ability (-0.183 SD, s.e. 0.043), on the other hand, are negative and statistically significant.

In Figure 4, I split the effect on the upper secondary school completion rate by birth cohort. The figure shows a large, stable gap in upper secondary school completion rate for the older cohorts, indicating that children with married parents had much better schooling outcomes around this time. This difference visibly shrinks for the cohorts affected by the divorce law reform.

A concern with this evaluation is that composition changes of married and unmarried parents over cohort groups are behind the observed effects. However, Figure 5 shows that the predicted upper secondary school completion rate based on background characteristics predicts a relatively

Table 2: Effect of divorce liberalization on educational, labor market, and conscription ability outcomes.

Outcome:	Upper sec. completion	University graduation	Log earnings	Employed	Cognitive ability	Non-cog. ability
$Married_i \times Cohort_i \geq 1956$	−0.046***	−0.031***	−0.029	−0.041***	−0.183***	−0.053
	(0.016)	(0.006)	(0.024)	(0.011)	(0.043)	(0.044)
$Married_i$	0.119***	0.042***	0.114***	0.082***	0.413***	0.329***
	(0.015)	(0.006)	(0.022)	(0.011)	(0.041)	(0.041)
Mean dep. var.	0.823	0.114	7.052	0.901		
Obs.	1,124,917	1,124,917	1,151,277	1,185,863	540,054	540,038
Cohort FE	✓	✓	✓	✓	✓	✓
Parent cohort FE	✓	✓	✓	✓	✓	✓

Note: ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$. Standard errors in parenthesis are clustered at the household level. "Upper sec. completion." is defined as upper secondary education of two years or more, or any higher education in 1990. "University graduation" is defined as at least three years of university education in 1990. "Log earnings" and "Employed" are defined as the natural logarithm of earnings and employment status in 1990. "Cognitive ability" and "Non-cog. ability" denotes standardized cognitive and non-cognitive ability measures from the conscription tests. "$Married_i$" indicates the children of married parents, where the reference category is children of unmarried parents. Marriage status is defined in 1970 as both parents being married, and the same definition follows for the unmarried parents. The interaction with cohort shows the difference in effect between cohort groups 1956–1964 and 1952–1955. Besides cohort and parent cohort effects, the controls include municipality of residence in 1970, labor market outcomes and education of parents in 1970, sex of the child, birth month, and indicators of missing values. "Mean dep. var." refers to mean dependent variable for the category with married parents born 1956–1964.
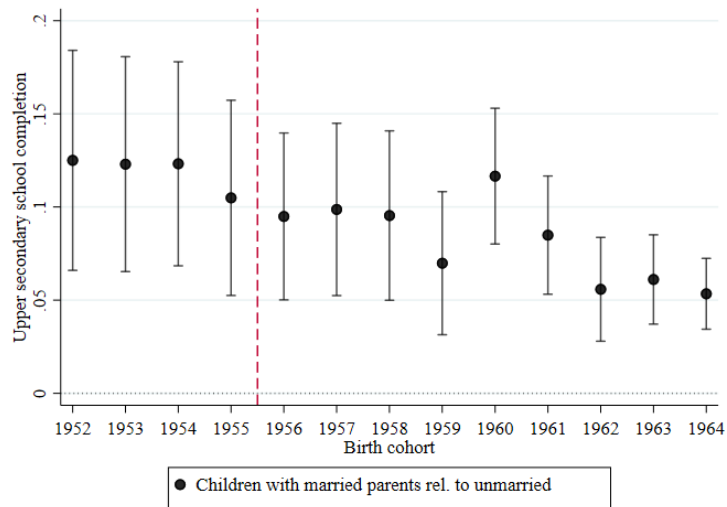
Figure 4: Effects of the divorce liberalization on upper secondary school completion.
The figure shows upper secondary school completion for children of married parents relative to children of unmarried parents. Parental marriage status is determined in 1970. The controls include parents' birth cohort, municipality of residence in 1970, labor market outcomes and education of parents in 1970, sex of the child, birth month, and indicators of missing values. CI95 are indicated in black, and standard errors are clustered at the household level.

stable positive difference between children with married and unmarried parents over time.[27] In other words, composition changes are not contributing to the partial convergence in schooling outcomes.

A further concern, which is harder to address, is the possibility that changing views on marriage drive the observed effects, or some other policy besides the divorce law reform which caused a partial convergence in educational attainment between the groups. A prime policy candidate is the abolition of joint marital taxation in 1971, which incentivized married women's labor supply (Selin, 2014), in turn potentially affecting children's outcomes. However, this reform is unlikely to be a concern since including controls for mothers' labor market outcomes in 1970–1973 leaves the main estimate on upper secondary school completion unchanged.[28]

A concern more difficult to rule out is a gradually changing culture with greater acceptance of cohabitation without marriage. This could affect the estimates if children with unmarried parents' were directly affected by a change in social stigma. I acknowledge this caveat and proceed to evaluate the divorce restriction element of the reform.

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[27]The characteristics include educational attainment of parents in 1970, parents' birth cohort, parents' labor market outcomes, sex of the child, birth month, and municipality of residence. The R^2 is 0.060. An F-test of the difference over cohort groups also shows that the difference is not statistically significant (p-value 0.538).

[28]Results are available upon request.

Figure 5: Balancing test for the divorce liberalization element.
Difference in predicted upper secondary school completion rate for children of married parents relative to children of unmarried parents. Characteristics included for the predicted outcome are parent cohort, municipality of residence, educational attainment, and labor market outcomes in 1970, sex of the child, and birth month of the child. Parental marriage status is determined in 1970. The R^2 of the prediction is 0.060. CI95 are indicated in black, and standard errors are clustered at the household level.

4.3 Evaluating the divorce restriction

The evaluation of the divorce restriction element (the reconsideration period) focuses exclusively on children with married parents. The identification strategy instead exploits variation in age spacing of children to the youngest sibling in their family and uses this to capture differential exposure to the reform element.

Effects on measures of parental marital instability. I validate that the reconsideration period for divorce affected the propensity of experiencing different measures of parental marital instability. The measures I consider are divorce by age 18, divorce in 15 years from 1970, and the fathers' multi-partner fertility.[29] Experiencing parental divorce within 15 years of 1970 follows all cohorts for 15 years and captures the long-term effects on marital instability. Multi-partner fertility is used as an indirect measure of marital instability, and is defined as an indicator if the father has

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[29]Experiencing parental divorce during childhood is defined as the biological parents divorcing at any observable year until the child is age 18. Since divorces are observed from 1970 and onward, parental divorce can only be measured for a short period of time for the oldest cohorts. This means that every new cohort's parental divorce outcomes by age 18 are observed an additional year. This matters for the interpretation but not the validity of the results, since outcomes are compared within each cohort.

a child with a new woman after 1974.

The results of the OLS regressions on measures of marital instability can be seen in Table 3. The reconsideration period significantly decreased the risk of parental divorce during childhood by 1.9 pp. (-0.019, s.e. 0.001) for the cohorts affected by the reform during childhood. The estimate is even greater in magnitude for elder siblings (age spacing 1–8 years) born the same years (-0.033, s.e. 0.002). The reduction in parental divorce incidence translates into a decrease of roughly 19–24%, relative to the control mean of the reference group born 1956–1964.[30] The main effect on experiencing parental divorce by age 18 for the placebo cohorts born 1952–1955 is statistically significant but small in magnitude when including all children (-0.002, s.e. 0.000), and is positive for elder siblings (0.003, s.e. 0.001). The point estimates for the placebo cohorts indicate that age spacing did not affect pre-reform parental marriage instability to any great extent.

The other measures of parental marital instability also provide evidence of less marital instability following the reform. The estimates for experiencing parental divorce in 15 years from 1970 are significantly negative for the exposed cohorts (-0.035, s.e. 0.002) and when focusing on elder siblings (-0.030, s.e. 0.003). Similarly, the estimates of fathers' multi-partner fertility for all children (-0.007, s.e. 0.001), and for elder siblings (-0.007, s.e. 0.001) are weaker in magnitude, but are statistically significant and similar in relative terms.

Figure 6 presents graphical results of the OLS regression split by birth cohort, where Panel 6a includes all children and Panel 6b focuses exclusively on elder siblings. The figures highlight that the effect is small in magnitude for the placebo cohorts, with no clear pre-trend. In addition, the figure shows that the increasingly negative effect on parental divorce incidence by birth cohort was phased in gradually for the cohorts affected by the reconsideration period during childhood. In other words, the effect appears to increase with each added year of exposure during childhood. The added effect by cohort could suggest that the reform goes beyond mere postponements and may have influenced the behavior of parents.

All in all, the reconsideration period appears to have had substantial effects on parental marital instability. While not capturing the full extent of the policy's effects on families (e.g. within-household bargaining, reducing the family conflict level, and parental investments), these

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[30]This effect is larger in magnitude than the 10% reduction found by Lee (2013) for South Korea's 30-day "cool-down period", which may be explained by the Swedish reconsideration period being considerably longer.

Table 3: Effect of a 6-month parental reconsideration period for divorce on measures of marital instability.

Sample: Outcome:	All children			Elder siblings		
	Divorce by 18	Divorce in 15 years	Father multip. fertility	Divorce by 18	Divorce in 15 years	Father multip. fertility
$Insulation_i \times Cohort_i \geq 1956$	−0.019***	−0.035***	−0.007***	−0.033***	−0.030***	−0.007***
	(0.001)	(0.002)	(0.001)	(0.002)	(0.003)	(0.001)
$Insulation_i$	−0.002***	0.018***	0.001***	0.003***	0.003	0.001**
	(0.000)	(0.001)	(0.000)	(0.001)	(0.002)	(0.001)
Mean dep. var.	0.101	0.167	0.024	0.139	0.231	0.041
Obs.	1,140,727	1,168,874	1,148,691	589,914	601,711	589,708
Cohort FE	✓	✓	✓	✓	✓	✓
Parent cohort FE	✓	✓	✓	✓	✓	✓

Note: ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$. Standard errors in parenthesis are clustered at the household level. "Elder siblings" excludes youngest siblings (spacing 0). "Divorce by 18" is an indicator for experiencing parental divorce by age 18. "Divorce in 15 years" changes the indicator to experiencing parental divorce in 15 years from 1970. "Father multip. fertility" is an indicator capturing multi-partner fertility of the father from 1975 (having a half-sibling born to a different mother after 1974). "$Insulation_i$" indicates greater age spacing (3–8 years to youngest sibling against 0–2 years). The interaction with cohort shows the difference in effect between cohort groups 1956–1964 and 1952–1955. Besides cohort and parent cohort effects, the controls include municipality of residence in 1970, labor market outcomes and education of parents in 1970, sex of the child, birth month, and indicators of missing values. "Mean dep. var." refers to mean dependent variable for the reference category with age spacing 0–2.
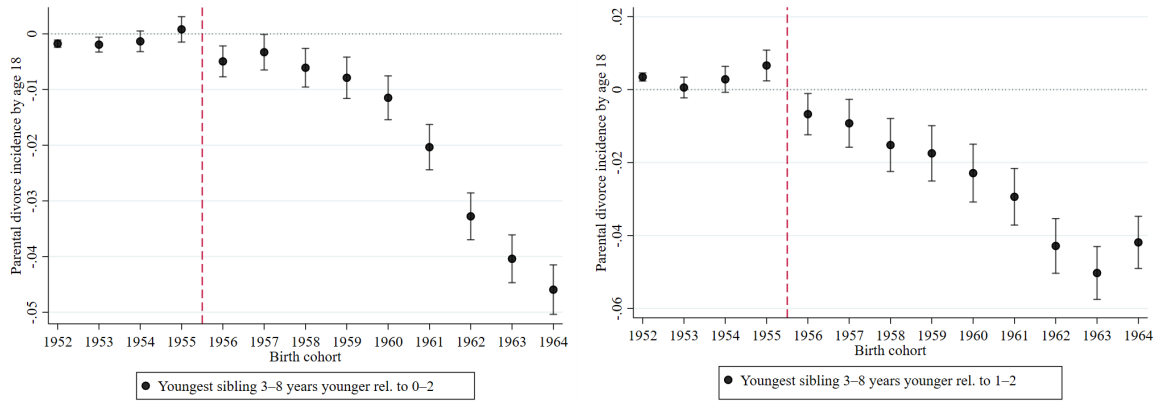
(a) Parental divorce rate by age 18, youngest sib. 3–8 years younger v. 0–2.

(b) Parental div. rate by age 18, youngest sib. 3–8 years younger v. 1–2 (excl. youngest sibs.).

Figure 6: Effects of the reconsideration period on parental divorce by age 18.
Panel 6a disaggregates the main estimate on experiencing parental divorce by following cohorts born 1952–1964 until age 18 separately by birth cohort and estimates the difference in parental divorce rate between the large age spacing group (3–8) against the small age spacing group (0–2). Panel 6b also excludes youngest siblings from the reference group (spacing 0). The controls include parents' birth cohort, municipality of residence in 1970, labor market outcomes and education in 1970, sex of the child, birth month, and indicators of missing values. CI95 are indicated in black, and standard errors are clustered at the household level.

outcomes provide valuable evidence that the reconsideration period affected family behavior.

Effects on educational outcomes. The preferred outcome where I expect this kind of a divorce restriction to affect children's outcomes is upper secondary school completion. Firstly, the reconsideration period is expected to strengthen the marital stability of parents and to increase parental investments, which could benefit the affected children. Moreover, upper secondary school completion is primarily determined during ages 16–19, which are the ages when the treated children are more shielded against parental divorce. Upper secondary school completion is also marked by its importance in predicting other important outcomes later in life, which makes this a key focus of study (Freudenberg & Ruglis, 2007; Heckman et al., 2008; Oreopoulos & Salvanes, 2011; Lochner, 2020). However, I also include an outcome capturing years of schooling to show a broader measure.

The OLS estimates for the effects on upper secondary school completion and the effects on years of schooling can be seen in Table 4. Greater exposure to the reconsideration period is shown to have significantly increased the upper secondary school completion rate for exposed cohorts (0.015, s.e. 0.002) when including all children, while the main effect for the placebo cohorts is significant and negative (−0.007, s.e. 0.002). Years of schooling increased by almost 0.11 years (0.106, s.e. 0.010) in response to the reform. The results for upper secondary schooling when focusing on elder siblings are also statistically significant and positive, albeit weaker, for the affected cohorts (0.008, s.e. 0.003), and not significant for the older placebo cohorts (0.004, s.e. 0.003). The same holds for the treatment effect on years of schooling (0.063, s.e. 0.017).[31]

The graphical representation of the effects on upper secondary school completion rate by cohort can be seen in Figure 7. Panel 7a shows results for all children, while Panel 7b focuses on elder siblings. The figures show that children's schooling outcomes were positively affected, starting around the time of the cohorts affected by the reform. For the placebo cohorts, there is no visible improvement or sign of a pre-trend.

Effects on educational attainment, labor market outcomes, and family outcomes. In order to provide a broader picture of children's outcomes later in life, I present a range of schooling outcomes, labor market outcomes, and family outcomes for the same study sample. These results

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[31]Relative to the mean outcome of the control group, the effects correspond to an increase of 0.5–1.8%.

Table 4: Effect of a 6-month parental reconsideration period for divorce on upper secondary school completion and years of schooling.

Sample:	All children		Elder siblings	
Outcome:	Upper sec. completion	Years of schooling	Upper sec. completion	Years of schooling
$Insulation_i \times Cohort_i \geq 1956$	0.015***	0.106***	0.008**	0.063***
	(0.002)	(0.010)	(0.003)	(0.017)
$Insulation_i$	-0.007***	-0.018**	0.004	0.004
	(0.002)	(0.009)	(0.003)	(0.015)
Mean dep. var.	0.825	11.670	0.815	11.630
Obs.	1,073,396	1,073,396	549,271	549,271
Cohort FE	✓	✓	✓	✓
Parent cohort FE	✓	✓	✓	✓

Note: ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$. Standard errors in parenthesis are clustered at the household level. "Elder siblings" excludes youngest siblings (spacing 0). "Upper sec. completion" is defined as upper secondary education of two years or more, or any higher education in 1990. "Years of schooling" denotes years of schooling. The empirical specification and controls are the same as those specified in Table 3. "Mean dep. var." refers to mean dependent variable for the reference category with age spacing 0–2.
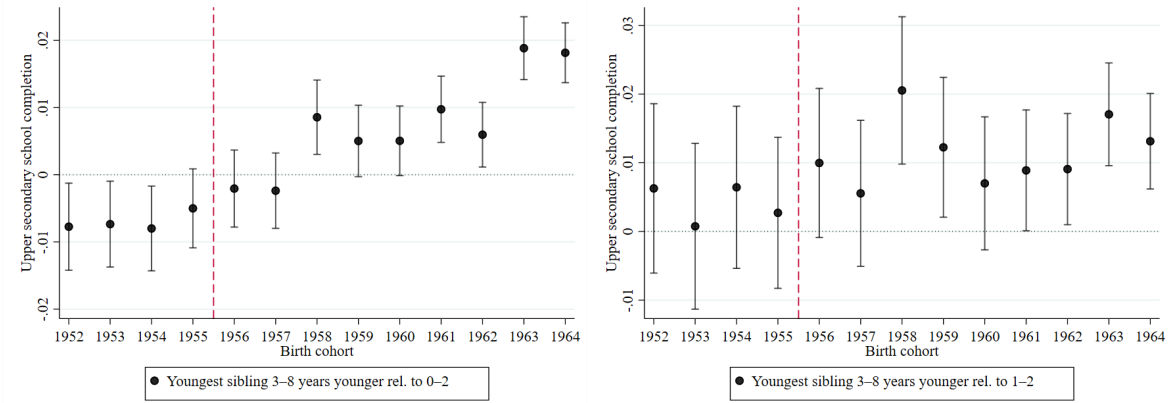
(a) Upper secondary school completion rate, youngest sibling 3–8 years younger v. 0–2 years younger.

(b) Upper secondary school completion rate, youngest sib. 3–8 years younger v. 1–2 years (excl. youngest siblings).

Figure 7: Effects of the reconsideration period on upper secondary school completion.
Panel 7a disaggregates the main estimate on upper secondary school completion by following cohorts born 1952–1964 separately by birth cohort and estimates the difference in upper secondary school completion rate between the large age spacing group (3–8) against the small age spacing group (0–2). Panel 7b does the same while excluding youngest siblings themselves from the reference group (spacing 0). The controls are the same as those specified in Figure 6. CI95 are indicated in black, and standard errors are clustered at the household level.

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and the following are shown for the main empirical specification with all children included.[32]

The first set of related outcomes includes further education and labor market outcomes (see Table 5). The estimated effect on having a university education (0.008, s.e. 0.02), earnings in 1990 SEK 100 (12.067, s.e. 3.323), log earnings (0.013, s.e. 0.003), and employment probability (0.004, s.e. 0.001) in 1990 are all positive and statistically significant, in line with the previous findings.[33]

Table 5: Effect of a 6-month parental reconsideration period for divorce on university and labor market outcomes in 1990.

Outcome:	University graduation	Earnings	Log earnings	Employed
$Insulation_i \times Cohort_i \geq 1956$	0.008***	12.067***	0.013***	0.004***
	(0.002)	(3.323)	(0.003)	(0.001)
$Insulation_i$	0.002*	2.323	−0.001	−0.001
	(0.001)	(3.035)	(0.003)	(0.001)
Mean dep. var.	0.110	1,320.449	7.044	0.898
Obs.	1,073,396	1,133,874	1,099,917	1,133,873
Cohort FE	✓	✓	✓	✓
Parent cohort FE	✓	✓	✓	✓

Note: ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$. Standard errors in parenthesis are clustered at the household level. "University graduation" refers to three years or more of university education in 1990. Earnings and employment outcomes are for the same year. The empirical specification and controls are the same as those specified in Table 3. "Mean dep. var." refers to mean dependent variable for the reference category with age spacing 0–2.

The quasi-experimental setting of the reform also allows me to investigate the extent to which family outcomes are transmitted across generations to the children themselves as adults (see Table 6). Previous research indicates that parental marriage stability transmits across generations, and suggests that parental divorce affects children's behavior as adults in their own marriages (e.g. Amato, 1996; Corak, 2001; Teachman, 2002). The extent to which these effects also are transmitted by growing up with more years of exposure to a divorce restriction is unclear ex ante.

The findings indicate that the children with greater exposure to the reconsideration period were themselves more likely to have ever married (0.008, s.e. 0.002), and less likely to have ever

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[32]Results for elder siblings on related outcomes can be seen in Appendix B (see Tables A5 & A6).

[33]I also show that these observed outcomes are of the same magnitude or even stronger when estimating the effects for the same outcomes ten years later in year 2000. Results are available upon request.

divorced (-0.005, s.e. 0.002). The results on family outcomes are validated in the 1990 census, where the treated children (ages 26–38) were less likely to be single parents (-0.003, s.e. 0.001), and more likely to be married or cohabiting (0.007, s.e. 0.002) at that time. The effect on being a parent is precisely measured at zero (-0.001, s.e. 0.002).[34] The effects on family outcomes are large in relative terms (1.3% ever married, -3.8% ever divorced, and -2.6% risk of single parenthood).

Table 6: Effect of a 6-month parental reconsideration period for divorce on family outcomes in 1990 & 2000.

Outcome:	Year 2000		Census 1990			
	Ever married	Ever divorced	Single parent	Marr./Cohab.	Cohabiting	Parent
$Insulation_i \times Cohort_i \geq 1956$	0.008***	−0.005***	−0.003**	0.007***	0.000	−0.001
	(0.002)	(0.002)	(0.001)	(0.002)	(0.002)	(0.002)
$Insulation_i$	−0.000	0.003**	0.005***	0.002	−0.000	0.012***
	(0.002)	(0.002)	(0.001)	(0.002)	(0.001)	(0.002)
Mean dep. var.	0.601	0.133	0.115	0.644	0.260	0.453
Obs.	1,120,451	1,120,451	1,069,027	1,069,027	1,069,027	1,168,874
Cohort FE	✓	✓	✓	✓	✓	✓
Parent cohort FE	✓	✓	✓	✓	✓	✓

Note: ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$. Standard errors in parenthesis are clustered at the household level. "Ever married" and "Ever divorced" refers to ever marrying or divorcing by year 2000. "Single parent" is defined through the census in 1990, "Marr./Cohab." is defined as cohabiting or being married, "Cohabiting" is defined as cohabiting without being married, and "Parent" is defined as having a child age 0–6 at the same year. The empirical specification and controls are the same as those specified in Table 3. "Mean dep. var." refers to mean dependent variable for the reference category with age spacing 0–2.

4.4 Heterogeneous treatment effects

In this section, I present heterogeneous treatment effects of the reconsideration period for children at risk of experiencing parental divorce, along with two categories of heterogeneity related to previous research: sex of the child and parental earnings. Sociological research has found indications of divorce and marriage stability affecting sons more than daughters, and a recent paper shows an increased divorce risk for parents with a daughter in the family (Kabátek & Ribar, 2020).[35] Relating to the theoretical evidence and existing empirical research, parental earnings are a prime candidate

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[34]These outcomes are measured when the children are ages 36–48. Marriage drives the effect on married or cohabiting, since the cohabiting outcome is precisely estimated at zero (0.000, s.e. 0.002).

[35]The mechanisms behind this are not clear. Boys are more prone to behavioral problems than girls and the literature has speculated that parental divorce may exacerbate this difference (Amato, 2001; Aggarwal, 2019).

to capture elements of within-household bargaining linked to monetary resources (Stevenson, 2007; Fernández & Wong, 2014; Voena, 2015).[36]

Effects on children at risk of experiencing parental divorce. To strengthen the link between the effects on marriage stability and the educational outcomes, I present evidence that the effects on upper secondary school completion are driven by parents in more unstable marriages responding to the reform. I do so by showing treatment effects by quintile of predicted risk of experiencing parental divorce in Figure 8. The at-risk split is performed by predicting the likelihood of experiencing divorce by age 18, using pre-determined characteristics, and subsequently dividing the sample into quintiles based on the risk of experiencing parental divorce.[37]

The figure shows that the families with the lowest predicted risk of divorce (Q1–Q2) exhibited no significant improvement in educational outcomes for their children. In contrast, the families at medium and high risk of divorce (Q3–Q5) show significant treatment effects. This pattern is consistent with more unstable families altering their behavior in response to the reform. This strengthens the argument that the reconsideration period for divorce is indeed the driving force behind the observed effects on upper secondary school completion.

Sex of the child. I investigate heterogeneous treatment effects of the reconsideration period on the main outcomes in Table 7 by fully interacting the previously specified regression equation with an indicator for sex of the child (female). Contrary to Kabátek & Ribar (2020), I find no significant difference in parental divorce incidence by sex of the child (0.002, s.e. 0.002). Although, it may be that the small magnitude of the effect sizes in the original paper translates into an effect size too small to capture with this policy reform. However, I find that daughters are significantly less affected in terms of upper secondary school completion rate than sons (−0.012, s.e. 0.004). This difference is substantial. Also, the effect on being a single parent in 1990 appears to be entirely driven by girls (−0.006, s.e. 0.003).

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[36]For instance, a father with greater monetary resources could be able to invest more in the marriage or have greater capacity to compensate mothers to keep the marriage intact, and allow for more time investments in children. Contrary to this, mothers with a greater labor market attachment may be less prone to increase their specialization in household activities following the reform.

[37]The prediction does well in capturing actual divorce behavior of parents, with the Q5-Q1 realized difference in divorce incidence being 25 pp.
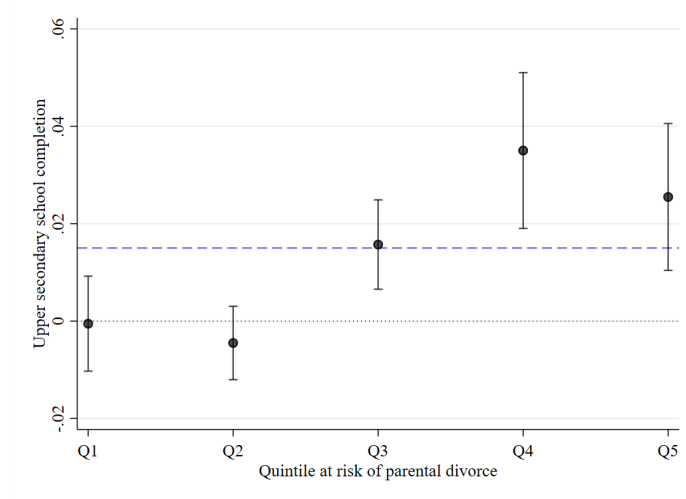
Figure 8: Treatment effect split by pre-determined quintile at risk of experiencing parental divorce. The figure splits the effects of the reconsideration period on upper secondary school completion by predicted quintile of experiencing parental divorce, based on background information (educational attainment, labor market outcomes, and municipality of residence) and family characteristics (parents' birth cohort, number of children, birth month, sex of the child, family status, and age of youngest sibling) from the 1970 census in separate regressions by each quintile. The dashed blue line marks the baseline estimate for upper secondary school completion presented in the paper. The underlying prediction of divorce by age 18 produces a Q5–Q1 realized divorce difference of 25 pp., with an R^2 of 0.145. CI95 are indicated in black, and standard errors are clustered at the household level.

Table 7: Effect of a 6-month parental reconsideration period for divorce on schooling, labor market, and family outcomes, by sex of the child.

Outcome:	Divorce by 18	Upper sec. completion	Log earnings	Ever married	Ever divorced	Single parent
$Insulation_i \times Cohort_i \times Female_i$	0.002	−0.012***	−0.005	−0.005	−0.005	−0.006**
	(0.002)	(0.004)	(0.006)	(0.004)	(0.003)	(0.003)
$Insulation_i \times Cohort_i \geq 1956$	−0.020***	0.021***	0.015***	0.011***	−0.003	−0.000
	(0.001)	(0.003)	(0.004)	(0.003)	(0.002)	(0.001)
Mean dep. var.	0.103	0.848	6.853	0.655	0.154	0.222
Obs.	1,140,727	1,073,396	1,099,917	1,120,451	1,120,451	1,069,027
Cohort FE	✓	✓	✓	✓	✓	✓
Parent cohort FE	✓	✓	✓	✓	✓	✓

Note: ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$. Standard errors in parenthesis are clustered at the household level. "Divorce by 18" refers to experiencing parental divorce by age 18. "Upper sec. completion" is defined as upper secondary education less or equal to three years. "Log earnings" is the natural logarithm of earnings in 1990. "Ever married" and "Ever divorced" refers to ever marrying or divorcing by year 2000. "Single parent" is defined as being a single parent in the 1990 census. "$Insulation_i$" indicates greater age spacing (3–8 years to youngest sibling against 0–2 years). The interaction with cohort shows the difference in effect between cohort groups 1956–1964 and 1952–1955. "$Insulation_i \times Cohort_i \times Female_i$" captures the difference in effect between women and men and indicates a model fully interacted by sex. The controls are the same as those specified in Table 3. "Mean dep. var." refers to mean dependent variable for women in the reference category with age spacing 0–2.

A graphical representation of the effects on upper secondary completion rate, split by sex of the child and educational attainment of the parents, is shown in Panel 9. The panel confirms that sons were more affected than daughters, and adds that the treatment effects stem from children whose parents have at most upper secondary education. The treatment effect is not visible for families with some university education, for the concerned period less than 10% of the adult population.
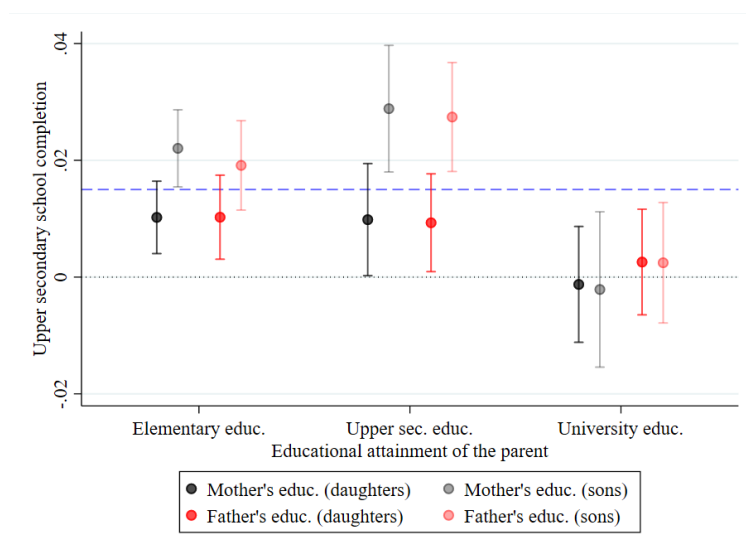

Figure 9: Treatment effects split by parental education, and sex of the child.
The figures split the effects of the reconsideration period on upper secondary school completion by parent's educational attainment, and sex of the child from separate regressions. The dashed blue line indicates the baseline effect estimated when pooling the categories. The controls are the same as those specified in Figure 6 (excluding sex and education of the relevant parent). CI95 are indicated in black, and standard errors are clustered at the household level.

Parental earnings. Next, I investigate a channel related to within-household bargaining of the parents in Table 8 by fully interacting the regression specification with an indicator for above-median maternal or paternal earnings in 1970. Ex ante, I expect higher earnings to be related to greater bargaining strength for the parent. I find that parental divorce incidence was almost equal for children with the mother earning above and below the median (0.001, s.e. 0.002), but children with the father earning above the median were significantly less likely to experience parental divorce (−0.006, s.e. 0.002) than the group with fathers earning below the median. This finding is consistent with financial resources being used to take advantage of the stabilizing effects of the divorce restriction.

The opposite pattern is found for upper secondary school completion rate, where the difference in effect with above-median earnings is statistically significant and negative for mothers (-0.010, s.e. 0.004), while positive but not significant for fathers (0.005, s.e. 0.004). A possible explanation for this is that greater maternal attachment to the labor market could lead to less household investments following the reform, which in turn could dampen the treatment effects.[38]

Table 8: Effect of a 6-month parental reconsideration period for divorce on parental divorce and upper secondary school completion, by parental earnings.

Split: Outcome:	Mothers' earnings 1970		Fathers' earnings 1970	
	Divorce by 18	Upper sec. completion	Divorce by 18	Upper sec. completion
$Insulation_i \times Cohort_i \times Earnings_{mother 1970}$	0.001 (0.002)	-0.010*** (0.004)		
$Insulation_i \times Cohort_i \times Earnings_{father 1970}$			-0.006*** (0.002)	0.005 (0.004)
$Insulation_i \times Cohort_i \geq 1956$	-0.015*** (0.001)	0.016*** (0.003)	-0.018*** (0.001)	0.010*** (0.003)
Mean dep. var.	0.081	0.822	0.108	0.778
Obs.	1,140,727	1,073,396	1,140,727	1,073,396
Cohort FE	✓	✓	✓	✓
Parent cohort FE	✓	✓	✓	✓

Note: ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$. Standard errors in parenthesis are clustered at the household level. "Divorce by 18" refers to experiencing parental divorce by age 18. "Upper sec. completion" is defined as upper secondary education less or equal to three years. "$Insulation_i$" indicates greater age spacing (3–8 years to youngest sibling against 0–2 years). The interaction with cohort shows the difference in effect between cohort groups 1956–1964 and 1952–1955 and those with below median parental earnings (reference category). "$Insulation_i \times Cohort_i \times Earnings$" captures the difference in effect between those with parental earnings above and below median earnings 1970 and indicates a model fully interacted by an indicator for above median earnings. The controls are the same as those specified in Table 3. "Mean dep. var." refers to mean dependent variable for those with below median earnings in the reference category with age spacing 0–2.

4.5   Robustness checks

In order to validate the results, I run a battery of robustness checks related to treatment definition, group composition changes, and the choice of control variables. The aim of these tests are to rule

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[38]These results are generally consistent with the theoretical framework in Appendix B, and indicate that the reform affects families differently depending on pre-defined characteristics related to earnings and household specialization.

out alternative explanations, and to attribute the observed effects to the divorce law reform.

Parallel trends.   As previously discussed, the identifying assumption needed for the identification strategy to give causal interpretation is that the effect of age spacing or marriage status must be constant over cohort groups. In other words, that the outcome in the treatment and comparison groups would evolve similarly over different birth cohorts under the absence of no policy reform. If this assumption holds, the cohorts having graduated from upper secondary school before the policy came into effect can be used to net out any pre-existing effects of age spacing on children's outcomes.

The primary evidence of parallel trends can be seen in Figures 4, 6, & 7. These figures show that, in terms of parental divorce and upper secondary school completion, there are no pre-trends for the placebo cohorts born 1952–1955.[39] Furthermore, I extend the number of placebo cohorts for the main educational outcome in Figure A3. This figure shows that there is also no pre-trend for the evaluation of the reconsideration period when considering cohorts as far back as those born in 1938.[40]

Group composition changes.   Another important robustness check related to the parallel trends assumption concerns the composition of parents with children of different age spacing over time. If, for instance, more educated parents increased the age spacing of their children over time, the validity of my identification strategy could be compromised. To address this concern, I predict the children's likelihood of experiencing parental divorce and upper secondary school completion using family characteristics from 1970, before the policy was implemented.

Given that children's age and sibling age spacing may affect outcomes for parents by 1970, an imbalance for observables in 1970 (e.g. parents' earnings) is expected. This problem is solved by using the characteristics of grandparents to predict the outcomes of interest. The grandparents should have been less directly affected by the age spacing of their grandchildren in 1970 in terms of their observables, but can still serve as a reasonable proxy for family characteristics. The set

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[39]A potential exception is the cohort born in 1955, which could be due to anticipation effects or direct policy effects.
[40]This extension is only feasible for the educational outcome, as the divorce data starts in 1969. The longer trends for the divorce liberalization element are more challenging to interpret, given the noisy estimates due to the decreasing number of unmarried parents with each older birth cohort.

of covariates for grandparents in 1970 includes educational attainment, hours worked, earnings, family type, and municipality of residence. The results with predicted outcomes based on these characteristics can be seen in Figure 10. The predicted outcomes are shown to be relatively stable over time around zero. An F-test of joint significance of the coefficients is unable to reject the null of no statistical difference between the affected cohorts and the placebo cohorts.[41]
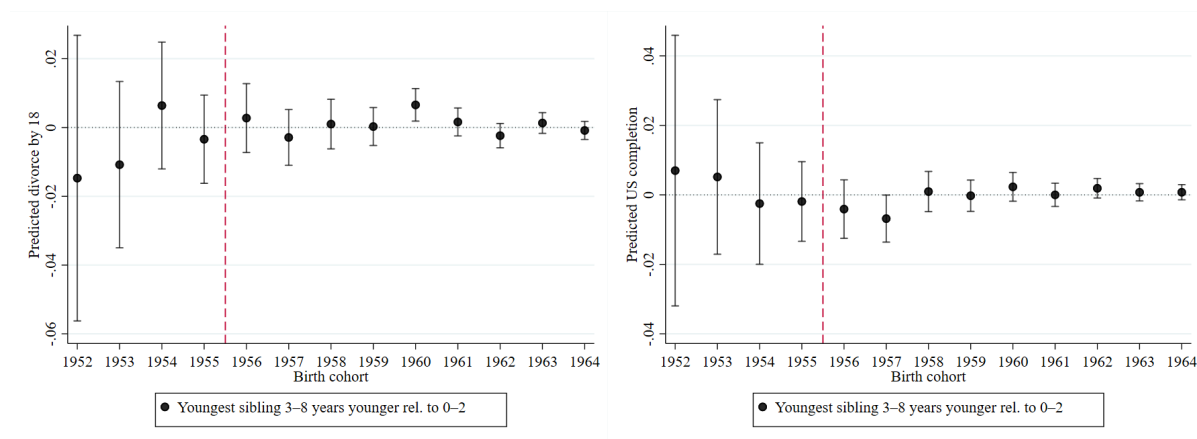
(a) Difference in predicted parental divorce rate by age 18, youngest sibling 3–8 years younger relative to 0–2 years younger.

(b) Difference in predicted upper secondary school completion rate, youngest sibling 3–8 years younger relative to 0–2 years younger.

Figure 10: Balancing tests for the divorce restriction element.

Panel 10a uses predicted parental divorce by 18 outcomes from grandparental characteristics in 1970 (a set of covariates capturing socioeconomic status: earnings, educational attainment, family type, hours worked and municipality of residence), with an R^2 of 0.087, and estimates the difference in parental divorce rate between the large age spacing group (3–8) against the small age spacing group (0–2). Panel 10b does the same when predicting the upper secondary school outcomes. An F-test of joint significance for the coefficients in the post period gives a p-value of 0.437 for predicted divorce and 0.675 for predicted upper secondary school completion. CI95 are indicated in black, and standard errors are clustered at the household level.

For completeness, Figure A5 shows predicted outcomes using parental and child characteristics in 1970, with the aforementioned caveats. The covariates used to predict the outcomes include parents' birth cohort, sex of the child, birth month, earnings and employment status of parents, educational attainment of parents, hours worked, municipality of residence, and indicators of missing values. The predicted outcomes show an increasing incidence of parental divorce and a decreasing

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[41]Due to the Multi-Generation Register containing parent-child linkages for individuals born 1932 and later, fewer grandparents are observable for the older cohorts. The grandparental characteristics do well in predicting actual outcomes, with the predicted Q1-Q4 difference (quartiles based on the predicted outcomes) in average actual outcomes being 20–24 pp. for parental divorce and upper secondary school completion. The R^2 of the predictions range from 0.044–0.068, which is relatively low but perhaps to be expected given the complexities of predicting actual behavior. The p-value is 0.437 for predicted parental divorce by 18 and 0.675 for predicted upper secondary school completion.

upper secondary school completion rate by birth cohort, in contrast with the observed effects of the opposite sign.[42] Thus, composition changes between cohorts do not appear to drive the effects.

Age spacing cutoff.   I also test to what extent the choice of treatment cutoff for the age spacing of children is behind the observed effects. The preferred choice of cutoff is set at three years in the main specification, mainly to correspond to the length of Swedish academic upper secondary school. As a robustness check, results on upper secondary school completion when changing the age spacing comparison groups step-wise are displayed in Table A2.[43] The treatment effects remain the same when the cutoff is moved closer to spacing 0 (0–1 against 2–8: 0.014, s.e. 0.002; and 0 against 1–8: 0.015, s.e. 0.002), highly similar to the baseline cutoff results, and when the comparison group is limited to a narrow age spacing range (0 against 1–3: 0.014, s.e. 0.002).

Contrarily, the effects weaken as the cutoff is moved in the other direction (0–3 against 4–8: 0.011, s.e. 0.002). This is to be expected, since changing the cutoff in this direction makes the comparison groups more similar. When restricting the age spacing range for elder siblings, the magnitude shrinks somewhat (2 against 3–4: 0.008, s.e. 0.004). However, the effect remains positive and is significant for the most restrictive comparison (2 years of spacing against 3: 0.010, s.e. 0.05). This indicates that the treatment effect is robust to different choices of age spacing cutoff.[44]

Placebo test: Children with unmarried parents.   Relating to the previous robustness check, the age spacing of siblings should not matter directly for children with unmarried parents, since there is no corresponding reconsideration period for separation between unmarried cohabiting couples. The main caveat with this test is the small number of children with unmarried parents, and that marriage status is defined in 1970, which entails a risk that some parents of this group marry over time and become affected by the reconsideration period. Figure A6 displays upper secondary school completion rate for the placebo group with unmarried parents in 1970, comparing children

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[42]These predictions perform similarly to the grandparental version, with the predicted Q1-Q4 actual difference in outcomes being 20–26 pp. The R^2 of the regressions range from 0.060–0.087.

[43]Further robustness tests on this note is presented Appendix B, Table A4. The table shows estimates including children with age spacing up to 18 years, and revising the age spacing assignment to 1973, which is shown to moderately strengthen the main effect on educational outcomes.

[44]A graphical representation of the effect on elder siblings compared to the youngest children can be seen in Panel A2. This shows that the effect is strong and relatively stable for all choices of spacing cutoff.

with 3–8 years of age spacing against 0–2 years. The figure exhibits wide confidence intervals for the oldest cohorts, but shows no clear pattern of improved schooling outcomes.[45]

Direct effects of experiencing divorce, family fixed effects, and excluding controls. In order to put the magnitudes of the main effects on upper secondary school completion in a context, and to further test the robustness of the estimates, Table A3 presents the direct effect of experiencing divorce by age 18 on upper secondary school completion, OLS family fixed effects models similar to previous research (e.g Björklund & Sundström, 2006; Chen et al., 2019), the baseline age spacing models augmented with family fixed effects, and the baseline age spacing regression on upper secondary school completion when excluding controls for background characteristics.[46]

The direct effect of experiencing parental divorce on upper secondary school completion is shown to be large and significant, even when including controls of background characteristics (−0.069, s.e. 0.002), but the estimate reduces substantially when including family fixed effects to account for selection into divorce (−0.028, s.e. 0.007). The family fixed effects estimate is almost twice as large as the estimate using age spacing for identification. Reassuringly, adding family fixed effects to the main specification based on age spacing and relying on variation within a given family as a robustness test leaves the estimates slightly stronger when including all children (0.017, s.e. 0.004) and when restricting the sample to elder siblings (0.013, s.e. 0.009). This indicates that the results are driven by differential parental investments within a given family.

I further test the robustness of the estimates by excluding all control variables related to background characteristics. The results can be seen in Table A3. Broadly, the estimates are slightly stronger when focusing on all children (0.018, s.e. 0.002), and slightly weaker for elder siblings (0.007, s.e. 0.004) compared to the baseline estimate, but all estimates remain statistically significant. All in all, the estimates are stable across specifications and choice of control variables, and the precision of the estimates improves somewhat with the added control variables.

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[45]A weighted F-test fails to reject the null of equality between the coefficients (p-value 0.576).

[46]In Appendix B (Table A4), I also show results when including extensive controls which are expected to capture much of the variation associated with age spacing (birth order effects, a linear age spacing control, and number of sibling fixed effects). These controls are shown to reduce the main estimate to around half the magnitude (0.06–0.08, s.e. 0.02). However, the effect remains statistically significant despite adding these extensive controls.

4.6 Mechanisms

The findings presented up until now warrant further investigation into the mechanisms of how families adjust their behavior in response to the divorce restriction. I proceed below by showing four tests that signal changes to within-household behavior, and while none of these in isolation provide conclusive evidence of such changes, together they suggest that this mechanism could be an important driver of the results.

Parental labor supply. The first mechanism relates to changes in parental labor supply. Previous research (e.g. Stevenson, 2007; Fernández & Wong, 2014; Ringdal & Sjursen, 2021) has focused on measuring responsiveness in spousal labor supply to divorce law reform and interpreted this as a sign of changing bargaining between spouses. This work has shown that changes to divorce laws can affect labor supply of married women, and that shifting bargaining power can translate into more parental investments. A change in labor supply could also indicate changes to parental investments.

I investigate the effects on labor supply by observing parental earnings and hours worked in the census of 1975. In order to avoid having duplicate observations of parents, I restrict the regressions to only include the oldest child in the family. The results in Table 9 show no consistent significant effects on fathers' labor market outcomes related to earnings in SEK 100 (0.235, s.e. 1.310), employment probability (0.002, s.e. 0.001), and hours worked (0.105, s.e. 0.054) in 1975. However, there is a substantial decrease in the mothers' earnings (−11.136, s.e. 0.646), employment (−0.022, s.e. 0.002), and hours worked per week (−1.376, s.e. 0.072), which could suggest changes to household bargaining and greater parental investments in response to the reconsideration period.[47]

Intergenerational transmission of human capital. The second mechanism provides additional evidence of increased parental investments following the reform by estimating the intergenerational correlation in educational outcomes (see Table 10). This measure is widely accepted to capture intergenerational transmission of human capital between parents and children, and can be linked to parental investments (Black et al., 2005). The findings show that the correlation

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[47]To alleviate concerns that I pick up effects of the age profile of children on mothers' labor supply, the regression model includes a control for age of youngest child and controls for the parent's pre-period labor market outcomes in 1970, which leaves the estimates for mothers strongly significant.

Table 9: Effect of a 6-month parental reconsideration period for divorce on parental labor market outcomes in 1975.

Outcome:	Fathers 1975			Mothers 1975		
	Earnings	Employed	Hours	Earnings	Employed	Hours
$Insulation_i \times Cohort_i \geq 1956$	0.235	0.002	0.105*	−11.136***	−0.022***	-1.376***
	(1.310)	(0.001)	(0.054)	(0.646)	(0.002)	(0.072)
$Insulation_i$	5.423***	0.005***	0.243***	12.326***	0.042***	1.705***
	(1.713)	(0.002)	(0.063)	(0.785)	(0.003)	(0.087)
Mean dep. var.	517.396	0.920	36.031	215.578	0.731	22.479
Obs.	653,281	648,552	648,552	671,237	667,966	667,966
Cohort FE	✓	✓	✓	✓	✓	✓
Parent cohort FE	✓	✓	✓	✓	✓	✓

Note: ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$. Standard errors in parenthesis are clustered at the household level. The outcomes are estimated using the oldest children in each family. The empirical specification is the same as the one specified in Table 3. Besides cohort and parent cohort effects, the controls include municipality of residence in 1970, labor market outcomes and education of parents in 1970, sex of the child, birth month, indicators of missing values, and age of the youngest child in the family. "Mean dep. var." refers to mean dependent variable for the reference category with age spacing 0–2.

strengthened significantly between children and their mothers following the introduction of the reconsideration period (0.005, s.e. 0.001), while the change in the link to the fathers is borderline significantly stronger (0.001–0.002, s.e. 0.001).[48] This result strengthens the case that children were positively affected by greater maternal investments in response to the reconsideration period.

Cognitive and non-cognitive development. The third mechanism uses the results from the conscription tests, which supplement the educational findings and help shed light on non-cognitive and cognitive development for children affected by the divorce law reform. Previous research has shown that reforms targeting adolescents can persistently improve the children's development, especially their non-cognitive ability (Heckman, 2000).

The effects on standardized measures of abilities for men around age 18 can be seen in Table 11. The estimates on both cognitive ability and non-cognitive ability are statistically significant and positive for the cohorts exposed to the reform, with greater exposure increasing ability by 0.027–0.050 standard deviations (0.027–0.050 SD, s.e. 0.006–0.007). To put these magnitudes in a context, the

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[48]The results remain stable for the mother-child link regardless of including child and parental controls to the regression, while the precision and magnitude of the estimate for fathers increases somewhat with added controls.

Table 10: Effect of a 6-month parental reconsideration period for divorce on the intergenerational correlation in education.

Outcome:	Mothers			Fathers		
	IGE	IGE	IGE	IGE	IGE	IGE
$Insulation_i \times Cohort_i \geq 1956 \times Educ_{p.1970}$	0.005***	0.005***	0.005***	0.001	0.002*	0.002**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
$Insulation_i \times Educ_{parent1970}$	0.007***	0.008***	0.005***	0.007***	0.010***	0.007***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
$Educ_{parent1970}$	0.265***	0.254***	0.144***	0.246***	0.235***	0.135***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)
Mean dep. var.	0.242	0.242	0.242	0.222	0.222	0.222
Obs.	1,033,397	1,033,397	1,033,397	991,844	991,844	991,844
Parent cohort FE	✓	✓	✓	✓	✓	✓
Child controls		✓	✓		✓	✓
Other parental controls			✓			✓

Note: ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$. The outcome is defined as child's length of education in years in 1990, which is regressed on the mother's or father's education in 1970 interacted with the reform evaluation specification. Standard errors in parenthesis are clustered at the household level. "Educ" denotes educational outcome of the parent in 1970. The empirical specification is the same as the one specified in Table 3 except for the interactions and main effects of the parent's educational attainment. Cohort, parent cohort effects and indicators of missing values are always included. "Child controls" include municipality of residence in 1970, sex of the child, and birth month. "Other parental controls" include labor market outcomes and educational attainment of the other parent in 1970. "Mean dep. var." refers to mean dependent variable for the reference category with age spacing 0–2.

effects on composite ability amount to roughly 30–40% of the birth order effects on ability when comparing first-born to second-born siblings (Black et al., 2018).[49] The increase in ability is the strongest for the youngest cohorts with more years of exposure to the reform. For the placebo cohorts, the main effects are weaker in magnitude and mostly not statistically significant. These findings are consistent with greater parental investments benefiting children's development, which could help explain the effects on long-term outcomes.

Table 11: Effect of a 6-month parental reconsideration period for divorce on male non-cognitive and cognitive abilities age 18.

Outcome:	Non-cognitive abilities NCA composite				Cognitive abilities CA composite			
$Insul_i \times Cohort_i \geq 1956$	0.040***				0.053***			
	(0.006)				(0.006)			
$Insulation_i$	−0.010*				−0.004			
	(0.006)				(0.005)			
Outcome:	Maturity	Intensity	Ps. energy	Stability	Logic	Verbal	Spatial	Technical
$Insul_i \times Cohort_i \geq 1956$	0.027***	0.028***	0.028***	0.033***	0.050***	0.044***	0.040***	0.033***
	(0.006)	(0.007)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
$Insulation_i$	−0.003	−0.004	0.003	−0.009	−0.005	−0.007	−0.009	0.017***
	(0.005)	(0.006)	(0.006)	(0.006)	(0.005)	(0.005)	(0.005)	(0.006)
Obs.	506,317	506,317	506,317	506,317	506,349	506,349	506,349	506,349
Cohort FE	✓	✓	✓	✓	✓	✓	✓	✓
Parent cohort FE	✓	✓	✓	✓	✓	✓	✓	✓

Note: ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$. Standard errors in parenthesis are clustered at the household level. "Maturity" refers to social maturity, "Ps. energy" to psychological energy, "Stability" to emotional stability. "Logic" refers to logical thinking, "Verbal" to verbal ability, "Spatial" to 3D spatial thinking, and "Technical" to a technical understanding test. All outcomes are measured at approximately age 18. The empirical specification and the controls are the same as those specified in Table 3.

Timing of fertility. The fourth and final mechanism investigated is the timing of fertility decisions, with focus on teenage parenthood. It has been well documented that early parenthood is associated with poor long-term economic outcomes for the parent and child (Kearney & Levine, 2012). This mechanism is related to family stability, and could indicate risky behavior among

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[49]The effects on cognitive ability are generally stronger than those on non-cognitive ability, which is surprising if cognitive ability, more so than non-cognitive ability, is mostly determined at relatively young ages. Alternatively, the cognitive ability effect equals the inverse effect of increasing class size by 2 children (Fredriksson et al., 2013).

adolescents relating to the findings on non-cognitive development (Heckman et al., 2006).

Delving deeper into these outcomes in Table 12, the null effect on ever becoming a parent (0.001, s.e. 0.002) is still observed even later in life up to year 2014. However, the age when having the first child is significantly higher by about two months (0.162, s.e. 0.025), and the risk of being a teen parent is significantly lower (−0.006, s.e. 0.001).[50] Splitting the teenage parenthood outcome by sex, the risk of becoming a teenage father is significantly lower (−0.003, s.e. 0.001) along with teenage motherhood (−0.009, s.e. 0.002), but the effect is stronger in absolute magnitude for girls. The estimated results are large, with the relative effects being equivalent to a 18–38% reduction in the risk of teenage parenthood.[51]

Table 12: Effect of a 6-month parental reconsideration period for divorce on fertility outcomes.

Outcome:	Ever parent	Age at first child	Teen parent	Teen mother	Teen father
$Insulation_i \times Cohort_i \geq 1956$	0.001	0.162***	−0.006***	−0.009***	−0.003***
	(0.002)	(0.025)	(0.001)	(0.002)	(0.001)
$Insulation_i$	0.011***	−0.219***	0.009***	0.015***	0.004***
	(0.001)	(0.022)	(0.001)	(0.002)	(0.001)
Mean dep. var.	0.763	27.587	0.029	0.051	0.008
Obs.	1,168,874	914,589	1,168,874	568,412	600,462
Cohort FE	✓	✓	✓	✓	✓
Parent cohort FE	✓	✓	✓	✓	✓

Note: ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$. Standard errors in parenthesis are clustered at the household level. "Ever parent" and "Age at first child" refers to being a parent by year 2014 (the final year of the Multi-Generation Register from which these outcomes are taken) and the age of the child at the time of birth of their own first child. "Teen parent" is defined as having a child before age 20, while "Teen mother/father." splits this outcome by the sex of the teenage parent. The empirical specification and the controls are the same as those specified in Table 3. "Mean dep. var." refers to mean dependent variable for the reference category with age spacing 0–2.

4.7 Mediation analysis

Next, I try to gauge at how much of the effects on later outcomes that can be linked to the outcomes determined during childhood. I follow Heckman et al. (2013) and Grönqvist et al. (2020)

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[50]The effect on the number of children is also not statistically significant (results available upon request). Figure A1 shows graphical evidence that the reduced risk of early parenthood profile is U-shaped starting at age 16–17. The effect is the strongest around age 18–19, only to reverse thereafter and become positive at ages 24–25.

[51]These estimates are comparable in magnitude to the reduction of teenage motherhood found when evaluating the 1-year expansion of vocational upper secondary school programs in Sweden 1988–1990 (Grönqvist & Hall, 2013).

and decompose how much of the effects on log earnings and ever marrying that can be explained by effects on observed abilities (Panel A of Table 13) and by effects on upper secondary school completion and parental divorce (Panel B of Table 13). The final columns (9–11) in the table show the relative contribution of the mediating factors and other residual factors (normalized to 100%). Panel A shows that the effect on non-cognitive ability explains a little more than a third of the reconsideration period's effects (36%) on log earnings and ever marrying, while cognitive ability only accounts for less than half of that (13–17%). These results indicate that non-cognitive ability is a more important factor in determining the effects on long-term outcomes than cognitive ability.

Panel B decomposes the effects on upper secondary school completion and experiencing parental divorce by age 18 on log earnings and ever marrying. Upper secondary school completion explains almost a quarter (24%) of the effects on earnings, while parental divorce accounts for 11%. For ever married, the effect on education appears to account for a similar share of the effects compared to parental divorce (12–13%). These intermediate outcomes account for a lower combined share of the effects on log earnings and ever married (25–35%) than non-cognitive and cognitive ability.

5.   Discussion

The findings of this paper show that the divorce law reform of 1974 had sizable and persistent effects on children's long-term outcomes. The magnitude of the effects raises the question to which extent the effects stem from divorces, or if they are mostly driven by changes to parental behavior. In line with the arguments presented by Gruber (2004), the relatively similar effect sizes on parental divorce and on upper secondary school completion, the suggestive mechanisms, and the mediation analysis all indicate that most of the reconsideration period's effects stem from within-marriage behavior rather than through divorces.

The treatment effects in this study are similar to that of the existing literature, despite being based on widely different reforms. The increase in upper secondary school graduation rate of 1.5 pp. (1.8%) in response to the reconsideration period translates into an effect of about 0.8 pp. (7.3%) increased likelihood of university graduation.[52] The evaluation of the liberalization

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[52]However, the average effect also contains the weaker treatment effects of partially treated cohorts with few years of exposure. Exploratory analysis of even younger cohorts' outcomes shows that the effect on upper secondary school

Table 13: Mediation analysis decomposing the effects of non-cognitive and cognitive ability, upper secondary school completion and divorce by age 18 on related long-run outcomes.

Panel A	Impact on NC	Impact on Cog.	Impact on outc.	NC	Cog.	NC part (1)x(4)	Cog. part (2)x(5)	Total (3)+(6) +(7)	Share NC (6)/(8)	Share Cog. (7)/(8)	Share resid. (3)/(8)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Outcome:											
Ln earn.	0.040	0.053	0.0089	0.1689	0.0621	0.0068	0.0033	0.0190	0.36	0.17	0.47
Ever marr.	0.040	0.053	0.0084	0.1452	0.0412	0.0058	0.0022	0.0164	0.36	0.13	0.51

Panel B	Impact on US.	Impact on Div.	Impact on outc.	US.	Div.	US. part (1)x(4)	Div. part (2)x(5)	Total (3)+(6) +(7)	Share US. (6)/(8)	Share Div. (7)/(8)	Share resid. (3)/(8)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Outcome:											
Ln earn.	0.015	−0.022	0.0068	0.1766	−0.0562	0.0026	0.0012	0.0106	0.24	0.11	0.64
Ever marr.	0.015	−0.022	0.0052	0.0565	−0.0405	0.0008	0.0009	0.0069	0.12	0.13	0.75

Note: The table presents the estimates used to calculate the shares for the mediation analysis, following Grönqvist et al. (2020). "NC" denotes non-cognitive ability, "Cog." denotes cognitive ability, "US." denotes upper secondary school completion, and "Div." denotes experiencing parental divorce by age 18. Columns (1)–(2) calculate the direct impact of the reconsideration period on the mediating factors. Columns (3)–(5) estimate the impact of the factors and the reconsideration period on the outcome in a joint regression and scales the effect of NC and Cog. by the reliability ratio previously established by the literature (0.5 for NC, 0.73 for Cog.). Columns (6)–(8) sums the partial and total contribution to the effects, and columns (9)–(11) shows the share of each contributing factor.

element indicates that the divorce liberalization decreased the affected children's upper secondary school graduation rate by 5.6%. These estimates are broadly similar to previous divorce law reform evaluations (Gruber, 2004; Heggeness, 2020), although the results from the study by Heggeness are of the opposite sign.

The conflicting sign of the estimates from the previous literature indicates that the underlying mechanisms and institutional context are key in understanding the observed treatment effects. In this paper, evidence from mechanisms related to parental labor supply in 1975 points to mothers reducing their hours worked in response to the policy, while no such change can be seen for fathers. Such a change suggests increased household specialization and could signal an increase of parental investments in children. In line with this, the intergenerational correlation in educational outcomes between mothers and their children strengthened following the reform. Moreover, the beneficial effects on children's emotional stability and cognitive ability lend strength to the notion of increased parental investment and marital stability positively affecting the children's development.

Delving further into the mechanisms, the heterogeneity results for parents with above-median earnings corroborate the previous argument by showing that treated children with mothers earning above the median in 1970 were equally likely to experience divorce compared to those below the median. Contrarily, children with fathers earning above the median were less likely to experience divorce. With some speculation, one could imagine that fathers with high earnings are better able to compensate their spouses following the reform. The treatment effects on upper secondary school completion are substantially weaker when mothers are earning above the median, which indicates a weaker response from families with less pre-reform household specialization of activities. Potentially, this could be due to working mothers being less prone to shift toward parental investments.

Another key lesson from previous literature is that the effects of divorce law reform on children's outcomes are dependent on the nature of the reform, the institutional setting, and the marginal respondents targeted. With this in mind, there are some possible explanations for why the results found in this study and by Gruber (2004) differ from that of Heggeness (2020). The setting for Heggeness' study is a middle-income catholic country with strong gendered family norms which legalized divorce and simultaneously transferred bargaining power to the mothers. The subsequent

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completion reaches its peak and levels off at around 3% starting with the cohort born 1963–.

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threat of marital dissolution for negatively selected families responding to a divorce legalization may explain why this reform had strong positive effects on children's schooling outcomes. For Gruber's study, the setting is a high-income country with the marginal divorces being couples that respond to unilateral divorce. It is possible that this kind of a policy accentuates conflict by allowing for divorce without the consent of both spouses, in turn negatively affecting children's outcomes.

For this study, the specific setting is a high-income country during the 1970s and onward, with the respondents being marginal divorcees and marriages. In this case, a 6-month parental reconsideration period for divorce is shown to reduces the number of more harmful divorce shocks and improve marital investments for positively selected families within the group of marginal marriages. Such families could have greater potential to reconcile or refrain from seeking a divorce. It is also possible that the "cool-down" period actively reduces the level of conflict during and after the divorce. Thus, the gains for children are more likely to be net positive for this kind of a restriction, compared to divorce restrictions targeting more dysfunctional families. Considering the current era with reduced stigma surrounding divorce, lower marriage rates, and increased female attachment to the labor market, it is, however, plausible that the contemporary effects of the intervention may be weaker.

The overall takeaway from this study is that a policy seeking to improve parental marriage stability can have beneficial impacts on children's long-term outcomes. In other words, restricting divorce for couples close to a "break-even" marriage can, under the right circumstances, shield children from experiencing a marginal divorce and potentially increase parental investments. However, it is also crucial to weigh these policies against potential effects on parents.[53]

6. Conclusion

This study investigates the effects of divorce law reform on children's long-term outcomes. Specifically, it evaluates the effects of the Swedish divorce law reform of 1974, which consisted of a general liberalization of the existing divorce laws and 6 months of parental reconsideration for divorce. The analysis focuses on late school-age children and adolescents at the time of the reform. While

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[53]The effects on parents can be seen in Table A1, which shows that parents with greater exposure to the reconsideration period exhibit lower long-run mortality risk. Thus, I can rule out some potential trade-offs for this reform.

much of the previous evidence of the effects of divorce and divorce laws on children are plagued by endogeneity concerns, this study uses a novel identification strategy where variation in family situation and cohort exposure to the reform elements are used for identification.

Using a DiD-related specification, exploiting marriage status of parents or age spacing of siblings and cohort variation in exposure to the policy, I find substantial treatment effects on children's long-term outcomes. The divorce liberalization element of the reform appears to have decreased the relative educational outcomes of children with married parents. Evaluating the divorce restriction, children with greater exposure to this reform element were less likely to experience parental divorce and exhibited improvements in the likelihood of graduating from upper secondary school. The magnitude of the treatment effects and the analysis of mechanisms indicate that changes to within-household behavior is the primary channel behind the effects on children's outcomes.

In conclusion, this study highlights that family responses to divorce law reform can be substantial. However, it is important to bear in mind that the reform effects estimated in this paper primarily occurred during the 1970s–1990s. The main limitation of the study is that more direct measures of parental investments are needed to better understand the intricate mechanisms behind the effects of such reforms on children's outcomes. Future research should further attempt to delve into how parental behavior affects children's outcomes. The findings of this paper provide evidence of trade-offs between freedom of choice for parents seeking divorce and externalities imposed on third parties, particularly children. Consequently, policy makers should consider the broader effects of divorce law reform on children when formulating policies related to marriage stability.

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Appendix A: Additional figures and tables
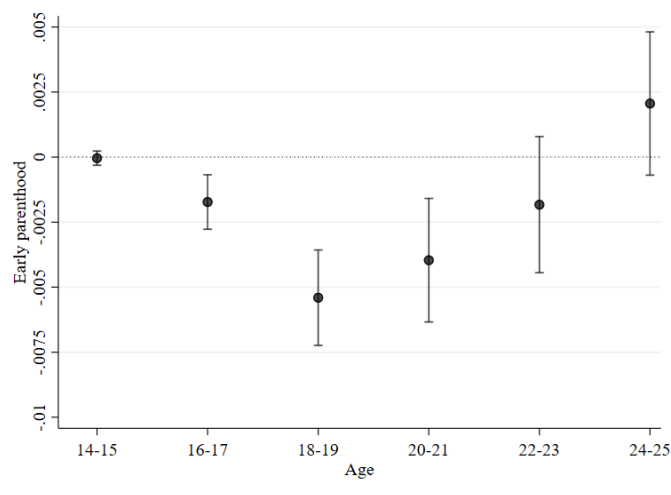
Figures

Figure A1: Effects of the reconsideration period on early parenthood, by age of becoming a parent.
The figure shows effects of the reconsideration period estimated by separate regressions. The controls are the same as those specified in Figure 6. CI95 are indicated in black, and standard errors are clustered at the household level.
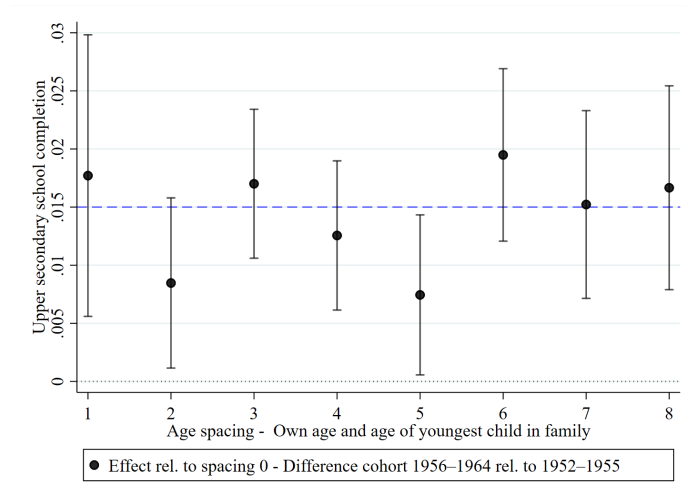

Figure A2: Treatment effects split by age spacing.
The figures split the effects of the reconsideration period on upper secondary school completion by each spacing category in relation to the effects on youngest siblings (spacing 0) from a joint regression. The dashed blue line indicates the baseline effect estimated when pooling the categories. The controls are the same as those specified in Figure 6. CI95 are indicated in black, and standard errors are clustered at the household level.
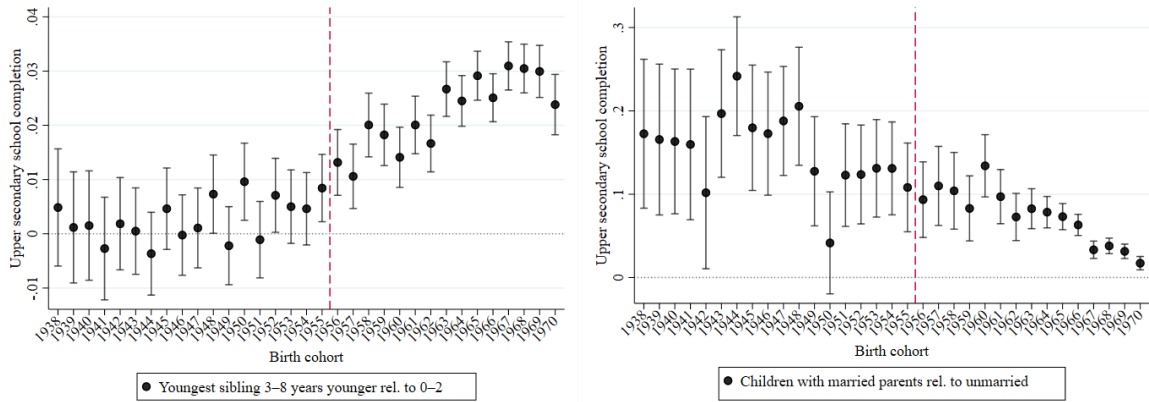
(a) Difference in upper secondary school compl. rate, youngest sib. 3–8 years younger v. 0–2.

(b) Difference in upper secondary school completion rate, married v. unmarried parents.

Figure A3: Extended pre- and post-trends for the upper secondary school completion outcomes.
Panel A3a extends the existing pre- and post-trends of the divorce restriction element and disaggregates the main estimate on experiencing upper secondary school completion by following cohorts born 1938–1970 and estimating the difference in upper secondary school completion rate between the large age spacing group (3–8) against the small age spacing group (0–2). Panel A3b does the same for the evaluation of the divorce liberalization element, thus estimating the difference in outcome between children with married and unmarried parents in 1970. The controls include parents' birth cohort, labor earnings in 1970, and sex of the child. CI95 are indicated in black, and standard errors are clustered at the household level.
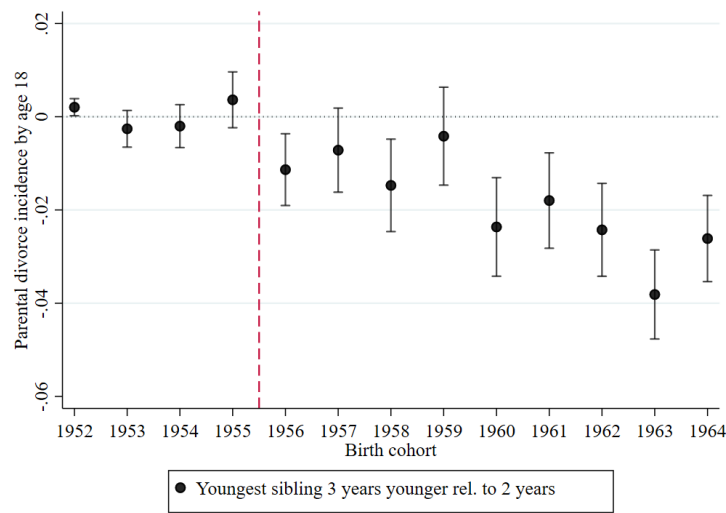

Figure A4: The effects of the reconsideration period on parental divorce for spacing 3 v. 2 years.
The figure shows the effect of the reconsideration period on experiencing parental divorce for very tight bounds of age spacing by following cohorts born 1952–1964 until age 18 separately by birth cohort and estimates the difference in parental divorce rate between those with 3 years of age spacing to their youngest sibling against the small age spacing group with 2 years of spacing. The controls are the same as those specified in Table 3. CI95 are indicated in black, and standard errors are clustered at the household level.
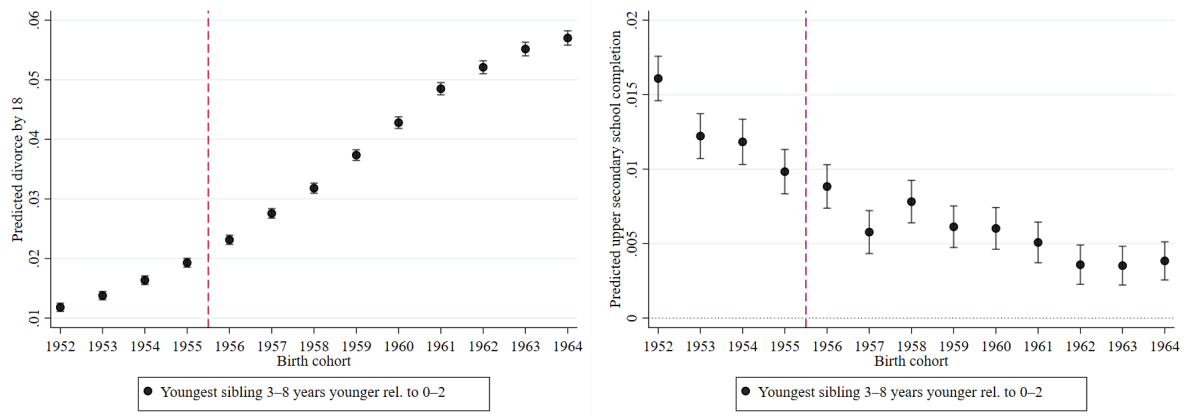
(a) Difference in predicted parental divorce rate by age 18, youngest sibling 3–8 years younger relative to 0–2 years younger.

(b) Difference in predicted upper secondary school completion rate, youngest sibling 3–8 years younger relative to 0–2 years younger.

Figure A5: Predicted outcomes for the divorce restriction element using parental characteristics. Panel A5a uses predicted parental divorce by 18 outcomes from parental and child characteristics in 1970, with the caveat that some characteristics risk being imbalanced due to direct effects of child age spacing. The characteristics include cohort, earnings, educational attainment, and hours worked of the parents, along with birth month, municipality of residence, and sex of the child. Panel A5b does the same when predicting the upper secondary school outcomes. CI95 are indicated in black, and standard errors are clustered at the household level.
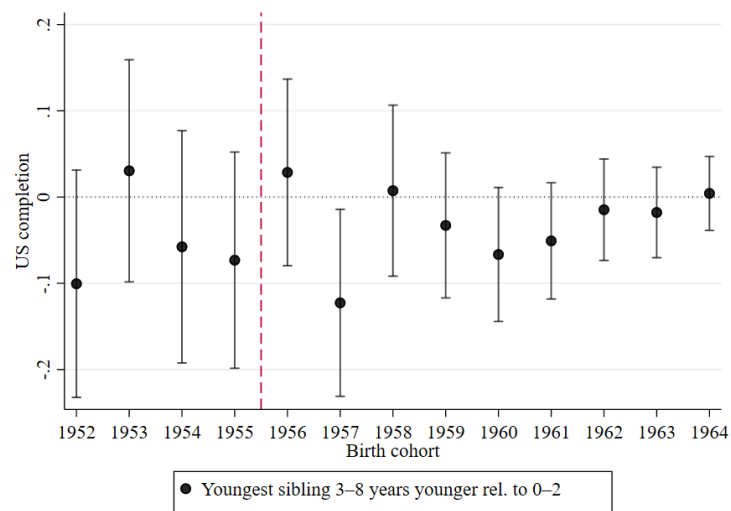

Figure A6: Placebo test on upper secondary school completion for children with unmarried parents. The figure shows a placebo test for the divorce restriction element using children with parents who are unmarried in 1970. The controls are the same as those specified in Table 3. An F-test of equality between the re-weighted outcomes 1952–1955 against 1956–1964 cannot reject the null of no difference (p-value 0.576). CI95 are indicated in black, standard errors are clustered at the household level.

Tables

Table A1: Effect of a 6-month parental reconsideration period for divorce on parental outcomes 1990–2015.

Outcome:	Fathers			Mothers		
	Earnings 1990	Marr./Cohab.	Death	Earnings	Marr./Cohab.	Death
$Insulation_i \times Cohort_i \geq 1956$	47.014***	0.021***	−0.020***	36.093***	0.023***	−0.023***
	(5.201)	(0.002)	(0.002)	(2.712)	(0.002)	(0.002)
$Insulation_i$	−6.281	−0.007***	0.004***	28.074***	−0.003	−0.014***
	(4.068)	(0.002)	(0.001)	(2.045)	(0.002)	(0.002)
Mean dep. var.	1,064.788	0.847	0.688	719.350	0.736	0.497
Obs.	553,059	541,990	680,542	630,284	620,427	688,241
Cohort & Parent cohort FE	✓	✓	✓	✓	✓	✓

Note: ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$. Standard errors in parenthesis are clustered at the household level. The outcomes are estimated using the oldest children in each family. "Marr./Cohab." refers to married or cohabiting in 1990, and "Death" refers to death by 2015. The empirical specification and the controls are the same as those specified in Table 3. "Mean dep. var." refers to mean dependent variable for the reference category.

Table A2: Robustness check: Effect of a 6-month parental reconsideration period for divorce on upper secondary school completion.

Sample: Age spacing cutoff definition: Outcome:	All children				Elder sibs.	
	0–1, 2–8	0, 1–8	0, 1–3	0–3, 4–8	2, 3–4	2, 3
	Upper secondary school completion				Upper sec. completion	
$Insulation_i \times Cohort_i \geq 1956$	0.014***	0.015***	0.014***	0.011***	0.008*	0.010**
	(0.002)	(0.002)	(0.002)	(0.002)	(0.004)	(0.005)
$Insulation_i$	−0.007***	−0.011***	−0.008***	−0.009***	0.007*	0.006
	(0.002)	(0.002)	(0.002)	(0.002)	(0.004)	(0.004)
Mean dep. var.	0.826	0.827	0.827	0.828	0.822	0.822
Obs.	1,073,396	1,073,396	722,671	1,073,396	268,962	172,644
Cohort FE	✓	✓	✓	✓	✓	✓
Parent cohort FE	✓	✓	✓	✓	✓	✓

Note: ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$. Standard errors in parenthesis are clustered at the household level. "$Insulation_i$" indicates greater age spacing than the reference group, where the cutoff varies by column. The interaction with cohort shows the difference in effect between cohort groups 1956–1964 and 1952–1955. The controls are the same as those specified in Table 3. "Mean dep. var." refers to mean dependent variable for the reference category with age spacing 0–2.

Table A3: Direct effects of experiencing divorce, family fixed effects, and excluding controls: Effect of a 6-month parental reconsideration period for divorce on upper secondary school completion.

Sample: Outcome:	Direct effect All children Upper sec. completion		Family FE All children Elder sibs. Upper sec. completion		Excl. controls All children Elder sibs. Upper sec. completion	
Divorce by 18	−0.069***	−0.028***				
	(0.002)	(0.007)				
$Insulation_i \times Cohort_i \geq 1956$			0.017***	0.013	0.018***	0.007**
			(0.004)	(0.009)	(0.002)	(0.004)
$Insulation_i$			−0.004	−0.007	−0.004**	0.006*
			(0.005)	(0.011)	(0.002)	(0.003)
Mean dep. var.	0.800	0.800	0.825	0.815	0.825	0.815
Obs.	1,200,340	1,200,340	1,073,396	549,271	1,073,396	549,271
Cohort FE	✓				✓	✓
Linear controls		✓	✓	✓		
Family FE		✓	✓	✓		

Note: ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$. Standard errors in parenthesis are clustered at the household level. "Divorce by 18" refers to experiencing parental divorce by age 18. The empirical specification and the controls are the same as those specified in Table 3. "Linear controls" replaces the indicators with linear controls under family FE. "Mean dep. var." refers to mean dependent variable for the reference category with age spacing 0–2.

Table A4: Robustness check: Effect of a 6-month parental reconsideration period for divorce on upper secondary school completion with restrictive controls and age spacing checks.

Specification:	Extensive controls			Full sp. 0–18	Rev. sp. 0–8*	1973 spacing 0–8**
Outcome:	Upper sec. completion			Upper sec. completion		
$Insulation_i \times Cohort_i \geq 1956$	0.008***	0.006***	0.007***	0.023***	0.016***	0.016***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
$Insulation_i$	-0.008***	0.014***	0.012***	-0.018***	-0.007***	-0.008***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Mean dep. var.	0.825	0.825	0.825	0.825	0.825	0.819
Obs.	1,073,396	1,073,396	1,073,396	1,195,055	1,036,637	1,096,878
Birth order FE	✓	✓	✓			
Linear age spacing control		✓	✓			
# siblings FE			✓			
Cohort & Parent cohort FE	✓	✓	✓	✓	✓	✓

Note: ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$. Standard errors in parenthesis are clustered at the household level. "Extensive controls" refers to adding potentially "bad controls" which strongly correlate with the age spacing groups used to capture the effects of the divorce restriction. "Full sp." refers to including children with age spacing 9–18 in the insulation group with greater age spacing. *"Rev. sp." removes the children where a new sibling born 1971–1973 moves them into an age spacing > 8 years. "1973 spacing" assigns age spacing at year 1973. "$Insulation_i$" indicates greater age spacing (3–8 years to youngest sibling against 0–2 years). The interaction with cohort shows the difference in effect between cohort groups. "Mean dep. var." refers to mean dependent variable for the reference category.

Table A5: Effect of a 6-month parental reconsideration period for divorce on elder siblings' university and labor market outcomes 1990.

Outcome:	University graduation	Earnings	Log earnings	Employment
$Insulation_i \times Cohort_i \geq 1956$	0.008***	16.196***	0.023***	0.006***
	(0.003)	(5.738)	(0.006)	(0.002)
$Insulation_i$	−0.003	5.105	−0.001	0.001
	(0.002)	(5.194)	(0.005)	(0.002)
Mean dep. var.	0.108	1,296.428	7.016	0.889
Obs.	549,271	582,428	565,175	582,427
Cohort FE	✓	✓	✓	✓
Parent cohort FE	✓	✓	✓	✓

Note: ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$. Standard errors in parenthesis are clustered at the household level. "Univ. grad." refers to three years or more of university education in 1990. Earnings and employment outcomes are for the same year. The empirical specification and the controls are the same as those specified in Table 3. "Mean dep. var." refers to mean dependent variable for the reference category with age spacing 1–2.

Table A6: Effect of a 6-month parental reconsideration period for divorce on elder siblings' family outcomes 1990 & 2000.

Outcome:	Year 2000		Census 1990			
	Ever married	Ever divorced	Single parent	Marr./Cohab.	Cohabiting	Parent
$Insulation_i \times Cohort_i \geq 1956$	0.001	−0.003	−0.002	0.007*	0.011***	−0.001
	(0.004)	(0.003)	(0.003)	(0.004)	(0.003)	(0.004)
$Insulation_i$	0.016***	−0.000	−0.004	0.013***	−0.006**	0.009**
	(0.003)	(0.003)	(0.002)	(0.003)	(0.003)	(0.003)
Mean dep. var.	0.590	0.142	0.130	0.631	0.266	0.457
Obs.	575,114	575,114	546,831	546,831	546,831	601,711
Cohort FE	✓	✓	✓	✓	✓	✓
Parent cohort FE	✓	✓	✓	✓	✓	✓

Note: ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$. Standard errors in parenthesis are clustered at the household level. "Ever married" and "Ever divorced" refers to ever marrying or divorcing by year 2000. "Single parent" is defined through the census in 1990, "Marr./Cohab." is defined as cohabiting or being married, "Cohabiting" is defined as cohabiting without being married, and "Parent" is defined as having a child age 0–6 at the same year. The empirical specification and the controls are the same as those specified in Table 3. "Mean dep. var." refers to mean dependent variable for the reference category with age spacing 1–2.
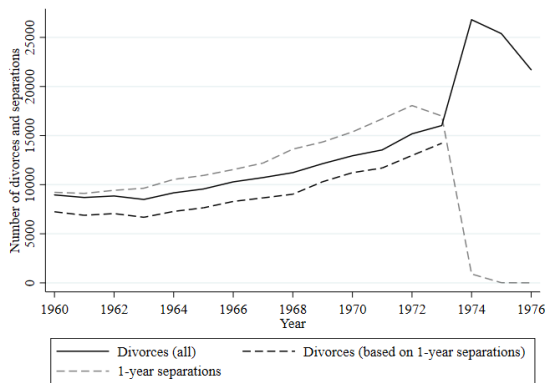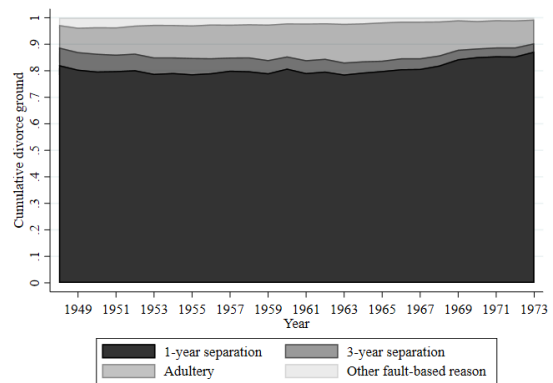
Appendix B: Supporting figures
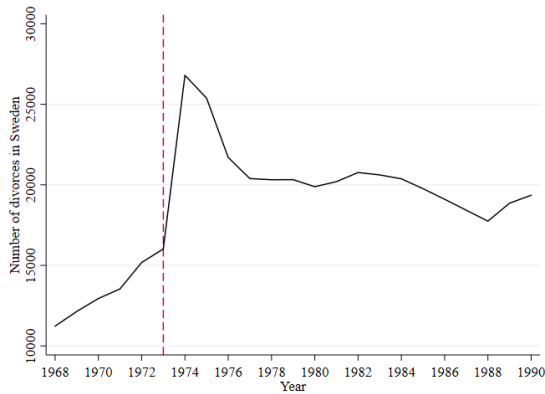
(a) Number of divorces in Sweden 1968–1990.
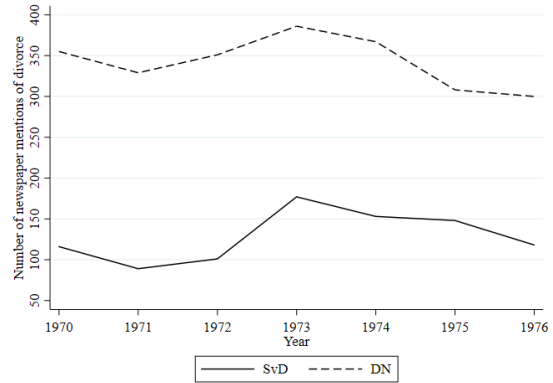
(b) Number of mentions of "divorce" in the leading morning newspapers in Sweden 1970–1976.

(c) Divorces and 1-year separations 1960–1976.

(d) Cumulative divorce reason shares 1948–1973.

Figure B1: Divorces and separations over time, newspaper word search, and divorce reasons.
Panel B1a shows the number of divorces in Sweden around the time of the divorce law reform in 1974. Panel B1b shows number of mentions of the word "divorce" in the two largest morning newspapers around the time of the reform from the historical archives of the newspapers DN and SvD. Panel B1c shows a stable relationship between 1-year separations and finalized divorces, and that the number of separations broke the trend in 1973 and sharply decreased following the new divorce policy in 1974. Excess separations could either revert back into marriage, or allow spouses to live financially separate lives while remaining legally married. The transition rules in place from 1974 allowed for courts to grant separations until 30 June 1975 if the application was submitted before 1 Jan 1974. Panel B1d category "Other fault-based reason" includes abuse, substance addiction, prison sentence for at least three years, insanity for at least three years with no hope of recovery, desertion, and infecting partner with a venereal disease. The new divorce law in 1974 removed all fault-based reasons.
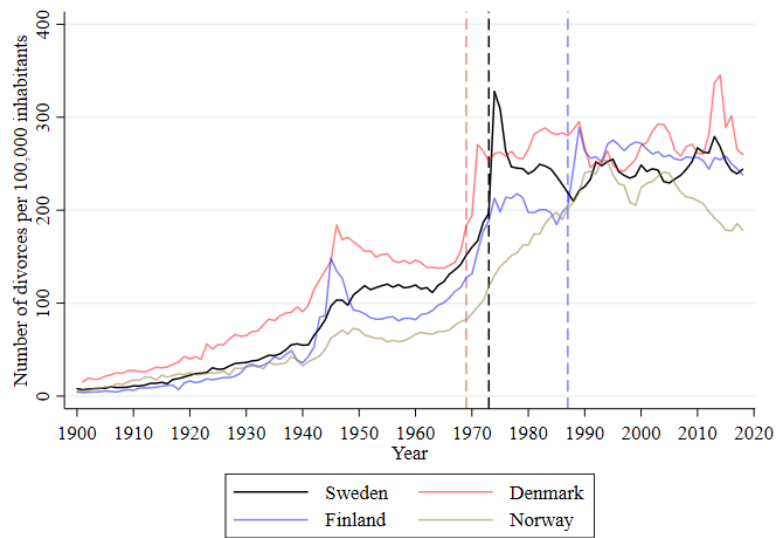
Figure B2: Number of divorces per 100,000 in Sweden, Denmark, Finland, and Norway over time. The vertical dashed lines mark the year before a divorce law liberalization in each country. Source: Statistics Sweden, Statistics Denmark, Statistics Finland, Statistics Norway.

Appendix B: Supporting information and results

Additional information on divorce laws in Sweden 1915–1973

Marriage counselling, which was mandatory for couples seeking a divorce during 1915–1973, was provided by the municipality and performed by a priest or a public counsellor. If the marriage was deemed to be beyond salvaging after the counselling, the spouses were granted a note valid for three months certifying that they had participated and were allowed to file for divorce. The spouses were supposed to live apart and support themselves financially during the separation period. Under disputes over alimony or other issues, the legal process of being granted a 1-year separation could be lengthy. Anecdotal evidence from a counsellor stated that it is often the case that one spouse reluctantly agrees to divorce, and that 80% of the mediation attempts were followed by a separation application (Svensk Tidskrift, 1952).

All separations did not result in divorce, as some couples reverted back to married life or simply chose to remain de facto separated without finalizing the divorce. The government bill from 1973 looking into this acknowledged that separations not leading to divorce may be due to some couples choosing to stay legally married while remaining separated. It was also stated in the bill that the vast majority of separations not being realized as divorces were due to the couple resuming the marriage, hinting at the potential stabilizing effect of divorce restrictions on marriages (SOU 1975:24, 1975). The number of divorces (based on 1-year separations) always exceeded the number of separations the previous year. The share of divorces to separations was roughly constant around 80–90% during 1960–1973 (see Panel B1c). Reports from the public investigation of 1972 on the ensuing divorce law reform indicate that 25% of all 1-year separations taking place did not result in divorce (SOU 1972:41, 1972). If the spouses did not finalize the divorce following the 1-year separation period, the separation appears to have continued indefinitely regardless of the couple resuming married life or not.

The divorce laws remained stable during the entire time period before the reform in 1974 with one exception. From July 1, 1969, the divorce laws were revised to allow the year-long separation period to also be granted based on unilateral divorce applications. The divorce law revision in 1969 also made it harder to divorce based on adultery (Inger, 2011). This change did not coincide with

any clear change in 1-year separations or divorces (see Panel B1d).

Additional information on the divorce law reform in 1974

The first step toward the divorce law reform was taken a few years prior to 1974 through a public policy report aimed at modernizing the divorce laws. The report was ordered by the government in 1969, and then presented to the parliament in 1972 (SOU 1972:41, 1972). The reform was then passed by the parliament in early 1973, and enacted January 1, 1974 (SOU 1975:24, 1975). The media coverage of the reform appears to have been extensive, with several front-page articles on the subject published by the leading morning newspapers during the years before the reform. The coverage increased substantially in 1972–1973 as extensive, front-page articles were published when the public investigation was presented to the parliament.[54] Panel B1c shows that the positive time trend in 1-year separations is reversed starting in 1973, indicating that this is the year when the new policy became evident for the general public.[55]

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[54]See Panel B1b for a count of articles containing the word "divorce" in the leading morning newspapers around the time of the reform. An example of a headline from the leading morning newspapers *Dagens Nyheter* (DN) and *Svenska Dagbladet* (SvD) on June 7, 1972 on the new divorce law translates roughly into "Maybe more will dare to marry now". On March 8, 1972, SvD published a front-page article on "Express divorces" prompted by leaked information from the upcoming public investigation.

[55]Anecdotal evidence from a public investigation in 1975 indicates that legal counsellors encouraged divorcing spouses in 1973 to postpone the divorce process until after the new year when the couple would face an easier divorce process (SOU 1975:24, 1975).

Theoretical framework

Theoretical research: Divorce and family behavior. Research in economics has long sought to model and explain family behavior and responses to policy. The first wave of theoretical research modeling family behavior and parental investments consists of unitary models, where households maximize a joint utility function subject to a budget constraint (Samuelson, 1956; Becker, 1981). Extensions to this work adds the component of within-household bargaining between spouses over the marital surplus, and highlight the outside option of spouses as key in affecting marital stability. Under full transferable utility within the marriage and excluding any other frictions, the Becker-Coase theorem guarantees that all divorces are efficient, and implies that divorce laws only affect the distribution of the marital surplus (Becker, 1973). However, this only holds under restrictive assumptions about preferences and public marital goods (Chiappori et al., 2015).

The second wave of research instead highlights within-household bargaining over common resources as central to family behavior, and this work paves the way for more realistic models of the family (e.g. Manser & Brown, 1980; McElroy & Horney, 1981; Lundberg & Pollak, 1993). Such a framework can be seen in Chiappori et al. (2002), which stresses that that divorce laws affect marriages by re-weighting the bargaining strength of spouses. With imperfect transferable utility, divorce laws can affect the steady state divorce rate, as some spouses are unable to use their marital surplus to compensate the dissatisfied spouse seeking a divorce. These findings highlight that divorce laws affect spouses within marriage as well as through divorces.

In more recent work, theoretical researchers have concentrated on the role of external policies, such as prenuptial contracts and the effects of introducing unilateral divorce in the U.S. on marital instability and investments within the marriage. According to these frameworks, the non-neutrality of divorce law liberalizations on marriages and the risk of experiencing divorce leads to lower marital investment and less specialization within the marriage (Anderberg et al., 2016; Reynoso, 2017). All in all, the lessons from this is that the effects of divorce law reform are likely dependent on the nature of the change taking place, factors related to the institutional setting, and the type of family affected by the reform.

The setting. The following theoretical framework is used to characterize the effects of a divorce restriction on parental investments in the marriage and labor supply decisions. The end goal is to help explain how divorce restrictions can prevent divorces in the long run, and also how parental investments in children are affected by such a restriction. The framework focuses on the decisions of the two individuals that make up the household. The main friction is the risk of future divorce, which leads to lower marriage investments than what is optimal had the future been fully deterministic. In short, a divorce restriction works by preventing divorces that would not be realized in the long run by allowing some marriages to revert back to a positive value. Also, the restriction positively affects marriage investments by reducing ex ante divorce risk and thus affects children positively. The setup and solution concepts of the framework are highly related to previous work, albeit adapted to this specific setting (Rainer, 2007; Anderberg et al., 2016).

The framework. Formally, the framework consists of two agents (husband h_i and wife w_i of family i) who are exogenously matched to each other and live for two time periods ($t = \{1, 2\}$). The first period symbolizes the early years of marriage with marital investments, family formation, and career development, while the second period captures the remainder of the time when the children are older. In the first period, $t = 1$, the wife chooses to invest in an intermediate marriage good (g_i, e.g. home production and children) with the price normalized to unity, which is carried forward into the next period. Investments in the marriage good g_i are assumed to be beneficial for the children and improve their long-term outcomes. The marriage good is then used as input in the production function $V(g_i)$, a strictly increasing, concave function ($V'(g_i) > 0$, $V''(g_i) < 0$) where the non-rivalrous output is enjoyed equally by both spouses during marriage. The husband and wife also invest in a private good (p_i^w and p_i^h, e.g. personal career and private contacts) which determines the private investment values for both the first and second period ($p_{i,1} = p_{i,2} = p_i$).

Investment allocations for the wife $\bar{p}_i^w = p_i^w + g_i$ are constrained by maximum private investments $\bar{p}_i^w$. This means that the wife faces a trade-off between marriage-specific investments and private investments. Husbands fully use their endowments for private investments $\bar{p}_i^h = p_i^h$.[56] The surplus

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[56]A more refined model could add investment decisions into the marriage good for husbands as well, but abstracting away from this simplifies the model somewhat and provides the same qualitative results as a model including investments from the father. This model is also likely a better fit when matching the conditions in the 1970s, given that the vast majority of the parental leave taken out in the 1970s were by the mother. From this, it is reasonable to

from private investments are enjoyed within the marriage according to a sharing rule $\mu \in [0, 1]$, where the share μ goes to the wife, and thus $1 - \mu$ is the husband's share.[57] Time period 1 actions of the husband and wife imply that the gains from marriage at this point in time are defined as:

$$u_{i,1}^w = \mu(p_i^h + p_i^w) + V(g_i) \equiv m_i^w$$
$$u_{i,1}^h = (1 - \mu)(p_i^h + p_i^w) + V(g_i) \equiv m_i^h$$

Divorce can be taken out unilaterally at any point in time, meaning that divorce is instigated as soon as the marriage value is less than the outside option defined below. In case of divorce, the marital investments turn into a divisible good which is split between the spouses by the share $\gamma \in [0, 1]$, which represents the reduced value of the joint marital good following the union's breakdown. The wife receives γ of the output, while the husband gets the remainder $1 - \gamma$. In order to guarantee participation and no divorces in period 1, I assume that the participation constraints $m_i^w > p_i^w + \gamma V(g_i)$ and $m_i^h > p_i^h + (1 - \gamma)V(g_i)$ are met. In other words that the gains from marriage are greater than the outside option for both spouses.

In period 2, the spouses are subject to an information shock $\varepsilon \sim F(\cdot)$ with support $(-\infty, \infty)$, which may drive the marriage value into the negative domain and incentivize divorce. Ex ante the spouses have no expectation of the sign of the shock ($\mathbb{E}[\varepsilon] = 0$), and it is assumed to affect both spouses in the same way once it is realized. For couples with a positive information shock, it simply increases the marriage value and causes no new actions. A novel feature of this model is that the shock to the marriage value consists of two uncorrelated components ν and δ (where $\varepsilon = \nu + \delta$, $Cov(\nu, \delta) = 0$). Just like the composite term (ε_i), the shocks are mean zero ex ante ($\mathbb{E}[\nu] = \mathbb{E}[\delta] = 0$). The first shock ($\nu$) is observable directly going into period 2, and the second shock (δ) is realized ex post during this period. The nature of these two shocks means that some marriages will have a perceived negative marriage value when observing the first shock's value, only that ex post observing the second shock would have reverted the marriage value back into the positive domain. Likewise, a marriage may be revealed over time be be of negative value as the second shock (δ) is realized, prompting a later divorce. The key part is that δ is never realized if

the divorce happens at the start of period 2 when the first shock ν is observed.

Period 2 starts with the first shock ν affecting the marriage value of the spouses. If the spouses choose to remain married, they are subject to the second information shock δ and again decide whether to remain married. By remaining married throughout the period they gain the marriage value and reap the benefits of the previous marriage investment. The gains for the husband and wife when remaining married in the last time period is defined as:

$$u_{i,2}^{h,m} = (1 - \mu)(p_i^h + p_i^w) + V(g_i) + \varepsilon_i$$
$$u_{i,2}^{w,m} = \mu(p_i^h + p_i^w) + V(g_i) + \varepsilon_i$$

And under divorce:

$$u_{i,2}^{h,d} = p_i^h + (1 - \gamma)V(g_i)$$
$$u_{i,2}^{w,d} = p_i^w + \gamma V(g_i)$$

Meaning that the divorce takes place if $u_{i,2}^{w,d} > u_{i,2}^{w,m}$ or $u_{i,2}^{h,d} > u_{i,2}^{h,m}$. The divorce decision $(D_i = \{0, 1\})$ at the start of period 2, when only the first shock ν_i has been realized, thus satisfies the following:

$$D_i = \begin{cases} 0 & \text{if } m_i^w + \nu_i + \mathbb{E}[\delta_i] \geq p_i^w + \gamma V(g_i) \\ & \text{and } m_i^h + \nu_i + \mathbb{E}[\delta_i] \geq p_i^h + (1 - \gamma)V(g_i) \\ 1 & \text{otherwise} \end{cases}$$

By definition a divorce takes place if the expected value of divorcing exceeds that of remaining married for either party. Substituting the marriage value, the expected value of the second information shock and rearranging, this can be simplified into:

$$D_i = \begin{cases} 0 & \text{if } \mu p_i^h + (1 - \gamma)V(g_i) + \nu_i \geq (1 - \mu)p_i^w \\ & \text{and } (1 - \mu)p_i^w + \gamma V(g_i) + \nu_i \geq \mu p_i^h \\ 1 & \text{otherwise} \end{cases}$$

Meaning that divorces are realized if the gains from the marriage after observing the first part

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of the information shock is greater than the the private investment shared with their partner. Since the only real decision in the model stems from the marital investments of the wife, the focus can be on her decision. Substituting the private investments, I write the expression for the threshold value of the information shock ν_i which leads the wife to instigate divorce as a function of the marital investments made:

$$\hat{\nu}_i(g_i) \equiv (1 - \mu)(\bar{p}_i^w - g_i) - (1 - \gamma)V(g_i) - \mu\bar{p}_i^h$$

Which is clearly a decreasing function of g_i. The same threshold holds for the composite information shock $\varepsilon_i = \nu_i + \delta_i$. From this, I can write the probability of divorce for couple i during period 2 as $F(\hat{\nu}_i(g_i))$, meaning that divorce risk decreases with the marital investments taking place in the first period. Analogously, the probability of remaining married is $[1 - F(\hat{\nu}_i(g_i))]$. Looking at the choices in period 1, the utility at that time is determined by the investment decision of the wife:

$$u_1^w = \mu(\bar{p}_i^h + \bar{p}_i^w - g_i) + V(g_i)$$
$$u_1^h = (1 - \mu)(\bar{p}_i^h + \bar{p}_i^w - g_i) + V(g_i)$$

When the investment decision is made to maximize the intertemporal utility, I get the following value function W_i^w for the wife:

$$W_i^w = u_1^w + \mathbb{E}_{\nu,\delta}[u_{i,2}^{w,m}|\nu_i > \hat{\nu}_i, \varepsilon_i > \hat{\nu}_i](1 - F(\hat{\nu}_i))^2 + u_{i,2}^{w,d}(1 - F(\hat{\nu}_i))F(\hat{\nu}_i) + u_{i,2}^{w,d}F(\hat{\nu}_i)$$

The intuition underlying this value function is that it combines the wife's utility from the first period with the expected value of the wife's utility in the second period. For the second period, the wife's utilities are weighted by the probability to remain married throughout the time period $(1 - F(\hat{\nu}_i))^2$, divorce following the first information shock $F(\hat{\nu}_i(g_i))$, or divorce after the second shock $(1 - F(\hat{\nu}_i))F(\hat{\nu}_i)$. The expected utility of remaining married is conditional on both δ_i and ε_i to be greater than the cutoff value $\hat{\nu}_i$, which means that the value of the shocks are only experienced given that they do not lead to a divorce. This value function can be used to solve for the optimal marital investments:

$$W_i^w = u_1^w + m_i^w(1 - F(\hat{\nu}_i))^2 + \left(\int_{\hat{\nu}_i}^{\infty} \nu f(\nu) d\nu + \int_{\hat{\nu}_i}^{\infty} \delta f(\delta) d\delta \right) (1 - F(\hat{\nu}_i)) + u_{i,2}^{w,d}(2 - F(\hat{\nu}_i))F(\hat{\nu}_i)$$

$$\frac{\partial W_i^w}{\partial g_i} = u_i^{w\prime}(g_i) + m_i^{w\prime}(g_i)(1 - F(\hat{\nu}_i))^2 - 2m_i^w f(\hat{\nu}_i)\hat{\nu}_i{}'(g_i)(1 - F(\hat{\nu}_i)) + u_{i,2}^{w,d\prime}(g_i)(2 - F(\hat{\nu}_i))F(\hat{\nu}_i)$$

$$+ 2u_{i,2}^{w,d} f(\hat{\nu}_i)\hat{\nu}_i{}'(g_i)(1 - F(\hat{\nu}_i)) - \mathbb{E}_{\nu,\delta}[\varepsilon_i | \nu_i > \hat{\nu}_i, \varepsilon_i > \hat{\nu}_i] f(\hat{\nu}_i)\hat{\nu}_i{}'(g_i)(1 - F(\hat{\nu}_i))$$

$$- 2\hat{\nu}_i(g_i) f(\hat{\nu}_i)\hat{\nu}_i{}'(g_i)(1 - F(\hat{\nu}_i)) = 0$$

Substituting $\hat{\nu}_i$, rearranging in terms of costs and benefits and noting that the values $\hat{\nu}_i < 0$ and $\hat{\nu}_i'(g_i) < 0$, the optimal marital investments $\hat{g}_i$ satisfies the following:

$$\underbrace{V'(\hat{g}_i)(1 + (1 - F(\hat{\nu}_i))^2)}_{\text{Benefits of marriage investment}} - \underbrace{\mathbb{E}_{\nu,\delta}[\varepsilon_i | \nu_i > \hat{\nu}_i, \varepsilon_i > \hat{\nu}_i] f(\hat{\nu}_i)\hat{\nu}_i{}'(\hat{g}_i)(1 - F(\hat{\nu}_i))}_{\text{Greater chance of experiencing the information shock}}$$

$$= \underbrace{\mu(1 + (1 - F(\hat{\nu}_i))^2)}_{\text{Cost of investment}} - \underbrace{u_{i,2}^{w,d\prime}(\hat{g}_i)(2 - F(\hat{\nu}_i))F(\hat{\nu}_i)}_{\text{Greater loss under divorce}}$$

Meaning that the optimal investments $\hat{g}_i$ balances the gains when remaining married to the losses under divorce, internalizing that the risk of divorce decreases with marital investments.

At this point, it is informative to ascertain how divorce risk affects optimal investments. Setting divorce risk to its extreme values 0 and 1, I get the following results:

$$F(\hat{\nu}_i) = 0 \Rightarrow \quad V'(\bar{g}_i) = \mu$$

$$F(\hat{\nu}_i) = 1 \Rightarrow \quad V'(\tilde{g}_i) = \frac{1 + \mu}{1 + \gamma}$$

Given the range of values for γ and μ, it is clear that $V'(\bar{g}_i) \leq V'(\tilde{g}_i)$, meaning that divorce risk weakly decreases investments in the marriage good. By extension, private investment for the wife are weakly smaller under lower divorce risk. Intuitively, what happens is that wives respond to the risk of divorce later in life during period 1 and decreases their marriage good investments to hedge the bet against future divorce. In the end, the optimal investment choice is determined by the perceived risk of divorce, bargaining within marriage, and the distaste parameter for divorce.

A few things can be learned from the model setup. The first information shock ν_i will lead to some impetuous divorces happening due to couples not remaining in the marriage until the second information shock (δ_i) is realized. With the marriage ending at the start of period 2, the remaining

information is never realized as the marriage has ended. Contrarily, some spouses remaining married in period 2 will divorce during this period when the second information shock δ_i is realized. From the wife's point of view, the optimal threshold for divorce, and thus divorce risk, increases with a spouse's bargaining position $(1 - \mu)$, with lower spousal investments $(\bar{p}_i^h)$ and higher own investment capabilities $(\bar{p}_i^w)$, with a high degree of capture of the joint marital investments following divorce (γ), and lower own marital investments (g_i). The risk of divorce causes women to reduce gainful investments in the marriage good due to them insuring against divorce with private investments. A condensed timeline of the model can be seen in Figure B1.

A final feature of the model is the introduction of a waiting period for divorce, in line with the divorce restriction introduced 1974 in Sweden. This is modelled as a constant friction component c imposed on all divorcing couples, regardless of their marriage value. The added friction changes the optimal divorce threshold to:

$$\hat{\nu}_i(g_i) \equiv (1 - \mu)(\bar{p}_i^w - g_i) - (1 - \gamma)V(g_i) - \mu\bar{p}_i^h - c$$

Which means that the threshold is lower than before, reducing the risk of divorce. The friction can be interpreted as an emotional or monetary friction associated with the waiting period for divorce which lowers the opportunity cost of marriage by reducing the value of the outside option. The direct effect of the friction means that fewer spouses are prone to take out a divorce at any point in time given the increased cost of doing so. In line with the previous results, this means that the friction also affects marital investments positively, to the benefit of the children. Another effect of the restriction is that and more couples wait to observe the realization of the second information shock due to the change of the optimal divorce threshold. Since only spouses with a sufficiently negative expected value of remaining married will pay the cost c as they seek a divorce, this friction will only change the long-term divorce decision outcome for the couples where the second information shock δ_i is positive and sufficiently large to push the value back above the divorce threshold. Although the friction c will hurt the welfare of divorcing spouses and those on the verge of divorcing, it will reduce number of "break-even" divorces and push some spouses to re-evaluate their decision to after the full information value is realized. The restriction thus also

acts as a deterrent to impetuous divorces and divorces in general.[58]
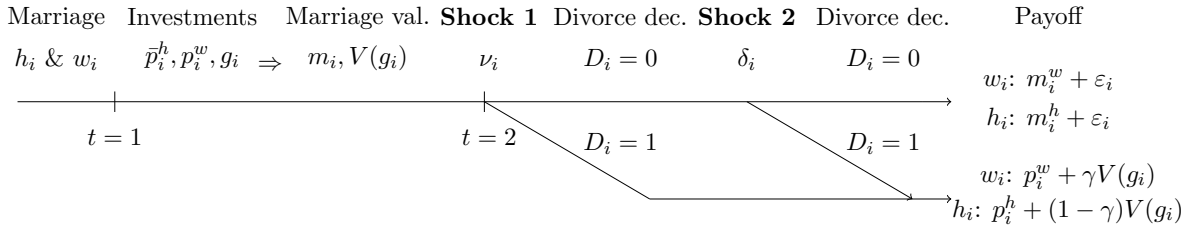
Framework timeline

Events/actions:

Marriage	Investments	Marriage val.	**Shock 1**	Divorce dec.	**Shock 2**	Divorce dec.	Payoff
h_i & w_i	$\bar{p}_i^h, p_i^w, g_i \Rightarrow$	$m_i, V(g_i)$	ν_i	$D_i = 0$	δ_i	$D_i = 0$	

$t=1$ $\quad$ $t=2$ $\quad$ $D_i = 1$ $\quad\quad$ $D_i = 1$

w_i: $m_i^w + \varepsilon_i$
h_i: $m_i^h + \varepsilon_i$

w_i: $p_i^w + \gamma V(g_i)$
h_i: $p_i^h + (1-\gamma)V(g_i)$

Figure B1: Timeline of the divorce framework.
Husband (h_i) and wife (w_i) choose private investments (p_i) in period 1. Besides private investments, the wife also chooses to allocate resources to the joint marriage good g_i. Going into period 2, the marriage value m_i is hit by a preference shock ν_i, after which the spouses can choose to divorce or not. If they survive the first shock, the spouses are then hit by a second shock δ_i after which they can again choose to take out a divorce or not. Should they survive both shocks, the spouses split the excess marriage value based on the marriage being intact. In case of divorce, they get the payoffs associated with that state.
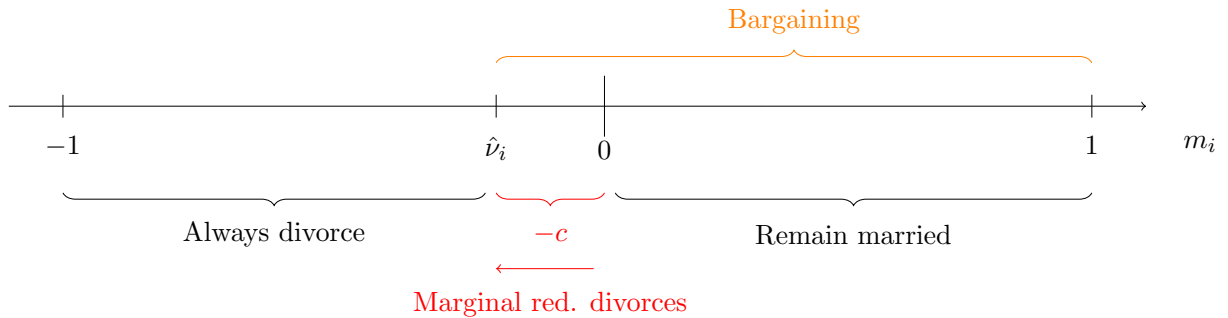

Figure B2: Marginal respondents to the reconsideration period.
The figure characterizes divorce responses to the realized marriage value under a divorce restriction. Realized marriage value m_i is shown on the unit interval relative to the normalized outside option (0). The divorce restriction c changes a spouse's optimal cutoff for divorce to $\hat{\nu}_i$, which is the cutoff value in order for the marriage value shock ν_i to induce divorce ($m_i \in [-1, \hat{\nu}_i)$). Marginal reduction in divorces ($m_i \in [\hat{\nu}_i, 0]$) affected by divorce restrictions are those with a relatively high marriage value closer to the outside option in comparison to the average divorce, and constitute marriages that would have divorced without the restrictions. Simultaneously, bargaining and within-household dynamics change for couples remaining married ($m_i \in [\hat{\nu}_i, 1]$), which in turn affects children related to the household.

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[58]See Figure B2 for an illustration of marginal divorces and marriage quality affected by the restriction.