DISSERTATION SERIES 2024:1

Essays on families, health policy, and the determinants of children's long-term outcomes

Edvin Hertegård

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Dissertation presented at Uppsala University to be publicly examined in Lecture Hall 2, Ekonomikum, Kyrkogårdsgatan 10, Uppsala, Monday, 12 June 2023 at 09:15 for the degree of Doctor of Philosophy.
Essay I has been published by IFAU as working paper 2024:11 and Swedish report 2024:12

ISSN 1651-4149

Economic Studies 211

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Essays on Families, Health Policy, and the Determinants of Children's Long-Term Outcomes

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ECONOMICS AT UPPSALA UNIVERSITY

The Department of Economics at Uppsala University has a long history. The first chair in Economics in the Nordic countries was instituted at Uppsala University in 1741.

The main focus of research at the department has varied over the years but has typically been oriented towards policy-relevant applied economics, including both theoretical and empirical studies. The currently most active areas of research can be grouped into six categories:

* Labour economics
* Public economics
* Macroeconomics
* Microeconometrics
* Environmental economics
* Housing and urban economics

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UPPSALA
UNIVERSITET

Dissertation presented at Uppsala University to be publicly examined in Lecture Hall 2, Ekonomikum, Kyrkogårdsgatan 10, Uppsala, Monday, 12 June 2023 at 09:15 for the degree of Doctor of Philosophy. The examination will be conducted in English. Faculty examiner: Professor Helmut Rainer (University of Munich, Department of Economics).

Abstract
Hertegård, E. 2023. Essays on Families, Health Policy, and the Determinants of Children's Long-Term Outcomes. *Economic studies* 211. 236 pp. Uppsala: Department of Economics, Uppsala University. ISBN 978-91-506-3008-4.

Essay I: Divorce laws are known to influence family behavior, but empirical evidence of their effects on children remains scarce. I shed more light on this by investigating the effects of the Swedish divorce law reform of 1974, which liberalized the existing divorce laws and introduced a 6-month parental reconsideration period for divorce. The results suggest that exposure to more liberal divorce laws decreases children's upper secondary school graduation rate by 5.6%. Evaluating the reconsideration period, I find that children more exposed to this reform element are 18.3% less likely to experience parental divorce and are 1.8% more likely to graduate from upper secondary school. The findings highlight a trade-off between parental freedom of choice and the beneficial effects of divorce restrictions on children's outcomes.

Essay II: Fluoridation of drinking water has remained controversial since its inception as a public policy. The fundamental concern is whether fluoride exposure affects children's cognitive development. This study leverages the water fluoridation experiment in the Swedish city of Norrköping 1952–1962 for causal evidence of the effects of fluoride exposure during childhood. The main findings are negative effects of water fluoridation exposure during childhood on cognitive ability and non-cognitive ability around age 18, and on the probability of graduating from high school. I find no effects for the cohorts born after the experiment ceased in 1962.

Essay III (with Helena Svaleryd and Jonas Vlachos): At the onset of the COVID-19 pandemic, Swedish upper secondary schools moved to online instruction, while lower secondary schools remained open. Leveraging rich Swedish register data, we find that exposure to open rather than closed schools resulted in a small increase in PCR-confirmed infections among parents. The results indicate that keeping lower secondary schools open had minor consequences for the overall transmission of SARS-CoV-2.

Essay IV (with Julien Grenet, Hans Grönqvist, Martin Nybom, and Jan Stuhler): We study how the next generation of workers adjust in response to economic crisis. The context is the massive economic recession that hit Sweden in 1990, which disproportionally affected the manufacturing and construction sectors. Our analysis shows that students experiencing paternal job loss from the crisis sectors before making their high school program choices select into programs less affected by the crisis. Early paternal job loss is also found to positively affect the students' lifetime earnings, and to increase their chances of being employed later in life. The results indicate that economic crisis may have lasting effects on the composition of the labor force.

Keywords: Family behavior, Children's outcomes, Fluoride exposure, COVID-19, Economic crisis

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ISSN 0283-7668
ISBN 978-91-506-3008-4
URN urn:nbn:se:uu:diva-499783 (http://urn.kb.se/resolve?urn=urn:nbn:se:uu:diva-499783)

To Ester, Gunnar, and all that will be

Acknowledgements

This thesis is the product of countless of hours of thinking, writing, and rewriting. Finally, I can say that things are at a point where the papers are ready to be presented at a dissertation defense. However, one thing is for certain: the work that led to this point is only a step forward in the grand scheme of things. I look forward to more meetings with brilliant researchers I have not met before, talking and sharing ideas with old colleagues, and to develop as much as possible in my role as an academic from this point on.

The fact that there exists a job out there where the point is to take the time to think about interesting things for a living still feels almost miraculous. I am very grateful to the Department of Economics at Uppsala University for making this my everyday life for almost five years. I remain in awe at this opportunity. Working in Uppsala also made it much easier to follow my partner, now wife, Ester Hertegård, to the city, and to start our life together. This turned out to be the most important move of my life, and I thank my lucky stars that they aligned and led us here. Given that all of my research projects to this date to some extent have been about families, some first-hand experience on the topic was much needed.

I want to extend my deepest gratitude to my supervisors, Hans Grönqvist and Helena Svaleryd, for always taking their time and giving me invaluable support and feedback on my work. The joint projects with Hans and Helena have given me the best possible start to life in academia, and I have learned much from both of them. Thank you for everything. Besides being great supervisors, Hans and Helena have provided me with fantastic opportunities to work with skilled co-authors on joint projects. Many thanks to Jonas Vlachos, Julien Grenet, Martin Nybom, and Jan Stuhler for sharing your vast experience and great ideas. In addition, I want to thank Peter Nilsson and Peter Sandholt Jensen for providing excellent input during the licentiate seminar and final seminar.

I further extend my thanks to the faculty members at the Department of Economics for their helpful and constructive comments. Among the faculty, I want to highlight the input from Erik Grönqvist, Peter Fredriksson, Georg Graetz, Eva Mörk, Lena Hensvik, Oskar Nordström Skans, and Arizo Karimi. I also want to give a special mention to Mattias Öhman, Linuz Aggeborn, and Björn Öckert. Thank you for being so generous with your time.

The first year of the doctoral program was filled with course work, with excellent teachers pushing my cohort colleagues and I to get ready for pursuing research of our own. I especially enjoyed the courses in microeconomics by Torben Mideksa, and the ones in econometrics by Alex Solis and Luca Repetto. The courses taught by Torben were very inspirational, and this was the first time during the Ph.D. program that I really started thinking about my own research, and what I wanted to do with my time at the department. The subsequent courses in econometrics, by Alex and Luca, helped set me on my way to tackle the projects that would later become the chapters of this thesis.

Working on the thesis during the pandemic went well to begin with. I was fortunate enough to have interacted with many interesting researchers, and had had the time to generate some research ideas of my own when the social interactions became more limited. With little interaction to be had during the pandemic, there were also few distractions, and even less reasons not to delve into my new projects. I thus had ample time to test and to develop my existing ideas during the second and third year of the program. Unfortunately, this meant that the creativity that comes with new interactions was depleted after a year or so. Luckily, things started to open up again during the fourth year of the program. I am very thankful to Lena Edlund for inviting me to spend two short, but intense, months in New York at Columbia University Department of Economics during spring 2022. I also gratefully acknowledge the financial support for the visit, provided by the Jan Wallander and Tom Hedelius foundation. This visit jump-started my research processes again and helped me to get back on track to generate new ideas and to improve my existing ones.

The time spent at the office would been of little use had it not been for the fellow Ph.D. students at the department. Many thanks to the fantastic colleagues who brightened up these days. My cohort colleagues Fei Ao, Zeynep Atabay, Simon Ek, Jan Lang, Maximiliano Sosa Andrés, Elin Sundberg, and Anton Sundberg were the best possible company from the first year and onward. I look forward to seeing you outside of the department, and to not just referring to you as my friends from work. Among the cohort colleagues, I want to give a special thanks to my office mate Simon, whom I enjoyed countless of interesting talks and fikas with. The figures inside my papers would have been more dull, and a lot less informative without your input.

I also want to mention some of the other colleagues who resided in the halls of the department during my time at the program. Dmytro, Cristina, Melinda, Kerstin, Lucas, Olle, Tamas, Arnaldur, Anna, Jonas, Fredrik, Daniel, Sofia, Raoul, Anna, Yaroslav, Alice, Markus, Adrian, Hanfeng, Erika, Malin, Lovisa, Akib, Rinni, Gabriella, Qingyan, Dogan, Adam, Majken, Alexander, Maria Elena, Zunyuan, Kristina, Tsz Chun, Tianze, Erik, and all the others who have made the time at the department so enjoyable and stimulating.

It would not have been possible to write this thesis without the members of the administrative staff, who went above and beyond in helping with any work-related questions. I want to thank Ulrika, Nina, Ann-Sofie, Johanna, Sara, Maria, Julia, and all the others involved in making sure that life at the department functioned seamlessly.

Support is much needed as a Ph.D. student, and I can think of no more supportive people than my parents, Ingegerd Nilsson and Stellan Hertegård, and my brother Axel Hertegård. Thank you for everything you have done for me. I also want to thank my extended family and their partners: Irene Hedin, Ulf Hedin, Simon Hedin, Jonatan Hedin, Lukas Hedin, and Per Gunnarsson.

Writing this thesis would have been a lot less fun without friends outside of work. Thank you Wilhelm Lindblad, Hjalmar Ekberg Skog, and Olov Ryding Hallin for being great friends. I hope that our discussions will stay sharp and relevant throughout our lives. Anton Hellström Persson, Erik Dahlman, and Freddy Persson, we have enjoyed much together through thick and thin since those formative years in compulsory school. I want you to know that I cherish and look forward to every time we meet.

Finally, I want to thank my wife Ester and son Gunnar Hertegård for their love. Every day with you is a blessing. You are my everything.

Uppsala, April 2023
Edvin Hertegård

Contents

Introduction

Families play a fundamental role in organizing our lives, and in shaping societies. This idea dates back as far as the work by Thomas Malthus (Pollak, 2003), but it has left a limited mark on research in economics. Economic models traditionally stipulate that individuals act in their own self-interest, without considering cooperation or conflict with members of the same household (Becker, 1981). By aggregating the individuals' decisions and their consequences to a societal level, the hopes are that one can explain economic behavior and events, and even predict future developments. However, in reality, people's decisions are almost always influenced by those around them. Any theory or model that fails to take this into account will likely be a poor road map when trying to understand economic outcomes.

In his seminal work, starting from the 1960s and 1970s, Gary Becker proposed to extend economic modeling to incorporate the family environment and its implications for everyday decision-making (Becker, 1973; Becker, 1974). Becker's early work set the stage for modern research in family economics, and subsequent work has shown that the family environment affects virtually all aspects of life, such as labor market decisions, place of residence, fertility, investments, consumption, and the outcomes of the next generation (Heckman et al., 2006). Given this significance, learning more about the family should be of first order importance for future research in economics.

With that being said, unpacking the family environment is a challenging task. To start with, family behavior and responses are endogenous processes with strong idiosyncratic components hard to fully explain with observable information. In addition, detailed data on family behavior for large samples is difficult to come by. To address these challenges, empirical research in family economics has frequently turned to negative shocks to the family environment, such as divorce and separation, for identification of effects related to family disruption. This type of an event is often visible in the data, and usually constitutes a shock strong enough to be able to estimate causal effects with. Even so, this strategy is not without its challenges. "Happy families are all alike; every unhappy family is unhappy in its own way." is a quote from Tolstoy's novel Anna Karenina which aptly suits these difficulties. Despite family disruption being an event we can identify in the data, it is, much like social science at large, riddled with heterogeneity and complexities, which make the interpretation of any information we learn difficult.

While challenging, these difficulties are not insurmountable. I firmly believe that the old maxim in economics "more is always better" holds especially true in this case. We need more data, and more creative use of the available variation to help piece the puzzle together and answer key empirical questions in economics. Although, it must be noted that we should never forget the importance if untangling mechanisms, and explaining the institutional background that underpins the evaluations and estimations we do. Without this understanding, we have no clear way to interpret and extrapolate the information at hand, and are just barely better off than we were before. In the first chapter of this thesis, I seek to contribute to this research by investigating the effects of a reform to the Swedish divorce laws in 1974, and how this affected family disruptions and children's long-term educational and social outcomes. In the same chapter, I delve into the mechanisms at play and seek to explain the findings in light of the institutional environment and previous studies on the topic.

A common finding in empirical research is that early exposure to policies can have large and lasting impacts on children's life trajectories (e.g. Cunha and Heckman, 2007). This usually also holds true for health policies, which is the focus of the next two chapters of this thesis. As economists, we are trained to quantify both costs and benefits of relevant policies using modern econometric tools, and the following two chapters are prime examples of this. The second chapter contributes to an ongoing policy debate on the effects of fluoride exposure during childhood by using experimental variation from 1952–1962 in Norrköping, Sweden. Specifically, I use this experiment to evaluate the effects of water fluoridation exposure during childhood on human capital outcomes later in life. This is a contested scientific question, with the potential to affect hundreds of millions of people (Aoun et al., 2018; Gravitz, 2021). The empirical toolkit, provided by economics, allows me to evaluate the overall effects of this policy, and to add valuable information to the scientific debate, as well as the public debate.

The same toolkit can also be used to answer urgent policy questions with contemporary data, and to contribute to the policy debate in research fields widely disparate from economics, such as epidemiology. This became evident during the COVID-19 pandemic, where drastic measures were performed to maintain the spread of the disease, while causal evidence of the effects of such policies was often lacking. One such example is school closures during the pandemic (UNESCO, 2020), which was widely implemented across the globe, despite clear indications of detrimental effects and with largely missing information on the potential gains of the policy (Viner et al., 2020; Dorn et al., 2020; Guessoum et al., 2020). The third chapter seeks to contribute to this policy debate by quantifying the effects of keeping schools open, relative to moving to online instruction, in terms of affecting the spread of the virus for parents, teachers, and teachers' partners.

Finally, the behavior of individuals and families are part of what shapes the labor market and the supply side of the economy. Recent evidence has highlighted that economic crisis can accelerate structural change on the labor market (e.g. Autor, 2010; Giuliano and Spilimbergo, 2014; Jaimovich and Siu, 2020; Howes, 2021), but there is limited evidence of how young individuals respond to such events, and how labor markets are shaped by their early career choices. The fourth chapter contributes to this research by investigating how exposure to economic crisis, through paternal job loss, can affect students' early career choices and subsequent labor market outcomes.

In the following summary of the individual chapters, I further specify the contributions of this thesis to the fields of family economics, health economics, and labor economics. In short, I contribute to the existing research by tackling the aforementioned empirical questions and show that for all of these topics, the family environment is fundamental in understanding the determinants of children's long-term outcomes, health policy effects, how economic crisis affects early career decisions, and epidemiology.

1 The essays

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

Family disruption through divorce is an increasingly common event in the Western world. While there is ample evidence that divorce laws can affect individuals and families in terms of investments and savings behavior (Stevenson, 2007; González and Özcan, 2013), domestic violence (Stevenson and Wolfers, 2006), women's labor supply (Fernández and Wong, 2014), and divorce decisions (Lee, 2013; Fallesen, 2021), there is limited evidence of how children are affected by exposure to more or less liberal laws governing divorce. In this chapter, I investigate how divorce laws affect children's long-term social and labor market outcomes. I also delve deeper into the mechanisms underlying these effects than what has been done in the previous literature. I do so by evaluating the effects of the Swedish divorce law reform of 1974, which **i)** liberalized the existing divorce process and **ii)** simultaneously introduced a 6-month reconsideration period for divorce when a child under age 16 is living in the household.

I combine rich Swedish register data on 1.17 million children, with differential exposure to the two reform elements, to estimate the effects of family (in)stability on children's long-term outcomes. The empirical analysis relies on a differences-in-differences (DiD) specification leveraging marital status of the parents or age spacing of siblings for cross-sectional variation, along with differential cohort exposure to the reform, to estimate causal effects related to greater reform exposure. While the main focus is to estimate the effects of the 6-month reconsideration period, I also present estimates on the effects of the liberalization element of the reform.

My findings indicate that exposure to more liberal divorce laws during childhood decreases the high school completion rate by 5.6% for children with married parents, relative to that of the children with unmarried parents. These results are in line with previous work showing that exposure to more liberal divorce laws can have detrimental effects on children's outcomes (Gruber, 2004; Cáceres-Delpiano and Giolito, 2012; González and Viitanen, 2018). Evaluating the effects of the reconsideration period for divorce, I find that children in families with greater exposure to this reform element are 18.3% less likely to experience parental divorce, 1.8% more likely to graduate from upper secondary school, and have better marriage market outcomes as adults. Delving into the mechanisms at play, the evidence suggests that the effects on children's long-term educational outcomes and social outcomes are mainly driven by changes to parental behavior within marriage, rather than the through the reduction in experiencing parental divorce.

The findings of the paper indicate that divorce laws can play a substantial role in affecting family behavior by setting the institutional environment for marriages. The results also highlight the need for policy makers to consider externalities when designing public policies related to marriage stability, and specifically to consider the long-term effects these policies can have on children.

1.2 The effects of water fluoridation during childhood on human capital outcomes

Water fluoridation of drinking water is a common public policy, which affects more than 380 million individuals worldwide (Aoun et al., 2018). Being hailed as one of the 10 great public health achievements of the 20th century by the Centers for Disease Control and Prevention in the U.S., this policy is deemed as a safe and cost-efficient way to prevent tooth decay among the population by major NGOs (WHO, 2019). However, recent observational studies have linked fluoride exposure during childhood, at or below the levels targeted by artificial water fluoridation, with detrimental effects on children's cognitive development (Choi et al., 2012).

Unfortunately, most existing studies rely on correlational evidence and face different challenges to identification, such as comparing urban to rural residents with differential exposure to fluoride (Gopu et al., 2022). Causal evidence is limited for this topic. Only two studies with credible identification strategies to capture causal effects exist to this date, and both papers report null effects of fluoride exposure during childhood on children's cognitive development (Glied and Neidell, 2010; Aggeborn and Öhman, 2021). Despite overcoming many empirical challenges with their respective identification strategy, these two studies still face some challenges before the concern can be put to rest. Primarily, the remaining challenges are to account for pre-existing differences between those residing in areas exposed to fluoride, or bundled treatment of differing water composition related to natural fluoride exposure.

This chapter provides a third piece of causal evidence by evaluating the effects of the Norrköping water fluoridation experiment during 1952–1962. In 1952, the local municipality started to fluoridate the water supply of roughly one third of the city, while keeping the natural, low fluoride concentration in the remainder of the city.

The results show that the treated children born during the water fluoridation experiment exhibit lower standardized non-cognitive ability and cognitive ability on the military conscription tests around age 18, and are significantly less likely to graduate from high school. Subsequently, I find no effects for the treated children born after the experiment ceased in 1962. The Norrköping experiment complements the existing work by overcoming their main empirical challenges, while being robust to a range of specification tests, such as cluster-robust inference and testing for composition changes in the treatment and control area over time. The findings warrant caution from policy makers, and further empirical studies regarding the safe levels of fluoride concentrations in drinking water.

1.3 The effects of school closures on SARS-CoV-2 among parents and teachers

Co-authored with Helena Svaleryd and Jonas Vlachos.

At the onset of the COVID-19 pandemic in mid-April, 2020, an estimated 1.3 billion students in 195 countries were affected by school closures (UNESCO, 2020). These school closures were put in place to help stop the spread of COVID-19, and were implemented despite the likely high costs in terms of learning loss from moving to online instruction for the affected students, and the uncertain gains in terms of stopping the spread of the disease (Viner et al., 2020; Dorn et al., 2020; Guessoum et al., 2020).

In this chapter, we quantify the effects of keeping lower secondary schools open in terms of new infections and COVID-19 diagnoses. We do so by leveraging the partial school closures in Sweden during the first wave of the pandemic. At the time, Swedish policy dictated that upper secondary schools moved to online instruction, while keeping lower secondary schools open. In our study, we combine rich Swedish register data and the quasi-experimental variation in exposure to open and online schools. From this, we can compare parents', teachers', and teachers' partners' PCR-confirmed SARS-CoV-2 infection outcomes and healthcare outcomes during the early stages of the pandemic.

Our results show that exposure to open schools resulted in a small increase in PCR-confirmed SARS-CoV-2 infections for parents. For teachers, however, the infection rate for lower secondary teachers was twice as high compared to their upper secondary counterparts. The increase in infections also spilled over to the partners of lower secondary teachers, who exhibit a higher infection rate than that of upper secondary teachers' partners. The findings for parents indicate that the effects of keeping lower secondary school open on the transmission of SARS-CoV-2 are small for society at large. However, preventative measures to protect teachers could be warranted given the high transmission for them and their partners.

1.4 Economic crisis and the career choices of the next generation of workers

Co-authored with Julien Grenet, Hans Grönqvist, Martin Nybom, and Jan Stuhler.

Structural change often occurs more rapidly in times of economic downturn (e.g. Autor, 2010; Jaimovich and Siu, 2020), but the understanding of how economic crisis affects individual career behavior, and the subsequent effects on the long-run development of labor markets is limited. Even more so is our understanding of the mechanisms producing these effects. According to economic theory, economic crisis may permanently affect the composition of the labor force by accelerating structural change (Howes, 2021), or altering career trajectories by changing perceived employment opportunities and economic preferences (e.g. Giuliano and Spilimbergo, 2014). In this chapter, we shed light on these issues by studying the behavioral adjustments to economic crisis exposure for the next generation of workers.

The context of the study is the massive economic recession that hit Sweden in 1990. The crisis disproportionally hit the manufacturing and construction sectors (Englund, 1999), and we use the timing of paternal job loss in these sectors to identify effects of information shocks during the crisis. Our paper focuses on compulsory school students about to apply to high school educational programs. These high school programs, which closely map into industries through job-specific apprenticeships and occupational licensing, are also strong predictors of long-run educational and labor market outcomes.

Our analysis shows that students experiencing paternal job loss before making their program choices select into programs less affected by the crisis. Early paternal job loss is also found to positively affect the students' lifetime earnings, increases their chances of being employed later in life, and deters the students from employment in the specific sector in the very long run. This indicates that economic crisis may contribute in shaping the composition of the labor force even in the very long-run, and that overcoming informational frictions along with educational choice flexibility can play a substantial role in parrying the effects of economic downturn.

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

Acknowledgements: I thank my supervisors, Hans Grönqvist and Helena Svaleryd, for their invaluable help throughout this project, and Per Johansson for data access. I also thank IFAU, the participants of ULG at Uppsala University, Linnaeus University Department of Economics seminar, Columbia University SRB, the participants of the UCLS Spring Workshop 2022, ESPE 2022 conference participants, Simon Ek, Peter Nilsson, Erik Grönqvist, Georg Graetz, Peter Sandholt Jensen, Douglas Almond, Lena Edlund, Jan Stuhler, Jonathan Gruber, Misty Heggeness, and Pierre-André Chiappori for their feedback on various parts of this project.

1 Introduction

Marriage is based on love. However, it is also 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 marked increase in divorces and coinciding divorce law reforms in many Western countries have led to a lessening of the stability aspects of marriage. In response, sociologists labeled the time period the "divorce revolution" (Weitzman, 1985).[1] Even though the aforementioned reforms tended to liberalize and simplify the divorce process, most countries have retained a reconsideration 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.[2]

Given the clear link between marriage status and parental time investments (Le Forner, 2020), financial resources (Amato, 2000), and social stigma (Gerstel, 1987), marriage instability during key years of the child's human capital formation could have substantial effects on children (Heckman, 2000; Cunha et al., 2006; Heckman, 2011).[3] Relating to this, a growing body of work presents evidence that divorce laws affect family behavior and children's outcomes (e.g. Gruber, 2004; Stevenson and Wolfers, 2006; Fernández and J. C. Wong, 2014; Heggeness, 2020). Estimating the effect of divorce laws on children's outcomes is, however, difficult. Not at least because it requires data tracking children over extended periods of time, but also because of the challenges in accounting for correlated unobservables.

In this paper, I study the effects of divorce law reform on children's long-term outcomes. I do so by evaluating the Swedish divorce law reform of 1974, which provides a rare possibility to investigate how different institutional factors governing marriage stability affect families and help shape children's life trajectories. An appealing aspect of the reform is that it consisted of a substantial liberalization of the existing divorce laws, paired with a divorce restriction affecting spouses with a child under the age of 16 residing in the household. The design of the new law allows me to empirically distinguish between both dimensions of the reform, and while the main focus is to evaluate the effects

[1] The decrease in marital stability is evident in aggregate family statistics; 50% of U.S. children experience parental divorce during childhood, and 27% of U.S. children reside with a single parent as of 2018 (Lansford, 2009; OECD, 2018). In Sweden, 30% of the children born in the year 2000 experience separation by age 18 (Statistics Sweden, 2018). The share of Swedish children residing with a single parent in 2018 is 21% (OECD, 2018).

[2] Lately, some countries and U.S. states have even started to reverse the liberalization of their divorce laws and reimposed or increased divorce restrictions. E.g. Louisiana (2007), South Carolina (2013) and Washington (2013). 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 for divorce, and South Korea also did so as early as 2008.

[3] Divorce risk is linked to SES and directly affects the family's financial situation, which further amplifies the inequality aspects of growing up in a broken home (Hogendoorn et al., 2020). For Swedish children born 1990, there is a 63% greater risk of experiencing parental divorce for those with low SES compared to the ones with high SES.

of the divorce restriction on children's outcomes, I also present evidence of the effects of the liberalization element of the reform.[4]

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.[5] This means that divorce law reform affects divorce decisions and intra-household behavior for spouses remaining married. The inability to separate between these two channels implies that empirical estimates from divorce law reforms should be seen as capturing the combined reduced-form effects related to divorce and changes to family behavior within marriage.

The empirical analysis draws on rich administrative data, allowing me to track 1.17 million Swedish children born 1952–1964 over six decades. With these data, I am able to link the universe of Swedish parents and children from 1932–2014, observe all civil state changes from 1969, and add information on the children's outcomes later in life. 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 the birth cohort and family situation of the child determines exposure to the reform elements. In order to capture the effects of the divorce liberalization element, I compare the outcomes of children with married parents to children with unmarried parents. The evaluation of the divorce restriction element instead narrows the focus to children of married parents, and uses the mandatory 6-month reconsideration period for divorcing spouses with a child under age 16 living in the household. Since the age of the youngest child in the family determines legal divorce frictions after the reform, sibling age spacing to the youngest child in the family is used for identification related to the divorce restriction. The evaluation of both elements also exploits the fact that older cohorts, with the same family background, who are adults when the reform comes into effect are less directly affected by the reform and can be used to account for main effects of marriage status or age spacing. Importantly, the identification will use the interaction between cohort exposure and family situation of the child, effectively netting out any main effects of marriage status or age spacing.

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[4] 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.

[5] 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.

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

Evaluating the divorce restriction element, I show that the 6-month reconsideration period decreases parental marriage instability. Greater exposure to the reconsideration period during childhood reduces the propensity of experiencing parental divorce by 18.3%, compared to the reference group with less exposure. This effect grows stronger for cohorts with every additional year of exposure, and other measures of parental marriage instability provide 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 probability of graduating from upper secondary school, the main outcome of interest, by 1.8%. Statistically significant positive effects are also found for additional long-term outcomes, such as the children's labor market outcomes later in life, and family outcomes for the children themselves as adults.

The effects of the divorce restriction on children's 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 small and not statistically significant for the children with a low pre-determined risk of experiencing parental divorce. 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, composition changes of parents in terms of observable characteristics 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 of divorce laws linked to within-household bargaining and behavior are inherently difficult to investigate due to the lack of detailed information on family 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: **i)** mothers with greater exposure to the reconsideration period reduce their hours worked and labor earnings after the reform while fathers' labor supply remains the same, indicating changes to within-household behavior. **ii)** 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 and parental investments. **iii)** The military conscription tests show that the children's cognitive and non-cognitive abilities also are positively affected by the reconsideration period, indicating that the effects run deep in affecting children's abilities. **iv)** Finally, the affected children 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 of divorce laws 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 run through children where the parents remain married, and through effects on their non-cognitive ability. These findings provide additional evidence that changes to divorce laws can affect children both through direct and indirect channels, and call for the need of further evidence to understand the effects of family policy reform on children's outcomes.

The previous literature on the effects of divorce and divorce law reform on children tends to find null or negative effects linked to divorce or more liberal divorce laws (e.g. Bhrolchain, 2001; Gruber, 2004; Björklund and Sundström, 2006; Frimmel et al., 2016).[6] These studies all vary in the type of outcomes studied, and the potential for their respective research design to deal with correlated unobservables. The exceptions to the null and negative effects in the literature are two studies in developing country contexts using divorce liberalization, divorce legalization, and court congestion as identifying variation for the effects of divorce laws. Both studies find positive effects for children related to exposure to more liberal divorce laws and less court congestion for divorce cases (Heggeness, 2020; Corradini and Buccione, 2023). 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 1974 reform in Sweden, coupled with access to rich administrative data, allows me to advance the literature in several ways. First, previous research has shown that formal "cool-down" (reconsideration) periods for divorce can prevent marginal divorces (Lee, 2013; Fallesen, 2021), but to the best of my knowledge no research has used a similar identification strategy to show the effect of such restrictions on children's long-term outcomes. Second, the data allow me to study the effects of the reform on a wider set of outcomes, within the same sample, from as early on as childhood until adulthood. Third, the setting and the data provide a rare opportunity to sepa-

[6]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.

rate between different mechanisms to learn more about how family behavior affects children's outcomes.

My findings indicate that divorce laws can play a substantial role in affecting family behavior by setting the institutional environment for marital stability. As such, the restrictiveness of divorce laws can bring about a fundamental trade-off between spousal freedom of choice and potential externalities. However, the causal effects of divorce law reform on children's long-term outcomes appear to be sizeable, and of mixed sign in relation to the nature of the change taking place. 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 findings also highlight the need for policy makers to consider externalities when designing public policies related to marriage stability, and specifically to consider the long-term effects these policies may have on children.

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 law 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: Divorce and family behavior

Research in economics has long sought to model and explain family behavior and responses to policy. The first wave of theoretical research modeling family behavior and parental investments consists of unitary models, where households maximize a joint utility function subject to a budget constraint (Samuelson, 1956; Becker, 1981). Extensions to this work adds the component of within-household bargaining between spouses over the marital surplus, and highlight the outside option of spouses as key in determining marital stability and spousal behavior. 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; Becker, 1974). However, this only holds under restrictive assumptions about preferences and public marital goods following divorce (Chiappori et al., 2015).

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

In more recent work, theoretical researchers have concentrated on the role of external policies, such as prenuptial contracts and the effects of introducing unilateral divorce in the U.S. on marital instability and investments within the marriage. According to these frameworks, the non-neutrality of divorce law liberalizations on marriages and the risk of experiencing divorce leads to lower marital investment and less specialization within the marriage (Anderberg et al., 2016; Reynoso, 2017; Reynoso, 2018). All in all, the lessons from this is that the effects of divorce law reform are likely dependent on the nature of the change taking place, factors related to the institutional setting, and the type of family affected by the reform.

2.2 Empirical research: Effects of divorce and divorce laws 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).[7] However, there exist only a limited number of credible microeconometric studies on the causal effects of divorce on children's outcomes. Identification strategies using family fixed effects show that much of the observed effects on children's outcomes associated with parental divorce are due to negative selection based on family characteristics (Piketty, 2003; Björklund and Sundström, 2006; Chen et al., 2019). Previous attempts at using exogenous variation to identify the effect of divorce on children are few, and the evidence is mixed. For instance, Frimmel et al., 2016 attempt to instrument for divorce using the father's exposure to women at the workplace in Austria, and find that divorce leads to worse schooling outcomes for affected children.

The more detailed mechanisms at play related to family behavior are inherently difficult to disentangle, but 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 and 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

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

exam reduces the chances of passing by 3–8%. Recent evidence of the channels through which divorce affects children has also singled out parental time investments as key to building children's human capital and setting the stage for outcomes later in life (e.g. Le Forner, 2020; Gould et al., 2020).

Gruber, 2004 and Cáceres-Delpiano and Giolito, 2012 instead use an indirect approach and leverage variation based on changes to U.S. state divorce laws, i.e. if the state allows for unilateral divorce. They find negative effects from exposure to unilateral divorce on children's long-term outcomes.[8] Gruber, 2004 also argues that instruments based on divorce law reform fail to satisfy the exclusion restriction and should instead be interpreted as providing reduced-form evidence of the effects of divorce laws on children.[9] Contrarily, Heggeness, 2020 uses the divorce legalization in Chile in 2004 and local court congestion for exogenous variation related to divorce liberalization and divorce restrictions. She estimates that legalizing divorce increases children's secondary schooling enrollment by 3.8–6.1%, and that every additional 6-months of court waiting time for divorce reduces the positive effects on schooling enrollment rate by 1.9%. However, the negative effects of divorce restrictions in this study may be driven by the setting, where court congestion could exacerbate conflict and uncertainty in the family environment.[10] Unfortunately, due to data limitations the existing studies give very limited insight into the mechanisms at play linking divorce law reform and marital stability to within-household behavior and children's outcomes.

A range of different studies have attempted to fill this research gap by showing that changes to divorce laws can affect mechanisms related to within-household bargaining of spouses, primarily by investigating spousal labor supply and savings behavior (e.g. Stevenson, 2007; González and Özcan, 2013; Fernández and J. C. Wong, 2014; Voena, 2015). While Stevenson and Wolfers, 2006 also show 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 tradeoffs related to family policy. A recent study by Ringdal and Sjursen, 2021

[8] Specifically, Gruber, 2004 finds that exposure to unilateral divorce leads to a 14.5% greater risk of living with a divorced mother and −6.5% probability of being a college graduate.

[9] 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. (Huber and Spitze, 1980).

[10] Likewise, (González and T. Viitanen, 2018) looks at the long-term effect of divorce legalization in several European countries on children's outcomes and find negative effects on the earnings and health of women growing up under the setting where divorce is legal. Linked to this, a recent study investigating a unilateral divorce reform in Egypt finds positive effects on parental investments in children following the reform and attributes this to improved bargaining power of the affected mothers (Corradini and Buccione, 2023).

sheds more light on within-household bargaining 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 in children take place when the most patient parents' bargaining strength increases in the household. Related changes to family policy have also been shown to elicit substantial responses to within-household behavior (Persson, 2020). These findings indicate that the effects of divorce law reform may bring about different effects depending on the setting, equilibrium effects, and the marginal divorces affected. 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 direct 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 (husband and wife), who are exogenously matched to each other and live for two time periods. The first time period symbolizes the early years of marriage with marital investments, family formation, and career development. The second period captures the remainder of the lifetime.[11] In the first period, the wife chooses to invest in an intermediate marriage good (g_i, e.g. home production and time investments in children), which is carried forward into the next period. Investments in the marriage good are defined to be 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 the marriage. The husband and wife also invest in a private good (p_i^w and p_i^h, e.g. personal career and private networks). 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.[12]

I also add divorce risk in the framework, in the form of an information shock affecting the marriage value. Divorce transforms 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 react

[11] A condensed timeline of the model can be seen in Figure B2.

[12] 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 to the conditions in the 1970s.

by ex ante reducing investments in the joint marriage good (g_i) in favor of the private good (p_i^w). In other words, the wives 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 reconsideration period for divorce, in line with the Swedish divorce restriction introduced in 1974. This is modeled 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 reconsideration 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 marginal divorces, to the benefit of children. See Figure 1 for an illustration of marginal divorces and marriage quality affected by the restriction.

The empirical predictions based on this framework imply that the introduction of a divorce restriction should lead to fewer divorces being observed. Also, I expect to observe changes to the behavior and within-household bargaining of parents remaining married based on differential exposure to the restriction, to the benefit of the affected children. These implications will be tested later on in the empirical framework.

3 Background

3.1 Institutional background

The 1960s and 1970s saw a large increase in divorces in the Western world, and in many countries this coincided with the implementation of divorce law liberalizations.[13] In 1974, Sweden made perhaps the most radical overhaul of them all by substantively simplifying the existing divorce process, removing all fault-based reasons for divorce, and making unrestricted divorce the new norm. At the time, the new law was deemed to be the most liberal divorce law in the Western world (Jänterä-Jareborg, 2014). Similar reforms have since then taken place around the world, but many countries still share similarities with the institutional setting of Sweden before 1974.[14] For instance, some U.S. states, the UK, Germany, and Canada still retain fault-based reasons as a primary way to divorce and restrict unilateral divorce to a substantial degree.

[13] For instance, the UK 1969, Denmark 1970, the U.S. 1970–, and Australia 1975.

[14] Many U.S. states introduced unilateral divorce around starting in the late 1960s, and mandatory reconsideration periods for divorce exist in states across the country.

Divorce laws in Sweden 1915–1973

Before the reform in 1974, during 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 (14%). Under mutual consent, the couple jointly filed for divorce at the local court. Following this, the couple had to go through 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. After the separation period had passed, the spouses were allowed to finalize the divorce. 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. 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, for instance, adultery.[15]

The divorce law reform of 1974

The divorce law reform of 1974, enacted on January 1, constituted a complete overhaul of the existing divorce legislation by removing all fault-based reasons, and making unrestricted divorce the new norm. This change means that divorcing spouses no longer need to wait before finalizing their divorce, nor to disclose any reason to the courts for instigating divorce. The motivating reason behind the new policy was changing views on family life and its value to society.[16] Especially women's growing economic freedom and lessened reliance on their husbands was a key reason behind the new policy.[17]

Despite the motivation to put the individual's freedom first for divorce decisions, the policy makers decided to implement a 6-month reconsideration period for divorce under unilateral divorce, and when a child under age 16 is living in the household. The restriction was meant to act as protection for children and spouses against impetuous divorces and the adverse effects these may have. The 6-month period was reasoned to be an adequate restriction bal-

[15] For more detailed information on the divorce laws in Sweden 1915–1973 and the following reform in 1974, see Appendix B.

[16] The dominant view at the time was that marriage is 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 costs of restricting the individual's freedom (SOU 1972:41, 1972).

[17] 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%.

ancing the needs of all parties, 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 2023.

While there is no conclusive evidence that the reconsideration period affects divorces more than through postponements, 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).[18]

Institutional context around the time of the reform

The 1970s were a formative time in Sweden, with many new policies being implemented. 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. Universal healthcare and the education system had long since been free of charge, and women were entering the labor market in never-before-seen numbers. In terms of marriage patterns, society was different from today when the majority of children are born to unmarried parents. Marriage was the predominant form of civil status for cohabiting couples at the time of the reform in 1974, with 88% of cohabiting couples being married. While cohabitation without marriage was on the rise, it was rare around this time, especially for couples with children.

The progressive new divorce laws in 1974 were paired with, in some aspects, equally progressive existing laws governing what happened after a divorce. Swedish law is based on the viewpoint that married spouses are obliged to support each other financially during marriage, but the economic ties are to be severed after divorce. Marital assets are generally divided equally between the spouses during the divorce process, and there is no default inheritance between former spouses. Alimony to the financially weaker party is rare, except for transition periods and when one parent takes the majority custody of any children.[19] Custody arrangements around this time, however, were traditional. Children alternating between living with both parents after a divorce is relatively common today, but it was not so at the time of the reform. The results of a governmental investigation in 1975 showed that the mother received full

[18] Using the research strategy later outlined in this paper, I show that parents with greater exposure to the divorce restriction were more likely to be married 16 years after the reform was enacted (see Table B2). This is in line with previous research indicating that divorce laws can affect short-run and long-run divorce behavior (González and T. K. Viitanen, 2009; Lee, 2013). However, these findings contradict work from the U.S. indicating the neutrality of divorce laws on divorce rates (Wolfers, 2006).

[19] This institutional setting discourages specialization into household production, as as spouses not active on the labor market risk financial difficulties following a divorce. The median child support per month for one child at the time of the reform was 1,500 SEK (roughly $170), and for two children 2,400 SEK ($280) in 2020 value (SOU 1975:24, 1975). Reportedly, only one in ten cases of divorce in 1974 led to a court mandated alimony for the financially weaker spouse.

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).

The education system in Sweden also underwent changes around the time of the divorce law reform. Following a comprehensive reform in 1962–1963, the Swedish schooling system is comprised of nine years of compulsory education. After completing compulsory schooling, students in Sweden are, if they choose to do so, able to enrol in upper secondary education. In 1971, the different upper secondary school systems in Sweden were replaced by a unitary system with vocational and academic tracks.[20][21] The share of children enrolling had been increasing over time starting in the 1960s.[22] This trend, and the other simultaneous policy reforms, implies that any policy evaluation in terms of educational outcomes for these cohorts needs to rely on within-cohort to net out the striking increase over time.

4 Data and empirical method

4.1 Data

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

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 assign-

[20]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).

[21]The adult education system in Sweden allows for those who lack any upper secondary education and those who dropped out before graduating to finalize a degree. It is also possible to supplement a vocational degree to obtain a three-year degree within the adult education system.

[22]In 1960, 10% of the cohort graduated from upper secondary school. By 1980, 85% completed upper secondary school, and by the end of the 1980s almost 90 percent of the children continued directly to upper secondary school after compulsory school.

ing children to their joint birth mother.[23] I restrict attention to outcomes for the years 1970–2000, since this time span is consistent with the data material and align with the education information in the censuses in 1970 and 1990. For some additional outcomes related to family formation, where longer time spans are readily available, I add information up to the year 2014.

I supplement the existing data by including information from the Swedish War Archive [Krigsarkivet] on eight dimensions of non-cognitive ability and cognitive ability. Roughly 90% of the Swedish men in the cohorts I study performed these mandatory tests around age 18. The measures of non-cognitive abilities are 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). 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 timed multiple-choice questions, and are also standardized by cohort. These abilities have previously been shown to strongly correlate with outcomes later in life, such as labor market outcomes (Lindqvist and Vestman, 2011), and are 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 based on the individual components in each ability group.

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 specific reform element 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 parental marriage status is defined in 1970. Thus, this evaluation supplements the original sample of 1,168,874 children of married parents with 9,805 children of the cohorts 1952–1964 with unmarried parents.

The evaluation of the divorce restriction instead focuses exclusively on children of married parents. The identifying variation is based on the interaction

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

between the 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.[24] This placebo sample consists of 314,974 individuals.

4.2 Identification strategy

The evaluations of the two reform elements are based on the same differences-in-differences (DiD) approach, where the interaction of exposure to the specific reform element and cohort group captures the coefficients of interest. While the two evaluations and their respective identification strategies are highly similar, they differ in terms of exposure definition (age spacing or marriage status), 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's outcomes, I focus on children of married parents and exploit the fact that the age of the youngest child in the family determines the exposure to the divorce restrictions for all of the children in the same family. Based on this, I can compare children of the same birth cohort with varying age spacing to the youngest child in their respective family. Marriage status and age spacing are defined in year 1970, well before the new divorce law became known to the general public.[25] 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 divorce restriction element.[26]

Specifically, the children are split into two groups: one with small to no age spacing, which consists of the youngest children themselves and those with a

[24] 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 cohort group will still reside in the household when the policy comes into effect.

[25] Setting marriage status and age spacing in 1970 gives me stable comparison groups but does not take any new siblings or changes to marital status into account. This reduces the risk of capturing selection induced by the new divorce policy, but may also attenuate the effects. I test these concerns in Table B1 by assigning marriage status and age spacing status in 1973 and show that the results for the main educational outcome remains qualitatively unchanged.

[26] 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 from upper secondary school. The corresponding number for the cohort born in 1964 is 83%.

sibling 0–2 years younger than they are, and the other group with greater age spacing consisting of those with a younger sibling 3–8 years younger. The first group is deemed to have weak to no extra insulation against parental divorce after age 16, while the second group has greater insulation by virtue of their age spacing. I set this cutoff at three years of spacing 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[27] of Swedish academic-track upper secondary school up to age 18.[28] Given previous evidence of the effects of 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 by increasing parental investments in the household. In addition, it should be noted that the reconsideration period very well also could also affect families by changing the level of conflict associated with the divorce, while not affecting the final divorce decision.

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; E. V. Nuttall and R. L. Nuttall, 1979; Buckles and Munnich, 2012). There is also evidence that birth order affects children's outcomes (Black et al., 2018). I will address this concern by also including 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 the policy 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.[29] Thus, any main effects of age spacing on children's outcomes should be present for these cohorts and can be accounted for.

[27] Vocational-track upper secondary school programs at the time usually only lasted two years.

[28] 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 that 90% of children in each cohort are included. I verify that this is not a concern by including all children regardless of age spacing, and excluding the children where the birth of a sibling 1971–1973 changes their age spacing status to more than 8 years (excluding 3.5% of the sample) and show that this gives unchanged or slightly stronger estimates on educational outcomes (see Table B1).

[29] 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 Figures 2b & 2c).

The cohorts born 1956–1957 are only affected by the policy for 1–2 years, and are therefore partially exposed to direct effects of the reform, while the 1958–1964 cohorts are fully exposed. I collapse the partially exposed and fully exposed groups into one group in the estimation of the effects. Including the partially exposed cohorts should attenuate the results slightly, but will help provide a better picture of the effects of the reform at large. 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.[30] This assumption corresponds to the parallel trends assumption in DiD terminology (Angrist and 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 0–2 years of age spacing. 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:

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

The indicator $Insulation_i$ takes the value one (1) for individual i if the age spacing to the youngest sibling is between 3–8 years, and zero (0) for the reference category with age spacing 0–2 years. This indicator corresponds to treatment assignment in the DiD terminology. $\mathbb{1}[Cohort_i \geq 1956]$ is an indicator function taking the value one for cohorts born 1956–1964 (ages 10–18 in 1974), and zero for cohorts born 1952–1955 (ages 19–22 in 1974). Cohort indicators D_j take the value one 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 cohorts, which are known in advance to be imbalanced across age

[30] 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.

spacing groups.[31] $\boldsymbol{X_i}$ 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 in 1970, educational attainment, municipality of residence, the child's birth month, and sex).

Missing values of control variables are included as separate indicators. Along with parental cohort 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 average 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 effect of age spacing is instead captured by β_2, which estimates the effect for the older cohorts born 1952–1955. By construction, roughly half of all children are the youngest in their family, which means that the reference group is heavily tilted toward the youngest children or those with no siblings. This implies that the comparison can be interpreted as the difference between youngest siblings with no insulation and those with 3–8 years of age spacing with greater insulation. The advantage of this approach is that it allows me to include cohort effects in the estimation and rely on within-cohort variation. 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.[32]

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. As mentioned, the sample of children with unmarried parents amount to 9,805, while the children with married parents number 1,168,874. These children are highly selected, and are very few compared to the sample of children with married parents, but serve as a valid counterfactual under the assumption of no composition changes over the cohort groups. The relatively few unmarried parents around this time will, however, make the comparison more unstable than a similar evaluation taking place today when the majority of children are born to unmarried parents.[33] The effects also risk

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

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

[33] Unmarried parents are defined as both parents having never been married before in 1970. The number of children with unmarried parents in 1970 are few and increasing by each cohort (roughly 1,300 children born 1952–1955, and 8,500 born 1956–1964). A concern based on this

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.[34] 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:

$$y_i = \phi_0 + \phi_1 \underbrace{Married_i \times \mathbb{1}[Cohort_i \geq 1956]}_{Treat \times Post} + \phi_2 \underbrace{Married_i}_{Treat} + \underbrace{\sum_{j=1953}^{1964} D_{i,j}}_{Post} + p_i + \boldsymbol{X_i'\gamma} + \varepsilon_i$$

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

4.3 Descriptive statistics

Descriptive statistics for the main sample for the first year of the panel in 1970 are presented in Table 1. Besides displaying descriptive statistics, the table is meant to visualize the identification strategy, and serve as an initial balancing test.[35] The average characteristics of the children, their parents, and

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 groups more similar for the younger cohorts. Under the caveats of conditioning on an outcome potentially related to the divorce law reform (marriage behavior), I also run specifications where I condition on the parents of children in the comparison group remaining unmarried by 1975 and 1980 separately and show that the main result remains qualitatively unchanged (results available upon request).

[34] 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 (15.5%).

[35] More formal joint composition and balancing tests will be presented later on.

their maternal grandparents are presented in columns (1), (2), (4), and (5) corresponding to their cohort group and reform element exposure group. The additional columns (3), (6), and (7) display the differences in average characteristics, and the final column presents the p-value of the double difference 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.

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 groups. Reassuringly, the differences in characteristics are similar for the older cohort group compared to the younger cohorts. In general, the double difference between cohort groups greatly reduces the magnitude of the observed differences, but they still remain statistically significant for some characteristics. This double difference gives an indication that the differences between the age spacing categories are not always stable across cohort groups, but they are generally small in magnitude. For the married and unmarried parents, the strategy serves to reduce the differences between the marriage status groups, but they 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 observed in 1974.[36] A crude analysis of this spike in Figure 3 shows that the primary respondents were older couples and couples without children, with no apparent immediate heterogeneity by earnings and educational attainment. However, all subgroups in terms of age, child status, earnings, and education show an increased number of divorces following the reform, indicating that there was pent up demand for the reform in all echelons of society.[37]

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

[36] The increase between 1973 and 1974 amounted to 67%, see Figure 2. Some claims have been made to attribute the increase to the 1973 Swedish television miniseries "Scenes from a Marriage" written and directed by Ingmar Bergman, but the far more likely reason is the substantial change to the existing divorce laws.

[37] 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.

figure shows a clear discontinuity in the divorce rates at ages below 16 starting in year 1974, indicating 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 at least parts of the reduction in divorce incidence 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 in Sweden 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 spouses previously married to other people 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 (H.-P. C. Wong, 2018). The increase in marriages was later followed by a spike in marriages after the 1989 widow's pension reform in Sweden, further showing the responsiveness of marriage behavior to public policy reform (Persson, 2020).

5.2 Evaluating the divorce liberalization

Moving on to the causal analysis, I first proceed by investigating the effects of the divorce liberalization element. As noted, the liberalization element of the 1974 reform affected the entire population of married parents, making the causal effects difficult to disentangle. However, a relatively small number of children (9,805) have parents that were unmarried in 1970, which means that these 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 upper secondary school completion rate between children of married and unmarried parents decreases significantly by 4.6 pp. (-0.046, s.e. 0.016) following the reform, which amounts to -5.6% relative to the mean of the outcome for the reference group. The partial 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 effect on university graduation rate is smaller in magnitude (-0.031, s.e. 0.006), but is still sizable and more prominent relative to the mean dependent outcome for the control group, which translates into a relative effect of -27%.

The estimates for labor market outcomes and the conscription measures of ability are somewhat noisy and do not translate into any significant effects on log earnings (-0.029, s.e. 0.024) or standardized non-cognitive ability (-0.053 SD, s.e. 0.044), even though the point estimates are negative. The effects on employment (-0.041, s.e. 0.011) and standardized cognitive ability (-0.183 SD, s.e. 0.043), on the other hand, are negative and statistically significant, with a relative effect of -4.6% for employment, and a substantial effect on cognitive ability in terms of standard deviations.

In Figure 5, I disaggregate the main finding by presenting a graphical representation of the effects of divorce liberalization on the upper secondary school completion rate, by birth cohort. The figure shows a large, stable gap in upper secondary school completion rate for the older cohorts, indicating that children with married parents had much better schooling outcomes around this time. The difference between the groups visibly shrink for the younger cohorts affected by the divorce law reform, except for the cohort born in 1960.[38]

The main concern with this evaluation is potential composition changes of married and unmarried parents over cohort groups driving the observed effects.[39] However, Figure 11 shows that the predicted upper secondary school completion rate based on parental and child characteristics in 1970[40] predicts a relatively stable, positive difference between the children of unmarried and married parents over time. An F-test of the difference over cohort groups also shows that the difference is not statistically significant (p-value 0.538). In other words, composition changes are not contributing to the partial convergence in schooling outcomes.

A further concern, which is harder to address, is the possibility of changing views on marriage driving the observed effects, or some other effect besides the reform, which caused a partial 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 reform. For now, I acknowledge this caveat and proceed to evaluate the divorce restriction element of the reform.

[38]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.

[39]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 only marginally affect the main results.

[40]The characteristics include educational attainment of parents, parents' birth cohort, parents' labor market outcomes, sex, birth month, and municipality of residence. R^2 0.060.

5.3 Evaluating the divorce restriction

Effects on measures of parental marital instability

Using the research strategy based on age spacing of siblings, I validate that the reconsideration period for divorce affects the propensity of experiencing different measures of parental marital instability. The measures include experiencing divorce by age 18, divorce in 15 years from 1970, and the fathers' multi-partner fertility. Experiencing parental divorce during childhood is defined as either of the parents divorcing at any observable year until the child is age 18.[41] Experiencing parental divorce within 15 years of 1970 instead follows all cohorts for 15 years and captures the long-term effects on marital stability. Multi-partner fertility is used as an 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 youngest child is born after 1974, and zero otherwise.

The results of the OLS regressions on measures of marital instability when pooling the cohort groups can be seen in Table 3. The main effect on experiencing parental divorce by age 18 for the placebo cohorts born 1952–1955 is precisely estimated at zero when including all children (-0.000, s.e. 0.001), but is statistically significant and positive for elder siblings (age spacing 1–8) born the same years (0.005, s.e. 0.001). The effect is substantially stronger and negative (-0.022, s.e. 0.001) for the cohorts affected by the reform during childhood, and even greater in magnitude for elder siblings (-0.033, s.e. 0.002). The reduction in parental divorce incidence in the main specification translates into a decrease of roughly 18–20%, relative to the control mean of the reference group born 1956–1964.[42]

The other measures of parental marital instability provide evidence in the same direction of less 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 focusing on elder siblings (-0.030, s.e. 0.003), with similar relative effects as the outcome divorce by age 18 (ranging from -13 to -21%). The same goes for the estimates of fathers' multi-partner fertility for all children (-0.007, s.e. 0.001), and for elder siblings (-0.007, s.e. 0.001), which are weaker in absolute magnitude but significantly negative and similar in relative terms compared to the other estimates (-17 to -29%).

Figure 6 presents graphical results of the OLS regression by cohort, where Figure 6a includes all children and the Figure 6b focuses exclusively on elder

[41] Since divorces are observed from 1970 and onward, parental divorce can only be measured for a short period of time for the oldest cohorts. This means that every new cohort's parental divorce outcomes by age 18 are observed an additional year. This is of limited importance for the estimation since outcomes are compared within each cohort.

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

siblings. The results show that the negative effect on parental divorce incidence by age 18 is phased in for the cohorts affected by the reconsideration period during childhood. The effect of age spacing on parental divorce does not exist for the placebo cohorts that should not be affected. The observed effects are also weak for the affected cohorts born 1956–1957, who are relatively old at the time of the reform and thus have few years of exposure. For the cohorts exposed to the reconsideration period during childhood, 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.

All in all, the reconsideration period appears to have substantial effects on parental marital instability. While not capturing the full extent of the policy's effects on families (e.g. changes to within-household bargaining, reducing the level conflict within the family but not preventing divorce, and parental investments), these outcomes provide valuable evidence that the reconsideration period affects family behavior.

Effects on educational outcomes

The preferred outcome where I expect this kind of a divorce restriction to affect children's outcomes is upper secondary school completion. Based on the theoretical framework presented in this paper, the reconsideration period is expected to strengthen the marital stability of parents and increase parental commitment, which could benefit the children. Changes to parents' marital stability and within-household behavior during adolescence could thus impact parental investments and the emotional stability of the affected children during formative years of human capital development. Also, upper secondary school completion is primarily determined during ages 16–19, which are the ages when the reconsideration period is not active for the reference group with no younger siblings or small age spacing.[43] Experiencing parental divorce or exposure to more liberal divorce laws has previously been associated with a decrease in children's educational outcomes. This means that a divorce restriction potentially could benefit children's schooling outcomes (Gruber, 2004; Steele et al., 2009). 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, and other indicators of economic well-being, which makes this an important focus of study (Freudenberg and Ruglis, 2007; Heckman et al., 2008; Oreopoulos and Salvanes, 2011; Lochner, 2020).

[43] Due to data restrictions, the educational 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.

The OLS estimates on the effects on upper secondary school completion and the broader effects on years of schooling can be seen in Table 4. Greater exposure to the reconsideration period is shown to significantly increase upper secondary school completion rate for the exposed cohorts (0.015, s.e. 0.002) when including all children, while the main effect for the placebo cohorts is significant and negative (−0.007, s.e. 0.002). The same general finding holds when evaluating the effects in terms of years of schooling. On average, years of schooling increases by almost 0.11 years (0.106, s.e. 0.010) in response to the reform. The results for upper secondary schooling when focusing on elder siblings are also statistically significant and positive, albeit weaker, for the affected cohorts (0.008, s.e. 0.003), and not significant for the older placebo cohorts (0.004, s.e. 0.003). The same goes for the treatment effect on years of schooling (0.063, s.e. 0.017). Relative to the mean of the dependent variable of the control group, the effects correspond to an increase of 0.5–1.8%. 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 treatment effect also exists within the elder siblings group.

The graphical representation of the effects on upper secondary school completion rate by cohort can be seen in Figure 7. Panel 7a shows results for all children, while Panel 7b focuses on elder siblings. The figures indicate that children's schooling results are positively affected by the reform, albeit weakly for the cohorts born 1956–1957 where the reform is only active 1–2 years at the end of upper secondary school, instead of the full 3 years. The main effect before the reform appears to be negative for the group of older cohorts born 1952–1955, and stable over the individual cohorts in the group.

Effects on related outcomes

In order to provide a broader picture of children's outcomes later in life, I present a range of related outcomes for the same study sample. These results and the following are shown for the main empirical specification with all children included.[44]

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. The estimates on 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 statistically significant, in line with the previous educational findings. 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 educa-

[44]Results for elder siblings on related outcomes can be seen in Appendix B (see Tables B3 & B4).

tional 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. The effect sizes 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%) (Edin and Holmlund, 1993; Harmon et al., 2000).

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

The results on family outcomes are validated in the 1990 census ten years prior (age 26–38), where the children with greater exposure are less likely to be single parents (−0.003, s.e. 0.001), and more likely to be married or cohabiting (0.007, s.e. 0.002) at that time. Delving deeper into the married or cohabiting outcome, it is clear that marriage is driving the observed effect since the cohabiting outcome is precisely estimated at zero (0.000, s.e. 0.002). The effect on being a parent with 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).[45] The effects on family outcomes are large in relative terms (1.3% greater chance of ever marrying, 3.8% less risk of ever divorcing, and 2.6% less risk of being a single parent).

5.4 Heterogeneous treatment effects

In this section, I present heterogeneous treatment effects of the reconsideration period for children at risk of experiencing parental divorce, along with two categories of heterogeneity related to previous research: sex of the child and parental earnings. Sociological research has found indications of divorce and marriage stability affecting boys more than girls, and a recent paper shows an increased divorce risk for parents with a daughter in the family (Kabátek and Ribar, 2020). The mechanisms behind boys being more sensitive to divorce

[45] As shown later in the paper, the effects on ever being a parent remains precisely estimated at zero when extending the time period up to 2014.

are not clear, but they are more prone to behavioral problems than girls and the literature has speculated that parental divorce may exacerbate this difference (Kaye, 1989; Amato, 2001; Aggarwal, 2019).

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

Effects on children at risk of experiencing parental divorce

In order to strengthen the link between the effects on marriage instability and the educational outcomes, I present evidence that the effects on upper secondary school completion 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 8. The at-risk split is performed 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 by predicted risk of experiencing parental divorce.

The prediction does well in capturing actual divorce behavior of parents, with the Q5-Q1 realized difference in divorce incidence being 25 pp.[46] The figure shows that the families with the lowest predicted risk of going through a divorce (Q1–Q2) exhibit no significant improvement in educational outcomes for the children, while the families at medium and high risk of divorce (Q3–Q5) are where the significant treatment effects are found.[47] The finding is in line with more unstable families changing their behavior in response to the reform. Families with low divorce risk are virtually unaffected. This strengthens the case that the reconsideration period for divorce, indeed, is driving the observed effects on upper secondary school completion.

Sex of the child

I investigate heterogeneous treatment effects of the reconsideration period on the main outcomes 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 and Ribar, 2020, I find no significant difference in parental

[46]The realized divorce outcomes in Q1-Q2 range from 0.9–2.4 pp. The Q3-Q5 outcomes range from 5.2–25.4 pp.

[47]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.

divorce incidence 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 effect sizes in the original paper translates into an effect size too small to capture with this policy reform. However, I find that girls are significantly less affected in terms of upper secondary school completion rate 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 they are driving this effect is reasonable given that women tend to get custody of the children following a separation.

A graphical representation of the effects on upper secondary completion rate, split by sex of the child and educational attainment of the parents, is shown in Figure 9b. The figure confirms that boys are affected more than girls, but also adds that the treatment effects stem from children whose parents have at most upper secondary education. The treatment effect is thus not visible for those with parents with some university education, for the concerned period less than 10% of the adult 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 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 incidence 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 in 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 pattern is found for upper secondary school completion rate, where the difference in effect with above-median earnings is significant and negative for mothers (−0.010, s.e. 0.004), while positive but significant for fathers (0.005, s.e. 0.004). A possible explanation for this is that 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 framework, and indicate that the reform affects families differently depending on pre-defined characteristics related to earnings and household specialization.

5.5 Robustness checks

In order to validate the results, I run a battery of robustness checks related to treatment definition, group composition changes and the choice of control variables. The aim of these tests are to rule out alternative explanations, and to attribute the observed effects to the divorce law reform.

Parallel trends

As previously discussed, the identifying assumption needed for the identification strategy to give causal interpretation is that the effect of age spacing or marriage status must be constant over cohort groups. In other words, that the outcome in the treatment and comparison groups would evolve similarly under the absence of no policy reform. If this assumption holds, the cohorts having graduated from upper secondary school before the policy came into effect can be used to net out any pre-existing effects of age spacing on children's outcomes. The primary evidence of parallel trends can be seen in Figures 5, 6, & 7. 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.[48]

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 1970 before the policy was implemented.

Since children's age and sibling age spacing is expected to affect outcomes for parents already at this time, an imbalance is expected for observables (e.g. parents' earnings) 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 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 12.[49] The predicted outcomes are shown to be relatively

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

[49] Due to the MGR containing 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. The grandparental characteristics do well in predicting actual

stable over time around zero. An F-test of joint significance of the coefficients for the affected cohorts is unable to reject the null of no statistical difference.[50]

For completeness, Figure 13 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. The covariates used to predict the outcomes include parents' birth cohort, 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 incidence of experiencing parental divorce and a decreasing upper secondary school completion rate, in contrast with the observed effects in the opposite direction.[51] Thus, age spacing group composition changes over time do not appear to be driving the observed effects.

Age spacing cutoff

I also 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 stepwise are displayed in Table 9.[52] The effects on upper secondary completion 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 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 focusing on elder siblings (spacing 1–8) and restricting the age 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

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. For experiencing parental divorce by age 18, Q4 actual value is 0.32, and Q1 actual value is 0.08. For upper secondary school completion, the Q4 actual value is 0.907, and the 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.

[50] The p-value is 0.437 for predicted parental divorce by 18 and 0.675 for predicted upper secondary school completion.

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

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

most restrictive comparison (2 years of spacing against 3: 0.010, s.e. 0.05). This indicates that the 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: Children with 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. Age spacing of siblings should not matter directly for children with unmarried parents, since there is no corresponding reconsideration period for separation. 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 reconsideration period. Figure 14 displays upper secondary school completion rate 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 shows no clear pattern of improved schooling outcomes.[53]

Direct effects of experiencing divorce, family fixed effects, and excluding controls

In order to put the magnitudes of the main effects on upper secondary school completion in a context, and to further test the robustness of the estimates, Table 10 presents the direct effect of experiencing divorce by age 18 on upper secondary school completion, OLS family fixed effects models similar to previous research (e.g Björklund and Sundström, 2006; Chen et al., 2019), the baseline age spacing models augmented with family fixed effects, and the baseline age spacing regression on upper secondary school completion when excluding controls for background characteristics.[54]

The direct effect of experiencing parental divorce on upper secondary school completion is shown to be large and significant, even when including controls of background characteristics (−0.075, s.e. 0.002), but the estimate 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

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

[54] 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, the effect remains statistically significant despite adding these extensive controls.

a comparable magnitude. This finding contradicts earlier work on parental divorce using Swedish register data, which finds null effects on children's educational outcomes using family fixed effects on a smaller, random sample of Swedish children experiencing divorce during childhood (Björklund and Sundström, 2006).[55]

Reassuringly, adding family fixed effects as a robustness test to the main specification based on age spacing leaves the estimates slightly stronger when including all children (0.017, s.e. 0.004). 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 restricting the sample to elder siblings, the estimate is larger relative to the main specification, but more imprecise (0.013, s.e. 0.009). This could be explained by families with 3 or more children being used to identify the effect. The point estimate for elder siblings remains positive and of comparable size to the other estimates, but is noisier and not statistically significant (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 10. Broadly, the estimates are slightly stronger when focusing on all children (0.018, s.e. 0.002), and slightly weaker for elder siblings (0.007, s.e. 0.004) compared to the baseline estimate, but all estimates remain statistically significant. All in all, the estimates are stable across specifications and choice of control variables, and the precision of the estimates improves somewhat with the added controls.

5.6 Mechanisms

The findings presented up until now warrant further investigation into the mechanisms of how families adjust their behavior in response to the divorce restriction. The prime mechanism to investigate and shed light on the reconsideration period's effect on spousal behavior would be a measure capturing within-household behavior and parental investments in children. However, it is virtually impossible to find such a measure in large-scale surveys. I proceed below by showing four tests that signal changes to within-household behavior, and while none of these in isolation provide conclusive evidence of such changes, together they suggest that this mechanism could be an important driver of the results.

Parental labor supply

The first mechanism relates to changes in parental labor supply. Previous research has focused on measuring responsiveness in spousal labor supply to

[55] However, the study by Björklund and Sundström, 2006 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).

divorce law reform and interpreted this as a sign of changing bargaining between spouses. This strand of research has shown that changes to divorce laws can affect labor supply of married women, and that shifting bargaining power can translate into more investments in children (Stevenson, 2007; Fernández and J. C. Wong, 2014; Ringdal and Sjursen, 2021).

Using the data at hand, I investigate the effects on labor supply in this study population by observing parental earnings and hours worked in the census of 1975, which is just after the divorce law reform in 1974. In order to avoid having duplicate observations of parents, I restrict the regressions to only include the oldest child in the family. 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 11 show no consistent significant effects on fathers' labor market outcomes related to earnings in SEK 100 (0.235, s.e. 1.310), employment (0.002, s.e. 0.001), and hours worked (0.105, s.e. 0.054) in 1975 following the reform, but a substantial decrease in the mothers' earnings (-11.136, s.e. 0.646), employment (-0.022, s.e. 0.002), and hours worked per week (-1.376, s.e. 0.072).

Fewer hours worked could potentially be the result of an increase in parental investments from the mothers, and may help 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 6.1% compared to the reference group average outcome, while the fathers leave their work hours virtually unchanged. However, these results should be interpreted with caution since the specification may not fully account for the direct effect of the children's differing age profile on the outcomes.

Intergenerational transmission of human capital

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

The results when estimating the effects on the intergenerational education correlation are presented in Table 12. The findings show that the correlation increases significantly between children and their mothers following the introduction of the reconsideration period (0.005, s.e. 0.001), while the change in the link to the fathers remains not significant 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 for children affected by the divorce law reform. Previous research has shown that reforms targeting adolescents can persistently improve the children's development, especially non-cognitive ability (Heckman, 2000).

The effects on standardized measures of abilities for men around age 18 can be seen in Table 13. In general, the estimates on both cognitive ability and non-cognitive ability are statistically significant and positive for the cohorts exposed to the reform, with exposure increasing ability by 0.027–0.050 standard deviations (0.027–0.050 SD, 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 SD, s.e. 0.006) and emotional stability (0.033 SD, s.e. 0.006) stand out as the strongest. The composite effect of non-cognitive ability is larger than the separate abilities (0.040 SD, s.e. 0.006), and the same holds for cognitive ability (0.053 SD, s.e. 0.006). To put these magnitudes in a context, the composite effects on ability amount to roughly 30–40% of the effect stemming from birth order when comparing first to second born siblings (Black et al., 2018).[56]

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 main effects on the components and composite terms are mostly not statistically significant and negative except for technical aptitude (0.017 SD, s.e. 0.006), and non-cognitive ability (−0.010 SD, s.e. 0.006). The results for non-cognitive ability 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 and Vestman, 2011). This claim will be investigated further in a mediation analysis presented later in the paper.

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

Timing of fertility

The fourth mechanism investigated is the timing of fertility decisions, especially teenage parenthood. This mechanism is potentially 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 documented that parenthood at young ages is associated with poor economic and social outcomes for the parent and child (Kearney and Levine, 2012).

Delving deeper into this outcome in Table 14 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 probability of being a parent is not affected by greater exposure to the reconsideration period.[57] 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 becoming a teenage father is significantly lower (−0.003, s.e. 0.001) along with teenage motherhood (−0.009, s.e. 0.002), but the effect is stronger in absolute magnitude for girls.

The estimated results are large, with the relative effects being equivalent to a 18–38% reduction in the risk of teenage parenthood. 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 (H. Grönqvist and Hall, 2013).

Figure 10 shows graphical evidence that the reduced risk of early parenthood is U-shaped starting at age 16–17, and is the strongest around age 18–19 only to reverse thereafter and become positive at ages 24–25. The main effects on older cohorts are generally significant for the fertility outcomes, although always of the opposite sign of the effects estimated for the treated cohorts. This is 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 education and help explain the observed increase in schooling and improved labor market outcomes.

5.7 Mediation analysis

Next, I try to gauge at how much of the effects on later outcomes which can be linked to the outcomes determined during childhood by following Heck-

[57] This outcome relates to the extensive margin of ever becoming a parent. The intensive margin outcome capturing the number of children is also not statistically significant and of a low magnitude (results available upon request).

man et al., 2013 and H. Grönqvist et al., 2020 and decompose how much of the effects on related labor market outcomes (log earnings in 1990) and family outcomes (ever married by 2000) which can be explained by changes to observed abilities (Panel A of Table 15) and from effects on upper secondary school completion 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 the effects on 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 (13–17%) and the residual accounts for around half of the total effect (47–51%) on the same outcomes. 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 school completion 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 evaluation of the liberalization element indicates that the reform negatively affected children of married parents relative to their counterparts with unmarried parents. The extensive analysis of the divorce restriction element shows persistent and positive effects on a broad range of long-term outcomes for the children, in particular boys, related to greater exposure to the reconsideration period for divorce. The family outcomes paint the picture that the policy spilled over on the children's own family behavior later in life, providing evidence that the effects of exposure to the policy transmit across generations.

The magnitude of the effects on children's outcomes raises the question to which extent the effects stem from divorces, or if they are mostly driven by changes to parental behavior. In line with the arguments presented by Gruber, 2004, the relatively similar magnitudes on experiencing parental divorce and the increase in upper secondary school graduation rate indicate that much of the effects run through within-marriage behavior rather 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 for above-median 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 educational outcomes are weaker for this group. Possibly, this could be due to working mothers being less prone to shift toward parental investments. For fathers earning above the median, the differential effect is positive but not significant. 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 children with mothers earning below the median exhibit larger effects on educational outcomes in response to the reconsideration period.

From the evaluation of the reconsideration period, 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 treatment 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.[58] The findings of this study also indicate that the divorce liberalization on average decreases the upper secondary school graduation rate by 5.6%. Comparing these estimates estimate to the previous literature, the results 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 probability 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 findings are similar in magnitude, although the results from the study by Heggeness are of the opposite sign. The findings highlight the differences in effects based on the direction of the reform, and how the effects 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 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 6 months of reconsideration for divorce, the children of these marginal marriages experience less marital instability and changes to within-household bargaining from parents with a relatively functioning marriage. Thus, the potential upside for children of experiencing less parental divorce or turmoil from bargaining may be net positive for this kind of a divorce restriction.

[58]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 overall takeaway from this paper is that a policy seeking to affect parents' marital stability could impact the schooling outcomes of children. Restricting divorce for couples close to a break-even marriage can, under the right circumstances, protect 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 divorce law reform on children's long-term outcomes by evaluating the effects of the Swedish divorce law reform of 1974. 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 of the effects of divorce on children are plagued by endogeneity concerns, this study uses a novel identification strategy, where variation in family status of parents and cohort exposure to the reform elements are used for plausibly exogenous identification.

Using a DiD-related specification exploiting marriage status or age spacing of siblings, and cohort variation in exposure to the policy, I find substantial effects on children's long-term outcomes related to the divorce law reform. The divorce liberalization appears to have partially converged the difference in observed schooling outcomes between children of unmarried and married parents, to the children of married parents' disadvantage. Evaluating the divorce restriction, the findings show a clear decrease in marital instability for the families with greater exposure to the reconsideration period, and improvements in the probability of the children with greater exposure of graduating from upper secondary school. The magnitude of the effects and the mechanisms related to within-household bargaining indicate that changes to within-household behavior is the primary channel contributing to the long-term effects on children's outcomes.

The findings are robust to a range of specification 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.

All in all, the findings indicate that family responses to divorce law reform can be substantial, with parts of the effects likely running through both parental divorce and changes to within-household behavior. Relating to previous work by Gruber, 2004, the results presented in this paper add evidence of trade-offs between freedom of choice for parents seeking divorce and externalities on third parties, such as children. Policy makers should thus internalize the broader effects of divorce law reform on children when formulating future policies related to marriage stability.

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Figures and tables

Supporting figures

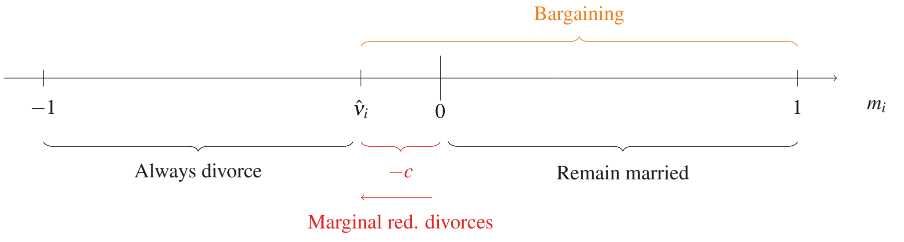
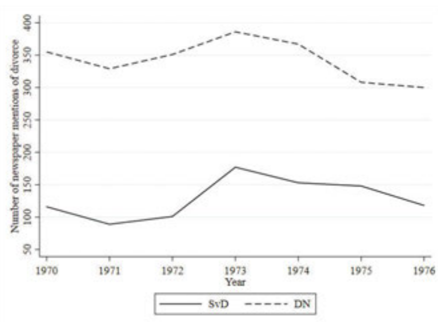
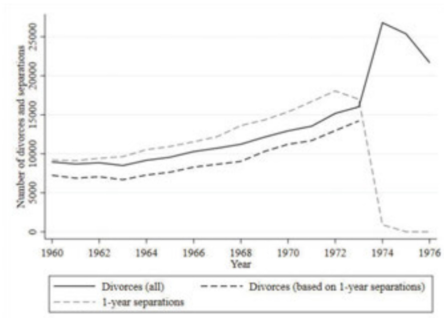


Figure 1. The figure characterizes divorce responses to the realized marriage value under a divorce restriction. Realized marriage value m_i is shown on the unit interval relative to the normalized outside option (0). The divorce restriction c changes a spouse's optimal cutoff for divorce to $\hat{v}_i$, which is the cutoff value in order for the marriage value shock v_i to induce divorce ($m_i \in [-1, \hat{v}_i)$). Marginal reduction in divorces ($m_i \in [\hat{v}_i, 0]$) 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 ($m_i \in [\hat{v}_i, 1]$), which in turn affects children related to the household.

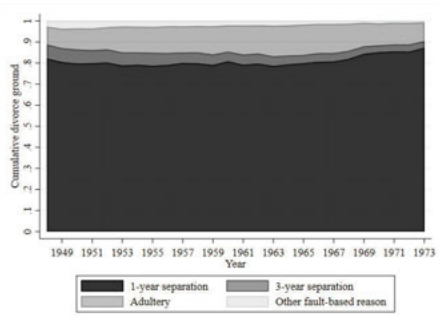
(a) Number of divorces in Sweden 1968–1990.



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

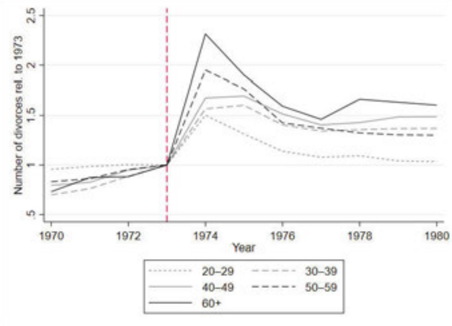


(c) Divorces and 1-year separations 1960–1976.

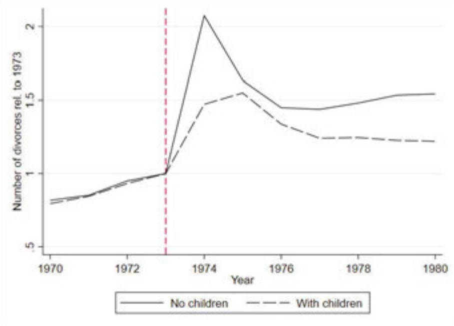


(d) Cumulative divorce reason shares 1948–1973.

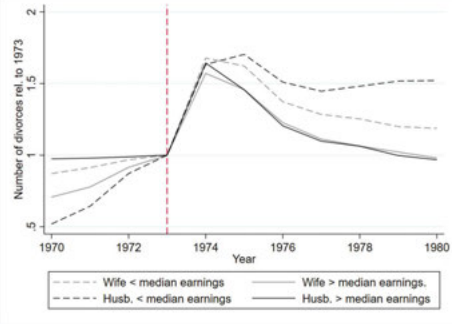
Figure 2. Figure 2a shows the number of divorces in Sweden around the time of the divorce law reform in 1974. Figure 2b shows number of mentions of the word “divorce” in the two largest morning newspapers around the time of the reform. Figure 2c shows a stable relationship between 1-year separations and finalized divorces, and that the number of separations broke the trend in 1973 and sharply decreased following the new divorce policy in 1974. Excess separations could either revert back into marriage, or allow spouses to live financially separate lives while remaining legally married. The transition rules in place from 1974 allowed for courts to grant 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.



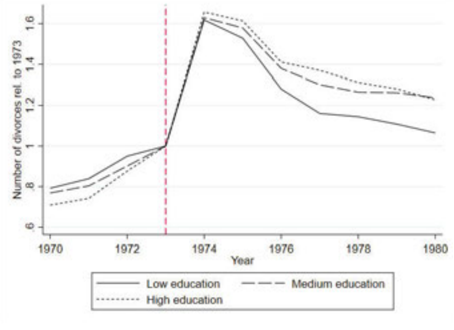
(a) Number of divorces by age group, relative to 1973.



(b) Number of divorces by child status, relative to 1973.



(c) Number of divorces above and below median earnings in 1970 for men and women separately, relative to 1973.



(d) Number of divorces by educational attainment 1970, relative to 1973.

Figure 3. The figure shows 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. Child status is defined as having a child age 0–18. Low education is defined as primary school education, medium education as upper secondary school education, and high education includes university education.

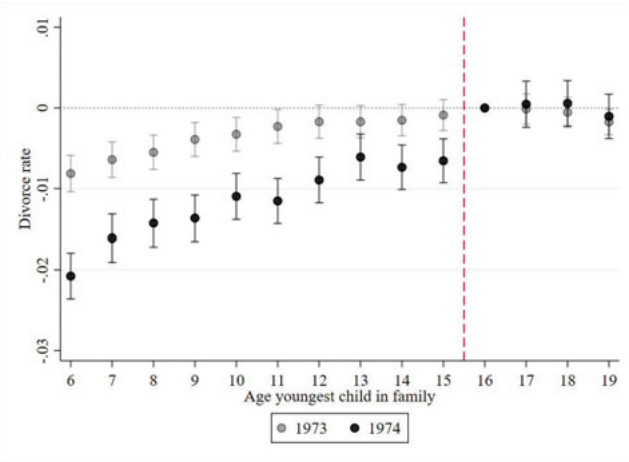


Figure 4. Parental divorce incidence 1973 and 1974 separately, sorted by age of the youngest child in the family. The reference age 16 is indicated by the dashed red line for both years. Average baseline divorce risk is 1.3% in 1973 and 2.1% in 1974. Estimations include parental age fixed effects and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

Outcome figures

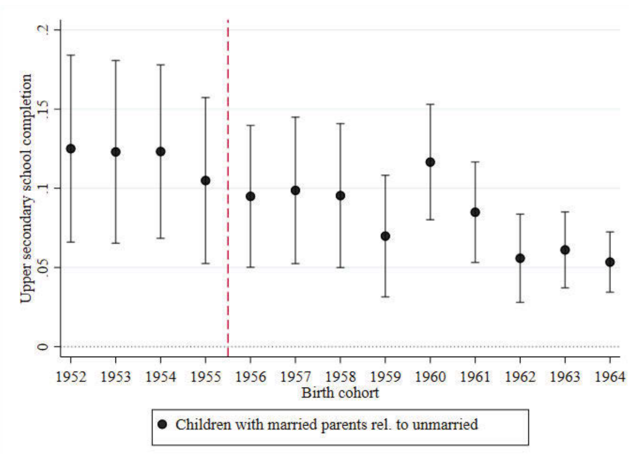
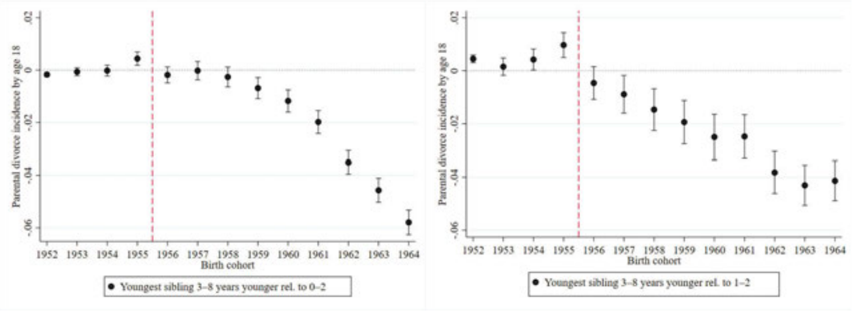


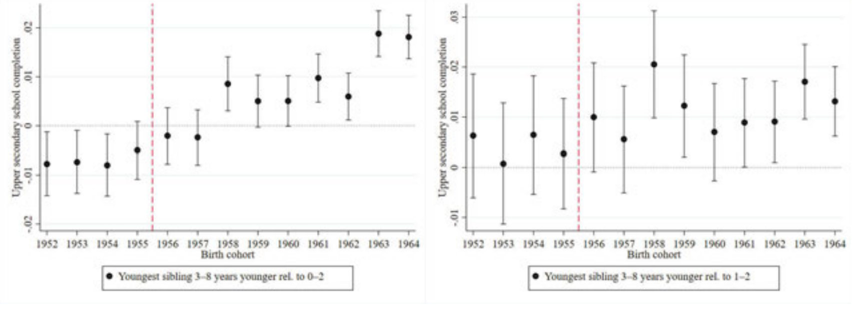
Figure 5. Difference in upper secondary school completion rate measured in the 1990 census. The figure shows the outcome for children of married parents relative to children of unmarried parents. Parental marriage status is determined in 1970. The controls include parents' birth cohort, municipality 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) Difference in parental divorce rate by age 18, youngest sib. 3–8 years younger rel. to 1–2 (excl. youngest sibs.).

Figure 6. Figure 6a disaggregates the main estimate on experiencing parental divorce by following cohorts born 1952–1964 until age 18 separately by birth cohort and estimates the difference in parental divorce rate between the large age spacing group (3–8) against the smaller age spacing group (0–2). Figure 6b 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 parents' birth cohort, 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 upper secondary school completion rate, youngest sibling 3–8 years younger relative to 0–2 years younger.

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

Figure 7. Figure 7a disaggregates the main estimate on upper secondary school completion by following cohorts born 1952–1964 separately by birth cohort and estimates the difference in upper secondary school completion rate between the large age spacing group (3–8) against the 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 parents' birth cohort, 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.

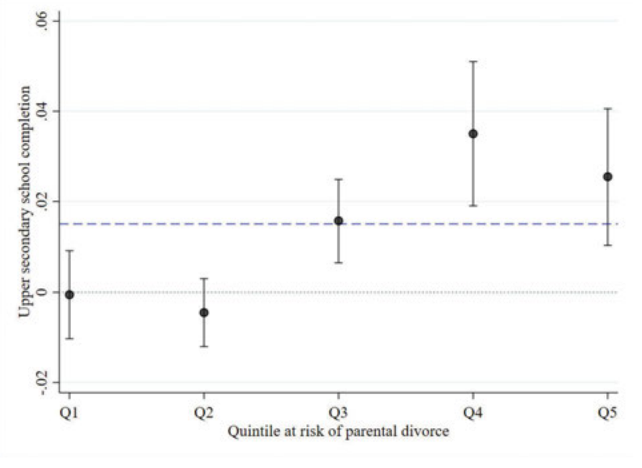
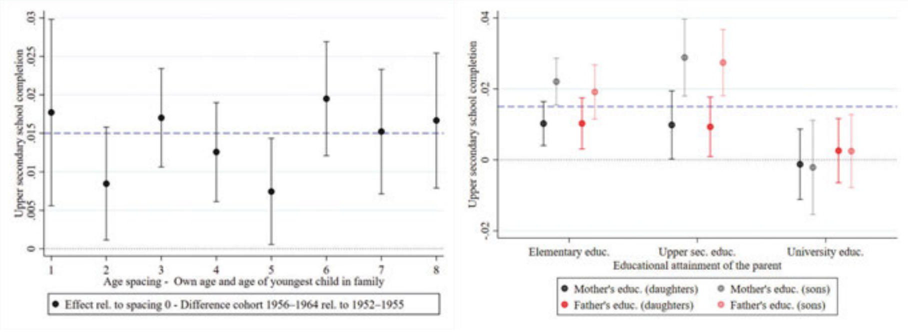


Figure 8. The figure splits the effects on upper secondary school completion by predicted quintile of experiencing parental divorce, based on background information (educational attainment, labor market outcomes, and municipality of residence) and family characteristics (parents' birth cohort, number of children, birth month, sex, 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: parents' birth cohort, 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 0.087. CI95 are indicated in black, and standard errors are clustered at the household level.



(a) Effects on upper secondary school completion 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 school completion 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 of the child. The controls include parents' birth cohort, 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.

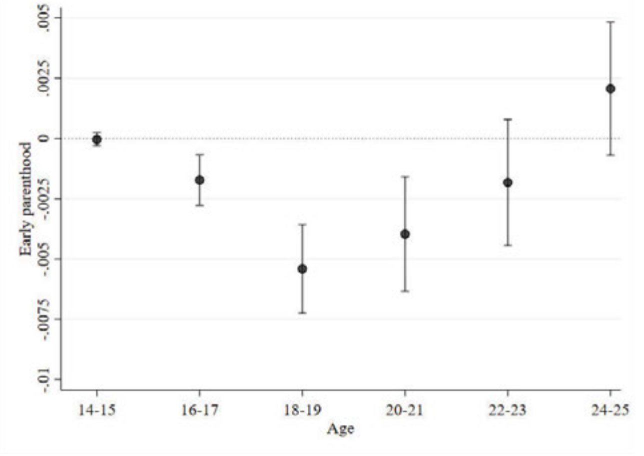


Figure 10. Effects on early parenthood, split by age of becoming a parent. The figure shows effects estimated by separate regressions. The controls include parents' birth cohort, municipality of residence in 1970, labor market outcomes and education of parents in 1970, sex, and birth month. CI95 are indicated in black, and standard errors are clustered at the household level.

Predicted outcome figures

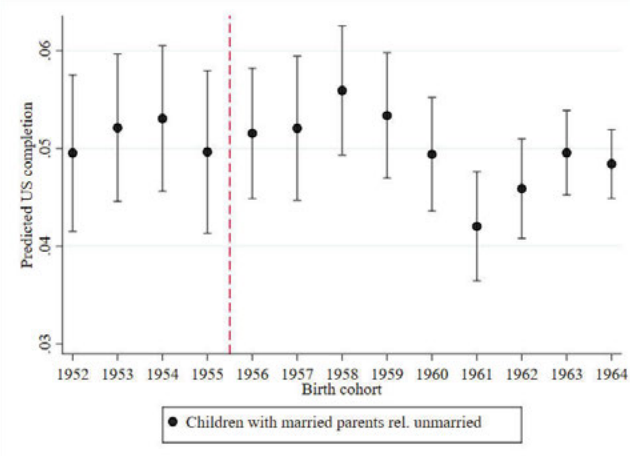
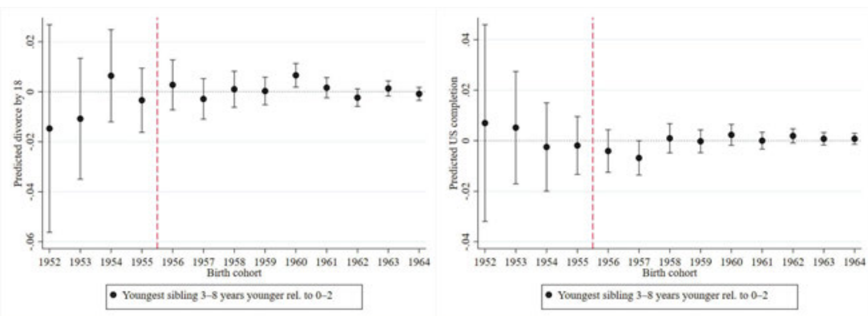


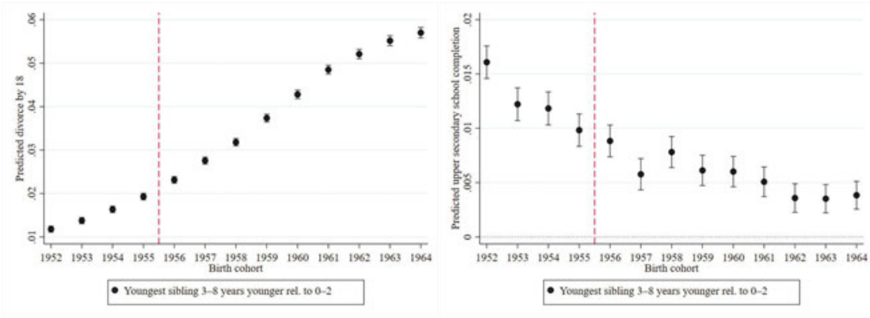
Figure 11. Difference in predicted upper secondary school completion rate for children of married parents relative to children of unmarried parents. Parental characteristics included for the predicted outcome are age, municipality of residence, educational attainment, and labor market outcomes in 1970, with an R^2 of 0.060. 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 12. Figure 12a uses predicted parental divorce by 18 outcomes from grandparental characteristics in 1970 (a set of covariates capturing socioeconomic status - earnings, educational attainment, family type, hours worked and municipality of residence), with an R^2 of 0.068, estimates the difference in parental divorce rate between the large age spacing group (3–8) against the smaller age spacing group (0–2). Figure 12b does the same when predicting the upper secondary school outcomes, with an R^2 of 0.044. 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 13. Figure 13a uses predicted parental divorce by 18 outcomes from parental and child characteristics in 1970, with the caveat that some characteristics risk being imbalanced due to direct effects of child age spacing (R^2 of 0.087). The characteristics include cohort, earnings, educational attainment, and hours worked of the parents, along with birth month, municipality of residence, and sex of the child. Figure 13b does the same when predicting the upper secondary school outcomes, with an R^2 of 0.060. CI95 are indicated in black, and standard errors are clustered at the household level.

Placebo figure

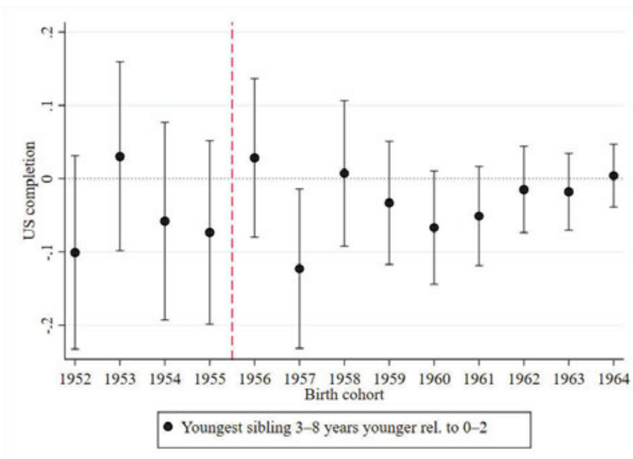


Figure 14. Upper secondary school completion rate for the placebo group with unmarried parents in 1970. The controls include parent cohort effects, municipality of residence in 1970, labor market outcomes and education of parents in 1970, sex, birth month, and indicators of missing values. An F-test of equality between the reweighted outcomes 1952–1955 against 1956–1964 produces the p-value 0.576. CI95 are indicated in black, and standard errors are clustered at the household level.

Result tables

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

Panel A	Cohort 1956–1964			Cohort 1952–1955			Cohort 1952–1964	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Spacing group, diff. (·)–(·), [p-val.]	Sp. 3–8	0–2	d. (1)–(2)	3–8	0–2	(4)–(5)	(3)–(6)	p-val.
Age 1970 - avg., (diff.), [p-val.]	9.791	9.681	(.111)	16.479	16.498	(–.019)	(.130)	[.000]
Age spacing*	5.100	.287	(4.813)	5.237	.407	(4.830)	(–.017)	[.007]
Share female	.484	.488	(–.003)	.486	.487	(–.001)	(–.002)	[.251]
Share foreign born	.069	.057	(.013)	.064	.050	(.014)	(–.001)	[.273]
Age mother 1970	34.729	39.256	(–4.527)	42.588	46.995	(–4.407)	(–.120)	[.000]
Age father 1970	37.463	42.122	(–4.659)	45.064	49.528	(–4.464)	(–.196)	[.000]
Mother's education in years	8.831	8.641	(.190)	8.404	8.206	(.198)	(–.008)	[.466]
Father's education in years	9.435	9.246	(.189)	9.105	8.877	(.227)	(–.039)	[.005]
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)	(4.650)	[.000]
US educ. mother	.291	.206	(.085)	.216	.081	(.136)	(–.051)	[.000]
US. educ. father	.417	.205	(.212)	.350	.108	(.242)	(–.030)	[.007]
Obs.	853,225	8,506	861,731	354,451	1,299	355,750	1,217,481	

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

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

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

Note: $***p < 0.01$, $**p < 0.05$, $*p < 0.1$. Standard errors in parenthesis are clustered at the household level. "Upper sec. completion." is defined as upper secondary education of two years or more, or any higher education in 1990. "University graduation" is defined as at least three years of university education in 1990. "Log earnings" and "Employed" are defined as the natural logarithm of earnings (SEK 100) and employment status in 1990. "Cognitive ability" and "Non-cog. ability" denotes standardized cognitive and non-cognitive ability measures from the conscription tests. "Married$_i$ " indicates the children of married parents, where the reference category is children of unmarried parents. Marriage status is defined in 1970 as both parents being married, and the same definition follows for the unmarried parents. The interaction with cohort shows the difference in effect between cohort groups 1956–1964 and 1952–1955. Besides cohort and parent cohort effects, the controls include municipality of residence in 1970, labor market outcomes and education of parents in 1970, sex, 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 parental marital instability.*

Sample:	All children			Elder siblings		
Outcome:	Divorce by 18	Divorce in 15 years	Father multip. fertility	Divorce by 18	Divorce in 15 years	Father multip. fertility
$Insulation_i \times Cohort_i \geq 1956$	-0.022***	-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	✓	✓	✓	✓	✓	✓
Parent cohort FE	✓	✓	✓	✓	✓	✓

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

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

Note: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Standard errors in parenthesis are clustered at the household level. "Elder siblings" excludes youngest siblings (spacing 0). "Upper sec. completion" is defined as upper secondary education of two years or more, or any higher education in 1990. "Years of schooling" denotes years of schooling. "$Insulation_i$" 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 university and labor market outcomes in 1990.*

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

Note: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Standard errors in parenthesis are clustered at the household level. "University graduation" refers to three years or more of university education in 1990. Earnings (SEK 100) 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 in year 1990 and 2000.*

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

Note: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Standard errors in parenthesis are clustered at the household level. "Ever married" and "Ever divorced" refers to ever marrying or divorcing by year 2000. "Single parent" is defined through the census in 1990, "Marr./Cohab." is defined as cohabiting or being married, "Cohabiting" is defined as cohabiting without being married, and "Parent" is defined as having a child age 0–6 at the same year. "$Insulation_i$" 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 on various outcomes, by sex of the child.*

Outcome:	Divorce by 18	Upper sec. completion	Log earnings	Ever married	Ever divorced	Single parent
$Insulation_i \times Cohort_i \times Female_i$	0.001	−0.012***	−0.005	−0.005	−0.005	−0.006**
	(0.002)	(0.004)	(0.006)	(0.004)	(0.003)	(0.003)
$Insulation_i \times Cohort_i \geq 1956$	−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	✓	✓	✓	✓	✓	✓
Parent cohort FE	✓	✓	✓	✓	✓	✓

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

Split:	Mothers' earnings 1970		Fathers' earnings 1970	
Outcome:	Divorce by 18	Upper sec. completion	Divorce by 18	Upper sec. completion
$Insulation_i \times Cohort_i \times Earnings_{mother1970}$	−0.000	−0.010***		
	(0.002)	(0.004)		
$Insulation_i \times Cohort_i \times Earnings_{father1970}$			−0.007***	0.005
			(0.002)	(0.004)
$Insulation_i \times Cohort_i \geq 1956$	−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	✓	✓	✓	✓
Parent cohort FE	✓	✓	✓	✓

Note: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Standard errors in parenthesis are clustered at the household level. "Divorce by 18" refers to experiencing parental divorce by age 18. "Upper sec. completion" is defined as upper secondary education less or equal to three years. "$Insulation_i$" indicates greater age spacing (3–8 years to youngest sibling against 0–2 years). The interaction with cohort shows the difference in effect between cohort groups 1956–1964 and 1952–1955 and those with below median parental earnings (reference category). "$Insulation_i \times Cohort_i \times Earnings$" captures the difference in effect between those with parental earnings above and below median earnings 1970 and indicates a model fully interacted by an indicator for above median earnings. 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. *Robustness test: Effect of a 6-month parental reconsideration period for divorce on upper secondary school completion.*

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

Note: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Standard errors in parenthesis are clustered at the household level. "$Insulation_i$" indicates greater age spacing than the reference group, where the cutoff varies by column. The interaction with cohort shows the difference in effect between cohort groups 1956–1964 and 1952–1955. 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 10. *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	
Sample:	All children		All children	Elder sibs.	All children	Elder sibs.
Outcome:	Upper sec. completion		Upper sec. completion		Upper sec. completion	
Divorce by 18	−0.075***	−0.022***				
	(0.002)	(0.008)				
$Insulation_i \times Cohort_i \geq 1956$			0.017***	0.013	0.018***	0.007**
			(0.004)	(0.009)	(0.002)	(0.004)
$Insulation_i$			−0.004	−0.007	−0.004**	0.006*
			(0.005)	(0.011)	(0.002)	(0.003)
Mean dep. var.	0.811	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
Cohort FE	✓				✓	✓
Linear controls		✓	✓	✓		
Family FE		✓	✓	✓		

Note: ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$. Standard errors in parenthesis are clustered at the household level. "Divorce by 18" refers to experiencing parental divorce by age 18. "$Insulation_i$" 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 11. *Effect of a 6-month parental reconsideration period for divorce on parental labor market outcomes in 1975.*

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

Note: ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$. Standard errors in parenthesis are clustered at the household level. The outcomes are estimated using the oldest children in each family. "$Insulation_i$" 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 12. *Effect of a 6-month parental reconsideration period for divorce on the intergenerational correlation in educational outcomes.*

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

Note: ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$. The outcome is defined as child's length of education in years 1990, which is regressed on the mother's or father's length of education in years. 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 13. *Effect of a 6-month parental reconsideration period for divorce on non-cognitive and cognitive abilities age 18.*

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

Note: ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$. Standard errors in parenthesis are clustered at the household level. "Maturity" refers to social maturity, "Ps. energy" to psychological energy, "Stability" to emotional stability. "Logic" refers to logical thinking, "Verbal" to verbal ability, "Spatial" to 3D spatial thinking, and "Technical" to a technical understanding test. All outcomes are measured at approximately age 18. "$Insulation_i$" 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 14. *Effect of a 6-month parental reconsideration period for divorce on fertility outcomes.*

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

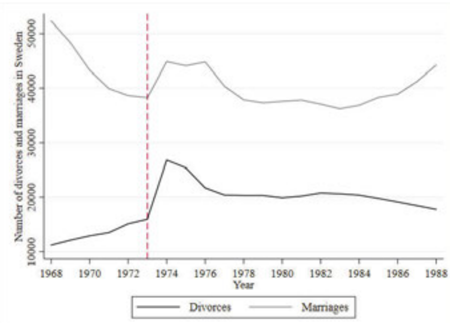
Note: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Standard errors in parenthesis are clustered at the household level. “Ever parent” and “Age at first child” refers to being a parent by year 2014 (the final year of the MGR from which these outcomes are taken) and the age of the child at the time 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 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.*

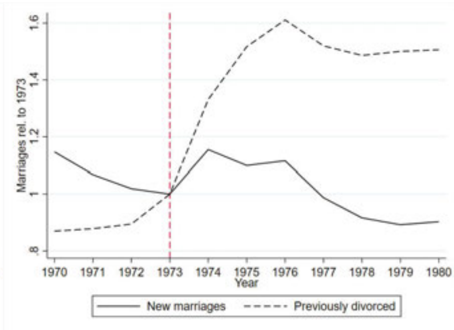
Panel A	Impact on NC	Impact on Cog.	Impact on outc.	NC	Cog.	NC part (1)x(4)	Cog. part (2)x(5)	Total (3)+(6) +(7)	Share NC (6)/(8)	Share Cog. (7)/(8)	Share resid. (3)/(8)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Outcome:											
Ln earn.	0.040	0.053	0.0089	0.1689	0.0621	0.0068	0.0033	0.0190	0.36	0.17	0.47
Ever marr.	0.040	0.053	0.0084	0.1452	0.0412	0.0058	0.0022	0.0164	0.36	0.13	0.51
Panel B	Impact on US.	Impact on Div.	Impact on outc.	US.	Div.	US. part (1)x(4)	Div. part (2)x(5)	Total (3)+(6) +(7)	Share US. (6)/(8)	Share Div. (7)/(8)	Share resid. (3)/(8)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Outcome:											
Ln earn.	0.015	-0.022	0.0068	0.1766	-0.0562	0.0026	0.0012	0.0106	0.24	0.11	0.64
Ever marr.	0.015	-0.022	0.0052	0.0565	-0.0405	0.0008	0.0009	0.0069	0.12	0.13	0.75

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

Appendix A Supporting figures

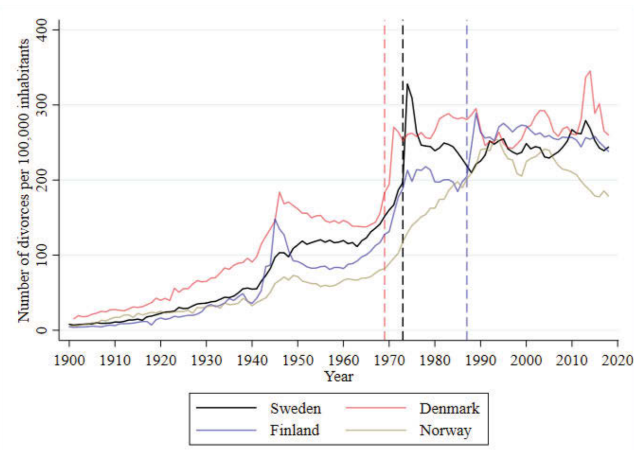


(a) Number of divorces and marriages in Sweden 1968–1988.



(b) Number of new marriages and marriages between divorced individuals 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.

Appendix B Supporting information and results

Additional information on divorce laws in Sweden 1915–1973

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

All separations did not result in divorce, as some couples reverted back to married life or simply chose to remain de facto separated without finalizing the divorce. The government bill from 1973 looking into this acknowledged that separations not leading to divorce may be due to some couples choosing to stay legally married while remaining separated. It was also stated in the bill that the vast majority of separations not being realized as divorces were due to the couple resuming the marriage, hinting at the potential stabilizing effect of divorce restrictions on marriages (Prop. 1973:32, 1973). 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 reform indicate that 25% of all 1-year separations taking place did not result in divorce (SOU 1972:41, 1972). If the spouses did not finalize the divorce following the 1-year separation period, the separation appears to have continued indefinitely regardless of the couple resuming married life or not.

The divorce laws remained stable during the entire time period before the reform in 1974 with one exception. From July 1, 1969, the divorce laws were revised to also 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).

Additional information on the divorce law reform in 1974

The first step toward the divorce law reform was taken a few years prior to 1974 through a public policy report aimed at modernizing the divorce laws. The report was ordered by the government in 1969, and then presented to the parliament in 1972 (SOU 1972:41, 1972). The reform was then passed by the parliament in early 1973, and enacted January 1, 1974 (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.[59] 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.[60]

[59] 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.

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

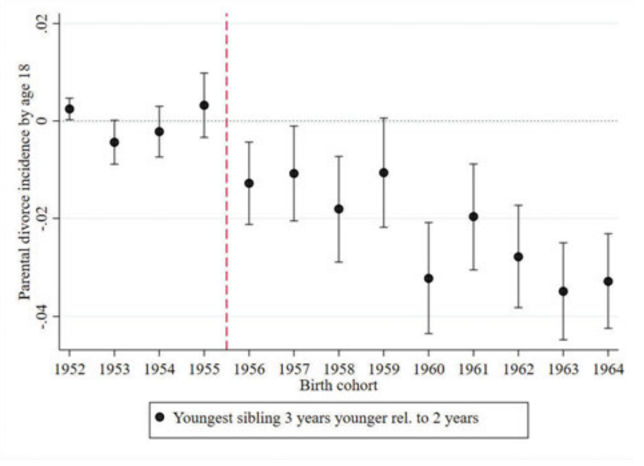


Figure B1. The figure disaggregates the main estimate on experiencing parental divorce by following cohorts born 1952–1964 until age 18 separately by birth cohort and estimates the difference in parental divorce rate between those with 3 years of age spacing to their youngest sibling against the smaller age spacing group with 2 years of spacing. The controls include parents' birth cohort, 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.

Table B1. *Robustness test: Effect of a 6-month parental reconsideration period for divorce on upper secondary school completion with restrictive controls and age spacing checks.*

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

Note: $***p < 0.01$, $**p < 0.05$, $*p < 0.1$. Standard errors in parenthesis are clustered at the household level. "Extensive controls" refers to adding potentially bad controls, which strongly correlate with the age spacing groups used to capture the effects of the divorce restriction. "Full sp." refers to including children with age spacing 9–18 in the insulation group with greater age spacing. *"Rev. sp." removes the children where a new sibling born 1971–1973 changes them into the category with age spacing greater than 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 in 1990–2015.*

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

Note: $***p < 0.01$, $**p < 0.05$, $*p < 0.1$. Standard errors in parenthesis are clustered at the household level. The outcomes are estimated using the oldest children in each family. "Marr./Cohab." refers to married or cohabiting in 1990, and "Death" refers to death by 2015. "$Insulation_i$" 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.

Table B3. *Effect of a 6-month parental reconsideration period for divorce on elder siblings' university and labor market outcomes in 1990.*

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

Note: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Standard errors in parenthesis are clustered at the household level. "University graduation" refers to three years or more of university education in 1990. Earnings (SEK 100) and employment outcomes are for the same year. "$Insulation_i$" indicates greater age spacing (3–8 years to youngest sibling against 1–2 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 1–2.

Table B4. *Effect of a 6-month parental reconsideration period for divorce on elder siblings' family outcomes in 1990 & 2000.*

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

Note: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Standard errors in parenthesis are clustered at the household level. "Ever married" and "Ever divorced" refers to ever marrying or divorcing by year 2000. "Single parent" is defined through the census in 1990, "Marr./Cohab." is defined as cohabiting or being married, "Cohabiting" is defined as cohabiting without being married, and "Parent" is defined as having a child age 0–6 at the same year. "$Insulation_i$" indicates greater age spacing (3–8 years to youngest sibling against 1–2 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 1–2.

Framework timeline

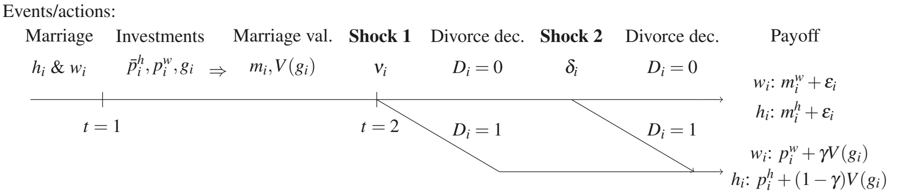


Figure B2. Timeline of the divorce framework. Husband (h_i) and wife (w_i) choose private investments (p_i) in period 1. Besides private investments, the wife also chooses to allocate resources to the joint marriage good g_i. Going into period 2, the marriage value m_i is hit by a preference shock ν_i, after which the spouses can choose to divorce or not. If they survive the first shock, the spouses are then hit by a second shock δ_i after which they can again choose to take out a divorce or not. Should they survive both shocks, the spouses split the excess marriage value based on the marriage being intact. In case of divorce, they get the payoffs associated with that state.

Theoretical framework

The setting

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

The framework

Formally, the framework consists of two agents (husband h_i and wife w_i of family i) who are exogenously matched to each other and live for two time periods ($t = \{1,2\}$). The first period symbolizes the early years of marriage with marital investments, family formation, and career development, while the second period captures the remainder of the time when the children are older. In the first period, $t = 1$, the wife chooses to invest in an intermediate marriage good (g_i, e.g. home production and children) with the price normalized to unity, which is carried forward into the next period. Investments in the marriage good g_i are assumed to be beneficial for the children and improve their long-term outcomes. The marriage good is then used as input in the production function $V(g_i)$, a strictly increasing, concave function ($V'(g_i) > 0$, $V''(g_i) < 0$) where the non-rivalrous output is enjoyed equally by both spouses during marriage. The husband and wife also invest in a private good (p_i^w and p_i^h, e.g. personal career and private contacts) 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 $\bar{p}_i^h = p_i^h$.[61] 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.[62] Time period 1 actions of the husband and wife imply that the gains from marriage at this point in time are defined as:

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

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

In period 2, the spouses are subject to an information shock $\varepsilon \sim F(\cdot)$ with support $(-\infty, \infty)$, which may drive the marriage value into the negative domain and incentivize divorce. Ex ante the spouses have no expectation of the sign of the shock ($\mathbb{E}[\varepsilon] = 0$), and it is assumed to affect both spouses in the same way once it is realized. For couples with a positive information shock, it simply

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

[62] 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.

increases the marriage value and causes no new actions. A novel feature of this model is that the shock to the marriage value consists of two uncorrelated components ν and δ (where $\varepsilon = \nu + \delta, Cov(\nu, \delta) = 0$). Just like the composite term (ε_i), the shocks are mean zero ex ante ($\mathbb{E}[\nu] = \mathbb{E}[\delta] = 0$). The first shock ($\nu$) is observable directly going into period 2, and the second shock (δ) is realized ex post during this period. The nature of these two shocks means that some marriages will have a perceived negative marriage value when observing the first shock's value, only that ex post observing the second shock would have reverted the marriage value back into the positive domain. Likewise, a marriage may be revealed over time be be of negative value as the second shock (δ) is realized, prompting a later divorce. The key part is that δ is never realized if the divorce happens at the start of period 2 when the first shock ν is observed.

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

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

And under divorce:

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

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

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

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

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

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

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

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

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

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

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

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

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

$$-2m_i^w f(\hat{\nu}_i)\hat{\nu}_i'(g_i)(1-F(\hat{\nu}_i)) + u_{i,2}^{w,d\prime}(g_i)(2-F(\hat{\nu}_i))F(\hat{\nu}_i)$$
$$+2u_{i,2}^{w,d} f(\hat{\nu}_i)\hat{\nu}_i'(g_i)(1-F(\hat{\nu}_i)) - \mathbb{E}_{\nu,\delta}[\varepsilon_i|\nu_i > \hat{\nu}_i, \varepsilon_i > \hat{\nu}_i] f(\hat{\nu}_i)\hat{\nu}_i'(g_i)(1-F(\hat{\nu}_i))$$
$$-2\hat{\nu}_i(g_i) f(\hat{\nu}_i)\hat{\nu}_i'(g_i)(1-F(\hat{\nu}_i)) = 0$$

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

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

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

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

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

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

A few things can be learned from the model setup. The first information shock ν_i will lead to some impetuous divorces happening due to couples not remaining in the marriage until the second information shock (δ_i) is realized. With the marriage ending at the start of period 2, the remaining information is never realized as the marriage has ended. Contrarily, some spouses remaining married in period 2 will divorce during this period when the second information shock δ_i is realized. From the wife's point of view, the optimal threshold for divorce, and thus divorce risk, increases with a spouse's bargaining position $(1-\mu)$, with lower spousal investments ($\bar{p}_i^h$) and higher own investment capabilities ($\bar{p}_i^w$), with a high degree of capture of the joint marital investments following divorce (γ), and lower own marital investments (g_i). The risk of divorce causes women to reduce gainful investments in the marriage good due to them insuring against divorce with private investments. A condensed timeline of the model can be seen in Figure B2.

A final feature of the model is the introduction of a waiting period for divorce, in line with the divorce restriction introduced 1974 in Sweden. This is modeled 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:

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

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 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.[63]

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

Essay II. The effects of water fluoridation during childhood on human capital outcomes

Acknowledgements: I thank my supervisors, Hans Grönqvist and Helena Svaleryd, for their help and feedback throughout this project, IFAU for data access, and Rolf Jonsson at Norrköping city archive for additional information related to the project. I also thank the participants of ULG at Uppsala University Department of Economics, participants at the 2022 SOFI Workshop on Children and Health, Simon Ek, Erik Grönqvist, Mattias Öhman, Linuz Aggeborn, Douglas Almond, Matthew Neidell, Lena Edlund, and Peter Sandholt Jensen for valuable feedback on various parts of this project.

1 Introduction

More than 380 million people globally are exposed to artificial fluoridation of their drinking water in order to improve their dental health (Aoun et al., 2018). The beneficial effects of water fluoridation in terms of dental outcomes have long since been confirmed by a range of studies in different settings (e.g. Dean, 1954; Twetman et al., 2003; O'Mullane et al., 2016), but the overall benefits of water fluoridation have been questioned. The criticism is based on scepticism toward water additives in general, and specifically on the fact that fluoride, at concentrations much higher than the levels given by artificial fluoridation, is a harmful neurotoxin, potentially deadly to humans (Zuo et al., 2018). The question for policy makers thus relates to the optimal concentration of fluoride in drinking water.

Since the inception of artificial water fluoridation in the 1940s, the sceptics have fiercely criticized these policies to the extent that the public debate was dubbed the "Fluoride war" (Gravitz, 2021). This war is still raging. But the proponents of water fluoridation have the support of major NGOs, such as the WHO, and can show a clear benefit of the policy in terms of improved dental health. However, the unconfirmed detrimental effects of water fluoridation on children's cognitive development are becoming increasingly studied (Saeed et al., 2020). A widely-cited meta study on the topic finds that exposure to high concentrations of drinking water fluoride is associated with almost half a standard deviation decrease in children's IQ (Choi et al., 2012). Unfortunately, many of the studies on the topic are either based on observational findings, originate from countries with poor data quality, or investigate exposure to fluoride levels far above the WHO-recommended threshold of 1.5 mg/L water (Gopu et al., 2022).

In this paper, I contribute to the existing literature by evaluating the effects of the water fluoridation experiment in Norrköping, Sweden, during 1952–1962 on human capital outcomes for the affected children. Roughly one third of the population in Norrköping was subject to uninformed fluoridation of their drinking water by the local municipality during this time period. The reasons for the experiment were that observational studies in the U.S. had shown beneficial effects of fluoride exposure on children's dental health, although the evidence at that time was far from conclusive. Simultaneously, policy makers in Norrköping were struggling with poor dental health in the city and a shortage of dentists. These factors, along with ambitious local health experts and the dual water system in the city, led to the Norrköping water fluoridation experiment of 1952–1962.

The dual water system in Norrköping, stemming from the same water supply, allowed the policy makers to treat parts of the population in the city while ensuring that the control group consumed identical water except for the added fluoride during 10 years. Evaluations during and after the experiment show sizable dental health improvements for the children affected by the water fluoridation, and later follow-ups found positive dental effects more than 20 years after the experiment ended (Melander, 1957; Sellman and Syrrist, 1968; Linder, 1971; Lundström et al., 1983). This study extends the existing research by evaluating the experiment in terms human capital outcomes.

The empirical analysis draws on rich, Swedish administrative data from the quinquennial censuses of 1960–1990, allowing me to track the children residing in Norrköping around the time of the experiment. My main sample consists of 10,164 children born 1951–1970 residing in the treatment and control zones of the experiment. The children are linked to registers with information on their schooling and labor market outcomes as adults in year 1990. I also link the children to data 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 at age 18–19.

The sampled children are assigned to the treatment and control zones in Norrköping through their parish of residence in the 1960 census. Given that the water fluoridation ceased in February 1962, the cohorts born from 1963–1970 are used as the primary placebo cohorts, while the cohorts affected by water fluoridation during childhood are those born 1951–1962. Combining treatment assignment with differential cohort exposure to water fluoridation, the main empirical specification is a differences-in-differences (DiD) design focusing on estimating average treatment effects for the cohorts born during the fluoridation period. An alternative specification instead focuses on estimating linear treatment effects based on years of exposure to fluoridation, while still netting out the main effects of living in the treated versus control zone.

The DiD-based evaluation shows that water fluoridation exposure is associated with negative but not statistically significant effects on the affected children's cognitive ability (-0.073 SD, s.e. 0.063), significant negative effects on their non-cognitive ability (-0.161 SD, s.e. 0.066), and large, detrimental effects on their high school completion rate (-0.048, s.e. 0.017). However, measuring treatment intensity based on years of exposure to water fluoridation produces statistically significant estimates for all of the aforementioned outcomes. For every additional year of exposure to fluoridation, the children's cognitive ability (-0.014 SD, s.e. 0.009) and non-cognitive ability (-0.019 SD, s.e. 0.009) at age 18–19 decrease by 1.4–1.9 pp. of a standard deviation, and the probability of graduating from high school decreases by 0.6 pp. (-0.006, s.e. 0.003).

I relate to the existing literature by providing empirical estimates, based on quasi-experimental variation, of the effects of water fluoridation on children's human capital outcomes. Contrary to previous work in the economics literature (Glied and Neidell, 2010; Aggeborn and Öhman, 2021), I leverage an experimental increase of fluoride concentration in drinking water relevant to those targeted by public policy today and find negative effects on human capital outcomes. These findings differ from the previous studies with causal interpretation presenting null effects from fluoride exposure during childhood on cognitive ability and non-cognitive ability.

The findings of this study complement the existing causal evidence by overcoming the main empirical difficulties of the previous two studies. The experimental setting in Norrköping allows me to account for pre-existing differences or sorting of those living in treatment and control areas, and it greatly reduces the risk of bundled treatment linked to natural fluoride exposure.[1] On the other hand, I face other empirical drawbacks, such as extensive mobility from my treatment and control areas and the fact that I rely on a single event for identification. I investigate these concerns by showing that my findings are robust to various specification tests, e.g. **i)** verifying that there are no great selection problems in the treatment and control area over time, and **ii)** using cluster-robust inference to account for intra-correlation in treatment and control area outcomes. The findings of this study warrant further empirical evidence and caution from policy makers regarding safe levels of fluoride concentration in drinking water.

2 Literature review

Fluoride and dental health outcomes

Fluorine is a common element existing naturally only in anionic form as fluoride, comprising 0.006–0.009% of the Earth's crust (WHO, 1994). Human exposure to fluoride most often occurs through food and drinks consumption, mainly from fluids such as water. In general, drinking water from freshwater lakes contain very little natural fluoride (0.01–0.3 mg/L), while water from deeper sources, such as groundwater, is potentially more rich in fluoride (0.1–6 mg/L) depending on the local bedrock (EFSA, 2013). In these quantities, fluoride is odor- and tasteless.

The empirical link between fluoride exposure and improved dental health is well established (Twetman et al., 2003; O'Mullane et al., 2016). Fluoride serves to strengthen teeth by making the enamel more resistant to acid attacks and caries, and research has shown that fluoride exposure is the most effective when new teeth erupt for children rather than later in life (Singh et al., 2003). The initial evidence of this link came about during the late 1930s to early 1940s in the U.S., with researchers observing a positive correlation between water fluoride concentration and improved dental health (Dean, 1954). Similar evidence has since then emerged across the world.

Previous studies have shown that water fluoridation can reduce caries by as much as 30–70%, but recent research suggests that the effect sizes have atten-

[1] In this case, bundled treatment refers to the general water composition shifting in tandem with the natural fluoride concentration. This implies that exposure to natural fluoride could be bundled with exposure to other compounds in the water.

uated, possibly due to other modern prophylactic measures and better health in general. The contemporary effect sizes may be closer to 10–20% (Slade et al., 2018). This reduction could be explained by other modern prophylactic dental health measures and better health in general. Nonetheless, the WHO still recommends artificial fluoridation of public water supply at a concentration of 0.7–1.5 mg/L water (WHO, 1994; WHO, 2019).

Fluoride and children's development

The scientific evidence linking fluoride exposure and children's development is mixed (Gopu et al., 2022). There is ample evidence that exposure to very high doses of fluoride is toxic and potentially lethal to humans (Zuo et al., 2018).[2] However, the levels of fluoride deemed toxic tend to far exceed the WHO-recommended threshold of 1.5 mg/L water. The vast majority of studies investigating the effects of low levels of fluoride exposure are based on observational work comparing individuals residing in areas with differential exposure to artificial or natural fluoride. This means that most evidence rests on correlational studies with different drawbacks to identification (Saeed et al., 2020; Gopu et al., 2022).

A wide range of observational studies find negative effects of fluoride exposure on children's IQ, and some of these studies indicate that fluoride is especially detrimental for children's developing brain at high levels of exposure (Choi et al., 2012). Yu et al., 2018, however, report findings of negative effects on IQ from exposure around the threshold value suggested by the WHO (1.5 mg/L water). In line with this, three separate studies from Canada and Mexico find that exposure to water fluoride during childhood or in utero is associated with significantly lower IQ later in life (Bashash et al., 2017; Green et al., 2019; Till et al., 2020). There is also a study showing detrimental effects of fluoride exposure on children's IQ starting at levels as low as 0.2 mg/L drinking water (Grandjean et al., 2020). Furthermore, Bashash et al., 2018 find evidence of effects beyond children's IQ by presenting a link between prenatal fluoride exposure and inattention difficulties for children, indicating potential effects on behavioral problems and non-cognitive abilities.

Some mechanisms explaining how fluoride exposure affects children's IQ have been proposed in the literature. A recent study presents evidence that fluoride exposure of an additional 0.5 mg/L water increases the risk of hypothyroidism for pregnant women (Hall et al., 2023). Since hypothyroidism is known to cause adverse effects on fetal development, this may help explain the observed negative effects on children's IQ. Based on animal studies, evidence from lab experiments exposing rats to high amounts of fluoride shows that it is

[2]The medical literature has not found any evidence that exposure to natural fluoride differs from that of artificial water fluoridation. Although, this claim is mainly founded on studies with very small sample sizes (e.g. Whitford et al., 2008).

able to pass the blood-brain barrier, and that high doses can impair cognitive functions such as memory and may be toxic to the developing brain (Mullenix et al., 1995; Bartos et al., 2018). However, it should be noted that not all lab studies exposing rats to fluoride find adverse effects (McPherson et al., 2018).

Contrarily to the studies with detrimental findings, Broadbent et al., 2015 find no association between water fluoride exposure during preschool ages and IQ outcomes in New Zealand. Likewise, Soto-Barreras et al., 2019 present null effects linking water fluoridation and children's IQ for a small sample of children in Mexico. A meta study on fluoride exposure and children's IQ by Miranda et al., 2021 also finds no clear evidence of negative effects linked to low levels of fluoride exposure, but the authors highlight that there are few high-quality studies of on the topic.

The most well-identified studies, with empirical strategies to capture the causal effects of fluoride exposure below 1.5 mg/L water, are found in the economics literature (Glied and Neidell, 2010; Aggeborn and Öhman, 2021). The first paper, by Glied and Neidell, 2010, presents evidence relying on timing differences in U.S. county adoption of water fluoridation for identification. They show that children affected by water fluoridation in the 1950s–1970s exhibit increased earnings as adults and no significant effects on their cognitive ability. The effect on earnings is driven by women, and the authors interpret their findings as positive returns to good dental health for women in service occupations. Aggeborn and Öhman, 2021 instead use Swedish data on geographical variation in exposure to natural fluoride in drinking water. Using a fixed effects strategy, they are able to compare individuals with differential fluoride exposure within tight geographical areas and find no effects on children's cognitive ability. These studies do, however, face empirical challenges in not accounting for pre-existing differences of those living in treatment and control areas or fully eliminating the risk of bundled treatment linked to natural fluoride exposure.

3 Background

The fluoridation of drinking water in Norrköping 1952–1962

The Norrköping water fluoridation experiment started in early February 1952 and ran for 10 full years until February 1962, with the low zone of the city's drinking water being fluoridated with 1–1.2 mg/L water. The high zone water supply was kept at the natural fluoride level of less than 0.1 mg/L water (Melander, 1957). The active compound used to fluoridate the water, added in the water plant in Fiskeby just west of Norrköping, was sodium fluorosilicate (Na_2SiF_6), a standard compound frequently used to artificially fluoridate drinking water.[3]

The main motivation for the experiment was to improve the dental health of the inhabitants and to alleviate pressure from the rapidly expanding public dental health care system. Instead of fluoridating the drinking water of the entire city at once, the stated aim of the water fluoridation was to use experimental methods to ascertain the true caries-reducing impact of fluoride exposure in a well-identified setting. In doing so, the policy makers in Norrköping kept all other factors affecting the outcome constant, unlike the previous evidence based on observational studies.

The idea from the start of the experiment was to study the effects of water fluoridation on children's dental health, since water fluoridation was expected to have the largest benefits when the permanent teeth erupt. Due to likely problems with spillovers within the city based on children living in one area and attending school in the other, the studied children were divided into 4 groups: Group 0 both lived in and went to school in the control zone, while Group 3 lived in and went to school in the fluoridated low zone. Group 1 and 2 were partially treated by either residing in or going to school in the opposite zone and were thus excluded from the original analysis.[4]

The municipal officials responsible for the experiment, Dr. Allan Melander, had no formal training as a researcher beyond attending medical school. For this reason, he cooperated with Professors Bengt Gustafsson and Arvid Syrrist and used them as a scientific consultants for the experimental setup and the subsequent evaluation. Prof. Bengt Gustafsson had previously been one of the chief architects of the notorious Vipeholm Dental Caries Study during the mid 1940s, where they used an experimental setup in a mental asylum to evaluate the effects of sugar consumption on caries prevalence.[5]

The fluoride concentration was measured regularly to ascertain that adequate amounts were in the water supply.[6] There are reports indicating some initial problems with the fluoride dispenser, which appear to have led to uneven and lower than intended fluoride levels before the problems were solved by 1953 (see Figure A15 in Appendix A). As mentioned, the baseline fluoride levels before the fluoridation was close to 0, with the water being sourced from a nearby freshwater lake.[7]

[3]Roughly 29% of the 200 million U.S. inhabitants exposed to artificial fluoridation in 2008 had access to water supply fluoridated with this particular compound (Whitford et al., 2008).

[4]Raw data from the original experiment for a sample of the children indicates that roughly 11% of the children belonged to Group 1 and 2 and were thus excluded from the analysis.

[5]The experiments performed at Vipeholm were later deemed to be highly unethical since the subjects living in the asylum had intellectual disabilities or mental health problems and did not consent to being part of the experiment. The archives show an extensive written communication between Melander and the two researchers before and during the experiment.

[6]The number of tests were usually more than 400 per year (see Figure A15).

[7]Urine samples of children living in and outside of the fluoridation zone before and after the experiment commenced showed highly similar concentrations of fluoride before the experiment.

The elevation difference between the two drinking water zones in the city means that the water supply, sourced from the river Motala ström and the freshwater lake Glansjön west of the city, is funneled through distinct pipe systems supplying the low zone and high zone respectively. To the best of my knowledge, it was not clear to the inhabitants which water zone they belonged to. It was also not clear which zone was subject to water fluoridation.[8] Or for that matter, that there are two different water zones in the city.

The inhabitants of Norrköping were not publicly informed about the experiment and were kept in the dark as much as possible to avoid behavioral responses. Any criticism or questions about the potential toxicity of fluoride surfacing as letters to the local newspaper were dismissed by the policy makers as being unscientific. The chief medical officer in Norrköping, Dr. Melander, mentions news items about the experiment having appeared in the local newspaper by 1955. However, Melander never publicly disclosed which part of Norrköping that was fluoridated. The local opponents of the experiment complained, for instance, of badly smelling water in areas not subject to fluoridation. These complaints were disregarded by Melander, since fluoride is odorless in the quantities added.[9]

The water fluoridation experiment in Norrköping met with resistance early on at the national level. The Royal Board of Health in Sweden never endorsed the experiment and tried continuously to stop it while keeping it under review throughout its duration. The local politicians in Norrköping fought fiercely against the decision to halt the experiment and only did so in 1962, when the Court of Administrative Justice made a ruling and ordered them cease with the water fluoridation.[10]

Evaluations of the experiment in terms of dental health outcomes

The Norrköping water fluoridation experiment was the first scientific experiment to use within-city variation to get causal estimates on the effects of water fluoridation in terms of dental outcomes (Melander, 1957). Dr. Melander, the medical expert responsible for the experiment, was the first to evaluate its effects four years after the start of the water fluoridation. His study finds that

Furthermore, a urine sample record of a handful of children subject to the experiment, albeit somewhat hard to interpret, indicates higher levels of fluoride in the treated children's urine one year after the experiment had started.

[8]For instance, two letters in the Norrköping city archive sent to the editor of the local newspaper in Norrköping indicate that the exact border of the water fluoridation zone was not salient to everyone in 1958 and 1963.

[9]Melander also made sure to write responses arguing for the substantial beneficial effects of fluoride, strongly pushing the positive aspects and benevolent intentions of the experiment. In 1955, Melander wrote in a report to an American colleague that only a few inhabitants had voiced concern of the experiment and that the general public had accepted the experiment.

[10]The court ruling took place on December 7, 1961, and stated that the experiment was not abiding to the national legislation. The fluoridation subsequently ceased February 1, 1962.

the treated children born 1947 exhibit a 40.2% reduction in caries compared to the children in the control group born the same year.[11] Checkups of the dental health of children at age 8–10 before the start of the water fluoridation show that dental health was almost perfectly balanced between the treated and control children at the onset of the experiment.[12]

Investigations 7 years into the experiment, in 1959, by Sellman and Syrrist, 1968, show a decrease in caries prevalence of 52.4% for the 7-year-olds fluoridated throughout their lives relative to the control group. For 14-year-olds, the reduction amounts to 31.4%.[13] Linder, 1971 later compares the 1955 and 1962 cohorts of children in the treatment and control group at age 7 and finds that caries activity increased by 30.8% after the water fluoridation had ceased for the treated cohort born 1962, relative to the control group and the treated cohort born 1955.

Finally, Lundström et al., 1983 performed a follow-up study 20 years after the water fluoridation had ended. They set out to recreate data from the original experiment by recruiting adults born 1953 in Norrköping who still resided in the city in 1981. Their findings show that the caries reductions in 1981 amounted to a statistically significant decrease of 30% for those who grew up in the fluoridated zone relative to the control area.

4 Data and empirical method

Data

The main data source for this project consists of Swedish census data from the quinquennial censuses [Folk- och bostadsräkningarna] of 1960–1990 and the Multi-Generation Register [Flergenerationsregistret] containing information on family linkages for all individuals born after 1932. The census includes information on parish of residence, which is used to assign treatment and control status for the relevant cohorts of children. These data are combined with register data on educational attainment and labor market outcomes in 1990 and 2010 for the affected children.[14] The sample used to evaluate the Norrköping experiment amounts to 10,164 children born 1951–1970 living in

[11] However, he finds small to no effects on the children age 14–18 years after four years of water fluoridation exposure.

[12] Allan Melander passed away from cancer in 1958 (age 61), and thus published no further findings from the Norrköping experiment.

[13] Comparing specific results from the Norrköping experiment to U.S. studies, Sellman and Syrrist, 1968 find a highly similar effect in the reduction of caries-damaged surfaces for 14-year-olds of 27.1–27.5% in Norrköping, Evanston, and Grand Rapids (other fluoridated cities).

[14] Dental health data which can be linked to the full population in Sweden only exist from around year 2010, and the information is deemed to be highly sensitive. For these reasons, I rely on the findings of the previous studies for dental health evaluations.

the treated and control parishes, with one third of the sample residing in the treated parish.

I supplement the data by adding information from the Swedish War Archive [Krigsarkivet] 1969–1997 on eight subscores of non-cognitive ability and cognitive ability, and the two composite ability measures. 88% of the Swedish men in the cohorts I study performed these mandatory tests around age 18–19. The measure of cognitive ability consists of four subtests of logical, verbal, and spatial abilities, as well as technical comprehension. The cognitive tests are based on timed multiple-choice questions, which I standardize to be mean-zero, standard deviation one by cohort. These outcomes have been shown to strongly correlate with important outcomes later in life, such as labor market outcomes (Lindqvist and Vestman, 2011).

The Swedish War Archive also contains information on the conscript's non-cognitive ability, which is based on a standardized psychological evaluation of 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' responses on a range of topics related to leadership and coping under pressure. The interviewer gives a high score if the conscript is deemed to be socially mature, persistent, willing to assume responsibility, able to take initiative, and emotionally stable (Black et al., 2018).

Identification strategy

Norrköping metropolitan area is encompassed by 6 different parishes, which are fundamental in assigning treatment and control status for the affected children.[15] Specifically, I use information on the parish of residence in the 1960 census to track the children affected by the water fluoridation.[16] The strategy relies on comparing inhabitants of the only fully treated eastern parish (Hedvig parish) in Norrköping to those residing in the two western parishes without water fluoridation (Östra Eneby and Borg), effectively excluding the three partially treated parishes (Matteus, S:t Olai, and S:t Johannes) with parts of the parish area included in or being directly adjacent to the fluoridation zone.[17] This restriction mimics the original experiment and reduces the risk of spillovers from children with partial exposure to the water fluoridation.

The extensive mobility between parishes during this time period means that any treatment assignment year will likely to give rise to non-trivial attenuation

[15] See Figure 1 for a parish map of Norrköping.

[16] Children born before 1961 are assigned treatment status based on their parish of residence in the 1960 census. The children born from 1961 and onward are too young to appear in that census, so they are assigned to their mother's parish of residence in the 1960 census.

[17] Contemporary water zone maps of Norrköping show that the water zones and their mapping to the experiment have remained consistent over time.

bias, which will lead me to underestimate any effects.[18] Treatment assignment in 1960 during the experiment implies that I also face a greater risk of capturing behavioral responses to the experiment. However, conditioning on being a resident in 1960 means that I guarantee that the treated children in my sample are exposed to water fluoridation for at least one full year. It also means that I avoid the high mobility around the time of childbirth for most cohorts born during the experiment.[19] This should give me the best chances of capturing the effects of water fluoridation, with the caveat that only a subset of the affected children are exposed for the number of years associated with their birth cohort.

The cohorts affected by water fluoridation when young are defined as those born 1951–1962, with the exposure length varying by birth cohort. This selection stems from the indication in the previous literature that any adverse effects of water fluoridation on children's development should be the strongest for exposure during childhood (e.g. Choi et al., 2015). Given that the fluoridation experiment ceased early on in 1962, the children born from 1963 are unexposed to the direct effects of water fluoridation, and will be used as a placebo group to net out any main effects of living in the treated and control parishes in the empirical specifications outlined below.[20]

The first of the two main empirical specifications used in the paper is based on the following DiD regression:

$$\begin{aligned} y_{i,j,p} = \beta_0 + \beta_1 Fluoride_p \times \mathbb{1}[Cohort_j \leq 1962] + \beta_2 Fluoride_p \\ + \sum_{j=1952}^{1970} D_j + \boldsymbol{X_i'\delta} + \varepsilon_{i,j,p} \end{aligned}$$

where the outcome of interest, $y_{i,j,p}$, is observed for the individual child i of cohort j residing in parish p. $Fluoride_p$ is a parish-level indicator taking the value one (1) for the children residing in the treated parish and zero (0) for those residing in the control parishes. The coefficient β_1 on the interaction term captures the treatment effects for those residing in the fluoridated parish, while netting out the main effects (β_2) for the placebo cohorts in the treatment and control parishes born after the fluoridation had ceased in 1962.[21]

[18] The extensive mobility around the time of childbirth is evident in aggregate statistics. For instance, more than 60% of the children born in the fluoridated parish in 1960 had moved to a different parish by 1970.

[19] In the robustness section, I add data on parish of birth and verify that conditioning on the children remaining in their parish of birth in the 1960 census does not affect the main estimates.

[20] These cohorts are deemed to be the best placebo cohort groups, but the identifying assumptions require that the differential behavioral responses to fluoridation after the policy had ceased are small or non-existent for the 1963–1970 cohorts.

[21] Heteroscedasticity-robust standard errors are used in the main specifications, however in the online Appendix I show that the findings are also robust to clustering at the parish-by-birth cohort level, and when using wild bootstrapping to cluster at the parish level.

D_j are cohort indicators, and $\boldsymbol{X_i}$ includes controls based on individual-level background characteristics from the 1960 census and the Multi-Generation Register.[22] The identifying assumption for this regression specification relies on the standard parallel trends assumption for DiD specifications (Angrist and Pischke, 2008). In other words, that the outcome in the treatment and control area evolves similarly under the absence of water fluoridation.[23]

I also use a secondary specification, aimed at capturing years of exposure to the water fluoridation, to investigate linear treatment effects. This is done by replacing the cohort group indicator with a linear term, $Intensity_j$, capturing years of exposure based on birth cohort.[24] In this specification, those born from 1963 are exposed for 0 years, with the years of exposure increasing incrementally by 1 year for those born 1962 up to 1952:

$$y_{i,j,p} = \gamma_0 + \gamma_1 Fluoride_p \times Intensity_j + \gamma_2 Fluoride_p + \sum_{j=1953}^{1970} D_j + \boldsymbol{X_i'\delta} + \varepsilon_{i,j,p}$$

Descriptive statistics

In Table 1, I present descriptive statistics for the main sample split by treatment and control parishes along with the DiD difference. The table shows that there are differences between the treated and control parishes. For instance, the children in the control parishes are more likely to have a working father, and less likely to have a working mother. The differences are not unexpected given that I partition the city into geographic sections where different types of families may reside.

These differences, however, are only a concern for the identification strategy if they should change over cohort groups and affect the parallel trends assumption. The DiD difference in the same table shows that the vast majority of characteristics are constant over cohort groups, with the only statistically

[22] Family characteristics include the parents' birth year, sex of the child, the child's birth order for the parents, birth month, and the parents' number of children. Parental characteristics include information on if the mother ever married, the father's employment status in 1960, and if he has any higher education the same year.

[23] I note that the positive effects of water fluoridation on dental health should only serve to attenuate any findings of negative effects on children's development, since there are no a priori reasons to believe that better dental health would cause negative effects on children's human capital development. Also, these positive effects from improved dental health should be already evident for the cohorts born from 1945, since their permanent teeth erupt around the time of the start of the experiment.

[24] I set exposure length for those born in 1952 to 10 years. Setting exposure to 1 year for those born 1962 assumes that prenatal exposure is relevant in this setting, an assumption which has some support in the literature (e.g. Bashash et al., 2017; Farmus et al., 2021).

significant diverging trends being birth year of the family members, birth order, and the share of working mothers. This indicates that parish composition changes over time are unlikely to be causing any observed results. I also test this concern more formally by predicting the outcomes of interest using these background characteristics in Table 2 and show that composition changes in terms of observable characteristics are not driving the results.

5 Results

Effects on cognitive ability, non-cognitive ability, and high school completion

Relating to the previous economics literature (Glied and Neidell, 2010; Aggeborn and Öhman, 2021), I investigate whether water fluoridation affects cognitive ability and non-cognitive ability for men around age 18–19. The regression outcomes can be seen in Table 2, columns 4–9. While the effect on standardized cognitive ability (−0.073 SD, s.e. 0.063) is negative but not statistically significant for the DiD specification, the estimate for standardized non-cognitive ability (−0.162 SD, s.e. 0.066) is stronger in magnitude and statistically significant. This result can be interpreted as exposure to water fluoridation on average reducing non-cognitive ability by 16.2% of a standard deviation.[25]

I also investigate the effects of water fluoridation on high school completion in year 1990, an outcome related to cognitive ability which I can observe for cohorts born before 1951.[26] The results for this outcome can be seen in Figures 2 and 3. The raw data in Figure 2 show a clear indication of high school completion rates decreasing for the treated cohorts born during the fluoridation experiment, which in turn is mirrored in the formal regression analysis pooled by three-year birth cohort groups in Figure 3. High school completion rates for the control area instead keep tracking the national average. These results indicate a decrease in schooling outcomes for the children residing in the fluoridated zone during the experiment, and a subsequent reconvergence in outcomes for those born after the experiment ended.

The regression outcomes when pooling the affected cohorts can be seen in Table 2. In the baseline regression with no added controls, I estimate a statistically significant average decrease of the high school completion rate for the treated cohorts of 4.2 pp. (−0.042, s.e. 0.018). Sequentially adding further controls of family characteristics results in a slightly stronger negative estimate, which remains statistically significant (−0.048, s.e. 0.017).

[25] See Table A3 in Appendix A for results on the 8 subscores of cognitive ability and non-cognitive ability.

[26] Results for additional outcomes based on having a university education, employment, earnings, and log earnings in 1990 and 2010 can be seen in Appendix A.

Linear treatment effects

The fact that the experiment ran for 10 years in total, along with the graphical evidence previously presented, warrant further investigation of linear treatment effects based on years of exposure to water fluoridation. This functional form is more potent in capturing effects that accentuate with additional years of exposure, which would be the case if water fluoridation is harmful beyond the very early years of childhood and infancy.

I investigate this by running a linear regression specification where those linked to the fluoridated zone born after 1962 are treated for zero years, while the children born before then are treated for an additional year by each birth cohort. The reference group remains those living in the control parishes with the same years of exposure. The results can be seen in Table 2.

The effects on standardized cognitive ability (-0.014 SD, s.e. 0.009) and non-cognitive ability (-0.019 SD, s.e. 0.009) are negative and statistically significant for the linear treatment effects specification. Similarly to the DiD specification, the linear treatment effect on high school completion is also statistically significant and negative (-0.006, s.e. 0.003). These estimates should be interpreted as an additional year of exposure to water fluoridation decreasing cognitive ability by 1.4 pp. of a standard deviation, non-cognitive ability by 1.9 pp. of a standard deviation, and the likelihood of graduating from high school by 0.6 pp. In summary, these findings yield qualitatively similar results compared to the DiD specification.

Heterogeneous treatment effects with respect to sex of the child

Given the results in Glied and Neidell, 2010 showing significant differences in water fluoridation treatment effects for girls, I investigate heterogeneous treatment effects with respect to sex of the child in Table 7. I investigate these effects by fully interacting the regression model with an indicator for being female. I focus on high school completion and earnings in 1990 (SEK 100), since the cognitive ability and non-cognitive ability outcomes are limited to men.

I find no significant heterogeneous treatment effects throughout the different empirical specifications for high school completion. However, in line with Glied and Neidell, 2010 I find significant differences in terms of earnings in 1990 for the DiD specification. These results indicate that both sexes are affected by water fluoridation, and could mean that girls are less detrimentally affected in terms of their labor market outcomes.

Robustness tests

Predicted outcomes based on family characteristics

Since the experiment was known, at least to some people, from early on and that it likely became increasingly salient over time, there may have been selective migration away from the treatment and control parishes. This could compromise the validity of the study and lead to biased estimates of the observed effects. I investigate this by predicting all of the main outcomes based on SES and family characteristics from the 1960 census and the Multi-Generation Register.[27] This allows me to observe if any of the estimated effects are potentially driven by changing observable characteristics in the treatment and control parishes over time.

The results of this robustness test can be seen in Table 2. The predicted outcomes are generally not statistically significant and are small in magnitude throughout the different specifications, indicating that the findings are not driven by changing characteristics or selection into the treatment and control group.

Cluster-robust standard errors

The heteroscedasticity-robust standard errors used in this paper assumes that outcomes within parishes are independent over time. In order to account for any intra-correlation for parish outcomes, I present a range of different types of heteroscedasticity-robust and cluster-robust standard errors in Table 4.

These results show that the estimations are robust to using standard errors clustered on the parish-by-cohort level, using wild bootstrapped standard errors on the parish level, and wild bootstrapped standard errors on the parish-by-cohort group level. The results also indicate that the heteroscedasticity-robust standard errors are almost as conservative as the most conservative standard errors tested here.

Results when conditioning on being born in the treatment and control area and remaining in the parish of birth

In Table 5, I test the robustness of the treatment assignment in 1960 by conditioning on the children being born in the treatment and control area and also on remaining in their parish of birth. In essence, this restricts attention to stayers within the treated and control parishes but substantially reduces sample size given the extensive moving patterns around childbirth. The benefit of this approach is that it captures exposure around childbirth well and likely better captures the true years of exposure for the stayers. On the other hand, conditioning on the outcome of staying may induce selection into the estimation if the staying behavior differs between the control and treatment parishes.

[27] The characteristics used in the prediction include cohort of the mother and father, mother and father's number of children, birth order of the child for the mother and father, birth month, if the father and mother has any higher education than elementary in 1960, if the mother and father are employed in 1960, and the mother's marriage status in 1960.

The results of this robustness test show estimates of similar magnitude compared to the main specification when also conditioning on the children being born in the treatment and control area, but with substantially larger standard errors leading to effects mostly not statistically significant. When conditioning on the children remaining in their parish of birth in 1960, the estimates are weaker in magnitude for almost all of the outcomes and specifications, and all estimated effects are not statistically significant. The only exception is the linear treatment effect on cognitive ability, which remains unchanged in magnitude for all versions of treatment assignment. However, all estimates remain negative in sign throughout the different versions, but the larger standard errors when including information on parish of birth make the interpretation of the estimates difficult.

I further investigate the findings from treatment assignment based on stayers in Figure A2. This figure presents raw data on high school completion rates collapsed by stayers' treatment status and birth cohort. The figure shows that there is still a decrease in the high school completion rate for the cohorts born during the water fluoridation, but that the reconvergence in outcomes after the end of the experiment is less clear for this treatment assignment.[28] This finding could thus be an artefact of the treatment assignment based on stayers inducing negative selection into the post-experiment cohorts by excluding families who reside in the treatment area in 1960 and later move into the more affluent control area at the time of childbirth.

In summary, this test indicates that the treatment assignment is somewhat sensitive to adding information on the children's parish of birth, and that this comes at a cost of reducing the sample size and potentially inducing selection into the sample.

Results when including partially treated parishes in the control group

In Table 6, I include the partially treated parishes in the control group to test the implications for the main findings. I do so to verify that the exclusion of these parishes are not fundamental for the identification of the estimated effects. Compared to the main specification, the estimates including the three partially treated parishes are slightly smaller in magnitude. These results are expected since this should attenuate the observed effects by making the comparison group and treatment group more similar.

Socioeconomic index fixed effects

A concern related to textile plant closures happening in Norrköping around the time of the water fluoridation experiment is that this could have differential

[28]Exploratory analysis of the weaker estimates indicates that excluding the children born in the control parishes whose mother resides in the treated parish in 1960 is driving this result. The effects are not symmetric for removing those born in the treated parish who reside (or whose mothers reside) in the control parishes in 1960, which could be explained by the treated parish being smaller and more sensitive to inflow of new inhabitants.

effects on inhabitants living in different parts of the city, and that these effects are captured by my identification strategy. I attempt to dispel this concern by adding fixed effects related to a socioeconomic index from the 1960 census to my main estimates. The 12 categories contained in this index broadly capture the occupational status and class of the parents in 1960, for instance if they are workers in factories or not employed, and should capture if the detrimental effects on human capital outcomes run through parental job loss or economic downturn in the textile sector.[29]

The results of this can be seen in Table 7. Adding these fixed effects causes the magnitude of the estimates to decrease by about -20%, and a bit more for cognitive ability. However, the same qualitative findings are still present and statistically significant for high school completion and non-cognitive ability.

Additional robustness tests

In addition to the main robustness tests presented in the paper, I run tests based on **i)** including family fixed effects in the specifications to account for potential selection issues, at the cost of relying exclusively on within-family variation in fluoride exposure, **ii)** checking for differential migration out of the treatment parish from 1960 and mover characteristics to see if the experiment could have spurred any selective migration, **iii)** using a synthetic control group for the treated parish in Norrköping, **iv)** comparing human capital outcomes of the two parishes in the closely situated city Linköping as a placebo test, and **v)** results using external variation from when Kungsbacka municipality changed their water supply to a freshwater lake with the same fluoride concentration as in the Norrköping experiment. The results of these tests can be seen in Appendix A. Broadly, the aforementioned results indicate that the findings from the Norrköping experiment are robust.

6 Discussion

In contrast with previous evidence presenting null effects related to fluoride exposure during childhood and human capital outcomes (Glied and Neidell, 2010; Aggeborn and Öhman, 2021), this paper presents evidence indicating such a link for children affected by the Norrköping water fluoridation experiment. However, it should be noted that the effect sizes presented in this paper are well below the findings of half a standard deviation decrease in children's IQ presented by Choi et al., 2012, and are estimated for policy-relevant levels of fluoride concentrations in drinking water.[30] The key question that follows is

[29] Although, I acknowledge the limitations that plant closures happening early on before 1960 are not perfectly captured with this control, and that data before 1960 would have been preferred to reduce the risk of capturing behavioral responses happening before 1960.

[30] Unfortunately, I cannot rule out that the smaller effect sizes in this paper are due to problems with attenuation bias.

why the estimated effects differ between this study and previous work finding null effects.

The first explanation is if there are differential effects of exposure to natural fluoride versus artificial water fluoridation, even when the fluoride concentration is similar or identical. The fact that the existing medical literature finds no difference in fluoride absorption through the metabolism between natural and artificial fluoride contradicts this explanation (Whitford et al., 2008).[31]

The second explanation is that the experimental water fluoridation in a confined area is more precise in capturing exposure to fluoride, with less risk of bundled treatment. For instance, differential exposure to natural fluoride in drinking water could shift the entire composition of the water in tandem with the fluoride concentration. This may impact the effects of natural fluoride exposure and compromise the comparison to that of artificial fluoridation. Although, Aggeborn and Öhman, 2021 try to address these concerns and show that natural fluoride concentration in Sweden does not correlate with a wide range of common minerals and compounds. It is, however, hard to completely dispel the concerns of bundled treatment or cocktail effects from exposure to other things than natural fluoride in the water. The evaluation of the Norrköping experiment relies on treatment exposure based on parish of residence, where there is little risk of exposure to other contaminants during the experiment.

A third explanation relates to the time and geographical difference for the cohorts in the different studies.[32] The study by Glied and Neidell, 2010 includes U.S. cohorts born during the 1950s and 1960s. Aggeborn and Öhman, 2021 investigate fluoride exposure for cohorts in Sweden born from 1985 and onward, while the exposed cohorts in this study are born in the same country during the 1950s and 1960s. It could be the case that fluoride exposure is only detrimental in tandem with exposure to other contaminants, and that these are present in this study and in the observational studies investigating fluoride exposure in developing countries, but not in the studies presenting null effects (Glied and Neidell, 2010; Aggeborn and Öhman, 2021). This is difficult to rule out without further empirical evidence. Thus, I leave this for future studies on the topic.

One of the main strengths of this study is being able to account for preexisting differences between treatment and control areas exposed to fluoride, and that the risk of capturing bundled treatment effects related to drinking water fluoride is low. Instead, a key concern is that treatment assignment takes place late during the experiment in 1960, rather than before or early on in the experiment.[33] However, there are no reports of widespread protests

[31] The findings in the medical literature on fluoride absorption relies on very few (10 in total) observations and does not investigate differing health effects from different kinds of fluoride.

[32] I thank Linuz Aggeborn for bringing this to my attention.

[33] As mentioned, this treatment assignment is likely to cause attenuation bias of the estimated effects, especially for the older cohorts born at the onset or early on during the experiment.

or extensive mobility during 1952–1962 as a result of the experiment. The exact location of the fluoridation zone appears to have been unclear to the public, and the medical officer responsible for the experiment claims that the inhabitants of Norrköping accepted the situation without taking any measures. Furthermore, the formal robustness test using predicted outcomes based on background characteristics indicates that this is not a concern.

A further concern relates to the nature of the experiment taking place in a single parish. Since parish outcomes can be correlated over cohorts, the heteroscedasticity-robust standard errors used in the paper may fail to take this into account and not to provide accurate inference. I verify that the p-values of the main estimates on cognitive ability, non-cognitive ability, and high school completion remain largely unchanged by clustering on the parish level using wild bootstrapped standard errors. Related to this, I also run a placebo check using the same empirical specification on Linköping's two parishes (the closest large city in the same region) during the same time period and show that there is no similar decrease in human capital outcomes (see Figure A3 in Appendix A). Furthermore, I use external variation from when Kungsbacka municipality changed their water supply from a lake with low fluoride concentration into a lake with a natural fluoride concentration of 1 mg/L water and show similar decreases in human capital outcomes for the children residing in this area (see Appendix A). This strengthens the causal interpretation of the findings in Norrköping.

In summary, the effect sizes presented in this paper fall somewhere in between the observational studies estimating negative effects and the studies presenting null effects from fluoride exposure. The experimental variation used in this study complement the existing causal work well, and there are several potential reasons why the results of this study differ from that of Glied and Neidell, 2010 and Aggeborn and Öhman, 2021. The conflicting evidence does, however, highlight the need for further empirical evidence of the effects of water fluoridation exposure during childhood on human capital outcomes.

7 Conclusion

This study evaluates the water fluoridation experiment of 1952–1962 in Norrköping to provide causal estimates on the effects of drinking water fluoridation exposure during childhood on a broad range of measures linked to human capital outcomes. While the previous evidence based on this experiment show substantial and positive effects on dental health (Melander, 1957; Sellman and Syrrist, 1968; Linder, 1971; Lundström et al., 1983), I extend the existing analysis and present novel evidence indicating that fluoride exposure of 1–1.2

On the other hand, it ascertains that everyone in the affected cohorts have at least one year of exposure to the water fluoridation by defining treatment status during the experiment.

mg/L drinking water during childhood is associated with detrimental effects on human capital outcomes.

Contrary to previous studies with causal interpretation, which find null effects on ability outcomes linked to water fluoride exposure (Glied and Neidell, 2010; Aggeborn and Öhman, 2021), the findings of this study are statistically significant, negative effects of water fluoridation exposure during childhood on human capital outcomes. Although, the effect sizes on cognitive ability are smaller than those shown in observational work on the topic (Choi et al., 2012). The estimated effects are found for fluoride concentration levels well below the WHO-recommended threshold of 1.5 mg/L water, and the results are robust to a range of specification tests.

However, it should be noted that research on the effects of fluoride exposure is exceedingly difficult due to the empirical challenges previously discussed. More causal evidence is needed before we can put the concerns of detrimental effects of fluoride exposure to rest. To conclude, the findings of this paper warrant caution from policy makers and further empirical studies regarding the safe levels of fluoride concentration in drinking water.

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Figures and tables

Supporting figure

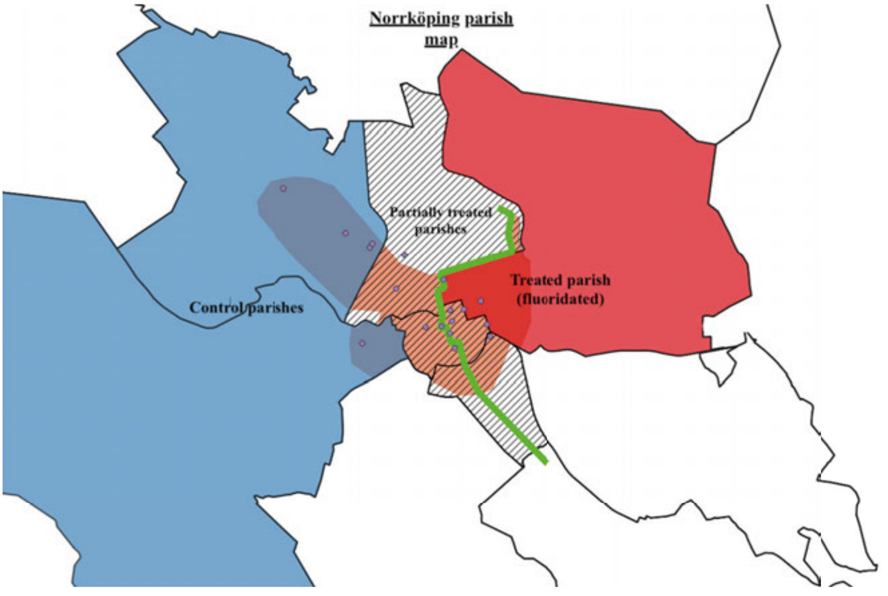
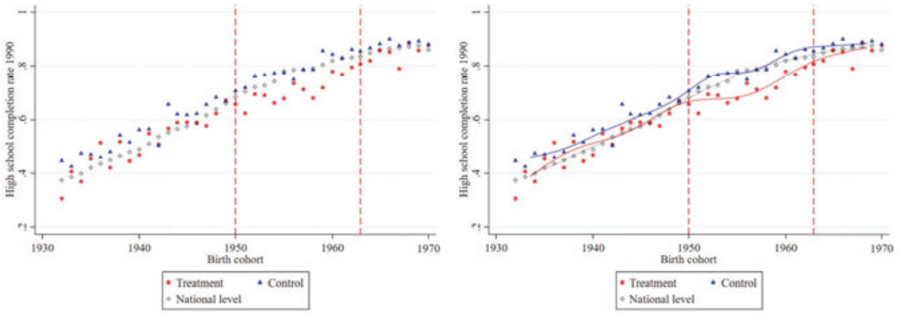


Figure 1. The figure shows Norrköping metropolitan area and the parishes affected by the water fluoridation experiment of 1952–1962. The map is based on an original map of the experiment ca. 1950 from the Norrköping city archive. The underlying shaded darker area approximately represents the Norrköping metropolitan area ca 1950. The green line divides the fluoridated treatment area (low zone) in red from the control area (high zone) in blue. The dashed area denotes partially treated parishes which are excluded from the main analysis. The dots represent schools operating during the time period of interest.

Result figures



(a) High school completion, split by treatment and control group.

(b) High school completion, split by treatment and control group and including local polynomials.

Figure 2. The figures show raw data on high school completion rates in 1990, collapsed by treatment status and birth cohort for those residing in the treated and control parishes in 1960. Figure 2b also includes local polynomials capturing the time trends, and both panels include the cohort high school completion rate on the national level. The dashed red lines mark the approximate cohorts that should have been affected by the water fluoridation.

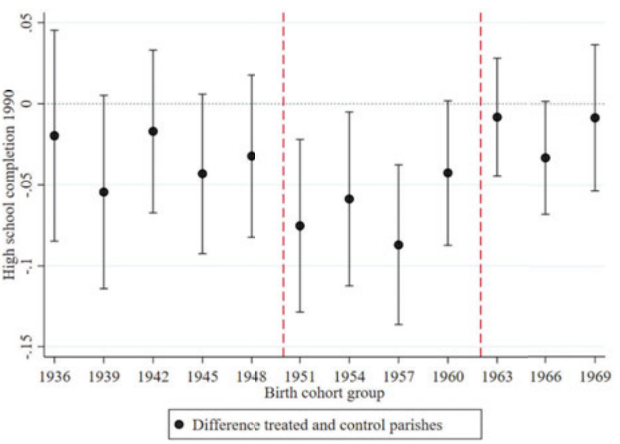


Figure 3. The figure shows the difference in regression outcomes for high school completion in 1990 between the treated and control parishes, by 3-year cohort group. The dashed red lines mark the the approximate cohorts affected by water fluoridation during childhood. Cohorts to the left of the dashed red lines are affected from age 2 or later, while those to the right of the red dashes lines are completely unexposed to water fluoridation. CI95 are shown in black.

Tables

Descriptive statistics

Table 1. *Descriptive statistics for the cohorts born 1951–1970, linked to Norrköping in 1960.*

	Treated parish	Control parishes	Difference	DiD difference	p-val.
Birth year	1961.317	1960.451	.866	.362	[.003]
Birth month	6.181	6.163	.017	-.074	[.602]
Share female	.497	.491	.006	.012	[.565]
Birth year father	1931.676	1930.341	1.335	.701	[.025]
Birth year mother	1934.905	1933.36	1.545	.734	[.008]
Father's nr of children	2.823	2.738	.086	-.015	[.798]
Mother's nr of children	2.779	2.689	.090	-.022	[.689]
Birth order, father's side	1.898	1.898	.000	-.098	[.037]
Birth order, mother's side	1.894	1.890	.005	-.105	[.024]
Share working father 1960	.803	.832	-.029	-.002	[.920]
Share working mother 1960	.380	.340	.039	.056	[.007]
Share higher educ. father 1960	.055	.108	-.053	-.009	[.416]
Share higher educ. mother 1960	.014	.025	-.011	-.002	[.656]
Share mother married 1960*	.957	.973	-.086	-.005	[.761]
Obs.	3,389	6,775	10,164	10,164	

Note: *Share of mothers married in 1960 includes the cohorts born 1951–1960 due to many marriages happening in conjunction with childbirth. "Treated parish" refers to characteristics for inhabitants in the treated (Hedvig) parish. "Control parishes" refers to the same for the control (Östra Eneby and Borg) parishes. "Difference" shows the difference in mean characteristics for the treatment and control parishes. "DiD difference" shows the double difference comparing birth cohort groups 1951–1962 in treated and control parishes against the same treatment definition for those born 1963–1970. The p-values in "p-val." are calculated with heteroscedasticity-robust standard errors for the DiD estimates.

Result tables

Table 2. *DiD and linear treatment effects on high school completion, cognitive ability, and non-cognitive ability.*

Outcome:	High school completion			Cognitive ability			Non-cognitive ability		
$Fluoride \times \mathbb{1}[Cohort \leq 1962]$	-0.042	-0.047	-0.048	-0.099	-0.086	-0.073	-0.175	-0.167	-0.162
	(0.018)	(0.017)	(0.017)	(0.065)	(0.064)	(0.063)	(0.067)	(0.066)	(0.066)
$Fluoride \times Intensity$	-0.004	-0.006	-0.006	-0.014	-0.017	-0.014	-0.018	-0.020	-0.019
	(0.003)	(0.003)	(0.003)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)
Obs.	9,211	9,211	9,211	4,441	4,441	4,441	4,362	4,362	4,362
Outcome:	Pred. high school compl.			Pred. cognitive ability			Pred. non-cognitive ability		
$Fluoride \times \mathbb{1}[Cohort \leq 1962]$	0.001	-0.000	-0.000	-0.019	-0.015	-0.000	-0.009	-0.005	0.000
	(0.004)	(0.003)	(0.000)	(0.016)	(0.012)	(0.000)	(0.010)	(0.007)	(0.000)
$Fluoride \times Intensity$	0.000	-0.000	-0.000	-0.001	-0.003	-0.000	-0.000	-0.001	0.000
	(0.001)	(0.000)	(0.000)	(0.002)	(0.002)	(0.000)	(0.001)	(0.001)	(0.000)
Obs.	8,804	8,804	8,804	4,376	4,376	4,376	4,297	4,297	4,297
Mean dep. var.	0.796	0.796	0.796						
Family characteristics		✓	✓		✓	✓		✓	✓
Parental char. controls			✓			✓			✓

Heteroscedasticity-robust standard errors are shown in parenthesis. “High school completion” refers to high school completion in 1990. “Cognitive ability” refers to standardized cognitive ability for men around age 18–19. “Non-cognitive ability” refers to standardized non-cognitive ability for men at the same age. “$Fluoride \times \mathbb{1}[Cohort \leq 1962]$” denotes the DiD interaction of residing in the fluoridated parish and being born during the fluoridation, and “$Fluoride \times Intensity$” is a linear years-of-exposure treatment measure capturing every additional years of exposure for those residing in the fluoridated parish, starting with the cohort born 1962 and ending with the one born in 1952. “Pred. high school compl.”, “Pred. cognitive ability”, and “Pred. non-cognitive ability” refer to specifications estimated on predicted outcomes of high school completion and ability outcomes. The predictions are based on family characteristics and information from the 1960 census, which together form predictions with R^2s of 0.031–0.111. The realized difference in high school completion rate between the 75th and 25th percentile of the predicted high school completion rate is almost 29 pp.

Table 3. *Heterogeneous treatment effects: High school completion and earnings in year 1990, by sex of the child.*

Outcome:	High school completion			Earnings in 1990		
$Fluor. \times \mathbb{1}[Cohort \leq 1962] \times Female$	-0.006	-0.013	-0.013	131.722	130.549	130.608
	(0.035)	(0.034)	(0.034)	(51.960)	(52.833)	(52.786)
$Fluor. \times \mathbb{1}[Cohort \leq 1962]$	-0.039	-0.043	-0.044	-127.003	-126.993	-129.211
	(0.025)	(0.024)	(0.024)	(42.268)	(43.065)	(43.086)
Obs.	9,359	9,211	9,211	9,808	9,645	9,645
$Fluoride \times Intensity \times Female$	0.000	0.000	-0.000	10.035	9.604	8.920
	(0.005)	(0.005)	(0.005)	(8.000)	(8.120)	(8.094)
$Fluoride \times Intensity$	-0.005	-0.006	-0.006	-12.455	-12.361	-12.231
	(0.004)	(0.004)	(0.004)	(6.626)	(6.711)	(6.686)
Obs.	8,934	8,804	8,804	9,364	9,219	9,219
Mean dep. var.	0.793	0.793	0.793	1,110	1,110	1,110
Family characteristics		✓	✓		✓	✓
Parental char. controls			✓			✓

Heteroscedasticity-robust standard errors are shown in parenthesis. "High school completion" refers to high school completion in 1990. "Earnings in 1990" refers to labor earnings (SEK 100) in levels during that year. "$Fluor. \times \mathbb{1}[Cohort \leq 1962]$" denotes the DiD interaction of residing in the fluoridated parish and being born during the fluoridation, and "$Fluoride \times Intensity$" a linear years-of-exposure treatment measure capturing additional years of exposure for those residing in the fluoridated parish, starting with the cohort born 1962 and ending with the one born in 1952.

Robustness tables

Table 4. *Different p-values for the outcomes high school completion, cognitive ability, and non-cognitive ability (clustering on the parish-by-cohort level, wild bootstrapping on the parish level) in Norrköping.*

Specification: Outcome:	DiD High school compl.	Linear	DiD Cognitive ability	Linear	DiD Non-cog. ability	Linear
Estimate	-0.048	-0.006	-0.073	-0.014	-0.161	-0.019
p-val. het. robust	[0.005]	[0.018]	[0.246]	[0.094]	[0.013]	[0.040]
p-val. cl. parish × cohort	[0.000]	[0.000]	[0.175]	[0.033]	[0.005]	[0.041]
p-val. wild bootstrap parish	{0.007}	{0.026}	{0.224}	{0.089}	{0.009}	{0.054}
p-val. wild bootstr. par. × cohort gr.	{0.005}	{0.020}	{0.232}	{0.085}	{0.017}	{0.040}
Mean dep. var.	0.789	0.789				
Obs.	9,211	8,804	4,442	4,376	4,363	4,297
Family characteristics	✓	✓	✓	✓	✓	✓
Parental char. controls	✓	✓	✓	✓	✓	✓

p-values for robust standard errors and when clustering on parish-by-cohort level are shown in brackets. p-values for standard errors based on wild bootstrapping on the parish level, and the parish-by-cohort group level (using three groups: Cohorts 1951–1957, 1958–1964, and 1965–1970) are shown in curly brackets. "High school compl." refers to high school completion in 1990. "Cognitive ability" refers to cognitive ability around age 18. "Non-cog. ability" refers to non-cognitive ability around the same age.

Table 5. *Results for the outcomes high school completion, cognitive ability, and non-cognitive ability in Norrköping when conditioning on still residing in the parish of birth in 1960.*

Outcome:	High school completion			Cognitive ability			Non-cognitive ability		
$Fluoride \times \mathbb{1}[Cohort \leq 1962]$	-0.048 (0.017)	-0.049 (0.023)	-0.027 (0.028)	-0.073 (0.063)	-0.015 (0.088)	-0.019 (0.104)	-0.162 (0.066)	-0.133 (0.093)	-0.046 (0.107)
Obs.	9,211	5,175	4,499	4,441	2,415	2,090	4,362	2,374	2,060
$Fluoride \times Intensity$	-0.006 (0.003)	-0.005 (0.004)	-0.001 (0.004)	-0.014 (0.009)	-0.013 (0.012)	-0.015 (0.014)	-0.019 (0.009)	-0.013 (0.013)	-0.004 (0.014)
Obs.	8,804	4,962	4,314	4,376	2,389	2,068	4,297	2,348	2,038
Mean dep. var.	0.796	0.796	0.800						
PoB in treatment and ctrl. area		✓	✓		✓	✓		✓	✓
1960 census & PoB same			✓			✓			✓
Family characteristics	✓	✓	✓	✓	✓	✓	✓	✓	✓
Parental char. controls	✓	✓	✓	✓	✓	✓	✓	✓	✓

Heteroscedasticity-robust standard errors are shown in parenthesis. "$Fluoride \times \mathbb{1}[Cohort \leq 1962]$" denotes the DiD interaction of residing in the fluoridated parish and being born during the fluoridation, and "$Fluoride \times Intensity$" a linear years-of-exposure treatment measure capturing additional years of exposure for those residing in the fluoridated parish, starting with the cohort born 1962 and ending with the one born in 1952. "High school completion" refers to high school completion in 1990. "Cognitive ability" refers to standardized cognitive ability around age 18. "Non-cognitive ability" refers to standardized non-cognitive ability around the same age. "PoB" refers to the parish of birth.

Table 6. *Results for the outcomes high school completion, cognitive ability, and non-cognitive ability in Norrköping when including partially treated parishes in the control group.*

Outcome:	High school completion	Cognitive ability	Non-cog. ability	High school completion	Cognitive ability	Non-cog. ability
$Fluoride \times \mathbb{1}[Cohort \leq 1962]$	-0.042 (0.015)	-0.051 (0.055)	-0.146 (0.057)			
$Fluoride \times Intensity$				-0.005 (0.002)	-0.012 (0.008)	-0.018 (0.008)
Mean dep. var.	0.789			0.789		
Obs.	22,201	10,759	10,570	21,175	10,583	10,394
Family characteristics	✓	✓	✓	✓	✓	✓
Parental char. controls	✓	✓	✓	✓	✓	✓

Heteroscedasticity-robust standard errors are shown in parenthesis. "High school completion" refers to high school completion in 1990. "Cognitive ability" refers to standardized cognitive ability around age 18. "Non-cog. ability" refers to standardized non-cognitive ability around the same age. The partially treated parishes include S:t Olai, S:t Johannes, and Matteus parish. "Treatment intensity" denotes a linear treatment measure capturing additional years of exposure, starting with the cohort born 1962 and ending with the one born in 1952.

Table 7. *Controlling for socioeconomic status in 1960.*

Outcome:	High school compl.		Cognitive ability		Non-cog. ability	
$Fluoride \times \mathbb{1}[Cohort \leq 1962]$	-0.038	-0.038	-0.028	-0.026	-0.134	-0.133
	(0.017)	(0.017)	(0.061)	(0.061)	(0.065)	(0.065)
Obs.	9,211	9,211	4,441	4,441	4,362	4,362
$Fluoride \times Intensity$	-0.005	-0.005	-0.009	-0.009	-0.015	-0.015
	(0.003)	(0.003)	(0.008)	(0.008)	(0.009)	(0.009)
Obs.	8,804	8,804	4,376	4,376	4,297	4,297
Mean dep. var.	0.796	0.796				
Controls	✓	✓	✓	✓	✓	✓
Mother SES	✓	✓	✓	✓	✓	✓
Father SES		✓		✓		✓

Heteroscedasticity-robust standard errors are shown in parenthesis. "$Fluoride \times \mathbb{1}[Cohort \leq 1962]$" denotes the DiD interaction of residing in the fluoridated parish and being born during the fluoridation, and "$Fluoride \times Intensity$" a linear years-of-exposure treatment measure capturing additional years of exposure for those residing in the fluoridated parish, starting with the cohort born 1962 and ending with the one born in 1952. Mother and father SES capture indicators for the parents' socioeconomic status in 1960. This consists of 12 occupational groups into which the head of the household is classified. "High school compl." refers to high school completion in 1990. "Cognitive ability" refers to standardized cognitive ability around age 18. "Non-cog. ability" refers to standardized non-cognitive ability around the same age.

Appendix A Additional information and results

Additional background information

Water fluoridation policies across the globe

Water fluoridation is a public policy widely used across the world. More than 380 million people globally are exposed to artificial water fluoridation (Aoun et al., 2018). For instance, countries such as the United States, the UK, Brazil, and Australia practice partial or extensive fluoridation of their public water supply. In addition, 57 million more people have access to naturally fluoridated drinking water. Sweden does not fluoridate drinking water at this date and recommends people to abstain from drinking water with a fluoride concentration above 1.5 mg/l water. However, due to variations in the drinking water source used there exists natural variation in the fluoride concentration of public drinking water up to and including the range of fluoride added artificially in many countries (Aggeborn and Öhman, 2021).

The recommended levels of water fluoridation have been subject to revision over time.[34] Recently, the Public Health Service (PHS) in the U.S. changed their guidelines for fluoridation to the previous lower bound recommendation of 0.7 mg/L water (prev. recommended range 0.7–1.2 mg/L) in response to observational evidence of dental fluorosis linked to the fluoridation levels previously believed to be safe (PHS, 2015).[35]

In some cases the public debate about water fluoridation has been livid. The debate even sometimes has an impact on public policy; as late as October 2021, the mayor of Anchorage in Alaska decided to turn off the city's water fluoridation system for a few hours, reportedly after hearing from employees that the fluoride was causing negative physical symptoms. The fluoridation was resumed a few hours later after it was discovered to be required by the city code.

The Norrköping water fluoridation experiment

Norrköping in 1950, just before the water fluoridation experiment took place, was the fourth largest city in Sweden with 85,000 inhabitants. The dominant industry at the time was textile production, with 54 active factories in the city employing roughly 6,600 people. Starting in the early 1950s, increasing competition from foreign textile makers led the textile industry and the city to

[34] This recommendation is based on findings in observational studies of fluoride concentration and dental fluorosis, but the WHO also acknowledge that this threshold for detrimental effects could potentially be lower (WHO, 2019).

[35] Depending on the severity of the condition, the symptoms of dental fluorosis range from discoloring of the teeth to weakened enamel and subsequent tooth decay. The PHS clearly states that they support public fluoridation of the water supply. They argue that the lower level of fluoridation will lead to very similar caries reductions while limiting the risks of potential detrimental effects of fluoridation, in this case dental fluorosis. The PHS also state that they find the case for detrimental effects linked to children's IQ to be unlikely.

slide into a recession, which culminated in 1970 when the final textile producer closed down and left city-wide textile employment at 300 individuals in total. However, active government policies and investments into new business ameliorated parts of the downturn.

In addition to the economic downturn affecting Norrköping around the time of the water fluoridation,[36] the time period was also characterised by extensive demolition and reconstruction of the old city center. Roughly 1930 old houses (700 residential, 30 industrial, and 1,200 other) were demolished and mostly replaced by modern construction during 1947–1970. The demolishing and rebuilding of the city center happened in many other Swedish cities at the time.

Simultaneously, the public dental system had been established and was struggling to meet the vast demand for dental health services. Caries and other dental problems were widespread, and few dentists were available for hire. Reportedly, 99.9% of all Swedish male recruits in the military draft of 1940 were found to have problems with dental cavities. It was against the backdrop of this institutional setting that the Norrköping water fluoridation experiment was undertaken. The solution to the problem from the local health experts' viewpoint was to make use of the recent finding that fluoride added to drinking water could be used to improve public dental health. The discussion of water fluoridation by the municipality had begun already in 1947–1948, but technical and administrative challenges delayed the project until 1952.

The experimental setup relies on the pre-existing water system, which divides the city's water supply into two separate zones (see Figure 1). Given that the water zone line runs through the city in an almost arbitrary fashion, the two study populations were deemed to be fundamentally similar with respect to the social, geographic, and climatic factors before the introduction of water fluoridation (Melander, 1957). In order to achieve the different water pressures in the systems, the high zone water reservoir was located 25 metres above sea level higher than the low zone reservoir. Given that the water was identical except for different water pressure and the added fluoride, the experiment should provide causal evidence of the effects on dental health of higher quality than had previously been available.

Importantly, the health experts responsible for the experiment tested for balancing between the control group and treatment group. Checkups of the dental health of children at age 8–10 before the start of the experiment showed that dental health was roughly balanced at the onset of the experiment (see Figure A17). After 3–4 years of drinking water fluoridation, the dental health of the treated group had improved substantially compared to the outcome of

[36]Over SEK 100 million (roughly $10,700,000), a very large sum at the time, was invested by the state and local municipality to attract new business to the city. 5 large government agencies were relocated to Norrköping to provide additional employment opportunities. The opening of the Lindö canal in 1962, which allowed ocean-going crafts to enter the city harbor, gave rise to increasing optimism about future prospects.

the control group. This was seen as strong evidence that the experimental setup was valid and able to capture causal effects related to water fluoridation.

Additional empirical results

University and labor market outcomes

I investigate additional outcomes related to the effects of water fluoridation on children's long-term educational and labor market performance in 1990 and 2010. See Table A2 for the effects on university education, employment rate, earnings, and log earnings. The table shows statistically significant negative effects on having a university education in 1990, which are similar in magnitude compared to the effect on high school completion. This holds both for the DiD specification (−0.048, s.e. 0.017) and linear treatment effects specification (−0.005, s.e. 0.003). The effects for the same outcome in 2010 are similar but weaker in magnitude. For the employment outcome in 1990, I see no significant effects for the DiD specification (−0.010, s.e. 0.014) or the linear specification (−0.002, s.e. 0.002). Earnings (SEK 100) and log earnings, on the other hand, are starkly affected in 1990. On average, the statistically significant earnings decrease (−63.533, s.e. 26.454) amounts to 9.4 pp. (−0.094, s.e. 0.034) with the log specification, which translates into a 0.9 pp. decrease per year of exposure with the linear specification (−0.009, s.e. 0.005). In 2010, the effects on employment are stronger in magnitude and statistically significant for both specifications. The effects on earnings and log earnings, however, are no longer statistically significant.

Subscores of cognitive and non-cognitive ability

In Table A3, I present results for the standardized eight subscores of cognitive ability and non-cognitive ability. All in all, the effects on the subscores are smaller in magnitude than for the composite scores. The only statistically significant effect for the DiD specification is on verbal cognitive ability (−0.116 SD, s.e. 0.064). For the linear specification on cognitive ability subscores, I find statistically significant negative effects on logical thinking (−0.018 SD, s.e. 0.009), verbal ability (−0.019 SD, s.e. 0.009), and technical comprehension (−0.017 SD, s.e. 0.009). For the same specification on non-cognitive ability, there are also statistically significant negative effects on social maturity (−0.016 SD, s.e. 0.010) and intensity (−0.017 SD, s.e. 0.009).

Robustness tests

Differential moving patterns from the treated parish 1960–1970 & mover characteristics

A key concern with the identification strategy is differential selection out of the treated and control parishes over time. I look into this by investigating differential moving patterns away from the treated and control parishes late during the experiment between the censuses of 1960–1965 and 1960–1970 in Table A4. I find no statistically significant evidence of greater outflow of inhabitants from the treated parish between 1960–1965 for both the DiD (0.030, s.e. 0.030) and linear specification (0.005, s.e. 0.003). For the 10-year horizon, however, I find a 7.9 pp. (0.079, s.e. 0.018) increase in the likelihood of moving away from the treated compared to the control parish.

In Table A5, I characterize the movers from the parishes of interest by comparing the individuals who move away from the treated and control parishes 1960–1965 and 1960–1970. In general, the children who move from the treated parish during the water fluoridation experiment tend to be positively selected, albeit not statistically significant, in terms of having a working father or a father with any higher education than elementary schooling. While these result do not allow me to rule out differential mobility out of the parishes as a confounder, the magnitudes of the estimates are generally moderate to small.

Family fixed effects

An effective way to control for confounders related to family characteristics is to include family (mother) fixed effects in the regression analysis. This restricts the comparison to siblings with differential exposure to the fluoridation on the basis of birth year. The cost is instead incurred by a substantial decrease in the precision of the estimates. The results when including family fixed effects can be seen in Table A6.

The findings indicate that the effects on high school completion either do not survive this or decrease substantially by around 70% depending on the specification used.[37] The standard errors also increase by more than 100%, leading to a negative estimate not statistically significant with a wide confidence interval (-0.002, s.e. 0.007) for the linear treatment specification compared to the baseline specification, and a positive estimate not statistically significant (0.017, s.e. 0.038) for the DiD specification.[38] For the linear specifications on cognitive ability (-0.014 SD, s.e. 0.028) and non-cognitive ability (-0.025 SD, s.e. 0.037), the estimates are relatively stable compared to the baseline specifications while the standard errors increase by roughly

[37] Potentially, this could be due to differential parental investments in the children to compensate for the difference in ability.

[38] A visual inspection of the latter estimate in Figure A4 shows a drop in the outcome for the treated cohorts born around 1952, just as with the main specification. However, this drop is not large enough to cancel out the average effect for the cohorts born after the fluoridation, which is stable but lower than with the main specification.

200%, leaving the estimates not statistically significant. The same pattern can be seen with the DiD specifications for cognitive ability (-0.065 SD, s.e. 0.180) and non-cognitive ability (-0.124 SD, s.e. 0.220). However, the fact that all estimates on ability measures remain negative, albeit not significant, of roughly similar magnitude to the main specification is a result that strengthens the causal interpretation of these findings.

Linköping placebo test

In order to rule out similar effects on human capital outcomes happening in the nearest large city, I run a placebo check where I present raw data and time trends on high school completion for Linköping's two parishes. This approach effectively splits the city in two and attempts to mimic the comparison I performed with the Norrköping parishes. Linköping is located in the same region approximately 40 kilometers southwest of Norrköping, and had roughly 55,000 inhabitants in 1950 compared to Norrköping's 85,000. The results of this can be seen in Figure A3. Broadly, the figures do not show any indication of a substantial drop in the high school completion rate for children residing in Linköping's eastern or western parish during the same time period as the water fluoridation experiment in Norrköping.

Additional validation: Synthetic control approach

The synthetic control approach allows me to construct alternative control groups for the experiments at hand, providing additional indications of the validity to the findings. See Figures A9–A13 in Appendix A for graphical results related to the synthetic control approach. Following Abadie et al., 2010, the synthetic control algorithm constructs a control group in order to best match the pre-outcomes and baseline covariates. For Norrköping, the algorithm selects among 128 other parishes in the Östergötland region where Norrköping is located.[39] Interestingly, when excluding the partially treated parishes from the algorithm selection, roughly 41% of the weight (the highest of all synthetic control parishes) is placed on Östra Eneby parish. This parish is one of the two parishes used as control group in the baseline specifications. In the main specification, Östra Eneby accounts for roughly 80% of the control parishes based on its population size.

Despite the difficulties in replicating the more volatile outcomes in small geographical areas such as Norrköping, the results show similar patterns of decreasing schooling results and ability measures in the treated areas following the fluoridation.[40] These results indicate that the main findings are somewhat robust to using alternative synthetic control groups.

[39] For Kungsbacka, the algorithm selects among 253 other municipalities in Sweden. Some parishes and municipalities are excluded from the selection process due to mergers or other events that led to missing yearly observations causing panel imbalance.

[40] Although, the schooling outcome also appears to decrease 10 years later in the synthetic control group for Kungsbacka.

Additional validation: Kungsbacka municipality and parish drinking water fluoride increase

In 1968–1969, Kungsbacka municipality on the west coast of Sweden changed their water supply from a source with almost no fluoride concentration (0.1 mg/L) to lake Lygnern, with a natural fluoride concentration of 1.0–1.2 mg/L water. This change in water source can thus be used to test whether similar effects as the ones in Norrköping are observed, and also by extension if a change in the natural fluoride concentration in drinking water can affect children's development in the same way as artificial fluoridation. The caveat with this evaluation is that, unlike with Norrköping, I lack a natural control group within the city or municipality. As a control group for Kungsbacka, I will primarily use Varberg, a west coast municipality situated 50 km from Kungsbacka, which continuously had a fluoride concentration in their water supply of 0.1 mg/L.

I use Varberg as a comparison since this municipality has already been used in terms of evaluating the effects on dental health from exposure to fluoride in Kungsbacka following the 1968-1969 change in fluoride concentration (see Figure A5), which appears to have led to a substantial decrease in caries prevalence in Kungsbacka compared to Varberg (SOU 1981:32, 1981).[41] I also present evidence based on a synthetic control approach with Kungsbacka compared to a composite control group consisting of algorithmic-selected control municipalities.

Given that the water supply change in Kungsbacka is permanent and still present to this day, the evaluation will consist of comparing children residing in the two municipalities during childhood when the fluoride exposure should have the greatest effects. Thus, I use a DiD-specification where the children residing in Kungsbacka are treated and Varberg children are used as control group.[42] The cohorts born 1960–1966 are used to net out pre-existing differences in outcomes between the two municipalities, while the cohorts born 1967–1985 are used to estimate the effects of exposure to fluoride throughout childhood including the early years. The coefficient of interest (ϕ_1) is based on the interaction between an indicator for residing in Kungsbacka and an indicator for being born 1967–1985. In the first specification, I use all children while in an alternate version I exclude partially treated children born 1966–1968 and focus on children in the treatment group who were exposed prenatally and

[41] This figure is highly reminiscent of a DiD approach, and gives a clear indication of the suitability of using Varberg as a comparison to Kungsbacka.

[42] I assign children to their location of residence based on the first census they appear in. For instance, a child born in 1965 is assigned to the municipality or parish of residence in the 1965 census, while a child born in 1966 is assigned to its residence in the 1970 census.

from birth. I also present a specification where I use parish outcomes instead of municipality to see how this affects the estimates.[43]

Almost identical to the specification used for Norrköping, the regression specification used to evaluate the effects in Kungsbacka for an individual i of cohort j residing in parish or municipality p is based on the DiD regression presented below:

$$y_{i,j,p} = \psi_0 + \psi_1 Fluoride_p \times \mathbb{1}[Cohort_j \geq 1967] + \psi_2 Fluoride_p + \sum_{j=1961}^{1985} D_j + \boldsymbol{X_i'\delta} + \varepsilon_{i,j,p}$$

where, again, the coefficient ϕ_1 on the interaction term captures the treatment effects for those residing in the fluoridated Kungsbacka compared to those living in Varberg, while netting out the main effects (ϕ_2) for the placebo cohorts residing in the same parishes well before the fluoride exposure had started. Again, the specification includes cohort indicators and controls based on background characteristics from the 1960 census and the Multi-Generation Register.

The results of the evaluation for the main outcomes of interest (cognitive, non-cognitive ability, and high school completion) can be seen in Tables A7 and A8. While the results for high school completion when excluding controls for family characteristics are negative and not significant at both the municipal (−0.018, s.e. 0.014) and parish level (−0.022, s.e. 0.015), the estimates strengthen somewhat when including control variables in the municipality (−0.025, s.e. 0.014) and parish level (−0.023, s.e. 0.015) and when excluding partially treated cohorts that risk attenuating the effects for the municipality (−0.023, s.e. 0.015) and parish level (−0.025, s.e. 0.015). The largest and consistently statistically significant effects are found when both excluding partially treated cohorts and controlling for family characteristics in the regressions for the municipality (−0.030, s.e. 0.014) and the parish level (−0.026, s.e. 0.016).

The same pattern can be seen when evaluating the effects on standardized cognitive and non-cognitive ability for men around age 18, where the strongest effects are observed when excluding partially treated cohorts and controlling for family characteristics. The effects on cognitive ability are negative but not statistically significant in the municipality regressions even when excluding partially treated cohorts (−0.115 SD, s.e. 0.074), but are statistically significant in the same parish regressions (−0.185 SD, s.e. 0.083). For non-cognitive ability, the effects are the strongest in magnitude when excluding partially treated cohorts and including family characteristics as controls in the municipal (−0.165 SD, s.e. 0.071) and parish (−0.173 SD, s.e. 0.081) regressions.

[43]For instance, parish outcomes increases the likelihood of the children being exposed to the municipal water supply and not having access to a privately dug well (which is more common in rural areas).

All in all, the findings indicate that water fluoride exposure decreases cognitive and non-cognitive ability by 0.115–0.173 standard deviations, which is broadly comparable to the findings in Norrköping.

Figures and tables

Tables

Descriptive statistics

Table A1. *Descriptive statistics displaying the number of observations for the Norrköping sample born 1951–1970, by each main outcome and treatment status.*

	Birth year																			
	1951	1952	1953	1954	1955	1956	1957	1958	1959	1960	1961	1962	1963	1964	1965	1966	1967	1968	1969	1970
HS compl.																				
# obs.	407	489	435	413	428	456	446	448	422	344	446	480	484	584	457	565	542	482	456	427
# tr. obs.	113	144	127	110	113	126	129	127	140	113	145	186	171	214	173	214	194	179	162	162
# contr. obs.	294	345	308	303	315	330	317	321	282	231	301	294	313	370	284	351	348	303	294	265
Cog. ability																				
# obs.	65	237	232	229	214	227	240	229	227	38	223	262	247	296	244	284	264	240	220	223
# tr. obs.	16	68	62	63	54	63	73	67	71	9	74	109	84	102	91	114	96	90	81	86
# contr. obs.	49	169	170	166	160	164	167	162	156	29	149	153	163	194	153	170	168	150	139	137
Non-c. ability																				
# obs.	65	237	232	229	214	227	240	229	227	38	223	262	246	296	243	252	255	234	210	203
# tr. obs.	16	68	62	63	54	63	73	67	71	9	74	109	83	102	90	100	92	89	77	78
# contr. obs.	49	169	170	166	160	164	167	162	156	29	149	153	163	194	153	152	163	145	133	125

Note: The table displays information on the number of observations per cohort for the main outcomes, by treatment status. “HS compl.” refers to high school completion in 1990. “Cog. ability” refers to standardized cognitive ability for men around age 18–19. “Non-c. ability” refers to standardized non-cognitive ability for men at the same age. “tr. obs.” refers to treated observations and “contr. obs.” refers to control observations. The low number of observations on cognitive ability and non-cognitive ability for the cohort born 1960 is due to missing data for that specific cohort.

Result tables

Table A2. *Additional outcomes: University education and labor market outcomes in year 1990 and 2010.*

Year:	1990				2010			
Outcome:	University education	Employed	Earnings	Log earnings	University education	Employed	Earnings	Log earnings
$Fluor. \times \mathbb{1}[Cohort \leq 1962]$	-0.048	-0.010	-63.533	-0.094	-0.041	-0.030	-140.652	-0.015
	(0.017)	(0.014)	(26.454)	(0.034)	(0.020)	(0.015)	(108.310)	(0.040)
Obs.	9,211	9,645	9,645	9,352	9,233	9,242	9,242	7,912
$Fluoride \times Intensity$	-0.005	-0.002	-7.855	-0.009	-0.004	-0.004	-14.142	-0.003
	(0.003)	(0.002)	(4.046)	(0.005)	(0.003)	(0.002)	(16.551)	(0.006)
Obs.	8,804	9,219	9,219	8,954	8,833	8,842	8,842	7,604
Mean dep. var.	0.301	0.909	1,439.3	7.153	0.356	0.839	2,915.4	7.932
Family characteristics	✓	✓	✓	✓	✓	✓	✓	✓
Parental char. controls	✓	✓	✓	✓	✓	✓	✓	✓

Heteroscedasticity-robust standard errors are shown in parenthesis. "University education" refers to having some university education in 1990 and 2010 respectively. "Employed" refers to being employed during those years. "Earnings" refers to the earnings in levels (SEK 100) during those years. "Log earnings" refers to the natural logarithm of earnings during those years. "$Fluor. \times \mathbb{1}[Cohort \leq 1962]$" denotes the DiD interaction of residing in the fluoridated parish and being born during the fluoridation, and "$Fluoride \times Intensity$" a linear years-of-exposure treatment measure capturing additional years of exposure for those residing in the fluoridated parish, starting with the cohort born 1962 and ending with the one born in 1952.

Table A3. *Composite and subscores of cognitive and non-cognitive ability age 18.*

Outcome:	Composite measures CA	NCA	Subscore CA Logic	Verbal	Spatial	Tech.	Subscore NCA Maturity	Intensity	Energy	Stability
$Fluoride \times \mathbb{1}[Cohort \leq 1962]$	-0.073	-0.161	-0.098	-0.116	0.018	-0.063	-0.089	-0.106	-0.075	-0.071
	(0.063)	(0.066)	(0.064)	(0.064)	(0.065)	(0.064)	(0.074)	(0.070)	(0.072)	(0.071)
Obs.	4,442	4,363	4,303	4,277	4,277	4,267	3,912	3,912	3,914	3,913
$Fluoride \times Intensity$	-0.014	-0.019	-0.018	-0.019	-0.002	-0.017	-0.016	-0.017	-0.011	-0.004
	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.010)	(0.009)	(0.010)	(0.009)
Obs.	4,376	4,297	4,238	4,212	4,212	4,202	3,847	3,847	3,849	3,848
Family characteristics	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Parental char. controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓

Heteroscedasticity-robust standard errors are shown in parenthesis. "$Fluoride \times \mathbb{1}[Cohort \leq 1962]$" denotes the DiD interaction of residing in the fluoridated parish and being born during the fluoridation, and "$Fluoride \times Intensity$" a linear years-of-exposure treatment measure capturing additional years of exposure for those residing in the fluoridated parish, starting with the cohort born 1962 and ending with the one born in 1952. "CA" refers to cognitive ability around age 18. "NCA" refers to non-cognitive ability around the same age. "Logic" refers to logical thinking, "Verbal" to verbal ability, "Spatial" to 3D spatial thinking, and "Technical" to a technical understanding test. "Maturity" refers to social maturity, "Ps. energy" to psychological energy, "Stability" to emotional stability. All outcomes are standardized by cohort and measured at approximately age 18.

Robustness tables

Table A4. *Moving away from the treated and control parish between 1960-1965 and 1960-1970.*

Outcome:	Moving 1960-65	Mov. 1960-70	Mov. 1960-65	Mov. 1960-70
$Fluoride \times \mathbb{1}[Cohort \leq 1962]$	0.030	0.079		
	(0.030)	(0.018)		
$Fluoride \times Intensity$			0.005	0.011
			(0.003)	(0.003)
Mean dep. var.	0.359	0.495	0.359	0.495
Obs.	6,737	9,418	6,310	8,987
Family characteristics	✓	✓	✓	✓
Parental char.	✓	✓	✓	✓

Heteroscedasticity-robust standard errors are shown in parenthesis. "Moving 1960–65" includes cohorts born until 1964, and "Mov. 1960–70" includes those born until 1969. "$Fluoride \times \mathbb{1}[Cohort \leq 1962]$" denotes the DiD interaction of residing in the fluoridated parish and being born during the fluoridation, and "$Fluoride \times Intensity$" a linear years-of-exposure treatment measure capturing additional years of exposure for those residing in the fluoridated parish, starting with the cohort born 1962 and ending with the one born in 1952.

Table A5. *Differential father characteristics of movers away from the treatment and control parishes.*

Sample:	Moving 1960–65				Mov. 1960–70			
Outcome:	B. year	B. order child	Working	High ed.	B. year	B. order child	Working	High ed.
$Fl. \times \mathbb{1}[Cohort \leq 1962]$	0.100	-0.088	0.036	0.026	0.266	-0.101	-0.010	-0.004
	(0.528)	(0.093)	(0.034)	(0.038)	(0.330)	(0.057)	(0.020)	(0.025)
Mean dep. var.	1,925.5	1.913	0.960	0.188	1,925.5	1.913	0.960	0.188
Obs.	6,755	6,755	6,600	6,600	9,436	9,436	9,106	9,106

Heteroscedasticity-robust standard errors are shown in parenthesis. "$Fl. \times \mathbb{1}[Cohort \leq 1962]$" denotes the DiD interaction of residing in the fluoridated parish and being born during the fluoridation for those who move during the specified time periods. "Moving 1960–1965" includes cohorts born until 1964, and "Mov. 1960–70" includes those born until 1969. "B. year" denotes birth year of the father. "B. order child" denotes the birth order of the child on the father's side. "Working" captures if the father is employed in 1960. "High ed." captures if the father has any higher education than elementary education in 1960.

Table A6. *Family fixed effects specification.*

Outcome:	High school compl.		Cognitive ability		Non-cog. ability	
$Fluoride \times \mathbb{1}[Cohort \leq 1962]$	-0.048	0.017	-0.073	-0.065	-0.162	-0.124
	(0.017)	(0.038)	(0.063)	(0.180)	(0.066)	(0.220)
Obs.	9,211	9,332	4,441	4,511	4,362	4,431
# of families		5,706		3,490		3,440
$Fluoride \times Intensity$	-0.006	-0.002	-0.014	-0.014	-0.019	-0.025
	(0.003)	(0.007)	(0.009)	(0.028)	(0.009)	(0.037)
Obs.	8,804	8,908	4,376	4,441	4,297	4,361
# of families		5,479		3,444		3,394
Mean dep. var.	0.789	0.789				
Controls	✓		✓		✓	
Family FE		✓		✓		✓

Heteroscedasticity-robust standard errors are shown in parenthesis. "$Fluoride \times \mathbb{1}[Cohort \leq 1962]$" denotes the DiD interaction of residing in the fluoridated parish and being born during the fluoridation, and "$Fluoride \times Intensity$" a linear years-of-exposure treatment measure capturing additional years of exposure for those residing in the fluoridated parish, starting with the cohort born 1962 and ending with the one born in 1952. "High school compl." refers to high school completion in 1990. "Cognitive ability" refers to standardized cognitive ability around age 18. "Non-cog. ability" refers to standardized non-cognitive ability around the same age.

Table A7. *High school completion rate 2010 in Kungsbacka compared to Varberg.*

Sample restriction: Outcome:	Municipality High school completion				Parish High school completion			
$Fluoride \times \mathbb{1}[Cohort \geq 1967]$	-0.018	-0.023	-0.025	-0.030	-0.022	-0.025	-0.023	-0.026
	(0.014)	(0.015)	(0.014)	(0.014)	(0.015)	(0.016)	(0.015)	(0.016)
Mean dep. var.	0.936	0.936	0.936	0.936	0.932	0.932	0.932	0.932
Obs.	22,797	19,963	22,442	19,640	8,639	7,255	8,516	7,155
Family characteristics			✓	✓			✓	✓
Excl. partially treated cohorts		✓		✓		✓		✓

Heteroscedasticity-robust standard errors are shown in parenthesis. "High school completion" refers to high school completion in 2010. "Excl. partially treated cohorts" refers to dropping the cohorts born 1966–1968 just before the fluoride concentration increased.

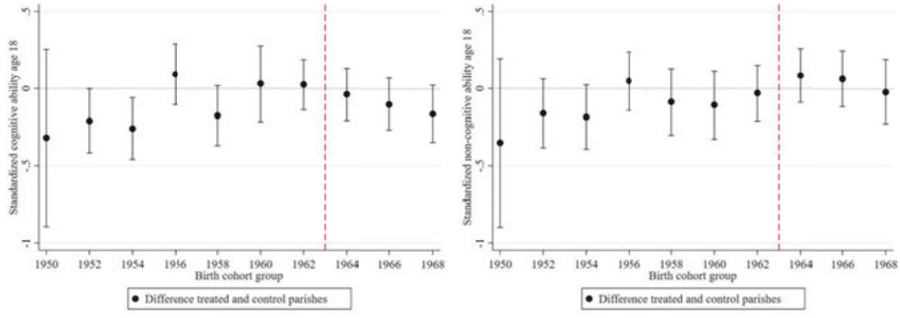
Table A8. *Standardized cognitive and non-cognitive ability age 18 in Kungsbacka compared to Varberg.*

Sample restriction:	Municipality				Parish			
Outcome:	Cog. ability		Non-cog. ability		Cog. ability		Non-cog. ability	
$Fluoride \times \mathbb{1}[Cohort \geq 1967]$	-0.078	-0.115	-0.137	-0.165	-0.140	-0.185	-0.171	-0.173
	(0.074)	(0.074)	(0.070)	(0.071)	(0.080)	(0.083)	(0.077)	(0.081)
Obs.	7,772	6,336	7,603	6,171	3,304	2,611	3,248	2,557
Family characteristics	✓	✓	✓	✓	✓	✓	✓	✓
Excl. partially treated cohorts		✓		✓		✓		✓

Heteroscedasticity-robust standard errors are shown in parenthesis. "Cog. ability" refers to standardized cognitive ability around age 18. "Non-cog. ability" refers to standardized non-cognitive ability around the same age. "Excl. partially treated cohorts" refers to dropping the cohorts born 1966–1968 just before the fluoride concentration increased.

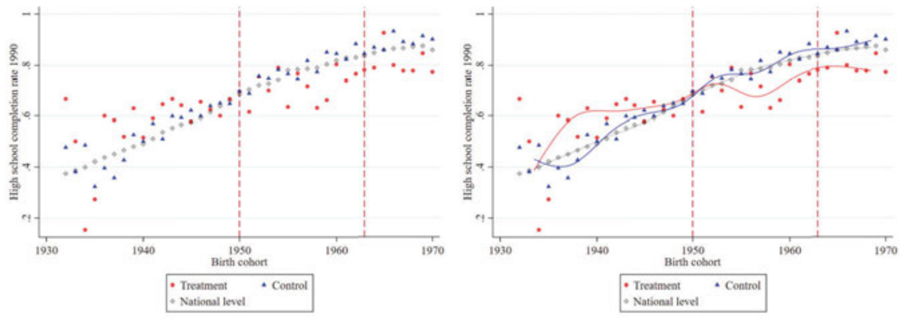
Figures

Result figures Norrköping



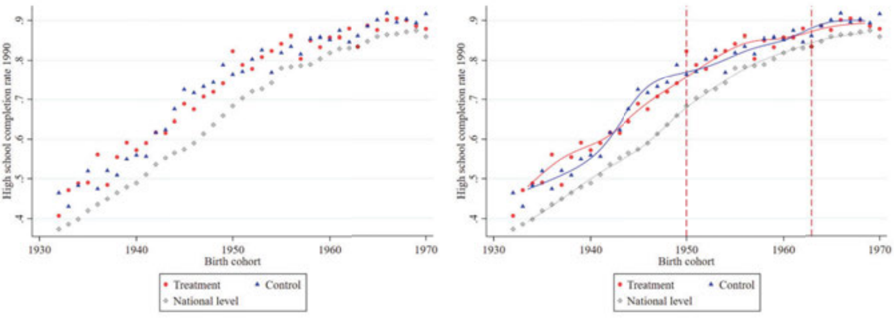
(a) Cognitive ability. *(b)* Non-cognitive ability.

Figure A1. The figures show the difference in regression outcomes on (non-)cognitive ability outcomes in 1990 between the treated and control parishes, by 2-year cohort group. The figures show data from the first available cohort group born from 1950 and onward. The dashed red line in each panel thus marks the first cohort groups that should not have been affected by the fluoridation. CI95 are shown in black.



(a) High school completion, split by treatment and control group. *(b)* High school completion, split by treatment and control group and with local polynomials.

Figure A2. The figures shows raw data of high school completion rates in 1990, collapsed by treatment status and birth cohort for those born in and residing in the same treated and control parishes in 1960 (stayers). Figure A2b also includes local polynomials capturing the time trends, and both panels include the high school completion rate on the national level. The dashed red lines mark the approximate cohorts that should have been affected by the fluoridation.



(a) High school completion in Linköping, split by treatment and control group.

(b) High school completion in Linköping, split by treatment and control group and with local polynomials.

Figure A3. The figures shows raw data of high school completion rates in 1990, collapsed by placebo treatment status and birth cohort for those residing in the treated western parts of Linköping and control parishes in the eastern parts of the city in 1960. Figure A3b also includes local polynomials capturing the time trends, and both panels include the high school completion rate on the national level. The dashed red lines mark the approximate cohorts affected by the fluoridation in Norrköping.

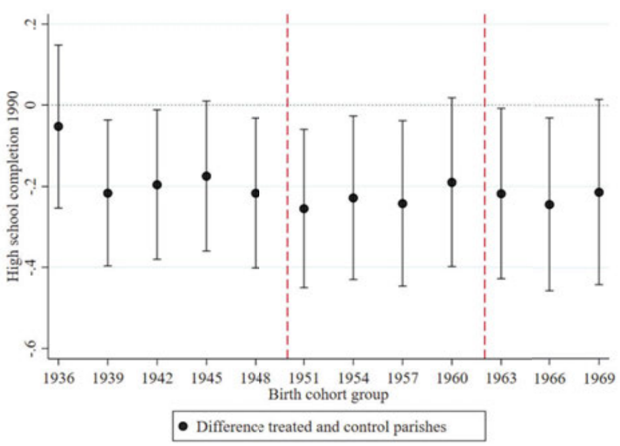


Figure A4. The figure show the difference in regression outcomes on high school completion outcomes in 1990 between the treated and control parishes, by 4-year cohort group and including family fixed effects. The dashed red lines mark the the approximate cohorts that should have been affected by the fluoridation. CI95 are shown in black.

Result figures Kungsbacka

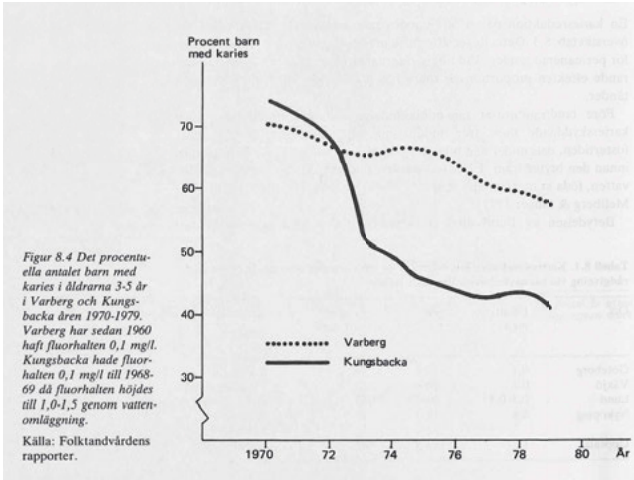
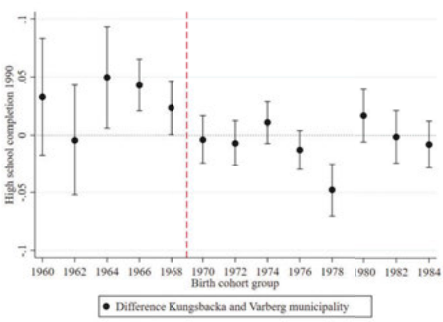
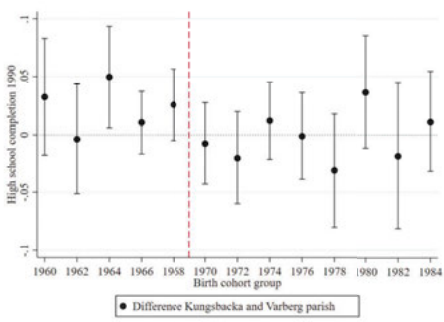


Figure A5. The figure shows a comparison of caries prevalence for Kungsbacka and Varberg municipality over time before and after the water source change in Kungsbacka 1968/69 to the freshwater lake with high fluoride concentration. The figure is from a government report on fluoridation published in 1981 (SOU 1981:32, 1981). The y-axis shows the percentage of children age 3–5 with caries in Kungsbacka (solid line) and Varberg (dotted line) municipality during the years 1970–1979 (x-axis). The underlying source of the data is from the public dental care system (Folktandvården).

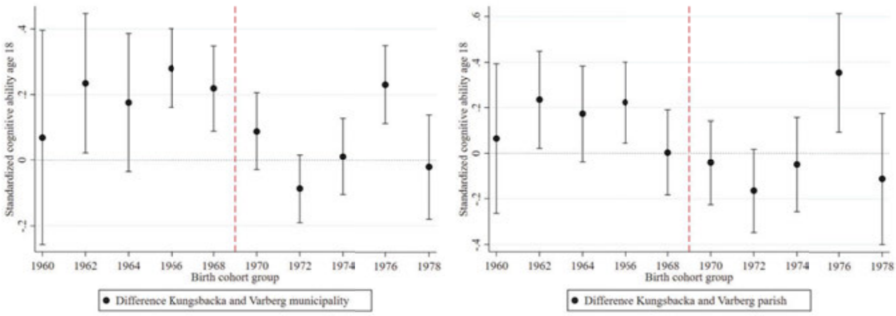


(a) High school completion in 2010, Kungsbacka municipality compared to Varberg municipality.



(b) High school completion in 2010, Kungsbacka parish compared to Varberg parish.

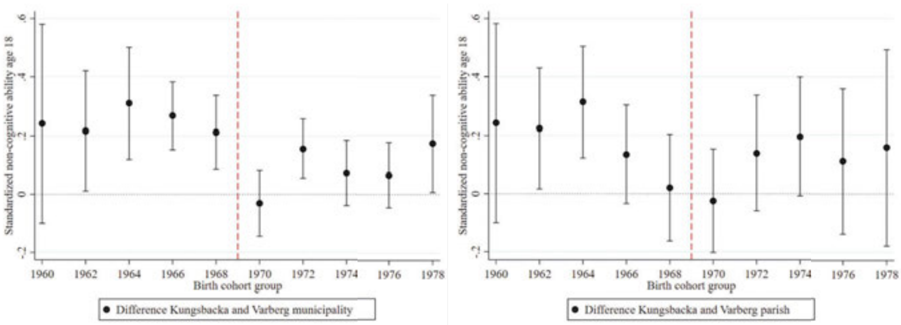
Figure A6. The figures shows regression outcome differences in high school completion rates in 2010 for Kungsbacka using Varberg as a control group, by cohort groups. CI95 are shown in black.



(a) Cognitive ability in Kungsbacka municipality.

(b) Cognitive ability in Kungsbacka parish.

Figure A7. The figures shows regression outcome differences in standardized cognitive ability for Kungsbacka using Varberg as control group, by cohort groups. CI95 are shown in black.

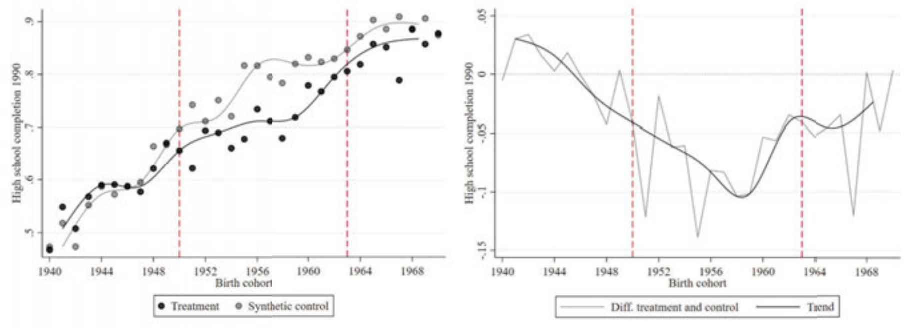


(a) Non-cognitive ability in Kungsbacka municipality.

(b) Non-cognitive ability in Kungsbacka parish.

Figure A8. The figures shows regression outcome differences in standardized non-cognitive ability for Kungsbacka using Varberg as control group, by cohort groups. CI95 are shown in black.

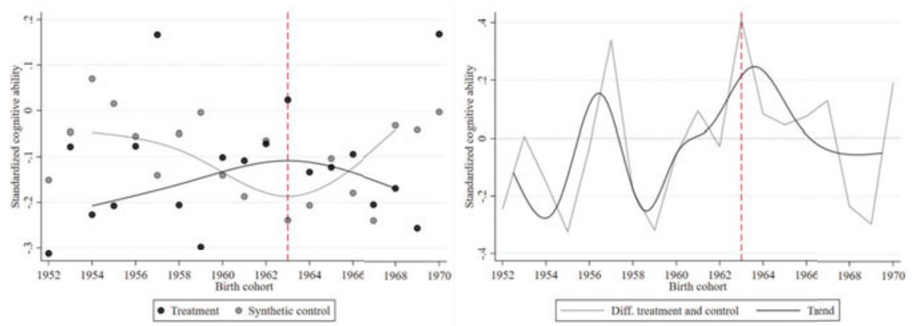
Synthetic control figures



(a) Norrköping raw data collapsed by treatment status and birth cohort.

(b) Norrköping difference in outcome between the treatment and synthetic control group.

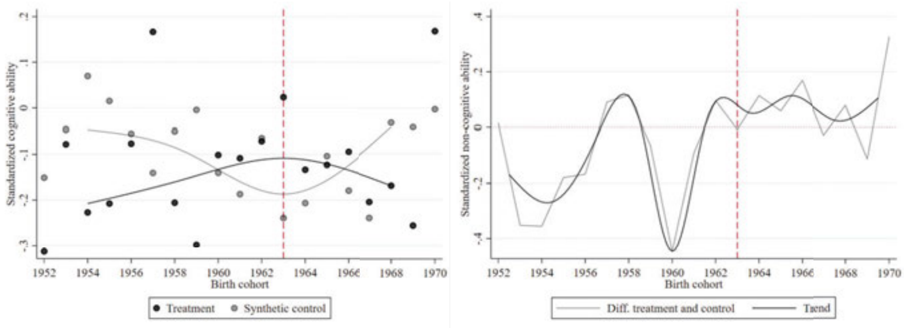
Figure A9. The figure shows raw data on high school completion in 1990 in Norrköping collapsed by treatment status and birth cohort compared to a synthetic control group consisting of a weighted average of algorithmic-selected parishes in the Östergötland region (where Norrköping is situated).



(a) Cognitive ability in Norrköping and the synthetic control group.

(b) Difference cognitive ability in Norrköping and the synthetic control group.

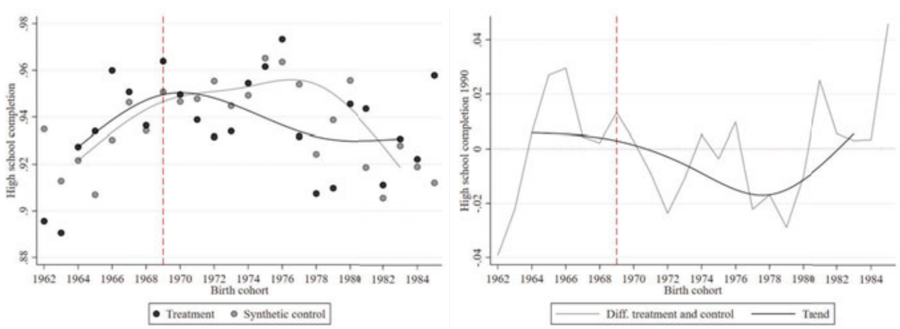
Figure A10. The figure shows raw data on standardized cognitive ability in Norrköping compared to a synthetic control group consisting of a weighted average of algorithmic-selected parishes in the Östergötland region (where Norrköping is situated), by cohort groups.



(a) Non-cognitive ability in Norrköping and the synthetic control group.

(b) Difference non-cognitive ability in Norrköping and the synthetic control group.

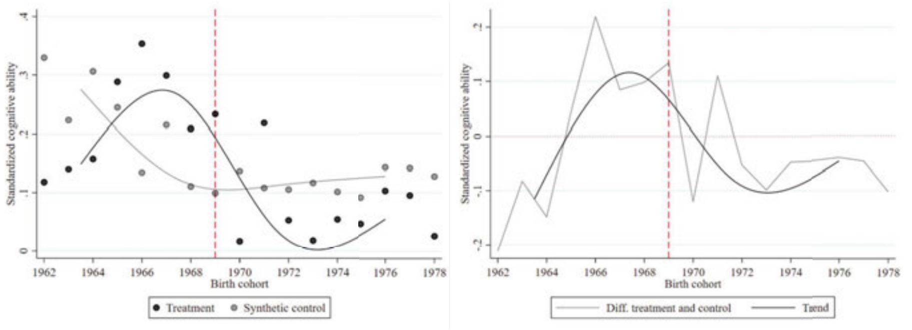
Figure A11. The figure shows raw data on standardized non-cognitive ability in Norrköping compared to a synthetic control group consisting of a weighted average of algorithmic-selected parishes in the Östergötland region (where Norrköping is situated), by cohort groups.



(a) Kungsbacka raw data collapsed by treatment status and birth cohort.

(b) Kungsbacka difference in outcome between the treatment and synthetic control group.

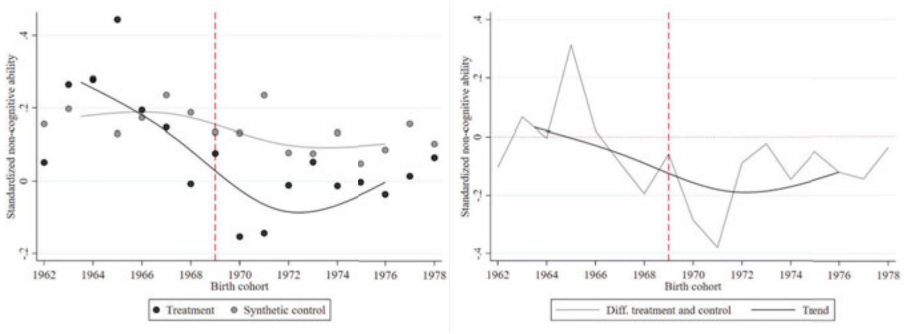
Figure A12. The figure shows raw data on high school completion in 1990 in Kungsbacka collapsed by treatment status and birth cohort compared to a synthetic control group consisting of a weighted average of algorithmic-selected municipalities in Sweden.



(a) Cognitive ability in Kungsbacka and the synthetic control group.

(b) Difference cognitive ability in Kungsbacka and the synthetic control group.

Figure A13. The figure shows raw data on standardized cognitive ability in Kungsbacka compared to a synthetic control group consisting of a weighted average of algorithmic-selected municipalities, by cohort groups.

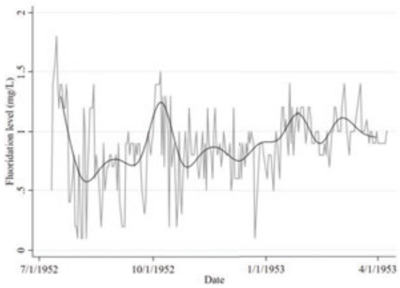


(a) Non-cognitive ability in Kungsbacka and the synthetic control group.

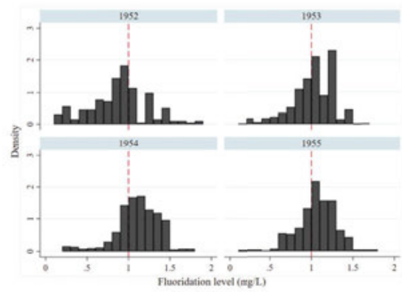
(b) Difference non-cognitive ability in Kungsbacka and the synthetic control group.

Figure A14. The figure shows raw data on standardized non-cognitive ability in Kungsbacka compared to a synthetic control group consisting of a weighted average of algorithmic-selected municipalities, by cohort groups.

Supporting figures



(a) Time series of the fluoride concentration level in the treated part of Norrköping 1952–1953.



(b) Histograms of the fluoridation levels in the treated part of Norrköping 1952–1955.

Figure A15. The figure shows measurements of the fluoridation level of the water supply in the treated part of Norrköping (low zone) for the early years of the experiment. The data is from the Norrköping city archive. The number of tests performed each year are 182 for 1952 and 405–675 for 1953–1955. The average fluoride concentration is somewhat lower in 1952 at 0.82 mg/L compared to the following years, with the values for 1953–1955 ranging from 0.96–1.07. For the years 1956–1957 (not shown here), the average fluoride levels are slightly higher at 1.20–1.24 mg/L.

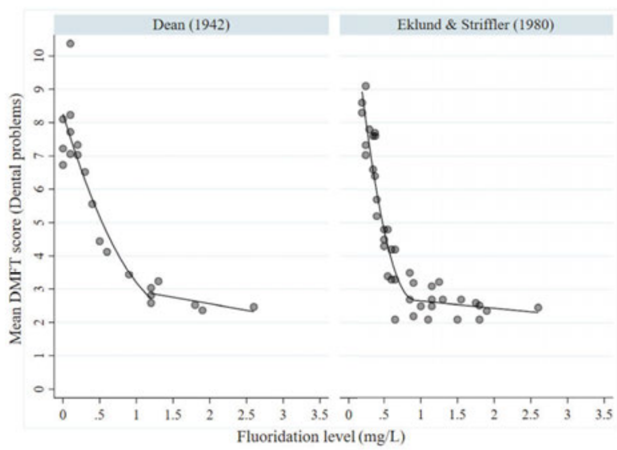


Figure A16. The figure shows scatter plots of water fluoride concentration and mean number of caries prevalence of 12–14-year-old children in 21 and 41 U.S. cities ca 1940–1980 (Dean, 1954). The left panel shows the original findings of Dean, 1954 for 21 U.S. cities, while the right panel shows additional data of temperature-adjusted water fluoride levels (> 0.1 mg/L) from the same and some additional U.S. cities (Eklund and Striffler, 1980). The work by Eklund and Striffler, 1980 thus adds data points and takes into account that cities with higher temperatures have a greater water consumption. A quadratic and linear polynomial are fitted around the fluoride values below and above 1 mg/L water.

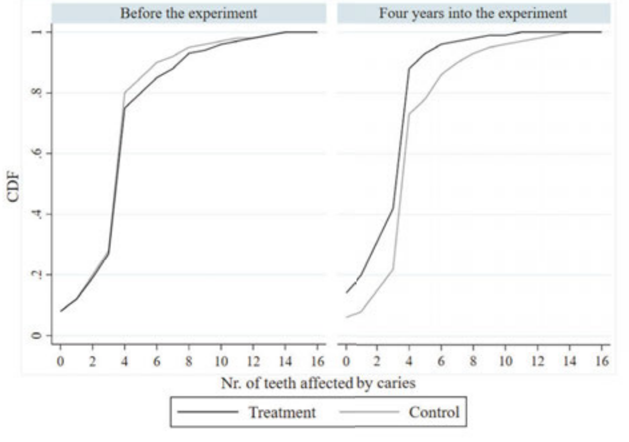


Figure A17. The figure shows cumulative distribution functions on dental health based on data from the original Norrköping water fluoridation experiment for children age 8–10 in the treatment and control group, before the experiment and four years into the experiment (Melander, 1957).

Essay III. The effects of school closures on SARS-CoV-2 among parents and teachers
Co-authored with Helena Svaleryd and Jonas Vlachos

Acknowledgements: We are grateful for comments by Jonas Björk, Lena Edlund, Jens Engleson, Tove Fall, Erik Grönqvist, Emily Oster, Oskar Nordström Skans and David Strömberg. This paper was published in PNAS (2021), Vol. 118, No. 9, e2020834118

1 Introduction

In the effort to contain the spread of severe acute respiratory syndrome coronavirus 2 (SARS-CoV-2), most countries closed schools. An estimated 1.3 billion students in 195 countries were affected by school closures in mid-April 2020 (UNESCO, 2020). These closures are likely to have a negative impact on student learning and well-being, especially for students from disadvantaged backgrounds (Dorn et al., 2020; Guessoum et al., 2020). School closures also affect labor supply, not least among healthcare workers, hence reducing healthcare capacity (Bayham and Fenichel, 2020). While the costs associated with school closures are high, modeling studies question their effectiveness in reducing the transmission of SARS-CoV-2 and direct evidence is largely missing (Viner et al., 2020). The absence of direct evidence is because school closures were usually implemented early, universally, and in close proximity to a raft of nonpharmaceutical interventions (NPIs) that have been documented and modeled to bring about large reductions in the basic reproduction number (Hsiang et al., 2020; Kraemer et al., 2020; Pan et al., 2020; Tian et al., 2020; Maier and Brockmann, 2020; Auger et al., 2020). This renders it difficult, if not impossible, to disentangle the effects of each specific intervention.

Sweden was an exception to the norm of universal school closures. On March 18, 2020, one week after the first reported death from COVID-19, upper secondary schools moved to online instruction, while schools for younger students remained open until the end of the school year in mid-June. While other NPIs were also implemented (see Appendix B), this partial school closure allows for a comparison of individuals and households who were differently exposed to open and closed schools, but otherwise faced similar conditions throughout the period of widespread contagion illustrated in Fig. 1. In this study, we link detailed register data from Statistics Sweden on the entire Swedish population to all PCR-identified cases of SARS-CoV-2 reported to the Public Health Agency of Sweden and COVID-19 cases requiring medical treatment reported to the National Board of Health and Welfare between the time of school closure to the end of the school year. To study the general impact of school closure on the transmission of the virus, we estimate differences in infection rates between parents exposed to lower and upper secondary students. We further analyze differences in infection rates between lower and upper secondary teachers, as well as their partners.

For school closures to affect virus transmission, they must affect behavior and contact patterns. The impact of school closures on the transmission of SARS-CoV-2 further depends on how the virus spreads between students, from students to adults, and among adults in school and at home. Current reviews of the evidence suggest that while children and adolescents do get infected, they usually develop mild or no symptoms (ECDC, 2020; Goldstein et al., 2020). The susceptibility to infection appears to be lower among the young, but there is some uncertainty regarding this, as a large number of cases probably go undetected. Children and adolescents with mild or no symptoms may still carry and spread the infection, but the evidence available indicates that infectiousness, just as the severity of symptoms, is increasing in age. Outbreaks have been reported in connection to school openings and overnight summer camps (Stein-Zamir et al., 2020; Szablewski, 2020), but transmission within schools prior to their closure at the onset of the pandemic appears to have been limited (Heavey et al., 2020; Macartney et al., 2020). A general caveat concerning the available evidence is that most studies on the susceptibility and infectiousness of children and adolescents have been conducted when schools were closed and other NPIs were in place.

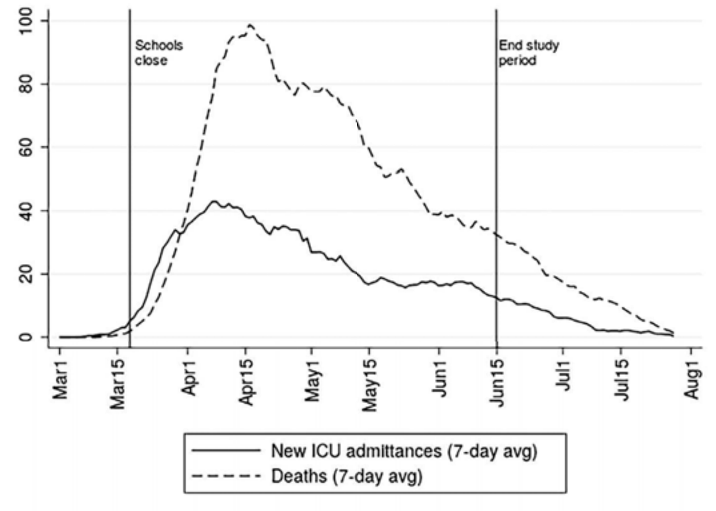


Figure 1. COVID-19 deaths and ICU admissions. 7-day averages (avg) of deaths and ICU admissions. Solid vertical lines mark the start of school closure and the end of the period of analysis. Data are from the Public Health Agency of Sweden (Public Health Agency of Sweden, 2020a).

Differences between groups can be attributed to school closures if the groups are behaviorally and biologically similar in all other respects that affect the probability to get infected and tested. Lower secondary school (school years 7–9, typical age 14–16) is compulsory. Attendance to upper secondary school (school years 10–12, typical age 17–19) is close to universal, but grade repetition is more common at the upper secondary level, in particular among students with non-EU background (Swedish National Agency for Education, 2020a). We therefore restrict the main sample to parents without such a background, but also present results for all parents. The main selection concern regards the age of parents and students. Parental characteristics (age, sex, income, occupation, region of origin and of residence) are controlled for, but the susceptibility and infectiousness are likely to increase in student age, and general behavior may differ between younger and older students. We therefore focus our attention on parents exposed to students in the final year of lower secondary and first year of upper secondary school. The main concern regarding differences between upper and lower secondary teachers and their partners refers to partner characteristics that are adjusted for. Given these restrictions and adjustments, the estimated differences can plausibly be attributed to the exposure to open and closed schools. The study thus offers credible, direct evidence of the impact of school closures on the SARS-CoV-2 pandemic.

Models based on influenza predict that school closures can be effective if they actually reduce the number of contacts, the basic reproduction number (R0) is below two, and the attack rate is higher in children than in adults (Jackson et al., 2014). The basic reproduction number for SARS-CoV-2 is above two (C.-C. Lai et al., 2020) and the attack rate in students is likely to be low relative to adults (ECDC, 2020). The theoretical prior is therefore that the impact of school closures on the transmission of SARS-CoV-2 among parents is low (Viner et al., 2020). For teachers and their partners, a more substantive impact can be expected. Teachers at open schools were not only exposed to students, but also to other adults, both at work and during their commute. Upper secondary teachers partly worked from school, but a substantive fraction did their teaching from home (see Appendix B).

2 Results

We estimate differences in infections among parents, teachers, and teachers' partners who were differently exposed to lower (open) and upper (online) secondary schools using linear probability models (ordinary least squares [OLS]) and logistic regressions (Logit). Descriptive statistics are shown in Table 2. The preferred outcome is PCR-confirmed SARS-CoV-2, which has the highest incidence (7.37 cases per 1,000 among lower and upper secondary parents and 4.69 per 1,000 among teachers). If we exclude healthcare workers, who were targeted for testing, the incidence drops to 4.33 per 1,000 among parents. One potential drawback of this outcome is that unbiased results rely on compared groups having equal propensity to get tested. In particular, it could be that those directly or indirectly exposed to open schools were more prone to get tested, which would exaggerate the impact of school closures. The risk of such bias is alleviated by the limited testing capacity that forced testing to be targeted towards those with severe symptoms and care workers throughout most of the relevant period (see Appendix B). However, we also analyze COVID-19 diagnoses from healthcare visits, which is less likely to suffer from bias due to behavioral differences. Healthcare coverage in Sweden is universal, and fees for doctor or hospital visits are low, assuring individuals in need will seek care. This is particularly true for hospitalizations, since admittance to hospital is determined strictly on medical grounds. As receiving a COVID-19 diagnosis is a less frequent event (3.11 per 1,000 among parents; 2.60 per

1,000 among teachers), these estimations have lower statistical power. Low incidence is an even larger problem for severe cases (hospitalizations or deaths), which has an incidence of 1.43 per 1,000 among parents and 1.59 per 1,000 among teachers. Results for severe cases are reported in Appendix B, Table B1.

The data covers the entire relevant Swedish population and contains all reported cases, as well as detailed information on covariates (see Materials and Methods in Appendix A and Appendix B for details). Upper secondary schools moved online on March 18. Allowing for an incubation period from infection to symptoms of about a week (Lauer et al., 2020), the cutoff date is set to March 25 for teachers and April 1 for parents and teachers' partners. The school year ends during the second week of June, and the end date is therefore set to June 15 for PCR tests and June 30 for diagnoses through healthcare contacts.

Parents

Parental school exposure is defined by the school year that the youngest child in the household attends. In order to attribute estimated differences to school closures, households must be similar in all aspects that affect the likelihood of getting infected or tested, except for their exposure to open and closed schools. By narrowing the comparison to parents with the youngest child in the final year of lower secondary (year 9) and first year of upper secondary school (year 10), we reduce the risk of introducing biases due to confounding factors. A potential threat to identification is that student with non-EU migrant background are more likely to repeat grades in upper secondary schools, in particular through preparatory programs (Swedish National Agency for Education, 2020a). Although upper secondary grade repetition occurs also for other groups, the concern is not as severe among families from Sweden, the EU, and the Nordic countries. To avoid selection into grade 10 in upper secondary school, we restrict the population to parents born in Sweden, EU and the Nordics (dropping 16% of the parental population). In Appendix B, we substantiate these claims by showing balance on covariates predicting the incidence of SARS-CoV-2 for the main sample (Appendix B, Fig. B2), while balancing tests perform worse when including non-EU migrants (Appendix B, Fig. B3).

Fig. 2 shows the estimated odds ratios (ORs) for PCR-confirmed SARS-CoV-2 parents from logistic regressions, where we adjust for age, sex, occupation, educational attainment, income, and regions of residence and of origin. Results for parents by school years 7–12 show that there is a tendency of a positive age gradient, potentially indicating a higher parental risk of infection when exposed to older children. The most relevant comparison is therefore

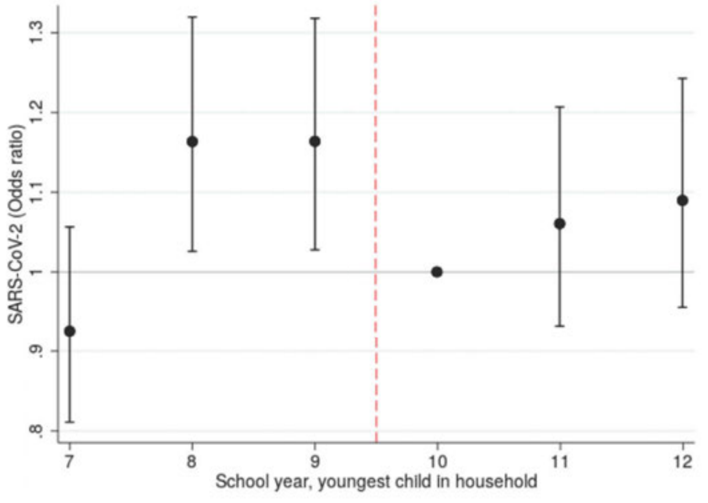


Figure 2. SARS-CoV-2 ORs for parents by school year of the youngest child in the household. ORs are estimated using logistic regression. The reference category is school year 10 and CI95 values are indicated.

between school years 9 and 10 (reference category), for which we in Table 1 estimate an OR of 1.17 [95% CI (CI95) 1.03–1.32].

Corresponding results using OLS are shown in Table 1, which also includes results for COVID-19 diagnoses from healthcare contacts. The estimates indicate that parental exposure to open schools results in 1.05 (s.e. 0.43) additional SARS-CoV-2 cases per 1,000 individuals and −0.17 (s.e. 0.26) additional COVID-19 diagnoses per 1,000. The OR for COVID-19 diagnoses is 0.94 [CI95 0.77–1.14]. The estimates for COVID-19 diagnoses are thus negative, albeit imprecise and statistically indistinguishable from zero. This indicates that the increase in PCR-confirmed cases does not necessarily translate into similar size effects on the probability to get a COVID-19 diagnoses when visiting a doctor or being admitted to hospital. The same applies to the estimates for severe cases shown in Appendix B, Table B1 [OR 0.84; CI95 0.64–1.11].

Teachers

We analyze differences between lower and upper secondary teachers and their partners. Upper secondary teachers constitute a relevant counterfactual to the work situation that lower secondary teachers had been in if their schools had moved to online instruction. The groups are also similar with respect to educational attainment and geographic dispersion. As there may still be differences in the household composition between the groups, we—in addition to the controls used for parents—adjust for the occupation and educational attainment of teachers' partners, the number of children in separate age groups linked to the household, and whether or not the teacher is single. Table 1 shows that the likelihood of a positive PCR test was twice as high for lower secondary than for upper secondary teachers [OR 2.01; CI95 1.52–2.67]. The table also shows a corresponding OLS estimate of 2.81 additional cases per 1,000 (s.e. 0.59). An identical estimate is found for COVID-19 diagnoses [OR 2.01; CI95 1.45–2.79], indicating that the PCR results are not due to biased testing. Appendix B, Table B1 shows an estimate for severe cases of similar magnitude [OR 2.15; CI95 1.41–3.29].

Table 1. *Effect of exposure to open schools on PCR tests and COVID-19 diagnoses.*

	Parents		Teachers		Partners	
Panel A	OLS (Cases/1,000)					
	PCR	Diag.	PCR	Diag.	PCR	Diag.
Open	1.05**	−0.17	2.81***	1.47***	1.47**	0.14
	(0.43)	(0.26)	(0.59)	(0.36)	(0.71)	(0.46)
M. dep.	6.37	2.74	2.96	1.61	5.10	2.29
Obs.	166,630	166,719	72,946	72,976	47,383	47,413
Panel B	Logit (Odds ratios)					
	PCR	Diag.	PCR	Diag.	PCR	Diag.
Open	1.17**	0.94	2.01***	2.01***	1.29*	1.04
	[1.03, 1.32]	[0.77, 1.14]	[1.52, 2.67]	[1.45, 2.79]	[1.00, 1.67]	[0.70, 1.52]
Obs.	163,195	163,155	70,151	64,080	44,025	41,775

Note: ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$. Results are estimated using linear probability models (OLS) in Panel A, and logistic regressions (Logit) in Panel B. CI95 are shown in brackets. Standard errors in parenthesis are clustered at the school level for teachers and their partners, and at the household level for parents. "Open" indicates exposure to (open) lower secondary schools. "M. dep." refers to mean dependent variable for the reference category exposed to (online) upper secondary schools. The outcome "PCR" refers to positive PCR tests and "Diag." refers to COVID-19 diagnoses.

In order to gauge the magnitude of the estimated effects for teachers, Fig. 3 compares the incidence of detected SARS-CoV-2 among teachers with occupations at the three-digit level with at least 1,000 employees in ages 25–65 (healthcare workers excluded). Among the 124 compared occupations, upper secondary teachers (3.25 per 1,000) are at the median, while lower secondary teachers (5.91 per 1,000) constitute the seventh-most-affected occupation. Drivers (which includes taxi drivers) are the at the top of the distribution, while driving instructors have the same level of infections as lower secondary teachers.

A list of all occupations is available in Appendix B, Table B11. As another comparison, Table 2 shows that the incidence of SARS-CoV-2 is higher among lower secondary teachers (5.91 per 1,000) than that of the parents to the students they teach (4.23 per 1,000, excluding healthcare workers). This is also the case for COVID-19 diagnoses and more severe health outcomes. Note that parents with non-EU background are excluded from these comparisons. The rate of infections is higher than average in this group, and, when included, the rate among lower secondary parents increases to 5.33 cases per 1,000 (excluding healthcare).

Fig. 3 also indicates the incidence of detected SARS-CoV-2 among lower primary (school years 1–3; 3.81 cases per 1,000) and upper primary (years 4–6; 4.82 cases per 1,000) teachers. These teachers are less specialized and therefore not only meet younger, but also fewer, students than teachers at the lower secondary level. They may also interact differently with their colleagues. The incidence among these teachers is below lower secondary teachers, but above upper secondary teachers, also when controlling for covariates (Appendix B, Table B2). These results are consistent with a positive risk gradient in student age, but could reflect other differences in the work environment.

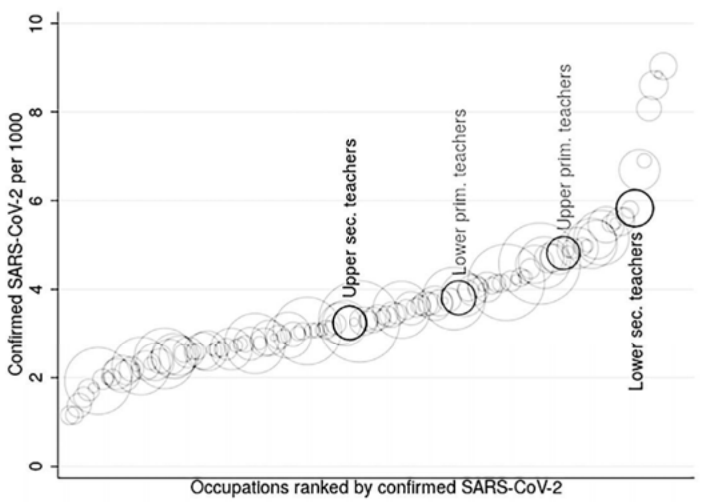


Figure 3. SARS-CoV-2 across occupations. Circle size corresponds to the number of employees in each occupation. Incidence (cases per 1,000) of detected SARS-CoV-2 by 3-digit occupational codes (SSYK2012) until June 15, 2020. Ages included are 25–65, and only occupations with at least 1,000 employees are reported. Values for the upper and lower secondary teachers (as well as lower and upper primary teachers in grey) from the Teacher Register in our sample are indicated in black.

Teachers' partners

The higher incidence of infections among lower secondary teachers spilled over to their partners, who have a higher incidence of positive PCR tests than their upper secondary counterparts [OR 1.29; CI95 1.00–1.67] (Table 1). This is evidence of within-household transmission from teachers to their partners. The estimates for teachers and their partners implies a secondary attack rate (SAR) between spouses of 0.52 [CI95 0.05–1.18].[1] This is well within the bounds of the between-spouse SAR of 0.43 [CI95 0.27–0.6] suggested from contact studies (Madewell et al., 2020). However, the estimates for COVID-19 diagnoses for teachers' partners are not statistically distinguishable from zero [OR 1.04; CI95 0.70–1.52], and the same applies for severe cases [OR 1.09; CI95 0.62–1.92] (Appendix B, Table B1). The relatively imprecise estimates for these outcomes also renders them statistically indistinguishable from the estimates for PCR-confirmed SARS-CoV-2.

Robustness

In the Appendix B, we provide several robustness tests of the main results. **1)** Students in lower and upper secondary school are not fully comparable, as grade repetition is more common among the latter. Excluding covariates (except age and sex) in Appendix, Table B3 leads to a reduction in the estimates for parents [OLS 1.01, s.e. 0.43]. This is consistent with socioeconomic factors correlating both with upper secondary grade repetition and the incidence of SARS-CoV-2. Dropping covariates (except age and sex) leads to a small increase in the estimated impact for teachers [2.94, s.e. 0.58] and their partners [1.58, s.e. 0.71]. Both results are consistent with lower secondary partners being employed in more exposed occupations. Test have already shown poor balance when including parents of non-EU background. However, widening the sample to include these parents does not substantially alter the results. The OLS estimates with controls [1.09, s.e. 0.42] and when only controlling for age and sex [1.02, s.e. 0.42] are similar to those for the main sample. ORs for both samples of parents are similar when only controlling for age and when excluding all controls (Appendix B, Fig. B4). Appendix B, Fig. B5 shows the ORs including all controls for the main sample (Fig. B5a) as well as when non-EU migrants are included (Fig. B5b). **2)** Media searches reveal that some lower secondary schools closed spontaneously and preemptively, albeit for brief periods of time (see Appendix B). As privately run independent schools were over-represented in this group, we exclude individuals connected to such lower secondary schools. This results in somewhat larger estimates for parents [1.33, s.e. 0.46] (Appendix B, Table B4), consistent with balancing tests reflecting high socioeconomic status and, hence, less predicted exposure among

[1] 1.47 cases per 1,000 among partners and 2.81 cases per 1,000 among teachers gives a SAR of 0.52. Bootstrapping with 2,000 repetitions gives a non-parametric CI95 of 0.05–1.18.

these parents (Appendix B, Fig. B6). Dropping independent lower secondary schools only slightly affects the estimates for teachers [2.63, s.e. 0.63] and their partners [1.64, s.e. 0.77] (Appendix B, Table B5). **3)** It may have been more common among vocational programs to let small groups of students return to school to complete practical assignments. We therefore exclude parents connected to vocational upper secondary programs. These tend to be of lower socioeconomic status, which is reflected in a poorly performing balancing test (Appendix B, Fig. B6). Consistent with this test, the point estimate is reduced [0.64, s.e. 0.53] (Appendix B, Table B4). **4)** Rather than controlling for employment in the healthcare sectors, we drop teacher households where the partner is a healthcare employee. As expected, the results remain unchanged (Appendix B, Table B5). **5)** We derive a slightly different measure of parental exposure to lower secondary schools that allows for a broader sample of parents, and the results are similar [0.98, s.e. 0.34] (Appendix B, Table B4). **6)** We broaden the comparison between lower and upper secondary parents by pooling those exposed to school years 8–11 and 7–12. This risks conflating the impact of exposure to open schools with student age. The estimate is lower for the 8–11 comparison [0.79, s.e. 0.31] and even lower, and not significant, for years 7–12 [0.20, s.e. 0.26] (Appendix B, Table B4). **7)** Household size might affect the risk of infection and it is decreasing by school year. Controlling for household size, however, does not affect the point estimates (Appendix B, Table B6). **8)** To ensure that the results are not sensitive to the choice of cutoff dates, we use March 25 and April 16 for all groups. Since fewer cases are detected from the latter date, the OLS estimates are slightly reduced, but the ORs are close to identical (Appendix B, Table B7).

Heterogeneity

How school closures affect the transmission of the virus depend on how they reduce contact between those potentially infected. This may differ depending on contextual factors, and we analyze two types of heterogeneity. First, we allow the estimates for exposure to lower secondary schools to differ by population density in the district of residence. Second, since the timing of NPIs may affect their effectiveness (Caselli et al., 2020), we let estimates vary by the regional rate of infections prior to school closure. The results in Appendix B, Table B8 reveal interaction terms with large standard errors, not allowing a clear interpretation.

Distribution of cases across schools

Past coronavirus outbreaks (SARS and MERS) have shown large individual variation in infectiousness, implying that some individuals infected large number of secondary cases, leading to ‘super-spreading events’ (Lloyd-Smith, James O and Schreiber, Sebastian J and Kopp, P Ekkehard and Getz, Wayne M, 2005). Estimates of the dispersion factor k—indicating heterogeneity in infectiousness—for SARS-CoV-2 vary, but suggest that this virus as well might spread in clusters (Endo et al., 2020; Riou and Althaus, 2020). If the spread is highly clustered and the virus spread at the schools, we would expect most of the cases to be concentrated to a few schools.

The data at hand is not ideal to study such transmission patterns, as the paucity of testing implies that a large number of cases goes undetected. With this caveat in mind, Appendix B, Fig. B7 shows how the cases are distributed across schools with different number of cases, and Appendix B, Fig. B8 shows how cases are clustered in time within schools, separately for teachers and parents. There is some indication that cases among lower secondary school teachers were relatively concentrated, but, among parents, the cases are more evenly spread across schools and over time.

Students

We do not study the impact of school closures on students, but for descriptive purposes, Appendix B, Table B9 shows estimates of infection rates for students under age 18 in school years 7–10. The incidence for students in year 10 is 0.53 PCR-confirmed cases per 1,000, and estimated differences between school years are not statistically significant. Because of age-related differences in access to testing (see Appendix B), the severity of symptoms, risk behavior, and patterns of socialization, results for students are likely to be biased and difficult to interpret. It can be mentioned that there were zero COVID-19 deaths recorded in age groups 2–19 in Sweden until late July, 2020. The rate of severe cases was also low; 94 hospitalizations were recorded among the 1.23 million students in compulsory school age (7–16) and 84 among the 339,000 youths in ages 17–19 (Appendix B, Table B10). There might be other health implications for children and adolescents, but analyzing this is beyond the scope of this study.

3 Discussion

On March 18, 2020, upper secondary schools in Sweden moved to online instruction, while lower secondary schools continued instruction as normal. This partial school closure provides a rare opportunity to study the impact on the transmission of SARS-CoV-2 during a period of widespread contagion. The impact of school closures on the transmission of the virus in society is best captured by the results for parents. We find that parental exposure to open, rather than closed schools, is associated with a somewhat higher rate of PCR-confirmed SARS-CoV-2 infections [OR 1.17; CI95 1.03–1.32]. The association is weaker for COVID-19 diagnoses from healthcare visits [OR 0.94;

CI95 0.77–1.14] and severe cases that include hospitalizations and deaths [OR 0.84; CI95 0.64–1.11].

The positive association for PCR-confirmed cases could partly reflect other behavioral or biological differences between households with slightly younger and older children, but if treated as a causal, the estimates indicate that a hypothetical closure of lower secondary schools in Sweden would have resulted in 266 fewer detected cases among the 253,538 parents in our sample. Limited testing capacity means that this only reflects a fraction of the actual number of cases, but it corresponds to a 15% reduction of the 1,825 detected cases among lower secondary parents until mid-June (1,072 cases when excluding healthcare workers). Since sample restrictions are made, the actual number of parents exposed to lower secondary schools is around 450,000 parents. The results thus indicate that closing lower secondary schools would have resulted in a 17% decrease in infections among 4.5% of the Swedish population. It is important to note that this captures both primary and secondary infections among household adults, and the full implications for virus transmission have to be derived using modeling. Although not conclusive in this regard, results are consistent with parental risk of infection increasing in student age. We might therefore somewhat underestimate the actual impact of keeping lower secondary schools open. More importantly, this means that the implications of keeping upper and lower secondary schools open may not be symmetric.

Teachers were more severely affected by the decision to keep lower secondary schools open. We estimate a PCR-confirmed infection rate twice as high among lower secondary teachers relative to teachers at upper secondary level [OR 2.01; CI95 1.52–2.67]. This is fully consistent with the results for COVID-19 diagnoses from healthcare visits [OR 2.01; CI95 1.45–2.79] and severe cases [OR 2.15; CI95 1.41–3.29]. When excluding healthcare workers, a comparison of SARS-CoV-2 infection rates across 124 occupations shows that upper secondary teachers are at the median, while lower secondary teachers constitute the seventh-most-affected group. Other occupations with high infection rates (e.g. taxi drivers, driving instructors, social assistants, and police officers) tend to have close interactions at work. This suggests that infections occur at school, and there are some indications of clusters of cases among teachers. However, we cannot determine to what extent this is due to infections from students to teachers or if they reflect interactions between teachers. Primary school teachers had lower rates of infection than teachers at the lower secondary level, and the patterns are consistent with teacher risk increasing in student age. Alternative explanations, such as different modes of interactions between the teaching staff, are possible, and this highlights that the impact of keeping schools open may not be symmetric across educational settings.

Increased infections among lower secondary teachers spill over to their partners, who have a higher PCR-confirmed infection rate than their upper secondary counterparts [OR 1.29; CI95 1.00–1.67]. As for parents, the estimates are lower for COVID-19 diagnoses [OR 1.04; CI95 0.70–1.52] and severe cases [OR 1.09; CI95 0.62–1.92] among teachers' partners.

Combining the estimates, 148 fewer cases of SARS-CoV-2 had been detected among lower secondary teachers (110) and their partners (38) if lower secondary schools had closed. To this, we can add an estimate of 472 fewer cases among 450,000 adults exposed to lower secondary students in their households. Most transmission is within households, so even if 620 fewer detected cases is a lower bound, this can be seen as relatively low compared to the country total of 53,482 detected cases until mid-June (35,556 excluding healthcare workers). Based on an age-specific case fatality rate (CFR) of 1.1% (Appendix B, Table B10), this corresponds to 6.5 fewer deaths, 5 among parents and 1.5 among teachers and their partners. This counterfactual inference regarding mortality is highly uncertain, however. In our sample, we count a total of 11 COVID-19 related deaths at the lower secondary level (9 parents, 1 teacher, 1 partner). The corresponding number at the upper secondary level is 16 (all parents). For severe health outcomes, we find 79 cases among 39,446 lower secondary teachers. According to the estimates, this number had been down to 46 if lower secondary schools had closed.

Closing the schools is a costly measure with potential long-run detrimental effects for students. The results presented are in line with theoretical work indicating that school closure is not an effective way to contain SARS-CoV-2 (Viner et al., 2020), at least not when facing as high a level of contagion as Sweden did during the spring of 2020. It is not clear how the results generalize to other settings, and studies have found both positive and negative associations between closed schools and the rate of transmission of SARS-CoV-2 (Hsiang et al., 2020; Auger et al., 2020; Haug et al., 2020; Isphording et al., 2020). The mixed evidence could reflect methodological differences and difficulties isolating the impact of schools. However, they could also reflect differences in how schools are organized and local conditions at the time of intervention. Unfortunately, our results do not allow any firm conclusions regarding interactions between school closures and local conditions. Another potentially important difference between settings is the level the precautionary measures undertaken within schools. According to an international comparison (Guthrie et al., 2020), the measures recommended in Sweden (Public Health Agency of Sweden, 2020b) are best described as mild. In particular, there is no quarantine of those exposed unless they show symptoms of infection, no imposed class size reductions, and face masks are rarely used (YouGov, 2020).

While the overall impact on overall virus transmission was limited according to this study, keeping lower secondary schools open had a quite substantial impact on teachers, and the results suggest that the risk to teachers can be increasing in student age. This should be taken into account, and precautionary measures could be considered.

Table 2. *Descriptive statistics: Parents, teachers and teachers' partners.*

	Parents school years 7–12			Parents school years 9–10		
	Full sample	Lower sec.	Upper sec.	Full sample	Lower sec.	Upper sec.
Cases/1,000	7.37	7.20	7.56	7.57	8.00	7.15
... ex health	4.33	4.23	4.43	4.51	4.70	4.32
...pre cutoff	0.64	0.56	0.74	0.73	0.67	0.79
Healthcare/1,000	3.11	2.91	3.34	3.22	3.06	3.37
Severe cases/1,000	1.43	1.27	1.61	1.48	1.37	1.59
# Deaths	25	9	16	6	3	3
Age	50.27	48.89	51.81	50.46	49.98	50.92
	(5.89)	(5.76)	(5.65)	(5.69)	(5.66)	(5.69)
Obs.	480,291	253,538	226,753	166,630	81,598	85,032
	Teachers			**Teachers' partners**		
	Full sample	Lower sec.	Upper sec.	Full sample	Lower sec.	Upper sec.
Cases/1,000	4.69	5.91	3.25	6.16	6.60	5.64
... ex health				4.21	4.99	3.26
...pre cutoff	0.27	0.25	0.30	0.59	0.63	0.55
Healthcare/1,000	2.60	3.29	1.79	2.65	2.77	2.51
Severe cases/1,000	1.59	2.00	1.10	1.25	1.33	1.16
# Deaths	1	1	0	1	1	0
Age	47.84	47.37	48.39	49.17	49.01	49.36
	(10.61)	(10.58)	(10.62)	(10.14)	(10.18)	(10.08)
Obs.	72,946	39,446	33,500	47,383	25,587	21,796

Note: The table shows descriptive statistics for the three study populations. "Cases/1,000" denotes positive PCR tests per 1,000 until June 15, 2020. "...ex. health" means that healthcare and care workers are dropped (occupational codes 15, 22, 32, 53). "...pre cutoff" refers to cases before the specified cutoff dates referring to school closures. Cutoff dates are March 25 for teachers, and April 1 for parents and teachers' partners. "Healthcare/1,000" shows open care, inpatient care, and deaths related to COVID-19 per 1,000, reported until June 30. "Severe cases/1,000" shows only inpatient care and deaths related to COVID-19 per 1,000, reported until June 30. The number of deaths shows reported deaths before July 26 among those tested positive until June 15. Standard deviation for age is shown in parenthesis. The number of observations refers to the sample of individuals with a positive or no PCR test during the study period. Individuals with a positive PCR test with an invalid date are excluded.

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Appendix A Materials and methods

We construct estimation samples for parents, teachers, and their partners using registers held by Statistics Sweden. Through the Multi-Generation Register (MGR) per December 31, 2019, and Longitudinal integrated database for health insurance and labor market studies (LISA) per December 31, 2018, we identify all parents with children in relevant ages in their households. Children are assigned to school year, schools, and upper secondary programs using the Student Register as per October 15, 2019. We sample all parents in Sweden and their partners living in households with the youngest child in lower or upper secondary school. We also include parents with a biological or adopted child who do not live in the same household, but in the same region. The main analysis excludes parents born outside Sweden, the Nordic countries and the EU. Information on detailed place of residence as of December 31, 2019, is available for all individuals in Sweden in the Register of the Total Population (RTB). The sample of teachers includes all teachers working at the lower or upper secondary levels in the Teacher Register and refers to the status of the teacher in the fall of 2019. Their partners are identified using the household identifier in LISA. See the supporting information for further details on the estimation samples. Information on the covariates—disposable income, educational attainment, and occupation—are available in LISA. Occupations are reported according to the Swedish Standard Classification of Occupations (SSYK 2012), which is based on the international classification (ISCO-08). There are 46 occupation categories on the 2-digit level.

Information on positive PCR tests of SARS-CoV-2 is from the Swedish Public Health Agency. Up until late July there were 75,933 reported cases of SARS-CoV-2, out of which test dates are missing for 2,506 cases. As majority of the cases without test date are reported outside the main period of analysis, they are discarded. Personal identifiers are available for all cases, making it possible to link the test results to register data. Information on COVID-19 diagnoses until June 30 from the Inpatient-and Outpatient register is available from the National Board of Health and Welfare and on deaths from the Cause of Death register held by Statistic Sweden. By June 30, 2020, 33,596 individuals had been diagnosed with COVID-19 (ICD 10 codes U07.1 or U07.2) either in the Patient registers or the Cause of Death register.

Table 2 reports descriptive statistics for parents, teachers, and teachers' partners, starting with the incidence of positive PCR tests of SARS-CoV-2 as of June 15. Since healthcare workers were prioritized for testing, we also present the incidence excluding those working in healthcare. Healthcare workers are excluded by dropping those with occupational codes 15, 22, 32, and 53 (SSYK2012). The table further shows the incidence of positive PCR test prior to the cutoff date chosen to reflect the infection rate prior to the move to online instruction at the upper secondary level (March 25 for teachers, April 1 for parents and partners). The table next displays the incidence of COVID-19 diagnoses from healthcare visits and the incidence severe cases as of June 30. Finally, it displays the number of COVID-19-related deaths in each sample as of July 25 and the number of individuals in each group.

We use ordinary least squares (OLS) and logistic regressions (Logit) to empirically analyze if the SARS-CoV-2 infection can be attributed to being exposed to open or closed schools. We estimate the following OLS regression model for the three populations: parents, teachers, and teachers' partners:

$$y_i = \beta_0 + \beta_1 Open_i + \beta_2 y_{i,prior} + \mathbf{X}'\boldsymbol{\gamma} + \varepsilon_i$$

The outcome y_i is an indicator variable for a positive SARS-CoV-2 PCR test or being diagnosed with COVID-19 by a doctor in outpatient care or at a hospital. There is just one positive test per individual, and $y_i, prior$ is an indicator for SARS-Cov-2/COVID-19 before the cutoff date. Including $y_i, prior$ is a way of excluding pre-period cases without dropping such observations. $Open$ is an indicator variable taking the value one if individual i is exposed to (open) lower secondary schools. Parents with the youngest child in lower secondary school are defined as exposed, and parents with the youngest child in upper secondary school are defined as unexposed. Lower secondary teachers and their partners are defined as exposed and their upper secondary counterparts as unexposed. **X** is vector of individual and household characteristics. When estimating the model for teachers the vector includes: 20 indicators for age categories (30 and below, 31–35, 36–40, bi-annual until age 66, 67–69, 70–74, 75–79, 80+), sex, 7 indicators for categories of educational attainment, 46 indicators of categories of partners' occupation, 12 region indicators of country of origin for those not born in Sweden, log of household income, indicator of having a teaching position, percent of full time position, 290 indicators of municipality of residence and household exposure to the number of children in age groups 2–6, 7–16, 17–19, and 20+ who reside in the same region as the teacher. The municipality fixed effects are exchanged for 21 region fixed effects when estimating the logistic model. The equivalent vector of variables is used for teachers' partners, with the exception of own occupation instead of partner occupation. The vector of controls for parents include a similar set of variables as for teachers: age group categories, sex, municipality of residence, educational attainment, occupational categories (own and partners'), region of origin for those not born in Sweden (3 indicators in the main sample), the log of disposable family income, and indicators for missing data on any of these variables. Migrants from non-EU/Nordic countries are excluded from the main sample of parents. Standard errors are clustered at the school level when estimating the model for teachers and teachers' partners and at the household level when studying parents.

This project was approved by the Swedish Ethical Approval Board (Etikprövningsnämnden) on May 19, 2020 (decision number 2020-02323).

Appendix B Supplementary information (SI)

The pandemic's development and non-pharmaceutical interventions

The first case of SARS-CoV-2 in Sweden was reported on January 31, 2020, and the disease was classified as a danger to public health and to society on the following day (Public Health Agency of Sweden, 2020a). Among other things, this classification means that all documented cases of active infection have to be reported to the Public Health Agency. The first death from COVID-19 occurred on March 11. The daily number of deaths increased rapidly and peaked in the first half of April, whereafter the daily number of deaths declined gradually. By the end of the school year in mid-June, the 7-day average of daily deaths was around 30, and the cumulative number of deaths 5,140 (51 per 100,000 inhabitants).

The hardest hit region in both absolute and relative terms was the Stockholm region with 2.4 million inhabitants. Stockholm recorded 2,211 deaths (93 per 100,000) and 16,275 cases (685 per 100,000) by mid-June. In deaths per 100,000 inhabitants, Stockholm was followed by Sörmland (79), Västmanland (55), and Dalarna (52). The second largest region of Sweden, Västra Götaland, had by June 15 reported 649 deaths among its 1.7 million inhabitants. Testing scaled up faster in this region than in Stockholm, and the total number of cases was 11,000. The region of Skåne with 1.4 million inhabitants was less affected and reported 16 deaths per 100.000, and a total of 2,300 cases by mid-June.

The Swedish Public Health Agency introduced several measures to reduce the transmission of the virus (Public Health Agency of Sweden, 2020c). On March 10, a recommendation against unnecessary visits to care facilities was issued, and on March 11, public gatherings of more than 500 people were banned. On March 13, people were recommended to stay at home when having symptoms of illness, and those who could work from home were recommended to do so on March 17. On March 18, upper secondary schools and institutions of higher education moved to online instruction. On March 19, a recommendation against unnecessary travel was issued, and on March 24, restaurants and bars were instructed to increase the distance between costumers. Public gatherings above 50 persons were banned on March 27, and visits to elderly care facilities were banned the following day. On April 1, stricter recommendations on social distancing for the public were issued. On June 13, the recommendation against unnecessary travel was lifted. Throughout the period, there was no official recommendation that those without symptoms should stay at home, even if the household was shared with individuals with confirmed SARS-CoV-2 infection.

Mobility both within and between Swedish regions declined substantially as a response to the pandemic and the recommendations issued by the authorities (Public Health Agency of Sweden, 2020d). The distance individuals moved from their homes during a day was substantially reduced, and the decline in mobility was similar for residents in areas with different socioeconomic and demographic characteristics (visible minorities, highly educated, poor, and being 70 years or older) (Dahlberg et al., 2020).

Swedish schools during the pandemic

Compulsory schools (age 7–16) were kept open for instruction, and to reduce transmission the following precautionary measures were recommended (Public Health Agency of Sweden, 2020b): enhanced facilities for hand washing and disinfection; posters encouraging hand washing; increased distance in classrooms and dining halls, if possible; avoidance of large gatherings, as far as possible; minimize activities like open houses and parental meetings; increased outdoor activities, if possible; avoidance of close contacts between staff and students and between students; enhanced cleaning of heavily exposed areas and keyboards/tablets. Compared to school opening policies in other countries, the precautionary measures in Sweden are best described as mild (Guthrie et al., 2020). In particular, there is no mandated quarantine of those exposed who do not show symptoms, no imposed reductions of class size, and no recommendations concerning the use of face masks.

On March 18, upper secondary schools and institutions of higher education moved to online instruction. Upper secondary schools thus closed for normal instructions just as the number of deaths and ICU admissions began to increase (see Fig. 1 in the main text). Although upper secondary school moved to online teaching, some teachers were still teaching online from the school premises. According to a survey conducted by a large teachers' union during the last week of April and first week of May, 21 percent taught from the school, 46 percent partly from home, and 33 percent only from home (National Union of Teachers, 2020). As expected, compulsory school teachers mainly taught from school; 2 percent of the teachers in compulsory schools had been partly teaching online from home and 1 percent had only been teaching from home. There have also been media reports of substantial student absenteeism in compulsory schools. Again, there are no official reports, but according to the same survey, 18 percent of compulsory students were absent on a typical day. In a survey of 27 compulsory schools conducted by the National Board of Education during late April, 7 schools reported that absenteeism among compulsory school students was about normal, 13 that there was an increase in absenteeism of between 20 and 50 percent, and 7 stated an increase of more than 50 percent (Swedish National Agency for Education, 2020c). The conclusion drawn from this survey is that student absenteeism increased, but not dramatically so.

Data and sample restrictions

The sample of parents is constructed as follows. We define household adults who are exposed to their own children (biological or adopted) or a new partner's children from a previous relationship as parents. For separated parents, we use the household identifier in LISA to identify any current partner. This enables us to identify new couples who are either married or have common children. Households consisting of unmarried cohabitant couples without common children cannot be identified, and will be categorized as single households. The study population consists of parents who have children in school years 7–12 in the household, or biological children in these school years living in the same region. Because parents are less likely to interact regularly with children living at a distance, only children residing in the same region are considered in the analysis. There are 21 regions in Sweden, and they are thus relatively large geographical areas. There were also recommendations against leaving the region of residence during most of the spring 2020.

We sort parents by the age of the youngest child connected to the parents in the household or through biological links. Parents are considered exposed to lower secondary schools if their youngest child is enrolled in school years 7–9. Unexposed parents are defined by their youngest child being enrolled in upper secondary school. In the analysis, we focus on parents with the youngest child in school years 9 and 10, since they are likely to be the most similar in other aspects, except for parents with their youngest child in school year 9 being exposed to an open school. We further exclude those born outside of Sweden, the Nordics, and the EU. After this restriction, the main sample consists of 166,630 parents connected to school years 9 and 10. 480,291 parents are connected to school years 7 though 12.

The teacher sample consists of teachers working in lower and upper secondary schools according to the Teacher Register. Teachers with children born in 2019 are excluded, as they are likely to be on parental leave during the spring of 2020. We also exclude those recorded as being on leave of absence during the fall of 2019. The final sample consists of 72,946 lower and upper secondary teachers. In a descriptive analysis, we include lower and upper primary school teachers (school years 1–6) identified in the Teacher Register. When including these, the teacher sample consists of 137,213 individuals. For the sample of partners to lower and upper secondary teachers, we connect partners to teachers using the household identifier from LISA. This enables us to identify partners who are either married to or have common children with the teacher. The resulting sample consists of 47,383 partners.

Our main outcome variable is positive PCR tests reported to the Public Health Agency, but we also analyze the incidence of COVID-19 diagnoses from healthcare visits, and severe cases of COVID-19 (hospitalizations and deaths) reported to the National Board of Health and Welfare. The first case of SARS-CoV-2 in Sweden was reported on January 31, 2020, and the disease was classified as a danger to public health and to society on the following day (Public Health Agency of Sweden, 2020a). Among other things, this classification means that all documented cases of active infection have to be reported to the Public Health Agency. Testing capacity was slow to expand, and from March 13 (week 11), testing was directed towards healthcare employees and individuals with symptoms of COVID-19 in need of healthcare. As shown in Fig. B1, testing increased substantially from early June (week 23). Healthcare is the responsibility of Sweden's 21 healthcare regions, as is testing for SARS-CoV-2. Thus, there are regional differences in testing capacity, as well as rules and recommendation regarding testing. Some regions have recommended not to test children under 16 (for example Västra Götaland and Uppsala), and some have not had any age restrictions (for example Skåne). The number of detected cases does therefore not well reflect the actual rate of infections, and the rate of positive tests remained high throughout June (week 27). By June 15, a total of 383,000 PCR tests had been performed (3,800 per 100,000 inhabitants) (Public Health Agency of Sweden, 2020e).

Covariate balance

For estimation of the causal effect on parents, the identification strategy hinges on the similarity of parents with their youngest child in school years 9 and 10. Apart from a 1-year age difference, these groups should be balanced on covariates in order to be valid counterfactuals. We test this assumption by showing balancing tests, where we first use OLS to linearly predict the incidence of SARS-CoV-2 using the observable covariates (apart from age group effects) of parents with the youngest child in school years 7–12. Using this prediction as the dependent variable, we next run an OLS regression using only indicator variables for school year of the youngest child in the family (school year 10 is the reference category). Fig. B2 shows the estimates from this second regression for the main sample of parents. The corresponding balancing test, when non-EU migrants are included, is shown in Fig. B3. The specified regressions equations are shown below. The outcome variable is actual infections (regression [B1]) or predicted infections (regression [B2]), and $\mathbf{X}$ is a vector capturing spouse's occupation, missing information for spouse, educational level, municipality of residence, log disposable family income, zero income, region of origin of birth, and sex. $year_{i,g}$ are indicator variables capturing the school year of the youngest child in the household. Equation [B1] shows the regression equation used to estimate the predicted infections, which is subsequently used as the dependent variable in the balancing equation [B2].

$$y_i = \beta_0 + \mathbf{X}'\boldsymbol{\gamma} + \varepsilon_i \tag{B1}$$

$$y_{i,predict} = \beta_0 + \sum_{\substack{g=7 \\ g \neq 10}}^{12} year_{i,g} + \varepsilon_i \tag{B2}$$

In order to judge the importance of covariates, ORs without controlling for covariates are shown in Fig. B4. Panels B4a and B4b show ORs when only controlling for age group effects and panels B4c and B4d show ORs without any controls. ORs with covariates for the main sample of parents are shown in Fig. B5a, and Fig. B5b shows results when including all parents. The OLS estimates for both samples of parents, with all covariates and only age group effects and sex, are shown in Table B3. Table B3 also shows estimates for teachers and teachers' partners, with the full set of covariates and only age group effects and sex. Age groups are included since the parental sample is imbalanced on age by construction, and sex is included since there are more female teachers in lower secondary school. In the parental sample, which is roughly balanced on sex, the incidence of positive tests among women is 9.47 cases per 1,000 and among men 5.77 cases per 1,000. This difference may be due to educational, occupational, or potential sex differences in testing or prevalence of COVID-19.

Results including primary school teachers

We extend the population of teachers at open schools to include lower (school years 1–3) and upper (school years 4–6) primary school teachers. Results for confirmed PCR-tests and COVID-19 diagnoses, when controlling for covariates, are shown in Table B2.

Additional results and robustness tests

The propensity to get tested for SARS-CoV-2 could be affected by being connected to open and closed schools, regardless of health status. This is less of a concern for COVID-19 diagnoses made by the healthcare sector, especially severe cases which require hospital care or cause death. Results for severe cases, defined as admittance to hospital or death due to COVID-19, are presented for all groups in Table B1.

Some lower secondary schools spontaneously moved to online instruction and may thus be classified as having on-site instruction, when they in fact conducted the teaching online. No official records on such closures exist, but media searches reveal that they were rare and short-lived (see below). Privately managed independent lower secondary schools are over-represented in reports on proactive closures, and we therefore exclude such schools as a robustness test. Students attending independent schools are generally from a more advantaged socioeconomic background, and excluding them introduces imbalance to the sample of parents (Fig. B6). OLS estimates excluding independent lower secondary schools for parents are shown in Table B4. Corresponding results for teachers and their partners are shown in Table B5.

Upper secondary schools were allowed to let small groups of students complete practical elements of education and assignments, provided that this could be done safely (Swedish National Agency for Education, 2020b). Such practices may have been more common at vocational programs, and as a robustness test we exclude parents exposed to such upper secondary programs. This amounts to excluding parents of relatively disadvantaged socioeconomic background, which means that the exclusion introduces imbalance among parents (Fig. B6). OLS estimates imposing this exclusion are shown in Table B4.

The baseline specifications controls for the occupation of teachers' partners. As a robustness test, we instead drop the teachers and partners who are exposed to the healthcare sector through the partners' occupation (occupational codes 15, 22, 32 and 53). The results are shown in Table B5.

We use an alternative measure of exposure to lower secondary school for parents. Parents are then defined as exposed if they have a child in the household, or a child residing in the same region, in lower secondary school. Families with children too old to be in secondary school are dropped, as are families whose youngest child attends school below year 7. We control for having a child in school years 11 and 12, and the results presented in Table B4 thus shows the impact of being exposed to a child in lower secondary school, compared to being exposed to a child in upper secondary school year 10. Table B4 also shows results where we pool parents with the youngest child in school years 8–11 and 7–12.

Household size tends to decrease in student age, and Table B6 shows results for parents when controlling for this variable. Table B7 presents the sensitivity to using the cutoff dates March 25 and April 16 for parents, teachers, and teachers' partners.

Heterogeneity analysis

The expected impact of school closures on virus transmission depends mainly on the magnitude of contact reduction. Two factors that may be of importance for the effect is population density and how widely spread the virus was prior to schools closing. A study of U.S. districts show that transmission of SARS-CoV-2 increases with population density (Korevaar et al., 2020). To investigate this matter, we implement a heterogeneity analysis by district population density, categorizing districts with a population density above the 75th percentile as high density districts.

Timing has been shown to be important for the effectiveness of NPIs (S. Lai et al., 2020). We therefore investigate whether the impact of school closures depends on the level of virus transmission prior to school closure. Regions with above the populated weighted median spread of 12 cases per 100,000 are categorized as high spread regions, i.e. the regions (cases per 100,000 in parenthesis): Stockholm (20), Uppsala (16), Östergötland (16), Skåne (16), Sörmland (13), and Jönköping (12).

The econometric model is modified by adding interaction terms between indicators for high population density, respective high initial contagion, and exposure to lower secondary school, as well as interactions with all control variables except for the municipality indicators. The results are reported in Table B8.

Distribution of cases across schools

Although limited by the low testing rate, an illustration of the aggregation of cases across schools and over time can provide some evidence of the role of super-spreading events. To investigate whether there is substantial heterogeneity across schools, we aggregate cases across schools for parents and teachers, respectively. Cases among parents connected to a school through students in school years 7–12 are aggregated to the school level, which means that cases among parents to several children are connected to more than one school. When excluding schools with less than 50 connected parents, there are 1,455 lower and 1,149 upper secondary schools in the data. Among these schools, 25% of upper secondary and 32% of lower secondary schools had no cases. Since upper secondary schools on average are larger (397 connections compared to 312 for lower secondary schools), we mechanically expect more cases in upper secondary schools. Fig. B7 shows the fraction of total cases in lower respective upper secondary schools with one to 28 cases. For both types of schools, a majority of cases occurred at schools with few cases. To analyze how the cases are clustered over time, we aggregate the cases into episodes. If all cases within a school occur the same or adjacent week, it is coded as one episode. If cases are more dispersed over time, the school is coded as having more than one episode of infection outbreaks. Fig. B8 displays the fraction of schools with at least two cases in total that have one or more than one episodes. The pattern is similar for lower and secondary schools, with about 60% of the schools having one outbreak episode, and 40% having more than one episode.

We conduct the same analysis for teachers at schools with more than 5 teachers. As for the analysis of parents, there are no cases in a majority of schools (90% of lower secondary and 93% of upper secondary schools). Moreover, most cases are recorded in schools with only one case (Fig. B7). Among upper secondary schools, there are no schools with more than two cases, whereas among lower secondary schools there are some schools with three or more cases. The main analysis shows that keeping lower secondary schools open resulted in approximately 100 additional cases among lower secondary school teachers. According the patterns of distribution presented here, about a third of these can be found in schools with many cases, and two thirds in schools with only one case. Turning to the analysis of outbreak episodes, there is some indication of more clustering of outbreaks among teachers in lower secondary than upper secondary schools (Fig. B8).

Students

We show descriptive results of infection rates for students by school year in Table B9. Due to the discussed age restrictions for testing and risk of differing behavior for students over 18, we show results for students below age 18 in school years 7–10. As with parents and teachers, we control for observable characteristics such as sex, region of origin, and mother and father log disposable income, occupation, region of origin, education, missing values, and number of siblings in different age groups. We restrict attention to students with parents born within the EU and Nordics due to balancing of covariates concerns.

Media searches

In order to get information on spontaneous closures of lower secondary schools, media searches were conducted using the service Mediearkivet/Retriver and on Sveriges Radio's web page (public service radio with substantial local presence). Search terms were permutations of "school closure" (skolstängning/skola stängd), "distance education" (distansundervisning), "online education" (onlineundervisning), "corona", and "covid". Results for individual schools were followed by web searches to find more information on each particular case. Spontaneous closures were recorded as proactive if they did not occur as a result of cases detected at the school, and reactive otherwise. Provided that information is available, a closure is labelled as brief if the duration was less than a week.

In total, reports on 40 closures were found (27 among privately managed independent schools). 29 of these were proactive (22 among independent schools), while 11 were reactive (5 among independent schools). Spontaneous closures thus appear to have been rare, and independent schools are vastly over-represented among those that closed proactively. Two of the reactive closures were on advice from the local disease protection officer, and they both occurred late in the school year (June 6 and 8). Information on the duration was usually not available, but of the 18 reports from which the duration can be judged, 12 were brief. Several of the closures were also partial, meaning that school days were cut short, rolling schedules introduced, or that instruction partially moved online. Details on each specific report are available from the authors.

Cases, deaths and the case fatality rate

To extrapolate the expected effect of school closure on the number of deaths in Sweden, we derive the case fatality ratio (CFR) for different age groups. CFR is calculated by dividing the number of deaths with the number of cases, and hence crucially depend on the testing regime. Table B10 shows the incidence of detected SARS-CoV-2 in different age groups until June 15, 2020, and the number of deaths among these cases reported until July 25. The numbers are shown both including and excluding healthcare workers, for which testing was more accessible. The CFR increases with age, except for the higher value for the youngest age group due to one dead child. This child was younger than one years old, and thus not directly exposed to schools. The average age among teachers is 48, their partners 49, and parents 50 years old. Based on the CFR distribution in Table B10, we calculate the expected effect on mortality among lower secondary parents using a CFR of 1.1%.

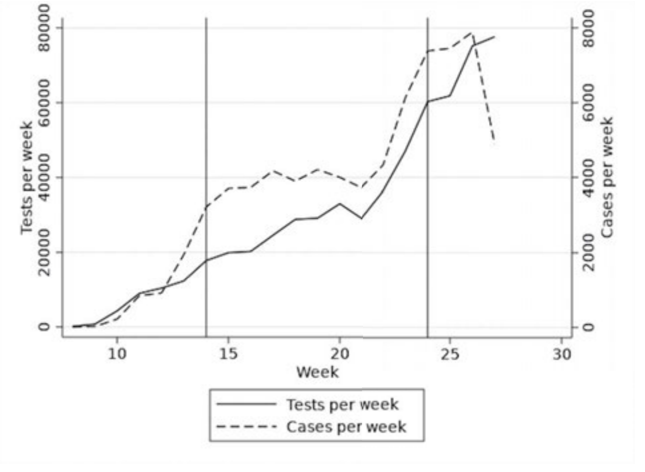


Figure B1. Tests and cases per week. Weekly number of PCR tests and positive cases. Vertical lines indicate weeks 14 and 24, the approximate period of analysis. Data from the Public Health Agency (Public Health Agency of Sweden, 2020e).

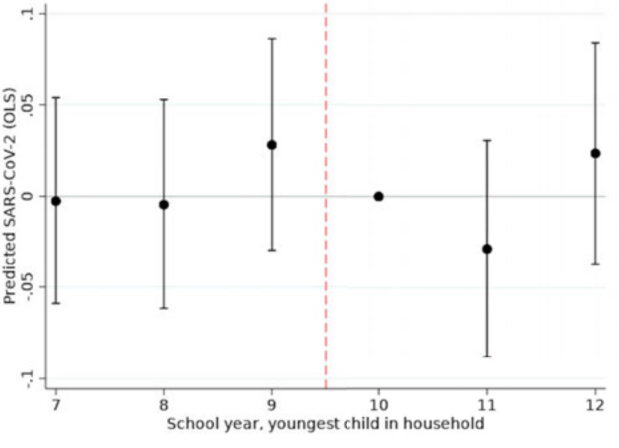


Figure B2. Covariate balance, main sample. Predicted SARS-CoV-2 regressed on school year of the youngest child in the household for parents born within EU and Nordics. Predicted outcome using sex, occupation, educational attainment, income, regions of residence and of origin for parents. The reference category is school year 10 and CI95 are indicated.

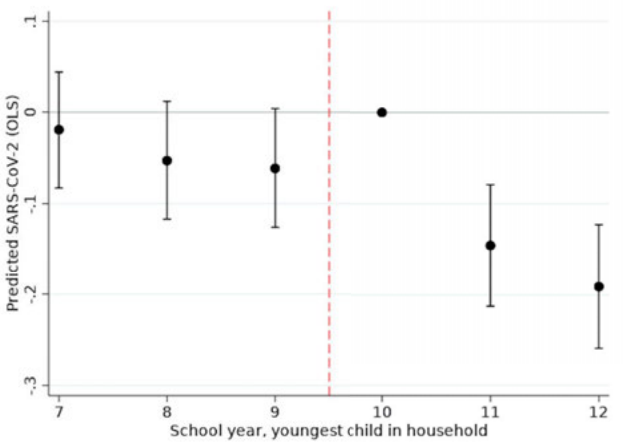
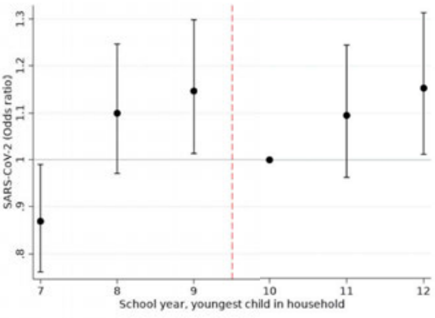
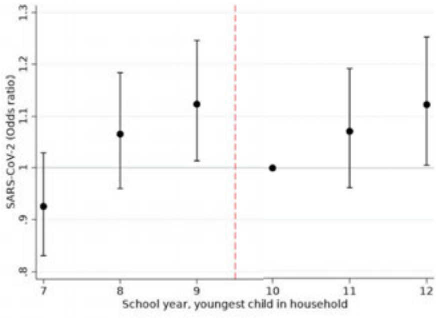


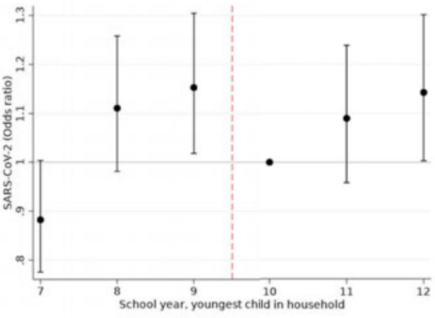
Figure B3. Covariate balance, all parents (including non-EU migrants). Predicted SARS-CoV-2 regressed on school year of the youngest child in the household for all parents. Predicted outcome using sex, occupation, educational attainment, income, regions of residence and of origin for parents. The reference category is school year 10 and CI95 are indicated.



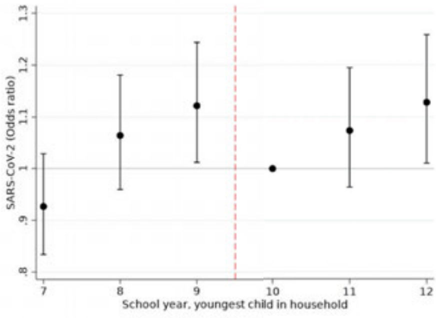
(a) Age group controls - Parents born within EU and the Nordics.



(b) Age group controls - All parents.

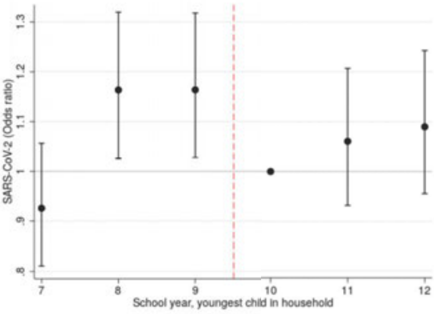


(c) Excl. all controls - Parents born within EU and the Nordics.

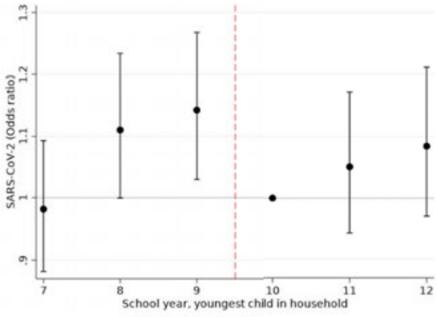


(d) Excl. all controls - All parents.

Figure B4. Results excluding covariates. SARS-CoV-2 odds ratios for parents by school year of the youngest child in the household excluding all control variables (except for age group effects in Fig. B4a and B4b and y_{prior}). Odds ratios estimated using logistic regression. The reference category is school year 10 and CI95 are indicated. Fig. B4a and Fig. B4c show outcomes for parents born within the EU and the Nordics, which is our main study population. Fig. B4b and Fig. B4d show outcomes including all parents.

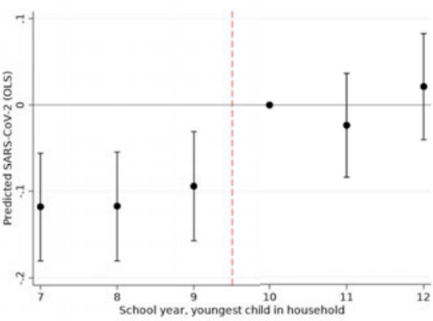


(a) Parents born within EU and the Nordics.

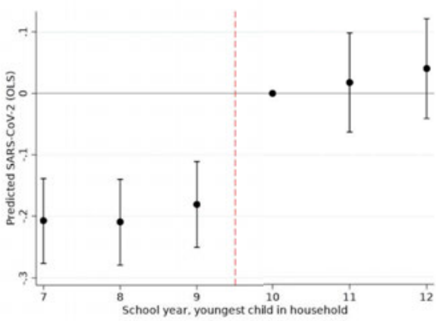


(b) All parents.

Figure B5. Results including covariates. SARS-CoV-2 odds ratios for parents by school year of the youngest child in the household. Odds ratios estimated using logistic regression. The reference category is school year 10 and CI95 are indicated. Fig. B5a shows outcomes including parents born within the EU and the Nordics, which is our main study population. Fig. B5b shows outcomes including all parents.



(a) Predicted outcome - Excl. private indep. schools.



(b) Predicted outcome - Excl. vocational program links.

Figure B6. Covariate balance for subsamples. Predicted SARS-CoV-2 regressed on school year of the youngest child in the household for parents born within EU and the Nordics, excluding private independent schools and vocational program links separately. Predicted outcome using sex, occupation, educational attainment, income, regions of residence and of origin for parents. The reference category is school year 10 and CI95 are indicated. Fig. B6a shows outcomes excluding private independent school links. Fig. B6b excludes vocational program links.

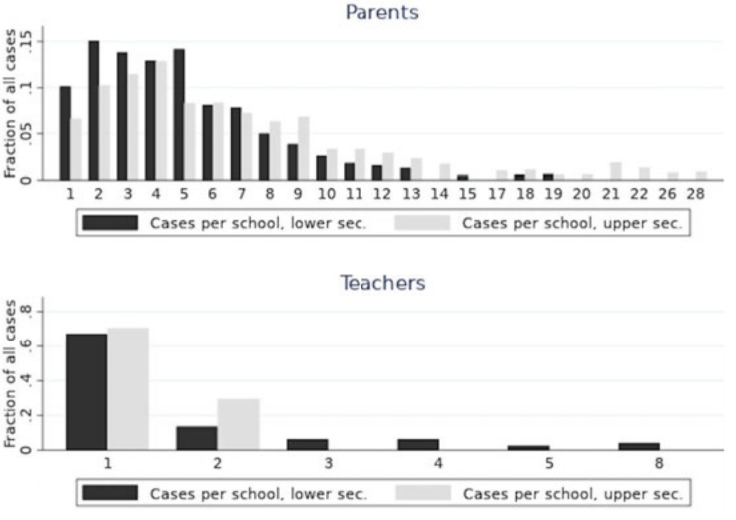


Figure B7. Distribution of cases across schools. The figure shows the fraction of total cases at schools with 1 to 28 cases.

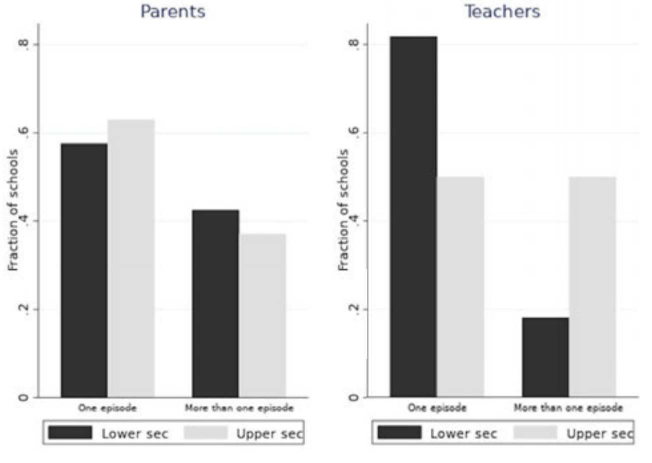


Figure B8. Episodes of cases within schools. The figure shows the fraction of schools with at least two cases which had all cases in one week or adjacent weeks and the fraction of schools with cases at least one week apart.

Table B1. *Impact of exposure to open schools on PCR tests and severe COVID-19 diagnoses.*

	Parents		Teachers		Teachers' partners	
	OLS (cases/1,000)					
	PCR	Severe cases	PCR	Severe cases	PCR	Severe cases
Open school	1.05**	-0.21	2.81***	0.84***	1.47**	0.08
	(0.43)	(0.18)	(0.59)	(0.28)	(0.71)	(0.31)
Mean dep. var.	6.37	1.40	2.96	0.96	5.10	1.01
Obs.	166,630	166,719	72,946	72,976	47,383	47,413
	Logit (odds ratios)					
	PCR	Severe cases	PCR	Severe cases	PCR	Severe cases
Open school	1.17**	0.84	2.01***	2.15***	1.29*	1.09
	[1.03,1.32]	[0.64,1.11]	[1.52,2.67]	[1.41,3.29]	[1.00,1.67]	[0.62,1.92]
Obs.	163,195	150,571	70,151	62,249	44,025	34,563

Note: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Standard errors in parenthesis are clustered the at the household level for parents and school level for teachers and partners. "Open school" indicates exposure to lower secondary schools. Severe cases include COVID-diagnoses registered at hospital or as death. The effects are estimated using linear probability models (OLS) and logistic regressions (Logit).

Table B2. *SARS-CoV-2 among lower primary, upper primary, and lower secondary teachers relative to upper secondary teachers (OLS).*

	PCR	Healthcare
Lower primary	1.66***	0.70**
	(0.53)	(0.34)
Upper primary	2.19***	1.24***
	(0.54)	(0.37)
Lower secondary	2.85***	1.44***
	(0.59)	(0.35)
Mean dep. var.	2.96	1.61
Obs.	137,213	137,272

Note: ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$. Standard errors in parenthesis are clustered at the school level for teachers. Upper secondary teachers are used as the reference category. All covariates included. The results are estimated using linear probability models (OLS).

Table B3. *Main results for parents, teachers & partners - when including and excluding controls. Outcome: Positive PCR tests per 1,000.*

	Parents (main)		Parents (all)		Teachers		Partners	
	Controls	Excl. controls	Controls	Excl. controls	Controls	Excl. controls	Controls	Excl. controls
Open school	1.05**	1.01**	1.09***	1.02**	2.81***	2.94***	1.47**	1.58**
	(0.43)	(0.43)	(0.42)	(0.42)	(0.59)	(0.58)	(0.71)	(0.71)
Mean dep. var.	6.37	6.37	7.58	7.58	2.96	2.96	5.10	5.10
Obs.	166,630	166,630	205,843	205,843	72,946	72,946	47,383	47,383

Note: ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$. Standard errors in parenthesis are clustered at the household level for parents and school level for teachers and partners. "Open school" is defined as exposure to lower secondary school. "Excl. controls" indicates a regression without covariates except for age group effects and sex. The results are estimated using linear probability models (OLS).

Table B4. *Robustness checks for parents. Outcome: Positive PCR tests per 1,000.*

	No indep.	No voc.	Alt. exposure	Pooling 7–12	Pooling 8–11
Open school	1.33***	0.64			
	(0.46)	(0.53)			
Open school*			0.98***	0.20	0.79**
			(0.34)	(0.26)	(0.31)
Mean dep. var	7.15	7.50	6.73	7.56	7.31
Obs.	150,326	124,527	327,209	480,291	322,446

Note: ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$. Standard errors in parenthesis are clustered at the household level. "Open school" is defined as having the youngest child in school year 9 relative to school year 10. "Open school*" is an indicator for living in a household with a child in lower secondary school (see Appendix B for details on sample restriction). In "Alt. exposure" we an alternative measure of exposure, and control for having a child in year 11 or 12. The results are estimated using linear probability models (OLS).

Table B5. *Robustness checks for teachers and teachers' partners. Outcome: Positive PCR tests per 1,000.*

	Teachers		Teachers' partners	
	Excluding independent schools	Excluding partner in healthcare	Excluding independent schools	Excluding partner in healthcare
Open school	2.63***	2.76***	1.64**	1.57**
	(0.63)	(0.59)	(0.77)	(0.64)
Mean dep. var.	2.96	2.83	5.10	2.78
Obs.	65,119	66,828	42,656	41,363

Note: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Standard errors in parenthesis are clustered the at the school level. "Open school" is defined as being a teacher or a teachers' partner at the lower secondary level. The results are estimated using linear probability models (OLS).

Table B6. *Parents - Controlling for household size.*

	OLS		Logit	
	PCR	Healthcare	PCR	Healthcare
Open school	1.04**	-0.18	1.17**	0.93
	(0.43)	(0.26)	(0.07)	(0.09)
Mean dep. var.	6.37	2.74		
Obs.	166,630	166,719	163,195	163,155

Note: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Standard errors in parenthesis are clustered the at the household level. "Open school" is defined as having the youngest child in school year 9 relative to school year 10. The results are estimated using linear probability models (OLS) and logistic regressions (Logit).

Table B7. *Robustness checks - Different cutoff dates for the pre-period. Outcome: Positive PCR tests per 1,000.*

	Parents		Teachers		Teachers' partners	
	OLS (cases/1,000)					
	March 25	April 16	March 25	April 16	March 25	April 16
Open school	1.16***	0.87**	2.81***	2.44***	1.43**	1.37**
	(0.43)	(0.40)	(0.59)	(0.55)	(0.73)	(0.69)
Mean dep. var.	6.54	5.74	2.96	2.66	5.32	4.59
Obs.	166,630	166,630	72,946	72,946	47,383	47,383
	Logit (odds ratios)					
	March 25	April 16	March 25	April 16	March 25	April 16
Open school	1.18***	1.16**	2.01***	1.96***	1.27*	1.30*
	[1.04,1.33]	[1.01,1.32]	[1.52,2.67]	[1.46,2.64]	[0.99,1.64]	[0.99,1.71]
Obs.	163,233	162,491	70,151	69,732	44,035	42,948

Note: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Standard errors in parenthesis are clustered at the school level for teachers and their partners, at the household level for parents. "March 25" refers to moving the start of the investigation period to that date. Similarly, "April 16" moves the date to April 16. "Open school" is defined as having the youngest child in school year 9 relative to school year 10. The results are estimated using linear probability models (OLS) and logistic regressions (Logit).

Table B8. *Heterogeneous treatment for teachers, teachers' partners, and parents. Outcome: Positive PCR tests per 1,000.*

	Parents		Teachers		Teachers' partners	
Open school	1.07**	0.72	3.05***	2.86***	1.83**	1.03
	(0.46)	(0.63)	(0.68)	(0.89)	(0.77)	(1.01)
Densely pop. district × Open school	0.09		-0.94		-1.50	
	(1.17)		(1.28)		(1.92)	
High pre-closure spread × Open school		0.71		-0.07		1.00
		(0.85)		(1.17)		(1.44)
Mean dep. var.	6.37	6.37	2.96	2.96	5.10	5.10
Obs.	166,425	166,425	72,942	72,946	47,383	47,383

Note: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Standard errors in parenthesis are clustered at the school level for teachers and their partners, at the household level for parents. "Open school" is defined as having the youngest child in school year 9 relative to school year 10. Densely populated districts are above the 75th percentile in the distribution of population density. High pre-closure spread is defined as above 12 detected cases per 100,000 inhabitants (Stockholm, Uppsala, Östergötland, Skåne, Sörmland, and Jönkoping). The results are estimated using linear probability models (OLS).

Table B9. *Students under age 18. Outcome: Positive PCR tests per 1,000.*

	OLS (cases/1,000)	Logit (odds ratios)
School year 7	-0.08	0.86
	(0.13)	[0.51,1.46]
School year 8	-0.17	0.70
	(0.13)	[0.40,1.22]
School year 9	-0.07	0.89
	(0.13)	[0.52,1.52]
Mean dep. var.	0.53	
Obs.	224,450	154,459

Note: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Standard errors in parenthesis are clustered at the school level. CI95 in brackets are shown for the odds ratios."School year ..." is in relation to school year 10 (reference category). The results are estimated using linear probability models (OLS) and logistic regressions (odds ratios).

Table B10. *COVID-19 cases, patients and deaths by age group.*

Age group	Cases	Cases ex. health	Deaths	Deaths ex. health	CFR (%)	CFR ex. health	# patients with diagnosis	# patients in hospital
0–6	152	152	1	1	0.66	0.66	132	67
7–16	457	457	0	0	0.00	0.00	230	94
17–19	614	611	0	0	0.00	0.00	230	84
20–29	5,730	3,204	7	7	0.12	0.22	2,114	784
30–39	7,396	3,544	13	11	0.18	0.31	3,185	1,456
40–49	8,586	4,262	40	36	0.47	0.84	3,997	1,930
50–59	9,978	5241	134	122	1.34	2.33	5275	3216
60–69	6,463	4,113	346	336	5.35	8.17	4,666	3,461
70–79	4,792	4,671	1,112	1,102	23.21	23.59	4,756	3,902
80–	9,314	9,301	3,749	3,741	40.25	40.22	6,050	5,151
Total	53,482	35,556	5,402	5,356	10.10	15.06	30,,635	21,045

Note: Test date until June 15, 2020. Deaths reported until July 25 for cases tested until June 15. "ex. health" means that healthcare and care workers are dropped (occupational codes 15, 22, 32, 53). CFR refers to the implied Case Fatality Rate.

Table B11. *Occupations ranked by incidence of positive PCR-tests (lowest to highest incidence).*

Rank	Occupation title (SSYK3)	Cases/1,000	Occ. size
1	Specialists within environmental and health protection	1.15	8,690
2	Mixed crop and animal breeders	1.15	7,807
3	Animal breeders and keepers	1.37	15,315
4	Museum curators and librarians and related professionals	1.56	10,931
5	Architects and surveyors	1.72	12,218
6	Mathematicians, actuaries and statisticians	1.76	2,267
7	ICT architects, systems analysts and test managers	1.93	127,722
8	Electronics and telecommunications installers and repairers	1.96	10,711
9	Library and filing clerks	1.98	3,533
10	Biologists, pharmacologists and specialists in agriculture and forestry	2.00	7,006
11	Sheet and structural metal workers, moulders and welders, and related workers	2.08	25,959
12	University and higher education teachers	2.08	37,457
13	Designers	2.11	16,579
14	Wood processing and papermaking plant operators	2.19	15,986
15	Ships' deck crews and related workers	2.22	1,350
16	Engineering professionals	2.26	95,496
17	Market gardeners and crop growers	2.27	23,319
18	Other service related workers	2.31	3,892
19	Marketing and public relations professionals	2.36	39,061
20	Metal processing and finishing plant operators	2.37	16,441
21	Carpenters, bricklayers and construction workers	2.44	106,364
22	Mobile plant operators	2.46	35,715
23	Financial and accounting associate professionals	2.49	57,856
24	Tax and related government associate professionals	2.50	44,063
25	Recycling collectors	2.55	8,634
26	Painters, Lacquerers, Chimney-sweepers and related trades workers	2.57	26,114
27	Research and development managers	2.58	6,192
28	Construction labourers	2.59	6,948
29	Electrical equipment installers and repairers	2.60	38,434
30	ICT operations and user support technicians	2.61	44,879
31	Berry pickers and planters	2.63	3,045
32	Physicists and chemists	2.63	6,462
33	Forestry and related workers	2.64	5,690
34	Creative and performing artists	2.64	12,491
35	Accountants, financial analysts and fund managers	2.65	49,484
36	Culinary associate professionals	2.72	4,046
37	Broadcasting and audio-visual technicians	2.76	4,350
38	Other stationary plant and machine operators	2.76	7,237
39	Physical and engineering science technicians	2.77	104,914
40	Information, communication and public relations managers	2.78	4,313
41	Legal professionals	2.80	22,498
42	Roofers, floor layers, plumbers and pipefitters	2.90	35,526
43	Commissioned armed forces officers	2.91	1,032
44	Postmen and postal facility workers	2.91	15,781
45	Precision-instrument makers and handicraft workers	2.94	4,762
46	Client information clerks	2.96	60,517
47	Production managers in manufacturing	2.98	16,439
48	Financial and insurance managers	3.00	4,995
49	Printing trades workers	3.05	7,542
50	Insurance advisers, sales and purchasing agents	3.06	132,527
...	...	...	...

51	Machine operators, textile, fur and leather products	3.07	5,538
52	Ship and aircraft controllers and technicians	3.07	5,535
53	Real estate and head of administration manager	3.08	3,898
54	Event seller and telemarketers	3.09	9,073
55	Armed forces occupations, other ranks	3.12	5,448
56	Information and communications technology service managers	3.22	11,185
57	Blacksmiths, toolmakers and related trades workers	3.22	49,318
58	Machinery mechanics and fitters	3.23	59,063
59	Upper secondary school teachers	3.24	32,130
60	Sports, leisure and wellness managers	3.26	1,533
61	Shop staff	3.27	194,098
62	Waiters and bartenders	3.28	21,963
63	Dockers and ground personnel	3.29	9,415
64	Production managers in construction and mining	3.30	17,554
65	Finance managers	3.36	17,544
66	Train operators and related workers	3.38	5,626
67	Supply, logistics and transport managers	3.39	11,223
68	Administrative and specialized secretaries	3.43	17,491
69	Wood treaters, cabinet-makers and related trades workers	3.46	11,287
70	Stores and transport clerks	3.54	93,586
71	Administration and planning managers	3.54	10,438
72	Sales and marketing managers	3.57	30,809
73	Photographers, interior decorators and entertainers	3.59	9,198
74	Vocational education teachers	3.64	9,888
75	Authors, journalists and linguists	3.67	16,615
76	Machine operators, rubber, plastic and paper products	3.67	13,342
77	Business services agents	3.69	32,825
78	Process control technicians	3.69	18,686
79	Elected representatives	3.74	1,070
80	Machine operators, food and related products	3.77	14,866
81	Organisation analysts, policy administrators and human resource specialists	3.79	112,865
82	Lower primary school teachers	3.81	31,992
83	Assemblers	3.88	55,141
84	Cashiers and related clerks	3.97	11,339
85	Athletes, fitness instructors and recreational workers	3.98	26,376
86	Manufacturing labourers	4.05	10,383
87	Hotel and conference managers	4.05	1,483
88	Construction and manufacturing supervisors	4.05	24,431
89	Other services managers not elsewhere classified	4.08	7,103
90	Mining and mineral processing plant operators	4.14	7,980
91	Butchers, bakers and food processors	4.14	8,215
92	Office assistants and other secretaries	4.16	168,407
93	Architectural and engineering managers	4.18	11,232
94	Croupiers, debt collectors and related workers	4.22	2,132
95	Human resource managers	4.27	8,663
96	Preschool managers	4.28	4,677
97	Retail and wholesale trade managers	4.46	10,304
98	Cooks and cold-buffet managers	4.57	40,728
99	Heavy truck and bus drivers	4.61	75,333
100	Administration and service managers not elsewhere classified	4.70	23,612
...	...	...	...

101	Childcare workers and teachers aides	4.70	124,777
102	Managing directors and chief executives	4.71	20,381
103	Teaching professionals not elsewhere classified	4.75	36,664
104	Upper primary school teachers	4.82	29,850
105	Tailors, upholsterers and leather craftsmen	4.85	3,298
106	Primary and secondary schools and adult education managers	4.93	10,557
107	Building caretakers and related workers	4.93	47,028
108	Cabin crew, guides and related workers	4.96	8,469
109	Religious professionals and deacons	4.98	3,615
110	Fast-food workers, food preparation assistants	5.04	71,276
111	Newspaper distributors, janitors and other service workers	5.12	41,773
112	Cleaners and helpers	5.16	85,416
113	Other surveillance and security workers	5.46	36,460
114	Washers, window cleaners and other cleaning workers	5.49	7,835
115	Legislators and senior officials	5.49	2,915
116	Hairdressers, beauty and body therapists	5.53	21,344
117	Restaurant managers	5.64	8,332
118	Driving instructors and other instructors	5.8	7,409
119	Lower secondary school teachers	5.83	37,894
120	Social work and counselling professionals	6.69	46,161
121	Machine operators, chemical and pharmaceutical products	6.9	5,072
122	Police officers	8.08	16,219
123	Social work and religious associate professionals	8.6	22,084
124	Education managers not elsewhere classified	8.85	1,243
125	Car, van and motorcycle drivers	9.03	19,594

Note: Incidence (cases per 1,000) of detected SARS-CoV-2 by 3-digit occupational codes (SSYK2012) until June 15, 2020. Ages included are 25–65, and only occupations with at least 1,000 employees are reported. Healthcare occupations are excluded from the ranking. Teachers at different levels are identified using the Teacher Register and not by using SSYK codes 233 (compulsory school teachers) and 234 (upper secondary school teachers).

Essay IV. Economic crisis and the career choices of the next generation of workers

Co-authored with Julien Grenet, Hans Grönqvist, Martin Nybom, and Jan Stuhler

Acknowledgements: We are grateful for comments by Peter Nilsson and the Brown Bag seminar participants at the Uppsala University Department of Economics.

1 Introduction

Economic crises are massive shocks to the economy that affect workers and their families regardless of whether they face unemployment.[1] A common feature of these relatively frequent events is that they often are more heavily concentrated in different industries and geographic areas. For instance, while the Great Recession hit many Americans hard, its impact did not spread equally across the U.S. The employment losses were relatively more severe in the construction and manufacturing sectors, and states like Florida, Arizona, Nevada, and much of California were hit harder (e.g. Hoynes et al., 2012). While there is plenty of evidence documenting immediate and intermediate adverse effects of economic crisis on both labor demand and labor supply (e.g. Chodorow-Reich, 2014; Huber, 2018; Yagan, 2019; Lachowska et al., 2020), there is less evidence of how young individuals respond to economic crisis, and how these responses affect the long-run development of labor markets. Economic crises may, according to theory, permanently affect the composition of the labor force by accelerating structural change (Howes, 2021), or altering career trajectories by changing perceived employment opportunities and economic preferences (e.g. Giuliano and Spilimbergo, 2014).[2] While recent evidence indeed shows that labor market polarization accelerates in recessions and leads to jobless recoveries (e.g. Autor, 2010; Jaimovich and Siu, 2020), potentially due to an increased skill mismatch (Zago, 2020), the exact mechanisms producing these effects are still not well understood.

In this paper, we estimate how economic crisis affects the early career choices of the next generation of workers. We do so by studying Swedish compulsory school students about to select into high school educational programs as the crisis hit. These programs, which closely map into industries and occupations through job-specific apprenticeship programs and occupational licensing, are also fundamental predictors of long-run educational and labor market outcomes.[3] The context of our study is the the unexpected and massive economic crisis in Sweden during the early 1990s. The crisis, among the five most severe financial crises in history (Reinhart and Rogoff, 2008), involved a sudden tightening of lending standards as well as sharp increases in the cost of external finance. The ensuing recession, which implied a five-fold increase in the aggregate unemployment rate, terminated a period of overheated labor

[1] Effects on those who remain employed could for instance operate through reduced wages either due to less rent-sharing or because few vacancies makes it less optimal for firms to pay efficiency wages. Alternatively, housing prices, tax revenues, and school spending may be lower during severe economic crises.

[2] For instance, during the Great Recession, adolescents reported more collectivist attitudes, increased support for government redistribution, and increased belief in luck versus work in determining success (e.g. Giuliano and Spilimbergo, 2014).

[3] For instance, 53.6 percent of the students born in 1980 who graduated from the high school construction program were working in the construction sector at age 35.

markets and marked the onset of a period of persistently higher levels of unemployment (Englund, 1999).

Economic crises typically coincide with other macroeconomic changes in society, which makes separating crisis exposure from confounding factors a difficult task. We are able to sidestep many of the methodological problems by focusing on within-cohort difference in exposure to the crisis based on the sector of employment for the father. Similarly to the Great Recession, the 1990s crisis in Sweden disproportionally affected some blue-collar sectors, such as the manufacturing and construction sector, while leaving most white-collar sectors and some other blue-collar sectors more unscathed.[4] Many of the jobs in the heavily-hit sectors were lost permanently. We use this asymmetry to estimate the effects of crisis exposure to the father's sector of employment in influencing the high school program choices of the affected students.

The fact that high school program choices are made at a single, specific point in time the year the students turn age 16 also helps us to disentangle crisis exposure from correlated unobservables. Specifically, we use the timing of paternal job loss from the heavily-hit sectors to get variation in crisis exposure based on age of the student when experiencing the job loss. We postulate that experiencing paternal job loss early on, at ages before the high school program choice has been made, will have a detrimental effect on selecting into a program linked to the paternal sector of employment. This comes as a consequence of paternal job loss making the short- and long-term gains and prospects of working in the affected sectors diminish and become more salient. We base this on the idea that experiencing adverse economic shocks at the family level can convey additional information about the uncertainty and risks in different sectors.[5]

Our analysis draws on administrative data covering the entire Swedish population aged 16 and above during the period 1985–2017, as well as the full population every five years from the censuses 1960–1990. The data span various registers that are connected through anonymized identification codes at the personal, family, and firm level. Using the data, we are able to link the universe of Swedish parents and children from 1932–2014. The sample consists of roughly 232,500 Swedish children born 1970–1988, who experience paternal job loss during or just after the 1990s crisis. The main analysis focuses on the 175,000 compulsory school students who are on the verge of making, or have just made, their high school program choices at age 16 as the economic crisis hit Sweden.

Our descriptive findings indicate that the economic crisis affected the educational choice of students by deterring them from completing high school

[4] See Figure 11 for a graphical illustration of the employment rate by sector before, during, and after the 1990s crisis in Sweden.

[5] Past work shows that risk-averse individuals tend to make less risky career choices (e.g. Della Vigna and Paserman, 2005; Bonin et al., 2007; Argaw et al., 2017).

programs linked to the heavily-hit manufacturing and construction sectors.[6] We then proceed with the causal analysis. We show that experiencing paternal job loss deters the students from entering the same sector of employment, with the clearest effects found for students with fathers experiencing job loss in the manufacturing sector. Specifically, we show that experiencing paternal job loss from the heavily-affected sectors before age 16 has a detrimental effect on school choice for the programs linked to heavily-affected sectors, relative to experiencing paternal job loss after age 16. The students affected by early paternal job loss during the economic crisis also exhibit higher lifetime earnings, are more likely to be employed later in life, and steer clear of the crisis sectors in the very long run. The results are robust to placebo checks of experiencing later paternal job loss, experiencing paternal job loss from any other sector, and adding sibling fixed effects to the specifications. Our findings indicate that economic crisis can cause substantial behavioral responses related to early career choices, and that information advantages along with educational choice flexibility can be key in parrying the adverse effects of economic downturn.

Our results connect to several strands of the literature. Most importantly, we relate to studies on the effects of graduating in a recession. Several studies show that adverse conditions at labor market entry negatively affects short-term wages for high school students, but that the effects fade after a few years (Genda et al., 2010; Hershbein, 2012; Kawaguchi and Murao, 2014; Schwandt and Von Wachter, 2019; Engdahl et al., 2022).[7] College students graduating during a recession suffer a modest reduction in employment, but a larger and longer lasting earnings loss (Oreopoulos et al., 2012; Altonji et al., 2016). Liu et al., 2016 show that the negative effects on earnings is partially explained by recessions inducing a mismatch between the skills supplied by college graduates and skills demanded by the labor market. While these studies focus on the net effect of macroeconomic shocks at the cohort level, our research design relying on within-cohort variation in crisis exposure allows us to separately study the role of information advantages about the uncertainty and risks in different sectors. Moreover, we provide novel evidence of how incumbent students change their behavior in response to economic crisis. While theory suggests that workers may respond to recessions by moving away from exposed sectors, such behavioral adjustments are difficult to examine.[8] The most important obstacle is that worker mobility is a slowly moving process that typically involve high opportunity costs connected to switching. By studying the educational choices of students about to select their high school program, we

[6]For instance, the share of students with a manufacturing-linked high school program at age 20 declines by more than 50% during a 10-year period around the time of the crisis.

[7]Raaum and Røed, 2006 also document negative effects on employment.

[8]A recent example of this is the study by Aalto et al., 2023, which investigates high school program choices in response to the COVID-19 pandemic and find evidence of students adjusting their program choices in response to the crisis.

can more easily identify immediate behavioral responses in an environment in which there are no switching costs.

We also add to the literature on the changing composition of the labor market over the business cycle. While there has been a long-standing interest in the contemporaneous consequences of natural business cycle fluctuations and economic crises,[9] one of the most fundamental long-run developments in the labor market has been the job polarization, whereby new technologies substitute for middle-skill jobs and are in turn complementary to high-skill jobs (e.g. Acemoglu and Autor, 2011). There is now direct evidence showing that the Great Recession accelerated firm-level adoption of technologies that replaced routine labor (Hershbein and Kahn, 2018). Our results contribute to this literature by showing that economic crises may have long-lasting effects on the composition of the labor force by making the next generation of workers shy away from the most strongly affected sectors when making their early career choices.

Our results also feed into a large literature on the effects of job displacement on individuals and their children. Displaced workers are found to suffer persistent and substantial earnings losses (Jacobson et al., 1993), and job loss from routine occupations during mass layoffs have been shown to be more persistent and severe than job loss for other occupations (Blien et al., 2021). Technological progress and structural change on the labor market are the prime candidates in explaining this. Displaced workers often resort to switching occupations resulting in skill mismatch, which leads to persistent drops in earnings following displacement (Nedelkoska et al., 2015). Previous research has shown that displacement of the father following plant closure leads to significantly lower earnings for the affected sons (Oreopoulos et al., 2008). There is also evidence of children with displaced fathers in manufacturing performing worse in school, and exhibiting lower earnings early on in the career (Gregg et al., 2012).[10]. In contrast to this literature, we provide evidence of the under-

[9]For instance, recent firm-level studies combine micro data with novel research designs to estimate the effects of the Great Recession on labor demand (e.g. Chodorow-Reich, 2014; Mian and Sufi, 2014; Giroud and Mueller, 2015; Bentolila et al., 2018; Huber, 2018; Greenstone et al., 2020). Although the evidence is slightly mixed, most of these studies conclude that economic crisis adversely affects labor demand and that financial constraints is a key mechanisms producing these effects. A few studies also provide worker-level evidence. Lachowska et al., 2020 use matched employer-employee data from administrative wage and unemployment insurance records in Washington State to estimate the earnings losses of workers displaced during the Great Recession. Five years after job loss, the earnings of displaced workers were 16 percent lower than those of non-displaced workers. The earnings losses are explained by a combination of reduced working hours and lost employer-specific premiums. Yagan, 2019 leverages regional variation in Great Recession severity and show that a one percentage point increase in the local unemployment rate during the downturn reduces individual employment propensity seven years later by 0.3 percentage points and also lower earnings. These studies mainly estimate immediate and intermediate effects on employment related outcomes.

[10]Parental job loss has also been shown to affect other outcomes such as school grades, income and mental health (e.g. Coelli, 2011; Rege et al., 2011).

lying behavioral adjustments of children facing parental job loss during large economic recessions.

More loosely, the empirical approach of this paper is connected to intergenerational mobility research. It is well known that occupations commonly are been passed down from parents to their children, sometimes over multiple generations (Adermon et al., 2021).[11] This parent-child coupling has likely contributed to the intergenerational persistence in labor market outcomes, and understanding the mechanisms behind this is of first order importance to learn more about the family environment's role in affecting social mobility.[12] Importantly, there are signs that this labor market coupling has weakened during the past century (e.g. Emran and Sun, 1988; Jarvis and Song, 2017; Modalsli, 2017).[13] Structural demand changes on the labor market, such as the decline of the agricultural sector, the later decline of the manufacturing sector in many Western countries, and the expansion of education opportunities are candidates that could help explain this trend.[14] While structural change tends to be gradual and slow, the role of economic crisis in accelerating the decoupling is largely unexplored. The only study we are aware of that examines the intergenerational consequences of economic crisis is by Feigenbaum, 2015, who shows that exposure to the Great Depression reduces earnings mobility in the United States. The focus of that study is, however, not on the early career choices of young workers.

The following structure outlines the paper: Section 2 presents a background to the institutional context, the educational system in Sweden, and the 1990s economic crisis. Section 3 outlines the data sources and empirical strategy. Section 4 presents estimation results, and Section 5 discusses the findings. Finally, Section 6 concludes.

[11] There are several possible reasons for this: inherited genetic predispositions to the specific tasks, lower barriers to entry, learning the trade directly from the parent, but also informational advantages related to the short and long-term prospects of the occupation learned from the parent.

[12] The previous literature on intergenerational occupational mobility has documented a strong link between occupational mobility and earnings mobility (Bachmann et al., 2020).

[13] In Figure 10, we estimate the probability of sons age 30 working in the same sector as their fathers around age 50 by birth cohort. This figure indicates that the intergenerational occupational mobility (persistence) has been increasing (decreasing) from the cohort born from 1932 in Sweden, with the trend accelerating for the cohorts who were young and about to enter the labor market (around age 20) when the 90s crisis hit. A similar pattern has previously been shown for intergenerational earnings mobility in (Brandén and Nybom, 2020).

[14] Expansions of schooling have previously been shown to affect intergenerational mobility positively, with the 1972–1977 expansion of upper secondary school in Finland leading to more income mobility among the affected students (Pekkarinen et al., 2009).

2 Background

2.1 Pre-crisis years

The late 1980s, the time period leading up to the crisis, was characterized by economic expansion in Sweden with the general unemployment rate being as low as 1.6%. Inflation was high, and the credit market was expanding. The employment rate in 1990 had reached record levels around 85% of the working-age population. For the public finances, the story was different; the deficits in the early 1980s had been replaced by small surpluses during the economic boom years, but the spending was still deemed to be net negative over a full business cycle. Simultaneously, the credit market deregulation in the mid 1980s and the expansive monetary policy regime led to an appreciation of asset prices, which peaked before the crisis. The expanding credit market meant that banks were making more risky lending than before, and the increased borrowing on the household side coincided with an increase in consumption. An expanding economy during the late 1980s meant that the ensuing crisis was not predicted or expected, at least until the fall of 1989 (Englund, 1999).

The Swedish labor market is characterized by a large public sector, which at the start of the crisis in 1990 amounted to nearly one third of total employment. Other major sectors at the time include manufacturing and mining (19% of total employment), the private service sector (17%), retail and trade (12%), construction (8%), and transportation (6%). Men tend to dominate employment in blue-collar work such as the manufacturing, construction, and transportation sector, but are a minority of workers in the retail sector.[15] The general trend in the years leading up to 1990 had been marked by a slow shift away from manufacturing, mining, and agricultural work toward more of the workforce working in private services.

2.2 The 1990s economic crisis

The massive economic crisis, which unexpectedly hit Sweden in late 1990, led to a sharp drop in the overall employment by more than 10 pp. and triggered the worst recession the country had seen since the Great Depression. As noted, the years leading up to the crisis had given few indications of the imminent economic downturn, making the suddenness and depth of the crisis widely unexpected. The crisis originated in the banking sector after a housing bubble burst, which quickly spread to other sectors. The stock market fell rapidly from its peak in August 1989, with the construction and real estate market stock index falling sharply. At the end of 1990, the real estate index had fallen by 52%, compared to 37% for the general index, from its highest level. The crisis was most likely triggered by the housing bubble bursting, but other

[15] In 1990, men constituted approximately 72% of total workers in the manufacturing sector and 78% in the construction sector.

shocks to fundamentals such as high inflation, expansionary macro policy, and low post-tax real interest rates are likely to have contributed (Englund, 1999).

The crisis strained public finances, and prompted austerity measures and extreme monetary policy reactions to defend the fixed exchange rate. The crisis also triggered a credit crunch which, combined with the increased interest rates, impaired the activities of the most indebted firms, led to more bankruptcies, and reduced investments (Englund, 1999). Almost 500,000 jobs were lost during the first half of the 1990s, which were the peak years of the crisis. The layoff rules based on "last-in-first-out" made the junior workers the most affected by job loss. For a relatively small country as Sweden, this constituted a massive blow to the aggregate employment rate which dropped from 86% to around 76% in just a few years.[16] 200,000 of the job losses were linked to the manufacturing sector, 100,000 jobs were lost in the construction sector, 100,000 jobs were cut in the public sector, and the remaining 100,000 jobs lost were in the service sector. Around half of the 10% fall in manufacturing production between 1990 and 1993 can be explained by the drop in domestic demand of manufacturing products stemming from the halting construction sector. The remainder of the decrease is likely explained by the fall in private consumption. Paired with a general productivity increase in the manufacturing sector during these years, this is the likeliest causes of the massive and lasting drop in manufacturing employment (Perbo, 1999).

The economic recovery ensued in the late 1992, when the Swedish currency (SEK) was allowed to fall, which served to boost exports and aid the heavily-hit manufacturing sector. While the drop in output between 1990 and 1993 had been substantial, the fall had been recovered by 1995 with a 20% increase in output over the period 1990–1995. Residential investments, which had fallen by 72% during that period, were more sluggish in recovering, which led to the construction sector recovering more slowly than the manufacturing sector. By 1994, the construction sector had started recovering and by 1999, the previous peak level in 1990 had been surpassed (Perbo, 1999).[17] The drop in employment proved to be long-lasting and was followed by permanently lower employment rates than before the crisis.

2.3 High school education in Sweden in the 1990s

Swedish high school education is voluntary, but almost 90% of the cohort graduating from compulsory school in 1988 went on to start high school directly after graduation.[18] Students generally attend a school within their own

[16] The recession induced by the crisis was substantial, with GDP falling for three consecutive years. The fall totalled –5.1% between 1991 and 1993. Private investment fell by 35% during the same years.

[17] However, residential construction by 1998 still remained 75% below the 1990 level.

[18] Around 85% of the children in the cohort set to graduate from high school in 1990 did so. All students graduating from compulsory school are eligible for high school studies. However,

municipality, but may in some cases attend one in a neighboring municipality. The high school system at the time consisted of many different programs within two tracks (vocational and academic). The choice of high school program is made at the end of the spring term of grade nine, which is usually the year the students turn 16.[19] Students are then assigned to programs based on their preferences and their compulsory school grades (Hall, 2012).

The high school system in Sweden underwent an extensive reform in the early 1990s by extending the duration of the vocational school track from two years to three years (the same as the academic track).[20] Some changes were also made to the program system. The previous high school system consisted of 27 different programs, with many of them being closely linked to a specific occupation or sector of employment. Following the reform, the existing 27 programs had been replaced by 16 programs (two theoretical and 14 vocational), all of which provided general eligibility for university studies (Hall, 2012).[21] Importantly for our identification strategy, there existed vocational programs tightly linked to the manufacturing sector or construction sector both before and after the high school reform.[22] This reform is, however, likely to have affected the time trend in pursuing different high school programs, and we deal with this in the estimation by including cohort fixed effects to rely exclusively on variation in crisis exposure within a given birth cohort.

3 Data and empirical strategy

3.1 Data

We leverage the rich Swedish register data to investigate how economic crisis affects the early career choices of students. The main data source used in the project is administrative data from the IFAU database covering the en-

students who start high school after age 20 must enter the adult education system instead of attending a regular high school.

[19]Switching programs or quitting high school altogether is relatively rare. A study from 2006 followed all the students starting high school for the first time that year and found that 10% of the students in vocational programs had switched program of study over the two following years. The share of switchers were roughly 9% for the manufacturing program and 5% for the construction program. 3% had dropped out of high school or taken a break from the studies.

[20]Engdahl et al., 2022 study the effect of graduating during the crisis on females' earnings, employment, and family formation by using variation in graduation years induced by the reform. The results show that graduating straight into the recession had negative labor market consequences during the first few years but there is no evidence of significant adverse effects on women's labor market outcomes.

[21]The changes were mainly motivated by the benefits of broadening the vocational education programs, and to grant all high school students some degree of university eligibility.

[22]However, a larger mass of students decided to pursue the two theoretical programs following the reform, which means that fewer students were completing a vocational high school program.

tire Swedish population aged 16 and above during the period 1985–2017.[23] The data span various registers that are connected through anonymized identification codes at the personal, family, and firm level. The main register is the employment register (RAMS), which contains information from the national taxation authorities. We use this register to collect information on the sector of occupation for the main source of earnings for all working-age individuals. A wide array of standard characteristics is added for all individuals: various income sources including earnings, educational attainment, schooling outcomes, demographics, and county or municipality of residence.[24] Parent-child linkages are based on the universe of Swedish parents and children from 1932–2014 through the Multi-Generation Register (MGR). The main analysis focuses on the roughly 232,000 students born between 1970 and 1988, who experienced paternal job loss around the time of the crisis and were on the verge of making or had just made their high school education choices at age 16. In the main specification, we restrict the sample to those who experienced paternal job loss between the ages of 10 and 21.[25]

The data on sector of employment is based on the yearly SNI sector code during the period 1981–2017. The code provides information on the sector of the main source of employment for each individual. This information allows us to map fathers experiencing job loss during the 1990s crisis to a specific sector, and is thereby key in the identification strategy presented below. We proxy job loss as the father being employed in the previous year, but not employed in the current year, and we map the job loss to a specific sector using the aforementioned data on sector of employment.[26] This approach provides us with a measure of job loss assigned to a specific sector of the economy. If a child experiences multiple instances of paternal job loss, we limit our attention to the first event.

Students' high school program choice is derived from the educational attainment codes around age 20, an age at which most of the students would have graduated from high school but not have had time to obtain any higher degree.[27] We classify the 238 program codes and group them into 17 cate-

[23] We also use data on the full population every five years from the censuses 1960–1990 in order to get information on the earnings and employment history on older workers.

[24] Municipalities are smaller geographic units contained within counties. There are 21 counties in Sweden, and at the time there were roughly 284 municipalities.

[25] We also show results using a tighter bandwidth for those experiencing job loss around age 16 when the high school program choice is made.

[26] Missing employment information in combination with zero earnings for a given year is also coded as not being in employment. To link the job loss to a specific sector, we use the sector of occupation for the year of the job loss, or the sector from the previous year if the father had a stable employment in the previous year's sector (for the two previous years) in order to get more accurate employment information and not risk mismatching sectors due to the father finding a temporary or minor employment in another sector following the job loss.

[27] The outcome variable for the (very few) individuals who have university credits from a free-standing course at age 20 is based on information on the highest education at age 19, in order to

gories according to the sector of employment the program is geared toward.[28] As an example, the Industry and Workshop Technician program is coded as a manufacturing-linked education, while the Broad and General Education programs are coded as theoretical-linked programs leading to university studies, and the General Construction program is coded as a construction-linked education. This classification is converted into indicator variables and is then used as the main outcomes in the analysis of students' career choice.[29]

We further investigate the specific effects linked to the economic crisis by characterizing the severity of the economic crisis in each of the 284 Swedish municipalities. Specifically, we define crisis severity as the local drop in the employment rate from the peak (1990) to the trough (1993). We also define an alternative measure of crisis exposure based on local unemployment rate increases. For ease of interpretation, the measures are standardized to have mean zero, standard deviation one across municipalities. To capture potential non-linear effects of crisis exposure, we further create non-parametric estimates by sorting and splitting the crisis severity into quartiles. These measures allow us to investigate different heterogeneous treatment effects with respect to local crisis severity.

3.2 Descriptive statistics

Descriptive statistics of the sample of students experiencing paternal job are provided in Table 1. The descriptive statistics show that average characteristics for the different kinds of job loss displayed are highly similar, although the fathers experiencing construction job loss are a year younger than the fathers experiencing manufacturing job loss. A graphical representation of the number of students experiencing paternal job loss, sorted by the year of experiencing job loss, is shown in Figure 8. This figure shows a marked increase in the number of children experiencing paternal job loss during the crisis years.

capture their high school education. Likewise, we also include students' high school education information at age 21 if they graduate late.

[28] Roughly 60% of all individuals have either one of the seven largest education codes, while the remainder have one of the smaller 231 codes.

[29] The classification is shown to predict future employment well for the manufacturing and construction-linked programs. 53.6% of the students born in 1980 who graduated from a construction program were working in the construction sector by age 35, compared to 6.5% in the entire cohort. For students born the same year who graduated from the manufacturing program, 41.8% were working in the broad manufacturing sector by age 35 (compared to 10% in the entire cohort). The outcomes for other programs are harder to evaluate given that the employment opportunities are more dispersed across the different sectors. For instance, individuals who graduate from the general theoretical programs are well equipped to work both in the public sector and in parts of the manufacturing sector. However, 18.3% of the graduates from the transportation program work in the transportation sector, compared to 4.2% in the entire cohort. Also, 15.7% of the graduates from the retail programs work in the retail sector, compared to 6.1% in the cohort.

3.3 Empirical strategy

We investigate the effects of economic crisis on high school program choice by restricting attention to the sample of students experiencing paternal job loss during the crisis.[30] The estimation strategy consists of comparing career program outcomes for the sample of students experiencing paternal job before and after the high school program choice around age 16, specifically focusing on the effects of paternal job loss in the crisis sectors. This approach thus relies on cross-sectional variation, stemming from the timing of paternal job loss from sectors heavily hit by the Swedish economic crisis of the early 1990s. The starting point where we compare the effects of experiencing paternal job loss in the crisis sectors before and after age 16 is the following empirical specification with outcomes defined for an individual student i, from cohort j, residing in county k:

$$y_{i,j,k} = \phi_0 + \phi_1 JL_i^{Cr} \times Early_i + \phi_2 JL_i^{Cr} + \theta_{j,k} + \boldsymbol{X_i'\delta} + \varepsilon_{i,j,k} \tag{1}$$

where JL_i^{Cr} is an indicator taking the value one if the student experienced paternal job loss in the sectors most affected by the crisis (manufacturing or construction), and zero if the paternal job loss occurs in any other sector.[31] $Early_i$ is an indicator taking the value one if the student experienced paternal job loss in any sector before age 16 (ages 10–15), and zero if the job loss occurred between ages 16–21. $\theta_{j,k}$ denotes cohort-by-county (of residence at age 16) fixed effects. $\boldsymbol{X_i}$ is a vector of controls containing information on paternal and maternal characteristics from the 1990 census, which is added to test the robustness of the estimates and to potentially increase their precision.

This specification captures the effect on program choice of experiencing paternal job loss in the most affected sectors from age 16 (ϕ_2), compared to those experiencing job loss in any other sector at age 10–21. We then identify the effect of experiencing early paternal job loss in the crisis sectors before age 16 (ϕ_1), which is the coefficient of interest. The cohort-by-county fixed effects isolate cross-county and cohort disparities for those living in different regions with potential differences in labor market structure, meaning that we estimate the within-county-cohort difference in outcome.[32] This helps to mitigate the concern that supply-side could affect the outcomes of interest. Standard errors are clustered at the level of the students' municipality of residence around age 16.[33]

[30] The education program outcome is defined as the highest realized level of education attained by age 20 for each individual in the sample. This is the age when most young adults have finished high school, but very few have a completed higher education from university (which would override the high school education choice).

[31] An observation of zero earnings is also coded as being not employed when employment information is missing.

[32] We also show results exchanging these indicators for the more demanding cohort-by-municipality fixed effects as a general robustness test.

[33] At the time, there were roughly 284 municipalities in Sweden.

A key concern with the aforementioned approach is that we risk capturing the effects of experiencing job loss before and after age 16 in general, and not effects specific to job loss in the heavily-hit sectors. We account for this in our main empirical specification:

$$y_{i,j,k} = \beta_0 + \beta_1 JL_i^{Cr} \times Early_i + \beta_2 JL_i^{Cr} + \beta_3 Early_i + \theta_{j,k} + \boldsymbol{X_i'\delta} + \varepsilon_{i,j,k} \quad (2)$$

which flexibly captures the general effect of experiencing paternal job loss in the crisis sector from age 16 (β_2) and job loss before age 16 in any other sector (β_3), allowing us to estimate the added effect of experiencing early paternal job loss before age 16 in the crisis sectors (β_1). In other words, this specification nets out the main effect of experiencing paternal job loss before and after age 16 in any other sector to focus on the differential effect of experiencing early job loss in the crisis-linked sectors.[34]

The identification strategy exploits the fact that the choice of high school program in Sweden occurs during the spring of the year in which students turn 16. This discontinuity, along with the timing of experiencing paternal job loss, is the main source of variation used in the paper. From this, we base our strategy on the idea that a composite information shock, stemming from paternal job loss occurring before age 16, should have a stronger effect in dissuading the student to select a career program linked to the father's previous sector of employment than if the paternal job loss occurs at age 16 or after, when the program choices have already been made. In a sense, we are postulating that students experiencing early paternal job loss respond to the information shock by substituting away from a career program that would have emulated the father's career.

By contrast, students experiencing later paternal job loss in the same sector will be highly similar to the students experiencing early job loss, but have limited means of adjusting their high school program choice in response to the shock. In our view, experiencing such an event during a massive economic crisis likely contains a number of elements contributing to the discouragement from entering the crisis sector. First, the family-based shock of paternal job loss is likely to make the economic conditions and prospects of the crisis-affected sector more salient to the students. Second, job loss during a crisis likely breaks the information network of the parent that the student is using to help start a career. Third, the job loss may alter the gains from working with the father or following him into the same career path. Fourth, the household's resource loss, when the usual main provider loses his job, may change the risk-preferences of students by causing them to opt for a more stable career program given the volatile economic conditions.

Our empirical specification also nets out the main effects of experiencing a paternal job loss shock on career choice, which should capture the reduction

[34] See Figure 9 for event study figures of the paternal job loss event in terms of his employment rate and earnings.

in resources stemming from the job loss of the main provider. Thus, we focus on capturing the informational aspects of experiencing paternal job loss linked to a specific career choice.

4 Results

4.1 High school program outcomes

General trend in high school program choice

We start by documenting the general time trend in realized high school career programs at age 20 for the cohorts born between 1964 and 1988 (see Figure 1). The drop in manufacturing and construction employment following the crisis was deep and persistent, and many of the jobs lost in these sectors during the crisis never returned. The cohorts born before 1971 would, with this outcome definition, not have had any chance to react to the economic crisis starting in 1990, while the following cohorts born after then could to some extent react to the information by switching program or choosing a different educational program.

The figure shows that the the overarching trend in high school program choice is a substantial increase in the share of high school students completing a general theoretical education track program, making them eligible for further university studies (see Figure 1a). For the smaller programs in the vocational track, there are also apparent changes over time in the program shares, with the majority of the changes occurring for those born during the mid 1970s (see Figure 1b). A notable trend is that the program share of manufacturing, a sector heavily affected by the crisis and the following restructuring of the labor market, was reduced by half in less than 10 years. On the other hand, the heavily affected construction program experienced less of a fall, and started to trend upward as the sector recovered. Other programs, such as the arts or childcare programs, increased in popularity over the time period. As mentioned, however, it is not possible to disentangle the specific contribution of the economic crisis to these patterns, since the high school reform was implemented simultaneously.

The effects of paternal job loss on high school program choice

Next, we proceed with our causal analysis. Our estimation strategy relies on within-cohort-county variation in the timing and sector of paternal job loss and their effects on the students' propensity to complete a crisis-linked high school program, i.e. a program in manufacturing or construction. We use the fact that early paternal job loss from the crisis sectors before age 16 should provide a negative signal that the affected students are able to act upon, compared to when the job loss occurs at age 16 or after, when the high school program choices have already been made. Further, we ensure that the effects are limited to paternal job loss from the crisis sectors by netting out the main effects of experiencing paternal job loss in any other sector before and after age 16.

The results for completing a crisis sector-linked high school program are presented in Table 2, and in Figure 2. The graphical results in Figure 2a indicate a clear discontinuity in the propensity to have a completed crisis-linked education if the paternal job loss in the sector occurred before age 16. These graphical results are confirmed in the pooled OLS regression analysis, where we pool the paternal job losses to compare those that occurred before age 16 to those that occurred after. The direct effect of early paternal job loss on having completed a crisis-linked career program (see Equation 1) is negative and statistically significant (-0.011, s.e. 0.003), and is very similar to the preferred DiD specification (see Equation 2) where we also net out the main effect of experiencing paternal job loss during the same ages in any other sector (-0.012, s.e. 0.003). In relation to the mean of the dependent variable for those experiencing paternal job loss in the crisis sectors from age 16, the DiD estimate corresponds to a relative change of -11%. When splitting the outcomes and job loss sectors into the separate manufacturing and construction career programs, we see similar effects: the direct difference and DiD estimates for the manufacturing program (ranging from -0.005 to -0.008, s.e. 0.003) and construction program (-0.011, s.e. 0.003) outcomes are statistically significant and negative. In columns 3, 6, and 9 in Table 2, we further show that the effects on crisis-linked program outcomes are robust to controlling for background characteristics from the 1990 census and to including cohort-by-municipality fixed effects (instead of cohort-by-county fixed effects). All in all, these results show that experiencing paternal job loss in the crisis sectors before age 16 is associated with students opting out of crisis-linked high school programs.

4.2 The effects of paternal job loss on lifetime earnings

Next, we investigate the labor market consequences of early paternal job loss using various measures of cumulative earnings. Based on our earnings panel, we define three measures: cumulative lifetime earnings, cumulative earnings between the ages 20 to 30, on the one hand, and between the ages of 30 and 40, on the other hand, to capture different aspects of the career profile. The results using these outcomes and the preferred DiD specifications are reported in Table 3 and Figure 5.

The effects on cumulative lifetime earnings (SEK $86,664$, s.e. $24,697$), cumulative earnings between ages 20–30 (SEK $44,975$, s.e. $9,967$), and cumulative earnings between ages 30–40 (SEK $28,989$, s.e. $15,789$) in Table 3 are all positive and statistically significant for the main DiD specification. Adding further controls, we see that the estimates decrease in magnitude, but mostly remain statistically significant. These results show that the students affected by early paternal job loss in the crisis sectors enjoyed better labor market outcomes compared to their peers who experienced paternal job loss at age 16 or later. The relative effects are the strongest in magnitude for cumulative earnings early in the career, and the same effects are also the most robust. This indicates that the positive labor market effects are more concentrated during the early career years age 20–30, although there are signs that the effects persist later in the career at age 30–40. The estimated effects are sizable and amount to roughly a 1–3% increase in cumulative earnings during different parts of the life cycle.

4.3 The effects of paternal job loss on substitution to other career programs

A key question remaining to be answered is what the students end up doing instead of completing a manufacturing- or construction-linked program. To answer this, we investigate students' substitution to other career programs after opting out of the crisis sector programs following paternal job loss. The results can be seen in Tables 4 & 5. The general substitution pattern away from manufacturing programs (−0.008, s.e. 0.002) and construction programs (−0.005, s.e. 0.002) does not lead to any statistically significant shift toward or away from the theoretical programs (−0.003, s.e. 0.005) or to having no high school education (−0.000, s.e. 0.004) at age 20. Rather, the results indicate that the students substitute into closely related career programs that were less affected by the crisis, such as in transportation (0.004, s.e. 0.002), hotel & restaurants (0.006, s.e. 0.002), and agriculture (0.004, s.e. 0.001).[35]

In a more refined analysis in Table 5, we investigate the characteristics of the high school programs that the students switched to. We characterize these programs by computing the early career average earnings and earnings variance of students who graduated from them before the crisis, using the outcomes for older cohorts born 1964/65 and 1968/69 separately. We then map these outcomes to the younger cohorts who selected into the same programs around the time of the crisis. The outcomes are standardized to have a mean of zero and a standard deviation of one across all programs. Our results indicate that following early paternal job loss in crisis sectors, the students did not switch to programs with higher earnings during age 22–25, as measured from the outcomes of earlier cohorts born in 1964/65 with outcomes defined before the economic crisis. They did, however, switch to career programs with higher standardized earnings (0.026 SD, s.e. 0.012), lower earnings variance (−0.045 SD, s.e. 0.010) as measured by the outcomes of the cohorts closer in age born in 1968/69, and lower earnings variance (−0.020 SD, s.e. 0.010) based on the outcomes of the earlier cohorts born in 1964/65. The students also seem to have sorted into career programs with higher post-graduation employment rate during the crisis (0.002, s.e. 0.001). Combining the measures of earnings

[35] Also, the students appear to steer away from the arts program (−0.004, s.e. 0.002).

and earnings variance, the students appear to have substituted into more stable careers in terms of risk-adjusted earnings (0.044 SD, s.e. 0.011), primarily based on the outcomes of the younger cohorts, who are close in age and with outcomes defined during the economic crisis.

4.4 The effects of paternal job loss on other long-run outcomes

The identification strategy is based on experiencing paternal job loss before or after high school program choices at age 16. From this, it is not clear that experiencing an information shock at such an early age should affect labor market outcomes in the very long run. It could very well be the case that the effects attenuate over time. We investigate the effects on students' long-term outcomes in 2015, i.e. 20 years after the economic crisis ended, by presenting results for sector of occupation, university studies, employment status, and being divorced.

The results reported in Table 6 show that experiencing early paternal job loss in the crisis sectors negatively affected the students' probability of completing university studies (−0.010, s.e. 0.005), but increased their chances of employment in the long run (0.008, s.e. 0.003). Further, we see that these individuals were less likely to be employed in the specific crisis sector in the long run, whether their father lost his job in manufacturing (−0.007, s.e. 0.003) or in construction (−0.007, s.e. 0.004), with the caveat that the effects from construction job loss are not statistically significant. This indicates that the effects are persistent throughout the individual's career.

4.5 Heterogeneous treatment effects

Municipal crisis exposure

We investigate whether the information shock stemming from early paternal job loss had differential effects on high school program choices depending on the severity of the economic crisis in the municipality of residence at age 16. It is not clear, ex ante, how the information shock from paternal job loss is affected by local crisis severity. Nor is it clear how students respond to local crisis severity in terms of opting out of crisis-linked high school programs. We look into this by defining two different measures of local crisis exposure: crisis severity in the municipality measured from the employment rate decrease in 1993 relative to 1990 for the full economy, or the increase in unemployment rate in the municipality during the same years.[36] The results on heterogeneous treatment effects based on the municipal crisis exposure are presented in Table 7.

[36] These splits pick up substantial spatial heterogeneity in terms of crisis exposure, with the average employment rate drop 1990–1993 for municipalities in the least affected quartile being 12 pp, and the same drop for the most affected quartile being 19 pp.

Splitting the effects into quartiles of crisis severity, we observe no statistically significant responses of students in terms opting out of the crisis-linked educations based on local crisis severity. This holds both for students in general and for students with fathers working in the crisis sectors in 1990.[37] For the students experiencing paternal job loss, the only statistically significant heterogeneous treatment effect on having attended a high school program linked to the manufacturing or construction sectors is observed for the most affected quartile in terms of municipal employment rate drop (0.019, s.e. 0.009). For these students, the treatment effect on educational choice stemming from experiencing early paternal job loss is close to zero. However, the other quartile 2–4 point estimates for the different measures of crisis severity are also positive, albeit not statistically significant. These results suggest that the information signal from experiencing paternal job loss is attenuated if the local municipality experiences a more severe economic downturn. The same general pattern can be observed for lifetime earnings. Similarly to the effects on educational program, all point estimates for the different measures of crisis severity are of the same sign, but are not statistically significant.

By student gender

Up to this point, the effects of parental job loss have not been estimated separately for male and female students. However, it is well known that the majority of vocational high school students are male and that the gender imbalance is larger in manufacturing and construction programs. Based on this, we present heterogeneous treatment effects for female students in Table 8.

The results confirm the low baseline rate of female students in crisis sector career programs. As a consequence, most of our estimates significantly differ by gender and are weaker for the female students. As expected following this, the estimates strengthen for the male students when separating the effects by student gender, which is reasonable given the higher baseline rate. The effect of paternal job loss on male students' propensity to sort into the crisis sector career program increases substantially (-0.024, s.e. 0.005), as does the effect on their lifetime earnings (SEK $130,357$, s.e. $35,953$). However, the relative effects only increase moderately since the mean dependent variable for these outcomes also tends to be larger for male students.

4.6 Robustness tests

Predicted crisis sector program education and earnings

A key robustness test relates to selection effects linked to the timing of paternal job loss. If very different families experience late versus early paternal job loss, then this selection may affect the results and lead to biased estimates. We

[37]The only exception are for students with the father working in the crisis sectors and residing in the most crisis-affected municipalities in terms of employment rate drop.

test this concern by predicting our main outcomes of interest using observable background characteristics and see if we can replicate any of the main findings. See Figure 6 for a graphical representation of the results. The characteristics used to predict the outcome are primarily from the 1990 census and the Multi-Generation Register, and include parents' year of birth, high school completion status, marriage status, cohort-by-county of the student, parents' number of children, birth order of the student, sex of the student, and indicators of missing values for the characteristics.[38]

The figures show no clear pattern of replicating the main findings for the predicted crisis-linked high school education outcome. Reassuringly, an F-test of joint significance fails to reject the null of no difference in effect (p-value 0.376). For predicted cumulative earnings age 20–30, the F-test also does not reject the null of no difference in effect (p-value 0.109). Although, the graphical results indicate minor imbalance at age 14–15 and some gradual decline in the predicted cumulative earnings appearing later in the paternal job loss age profile, which are not statistically significant. In addition, the predicted values are much smaller in absolute magnitude compared to the estimated treatment effects.[39]

Sibling fixed effects

As a robustness check, we estimate effects of early paternal job loss, in the crisis sectors, on the main outcomes conditional on sibling fixed effects (see Table 11). These specifications rely on siblings experiencing paternal job loss before and after age 16. While being a demanding specification to estimate, it controls for all common characteristics shared through the mother.

In general, the job loss effects are robust to the inclusion of sibling fixed effects. The estimate for the crisis sector program outcomes are negative and statistically significant (−0.015, s.e. 0.008), albeit with much larger standard errors. The estimates for cumulative lifetime earnings and cumulative earnings between age 20 and 30 often remain highly similar to the baseline specification in terms of magnitude, but are no longer statistically significant.

Alternative job loss ages bandwidth

We further test the robustness of our estimates by using tighter bandwidths of the students' paternal job loss ages in Tables 9 & 10. In line with the graphical evidence in Figures 2 & 5, the effects tend to become smaller when we use a tighter bound on the job loss ages. Restricting the analysis to job losses that occurred between the ages of 14 and 17 reduces the effects on having a crisis-linked education to −0.9 pp. (−0.09, s.e. 0.004), which is still statistically significant. The tightest possible comparison, ages 15–16, is still negative

[38] The R^2s of the predictions range from 0.074–0.099.

[39] In Table 3, we show that the main treatment effects on cumulative earnings reduce in magnitude but stay statistically significant when including the controls used in the aforementioned balancing test.

of a similar magnitude, but is no longer statistically significant (-0.007, s.e. 0.006). The same general pattern can be seen for lifetime earnings: restricting the analysis to the discontinuity at ages 15–16 leads to an estimate not statistically significant ($74,768$, s.e. $55,202$), which is comparable in magnitude to the main estimate.

4.7 Mechanisms

Triple difference

The main empirical specification uses the timing of paternal job loss during the peak years of the economic crisis affecting Sweden in the early 1990s. We investigate the extent to which the observed effects on educational program choice are limited to experiencing paternal job loss during the crisis years in Table 12. We do so by estimating a triple difference where we, in addition to the main DiD specification, net out the same effects from experiencing paternal job loss in the crisis sectors after the economic crisis in 1996–1999:

$$\begin{aligned} y_{i,j,k} = \psi_0 + \psi_1 JL_i^{Cr} \times Early_i \times Crisis_i + \psi_2 JL_i^{Cr} \times Early_i + \psi_3 JL_i^{Cr} \times Crisis_i \\ + \psi_4 Early_i \times Crisis_i + \psi_5 Early_i + \psi_6 Jl_i^{Cr} + \psi_7 Crisis_i + \theta_{j,k} + \boldsymbol{X_i'\delta} + \varepsilon_{i,j,k} \end{aligned}$$

where $Crisis_i$ is an indicator taking the value one for paternal job losses occurring during 1991–1995, and zero for job losses in 1996–1999 after the economy had started to recover. The coefficient on the triple interaction term (ψ_1), which is the coefficient of interest, thus captures the triple difference effect of paternal job loss on educational choice unique to the economic crisis 1991–1995, compared against the DiD job loss effect 1996–1999. See Figures 2b, 3b, 4b, and 5b for graphical results of experiencing job loss after the crisis on the main outcomes.

In general, the triple difference leads to small changes in magnitude compared to the baseline DiD specification. The triple difference estimate on educational choice is roughly 0.1–0.3 pp. smaller in magnitude compared to the DiD specification, but the estimates are now sometimes marginally not statistically significant due to the demanding empirical specification leading to larger standard errors. This result indicates that the empirical findings on high school program choice stemming from paternal job loss is largely limited to the crisis years.

Main effect of experiencing paternal job loss from any other sector

In addition to investigating the triple difference including job loss after the economic crisis, we present graphical evidence of the main effects of experiencing early job loss from any other sector on crisis-linked high school education choice. These figures are meant to show the extent to which paternal job loss in general can affect the students' choice of completing a crisis-linked high school program. The results can be seen in Figure 7.

Figure 7a shows that there is no clear discontinuity in the propensity of having a crisis-linked education based on the age of experiencing paternal job loss from any other sector. Conversely, those experiencing early paternal job loss appear to be slightly more likely to pursue such an education, although this effect is not statistically significant. This result indicates that job loss or the resource loss associated with this does not lead to any significant shift away from the crisis-linked programs.

5 Discussion

The findings of this paper indicate that behavioral responses to economic crisis in terms of early career choice can be rapid and substantial. The educational responses we observe imply that the rapid adaption to economic crisis contribute to structural change on the labor supply side. Based on our empirical approach, we interpret our findings as the causal effects of informational frictions interacted with educational choice flexibility before age 16, net of any main main effects of experiencing paternal job loss in any other sector. We argue that this interpretation is further strengthened by the empirical result that these effects are attenuated based on local crisis severity, which we interpret as a weakening of the information signal stemming from paternal job loss. In other words, that the information shock within the family becomes less important if the local conditions in terms of job loss are more severe.

We interpret the magnitude of our estimate on having a crisis sector-linked high school education (-11%) as being large, since these effects are in addition to marked shift away from these high school programs in the general. With the baseline rate of pursuing such an education being higher for the students with fathers working in the crisis sectors, the effects can be interpreted as a partial convergence in high school program outcomes compared to the full population of students. The same interpretation of effect size holds true for the estimated effect on cumulative lifetime earnings (-2%), where our measure based on the timing of paternal job loss leads to a clear decrease in lifetime earnings.

A key limitation of our empirical approach is that we cannot disentangle the effects of informational frictions on the sector's economic prospects from the student's preferences for entering the same sector as the father. It could be the case that students value entering the same sector as their father and base their educational choices on this, and that paternal job loss removes the utility component of following in the father's footsteps. The fact that we observe only weak treatment effects in municipalities heavily hit by the crisis does, however, strengthens the case that the economic prospects component of the information shock is a relevant part of experiencing paternal job loss. We thus interpret our estimates as a composite information shock containing both information on the economic prospects and utility stemming from intergenerational persistence in sector of occupation. Our findings suggest that economic crisis exposure and paternal job loss can weaken the link between father and child, and affect economic mobility and intergenerational persistence in labor market outcomes.

In terms of policy implications, our findings suggest that overcoming informational frictions associated with educational choice can significantly improve the labor market consequences of students affected by economic crisis. One way to try to reduce these frictions could be to target students and provide career counselling about the prospects of entering each sector before making the high school education choices, or other measures to make the economic consequences of early educational choices more salient. This, however, requires that our estimated effects are not solely capturing preferences linked to working in the same sector as the father interacted with educational choice flexibility. If not, then the largest gains from policies to reduce information frictions are likely found for individuals who have some flexibility to alter their career choices. Extending informational policies beyond students on the verge of making their high school choices would then ideally be paired with measures to increase the flexibility of more mature workers, such as providing retraining programs.

6 Conclusion

In this paper, we estimate how economic crisis affects the early career choices of the next generation of workers. By exploiting the timing of paternal job loss during the massive economic crisis which hit Sweden in 1990, we estimate causal effects and study mechanisms related to crisis exposure and information shocks for students before they enter the labor market. Specifically, our identification strategy relies on within-cohort variation in experiencing paternal job loss in the heavily affected manufacturing and construction sectors before or after high school program choices are made. Our results show a substantial decrease in students' probability of pursuing a manufacturing- or construction-linked high school career program during and after the economic crisis, with students who experienced early paternal job loss substituting more strongly away from educations linked to these sectors. The substitution patterns away from the paternal crisis sectors mainly lead the affected students into other vocational education career programs. In the long run, the affected students are more likely to steer clear from the paternal crisis sectors, and exhibit greater lifetime earnings and employment rates. In turn, this contributes to a weakening of the intergenerational link between fathers and their children, to the benefit of the children's labor market outcomes.

Delving deeper into the results, we find that students affected by early paternal job loss substitute into more stable education programs with lower earnings variance. The students thus respond to the crisis by making less risky early careers choices, which has a positive impact on their lifetime earnings. Further, our results indicate that exposure to early information signals during economic crisis, in this case paternal job loss, is less helpful for students if the local area was hit hard by the crisis in terms of job losses. We interpret this as the information signal stemming from the paternal job loss being attenuated by the crisis severity of the local labor market conditions.

Finally, the findings of this paper show that there are clear margins of improvement to help individuals overcome informational frictions during economic crisis. One way to do this would be to try to target students and workers and inform about the current economic conditions and long-term prospects of the sector using, for instance, career counselling. Extending the results further, another policy implication of this paper would be to improve educational flexibility for young people and possibly retraining opportunities for older workers. The bottom line is that educational choice flexibility, combined with informational advantages, can be important factors in parrying the individual consequences of economic crisis.

Result tables and figures

Tables

Table 1. *Descriptive statistics in 1990 for the main sample of students experiencing paternal job loss.*

	Job loss any sector	Manufacturing job loss	Construction job loss
Year of birth	1977.937	1977.558	1977.710
Share female	.485	.482	.484
Year of birth father	1946.918	1946.539	1947.789
Year of birth mother	1950.403	1950.239	1950.855
Earnings father 1990	1665.873	1683.317	1809.283
Earnings mother 1990	1032.255	994.733	1019.456
Employment rate father 1990	.976	.990	.994
Employment rate mother 1990	.853	.841	.876
Share fathers married	.722	.73	.707
Share mothers married	.722	.738	.712
Share fathers completed high school	.547	.483	.503
Share mothers completed high school	.623	.557	.610
Job loss years	1991–1999	1991–1999	1991–1999
Cohorts	1970–1988	1970–1988	1970–1988
Obs.	232,566	54,146	37,564

The table shows descriptive stats on the main sample from the 1990 census. The descriptives are split by the students experiencing paternal job loss in any sector, in the manufacturing sector, and in the construction sector during 1991–1999.

Table 2. *Effect of paternal job loss on crisis sector program outcomes age 20.*

Job loss sector:	Crisis sector job loss			Manufacturing sector job loss			Construction sector job loss		
Specification:	1x Diff.	DiD	DiD	1x Diff.	DiD	DiD	1x Diff.	DiD	DiD
Outcome:	Crisis sector education			Manufacturing education			Construction education		
Job loss b. age 16 × Sector	-0.011***	-0.012***	-0.011***	-0.007**	-0.008***	-0.005*	-0.011***	-0.011***	-0.011***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Cohort × County FE	✓	✓		✓	✓		✓	✓	
Cohort × Municipality FE			✓			✓			✓
Controls			✓			✓			✓
Job loss years	1991–95	1991–95	1991–95	1991–95	1991–95	1991–95	1991–95	1991–95	1991–95
Mean dep. var.	0.107	0.107	0.107	0.072	0.072	0.072	0.060	0.060	0.060
Obs.	175,428	175,428	176,658	175,428	175,428	176,658	175,428	175,428	176,658

Note: $***p < 0.01$, $**p < 0.05$, $*p < 0.1$. Standard errors are clustered on the student's municipality of residence around age 16 level. "1x Diff." refers to the specification capturing the direct difference in outcome for those experiencing paternal job loss in the specified sector during ages 10–15 against those experiencing paternal job loss in the same sector age 16–21. "DiD" refers to the double difference specification where we, in addition to the "1x Diff." specification, net out the effect of experiencing paternal job loss from any other sector during the same ages. "Controls" refers to including control variables based on parental information from the 1990 census, which includes parental birth cohort, sex of the student, parental labor market outcomes, marriage status, and high school completion status. "Crisis sector education" refers to combining the manufacturing and construction educational outcome into one category, while "Manufacturing education" and "Construction education" shows the outcome separately for the two career programs.

Table 3. *Effect of crisis sector paternal job loss on earnings during the life cycle.*

Specification:	DiD	DiD	DiD	DiD	DiD	DiD
Outcome:	Lifetime earnings		Earnings age 20–30		Earnings age 30–40	
Crisis sect. job loss b. age 16	86664***	58029**	44975***	36557***	28989*	15462
	(24697)	(24523)	(9967)	(9509)	(15789)	(16000)
Cohort × County FE	✓		✓		✓	
Cohort × Muni. FE		✓		✓		✓
Controls		✓		✓		✓
Job loss years	1991–95	1991–95	1991–95	1991–95	1991–95	1991–95
Mean dep. var.	5,210,000	5,210,000	1,510,000	1,510,000	2,590,000	2,590,000
Obs.	175,428	176,658	175,428	176,658	170,671	171,877

Note: $***p < 0.01$, $**p < 0.05$, $*p < 0.1$. Standard errors are clustered on the student's municipality of residence around age 16 level. "DiD" refers to the double difference specification where we compare the outcome for those experiencing paternal job loss before and after age 16 and also net out the effect of experiencing paternal job loss from any other sector during the same ages. "Lifetime earnings" refers to cumulative earnings during the lifetime (up until the final year of our earnings panel in 2017). "Controls" refers to including control variables based on parental information from the 1990 census, which includes parental birth cohort, sex of the student, marriage status, and high school completion status.

Table 4. *Effect of crisis sector paternal job loss on substitution to other career programs.*

Outcome:	Crisis sector ed.	Manufacturing	Construction		
Crisis sect. job loss before age 16	-0.012***	-0.008***	-0.005**		
	(0.003)	(0.002)	(0.002)		
Mean dep. var.	0.107	0.067	0.041		
Obs.	175,428	175,428	175,428		
Outcome:	**Theoretical**	**Nursing**	**Electrician**	**Retail**	**No HS degree**
Crisis sect. job loss before age 16	-0.003	-0.001	0.001	0.003	-0.000
	(0.005)	(0.001)	(0.002)	(0.002)	(0.004)
Mean dep. var.	0.188	0.030	0.042	0.057	0.179
Obs.	175,428	175,428	175,428	175,428	175,428
Outcome:	**Transportation**	**Arts**	**Childcare**	**Hotel & Rest.**	**Agriculture**
Crisis sect. job loss before age 16	0.004**	-0.004*	0.004	0.006***	0.004***
	(0.002)	(0.002)	(0.003)	(0.002)	(0.001)
Mean dep. var.	0.034	0.026	0.067	0.024	0.017
Obs.	175,428	175,428	175,428	175,428	175,428
Cohort × County FE	✓	✓	✓	✓	✓
Job loss years	1991–95	1991–95	1991–95	1991–95	1991–95

Note: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Standard errors are clustered on the student's municipality of residence around age 16 level. The main empirical double difference (DiD) specification is used, where we compare the outcome for those experiencing paternal job loss before and after age 16 while also netting out the effect of experiencing paternal job loss from any other sector during the same ages. "Crisis sector ed." refers to combining the manufacturing and construction educational outcome into one joint outcome.

Table 5. *Effect of crisis sector paternal job loss on substitution to more stable career programs.*

Obs. years:	Pre-crisis	During crisis	Pre-crisis	During crisis	Pre-crisis	During crisis	During crisis
Outcome:	Standardized earnings		Standardized earnings variance		Standardized risk-adjusted earnings		Employment rate
Crisis sect. job loss b. age 16	-0.003	0.026**	-0.020**	-0.045***	0.015	0.044***	0.002*
	(0.012)	(0.012)	(0.010)	(0.010)	(0.012)	(0.011)	(0.001)
Cohort × County FE	✓	✓	✓	✓	✓	✓	✓
Mean dep. var.	0.102	0.103	-0.002	0.060	0.085	0.059	0.570
Observation years	1986–1990	1990–1994	1986–1990	1990–1994	1986–1990	1990–1994	1991–1993

Note: $***p < 0.01$, $**p < 0.05$, $*p < 0.1$. Standard errors are clustered on the student's municipality of residence around age 16 level. The outcomes are based on average outcomes for the specific programs for cohorts who made their high school program choices before the 1990s crisis. The main empirical double difference (DiD) specification is used, where we compare the outcome for those experiencing paternal job loss before and after age 16 while also netting out the effect of experiencing paternal job loss from any other sector during the same ages. "Standardized earnings." denotes the standardized (mean-zero, standard deviation one) cumulative earnings outcome age 22–25 based on the outcome of individuals born during 1964–1965 or 1968–1969, having chosen the same program as the students affected by paternal job loss during the crisis. "Standardized earnings variance" and "Standardized risk-adjusted earnings" denotes the variance of the cumulative earnings outcome for the same cohorts and the quotient of the earnings divided by the variance for the same definition. All of the standardized outcomes are standardized by cohort. "Employment rate" denotes an outcome based on the average employment rate for the individuals by program at age 20 during the early crisis years 1991–1993.

Table 6. *Effect of paternal job loss on students' long-run outcomes in 2015.*

Job loss sector:	Crisis sector job loss				Manuf. sector job loss		Constr. sector job loss	
Outcome:	Crisis industry	University education	Employed	Divorced	Manuf. industry	Constr. industry	Constr. industry	Manuf. industry
Job loss b. age 16 × Sector	-0.002	-0.010*	0.008**	0.002	-0.007**	0.003	-0.007	0.007**
	(0.004)	(0.005)	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)	(0.003)
Cohort × County FE	✓	✓	✓	✓	✓	✓	✓	✓
Job loss years	1991–95	1991–95	1991–95	1991–95	1991–95	1991–95	1991–95	1991–95
Mean dep. var.	0.165	0.312	0.881	0.101	0.072	0.096	0.080	0.076
Obs.	150,047	165,251	166,118	166,118	150,047	150,047	150,047	150,047

Note: $***p < 0.01$, $**p < 0.05$, $*p < 0.1$. Standard errors are clustered on the student's municipality of residence around age 16 level. The main empirical double difference (DiD) specification is used, where we compare the outcome for those experiencing paternal job loss before and after age 16 while also netting out the effect of experiencing paternal job loss from any other sector during the same age. "Crisis industry" denotes being employed in the manufacturing or construction industry in 2015.

Table 7. *Effect of municipal crisis or paternal job loss by quartile of municipal crisis exposure.*

Crisis definition:	Employment rate drop		Unemployment rate increase		JL×Employment rate drop		JL×Unemployment rate increase	
Outcome:	Crisis sector education				Crisis sector education	Lifetime earnings	Crisis sector education	Lifetime earnings
Crisis × Quartile 4	0.003	0.009*	-0.005	-0.003	0.019**	-91128	0.011	-19979
	(0.004)	(0.004)	(0.004)	(0.005)	(0.009)	(91551)	(0.009)	(82338)
Crisis × Quartile 3	0.002	0.006	-0.002	-0.003	0.013	-90181	0.004	-80049
	(0.004)	(0.005)	(0.004)	(0.005)	(0.009)	(90192)	(0.009)	(85356)
Crisis × Quartile 2	0.003	0.006	-0.004	-0.004	0.005	-50107	0.003	-58662
	(0.004)	(0.005)	(0.004)	(0.004)	(0.009)	(90429)	(0.008)	(86109)
Crisis × Quartile 1					-0.022***	144275*	-0.017**	126255*
					(0.008)	(80156)	(0.007)	(70527)
Crisis	-0.000	-0.001	0.003	0.003				
	(0.003)	(0.003)	(0.003)	(0.003)				
Fathers in crisis sect. 1990		✓		✓				
Cohort × County FE	✓	✓	✓	✓	✓	✓	✓	✓
Job loss years					1991–95	1991–95	1991–95	1991–95
Mean dep. var.	0.050	0.070	0.050	0.071	0.075	4,690,000	0.075	4,690,000
Obs.	2,663,762	839,721	2,647,706	833,624	175,428	175,428	174,510	174,510

Note: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Standard errors are clustered on the student's municipality of residence around age 16 level. The first four columns show the interaction effect of residing in a quartile 2–4-exposed municipality around age 16 and being born 1976–1988 and the main linear effect of crisis exposure split by the type of crisis exposure measure used. "Crisis" is standardized to be mean-zero, standard deviation one. The other four columns show treatment effects split by the quartile of crisis severity of the municipality of residence (defined as the employment drop in 1993 relative to 1990, for the full economy, or the increase in unemployment in the municipality the same years). For the last four columns estimating the effects of crisis exposure by quartile, we use the main double difference (DiD) specification comparing the outcome for those experiencing paternal job loss before and after age 16 and also net out the effect of experiencing paternal job loss from any other sector during the same ages.

Table 8. *Effect of crisis sector paternal job loss on students' outcomes, split by sex of the student.*

					2015 outcomes		
Outcome:	Crisis sector education	Lifetime earnings	Earnings age 20–30	Earnings age 30–40	Employed	University education	Divorced
Crisis sect. job loss b. age 16 × Female	0.024***	-92318**	-41581**	-38244	-0.007	-0.001	0.009*
	(0.006)	(43708)	(17827)	(28257)	(0.006)	(0.011)	(0.005)
Crisis sect. job loss before age 16	-0.024***	130357***	63431***	45670**	0.011***	-0.009	-0.003
	(0.005)	(35953)	(15018)	(22880)	(0.004)	(0.007)	(0.003)
Cohort × County FE	✓	✓	✓	✓	✓	✓	✓
Job loss years	1991–95	1991–95	1991–95	1991–95	1991–95	1991–95	1991–95
Mean dep. var.	0.193	6,030,000	1,770,000	3,050,000	0.885	0.284	0.085
Obs.	175,428	175,428	175,428	170,671	166,118	165,251	166,118

Note: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Standard errors are clustered on the student's municipality of residence around age 16 level. The main empirical double difference (DiD) specification is used, where we compare the outcome for those experiencing paternal job loss before and after age 16 while also netting out the effect of experiencing paternal job loss from any other sector during the same ages. "Crisis sector education" denotes having a manufacturing or construction-linked high school education at age 20. "Lifetime earnings" refers to cumulative earnings during the lifetime (up until the final year of our earnings panel in 2017).

Table 9. *Effect of paternal job loss on career choice using alternative bandwidths for the age of experiencing paternal job loss.*

Job loss sector: Outcome:	Crisis sector job loss Crisis sector education			Manuf. sector job loss Manufacturing education			Constr. sector job loss Construction education		
Job loss b. age 16 × Sector	-0.012***	-0.009**	-0.007	-0.008***	-0.007*	-0.005	-0.011***	-0.003	-0.003
	(0.003)	(0.004)	(0.006)	(0.003)	(0.004)	(0.005)	(0.003)	(0.005)	(0.006)
Cohort × County FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Students' job loss ages	10–21	14–17	15–16	10–21	14–17	15–16	10–21	14–17	15–16
Job loss years	1991–95	1991–95	1991–95	1991–95	1991–95	1991–95	1991–95	1991–95	1991–95
Mean dep. var.	0.107	0.107	0.107	0.072	0.072	0.072	0.060	0.060	0.060
Obs.	175,428	56,423	28,114	175,428	56,423	28,114	175,428	56,423	28,114

Note: $***p < 0.01$, $**p < 0.05$, $*p < 0.1$. Standard errors are clustered on the student's municipality of residence around age 16 level. The main empirical double difference (DiD) specification is used, where we compare the outcome for those experiencing paternal job loss before and after age 16 in the specific sector while also netting out the effect of experiencing paternal job loss from any other sector during the same ages. "Crisis sector education" refers to combining the manufacturing and construction educational outcome into one category, while "Manufacturing education" and "Construction education" shows the outcome separately for the two career programs.

Table 10. *Effect of paternal job loss on lifetime earnings using alternative bandwidths for the age of experiencing paternal job loss.*

Job loss sector: Outcome:	Crisis sector job loss Cumulative lifetime earnings			Manuf. sector job loss Cumulative lifetime earnings			Constr. sector job loss Cumulative lifetime earnings		
Job loss b. age 16 × Sector	86664***	54175	74768	72727***	41422	97495	51696*	38436	-3884
	(24697)	(42952)	(55202)	(27248)	(48981)	(60931)	(30408)	(46981)	(62918)
Cohort × County FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Students' job loss ages	10–21	14–17	15–16	10–21	14–17	15–16	10–21	14–17	15–16
Job loss years	1991–95	1991–95	1991–95	1991–95	1991–95	1991–95	1991–95	1991–95	1991–95
Mean dep. var.	5,210,000	5,210,000	5,210,000	4,630,000	4,630,000	4,630,000	4,590,000	4,590,000	4,590,000
Obs.	175,428	56,423	28,114	175,428	56,423	28,114	175,428	56,423	28,114

Note: $***p < 0.01$, $**p < 0.05$, $*p < 0.1$. Standard errors are clustered on the student's municipality of residence around age 16 level. The main empirical double difference (DiD) specification is used, where we compare the outcome for those experiencing paternal job loss before and after age 16 while also netting out the effect of experiencing paternal job loss from any other sector during the same age. "Cumulative lifetime earnings" refers to cumulative earnings during the lifetime (up until the final year of our earnings panel in 2017).

Table 11. *Effect of paternal job loss on the main outcomes including sibling fixed effects.*

Job loss sector:	Crisis sector job loss			Manuf. sector job loss			Constr. sector job loss		
Outcome:	Crisis sector education	Lifetime earnings	Earnings age 20–30	Manuf. education	Lifetime earnings	Earnings age 20–30	Constr. education	Lifetime earnings	Earnings age 20–30
Job loss b. age 16 × Sector	-0.015*	73766	28941	-0.007	150030*	33263	-0.013	-70202	5941
	(0.008)	(72432)	(25871)	(0.008)	(76263)	(31761)	(0.008)	(90875)	(34720)
Sibling FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Job loss years	1991–95	1991–95	1991–95	1991–95	1991–95	1991–95	1991–95	1991–95	1991–95
Mean dep. var.	0.107	5,210,000	1,510,000	0.072	4,630,000	1,530,000	0.060	4,590,000	1,530,000
Obs.	176,045	176,045	176,045	176,045	176,045	176,045	176,045	176,045	176,045

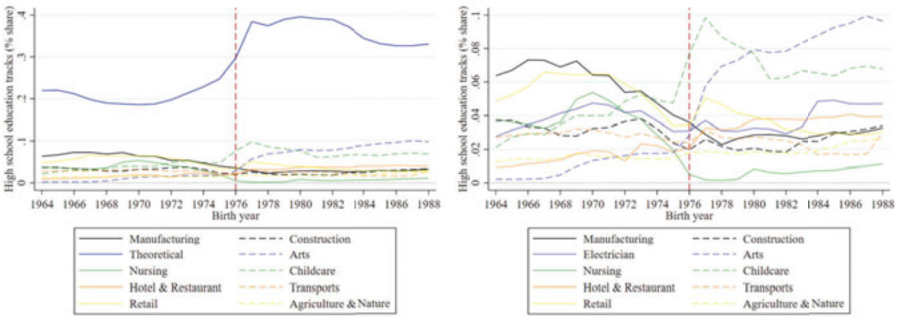
Note: $***p < 0.01$, $**p < 0.05$, $*p < 0.1$. Standard errors are clustered on the student's municipality of residence around age 16 level. The main empirical double difference (DiD) specification is used, where we compare the outcome for those experiencing paternal job loss before and after age 16 while also netting out the effect of experiencing paternal job loss from any other sector during the same age. "Crisis sector education" denotes having a manufacturing or construction-linked high school education at age 20, while "Manuf. education" and "Constr. education" shows the outcome separately for the two career programs. "Lifetime earnings" refers to cumulative earnings during the lifetime (up until the final year of our earnings panel in 2017).

Table 12. *Effect of paternal job loss on career choice outcomes age 20, including additional controls and the triple difference removing job loss effects from the specific sector after the economic crisis.*

Job loss sector:	Crisis sector job loss			Manufacturing sector job loss			Construction sector job loss		
Specification:	DiD Post	3x Diff.	3x Diff.	DiD Post	3x Diff.	3x Diff.	DiD Post	3x Diff.	3x Diff.
Outcome:	Crisis sector education			Manufacturing education			Construction education		
Job loss b. age 16 × Sector	-0.003	-0.009	-0.010	-0.003	-0.006	-0.004	-0.001	-0.010*	-0.010*
	(0.005)	(0.006)	(0.006)	(0.005)	(0.006)	(0.006)	(0.005)	(0.006)	(0.006)
Cohort × County FE		✓			✓			✓	
Cohort × Municipality FE	✓		✓	✓		✓	✓		✓
Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓
Job loss years	1996–99	1991–99	1991–99	1996–99	1991–99	1991–99	1996–99	1991–99	1991–99
Mean dep. var.	0.107	0.107	0.107	0.072	0.072	0.072	0.060	0.060	0.060
Obs.	54,856	229,928	231,514	54,856	229,928	231,514	54,856	229,928	231,514

Note: $***p < 0.01$, $**p < 0.05$, $*p < 0.1$. Standard errors are clustered on the student's municipality of residence around age 16 level. "DiD Post" refers to the double difference specification, where we compare the outcome for those experiencing paternal job loss before and after age 16 and also net out the effect of experiencing paternal job loss from any other sector during the same age for job loss years 1996–1999 after the crisis. "3x Diff." shows the triple difference, where we, in addition, net out the same DiD effect during the years after the crisis (1996–1999). "Controls" refers to including control variables based on parental information from the 1990 census, which includes parental birth cohort, sex of the student, marriage status, age, and high school completion status. "Crisis sector education" refers to combining the manufacturing and construction educational outcome into one category, while "Manufacturing education" and "Construction education" shows the outcome separately for the two career programs.

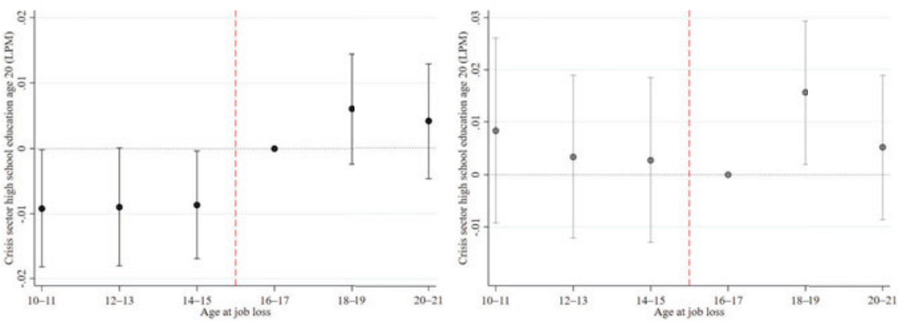
Figures



(a) General trend in high school education program shares, by birth cohort and including the theoretical (science and social science) track.

(b) General trend in vocational high school education, by birth cohort and excluding the theoretical education track.

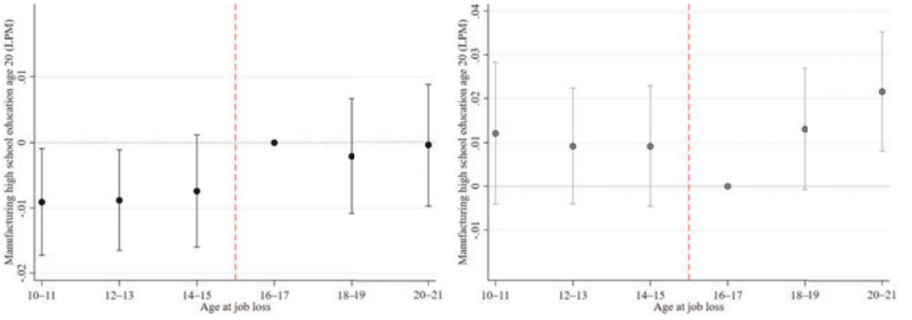
Figure 1. The figures show the general trend in high school programs around age 20, by birth cohort. The individuals excluded from these figures are those who had not completed high school by age 21 (14.4%), and those who had university education already at age 19 (4.5%). Also, some minor programs, unidentifiable programs, and programs that essentially disappeared with the high school reform of 1993/94 (17.3%) are excluded. The dashed red line marks the approximate last cohort not affected by the high school reform of 1993/94.



(a) Crisis sector job loss 1991–1995.

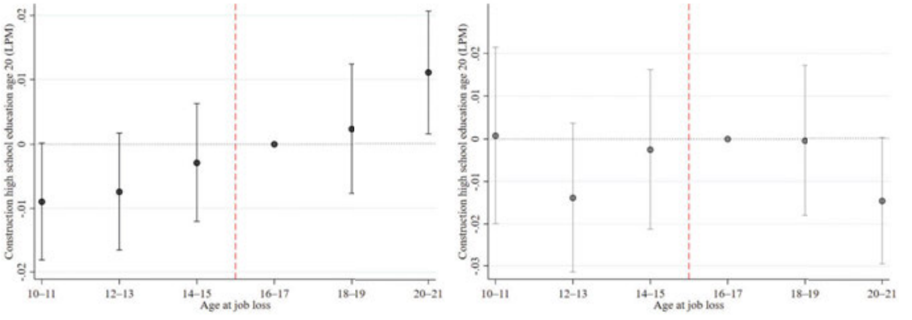
(b) Crisis sector job loss 1996–1999.

Figure 2. The figure shows the effect of paternal crisis sector job loss at a particular age on the student's propensity to have a crisis-linked high school education around age 20. The displayed effects net out the main effect of experiencing job loss at the same age from the father working in any other sector. The outcome is pooling manufacturing and construction-linked high school education age 20. Standard errors are clustered on the municipality of residence at age 16 level. All regressions include cohort-by-county FE. CI95 are indicated in black.



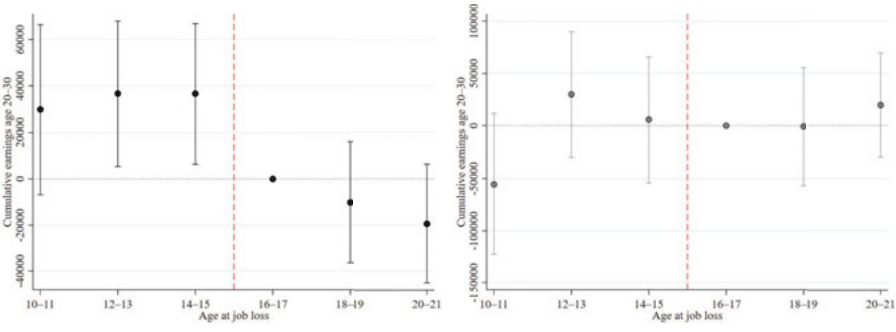
(a) Manufacturing job loss 1991–1995. *(b)* Manufacturing job loss 1996–1999.

Figure 3. The figures show the effect of paternal manufacturing job loss at a particular age on the student's propensity to have a manufacturing-linked high school education around age 20. The displayed effects net out the main effect of experiencing job loss at the same age from the father working in any other sector. The outcome is shown for the main crisis years 1991–1995 and the post crisis years 1996–1999 separately. Standard errors are clustered on the municipality of residence at age 16 level. All regressions include cohort-by-county FE. CI95 are indicated in black.



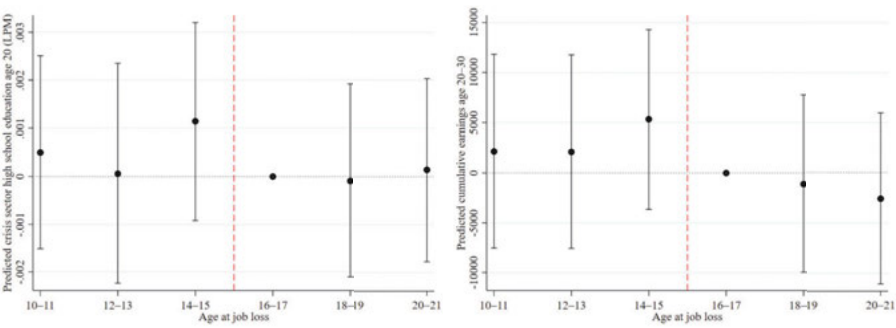
(a) Construction job loss 1991–1995. *(b)* Construction job loss 1996–1999.

Figure 4. The figures show the effect of paternal construction job loss at a particular age on the student's propensity to have a construction-linked high school education around age 20. The displayed effects net out the main effect of experiencing job loss at the same age from the father working in any other sector. The outcome is shown for the main crisis years 1991–1995 and the post crisis years 1996–1999 separately. Standard errors are clustered on the municipality of residence at age 16 level. All regressions include cohort-by-county FE. CI95 are indicated in black.



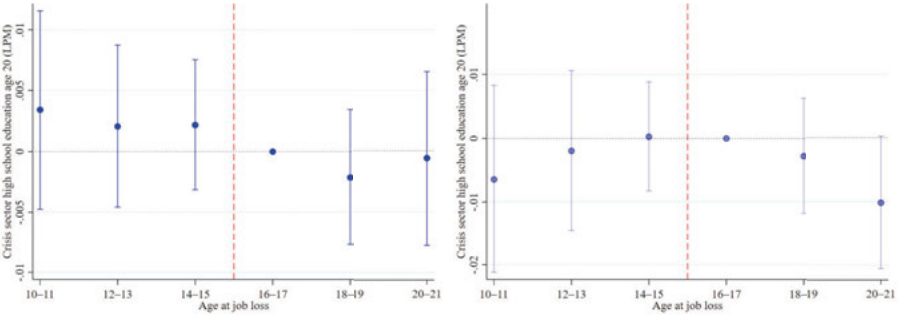
(a) Crisis sector job loss 1991–1995. *(b)* Crisis sector job loss 1996–1999.

Figure 5. The figures show the effect of paternal crisis sector job loss at a particular age on the student's cumulative earnings age 20–30. The displayed effects net out the main effect of experiencing job loss at the same age from the father working in any other sector. Standard errors are clustered on the municipality of residence at age 16 level. All regressions include cohort-by-county FE. CI95 are indicated in black.



(a) Predicted crisis sector education. *(b)* Predicted cumulative earnings age 20–30.

Figure 6. The figures show the effect of paternal crisis sector job loss at a particular age on the student's predicted propensity to graduate from high school with a crisis sector-linked education and the predicted cumulative earnings age 20–30. The characteristics used to predict the outcome are primarily from the 1990 census and the Multi-Generation Register and include parents' year of birth, high school completion status, marriage status, cohort-by-county of the student, parents' number of children, birth order of the student, sex of the student, and indicators of missing values for the characteristics. The adjusted R^2 of the predictions range from 0.074–0.099. The displayed effects net out the main effect of experiencing job loss at the same age from the father working in any other sector. Standard errors are clustered on the municipality of residence at age 16 level. CI95 are indicated in black.



(a) Main effect of job loss 1991–1995. *(b)* Main effect of job loss 1996–1999.

Figure 7. The figure shows the main effect of paternal job loss from any sector other than a crisis-linked one at a particular age on the student's propensity to have a crisis sector-linked high school education around age 20. The outcome is crisis sector-linked high school education age 20 (pooling manufacturing and construction). Standard errors are clustered on the municipality of residence at age 16 level. All regressions include cohort-by-county FE. CI95 are indicated in blue.

Supporting figures

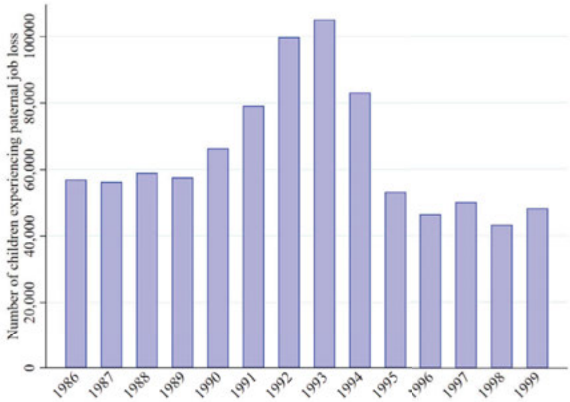
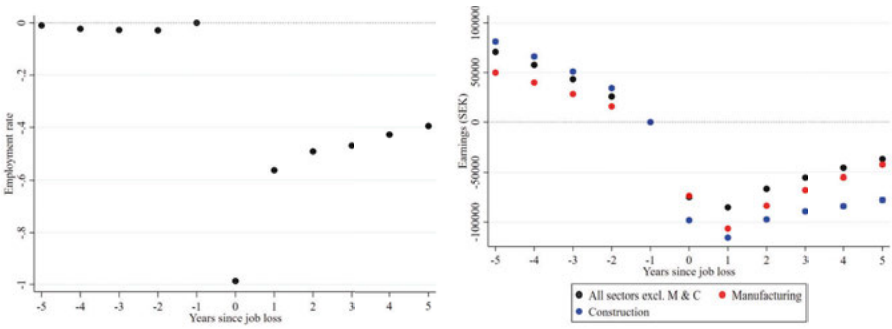


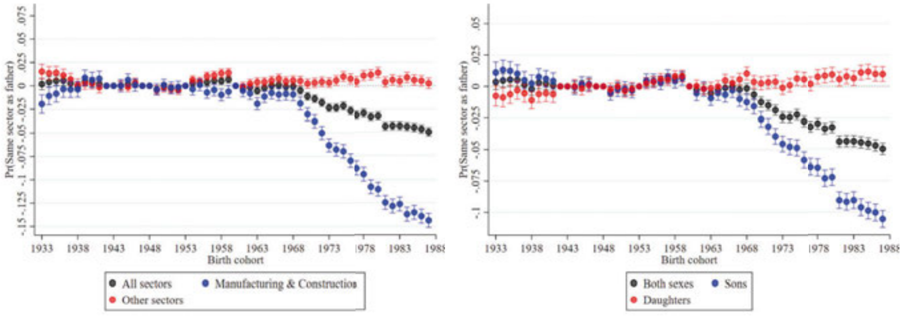
Figure 8. The figure shows the number of children experiencing paternal job loss each year in Sweden during 1986–1999.



(a) Event study of employment rate ± 5 years around the job loss event.

(b) Event study of earnings ± 5 years around the job loss event.

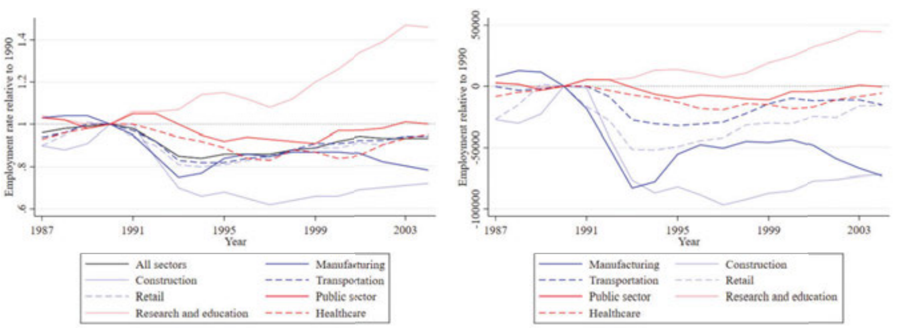
Figure 9. The figures show the evolution of employment rate and earnings in levels relative to the job loss event for all individuals experiencing job loss at ages 20–59 during 1991–1995. Year fixed effects are included in the estimations. CI95 are included in each respective color. The average earnings in 1990 for those experiencing job loss in 1991 is approximately SEK 128,000.



(a) Trend in intergenerational sector of occupation correlation, split by sectors most affected by the economic crisis (manufacturing and construction) and other sectors.

(b) Trend in intergenerational sector of occupation correlation, split by sex of the child.

Figure 10. The figures shows the probability for children at age 30 to work in the same sector as their fathers (around age 50), by birth cohort. The base probability for the reference cohort 1960 is 26% for sons and 13% for daughters. Controls include father's birth cohort effects to ensure that fathers are as similar as possible despite changes in sector definition over the years. The outcome measured during the years 1960–1990 is defined using the censuses every five years until the last census in 1990. After that, we use yearly sector codes based on the main employment of the individual. The level difference in outcome between the two data sources are handled by using cohort 1961 levels as a reference cohort for outcomes after 1960 defined through the yearly employment statistics (mean outcome in 1961 is 20% for sons and 12% for daughters). All outcomes are matched to a specific sector in order for the comparison to be valid over the cohorts, and the regression controls for the mean difference in outcome between the two data sources to estimate the trend correctly.



(a) Employment rate for men by sector of occupation, relative to 1990.

(b) Employment in levels for men by sector of occupation, relative to 1990.

Figure 11. The figures show the evolution of employment rate and employment in levels relative to the last year before the economic crisis hit Sweden. Source: Statistics Sweden.

Appendix A Additional empirical results

Additional tables

Table A1. *Effect of crisis sector paternal job loss on long-run earnings variance and earnings growth.*

Outcome:	Standardized earnings variance		Standardized earn. variance age 20–30		Standardized earn. variance age 30–40		Standardized earnings growth	
Crisis sect. job loss b. age 16	-0.003	-0.004	-0.007	-0.008	-0.001	0.001	0.003	0.004
	(0.004)	(0.004)	(0.005)	(0.005)	(0.005)	(0.005)	(0.010)	(0.010)
Cohort × County FE	✓		✓		✓		✓	
Cohort × Muni FE		✓		✓		✓		✓
Controls		✓		✓		✓		✓
Mean dep. var.	-0.013	-0.013	-0.019	-0.019	-0.013	-0.004	0.001	0.001
Obs.	175,428	176,658	175,042	176,270	169,932	171,138	167,553	167,553

Note: ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$. Standard errors are clustered on the student's municipality of residence around age 16 level. The main empirical double difference (DiD) specification is used, where we compare the outcome for those experiencing paternal job loss before and after age 16 while also netting out the effect of experiencing paternal job loss from any other sector during the same ages. "Standardized earnings variance" denotes the standardized (mean-zero, standard deviation one by cohort) cumulative earnings variance over the life cycle "Standardized earnings growth" denotes the standardized quotient of the cumulative earnings age 30–40 divided by the earnings age 20–30. "Controls" refers to including control variables based on parental information from the 1990 census, which includes parental birth cohort, sex of the student, marriage status, and high school completion status.

Table A2. *Effect of paternal job loss on career choice using an alternative control group.*

Job loss sector:	Crisis sector job loss			Manufacturing sector job loss		Construction sector job loss	
Outcome:	Crisis sector education	Manufacturing education	Construction education	Manufacturing education	Construction education	Construction education	Manufacturing education
Job loss b. age 16 × Sector	-0.008***	-0.006***	-0.002	-0.004*	-0.000	-0.003	-0.010***
	(0.002)	(0.002)	(0.001)	(0.002)	(0.002)	(0.003)	(0.003)
Cohort × County FE	✓	✓	✓	✓	✓	✓	✓
Job loss years	1991–95	1991–95	1991–95	1991–95	1991–95	1991–95	1991–95
Mean dep. var.	0.102	0.062	0.040	0.069	0.030	0.058	0.051
Obs.	809,785	809,785	809,785	531,285	531,285	282,298	282,298

Note: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Standard errors are clustered on the student's municipality of residence around age 16 level. The control group is defined as those with a father in the manufacturing or construction sector in 1990, just before the economic crisis. The outcome is compared against this group and the ones experiencing paternal job loss in the specified sector (crisis sector, manufacturing, or construction sector) before age 16. "Crisis sector education" denotes having a manufacturing or construction-linked high school education at age 20, while "Manufacturing education" and "Construction education" shows the outcome separately for the two career programs.

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