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

Edvin Hertegård*

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Abstract

The Swedish divorce law reform of 1974 **i)** liberalized the existing divorce laws and **ii)** simultaneously introduced a 6-month parental reconsideration period for divorce. This study exploits variation in exposure to the two reform elements and population-wide register data of 1.17 million Swedish children born 1952–1964 to evaluate the reform's effects on children's long-term educational and social outcomes. The evaluation of the divorce liberalization element suggests that it decreases children's chances of graduating from upper secondary school by 5.6%. The evaluation of the divorce restriction element shows that the reconsideration period decreases the risk of experiencing parental divorce during childhood by 18%. The exposed children also exhibit a 1.8% greater chance of graduating from upper secondary school, do better on the labor market, and have better marriage market outcomes themselves as adults. Changes to within-marriage behavior of parents is found to be the prime candidate causing the observed effects. The findings highlight the potential trade-off between restricting parental choice and the beneficial effects of such policies, and also the potential negative effects of divorce law liberalization on children's outcomes.

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1. Introduction

Marriage is an institution which allows for individuals to share risk over the life-cycle, specialize in activities, and facilitate stable joint investments. From the 1960s and onward the stability aspects of marriage have lessened, with the share of marriages ending in divorce having increased dramatically in most Western countries. In response, sociologists labeled this time period the "divorce revolution" (Weitzman, 1985).[1] The increased marital instability is clearly visible in aggregate family statistics; 27% of U.S. children live with a single parent as of 2018. For Swedish children, the same number is 21% (OECD, 2018).[2]

The rising divorce rate during the previous century coincided with a general trend of divorce law reform. While the first wave of reforms starting in the late 1960s tended to be unidirectional in liberalizing the divorce process, most countries have retained some kind of a trial separation period before a couple is allowed to legally divorce. The main reason for doing so is to protect spouses and children from the effects of impetuous marital dissolution.[3]

There is a clear link between marriage status and parental time investments, financial resources, and social stigma (Gerstel, 1987). Given this, marriage instability during key years of the child's human capital formation could have substantial effects on children and their right to equality of opportunity (Heckman, 2011).[4] Numerous studies report negative correlations between parental divorce and children's outcomes later in life (Amato, 2010; Ahrons, 2007; Bhrolchain, 2001).[5] Whether these correlations reflect causal effects still remains an open question. Relating to this, other studies have presented evidence that divorce laws affect family behavior and children's outcomes (Fernández & Wong, 2014; Gruber, 2004; Heggeness, 2020; Stevenson & Wolfers, 2006). As such, divorce law reform bring about a fundamental trade-off between spousal freedom of choice and potential externalities. Estimating the effect of divorce laws on

[1] Crude divorce rates over time for Western countries (current number in 2016): UK - 180 divorces per 100,000 people in 2016, up from 70 in 1965; Germany - 190, up from 110; Sweden - 244, up from 160; U.S. - 300, down from 350 in 1970; Spain - 210, up from 30 in 1981; Italy - 150, up from 30 in 1971 (OECD, 2018).

[2] Also, it is estimated that roughly 50% of U.S. children experience parental divorce during childhood (Lansford, 2009). In Sweden, 30% of the children born in the year 2000 experienced separation by age 18 (Statistics Sweden, 2018).

[3] 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. Most U.S states have mandatory waiting periods for finalizing divorce of at least 30–90 days. Since the 2000s, Louisiana (2007), South Carolina (2013) and Washington (2013) have proposed or implemented longer waiting periods for divorce. However, Pennsylvania reduced the waiting time for no-fault divorce from 2 years to 1 year in 2016. Denmark imposed a mandatory three-month trial period and mandatory counselling in 2019 for parents seeking a divorce. In 2020, China imposed a 30-day waiting period, and South Korea also did so as early as 2008.

[4] Divorce risk is both strongly tied to SES and directly affects the financial situation of the family, which risks further amplifying the inequality aspects of growing up in an intact family environment versus a broken home (Hogendoorn et al., 2020). There is a 63% greater risk of experience parental divorce by age 18 for children born 1990 of married parents in Sweden with low SES (bottom 25 pct. of parental earnings distribution in 1990) compared to the children with high parental SES (top 25 pct.). 44.5% experience divorce in the low-SES group compared to 27.2% in the high-SES group.

[5] Comparing children of divorce to those with intact nuclear families, parental divorce is associated with e.g. lower educational attainment, lower household income as adults, and a higher likelihood of being a welfare recipient later in life (Furstenberg & Kiernan, 2001).

children's outcomes is however complicated, not at least because it requires data tracking children over extended periods of time, but also because of the challenges in accounting for correlated unobservables.

In this paper, I evaluate the Swedish divorce law reform of 1974 and its effects on children's long-term outcomes. This reform provides a rare possibility to investigate how institutional factors governing marriage stability affect families and help shape children's life trajectories. The reform led to a substantial liberalization of the existing divorce laws, paired with a divorce restriction (reconsideration period) affecting spouses with children under age 16. The design of the new law allows me to distinguish empirically between both dimensions of the reform and while the main focus is to evaluate the divorce restriction[6], I also present evidence on the effects of the liberalization element of the reform.

According to marriage market theory (e.g. Chiappori et al., 2002), a parental reconsideration period for divorce affects existing marriages through **(i)** increased marital stability, with fewer couples divorcing than if the restriction had not been in place, **(ii)** changes to intra-household bargaining between spouses remaining married due to less credible exit from the union, and **(iii)** an increase in relation-specific investments due to lower divorce risk and stronger commitment from the spouses.[7] As such, divorce law changes affect both divorces and intra-household bargaining for spouses remaining married. The inability to separate these two channels means that divorce law reforms are best suited to estimate reduced-form effects related to divorce.

The empirical analysis draws on rich administrative data allowing me to track 1.17 million Swedish children born 1952-1964 over six decades. With this data, I am able to link the universe of Swedish parents and children from 1969, observe all civil state changes over the time period, and add information on outcomes later in life for the children. I supplement these data with information from the Swedish military conscription tests, which provide information on a range of cognitive and non-cognitive abilities for almost the full population of Swedish men around age 18.

The evaluation of both elements of the reform is based on a differences-in-differences (DiD) approach where birth cohort and family situation of the child determines exposure to the reform elements. In order to focus on the divorce liberalization element, the evaluation is based on comparing children of married parents to children with unmarried parents. The evaluation of the divorce restriction element instead focuses on children of married parents and uses the mandatory 6-month reconsideration period for divorcing parents with a child under 16 living in the household. Since the age of the youngest child in the family determines legal divorce frictions, within-cohort differences in sibling age spacing to the

[6] 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. external and internal validity reasons.

[7] Lower risk of divorce could potentially lead to less investments in the marriage if this induces shirking, but standard economic models of marriage behavior tend to abstract away from this and assume that spouses maximize the marriage value under the given circumstances.

youngest child in the family can be used for identification. Importantly, the identification will focus on the interaction between cohort exposure and age spacing, effectively netting out the main effect of age spacing. The divorce restriction affects children mainly during adolescence and childhood, when a potential disruption to family stability and the continuous human capital investments may have long-lasting effects (Heckman, 2000; Cunha et al., 2006). Both evaluations also exploit the fact that older cohorts who are adults when the reform comes into effect are less directly affected by the reform. Relying on within-cohort variation for identification alleviates concerns about confounding cohort effects (e.g. schooling reforms) and attributes the effects on children to the divorce law reform.

I start by decomposing the responses to the liberalization element of the reform, which reveals that the main immediate respondents were older couples and those without children. I also document that the 6-month reconsideration period for spouses with children under age 16 creates a sharp discontinuity in the divorce rate at that age of the youngest child in the family. Moving on to the causal analysis, the evaluation of the divorce liberalization indicates that being exposed to the liberalized legislation reduces upper secondary school graduation by 5.6% relative to the children of unmarried parents and cohorts graduating before the reform was implemented. Negative effects are also found for university graduation, employment, and cognitive outcomes measured by the conscription test around age 18.

Evaluating the divorce restriction element, I show that the 6-month reconsideration period increases parents' marriage stability. Greater exposure to the reconsideration period during childhood reduces the risk of experiencing parental divorce by 18% compared to the reference group with less exposure. This effect grows stronger for cohorts with every extra year of exposure. Other measures of parental marriage instability also give the same qualitative result. 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 chance of graduating upper secondary school, the main outcome of interest, by 1.8%. The effects on educational attainment are stronger for boys, children with parents who have at most upper secondary schooling themselves, and children whose mothers have weaker attachment to the labor market. Also, the effects are statistically insignificant for the children with a low predicted risk of experiencing parental divorce. Statistically significant positive effects are found for related long-term outcomes such as the children's labor market outcomes and family outcomes for the children themselves as adults. The results are robust to a wide variety of specification checks, such as changing the exposure definition and the inclusion of family fixed effects. Furthermore, group composition changes of parents over time do not appear to be driving the findings. Combining the evidence, the policy is found to affect children's long-term outcomes by sizable magnitudes.

As noted by Gruber (2004) and others, the mechanisms behind the effects linked to within-household

bargaining and behavior are inherently difficult to investigate due to the lack of information on spousal behavior. However, the rich data allow me to shed some light on this channel by studying several indicators that have been proposed in the literature. Mothers more exposed to the reconsideration period reduce their hours worked and labor earnings around the time of the reform while fathers' labor supply remains the same, indicating changes to within-household bargaining. Simultaneously, the intergenerational correlation in educational attainment between mothers and their children significantly strengthens following the reform, providing evidence of greater transmission of human capital. The military conscription tests show that the children's cognitive and non-cognitive abilities also are positively affected by the reconsideration period. Finally, the children appear to delay their fertility decisions away from teen parenthood and early parenthood, which is indicative of less risky behavior and a more stable family environment during childhood. While part of the effects on children's long-term outcomes likely work through parental divorce and separation, the suggestive mechanisms and a mediation analysis indicate that the policy effects mainly runs through children where the parents remain married. These findings provide additional evidence that changes to divorce laws may affect children both through direct and indirect channels, and call for the need of further evidence on the effects of family policy reform on children's outcomes within marriage and through divorce.

The previous literature on the effects of divorce on children tends to find null or negative effects (Gruber, 2004; Frimmel et al., 2016; Björklund & Sundström, 2006; Bhrolchain, 2001).[8] These studies all vary in the type of outcomes studied and the potential for the research design to deal with correlated unobservables. The exception to the null and negative effects in the literature is a recent study in the Chilean context using court congestion as identifying variation for divorce restrictions, which finds positive effects for children related to less court congestion for divorce cases (Heggeness, 2020). The conflicting evidence and the respective challenges to each identification strategy highlight the need for more empirical studies related to divorce policies and children's outcomes.

The nature of the reform 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, 2018), but to the best of my knowledge no research has used a similar identification strategy to show the effect of such restrictions on children's long-term outcomes. Second, the data used allow me to study the effects of the reform on a wider set of outcomes within the same sample as early as childhood until adulthood. Third, the setting and the data provides a rare opportunity to separate between different mechanisms and to conduct a

[8]Research in sociology and economics uses identification strategies based on: **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)** one paper instrumenting for divorce using husbands' exposure to women at the workplace.

mediation analysis.

My findings indicate that divorce laws can play a substantial role in affecting family behavior by setting the institutional environment for marital stability. The causal effects of divorce law reform on children's long-term outcomes appear to be large and of mixed sign relating to the nature of the change taking place. The findings highlight the need for policy makers to consider externalities when designing public policies related to marriage stability, and specifically consider the long-term effects these policies may have on children. Learning from the divorce law reform of Sweden in 1974 could shed important light on the effects of public policy and demographic transition relevant for countries with similar institutional setting and demographic trajectory.

The following structure outlines the paper: Section 2 presents a literature review and theoretical framework. Section 3 presents a background to the institutional context, the educational system in Sweden, and the divorce policy reform of 1974. Section 4 outlines the data sources and empirical strategy. Section 5 presents estimation results, and Section 6 discusses the findings. Finally, Section 7 concludes.

2. Literature review

2.1 Theoretical research: Marginal divorces & family behavior

Marriage and divorce laws have been a topic in theoretical research for several decades. The first wave of research modeling family behavior and parental investments focused on unitary models where households maximize a joint utility function subject to a budget constraint (Becker, 1981; Samuelson, 1956). Extensions to this work added within-household bargaining between spouses over the marital surplus and highlighted the outside option of spouses as key in determining 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, 1974). However, this only holds under restrictive assumptions about preferences and public marital goods following divorce (Chiappori et al., 2015). Empirical observations of divorce rates changing in tandem with divorce law changes provides evidence against perfectly transferable utility (Gruber, 2004), and there is empirical evidence of changes to within-household behavior associated with divorce law changes (Fernández & Wong, 2014; Stevenson, 2007).

The second wave of research highlighted within-household bargaining over common resources as central to family behavior, and this work paved the way for more realistic models of the family (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 affects marriages through re-weighted

bargaining strength of spouses. With imperfect transferable utility, divorce frictions could affect the steady state divorce rate as some spouses are unable to use their marital surplus to compensate the spouse seeking a divorce. These findings highlight that divorce laws affect spouses within marriage as well as through divorces. Empirical findings investigating divorce law changes also show that restrictive divorce laws and more equal division of marital assets are shown to correlate empirically with spousal labor supply decisions (Gray, 1998). In more recent work, theoretical researchers have focused on the role of external policies such as prenuptial contracts and the introduction of unilateral divorce in the U.S. on marital instability and investments into the marriage. 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 according to these frameworks (Reynoso, 2017, 2018; Anderberg et al., 2016). All in all, the effect of divorce law reform is likely dependent on the nature of the reform, factors related to the institutional setting, and the type of family affected by the reform.

2.2 Empirical research: Effects on children

Research in economics and sociology tends to find that divorces on average are linked with detrimental effects on children's outcomes (Amato, 2010).[9] However, there are few credible micro-econometric studies on the causal effects of divorce on children's outcomes. Identification based on fixed effects show that much of the observed effects on children associated with parental divorce are due to negative selection based on family characteristics (Piketty, 2003; Björklund & Sundström, 2006; Chen et al., 2019). Gruber (2004) and Cáceres-Delpiano & Giolito (2012) instead use variation based on changes to U.S. state divorce law (exposure to unilateral divorce) and provide evidence of negative effects on children's long-term outcomes.[10] Gruber also argues that divorce laws fail to satisfy the exclusion restriction and should instead be interpreted as providing reduced-form evidence of the effects of divorce laws on children.[11] Other researchers have followed this line of thinking and evaluated divorce law changes as reduced-form evidence (e.g. Heggeness, 2020). Unfortunately, due to data limitations this work gives very limited insight into the mechanisms at play linking divorce law reform and marital stability to within-household bargaining and children's outcomes.

[9] One could imagine that whether parental divorce is harmful or not likely depends on the status of the marriage. A marriage ridden with violence and conflict which is terminated following divorce may be to the benefit of common children. On the other hand, these kinds of marriages are probably not the ones responding to marginal policy changes which is the focus of this study.

[10] They find that exposure to unilateral divorce leads to a 14.5% greater risk of living with a divorced mother and −6.5% chance of being a college graduate.

[11] The idea that divorce laws can affect family behavior beyond divorces relies on spouses changing their behavior in response to the laws, which in turn partially relies on the salience and relevance of these. A study performed in 1978 shows that 30% of interviewed married women at the time reported having considered divorcing their husbands, which indicates that divorce laws are relevant for more than the spouses that actually divorce every year (at least in the U.S. setting) Huber & Spitze (1980).

Some studies have shown that changes to divorce laws can affect mechanisms related to within-household bargaining of spouses, primarily by investigating spousal labor supply (Voena, 2015; Fernández & Wong, 2014; Stevenson, 2007). While Stevenson & Wolfers (2006) also showed that divorce liberalizations lead to less domestic violence affecting women, the evidence of increased suicide rates among the children affected by unilateral divorce shown by Gruber (2004) indicates potential trade-offs related to family policy. A recent study by Ringdal & Sjursen (2021) sheds more light on how bargaining affects behavior of married spouses in a developing country setting using experimental variation varying fixed endowments between a husband and wife. Their study finds that investments in children can vary depending on parental bargaining strength, and that more investments take place when the most patient parents' bargaining strength increases in the household. Other changes to family policy have also shown to elicit large responses to within-household behavior (Persson, 2020). The mechanisms at play are inherently difficult to disentangle, and some researchers have tried to investigate psychological effects associated with divorce. These studies show that the negative effects associated with parental divorce can be substantial even when shocks happen during adolescence (Chen et al., 2019; Kravdal & Grundy, 2019). There is also clear evidence that divorce shocks can affect educational outcomes for children. For instance, using family fixed effects Gould et al. (2020) show that experiencing parental divorce before the Israeli matriculation exam reduces the chance of passing by 3–8%.

Previous attempts at using exogenous variation to identify the effect of divorce on children are few and the evidence is mixed. Frimmel et al. (2016) instrument for divorce using the father's exposure to women at the workplace finds that divorce leads to worse schooling outcomes for affected children in Austria. Contrarily, Heggeness (2020) uses court congestion as divorce restrictions and estimates that every additional 6-months of waiting time for divorce court cases reduces children's upper secondary school enrollment rate by 1.9%. However, the contradictory effects in the court congestion study may be driven by the setting, where court congestion could exacerbate conflict and uncertainty in the family environment. New evidence on the channels through which divorce affects children has singled out parental time investments as key to building children's human capital and setting the stage for outcomes later in life (Gould et al., 2020). These findings indicate that the effects of divorce and policy restrictions may bring about different effects depending on the setting, equilibrium effects, and the marginal divorces induced. The need for a theoretical framework to make sense of the conflicting evidence is evident.

2.3 Theoretical framework

In order to structure the research process, I set up a basic model of marriage behavior and its effects on children to match the conditions of the 1970s in Sweden. The model draws inspiration and solution

concepts from previous theoretical work on marriage, divorce, and family investments (Rainer, 2007; Anderberg et al., 2016). The framework, which is compactly presented and discussed here, is presented in its entirety in Appendix B.

The framework is based on two individuals (parents) who are exogenously matched to each other and live for two time periods. 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.[12] In the first period the wife chooses to invest in an intermediary marriage good (g_i, e.g. home production and children), which is carried forward into the next period. Investments in the marriage good are beneficial for the children and improve their long-term outcomes. The marriage good is then used as input in the marriage production function where the output is non-rivalrous and 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, private contacts). Scarcity of time and resources means that the wife faces a trade-off between marriage-specific investments and private investments. Husbands fully use their endowments for private investments.[13]

In the framework I also add divorce risk (in the form of an information shock affecting the marriage value), and that divorces transform the joint marriage good into a private good which both spouses cannot enjoy to the same extent as when they were still married. Internalizing this divorce risk, the wives will react by ex ante reducing investments in the joint marriage good (g_i) in favor of the private good (p_i^w). In other words, they increase investments in the private good in order to insure against the divorce event. This decrease in marriage-specific investments will in turn have adverse effects on children's long-term outcomes.

A final feature of the model is the introduction of a waiting period for divorce, in line with the Swedish divorce restriction introduced in 1974. This is modelled as a constant friction component c imposed on all divorcing couples, regardless of their marriage value. 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. In the context of this framework, a divorce restriction will increase the threshold for marginal divorces and reduce the risk of divorce for all couples. In line with the previous results, this means that the friction also affects marital investments positively. The restriction thus acts as a deterrent to impetuous divorces and divorces in general, to the benefit of children. See Figure 1 for an illustration of marginal divorces and marriage quality affected by

[12] A condensed timeline of the model can be seen in Figure A3.

[13] 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 more than 99% of the parental leave taken out in the 1970s were by the mother. From this, it is reasonable to believe that the majority of the investments in children at the time were by the mother.

the restriction.

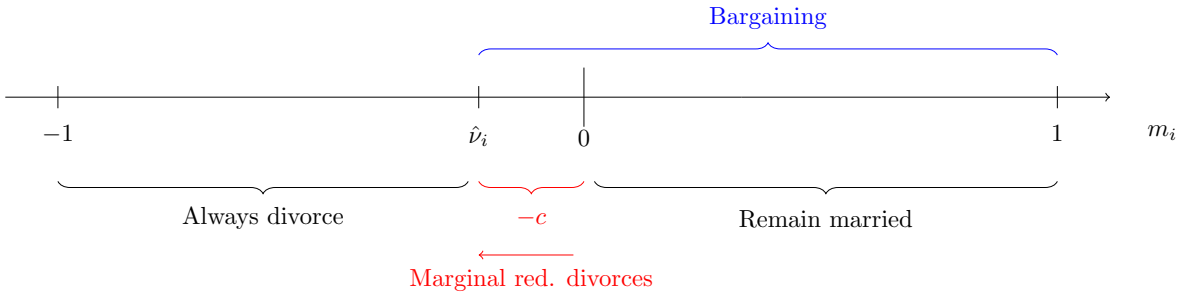

Figure 1: A figure characterizing divorce responses to the realized marriage value and a divorce restriction. Realized marriage value m_i is shown on the unit interval relative to the normalized outside option (0). The divorce restriction c changes a spouse's optimal cutoff for divorce to $\hat{\nu}_i$, 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.

3. Background

3.1 Western divorce laws 1960s–

The decrease in marital stability during the past century has led to more children growing up in other family constellations than the traditional nuclear family. A common saying is that half of all marriages end in divorce, and this seems to roughly be true for both the US and Sweden (Lansford, 2009; Statistics Sweden, 2018). While many countries exhibit a high divorce and separation rate for couples, this trend appeared early on in the Nordics. Sweden and the other Nordic countries are widely considered to be forerunners of this demographic transition that started in the 1960s.[14] Institutional factors may partly explain why the demographic transition first started here. Among other things, the divorce laws in Sweden from 1915 had already introduced no-fault divorce more than 50 years before this was introduced to most other countries in Western Europe and in the U.S. (Brattström & Agell, 2018).

The 1960s and 1970s saw a large increase in divorces in the Western world, coinciding with many countries implementing divorce law liberalizations (e.g. UK 1969, Denmark 1970, US 1970–, Australia 1975). In 1974, Sweden made perhaps the greatest overhaul of them all by substantively simplifying the divorce process, removing all fault-based reasons for divorce and making unrestricted divorce the

[14]Figure A2a displays the number of divorces in the Nordic countries from 1900–2018. Starting from the 1960s, the Nordics were early outliers in terms of exhibiting a decreasing fertility rate, fewer marriages and more divorces over time. Changing family preferences, economic opportunities, technological progress, and the expansion of the public sector are possible factors driving the demographic change. Notably, the increase in divorce numbers in Sweden starting in 1964 coincides with the expansion of the welfare state, and the introduction of the birth control pill.

new norm. At the time, the new law was deemed to be the most liberal divorce law in the Western world (Jänterä-Jareborg, 2014). A novel feature of the divorce law was a 6-month reconsideration period implemented for unilateral divorce and families with children, mainly motivated by preventing impetuous divorces and protecting children from experiencing parental divorce. Similar law changes imposing mandatory reconsideration periods for divorce have taken place around the world, but many countries still share similarities with the institutional setting of Sweden before 1974.[15] For instance, some U.S. states, the UK, Germany, and Canada still retain fault-based reasons as primary reason for divorce and restrict unilateral divorce to a substantial degree.[16]

3.2 Institutional background

Divorce law in Sweden of 1915–1973

Between 1915–1973, divorce could be granted based on three principles: divorce under mutual consent (82% of all cases), unilateral divorce (4%), and fault-based reasons (adultery 12%, other 2%). Under mutual consent, the couple jointly filed for divorce at the local court. Following this, the couple had to go through a mandatory counselling with the stated aim of trying to salvage the marriage. Should the counsellor find the marriage beyond salvaging, the couple was allowed to file for a year-long separation period.[17] After the separation period had passed, the spouses were allowed to finalize the divorce.

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.[18] 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

[15] Many U.S. states introduced unilateral divorce around starting in the late 1960s, and mandatory reconsideration periods for divorce exists in states across the country. Some states also extend the reconsideration period up to 1 year when joint children are involved in the divorce process.

[16] South Carolina still retains no-fault and fault based grounds for divorce. For a no-fault divorce, the parties must have lived separately for at least one year. Fault-based reasons, such as adultery, are recognized 90 days after filing for divorce. Fault-based reasons in the UK are adultery and unreasonable behavior. For no-fault divorce, the couple needs to be separated for two years under mutual consent and five years under unilateral divorce. Germany allows unilateral divorce following a three-year separation period. Consensual divorce is granted after a year-long separation. Divorce in Canada is still based on one-year separation, adultery or abuse.

[17] The counselling was provided by the municipality and performed by a priest or a public counsellor, after which the spouses were granted a note valid for three months certifying that they had participated and were allowed to file for divorce. Under disputes over alimony or other issues, the legal process of being granted a 1-year separation could be lengthy. The spouses were supposed to live apart and support themselves financially during the separation period. 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).

[18] 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 Figure 2c). Reports from the public investigation of 1972 on the ensuing divorce law change 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. Spouses could also divorce through fault-based reasons instead of waiting for the reconsideration period to finish.

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 (Prop. 1973:32, 1973). The restrictive divorce laws at the time were motivated with families being the building blocks of society, and that marriage stability was deemed as important for society at large.

Under unilateral divorce decisions before 1974, the divorcing spouse originally had to prove the breakdown of the marriage through "long and irreconcilable marital differences". This was usually done by proving that the couple had been separated for at least three years. After having proven this, the divorce could be granted by the courts without any reconsideration period.[19] Also, there were a number of fault-based reasons that could be used as grounds for divorce without any reconsideration period if there was proof of e.g. adultery or a criminal sentence for more than 3 years. For details on the divorce policies in Sweden and the rest of the Nordics, see Table B3 in Appendix B.

Divorce law reform of 1974

The divorce law reform of 1974 removed all fault-based reasons and made unrestricted divorce the new norm. This change meant that divorcing spouses no longer needed 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.[20] Especially women's increased economic freedom and lessened reliance on their husbands was a key reason behind the new policy.[21] Despite the motivation to put the individual's freedom first for divorce decisions, the policy makers implemented a reconsideration period under unilateral divorce and when a child under age 16 was living in the household. The restriction was motivated with a risk of adverse effects of divorces on children and to prevent impetuous divorces. The 6-month reconsideration period was reasoned to be an adequate restriction, since the reconsideration period could allow for some couples to reconcile while not being overly restrictive (Inger, 2011). This divorce restriction still remains in place as of 2021. While there is no conclusive evidence that the reconsideration period affects divorces more than through mechanical postponement, statistics from the Swedish Courts show that 11% of joint applications for divorce, and 21% of unilateral divorce decisions were retracted before being finalized during 2000–2010 (Swedish Courts, 2014).[22]

[19] From July 1, 1969, the divorce laws were revised to allow the year-long separation period to be granted based on unilateral divorce applications. The divorce law revision in 1969 also made it harder to divorce based on adultery (Hafström, 1965; Inger, 2011). This change did not coincide with any clear change in 1-year separations or divorces (see Figure 2d)

[20] The dominant view at the time, as expressed in the public investigation of 1972, was that marriage was to be a private and voluntary commitment with full freedom to opt out of. Any stabilizing effect of divorce restrictions on marriages was not deemed to outweigh the direct costs of restricting the individual's freedom (SOU 1972:41, 1972).

[21] The employment rate for women had rapidly increased to around 60% in 1970, and 20 years later this share had grown to over 81%. The employment rate for men had since long been stable at around 85%.

[22] Using the research strategy later outlined in this paper, I show that parents affected by the divorce restriction were more likely to be married or cohabitating 16 years after the reform (see Table B2). Also, a study investigating the introduction of a 30-day "cool-down" period for divorce found that this decreased divorces by about 10% in South Korea (Lee, 2013).

The first step toward the divorce policy 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 passed by the parliament in early 1973, and enacted January 1, 1974 (Prop. 1973:32, 1973). 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.[23] Figure 2c 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.[24]

Institutional context around the time of the reform

The educational attainment in Sweden underwent large changes around the time of the divorce law reform. Following an education reform in 1962/63, the Swedish schooling system is comprised of nine years of compulsory education.[25] After completing compulsory schooling, students in Sweden are able to enrol in upper secondary education. In 1971, the different upper secondary school systems in Sweden were replaced by a unitary system with vocational and academic tracks. Schooling at the upper secondary level after this reform is voluntary, and the share of children enrolling has been increasing over time starting in the 1960s. In 1960, 10% of the cohort graduated upper secondary school, and by 1970 this number had grown to 30%. Ten years later, 85% completed upper secondary school, and by the end of the 1980s, almost 90 percent of the children continued directly to upper secondary school.[26] 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 (Hall, 2012).

The 1970s were a formative time when many new family policies were implemented in Sweden. The welfare state was rapidly expanding with new family policies: joint marital taxation was abolished in 1971, the parental leave system was implemented in 1974, and the abortion laws were revised the same year.

[23] See Figure 2b 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.

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

[25] By 1960, 31% of Swedish children age 7–15 were enrolled in the 9-year compulsory school. By 1990, 99% were enrolled.

[26] 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 two-year upper secondary degree in order to obtain a three-year degree within the adult education system.

Universal healthcare and education was long since free of charge, and women were entering the labor market in never-before-seen numbers. Equality in society was at its peak around this time, and the latest national schooling reform of 1962/63 had ensured that more students could access secondary education. The comprehensive nature of the welfare state around this time was almost unique in reducing inequality and ensuring that those worst off in society were taken care off. In terms of marriage patterns, society was quite different from today when the majority of children are born to unmarried parents. Marriage was the pre-dominant form of civil status for cohabitating couples at the time of the reform in 1974, with 88% of cohabitating couples being married. While cohabitation was on the rise it was very rare around this time, especially for spouses with children.

Swedish law is based on the viewpoint that married spouses are obliged to support each other financially during marriage, but that the economic ties are to be severed after divorce. Marital assets are generally to divided equally between the spouses during the divorce process, and there is no default inheritance between former spouses. Alimony to the financially weaker spouse is rare except for transition periods and when one spouse takes the majority custody of any children.[27] Reportedly, only one in ten cases of divorce in 1974 led to a court mandated alimony for the financially weaker spouse. Children alternating between living with both parents after a divorce is relatively common today, but was not so at the time of the reform. The results of a governmental investigation in 1975 showed that the mother received full custody in 84% of cases with a custody dispute (SOU 1975:24, 1975). In the middle of the 1980s, only 1% of children alternated between living with both parents following parental separation (Statistics Sweden, 2019). Even today, the vast majority of children with separated parents live with the mother full-time (Statistics Sweden, 2018).

4. Empirical method

4.1 Data

The main data source used in the project consists of full-population data based on Swedish taxation registers (RTB) and other linked registers. These data combine information on civil status, family linkages, educational attainment and labor market outcomes. Using these data, I am able to construct a panel of the full universe of parents and their children from 1969–2000, including information on all civil state changes (Historiska FBR), parent-child linkages through the Multi-Generation register (MGR) for those

[27] This institutional setting discourages specialization of spouses into household production, as as spouses not active on the labor market risk financial difficulties following a divorce. The median child alimony for one child at the time of the reform was 1500 SEK per month (roughly $170), and for two children 2400 SEK ($280) per month in 2020 value. The size of the alimony is weakly positively correlated with the husband's earnings (SOU 1975:24, 1975).

born 1932 and later, place of residence, earnings and educational attainment for select years from the census (FoB) and other demographic information. This panel allows me follow civil state changes over time and enables for a detailed analysis of marital stability for the affected parents and children.

As will be discussed more in detail in the following section, the evaluation of both elements of the reform focuses on the interaction between an individual's exposure to the reform and cohort group. The divorce liberalization element of the reform is evaluated by comparing outcomes for children with married parents against those with unmarried parents, where marriage status is defined in 1970. Thus, the original sample of children with married parents is supplemented with 9,805 children of the cohorts 1952–1964 with unmarried parents. The identification strategy relies on the interaction of the cohorts born 1956–1964 being exposed to the reform during childhood, while the main effect of marriage status is captured using the placebo cohorts born 1952–1955. Contrarily to the younger cohorts, those born 1952–1955 were exposed to the policy as adults when most would have graduated upper secondary school, and were thus less directly affected by the reform.

The evaluation of the divorce restriction focuses exclusively on children of married parents and the interaction between cohort group of the child and its age spacing to the youngest sibling in the family. Children born during 1956–1964, who were exposed to the new divorce law during childhood, are the primary cohorts of interest. This sample consists of 853,900 children. The placebo cohorts born 1952–1955 are, as with the previous evaluation, used to capture the main effects of age spacing since these children were exposed to the policy as adults.[28] This placebo sample consists of 314,974 individuals.

Siblings are linked together using the mother's ID from the MGR. Due to the lack of a household identifier in the data, households are created by adding fathers to the same household as the woman he has a joint child with. This assignment means that children with the same father but with different mothers will be categorized as separate households, while children with different fathers will be assigned into the same household.[29] I restrict attention for outcomes to 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 where longer time spans are readily available, I add information up to the year 2014. The identification strategy and descriptive statistics are presented in the following subsections.

I also supplement the existing data by adding information from the Swedish War Archive on eight dimensions of non-cognitive and cognitive ability. Roughly 90% of the Swedish men in the cohorts I study performed these mandatory tests around age 18. The measures of non-cognitive abilities are

[28]The median age of leaving the family household is 21 for the cohort born in 1965, this means that some of the children in the placebo group will still be in the household when the policy comes into effect.

[29]The 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.

from on a standardized psychological evaluation aimed at determining the conscripts' capacity to fulfill the requirements of military service. The 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 conscripts' different answers and discussions 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).[30] The non-cognitive abilities are graded by the psychologist in four subscores measured on a 1 to 5 scale, which I standardize to be mean-zero, standard deviation one by cohort.

The Swedish War Archive also contains information on the conscript's cognitive ability. This consists of four subtests of logical, verbal, and spatial abilities, as well as a test of technical comprehension. The cognitive tests are based on speeded multiple-choice questions, and are also standardized. These abilities have previously been shown to strongly correlate with outcomes later in life, such as labor market outcomes (Lindqvist & Vestman, 2011), and are also determined around the time when the divorce law reform is expected to affect the children. All eight measures are used in the analysis, along with the two composite terms (non-cognitive ability, cognitive ability) based on the four components in each ability group.

4.2 Identification strategy

The evaluation of the different reform elements are based on the same DiD approach where the interaction of exposure and cohort group captures the coefficients of interest, with the main effects being netted out. While the two evaluations and their respective identification strategies are highly similar, they differ in terms of exposure definition (age spacing and marriage status) and will be outlined in the following subsections. I start by outlining the strategy for evaluating the divorce restriction element, since this is the main evaluation of the paper. Lastly, I outline the evaluation for the divorce liberalization element.

The divorce restriction element

In order to evaluate the effects of the divorce restriction element on children, I focus on children of married parents and exploit the fact that the age of the youngest child determines the exposure to the divorce restrictions for all the children in the family. I define Marriage status and age spacing in year 1970, well

[30] The subcategories of non-cognitive ability and the Big Five personality traits to which they are related (Openness to experience [O], conscientiousness [C], extraversion [E], agreeableness [A], and neuroticism [N].): (1) psychological energy (perseverance [C], ability to fulfill plans [C], and ability to remain focused [C]); (2) emotional stability (ability to control and channel nervousness [N], tolerance of stress [N], and disposition to anxiety [N]); (3) social maturity (extraversion [E], having friends [E], taking responsibility [C], and independence [O]); and (4) intensity (capacity to activate oneself without external pressure [C] and intensity and frequency of leisure activities [O]) (Grönqvist et al., 2020).

before the new divorce law became known to the general public. With this, I can compare children of the same birth cohort with varying age of the youngest child in their respective family.[31] 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 mitigating and confounding the effects of the reconsideration period.[32]

Specifically, the children are split into two groups: one which consists of the youngest children themselves and those with a sibling 0–2 years younger than they are, and the other group consisting of those with a younger sibling 3–8 years younger. The first group is deemed to have weak to no insulation against parental divorce, while the second has greater insulation by virtue of their age spacing. I set this cutoff since the second group of children will be more insulated against parental divorce for at least three years after age 16, which approximately corresponds to the 3-year duration of Swedish academic-track upper secondary school up to age 18.[33] Given previous evidence on divorce shocks during childhood (e.g. Chen et al., 2019; Gould et al., 2020) and the theoretical framework presented in Section 3.3, I expect the reconsideration period to affect children's outcomes positively due to a larger age spacing insulating against shocks related to parental marital instability, and to increase marital investments.

A key concern with comparing children of differing age spacing is that this spacing is potentially endogenous, and may affect children's outcomes in other ways not related to the divorce policy. 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; Nuttall

[31] Setting marriage status and age spacing in 1970 gives me stable groups that can be followed over time but does not take any new siblings or changes to marital status into account. This reduces the risk of capturing changes (to marriage behavior and fertility) induced by the new divorce policy, but also risk attenuating the effects since the birth of new siblings could move the children from the comparison group with low spacing to the group with greater spacing where we expect to see an effect. Parents divorcing differentially in the age spacing groups during 1971–1973 unrelated to the reform could also cause thinning that would affect the results. The fact that year-to-year marriage status is very stable for most parents during the years before the new divorce law, and that very few extra siblings are born during those years reduces the risk of this affecting the results. To test these concerns empirically, I run specifications where I define the marriage status and age spacing in year 1973 instead of 1970 and observe slightly stronger but qualitatively unchanged results for the main educational outcomes (see Table B1). Including all children with age spacing 0–8 years regardless of parental marriage status in 1970 also strengthens the main estimate (results available upon request).

[32] The rapid expansion of compulsory school translates into substantial changes to the cohort upper secondary school graduation rate. By 1990, 71% of the cohort born in 1952 had graduated upper secondary school. The corresponding number for the cohort born in 1964 is 83%. Cohort-specific shocks risk confounding the estimates related to the divorce restriction, which is the focus of this paper. These potential confounders are captured by including cohort fixed effects, and any potential spatial variation in the implementation of the schooling reform will be handled by including municipal fixed effects.

[33] Vocational-track upper secondary school at the time usually only lasted two years. This cutoff means that the reference group is exposed to unrestricted parental divorce for at least one more year during childhood. Bounding the age spacing at 0–8 years means 90% of children in each cohort are included, and this restriction is made to keep track of exposure to the reform for the youngest cohorts. The mechanical effect of this bounding means that I exclude more children from older cohorts since these are at a greater risk of me observing a much younger sibling in year 1970, and also that I include more children of primarily the younger cohorts where some younger siblings have yet to be born. I verify that this is not a concern by first including all age spacings (> 8), excluding the children whose siblings born 1971–1973 change their age spacing status to $8 >$ years (excluding 3.5% of the sample), and change the age spacing definition to year 1973 and show that this gives unchanged or slightly stronger estimates on educational outcomes (see Table B1).

& Nuttall, 1979). There is also evidence that birth order affects children's outcomes (Black et al., 2018). I will address this concern by looking at older cohorts of children where the age spacing is the same as for the treated children, but for whom the policy should have less of an effect since they are old enough for most to have left upper secondary school when it came into effect. The children of birth cohorts 1952–1955 were age 19–22 in 1974 and had passed the age when most are enrolled in upper secondary school (age 16–19). Hence, the cohorts born 1952–1955 are "unexposed" to the reform.[34] Thus, any effects of age spacing on children's outcomes should be present for these cohorts and can be accounted for.

The cohorts born 1956–1957 are only affected by the insulation for 1–2 years and are therefore "partially exposed" to the policy, while the 1958–1964 cohorts are "fully exposed".[35] 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.[36] This assumption corresponds to the parallel trends assumption in DiD terminology (Angrist & Pischke, 2008).

I thus define 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 the age spacing of 0–2 years. The results of this difference will be 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. In the 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. The effects of exposure to the reconsideration period is estimated through the following regression equation:

$$y_i = \beta_0 + \beta_1 \underbrace{Insulation_i \times \mathbb{1}[Cohort_i \geq 1956]}_{Treat \times Post} + \beta_2 \underbrace{Insulation_i}_{Treat} + \underbrace{\sum_{j=1953}^{1964} D_{i,j}}_{Post} + p_i + \boldsymbol{X_i'\delta} + \varepsilon_i$$

The indicator $Insulation_i$ takes the value one (1) for individual i if the age spacing with the younger 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 (1) for cohorts born 1956–1964 (ages 10–18 in 1974) and zero for cohorts born

[34] The children born in 1955 are a borderline case, since some of them will be affected during their final 6 months in 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 Figure 2b & 2c). I simplify things by including the 1955 cohort in the unexposed cohort group, since any spillover should be minor and only serve to attenuate the effects.

[35] I collapse the partially and fully exposed groups into one group in the estimation of the effects. Including the partially exposed cohorts should attenuate the results slightly, but help provide a better picture of the effects of the reform at large.

[36] 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.

1952–1955 (ages 19–22 in 1974). Cohort indicators $D_{i,j}$ take the value 1 if individual i is born in year j, which capture cohort fixed effects and correspond to controlling for differences between the pre and post groups. p_i are fixed effects flexibly capturing the mother and father's age, which are known in advance to be somewhat imbalanced across age spacing categories.[37] $\boldsymbol{X_i}$ is a vector of controls which captures pre-determined characteristics of the child and the parents from the 1970 census (including parental labor market outcomes 1970, municipality of residence, educational attainment, child's birth month indicator and sex). Missing values of control variables are included as separate indicators. Along with parental age effects, these controls are also included to ensure that the children are as comparable as possible and to potentially improve the precision of the estimates. Later on in the robustness section, I present results excluding controls. Standard errors are clustered on the household level to account for correlation of outcomes within the same family (e.g. Bertrand et al., 2004).

This specification allows β_1 to capture the difference in insulation effect of the reconsideration period between cohorts born 1956–1964 and 1952–1955, effectively netting out any pre-existing effects of age spacing on children's outcomes and cohort effects. The pre-existing effects 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 (see Figure 5). This means that the comparison can be interpreted as the difference between youngest siblings (no insulation) and spacing 3 years and greater (insulation). The strength with this approach is that it allows me to flexibly include cohort effects in the estimation. Further, excluding the youngest children in the family from the estimation allows me to estimate the effects exclusively on elder siblings and remove changes to birth order effects for the youngest child in the family.[38]

The divorce liberalization element

The evaluation of the divorce liberalization element of the policy consists of comparing the children of married parents to the children of the same cohorts with unmarried parents.[39] The sample of children with unmarried parents amount to 9,805, while the children with married parents number 1,168,874. The relatively few unmarried parents around this time will make the comparison more unstable than a similar evaluation taking place today, when the majority of children are born to unmarried parents.[40] The effects

[37] Combining parental age effects and cohort effects means that I effectively control for the age at birth for both parents.

[38] I also show that the effects are robust to including family fixed effects.

[39] Unmarried parents are defined as both parents having never been married before, i.e. does not include divorcees and widow(er)s.

[40] 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). An initial concern based on this is that defining marriage status in 1970 at different ages for the children causes thinning of the distribution for older children and mechanically makes the comparison

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, in 1975, 89% of the parents who were unmarried in 1970 remained unmarried, while 10% had married and 1% had divorced since 1970. This indicates that the contamination is relatively small, but still non-negligible especially for the youngest cohorts born 1961–1964 where the contamination of married or divorced parents is higher at around 15.5%.[41] However, this is the best possible counterfactual available to evaluate a reform which affected the entire population of married parents.

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:

$$y_i = \beta_0 + \beta_1 \underbrace{Married_i \times \mathbb{1}[Cohort_i \geq 1956]}_{Treat \times Post} + \beta_2 \underbrace{Married_i}_{Treat} + \underbrace{\sum_{j=1953}^{1964} D_{i,j}}_{Post} + p_i + \boldsymbol{X_i'\delta} + \varepsilon_i$$

Similarly to the previous specification related to the divorce restriction, β_1 captures the difference in outcome between children of married and unmarried parents (reference group) and the cohort groups born 1956–1964 and 1952–1955, effectively netting out any pre-existing effects of parental marriage status between the cohort groups. The pre-existing differences between children of married and unmarried parents is instead captured by β_2, which estimates the difference for the older cohorts born 1952–1955.

4.3 Descriptive statistics

The main sample thus consists of 1,168,874 children of married parents born 1952–1964, and descriptive statistics are presented for the first year of the panel 1970 (corresponding to the 1970 census) in Table 1

groups more similar for the younger cohorts, meaning that the group of younger children consists of more parents of higher match quality that would eventually marry. This kind of thinning could lead to observing a convergence in the outcomes over time. The relatively stable marriage behavior for parents at these ages point to this being less of a problem, but some effort should be made to dispel these concerns. Under the caveats of conditioning on an outcome potentially related to the divorce law reform (marriage behavior), I run specifications where I condition on the parents of children in the comparison group remaining unmarried by 1975 and 1980 separately. Conditioning on 1975 civil status, the results on upper secondary schooling are unchanged. Conditioning on 1980 status, the magnitude of the results decreases slightly but remains qualitatively unchanged (results available upon request).

[41] Contrary to the oldest cohorts born 1952–1955, where the share of married or divorced parents in 1975 was 4% which is deemed as negligible. In 1980, the contamination had grown to 18% (24% for the youngest cohorts and 7% for the oldest cohorts).

when the children are 6–18 years old.[42] The descriptive statistics are meant to visualize the identification strategy and serve as an initial balancing test.[43] The statistics of the children, their parents, and their maternal grandparents in 1970 are presented in columns (1), (2), (4) and (5) corresponding to their spacing category and cohort group. The additional columns (3), (6) and (7) display the difference in average characteristics, and the final column presents the p-value of the double difference related to the identification strategy. 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.[44]

The table shows that most characteristics vary substantially for children of the same cohorts with different age spacing, indicating large differences between the age spacing categories. Reassuringly, the difference in characteristics for age spacing is similar for the older cohort group compared to the younger cohorts. In general, the double difference between age spacing category and cohort group greatly reduces the magnitude of the observed difference, but it still remain significant for some variables. This double difference gives an indication that the differences between the age spacing categories are not always stable across cohort groups, but are small in magnitude. For the married and unmarried parents, the strategy serves to reduce the difference between the groups but it always remains statistically significant.

5. Results

5.1 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 in 1974 (67% increase from 1973, see Figure 2). A crude analysis of this spike shows that the primary respondents were older couples and couples without children, with no apparent immediate heterogeneity by earnings and educational attainment (see Figure 3). However, all subgroups in terms of age, child status, earnings, and education show an increased number of divorces at the time following the reform, indicating that there was pent up demand for this in all echelons of society. After a few years the pattern indicates that divorce is more prevalent relative to before the reform among spouses with some education and those earning below the median in 1970.

[42] As mentioned, the sample of children will be supplemented by adding 9,805 children of unmarried parents to evaluate the divorce liberalization element, with descriptive statistics presented in Panel B. These children are highly selected and very few compared to the sample of children with married parents, but serve as a valid counterfactual under the assumption of no composition changes over cohorts.

[43] More formal joint composition/balancing tests will be presented later on in the Results section.

[44] The correlation in earnings and educational attainment between grandparents and the parent is always positive and statistically significant. Also, the partial effect is larger within the same sex (i.e. from maternal grandmother to mother and paternal grandfather and father).

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 4, 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 divorce rates at ages below 16 starting in year 1974, indicative that the policy had large effects on short-run divorce incidence (roughly a 30% decrease relative to the baseline risk). This finding confirms that of the crude respondent analysis and pinpoints that the attenuated effects on divorce prevalence for parents stems from those with children below age 16.

Simultaneously, the general equilibrium effects of the new divorce law became evident as the number of marriages reversed its declining trend and increased by 17% in 1974 compared to the previous year. While the bulk of these new marriages were between previously unmarried individuals, the number of marriages between previously divorced spouses increased by 60% over the following three years relative to the year before the new policy (see Figure A1). This finding is consistent with evidence from the U.S. that reductions in waiting time for divorce increases remarriages (Wong, 2018). The massive spike in divorces was later mirrored by a spike in marriages following the widow's pension reform in 1989, further showing the responsiveness of marriage behavior to public policy reform (Persson, 2020).

5.2 Evaluating the divorce liberalization

As noted, the liberalization element of the 1974 reform affected the entire population of married parents, making the causal effects difficult to disentangle. A relatively small number of parents $(9,805)$ at the time were unmarried in 1970, which means that their children could be used as a plausible counterfactual for the direct effects of the reform.

Effects on various long-term outcomes

Using the outline identification strategy of comparing children of married parents to the children with unmarried parents, I presents results on various outcomes for the affected children in Table 2. For educational outcomes, the table shows that the difference in schooling between children of married and unmarried parents decreases significantly by 4.6 pp. $(-0.046$, s.e. $0.016)$ following the reform, which is -5.6% relative to the mean of the outcome of the reference group. The convergence in schooling outcomes corresponds to closing the educational gap between children of married and unmarried parents observed for the cohorts born 1952–1955 by more than a third. The effects on later university graduation are smaller, but still sizable and more prominent relative to the mean dependent outcome for the control

group (−0.031, s.e. 0.006), which translates into a relative effect of −27%. For labor market outcomes and the conscription measures of ability the estimates 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, 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, s.e. 0.043), on the other hand, are negative and statistically significant with a relative effect of −4.6% for employment and a substantial effect on cognitive ability in terms of standard deviations.[45]

In Figure 6, I present a graphical representation of effects of the divorce liberalization on upper secondary school completion by birth cohort. The figure shows a large, stable gap in upper secondary school completion for the older cohorts, indicating that married parents' children had much better schooling outcomes around this time. The difference between the groups visibly shrink for the younger cohorts affected by the divorce law reform, except for the cohort born in 1960.[46]

The main concern with this evaluation is potential composition changes over cohort groups of married and unmarried parents driving the observed effects.[47] However, Figure 12 shows that the predicted upper secondary school completion based on parental and child characteristics in 1970 (educational attainment of parents, age of parents, parents' labor market outcomes, sex, birth month, and municipality of residence) predicts a relatively stable, positive difference between the children of unmarried and married parents over time. A test of the difference over cohort groups shows that the difference is insignificant (p-value 0.538). In other words, composition changes are not contributing to the convergence in schooling outcomes.

A further concern which is harder to address is the possibility of changing views on marriage driving the observed effects, or some other effect besides the reform which caused a convergence in educational attainment between the groups. A gradually changing culture with greater acceptance of cohabitation without marriage could have helped reduce the difference between the comparison groups if children with unmarried parents' outcomes were directly affected by a change in attitudes. Such a change is hard to disentangle, and is most likely also directly related to the divorce law change. For now, I acknowledge this caveat and present the results of the evaluation in the paper for transparency reasons.

[45] Changing the labor market outcome into employment at age 35 flips the sign of the estimate and makes it significantly positive. The effect on log earnings, however, remains statistically insignificant. This result may be related to the outcomes being measured during the severe 90s financial crisis in Sweden, but also reflects the instability of the labor market outcome estimates.

[46] The effect on schooling jumps visibly for cohort born in 1960 back to the level before the reform. While it may be the sign of something else affecting this result, the jump is quite small and may be driven by noise and the relatively small comparison group.

[47] As mentioned in the Method section, differential thinning of the distribution (unmarried parents marrying over time) or other concerns related to parents' marriage behavior after 1970 does not appear to affect the results more than marginally.

5.3 Evaluating the divorce restriction

Effect on measures of parental marital instability

Using the research strategy based on age spacing of siblings, I validate that the policy affected the risk of experiencing different measures of marital instability (divorce during childhood, divorce in 15 years from 1970, and fathers' multipartner fertility) by estimating the effect of the reconsideration period on these outcomes. Experiencing parental divorce during childhood is defined as either of the parents divorcing at any observable year until the child is age 18. Since divorces are first observable from 1970 and onward, parental divorce can only be observed 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 less for the estimation since outcomes are compared within each cohort. Experiencing parental divorce within 15 years of 1970 instead follows all cohorts for 15 years and captures the more long-term effects on marital stability. Multipartner fertility is used as a more indirect measure of marital stability, and is defined as an indicator taking the value one if the father has a child with more than one woman and that the child is born after 1974.

The results of the OLS regressions on measures of marital instability when pooling the cohort groups can be seen in Table 3. The effect on experiencing parental divorce by age 18 for the cohorts 1952–1955 is precisely insignificant at zero when including all children (−0.000, s.e. 0.001), but significant and positive when looking at elder siblings (0.005, s.e. 0.001). For the cohorts born 1956 and later this insulating effect is substantially stronger and negative (−0.022, s.e. 0.001) and even greater for elder siblings (−0.033, s.e. 0.002). The reduction in parental divorce in the main specification translates into a relative effect of roughly 18–20% lower parental divorce incidence compared with the control mean of the reference group born 1956–1964. This effect is larger in magnitude than that found by Lee (2013) for South Korea's 30-day "cool-down period" (10% relative effect), which may be explained by the Swedish reconsideration period being considerably longer. The other measures of marital instability provide evidence in the same direction of less parental marital instability following the reform. The estimates for experiencing parental divorce in 15 years from 1970 are also significantly negative for the exposed cohorts (−0.035, s.e. 0.002) and when looking at elder siblings (−0.030, s.e. 0.003), with similar relative effects as experiencing divorce by age 18 (13–21%). The same goes for the estimates of fathers' multipartner fertility for all children (−0.007, s.e. 0.001) and for elder siblings (−0.007, s.e. 0.001), which are weaker in absolute magnitude but significantly negative and similar in relative terms compared to the other estimates (17–29%).

Figure 7 presents graphical results of the OLS regression by cohort, where Figure 7a includes all children and the Figure 7b focuses exclusively on elder siblings (age spacing 1–8). The results show a

significant, negative effect of insulation on parental divorce incidence by age 18 for the cohorts affected by the reform. In other words, large age spacing between siblings appears to insulate against parental divorce. The insulation effect of age spacing on parental divorce does not exist for the cohorts that should be less affected (born 1952–1955). For the cohorts exposed to the reform (born 1956–1964) the effect appears to increase with every extra year of exposure. The added effects by cohort could be an indication that the policy works beyond mechanical postponements and prevents some divorces from occurring by changing the behavior of parents.[48] The observed effects are weak for the affected cohorts who are relatively old at the time of the reform and thus have few years of exposure (cohorts 1956–1957).

All in all, the reform appears to have substantial effects on the marital stability of parents. While not capturing the full extent of the policy's effect on families (e.g. changes to within-household bargaining and parental investments) or being perfect measures to capture marital instability, this and the other outcomes provide valuable evidence that the policy affects family behavior.

Effect on upper secondary school completion

The preferred outcome where I expect this kind of a divorce restriction to affect children's outcomes is upper secondary school completion. Based on the theoretical framework presented in this paper, the restriction is expected to strengthen marital stability of the parents and increase parental commitment, which could benefit the children. Changes to parents' marital stability and within-household bargaining during adolescence could thus impact parental investments and the emotional stability of the affected children during formative years of human capital development. Upper secondary school completion is primarily determined during ages 16–19, which are the ages directly affected by the new divorce restriction.[49] Experiencing parental divorce or exposure to more liberal divorce laws has also previously been associated with a decrease in children's educational outcomes. This means that a divorce restriction potentially could benefit children's schooling outcomes (Steele et al., 2009; Gruber, 2004). Upper secondary school completion is also marked by its importance in predicting outcomes later in life beyond schooling, such as labor market outcomes, criminal behavior, health and other indicators of economic well-being, which makes this an important focus of study (Oreopoulos & Salvanes, 2011; Lochner, 2020; Heckman et al., 2008; Freudenberg & Ruglis, 2007).

The OLS estimates for the upper secondary schooling outcome can be seen in Table 4. Greater exposure to the reform, when including all children, is shown to significantly increase upper secondary

[48] Apart from the results investigating parental divorce in 15 years from 1970, I investigate this by looking at long-term marriage rates for the affected parents and find that the added insulation is associated with a greater chance of being married or cohabitating in the year 1990 (see Table B2.

[49] Due to data restrictions, the schooling outcomes are first observed in the 1990 census when the affected children are 26–38 years old. This gives the children who did not complete their schooling the chance to complete it through adult education, which could attenuate the observed results.

school completion for the exposed cohorts (0.015, s.e. 0.002), while the effect for the older cohorts is significant and negative (−0.007, s.e. 0.002). The results when only looking at elder siblings are also significant and positive (albeit weaker) for the main cohorts (0.008, s.e. 0.003), and insignificant for the older cohorts (0.004, s.e. 0.003). Relative to the mean of the dependent variable of the control group, the effects correspond to an increase of 1.0–1.8%. These results indicate that the effects of the reconsideration period on educational outcomes are the strongest when comparing youngest siblings to elder siblings, but that the insulation effect from greater age spacing also exists within the elder siblings group.

The graphical representation of the effects by spacing category and cohort can be seen in Figure 8. Figure 8a shows results for all children, while Figure 8b focuses on elder siblings. The figures indicate that schooling results were positively affected by the reform, albeit not very strongly for the cohorts born 1956–1957 where the reform insulation is only 1–2 years at the end of upper secondary school instead of the full 3 years. The effect of larger age spacing appears to be negative for the group of older cohorts born 1952–1955, and stable over the individual cohorts in the group.

Effect on children at risk of experiencing parental divorce

In order to strengthen the link between marriage instability and the schooling outcome, I present evidence that the effects on schooling are driven by children with parents in more unstable marriages. I do so by showing treatment effects by quintile of predicted risk of experiencing parental divorce in Figure 10. The at-risk split is formed by predicting the outcome of experiencing divorce by age 18 based on pre-determined background characteristics and then splitting the sample of children into quintiles based on the predicted risk of experiencing divorce. The prediction does well in capturing actual divorce behavior of parents, with the Q5-Q1 difference in actual divorces being 25 pp.[50] The figure shows that the families with the lowest predicted risk of going through a divorce (quintile 1–2) exhibit no significant improvement in educational outcomes for the children, while the families at medium and high risk of divorce (quintile 3–5) are where the treatment effects are found.[51] The finding is in line with more unstable families changing their behavior in response to a divorce law change, while families associated with less divorce risk are virtually unaffected by the reform. This strengthens the case that the divorce law reform indeed is driving the observed effects on upper secondary school completion.

[50] The realized divorce outcome in Q1-Q2 ranges from 0.9–2.4 pp., while the Q3-Q5 outcomes range from 5.2–25.4 pp.

[51] The slight dip in effect magnitude for families with the highest divorce risk (Q5) may be an indication of non-linear effects by divorce risk, i.e. that the families where divorce risk is sufficiently high the parents respond less in terms of changing behavior since the marriage is beyond salvaging.

Effect on related outcomes

In order to provide a broader picture of children's outcomes later in life, a range of related outcomes for the same study sample is presented next. These results and the following are shown for the main specification with all children included.

The first set of related outcomes includes further education and labor market outcomes (university graduation, earnings, and employment status) in year 1990. The results of the OLS regressions can be seen in Table 5. University education for three years or more (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 (0.004, s.e. 0.001) in 1990 are all positive and significant, in line with the previous educational finding. Given that the youngest cohort is only age 26 when the outcomes are measured in the 1990 census, one concern could be that the reform causes children to re-time their university education and that this drives the result instead of long-run differences in educational attainment. However, the observed outcomes are of the same magnitude (university education) or even stronger (earnings, employment) when estimating the effects for the same outcomes ten years later in year 2000.[52] The effects on log earnings and employment are modest, which may be explained by the compressed wage structure and relatively low returns to education in Sweden, and that the employment outcome is measured at the peak of the business cycle in 1990 (mean employment rate for the reference group is 89.8%) (Harmon et al., 2000; Edin & Holmlund, 1993).

The quasi-experimental setting also allows me to investigate the extent to which family outcomes are transmitted across generations to the children themselves as adults. 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 (Amato, 1996; Teachman, 2002). The extent to which these effects also are transmitted by growing up under a divorce restriction is unclear ex ante, but could be similar. The results on family outcomes can be seen in Table 6. The findings indicate that exposed children are more likely to have ever married (0.008, s.e. 0.002) and less likely to have ever divorced (−0.005, s.e. 0.002) by year 2000, age 36–48. These results are validated in the 1990 census ten years prior (age 26–38), where the children are less likely to be single parents (−0.003, s.e. 0.001), and more likely to be married or cohabitating (0.007, s.e. 0.002) at that time. Delving deeper into the married/cohabitating outcome, it is clear that marriage is driving the observed effect, since the cohabitation outcome is precisely estimated at zero (0.000, s.e. 0.002). The effect on having young children at home is precisely measured at zero (−0.001, s.e. 0.002). However, this outcome is measured at young ages for some cohorts (age 26–38).[53] The effects on family outcomes are relatively large (1.3% greater chance of ever marrying,

[52] Estimating the same regressions around age 35 instead of year 1990 or 2000 gives stronger but qualitatively unchanged estimates for the labor market outcomes.

[53] As shown later in the paper, the effects on ever being a parent remains precisely estimated at zero when extending the

3.8% less risk of ever divorcing, and 2.6% less risk of being a single parent), which indicates that there is intergenerational transmission of marriage stability linked to the divorce restriction.

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

5.5 Parallel trends

As previously discussed, the identifying assumption needed for the identification strategy to give causal interpretation is that the effect of age spacing must be constant over cohort groups. If this assumption holds, the cohorts having graduated 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 7, 8 & 6. These figures show that the pre-trends of the main outcomes for cohorts born 1952–1955 are roughly constant for experiencing parental divorce by age 18 and upper secondary school completion by year 1990.[54]

Group composition changes

Another important robustness check related to the parallel trends assumption is about whether the composition of parents with children of different age spacing is constant over time. If, for instance, more educated parents increase the spacing of their children over time the validity of my identification strategy would be compromised. I verify that this is not a concern by predicting parental divorce by age 18 and upper secondary school completion for the children using family characteristics from before the policy was implemented. Due to data limitations, the earliest census information (1970) for the pre-characteristics is used.

Unfortunately, since children's age and spacing is expected to affect outcomes for parents already at this time, an imbalance is expected for e.g. parents' earnings and other observables already in 1970. This problem is solved by using the characteristics of grandparents to predict the outcomes of interest. The grandparents should be less directly affected by the age spacing of their grandchildren in 1970 in terms of their observables, but still be a reasonable proxy for family characteristics. The set of covariates

time period up to 2014.

[54] A potential exception being the cohort born 1955, which as discussed could be due to anticipation effects or direct effects of the policy.

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 13. Due to the nature of the MGR (the register covers parent-child linkages for individuals born 1932 and later), fewer grandparents are observable for the older cohorts. This is shown by the relatively wider confidence intervals for these groups. As time passes, more grandparents are captured by the data and the confidence intervals narrow. 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 experiencing parental divorce and upper secondary school completion.[55] Despite the caveat of missing grandparents, the predicted outcomes are shown to be relatively stable over time around zero. An F-test of joint significance of the coefficients for the affected cohorts gives the p-value of 0.437 for predicted parental divorce by 18 and 0.675 for predicted upper secondary school completion.

For completeness, Figure 14 shows the same predicted outcomes using parental and child characteristics in 1970, with the aforementioned caveats of direct effects from the age spacing affecting these results. Coviariates used to predict the outcomes include age of parents, sex, 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 based on these characteristics are shown to predict an increasing rate of experiencing parental divorce and a decreasing upper secondary school completion rate, in contrast with the observed effects in the opposite direction. These predictions perform similarly to the grandparental version, with the predicted Q1-Q4 actual difference in outcomes being 20–26 pp.[56] Thus, composition changes do not appear to be driving the observed effects.

Age spacing cutoff

First, I test whether the choice of treatment cutoff for the age spacing of children is driving 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 13.[57] The effects on schooling remain the same as 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

[55] For experiencing parental divorce by age 18, Q4 actual value 0.32, Q1 actual value 0.08. For upper secondary school completion, Q4 actual value is 0.907, Q1 actual value is 0.711. 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.

[56] The R^2 of the regressions range from 0.06–0.09.

[57] Further robustness tests on this note is presented Appendix B, Table B1. Including children with age spacing up to 18 years, and revising the age spacing assignment to 1973 is shown to moderately strengthen the main effect on educational outcomes.

results, and when the comparison group is limited to the narrowest age spacing (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). Broadly, this is to be expected since changing the cutoff in this direction makes the comparison groups more similar. When looking solely at elder siblings (spacing 1–8) and restricting the spacing, the estimate becomes more imprecise (2 against 3–4: 0.008, s.e. 0.004). However, the effect remains positive and is also significant for the most restrictive comparison (2 years of spacing against 3: 0.010, s.e. 0.05). This indicates that the relative effect of age spacing is robust and the strongest when including youngest siblings in the comparison group. A graphical representation of the effect on elder siblings compared to the youngest children can be seen in Figure 9a. The figure shows that the effect is strong and relatively stable for all choices of spacing cutoff, albeit somewhat weaker for spacing 2 and 5 years.

Placebo test - unmarried parents

Relating to the previous robustness check, the age spacing of children with unmarried parents in 1970 can be used as a placebo test for the estimated effects. This since the age spacing should not matter directly for this group due to the lack of a legal divorce friction. 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 directly affected by the divorce law reform.[58] Figure 15 displays upper secondary school completion for the placebo group with unmarried parents in 1970, comparing children with 3–8 years of age spacing against 0–2 as in the main specification. The figure exhibits wide confidence intervals for the oldest cohorts, but no clear pattern of improved schooling outcomes is visible.

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 test the robustness of the estimates, Table 14 presents the direct effect of experiencing divorce by age 18 on upper secondary school completion, an OLS family fixed effects model similar to previous research (e.g Chen et al., 2019; Björklund & Sundström, 2006), 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.[59] The direct effect of experiencing parental divorce

[58] As previously mentioned, the contamination of parents marrying after 1970 for the youngest cohorts in 1980 are as high as 24%.

[59] In Appendix B (Table B1), 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,

on secondary school completion is shown to be large and significant even when including controls of background characteristics (−0.075, s.e. 0.002), but reduces substantially when including family fixed effects to account for parts of the selection problem (−0.022, s.e. 0.008). The family fixed effects estimate is larger than the baseline estimate using age spacing for identification, but of a comparable magnitude. This finding contradicts earlier work on parental divorce using Swedish register data, which found null effects on children's educational outcomes using family fixed effects on a smaller, random sample of Swedish children experiencing divorce during childhood (Björklund & Sundström, 2006). However, that study uses a more imprecise measure of parental divorce (measured in the censuses every five years). Instead, my findings are more in line with studies in the French and Taiwanese context, which find effects of parental divorce on schooling outcomes before age 18 (Piketty, 2003; Chen et al., 2019).

Reassuringly, adding family fixed effects as a robustness check to the main specification based on age spacing leaves the estimates slightly stronger when including all children (0.017, s.e. 0.04). Similar to the baseline regression models, the identifying variation here stems from age spacing groups but restricts attention to variation within a given family. When looking solely at elder siblings, the estimate is larger relative to the main specification but more imprecise (0.013, s.e. 0.09). This could be explained by families with 3 or more children being used to identify the effect. The noisier point estimate for elder siblings remains positive and of comparable size to the other estimates, but is statistically insignificant (p-value 0.150).

I further test the robustness of the estimates by excluding all control variables related to background characteristics. The results can be seen in Table 14. Broadly, the estimates are slightly stronger when looking at 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 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 controls.

5.6 Heterogeneous treatment effects

In the following section, I present heterogeneous treatment effects for two categories related to previous research: sex of the child and parental earnings. Sociological research has found indications of divorce and marriage stability affecting boys more than girls, and a recent paper showed an increased divorce risk for parents with a daughter in the family (Kabátek & Ribar, 2020). The mechanisms behind boys being more sensitive are not clear, but it has been documented that boys are more prone to behavioral problems than girls and the literature has speculated that parental divorce may exacerbate this difference

the effect remains statistically significant despite adding these extensive controls.

(Amato, 2001; Kaye, 1989; Aggarwal, 2019).

Relating to the theoretical model and existing empirical evidence, parental earnings are a prime candidate to capture elements of within-household bargaining related to monetary resources (Voena, 2015; Fernández & Wong, 2014; Stevenson, 2007). 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.

Sex of the child

I investigate potential heterogeneous treatment effects of the main outcomes by fully interacting the previously specified model with an indicator for sex of the child (female). The results can be seen in Table 7. Contrary to Kabátek & Ribar (2020), I find no significant difference in parental divorce by sex of the child (0.001, s.e. 0.002), and thus provide no evidence of this phenomenon in the Swedish context. Although, it may be that the small magnitude of the effects in the original paper translates into an effect size too small to measure precisely with this policy reform. However, I find that girls are significantly less affected in terms of upper secondary school completion and exhibit a smaller increase in educational outcomes than boys (-0.012, s.e. 0.004). This effect is substantial. Also, the effects on being a single parent in 1990 appears to be entirely driven by the girls (-0.006, s.e. 0.003). The fact that women are driving this effect is reasonable given that women tend to get custody of the children following a separation. There is also some indication that the effects on log earnings and ever married are weaker and that the effects on ever divorce is stronger for girls, but these results are not statistically significant on conventional levels.

A graphical representation of the effects on upper secondary schooling split by sex of the child and educational attainment of the parent is shown in Figure 9b. The figure confirms that boys are affected more than girls, but also adds that the effects stem from children whose parents have at most upper secondary education. The main effects are thus not visible for those with parents at the top of the educational attainment distribution, who at this point in time number less than 10% of the population.

Parental earnings

Next, I investigate a channel related to within-household bargaining of the parents by fully interacting the regression model with an indicator for above median parental earnings (mothers and fathers separately) in 1970. Ex ante, I expect higher earnings to be related to greater bargaining strength for the parent. The

results of this split can be seen in Table 8. I find that parental divorce is precisely equal for children with the mother earning above and below the median (−0.000, s.e. 0.002), but children with the father earning above the median are significantly less likely to experience parental divorce (−0.007, s.e. 0.002) than the group with fathers earning below the median. Since husbands often were the primary breadwinners during the 1970s, this finding is consistent with couples using the greater financial resources to better take advantage of the stabilizing effects of the divorce restriction.

The opposite effect is found for upper secondary school completion, where the difference in effect with above median earnings is significant and negative for mothers (−0.010, s.e. 0.004), while insignificant and positive for fathers (0.005, s.e. 0.004). Greater attachment to the labor market for mothers could lead to less household investments following the reform, which in turn could reduce the benefits for the children. These results are generally consistent with the implications of the theoretical model and indicate that the reform affects families differently depending on pre-defined characteristics related to earnings and household specialization.

5.7 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. The prime mechanism to investigate and shed light on the policy's effect on spousal behavior would be a measure capturing within-household bargaining between spouses and parental investments in children. However, it is virtually impossible to directly capture such a measure in large-scale surveys. I proceed below by showing four tests that signal changes to within-household bargaining, and while none of these in isolation provide conclusive evidence of such changes, together they suggest that this mechanism could be important and a potential driver of the results.

Parental labor supply

The first mechanism relates to changes in parental labor supply. Previous research has focused on measuring responsiveness in spousal labor supply and interpreted this as a sign of changing bargaining between spouses. This strand of research has shown that changes to divorce laws can significantly affect labor supply of married women, and that shifting bargaining power can translate into more investments in children (Fernández & Wong, 2014; Stevenson, 2007; Ringdal & Sjursen, 2021). Using the data at hand, I investigate the effects on labor supply in this study population by looking at parental earnings and hours worked in the census of 1975, which is just after the divorce law reform in 1974. A change in labor supply could indicate changes to parental investments in children (assuming a saturated budget

constraint on activities), and is consistent with the theoretical framework presented in Section 3.3. The results in Table 9 show no significant effects on fathers' labor market outcomes (earnings −1.451, s.e. 1.260; employment 0.000, s.e. 0.001; hours worked 0.035, s.e. 0.054) in 1975 following the reform, but a substantial decrease in the mothers' earnings in SEK 100 (−16.348, s.e. 0.613), employment (−0.046, s.e. 0.002) and hours worked per week (−2.070, s.e. 0.069).

Fewer hours worked per week could be the result of an increase in parental investments from the mothers, and may explain the positive effects found on upper secondary school completion for the children. A concern here is that parts of the effect on mothers' labor market activities may be driven by their work life being more strongly linked to the age profile of their children than for fathers, and that this is picked up by the identification strategy. To alleviate some of these concerns, the regression model includes a control for age of youngest child and controls for the parent's pre-period labor market outcomes in 1970 (when the children were younger and the age profile effect should be even more prevalent), which leaves the estimates for mothers strongly significant. Notably, mothers reduce their weekly hours worked by on average 9% compared to the reference group average outcome, while the fathers leave their work hours unchanged. However, this result should be interpreted with caution since the controls may not fully capture the direct effect of the children's differing age profile.

Intergenerational transmission of human capital

The second mechanism provides additional evidence of increased parental investments following the reform by looking at the intergenerational correlation in in educational outcomes. This measure is widely accepted to capture persistence and intergeneral transmission of human capital between parents and children (Black et al., 2005).

The results when estimating the effects on the intergenerational education correlation are presented in Table 10. The findings show that the correlation increases significantly between children and their mothers following the reform (0.005, s.e. 0.001), while the link to the fathers remains insignificant or borderline significantly stronger (0.001–0.002, s.e. 0.001). The results remain stable for the mother-child link regardless of including child and parental controls from the 1970 census to the regression, while the precision and magnitude of the estimate for fathers increases somewhat with added controls.

Cognitive and non-cognitive development

The third mechanism uses the results from the conscription tests, which can supplement the educational findings and help shed light on non-cognitive and cognitive development around the ages affected by the divorce law reform. Previous research has shown that reforms targeting adolescents can persistently

improve the child's development, especially non-cognitive ability (Heckman, 2000).

The effects on standardized measures of abilities around age 18 can be seen in Table 11. In general, the estimates on both cognitive and non-cognitive ability are significant for the cohorts exposed to the reform, with exposure increasing ability by 0.027–0.050 standard deviations (0.027–0.050, s.e. 0.006–0.007). The effects on cognitive ability are in general stronger than those on non-cognitive ability, which is surprising given the evidence that cognitive ability more so than non-cognitive ability is mostly determined at relatively young ages. Within each ability group, the effects on logical thinking (0.050, s.e. 0.006) and emotional stability (0.033, s.e. 0.006) stand out as the strongest. The composite effect of non-cognitive ability is larger than the separate abilities (0.040, s.e. 0.006), and the same holds for cognitive ability (0.053, s.e. 0.006). To put these magnitudes in a context, the composite effects on cognitive and non-cognitive ability amount to roughly 30–40% of the effect stemming from birth order when comparing first to second born siblings (Black et al., 2018).[60]

The increase in ability is the strongest for the younger cohorts with longer exposure to the reform, but is also present when excluding the youngest cohorts born 1959–1964. For older cohorts, the effects on the components and composite terms are mostly insignificant and negative except for technical aptitude (0.017, s.e. 0.006), and non-cognitive ability which is significantly negative of a small magnitude (−0.010, s.e. 0.006). The results for non-cognitive (and cognitive) ability could help explain the observed effects on social outcomes (ever marrying, ever divorce) and other related outcomes during adulthood, since such skills have been shown to predict future success in e.g. the labor market (Lindqvist & Vestman, 2011). This claim will be investigated further in a mediation analysis presented later in the paper.

Timing of fertility

The fourth mechanism is the timing of fertility decisions, especially teenage parenthood. This mechanism is related to family stability, and could indicate risky behavior among adolescents and young adults relating to the findings on non-cognitive and cognitive development (Heckman et al., 2006). It is well-documented that parenthood at young ages is associated with poor economic and social outcomes for the parent and child (Kearney & Levine, 2012).

Delving deeper into this outcome in Table 12 using the MGR up to year 2014, the null effect on the fertility outcome is still observed even later in life (0.001, s.e. 0.002). This indicates that the long-term chance of being a parent is not affected by the reform. 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). Splitting the teenage parenthood outcome by sex, the risk of

[60] Alternatively, the effect on cognitive ability is consistent with the inverse effect of increasing class size by roughly 2 children (Fredriksson et al., 2013).

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 the girls.

The estimated results are large, with the relative effects being equivalent to a 18–38% reduction in the risk of teenage parenthood. The estimates are comparable in magnitude to the 20% reduced-form reduction of teenage motherhood found when evaluating the 1-year expansion of vocational upper secondary school programs in Sweden 1988–1990 (Grönqvist & Hall, 2013). Figure 11 shows graphical evidence that the reduced risk of early parenthood is U-shape starting at age 16–17, and is the strongest around age 18–19 only to reverse thereafter and become (insignificantly) positive at ages 24–25. The effects on older cohorts are generally significant for the fertility outcomes, although always of the opposite sign of the main effects estimated for the treated cohorts, indicative of pre-existing differences in fertility behavior between the cohort groups which is accounted for in the identification strategy. As mentioned, the fertility postponement may be an indication of the child's family situation during childhood being more stable, and that this allows parent to better steer their children away from early parenthood. This could have facilitated investments in schooling, and help explain the observed increase in schooling and improved labor market outcomes.

5.8 Mediation analysis

Trying 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) & Grönqvist et al. (2020) and decompose how much of the effects on related labor market outcomes (log earnings 1990) and family outcomes (ever married by 2000) that can be explained by changes to observed abilities (Panel A of Table 15) and from effects to upper secondary school and experiencing parental divorce (Panel B of Table 15). The final columns (9–11) in the table shows the relative contribution of the mediating factors and other residual factors to the total effect (normalized to 100%). The analysis in Panel A shows that non-cognitive ability explains a little more than a third of the effects (36%) of the reconsideration period on log earnings and ever married, while the cognitive effects only account for less than half of that effect (13–17%) and the residual accounts for around half of the total effect (47–51%) on the same outcomes. All in all, non-cognitive ability appears to be a more important factor in determining the effects on later outcomes than cognitive ability.

Panel B instead decomposes the effects on upper secondary school completion and experiencing parental divorce by age 18 on earnings and ever married. 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. Upper secondary schooling explains almost a quarter (24%) of the effects on earnings,

while experiencing parental divorce only accounts for 11%. For ever married, the effects from education appears to account for a similar share of the effects compared to experiencing parental divorce (12–13%), while the residual effect is large. It is somewhat surprising that upper secondary completion does not account for a larger share of the effects on earnings. This could indicate that the largest impact on the children stem from less salient effects on non-cognitive ability (e.g. social maturity and emotional stability) rather than direct and signaling effects of upper secondary school completion.

6. 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 extensive analysis of the divorce restriction shows persistent and positive effects on a broad range of long-term outcomes for the children, in particular the boys, related to restricted parental divorce. In particular, the family outcomes paint the picture that the policy spilled over on the children's own family behavior later in life, providing evidence that exposure to policy transmits across generations. The evaluation of the liberalization indicates that the reform may have had negative effects on children of married parents relative to their counterparts with unmarried parents.

The substantial magnitude of the effects on children's outcomes raises the question to which extent the effects stem from divorces, or if they mostly are driven by changes parental behavior. In line with the arguments presented by Gruber (2004), the relatively similar magnitudes on experiencing parental divorce and increase in upper secondary school graduation indicate that much of the effects run through within-marriage behavior than through divorces. The mediation analysis in Table 15 supports this claim, with effects on primarily non-cognitive ability accounting for a larger share of the effects on later labor market and family outcomes than upper secondary school completion and experiencing parental divorce by age 18. Evidence from parents' labor supply in 1975 further corroborates this and points to mothers reducing their hours worked in response to the policy, while no such change can be seen for fathers. Related to the theoretical framework, such a change could indicate 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. Further, 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 into the mechanisms potentially related to bargaining and labor supply, the heterogeneity results related to above and below median earnings of parents indicate that children with mothers earning above the median in 1970 were equally likely to divorce compared to those below the median. Contrarily,

children with fathers earning above the median are less likely to experience divorce. With some speculation, one could imagine that fathers with high earnings are able to compensate a divorcing spouse following the reform, while those earning less are not able to prevent a divorce through redistribution of resources. This capability of economic compensation may mean less when the wife is earning above the median, since husbands at this time tended to be the main breadwinner. In terms of upper secondary school completion, the effects are substantially weaker when mothers are earning above the median and indicate that the benefits of the divorce restriction on children's schooling are weaker here. Possibly, this could be due to working mothers being less prone to shift toward parental investments. For fathers earning above the median, the effect is positive but insignificant. Exploratory analysis of parental labor supply in 1975 reveals that specialization increased more in households where the wife was earning below the median in 1970. Better data on parental investments would help substantiate this last claim and explain why the mothers with below median earnings exhibit larger effects on children's schooling.

For the divorce restriction, the increase in upper secondary school graduation rate of 1.5 pp (1.8%) translates into an effect of about 0.8 pp. (7.3%) for university graduation. However, these average effects also contain the weaker effects of partially treated cohorts with few years of exposure. Focusing on the effects for the very youngest cohorts (born 1963–1964) with the most years of exposure reveals larger treatment effects of about 3% for upper secondary school completion and 10% for university graduation.[61] The findings of this study also indicate that the divorce liberalization on average decreased upper secondary school graduation by 5.6%. Comparing these estimates estimate to the previous literature, the magnitudes are broadly similar despite being different reforms. Gruber (2004) estimates that the effect of exposure to unilateral divorce results in a 1.5 pp. (6.5%) reduced chance of being a college graduate. Contrarily, Heggeness (2020) finds that legalizing divorce increases upper secondary school enrolment by 5.1–9.0 pp. (5.5–9.8%), and that an additional 6 months of divorce court congestion reduces secondary schooling enrolment by 1.7 pp. (1.9%). These results are broadly similar in magnitude, although the study by Heggeness is of the opposite sign. The findings highlight the differences in effects from the reform direction, and how the interpretation and effects of these on children may differ due to the setting.

Thus, the key takeaway from previous literature is that the effect of divorce law reform on children's outcomes are highly likely to be dependent on the direction of the reform, the institutional setting, and the marginal respondents targeted. With this in mind, there are some 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 also legalized divorce at the time of the study evaluation and simultaneously transferred bargaining power to

[61] Exploratory analysis of even younger cohorts' outcomes shows that the effect on upper secondary school completion reaches its peak and levels off at around 4% starting with the cohort born 1967–.

the mothers. The respondents of this reform may thus be couples with a substantially negative influence on the children who are held up in court, thus accentuating the within-family conflict and turmoil. For Gruber's study, the setting is a wealthy country in the 1970's and onward with the marginal divorces being couples that respond to unilateral divorce. It is possible that this kind of a policy accentuates conflict when allowing one spouse to unilaterally seek a divorce without needing the explicit consent of the other spouse. For this study, the specific setting is a wealthy country with the respondents being marginal divorcees and marriages, where a divorce restriction may positively affect marital behavior and potentially reduce more harmful divorce shocks on children than marital instability from e.g. abusive parents with substantial discord and low marriage value. With this policy, the children of these "marginal marriages" experience less marital instability and changes to within-household bargaining for parents with a relatively functioning marriage. Thus, the potential upside of experiencing less parental divorce or turmoil from bargaining may be net positive for this kind of a divorce restriction.

The overall takeaway from this paper is that an increase in marital stability could improve the schooling outcomes of children, and that restricting divorce for couples close to a "break-even" marriage protects their children from experiencing a net harmful parental divorce and potentially increases parental investments, which benefits the children's long-term outcomes.

7. Conclusion

This study investigates the effects of the Swedish divorce law reform implemented in 1974 on children's long-term outcomes. The reform consisted of a general liberalization of the existing divorce laws and the implementation of 6 months of parental reconsideration for divorce. While much of the previous evidence on the effects of divorce on children are plagued by endogeneity concerns, this study uses a novel identification strategy where marriage status of parents, cohort, and variation in exposure to the reform elements are used for plausibly exogenous identification.

Using a DiD-related specification exploiting age spacing of siblings and cohort variation in exposure to the policy, I find a substantial and positive effects on children's long-term outcomes related to the divorce law reform. The divorce liberalization appears to have converged the difference in observed schooling outcomes between children of unmarried and married parents, to the children of unmarried parents' advantage. Composition changes of the unmarried and married parental groups can be ruled out as an alternative explanation for this convergence, since the composition changes predict stable differences between these groups. Evaluating the divorce restriction, the findings show a clear reduction in the risk of experiencing parental divorce during childhood, the chance of graduating upper secondary school, and

also related outcomes such as earnings, ever marrying, and marital stability as adults. The mechanisms related to within-household bargaining provide an indication that the mothers, contrarily to the fathers, reduce their labor market attachment and hours worked substantially in response to the policy. In tandem with this, the link to mothers' educational attainment and the child's own educational attainment later in life increases, and the cognitive and non-cognitive ability of the child increases.

The magnitude of the effects, and the evidence presented on parental labor supply around the time of the policy change indicate that both parental divorce and changes to within-household bargaining contribute to the long-term effects on children's outcomes. The findings are robust to a range of tests, including alternative age spacing group definitions, group composition checks of grandparental and parental characteristics, and the inclusion of family fixed effects. The main limitation of the paper is that more direct measures of parental investments are needed to better understand the complex mechanisms behind the observed effects of divorce law reform on children's outcomes. Future research should further attempt to open the black box of parental behavior affecting children's outcomes. Relating to previous work by Gruber (2004), the results presented in this paper adds evidence of trade-offs between freedom of choice for parents and externalities on third parties, such as children. All in all, the findings indicate that responses to the divorce law reform are substantial, with parts of the effects likely running through both parental divorce and changes to within-household bargaining. Future policy makers should internalize the broader effects of divorce restrictions on children when formulating policies related to marriage stability.

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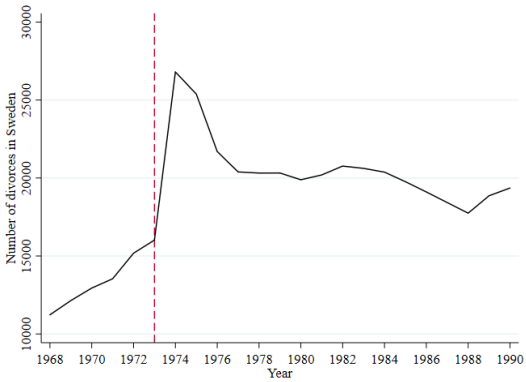
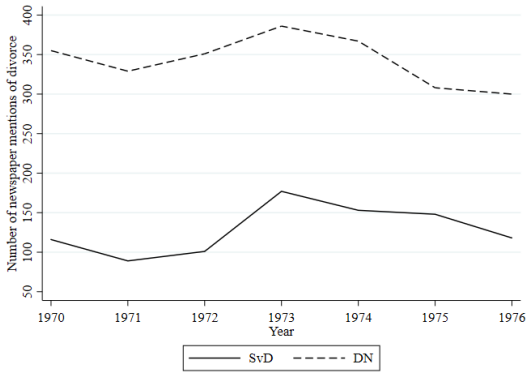
Figures and tables

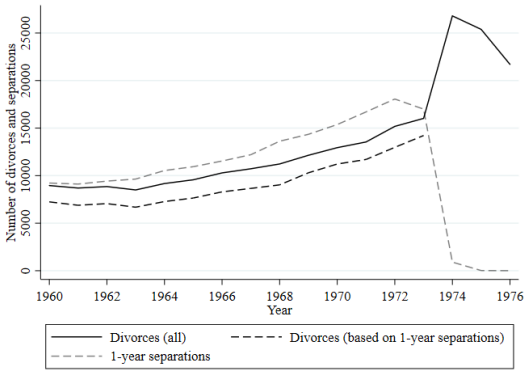
Supporting figures



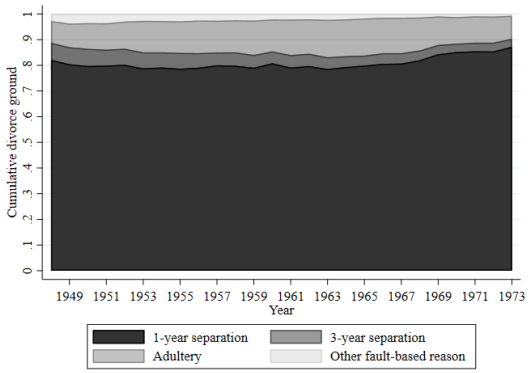
(a) Number of divorces in Sweden 1968–1990.



(b) Number of mentions of "divorce" in the leading morning newspapers in Sweden 1970–1976.



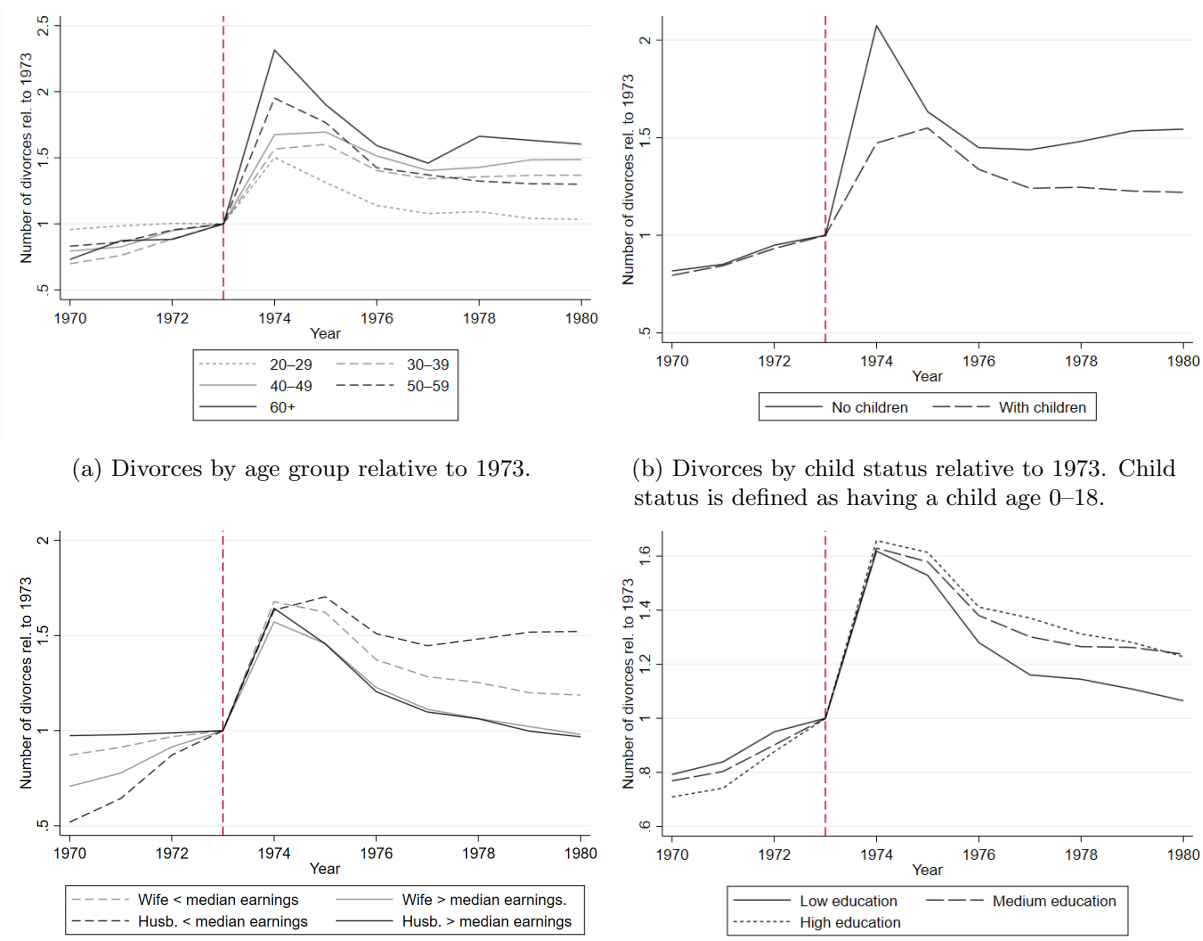
(c) Divorces and 1-year separations 1960–1976.



(d) Cumulative divorce reason shares 1948–1973.

Figure 2: Figure 2a shows the number of divorces in Sweden around the time of the divorce law reform in 1974. Figure 2b shows number of mentions of the word "divorce" in the two largest morning newspapers around the time of the reform. Figure 2c shows a stable relationship between 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 separation to applicants until 30 June 1975 if the application was submitted before 1 Jan 1974. Figure 2d 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, thus ending the time series.

Source: Statistics Sweden and the historical archives of DN and SvD.



(a) Divorces by age group relative to 1973.

(b) Divorces by child status relative to 1973. Child status is defined as having a child age 0–18.

(c) Divorces above and below median earnings in 1970, men and women separately relative to 1973.

(d) Divorces by educational attainment 1970 relative to 1973. Low education is defined as primary school education, medium education as upper secondary school education, and high education includes university education.

Figure 3: Divorce responses by pre-reform characteristics: Age, earnings, child status, and education. The red line marks the last year before the new divorce policy. All changes are relative to 1973 before the new policy. The figures indicate that older couples and couples without children responded more to the new divorce law in 1974.

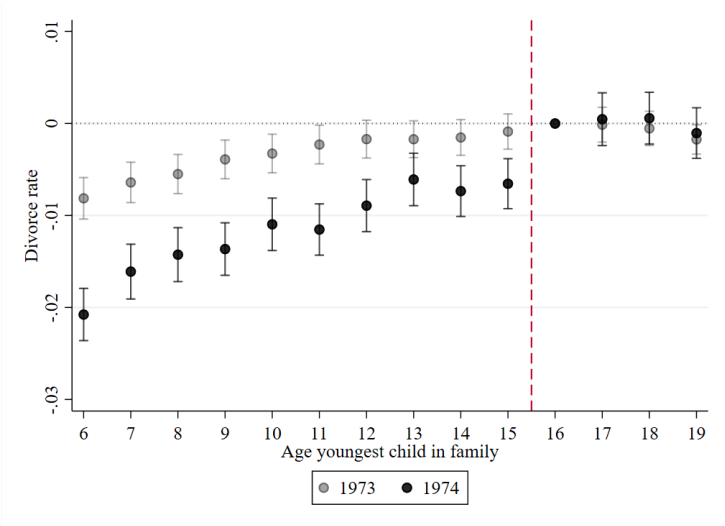


Figure 4: Parental divorce incidence 1973 and 1974 separately, sorted by age of the youngest child in the family. The reference age 16 is indicated by the dashed red line for both years. Average baseline divorce risk is 1.3% in 1973 and 2.1% in 1974. Estimations include parental age fixed effects and an indicator for sex. CI95 are indicated in black, and standard errors are clustered at the household level.

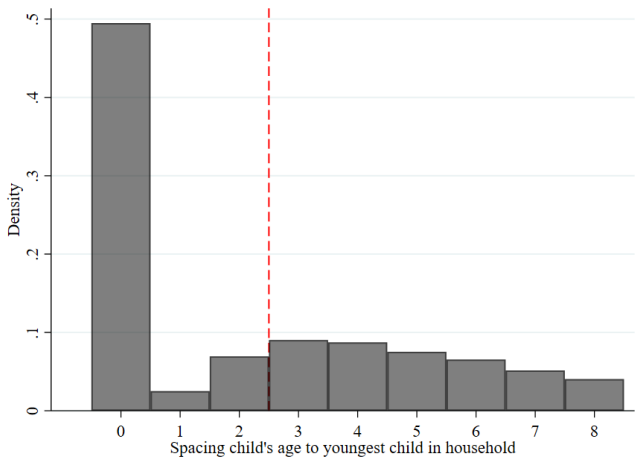


Figure 5: Histogram displaying mass of children in each spacing bin. The figure shows the distribution for the main estimation sample - cohorts born 1952–1964, spacing 0–8. The dashed red line marks the age spacing cutoff used for the main specification (spacing 0–2 against 3–8).

Outcome figures

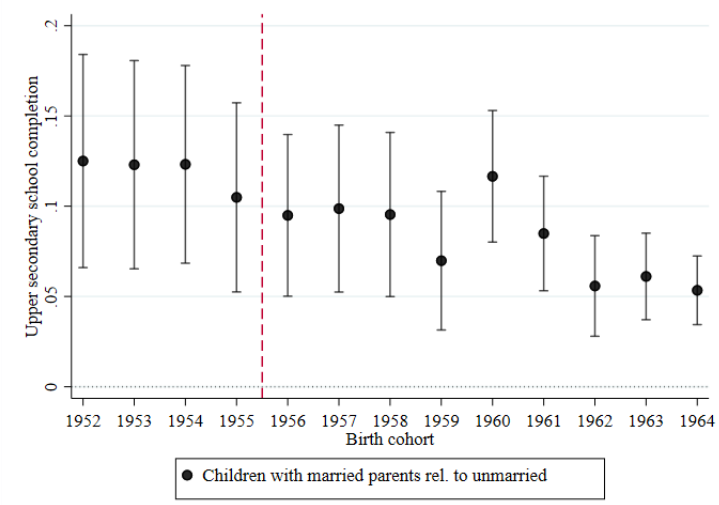
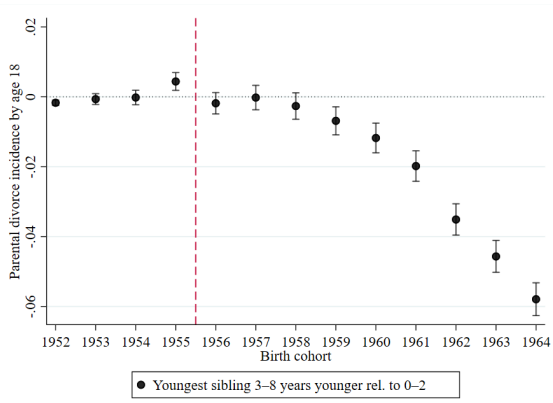
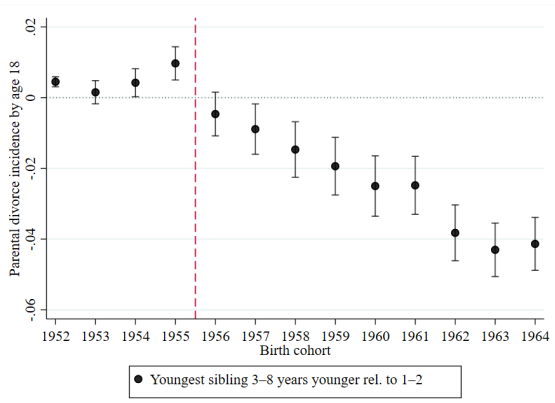


Figure 6: Difference in upper secondary school completion rate measured in the 1990 census. The figure shows the outcome for children of married parents relative to children of unmarried parents. Parental marriage status is determined in 1970. The controls include age of parents, municipality of residence in 1970, labor market outcomes and education of parents in 1970, sex, birth month, and indicators of missing values. CI95 are indicated in black, and standard errors are clustered at the household level.

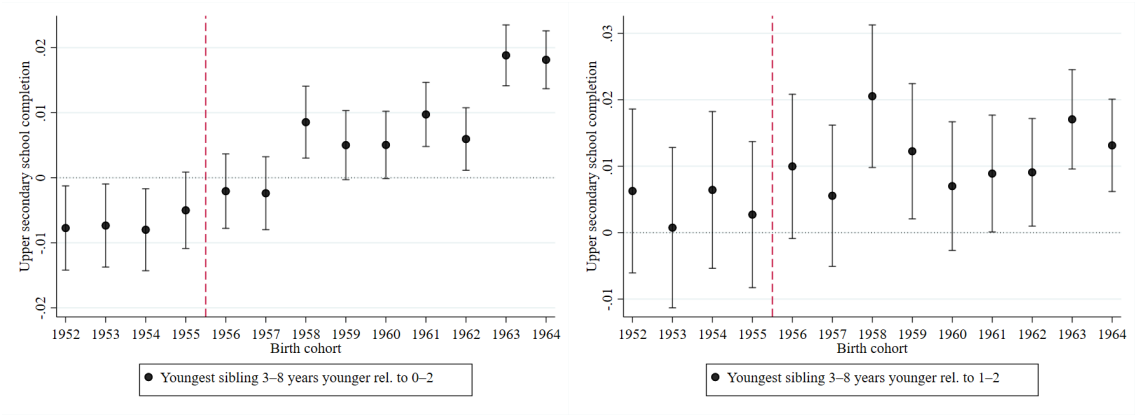


(a) Difference in parental divorce rate by age 18, youngest sib. 3–8 years younger rel. to 0–2.



(b) Diff. in parental divorce rate by age 18, youngest sib. 3–8 years younger rel. to 1–2 (excl. youngest sibs.).

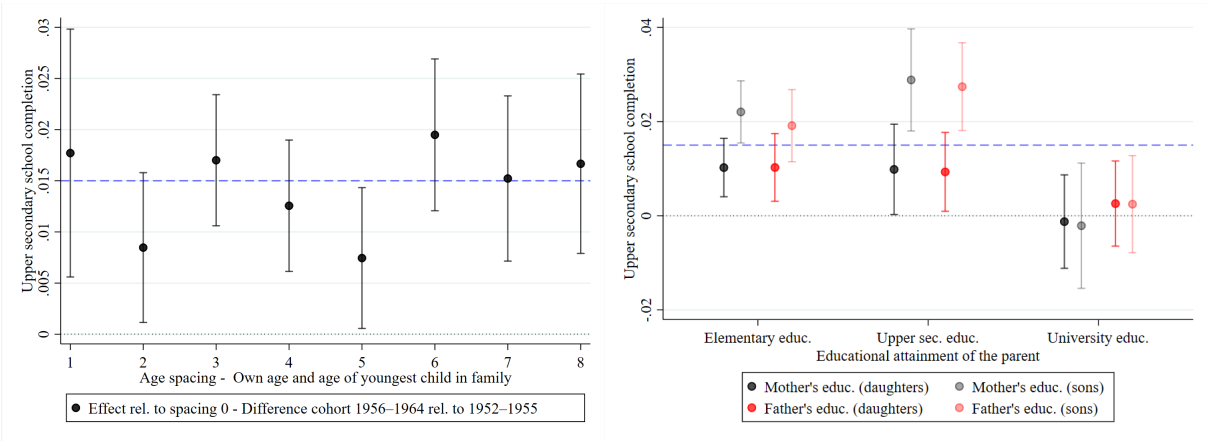
Figure 7: Figure 7a follows cohorts born 1952–1964 until age 18 separately by birth cohort and estimates the difference in parental divorce rate between the large age spacing group (3–8) against the smaller age spacing group (0–2). Figure 7b does the same while excluding youngest siblings themselves from the reference group (spacing 0), thus estimating the difference in outcome between spacing 3–8 and 1–2. The controls include age of parents, municipality of residence in 1970, labor market outcomes and education of parents in 1970, sex, birth month, and indicators of missing values. CI95 are indicated in black, and standard errors are clustered at the household level.



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

(b) Diff. in upper secondary school completion rate, youngest sib. 3–8 years younger rel. to 1–2 years (excl. youngest siblings).

Figure 8: Figure 8a follows cohorts born 1952–1964 separately by birth cohort and estimates the difference in upper secondary school graduation rate by 1990 between the large age spacing group (3–8) against the smaller age spacing group (0–2). Figure 8b does the same while excluding youngest siblings themselves from the reference group (spacing 0), thus estimating the difference in outcome between spacing 3–8 and 1–2. The controls include age of parents, municipality of residence in 1970, labor market outcomes and education of parents in 1970, sex, birth month, and indicators of missing values. CI95 are indicated in black, and standard errors are clustered at the household level.



(a) Effects on upper secondary schooling by each spacing category separately in relation to the effects on youngest siblings (spacing 0). The figure is estimated in a joint regression comparing each spacing against youngest siblings.

(b) Effects on upper secondary schooling by parental education in 1970 and sex of the child from separate regressions. The dashed blue line indicates the baseline effect estimated when pooling the categories.

Figure 9: The figures split the effects on upper secondary school completion by age spacing, parent's educational attainment, and sex. The controls include age of parents, municipality of residence in 1970, labor market outcomes and educational attainment of the parents in 1970, sex (excluding sex and education of the relevant parent in Figure 9b), birth month, and indicators of missing values. CI95 are indicated in black, and standard errors are clustered at the household level.
Source: Own manipulation of RTB data

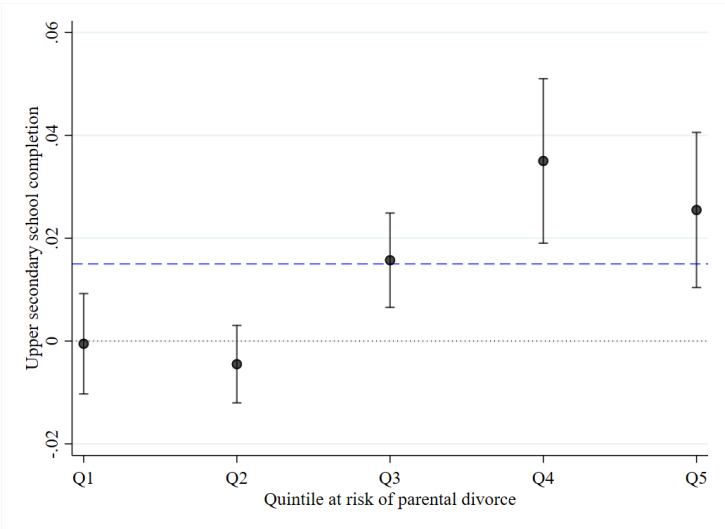


Figure 10: The figure splits the effects on upper secondary school completion by predicted quintile of experiencing parental divorce, based on background information (education and labor market outcomes, municipality of residence) and family characteristics (age of parents, number of children, birth month, sex, family status, and age of youngest sibling) from the 1970 census. The regressions are then run separately by quintile of predicted parental divorce by age 18, and the controls include the standard ones: age of parents, municipality of residence in 1970, labor market outcomes and education of parents in 1970, sex, and birth month. The dashed blue line marks the baseline estimate for upper secondary school completion presented in the paper, which is equivalent to pooling the quintiles. The underlying prediction of divorce by age 18 produces a Q5–Q1 realized divorce difference of 25 pp (Q1 actual divorce rate is 0.86 pp., and Q5 divorce rate is 25.42 pp.) with an R^2 of 0.145. CI95 are indicated in black, and standard errors are clustered at the household level.

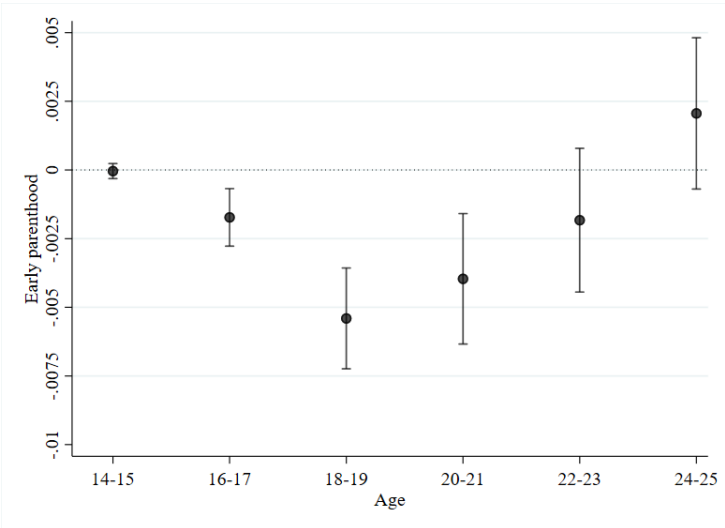


Figure 11: Effects on early parenthood, split by age of becoming a parent. The figure shows effects estimated by separate regressions. The controls include age of parents, municipality of residence in 1970, labor market outcomes and education of parents in 1970, sex, and birth month. CI95 are indicated in black, and standard errors are clustered at the household level.

Predicted outcome figures

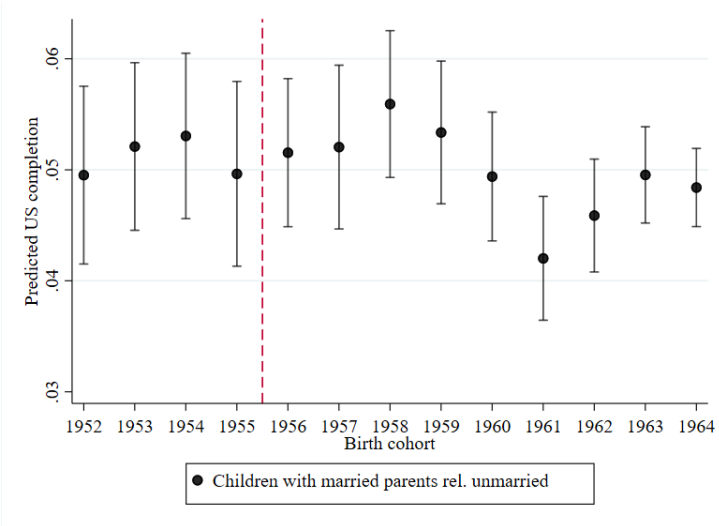
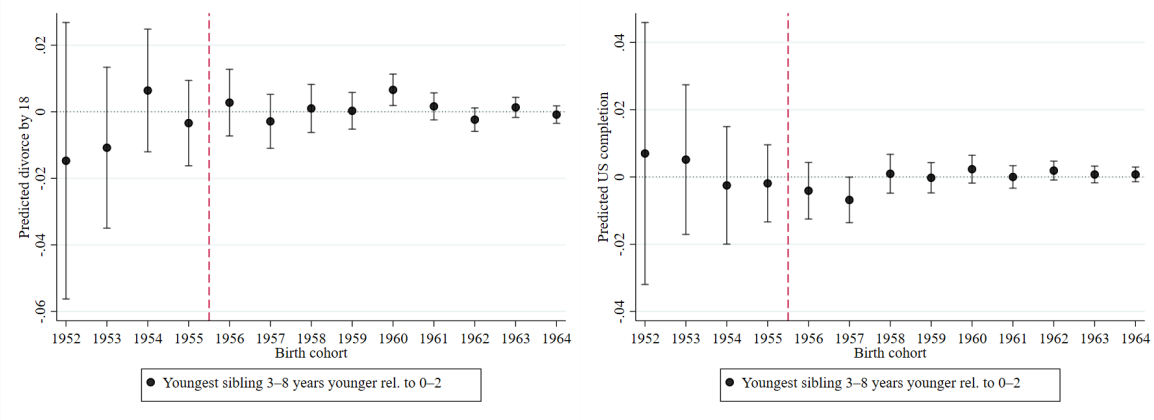


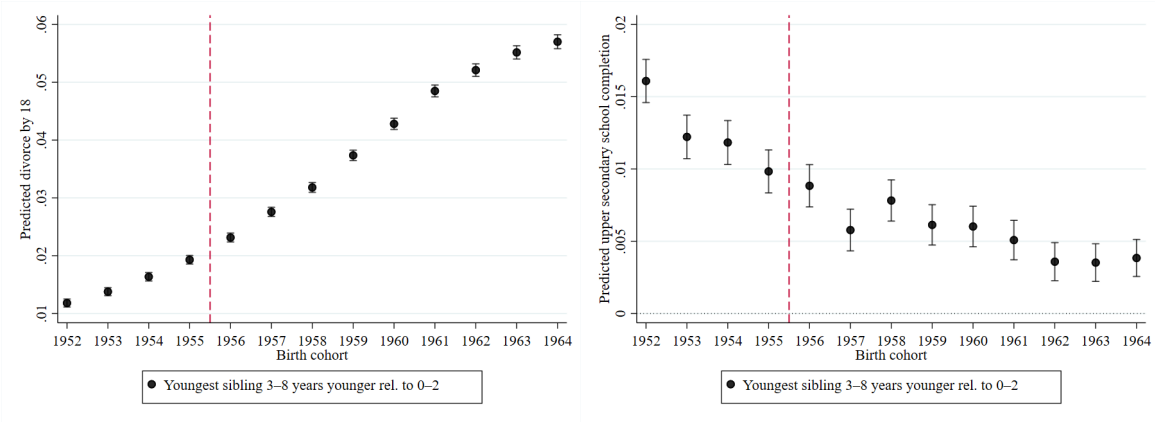
Figure 12: Difference in predicted upper secondary school completion rate for children of married parents relative to children of unmarried parents. Parental characteristics included for the predicted outcome are age, municipality of residence, educational attainment, and labor market outcomes in 1970. Parental marriage status is determined in 1970. 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 13: Figure 13a uses predicted parental divorce by 18 outcomes from grandparental characteristics in 1970 (a set of coviariates capturing socioeconomic status - earnings, educational attainment, family type, hours worked and municipality of residence) estimates the difference in parental divorce rate between the large age spacing group (3–8) against the smaller age spacing group (0–2). Figure 13b 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.



(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 14: Figure 14a uses predicted parental divorce by 18 outcomes from parental and child characteristics in 1970 - with the caveat that some parental characteristics risk being imbalanced due to direct effects of child age spacing. The characteristics include a set of coviariates capturing age, earnings, educational attainment, hours worked of the parents, along with birth month, municipality of residence, and sex of the child. Figure 14b does the same when predicting the upper secondary school outcomes. CI95 are indicated in black, and standard errors are clustered at the household level.

Placebo figure

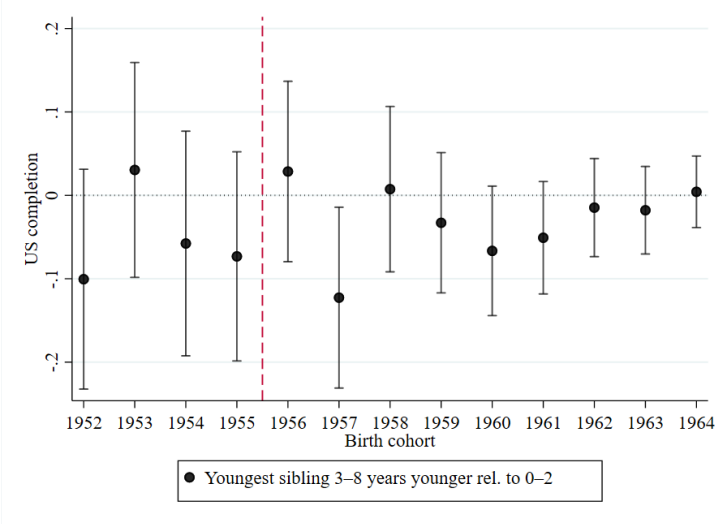


Figure 15: Upper secondary school completion for the placebo group with unmarried parents in 1970. The controls include parent cohort effects, municipality of residence in 1970, labor market outcomes and education of parents in 1970, sex, birth month, and indicators of missing values. There is a risk of contamination from unmarried parents marrying after 1970, especially for the youngest cohorts, and becoming directly exposed to the reform. A test of equality between the predicted outcomes 1952–1955 against 1956–1964 gives the p-value 0.538. CI95 are indicated in black, standard errors are clustered at the household level.

Result tables

Table 1: Descriptive statistics 1970 for children in the comparison groups, by cohort and age spacing/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]
US educ. mother	.310	.286	(.024)	.243	.216	(.027)	(–.003)	[.181]
US educ. father	.435	.409	(.026)	.376	.346	(.030)	(–.005)	[.058]
Earnings grandfather 1970	212.386	204.353	(8.033)	164.144	160.427	(3.717)	(4.316)	[.114]
Hours worked grandf.	23.103	22.361	(.742)	17.374	16.817	(.557)	(.185)	[.615]
US educ. grandfather	.056	.051	(.005)	.014	.012	(.003)	(.002)	[.305]
Share married grandf.	.856	.850	(.006)	.818	.819	(–.001)	(.007)	[.330]
Earnings grandmother 1970	69.756	69.582	(.174)	61.633	61.735	(–.102)	(.276)	[.822]
Hours worked grandm.	9.947	9.664	(.284)	7.329	7.027	(.302)	(–.018)	[.938]
US educ. grandmother	.042	.037	(.005)	.013	.016	(–.003)	(.008)	[.000]
Share married grandm.	.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)	(4.65)	[.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. *The age spacing information is measured in 1973 to ensure that the birth cohorts 1963–1964 also have the same potential range of age spacing values (0–8).

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

	US compl.	Univ. grad.	Log. earn.	Empl.	Cog. ability	NC ability
Married$_i$× 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	Yes	Yes	Yes	Yes	Yes	Yes
Parent cohort FE	Yes	Yes	Yes	Yes	Yes	Yes

Note: ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$. Standard errors in parenthesis are clustered at the household level. "US compl." is defined as upper secondary education of two years or more, or any higher education in 1990. "Univ. grad." is defined as at least three years of university education in 1990. "Log. earn." and "Empl." are defined as the natural logarithm of earnings and employment status in 1990. "Cog. ability" and "NC 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, birth month, and indicators of missing values. "Mean dep. var." refers to mean dependent variable for the category with married parents born 1956–1964.

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

	All children			Elder sibs.		
	Div. by 18	Div. in 15 y.	Fath. multip.	Div. by 18	Div. in 15 y.	Fath. multip.
Insulation$_i$× Cohort$_i \geq 1956$	-0.022***	-0.035***	-0.007***	-0.033***	-0.030***	-0.007***
	(0.001)	(0.002)	(0.001)	(0.002)	(0.003)	(0.001)
Insulation$_i$	-0.000	0.018***	0.001***	0.005***	0.003	0.001**
	(0.001)	(0.001)	(0.000)	(0.001)	(0.002)	(0.001)
Mean dep. var.	0.120	0.167	0.024	0.164	0.231	0.041
Obs.	1,168,874	1,168,874	1,148,691	601,711	601,711	589,708
Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
Parent cohort FE	Yes	Yes	Yes	Yes	Yes	Yes

Note: ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$. Standard errors in parenthesis are clustered at the household level. "Elder sibs." excludes youngest siblings (spacing 0). "Div. by 18" is an indicator for experiencing parental divorce by age 18. "Div. in 15 y." changes the indicator to experiencing parental divorce in 15 years from 1970. "Fath. multip." is an indicator capturing multipartner fertility by the father from 1975 (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, birth month, and indicators of missing values. "Mean dep. var." refers to mean dependent variable for the reference category with age spacing 0–2.

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

	All children	Elder sibs.
	Upper sec.	completion
Insulation$_i$× Cohort$_i \geq 1956$	0.015***	0.008**
	(0.002)	(0.003)
Insulation$_i$	−0.007***	0.004
	(0.002)	(0.003)
Mean dep. var.	0.825	0.815
Obs.	1,073,396	549,271
Cohort FE	Yes	Yes
Parent cohort FE	Yes	Yes

Note: ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$. Standard errors in parenthesis are clustered at the household level. "Elder sibs." 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. "Insulation$_i$" indicates greater age spacing (3–8 years to youngest sibling against 0–2 years). The interaction with cohort shows the difference 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, birth month, and indicators of missing values. "Mean dep. var." refers to mean dependent variable for the reference category with age spacing 0–2.

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

	Univ. grad.	Earnings	Log earnings	Employment
Insulation$_i$× 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	Yes	Yes	Yes	Yes
Parent cohort FE	Yes	Yes	Yes	Yes

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. "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, birth month, and indicators of missing values. "Mean dep. var." refers to mean dependent variable for the reference category with age spacing 0–2.

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

	Year 2000		Census 1990			
	Ever married	Ever divorced	Single parent	Marr./Cohab.	Cohab.	Young child
Insulation$_i$× 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	Yes	Yes	Yes	Yes	Yes	Yes
Parent cohort FE	Yes	Yes	Yes	Yes	Yes	Yes

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 cohabitating or being married, "Cohab." is defined as cohabitating without being married, and "Young child" is defined as having a child age 0–6 at the same year. "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, birth month, and indicators of missing values. "Mean dep. var." refers to mean dependent variable for the reference category with age spacing 0–2.

Table 7: Effect of a 6-month parental reconsideration period for divorce for various outcomes by sex.

	Parental div.	US compl.	Log earn.	Ever marr.	Ever div.	Single par.
Insulation$_i$× Cohort$_i$× Female$_i$	0.001	−0.012***	−0.005	−0.005	−0.005	−0.006**
	(0.002)	(0.004)	(0.006)	(0.004)	(0.003)	(0.003)
Insulation$_i$× Cohort$_i \geq 1956$	−0.022***	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.	0.123	0.848	6.853	0.655	0.154	0.222
Obs.	1,168,874	1,073,396	1,099,917	1,120,451	1,120,451	1,069,027
Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
Parent cohort FE	Yes	Yes	Yes	Yes	Yes	Yes

Note: ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$. Standard errors in parenthesis are clustered at the household level. "Parental div." refers to experiencing parental divorce by age 18. "US compl." is defined as upper secondary education less or equal to three years. "Log earn." is the natural logarithm of earnings in 1990. "Ever marr." and "Ever div." refers to ever marrying or divorcing by year 2000. "Single par." 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. "Insul$_i$× Cohort$_i$×Female$_i$" captures the difference in effect between women and men and indicates a model fully interacted by sex. Besides cohort and parent cohort effects, the controls include age of parents, municipality of residence in 1970, labor market outcomes and education of parents in 1970, sex, birth month, and indicators of missing values. "Mean dep. var." refers to mean dependent variable for women in the reference category with age spacing 0–2.

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

	Mothers' earnings 1970		Fathers' earnings 1970	
	Parental div.	US compl.	Parental div.	US compl.
Insulation$_i$× Cohort$_i$ × Earnings$_{mother1970}$	−0.000	−0.010***		
	(0.002)	(0.004)		
Insulation$_i$× Cohort$_i$ × Earnings$_{father1970}$			−0.007***	0.005
			(0.002)	(0.004)
Insulation$_i$× Cohort$_i$ ≥ 1956	−0.017***	0.016***	−0.021***	0.010***
	(0.001)	(0.003)	(0.001)	(0.003)
Mean dep. var.	0.097	0.822	0.134	0.778
Obs.	1,168,874	1,073,396	1,168,874	1,073,396
Cohort FE	Yes	Yes	Yes	Yes
Parent cohort FE	Yes	Yes	Yes	Yes

Note: ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$. Standard errors in parenthesis are clustered at the household level. "Parental div." refers to experiencing parental divorce by age 18. "US compl." 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). "Insul$_i$× Cohort$_i$× 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. Besides cohort and parent cohort effects, the controls include municipality of residence in 1970, education of parents in 1970, sex, birth month, and indicators of missing values. "Mean dep. var." refers to mean dependent variable for those with below median earnings in the reference category with age spacing 0–2.

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

	Fathers 1975			Mothers 1975		
	Earnings	Empl.	Hours	Earnings	Empl.	Hours
Insulation$_i$ × Cohort$_i$ ≥ 1956	−1.451	0.000	0.035	−16.348***	−0.046***	−2.070***
	(1.260)	(0.001)	(0.054)	(0.613)	(0.002)	(0.069)
Insulation$_i$	6.713***	0.006***	0.296***	13.878***	0.051***	1.973***
	(1.514)	(0.001)	(0.060)	(0.717)	(0.002)	(0.080)
Mean dep. var.	537.670	0.929	36.395	217.520	0.746	22.860
Obs.	1,106,060	1,097,141	1,097,141	1,139,434	1,133,426	1,133,426
Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
Parent cohort FE	Yes	Yes	Yes	Yes	Yes	Yes

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 (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, 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.

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

	Mothers			Fathers		
	IGE	IGE	IGE	IGE	IGE	IGE
Insulation$_i$× Cohort$_i \geq 1956$ × 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$_i$ × 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	Yes	Yes	Yes	Yes	Yes	Yes
Child controls		Yes	Yes		Yes	Yes
Other (parental) controls			Yes			Yes

Note: ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$. The outcome is defined as child's length of education in years 1990, which is regressed on the mother's or father's education. Standard errors in parenthesis are clustered at the household level. "Educ" denotes educational outcome of the parent in 1970. "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. Cohort, parent cohort effects and indicators of missing values are always included. "Child controls" include municipality of residence in 1970, sex, 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.

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

	Non-cognitive abilities				Cognitive abilities			
	NCA composite				CA composite			
Insul$_i$× Cohort$_i \geq 1956$	0.040***				0.053***			
	(0.006)				(0.006)			
Insulation$_i$	−0.010*				−0.004			
	(0.006)				(0.005)			
	Maturity	Intensity	Ps. energy	Stability	Logic	Verbal	Spatial	Technical
Insul$_i$× 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

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. "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, birth month, and indicators of missing values.

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

	Fertility	Age first child	Teen parent	Teen mother	Teen father
Insulation$_i$× 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	Yes	Yes	Yes	Yes	Yes
Parent cohort FE	Yes	Yes	Yes	Yes	Yes

Note: ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$. Standard errors in parenthesis are clustered at the household level. "Fertility" and "Age first child" refers to being a parent by year 2014 (the final year of the MGR 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. "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, birth month, and indicators of missing values. "Mean dep. var." refers to mean dependent variable for the reference category with age spacing 0–2.

Table 13: Robustness check: Effect of a 6-month parental reconsideration period for divorce on upper secondary school completion.

	Age spacing cutoff and restriction				Elder sibs.	
	Sp. 0–1, 2–8	0, 1–8	0, 1–3	0–3, 4–8	2, 3–4	2, 3
	US completion				US completion	
Insulation$_i$× 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	Yes	Yes	Yes	Yes	Yes	Yes
Parent cohort FE	Yes	Yes	Yes	Yes	Yes	Yes

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, where the cutoff varies by column. 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, birth month, and indicators of missing values. "Mean dep. var." refers to mean dependent variable for the reference category with age spacing 0–2.

Table 14: 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.

	Direct effect		Family FE		Excl. controls	
	All children	All children	All children	Elder sibs.	All children	Elder sibs.
	US completion		US completion		US completion	
Parental div. by 18	−0.075***	−0.022***				
	(0.002)	(0.008)				
Insulation$_i$× Cohort$_i$ ≥ 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.	0.811	0.811	0.825	0.815	0.825	0.815
Obs.	1,073,396	1,073,396	1,073,396	549,271	1,073,396	549,271
Age FE	Yes				Yes	Yes
Linear controls		Yes	Yes	Yes		
Family FE		Yes	Yes	Yes		

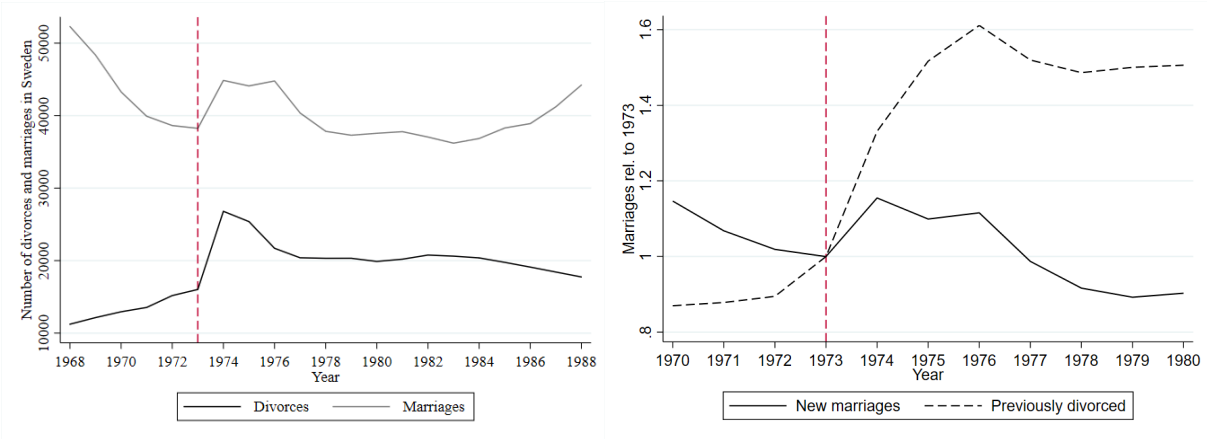
Note: ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$. Standard errors in parenthesis are clustered at the household level. "Par. div. by 18" refers to experiencing parental divorce by age 18. "Insulation$_i$" indicates greater age spacing (3–8 years to youngest sibling against 0–2 years). The interaction with 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, birth month, and indicators of missing values. "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 15: 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)
Outcome	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
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)
Outcome	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
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 used to represent the mediation analysis, following Grönqvist et al. (2017). "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.

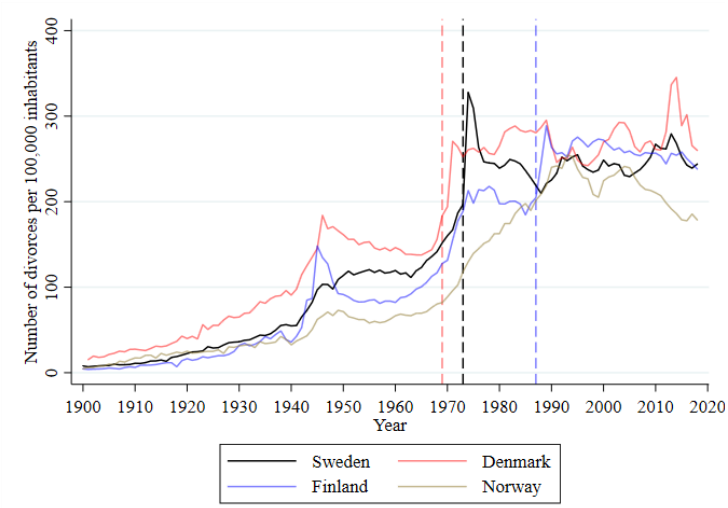
Appendix A: Supporting figures



(a) Number of divorces and marriages in Sweden 1968–1988.

(b) New marriages and marriages between previously divorced in 1970–1980, relative to 1973.

Figure A1: General equilibrium effects of the divorce policy on divorces and marriages. "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. Figure A1a shows number of divorces and marriages over time in levels. Figure A1b shows marriages relative to 1973, split by previous civil state.



(a) Number of divorces per 100,000 inhabitants in Sweden, Denmark, Finland and Norway over time.

Figure A2: The vertical dashed lines mark the year before a divorce law liberalization in each country. Source: Statistics Sweden, Statistics Denmark, Statistics Finland, Statistics Norway.

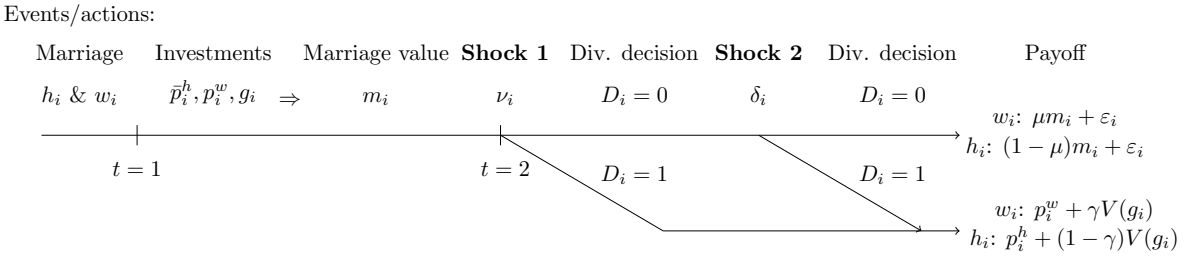


Figure A3: A timeline of the divorce model. Husband (h_i) and wife (w_i) choose private investments (p_i) in period 1. Besides private investments, the wife also chooses to allocate resources to the joint marriage good through g_i. Going into period 2, the marriage value m_i is hit by a preference shock 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. Under divorce, they get the payoffs associated with divorce.

Appendix B: Supporting information and results

Empirical results

Table B1: Upper secondary school completion outcome with restrictive controls and age spacing checks

	Extensive controls US compl.			Full sp. 0–18	Rev. sp. 0–8* US compl.	1973 spacing 0–8**
Insulation$_i$× 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	Yes	Yes	Yes			
Linear age spacing control		Yes	Yes			
# siblings FE			Yes			
Cohort & parent cohort FE	Yes	Yes	Yes	Yes	Yes	Yes

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 B2: Effect of a 6-month parental reconsideration period for divorce on parental outcomes 1990–2015.

	Fathers			Mothers		
	Earnings 1990	Marr./Cohab.	Death	Earnings	Marr./Cohab.	Death
Insulation$_i$× 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	Yes	Yes	Yes	Yes	Yes	Yes

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 cohabitating in 1990, and "Death" refers to death by 2015. "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. "Mean dep. var." refers to mean dependent variable for the reference category.

Theoretical framework

Two individuals (husband h_i and wife w_i of family i) 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 intermediary marriage good (g_i, e.g. home production and children), 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, private contacts) according to their investment capabilities 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 $\bar{p}_i^h = p_i^h$.[62] The surplus 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.[63] Period 1 actions of the husband and wife mean that the gains from marriage at

[62] 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 more than 99% of the parental leave taken out in the 1970s were by the mother. From this, it is reasonable to believe that the majority of the investments in children at the time were by the mother.

[63] The sharing rule is assumed to be exogenously determined by the relative bargaining strength within the marriage, where the spouse receiving the largest share have the potential to transfer resources to to compensate the weaker spouse should he/she find it necessary to prevent a divorce later on.

this point in time is defined as:

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

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. 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 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 forces no new actions. A novel feature of this model is that the shock to the marriage value consists of two uncorrelated components: $\varepsilon = \nu + \delta$, $Cov(\nu, \delta) = 0$. Just like the composite term ε_i, the shocks are mean zero ex ante $\mathbb{E}[\nu] = \mathbb{E}[\delta] = 0$. The first shock ν is observable directly going into period 2, and 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 shock value ν, only that ex post observing the remaining term δ 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 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 ν is observed.

Period 2 starts with the 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 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 $\varepsilon_i > \hat{\nu}_i$, which means that the value of the shock is only experienced given that the shocks do not trigger a divorce. This value function can be used to solve for the optimal marital investments:

$$\begin{aligned}
W_i^w &= u_1^w + m_i^w(1 - F(\hat{\nu}_i))^2 + \left(\int_{\hat{\nu}_i}^{\infty} \nu f(\nu)d\nu + \int_{\hat{\nu}_i}^{\infty} \delta f(\delta)d\delta\right)(1 - F(\hat{\nu}_i)) + u_{i,2}^{w,d}(2 - F(\hat{\nu}_i))F(\hat{\nu}_i)) \\
\frac{\partial W_i^w}{\partial g_i} &= u_i^{w\prime}(g_i) + m_i^{w\prime}(g_i)(1 - F(\hat{\nu}_i))^2 - 2m_i^w f(\hat{\nu}_i)\hat{\nu}_i{}'(g_i)(1 - F(\hat{\nu}_i)) + u_{i,2}^{w,d\prime}(g_i)(2 - F(\hat{\nu}_i))F(\hat{\nu}_i) \\
&\quad + 2u_{i,2}^{w,d} f(\hat{\nu}_i)\hat{\nu}_i{}'(g_i)(1 - F(\hat{\nu}_i)) - \mathbb{E}_{\nu,\delta}[\varepsilon_i|\nu_i > \hat{\nu}_i, \varepsilon_i > \hat{\nu}_i]f(\hat{\nu}_i)\hat{\nu}_i{}'(g_i)(1 - F(\hat{\nu}_i)) \\
&\quad - 2\hat{\nu}_i(g_i)f(\hat{\nu}_i)\hat{\nu}_i{}'(g_i)(1 - F(\hat{\nu}_i)) = 0
\end{aligned}$$

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:

$$\begin{aligned}
&\underbrace{V'(\hat{g}_i)(1 + (1 - F(\hat{\nu}_i))^2)}_{\text{Benefits of marriage investment}} - \underbrace{\mathbb{E}_{\nu,\delta}[\varepsilon_i|\nu_i > \hat{\nu}_i, \varepsilon_i > \hat{\nu}_i]f(\hat{\nu}_i)\hat{\nu}_i{}'(\hat{g}_i)(1 - F(\hat{\nu}_i))}_{\text{Greater chance of experiencing the information shock}} \\
&= \underbrace{\mu(1 + (1 - F(\hat{\nu}_i))^2)}_{\text{Cost of investment}} - \underbrace{u_{i,2}^{w,d\prime}(\hat{g}_i)(2 - F(\hat{\nu}_i))F(\hat{\nu}_i)}_{\text{Greater loss under divorce}}
\end{aligned}$$

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:

$$\begin{aligned}
F(\hat{\nu}_i) = 0 \Rightarrow \quad V'(\bar{g}_i) &= \mu \\
F(\hat{\nu}_i) = 1 \Rightarrow \quad V'(\tilde{g}_i) &= \frac{1 + \mu}{1 + \gamma}
\end{aligned}$$

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 A3.

A final feature of the model is the introduction of a waiting period for divorce, in line with the Swedish divorce restriction introduced in 1974. 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.[64]

[64]See Figure 1 for an illustration of marginal divorces and marriage quality affected by the restriction.

Table B3: Divorce laws in the Nordics over time

Country	Time span	Divorce law
Sweden	1915–1968	Marriage counselling was mandatory for couples seeking a divorce, where the couple needed to prove that there was no hope in saving the marriage. If counselling failed, the court could mandate a reconsideration period of 1-year separation ("hemskillnad") before the divorce would be finalized. For couples not agreeing to divorce, the divorcing spouse needed to prove that the marriage had broken down, most often by showing that the spouses had not co-habitated for 3 years or that the spouse had committed adultery, abuse, desertion, risked infecting the spouse with a venereal disease, or suffered from substance addiction or insanity for at least three years
	1969–1973	The need for a divorcing spouse to prove "long and lasting marital differences" in order to be granted a 1-year separation was removed. The unilateral divorce application was considered enough evidence. The courts became more restrictive in granting divorce based on adultery
	1974–	Mandatory counselling was made voluntary, and couples jointly seeking a divorce could do so without any delay. Unilateral divorce decisions and divorces for couples with children under the age of 16 was subject to a 6 month reconsideration period before being finalized. The reconsideration period can be waived if the couple has been separated for at least two years. Spouses jointly seeking a divorce could also be granted a 6-month trial period if they both requested it.
Denmark	1922–1969	A legal separation could be obtained by mutual agreement and in more limited circumstances unilaterally. The separation could be converted into divorce after a reconsideration period.
	1970–1988	The reconsideration period for legal separations under mutual consent was reduced to 1 year. Unilateral divorce was allowed after living apart for 3 years.
	1989–2012	The right to separation became unilateral, and the reconsideration period was reduced to 6 months under mutual consent, 1 year for unilateral divorce
	2013–2018	Divorce is granted immediately if both spouses agree to divorce. If one spouse disagrees, the couple must go through a 6 month reconsideration period. This can be waived in some cases (e.g. adultery, abuse).
Finland	1948–1987	Divorce under mutual consent was allowed after period of at least one year of judicial separation and mandatory marriage counselling.
	1988–	Unilateral divorce allowed after a reconsideration period of 6 months. The same applies for divorce under mutual consent.
Norway	1909–1992	Marriage counselling is mandatory for couples. Divorce is granted after a year of separation if the couple agrees to divorce. When the spouses did not agree to a divorce, several fault grounds existed, and in addition there was also divorce on the ground of irretrievable breakdown.
	1993–	Marriage counselling is mandatory for couples seeking a divorce if the couple has children under the age of 16. A petition for divorce can be filed one year after a separation period is granted by the courts.

Source: Commission on European Family Law (CEFL, 2002)