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Anne Sophie Lassen

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Gender in the Labor Market

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CBS

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Copenhagen, September 2022

Summary

This thesis examines the link between gender norms, parenthood, and labor market segregation. It does so by focusing on three different aspects of gender equality: the role of parental leave policies in reducing gender gaps, men's and women's ability to adjust to labor market shocks, and role models' influence on labor market choices. The thesis consists of four independent chapters that are placed at the intersection of labor and gender economics. The first chapter presents a survey of a large and active literature on the causal studies of parental leave. The three following chapters employ causal frameworks to Danish register data to document and explain gender gaps in labor market outcomes. Each chapter uses the tool of applied micro-econometrics that has become central in the field of economics. Combined, these different angles shed new light on how parenthood, labor market shocks, and gender segregation contribute to gender inequality. Specifically, the thesis contributes by bringing in gender norms as a mechanism for the persistent gender gaps following parenthood and in labor market segregation.

Chapter 1 presents a survey of a large and active literature on the causal studies of parental leave and is joint work with Herdis Steingrimsdottir, Philip Rosenbaum, and Serena Canaan. Labor market policies for expecting and new mothers emerged at the turn of the nineteenth century. The main motivation for these policies was to ensure the health of mothers and their newborn children. With increased female labor market participation, the focus has gradually shifted to the effects that parental leave policies have on women's labor market outcomes and gender equality. Proponents of extending parental leave rights for mothers in terms of duration, benefits, and job protection have argued that this will support mothers‘ labor market attachment and allow them to take time off from work after childbirth and then safely return to their pre-birth job. Others have pointed out that extended maternity leave can work as a double-edged sword for mothers: If young women are likely to spend months, or even years, on leave, employers are likely to take that into consideration when hiring and promoting their employees. These policies may therefore end up adversely affecting women's labor market outcomes. This has led to an increased focus on activating fathers to take parental leave.

A large number of studies on the topic have brought forth some consistent findings. First, the introduction of short maternity leave is found to be beneficial for both maternal and child health and mothers' labor market outcomes. Second, there appear to be negligible benefits from a leave extending beyond six months in terms of health outcomes and children's long-run outcomes. Furthermore, longer leaves have little, or even

adverse, influence on mothers' labor market outcomes. However, some evidence suggests that there may be underlying heterogeneous effects from extended leaves among different socioeconomic groups. The literature on the effect of earmarked paternity leave indicates that these policies prove effective in increasing fathers' leave-taking and involvement in childcare. However, the evidence on the influence of paternity leave on gender equality in the labor market remains scarce and somewhat mixed. Finally, recent studies that focus on the effect of parental leave policies for a firm find that in general, firms are able to compensate for lost labor when their employees go on leave. However, if firms face constraints when replacing employees, it could negatively influence their performance.

Chapter 2 studies the role of parental leave policies in gender gaps following parenthood. The arrival of children implies a sharp reduction in mothers' earnings and labor supply while fathers' labor market trajectories are unaffected. While this divergence is well documented, it is poorly understood. To understand this specialization, an unexpected and rapid institutional change in Denmark is utilized. In 2002, a parental leave reform increased the duration of parental leave with well-paid leave by 22 weeks. At the same time, policy makers removed two weeks of earmarked leave specifically allocated to fathers. With the reform, the household could decide how to distribute the extended parental leave within the couple. ask which factors influence take-up. The analysis is two-fold. First, the reform effect and the role of relative earnings within couples are investigated. Second, the link between leave behavior and family behavior is evaluated. This is done by investigating inter-generational spillover in time allocation, and by identifying sister pairs to evaluate the transmission of leave behavior.

Upon reform implementation, mothers increase their leave by 5 weeks while the average leave duration of fathers is unchanged. Contrary to the predictions from a standard model of house specialization, estimates are largely unaffected by relative earnings in the household. Instead, the results are highly consistent with the notion of gender identity. Women who had a mother with a high labor supply take a shorter leave compared to those who had a stay-at-home mother. Moreover, there are sizeable peer effects among sisters. Women with a sister in the reform treatment group take on average take 1.1 weeks longer leave compared to those with a sister in the reform control group. This corresponds to peer effects of 17 %. Importantly, all these women give birth at least 9 months after the reform was introduced and face identical institutional settings. The reform-induced change in leave duration implies that women with a sister in the reform treatment group face a new norm of extensive leave. This shows up here as peer effects and reaffirms gender-specific intra-household specialization.

Chapter 3 is joint work with Ria Ivandic, and investigates the effects of *women's* and *men's* job loss on future labor market outcomes. While it is well-established that job loss leads to persistent adverse labor market outcomes, assessments of gender differences in labor market recovery are lacking. The literature provides several potential explanations for why there may exist substantial gender gaps after job loss. One important factor is the constraint that may child care impose on women's labor market trajectory. Another important source of overall gender gaps is differences in human capital, broadly defined to include education, occupation, and other types of sorting in the labor market.

By using plant closures in Denmark, we estimate gender gaps in labor market outcomes and document that women face an increased risk of unemployment in the two years following job displacement. Women on average experience a 14.2 % point increase in the probability of unemployment over the first two years, while for men this is lower at a 9.8 % point. Relatively, this amounts to a 45 percent greater unemployment shock for women. While the absolute drop in earnings is larger for men, they have higher initial earnings and overall lose a smaller relative share of their income. When comparing similar men and women, the conditional gaps are reduced, but never fully closed. To disentangle why women are consistently worse off, we turn to the relative importance of human capital and the role childcare plays. Observable differences explain approximately half of the gender gap in unemployment. Pre-displacement earnings and education are particularly important characteristics. In addition, both the absolute and the unexplained parts of the gender gap grow in the presence of children, suggesting that childcare imposes a barrier to women's recovery regardless of individual characteristics. Finally, initial sorting across occupations and sectors barely affects the gender gap in unemployment following displacement.

Chapter 4 is concerned with persistent labor market segregation. Many western economies have seen a fall in the employment share of the traditionally male-dominated, manufacturing sector, while demand is increasing in female-dominated jobs. Still, men appear reluctant to enter these occupations. This highlights the importance of an improved understanding of persistent gender segregation in the labor market. In particular, the notion of masculinity and related gender norms may impose a potential source of adjustment friction.

To understand the gender norms that are influencing men, same-sex role models are explored and particular attention is paid to fathers and schoolmates' fathers. To this end, I construct measures of gender-stereotypical behavior of parents, schoolmates' parents, and wider municipal measures of norms. These measures are intended to capture transmitted norms of the 'appropriate role' of men and women within communities and families. First, I estimate the parent-child associations of the share of women in their occupations separately for boys and girls, and mothers and fathers. Second, within-school-across-cohort variation in the gender composition of the occupations of the schoolmates' parents is used to document that gender segregation is transmitted from one generation to the next. Boys who were exposed to gender-stereotypical male role models enter male-dominated occupations, while those socialized in cohorts with peers whose fathers work alongside women enter occupations with more women. This effect goes beyond the influence of their fathers. In general, mothers' labor market behavior has negligible effects on their sons. In contrast, girls are mainly influenced by female role models, and compared to boys the effects are much smaller. However, when a larger share of mothers works full-time gender segregation decreases in the next generation.

Dansk Resumé

Denne ph.d.-afhandling undersøger, hvordan forældreskab, kønsnormer og kønsopdeling på arbejdsmarkedet påvirker ligestilling. Afhandlingen består af fire uafhængige kapitler, der alle omhandler kønsforskelle på arbejdsmarkedet og i familien. Således kan alle kapitlerne placeres i krydsfeltet mellem arbejdsmarkedsøkonomi og familieøkonomi. Første kapitel indeholder en gennemgang af den forskning, der beskæftiger sig med effekterne af indførelse af barsels- og forældreorlov. De tre efterfølgende kapitler anvender dansk registerdata og værktøjer fra den anvendte mikroøkonometri, som er blevet centrale i det økonomiske forskningsfelt, til at foretage kausale analyser. Tilsammen kaster analyserne nyt lys over, hvordan forældreskab, arbejdsmarkedschok og kønsopdelte arbejdsmarkeder bidrager til ulighed mellem mænd og kvinder. Et af afhandlingens centrale bidrag er at inddrage kønsnormer som en mekanisme til at forklare den ulighed, der opstår omkring forældreskabet og på det kønsopdelte arbejdsmarked.

Kapitel 1 indeholder en gennemgang af det store og aktive forskningsfelt, der undersøger kausale effekter, som følger af introduktionen af barsels- og forældreorlov. Det er udarbejdet i samarbejde med Herdis Steingrimsdottir, Philip Rosenbaum og Serena Canaan. De første barselspolitikker for gravide og nybagte mødre blev indført i begyndelsen af det nittende århundrede. Motivationen var primært at forbedre mødres og nyfødtes helbred. I takt med at kvinders arbejdsmarkedsdeltagelse er steget, er fokus dog gradvist flyttet til ligestilling på arbejdsmarkedet. Fortalere for at forlænge barsel og orlov for mødre har argumenteret for, at dette vil understøtte mødres tilknytning til arbejdsmarkedet. Barsel med jobsikring og offentlige ydelser vil give kvinder mulighed for at vende tilbage til deres job efter fødsel og barsel. På den anden side har andre påpeget, at en lang barselsorlov kan blive et tveægget sværd: Hvis unge kvinder forventes at bruge måneder eller endda år på barsel, vil arbejdsgiverne tage det i betragtning, når de ansætter og forfremmer. Lang barsel kan derfor påvirke kvinders arbejdsmarkedsresultater negativt. De seneste år har der derfor været et øget fokus på at involvere fædre i barselsordninger.

Et stort antal undersøgelser af barsels- og orlovspolitikker på tværs af lande har produceret en række enslydende resultater. Indførelsen af kort barselsorlov har en gavnlig effekt på både mødres og børns helbred og på mødres arbejdsmarkedstilknytning og indkomst. Når orloven forlænges til længere end seks måneder, er der tale om meget små positive effekter på helbred og børns langsigtede resultater i fx skolen. En længere orlov har ingen eller endda negativ indflydelse på mødres arbejdsmarkedsresultater. Der kan dog være forskelligartede effekter på tværs af socioøkonomiske grupper. Øremærket fædreorlov er effektiv til at øge fædres brug af orlov og involvering i børnepasning, men forskning med fokus på ligestilling på arbejdsmarkedet er dog stadig sparsom, og resultaterne er blandede. Endelig viser nyere forskning, at virksomheder generelt er i stand til at omstille sig, når ansatte går på barsel ved fx at ansætte vikarer eller øge brugen af overtid. I de tilfælde, hvor virksomheder har svært ved at tilpasse sig, kan det påvirke dem negativt.

Kapitel 2 undersøger også barselslovgivningens rolle i den ulighed, der opstår omkring forældreskabet. For kvinder er forældreskabet forbundet med en betydelig reduktion i indkomst og arbejdstimer, mens nye fædres arbejdsliv stort set er upåvirket. For at forstå det mønster evaluerer jeg en barselsreform i Danmark. I 2002 forøgede man den økonomiske kompensation som nye forældre kunne modtage, mens de var på barsel. Samtidig fjernede man to ugers øremærket barsel til fædre. I praksis betød det en forlængelse af forældreorloven, og familier kunne selv bestemme, hvordan den nye orlov skulle fordeles forældrene imellem. Jeg undersøger, hvilke faktorer der påvirker brug og fordeling af barsel. Først evalueres den rolle, som den relative indkomst spiller i parret. Derefter undersøges, hvordan orlov påvirkes af øvrige familiemedlemmers adfærd. Ved at bruge historiske oplysninger om mødres arbejdsmarkedsdeltagelse undersøges intergenerationel transmission af normer. For at forstå reformens bredere effekter analyseres barselslængden hos kvinder, der får et barn, efter reformen er udrullet, og hvis søster var påvirket af reformen.

Reformen medførte, at mødre forlængede deres orlov med 5 uger, mens fædres gennemsnitlige orlovslængde var uændret. Der er ingen forskel på tværs af relativ indkomst parret imellem. Det står i kontrast til, hvad en model med udgangspunkt i komparative fordele vil forudsige. I stedet er resultaterne i overensstemmelse med, hvad en model med udgangspunkt i kønsidentitet vil forudsige: Kvinder, der voksede op med mor med en mange timer på arbejdsmarkedet, reagerede mindre på reformen end dem, som voksede op med en mor med en lavere arbejdstid. Derudover dokumenteres en afsmittende effekt fra søster til søster: Kvinder med en søster, der blev påvirket af reformen, holdt i gennemsnit 1,1 uge længere orlov sammenlignet med dem, hvis søstre akkurat ikke blev påvirket af reformen. Dette svarer til en peer-effekt på 17 %. Alle disse kvinder fødte selv mindst 9 måneder efter reformen blev indført. Reformen forstærkede således ulighed mellem mænd og kvinder – både gennem den direkte reformeffekt og ved at skabe en ny norm, som blev spredt igennem sociale netværk.

Kapitel 3 er udarbejdet i samarbejdet med Ria Ivandic og undersøger, hvordan kønsforskelle i ledighed og lønninger udvikler sig efter fyringer. Det er veldokumenteret, at det at blive fyret har langvarige negative konsekvenser, men der mangler systematiske vurderinger af, hvorvidt mænd og kvinder påvirkes forskelligt og hvorfor. Der er flere grunde til, at mænd og kvinder kan have forskellige forudsætninger for at omstille sig, hvis de oplever at blive fyret. For det første er der en ulige fordeling af ansvar for børn, som kan skabe en udfordring for kvinder, når de skal finde et nyt job. For det andet er der store forskellige i mænd og kvinders udgangspunkt: Mænd og kvinder arbejder i forskellige fag, i forskellige typer af virksomheder og har forskellige uddannelser. Med udgangspunkt i fabrikslukninger i den danske produktionssektor dokumenteres det, at kvinder har en større risiko for langtidsledighed i de første to år efter fyringen. Mens risikoen for ledighed er 9,8 procent for mænd, er den tilsvarende risiko 14,2 procent for kvinder. Mænd oplever et større absolut fald i løn, men de tjener også mere, før de bliver fyret, så kvinder mister en større andel af deres løn. Sammenlignes mænd og kvinder med samme uddannelse og erhvervserfaring er forskellen halveret. For at forstå vigtigheden af børnepasning i forhold til uddannelse, løn og erhvervserfaring foretages der en dekomponering af forskellen i risiko for arbejdsløshed. Tidligere løn og uddannelse spiller en stor rolle, og da kvinderne i gennemsnit har mindre uddannelse end mændene, er de mere udsatte. Når mænd og kvinder med hjemmeboende børn sammenlignes, stiger både den absolutte forskel og den del af uligheden, som ikke kan forklares med uddannelse, løn og erhvervserfaring. Det tyder på, at ansvar for børn udgør en barriere for kvinders muligheder for at finde et nyt arbejde. Endeligt viser vi, at selvom mænd og kvinder generelt er ansat i forskellige virksomheder, forklarer det stort set ikke kønsforskellen i risiko for ledighed og fald i løn.

Kapitel 4 fokuserer på den vedvarende kønsopdeling af arbejdsmarkeder. I mange vestlige lande har der gennem de sidste årtier været et fald i andelen af arbejdsstyrken, som er beskæftiget i produktion og andre mandsdominerede sektorer. Samtidig er efterspørgslen efter arbejdskraft stigende i såkaldte kvindefag i sundheds- og servicesektoren. Mænd ser dog ud til at være tilbageholdende ift. at arbejde i de sektorer. Det understreger vigtigheden af en forbedret forståelse for kønsopdelte arbejdsmarkeder, og især hvilken rolle forestillinger om maskulinitet og relaterede kønsnormer spiller for mænd. I kapitlet undersøges indflydelsen af rollemodeller af samme køn – mere specifikt undersøges den rolle, forældre og klassekammeraters forældre spiller. Først konstrueres mål for 'kønsstereotypisk' adfærd i forældregenerationen: andelen af kvinder i forældrenes fag samt mødres arbejdsmarkedsdeltagelse. Derefter estimeres forældre-barn korrelationen i andelen af kvinder i ens fag. Dette gøres separat for fædre og mødre og drenge og piger. I det næste skridt udnyttes variationen i klassekammeraters forældres arbejdsmarkedsadfærd inden for skoler og på tværs af årgange. Denne variation bruges til at estimere en kausal effekt af eksponering for kønsstereotyp adfærd.

Drenge, der socialiseres i et miljø med kønsstereotype mandlige rollemodeller, arbejder selv i mere mandsdominerede fag. Drenge, som socialiseres i miljøer, hvor flere fædre arbejder i fag med kvinder, kommer også selv til at arbejde i fag med flere kvinder. Indflydelsen fra klassekammeraternes fædre er separat fra effekten fra ens egen far. Generelt har mødres arbejdsmarkedsadfærd kun en lille effekt på drenge, men når en større andel af mødre arbejder fuld tid, reducerer det kønsopdelingen på arbejdsmarkedet i den næste generation. Piger påvirkes hovedsagligt af kvindelige rollemodeller, men effekterne er mindre end for drenge.

Nøgleord: arbejdsmarkedsøkonomi, familieøkonomi, anvendt mikroøkonometri, ligestilling, barsel, fyringer, normer

Contents

Introduction

In the novel Orlando: A Biography, Virginia Woolf (1928) tells the story of the nobleman Orlando. By the age of 30, he undergoes a mysterious change of sex and lives the rest of his life as a woman. As a woman, Orlando soon learns that certain acts, behaviors, and opportunities are no longer suited for her, while others are expected. This leaves her in despair:

"The comforts of ignorance seemed utterly denied her. She was a feather blown on the gale. Thus it is no great wonder, as she pitted one sex against the other, and found each alternately full of the most deplorable infirmities, and was not sure to which she belonged–it was no great wonder that she was about to cry out" (Woolf, 1928, p. 77-78).

As economists, we might think of these *deplorable infirmities* in other ways than Orlando does, but we are well aware of gender differences in behavior and inequalities in outcomes.[1] While men and women's roles in society have converged over the last century, substantial gender gaps remain (e.g. Bertrand, 2011; Goldin, 2014; Lundberg, 2022).

This thesis examines the link between gender norms, parenthood, and labor market segregation. I approach the question of gender equality from different angles to shed new light on how parenthood, labor market shocks, and gender segregation contribute to gender inequality. The thesis consists of four independent chapters that all address gender gaps in labor market outcomes, and are all placed at the intersection of labor and gender economics. The first chapter presents a survey of a large and active literature on the causal studies of parental leave. The following chapters each focus on different aspects of gender equality: the role of parental leave policies in reducing gender gaps, men's and women's ability to adjust to labor market shocks, and role models' influence on labor market outcomes. Each of these chapters employs a causal framework to document and explain gender gaps in labor market outcomes and does so by using the tool of applied micro-econometrics that has become central in the field of economics. Specifically, the thesis contributes by bringing in gender norms as a mechanism for persistent gender gaps.

[1]This is also how I began my Master's Thesis (Lassen, 2019).

Gender Norms

There exists a direct link between gender norms and gender inequality, and this link is receiving increased attention in the field of gender economics (see reviews by Giuliano, 2021; Bertrand, 2020; Lundberg, 2022). Lundberg (2022) recently wrote:

"It's time to be more intentional about incorporating social forces into economics, and this is particularly true of work of gender. Understanding the social construction and enforcement of gender roles is a central component of this endeavour." (p. 18)

As the notion of gender norms makes its way into economics it is useful to pay attention to insights from the other social sciences. A useful example of this is the work by sociologists Pearse and Connell (2016), who engage with the use of gender norms among economists.

An important point to recognize is that gender norms are neither static nor monolithic. In applications of gender norms, gender roles, and social role theory, economists often - either explicitly or implicitly - assume a strong consensus in society. Pearse and Connell (2016) show how treating norms as "structures of collective constraints" as formulated by Folbre (1994) is not convincing. Among other things, this definition fails to acknowledge the within-society variation regarding gender-related attitudes.

Moreover, the concepts of gender norms, gender roles, and social role theory are often used interchangeably. In addition, concepts like socialization and social reproduction are often mentioned but rarely discussed in detail. The bundling of these concepts proposes a smooth path for gender norms to be transmitted from one generation to the other. Among other things, this removes agency from the socialized. Pearse and Connell (2016) argue that pairing these concepts with the idea of social consensus comes with the risk that norms are modeled to be static. It implies that compliance is a rational strategy. However, Pearse and Connell (2016) show that neither strong consensus nor passivity on the socialized part is found in practice.

A second point to recognize is that gender norms are not just attitudes held by individual members of society. Drawing on insights from the broader social sciences, Pearse and Connell (2016) argue that while gender norms can be stated abstract (e.g. in value surveys), what matters is the way norms function in social life. Treating norms as something that inhabits the mind of individuals (and not too different from preference), overlooks norms as the pillars of a community or society, and how they are embedded in institutions: whether that is organizations, public policies, or collective identities. Parental leave policies provide a useful example of this.

Parental Leave Policies

Parenthood and persistent gender inequalities are intricately linked. In high- and middle-income countries, parenthood is the main driver of gender inequality in the labor market. New mothers face large and persistent drops in earnings and labor supply, while fathers' labor market trajectories are unaffected (e.g. Ejrnæs and Kunze, 2013; Angelov et al., 2016; Kleven et al., 2019). To help mothers balance work and family considerations, most countries have implemented some sort of maternity leave system.

Chapter 1 contains a survey of the causal literature on the effect of maternity, parental, and paternity, and is joint work with Herdis Steingrimsdottir, Philip Rosenbaum, and Serena Canaan. Labor market policies for expecting and new mothers emerged at the turn of the nineteenth century. The main motivation for these policies was to ensure the health of mothers and their newborn children. With increased female labor market participation, the focus has gradually shifted to the effects that parental leave policies have on women's labor market outcomes and gender equality. Recently, there has been an explicit aim to involve fathers.

A large and growing number of studies on the topic has brought forth some consistent findings. First, the introduction of short maternity leave is found to be beneficial for both maternal and child health and mothers' labor market outcomes. Second, there appear to be negligible benefits from a leave extending beyond six months in terms of health outcomes and children's long-run outcomes. Furthermore, longer leaves have little, or even adverse, influence on mothers' labor market outcomes. However, some evidence suggests that there may be underlying heterogeneous effects from extended leaves among different socioeconomic groups. The literature on the effect of earmarked paternity leave indicates that these policies prove effective in increasing fathers' leave-taking and involvement in childcare. However, the evidence on the influence of paternity leave on gender equality in the labor market remains scarce and somewhat mixed. Finally, recent studies find that in general, firms are able to compensate for lost labor when their employees go on leave. However, if firms face constraints when replacing employees, it could negatively influence performance.

In **Chapter 2**, I contribute with an improved understanding of the striking gender gaps following parenthood. I do this by exploiting an unexpected and rapid institutional change in Denmark. In 2002, a parental leave reform improved the economic compensation new parents received while on leave, effectively increasing the leave duration. At the same time, policy makers removed two weeks of earmarked leave specifically allocated to fathers. With the reform, the household could decide how to distribute the extended parental leave within the couple. I evaluate the reform effect on leave duration for the universe of eligible parents and ask which factors are influencing take-up. First, I investigate the role of relative earnings within couples. Second, I evaluate how behavior is affected by gender norms. I explore inter-generational spillover in time allocation by linking historical information on maternal labor supply for women in the reform window. Further, I identify sisters who have a child after the reform and investigate peer effects on leave duration. This explicitly highlights the social aspects of norms and how they interact with institutions.

Upon reform implementation, mothers increase their leave by 5 weeks while the average leave duration of fathers remains unchanged, irrespective of relative earnings. Consistent with the role of gender identity, women who had a working mother take a shorter leave than those with a stay-at-home mother. Moreover, I document peer effects among sisters who take a longer leave if exposed to the reform-induced change in leave duration. While many modern parental leave policies have the explicit goal of allowing women to combine paid work and family, my results suggest that long maternity leave policies may not be effective. I show that a general extension of parental leave reinforces an existing gender gap in time allocation as a result of both the reform effect and subsequent peer effect.

Labor Market Shocks

When the responsibility of childcare falls disproportionately on women, it likely imposes constraints on how women can adjust to labor market shocks. Job loss is a shock that leads to persistently lower earnings and higher unemployment rates in the long run (Jacobson et al., 1993; Lachowska et al., 2020) but gender differences in labor market recovery following job loss remain unexplored.

Chapter 3 investigates the effects on women's and men's job loss and is joint work with Ria Ivandic. Besides the constraint child care may impose on women's labor market trajectory, there are meaningful gender gaps in human capital, broadly defined to include education, occupation, and other types of sorting in the labor market. We try to disentangle the roles these two channels play for recovery following job loss.

We utilize plant closures in Denmark to estimate gender gaps in labor market outcomes and document that women face an increased risk of unemployment in the two years following job displacement. Women on average experience a 14.2 % point increase in the probability of unemployment over the first two years, while for men this is lower at a 9.8 % point. Relatively, this amounts to a 45 percent greater unemployment shock for women. While the absolute drop in earnings is larger for men, they have higher initial earnings and overall lose a smaller relative share of their income. When we compare similar men and women, the conditional gaps are reduced, but never fully closed. To disentangle why women are consistently worse off, we turn to the relative importance of human capital and the role childcare plays. Observable differences explain approximately half of the gender gap in unemployment. Pre-displacement earnings and education are particularly important characteristics. Women are particularly over-represented among workers with little formal education and they are worse off than their male counterparts. In addition, both the absolute and the unexplained parts of the gender gap grow in the presence of children, suggesting that childcare imposes a barrier to women's recovery regardless of individual characteristics. Finally, we show that initial sorting across occupations and sectors barely affects the gender gap in unemployment following displacement.

Labor Market Segregation

Finally, labor market segregation contributes to the gender wage gap. Women are over-represented in low-paid professions, and under-presented in high-paying professions (Cortes & Pan, 2017). However, male-dominated, manufacturing jobs are disappearing in many western economies and female-dominated sectors are growing (Petrongolo & Ronchi, 2020). While women have entered and altered many previously male-dominated occupations (Goldin, 2014; Goldin, 2015; Pan, 2015), men appear more reluctant to enter feminized occupations. This highlights the importance of an improved understanding of persistent gender segregation in the labor market. In particular, the notion of masculinity and related gender norms may impose a potential source of adjustment friction.

In **Chapter 4**, I investigate how the gender composition of occupations transmits across generations. I explore the influence of same-sex role models and pay particular attention to fathers and schoolmates' fathers for understanding the gender norms that are influencing men. To this end, I construct measures of gender-stereotypical behavior of parents, schoolmates' parents, and wider municipal measures of norms. These measures are intended to capture transmitted norms of the 'appropriate role' of men and women within communities and families. First, I estimate the parent-child associations of the share of women in their occupations separately for boys and girls, and mothers and fathers. Second, I exploit within-school-across-cohort variation in the gender composition of the occupations of the schoolmates' parents, and document that gender segregation is transmitted from one generation to the next.

Boys who were exposed to gender-stereotypical male role models enter male-dominated occupations, while those socialized in cohorts with peers whose fathers work alongside women enter occupations with more women. This effect goes beyond the influence of their fathers. In general, mothers' labor market behavior has negligible effects on their sons. In contrast, girls are mainly influenced by female role models, and compared to boys the effects are much smaller. However, when a larger share of mothers works full-time gender segregation decreases in the next generation.

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CHAPTER 1

Maternity Leave and Paternity Leave: Evidence on the Economic Impact of Legislative Changes in High Income Countries

with Serena Canaan, Philip Rosenbaum & Herdis Steingrimsdottir

Labor market policies for expecting and new mothers emerged at the turn of the nineteenth century. The main motivation for these policies was to ensure the health of mothers and their newborn children. With increased female labor market participation, the focus has gradually shifted to the effects that parental leave policies have on women's labor market outcomes and gender equality. Proponents of extending parental leave rights for mothers in terms of duration, benefits, and job protection have argued that this will support mothers' labor market attachment and allow them to take time off from work after childbirth and then safely return to their pre-birth job. Others have pointed out that extended maternity leave can work as a double-edged sword for mothers: If young women are likely to spend months, or even years, on leave, employers are likely to take that into consideration when hiring and promoting their employees. These policies may therefore end up adversely affecting women's labor market outcomes. This has led to an increased focus on activating fathers to take parental leave, and in 2019, the European Parliament approved a directive requiring member states to ensure at least two months of earmarked paternity leave. The literature on parental leave has proliferated over the last couple of decades. The increased number of studies on the topic has brought forth some consistent findings. First, the introduction of short maternity leave is found to be beneficial for both maternal and child health and for mothers' labor market outcomes. Second, there appear

to be negligible benefits from a leave extending beyond six months in terms of health outcomes and children's long-run outcomes. Furthermore, longer leaves have little, or even adverse, influence on mothers' labor market outcomes. However, some evidence suggests that there may be underlying heterogeneous effects from extended leaves among different socioeconomic groups. The literature on the effect of earmarked paternity leave indicates that these policies prove effective in increasing fathers' leave-taking and involvement in childcare. However, the evidence on the influence of paternity leave on gender equality in the labor market remains scarce, and somewhat mixed. Finally, recent studies that focus on the effect of parental leave policies for a firm find that in general, firms are able to compensate for lost labor when their employees go on leave. However, if firms face constraints when replacing employees, it could negatively influence their performance.

1.1 Introduction

One of the most notable changes in the labor market over the last century has been the significant rise in female labor force participation. As a result, parental leave systems have become increasingly important and are now a critical component of labor market policies in most high-income countries. Today, all OECD countries apart from the United States have federally funded parental leave programs. However, these programs vary substantially in terms of key features, such as duration and benefits. For example, the length of paid parental leave in Spain and the Netherlands is 16 weeks, while the total paid leave in countries such as Finland, Hungary, Estonia, and the Slovak Republic is more than 160 weeks. Moreover, policies have changed significantly and rapidly over the last decades. In 1980, the average duration of paid leave in the OECD was 14 weeks, compared to over 53 weeks on average in 2018. Another major change in parental leave systems is the recent focus on incentivizing fathers to take leave. Norway was the first country to introduce a fathers' quota (paternity leave earmarked for fathers) in 1993 and was soon followed by other countries such as Sweden, Iceland, and Spain. In 2015, three-quarters of OECD countries provided at least a few days of paid leave that can only be used by a father, and in 2019, the European Parliament approved a directive requiring member states to ensure at least two months of earmarked paternity leave.

Evaluating the effects of different parental leave policies is a complex task. First, the aim of parental leave policies is multifaceted. Initially, the main motivation for parental leave provisions was to ensure the health and survival of infants and to allow mothers to recover after childbirth. More recently, increased attention has focused on the influence of family policies on labor market outcomes and gender equality. Additionally, governments need to consider other factors such as firms' productivity and government expenditures. Second, the effects of parental leave policies can depend significantly on the setting. More specifically, the impact of extending maternity leave can depend on social norms, availability of daycare, and how long the initial leave is, and it can vary by demographic group as well. Further, parental leave policies can vary along several dimensions, such as the length of leave, benefits, eligibility, and division of childcare responsibilities between parents.

This review focuses on studies that allow for causal inference. Many of the articles included in this review apply regression discontinuity (RD), a difference-in-differences (DD) approach, or a combination of the two, to study the effects of policy reforms. When done well, and with appropriate data, using within-country policy changes has important advantages. When policy reforms happen unexpectedly, there is limited scope for manipulation into treatment and arguably little reason for concern about omitted variable bias. The studies therefore provide a causal estimate of the effects of the policy change on those parents who just became eligible compared to those just rendered ineligible. However, these studies also have limitations that are important to keep in mind. First, they do not capture broader effects. For example, a parental leave policy can affect employers' expectations about the behavior of *all* women in the labor market, which will affect outcomes for both the treatment and the control group. Second, these studies rely on the immediate effectiveness of the policies. If, for example, following a reform introducing earmarked paternity leave, fathers' use of paternity leave increases only gradually, a study applying RD or DD may underestimate the true impact of the policy. Finally, it is important to keep in mind that these studies estimate the average treatment effect, referring to the weighted average effect on people who changed their behavior after the policy reform and those who did not. Any effect on the outcome of interest depends on the uptake rates and which part of the population exhibits the behavioral change.

The studies reviewed in this chapter focus on different settings and time periods. Dissimilarities in labor market policies, social norms, and other factors can play a significant role in outcomes and interact in various ways with different parental leave systems. However, the review highlights some key findings in the literature that are remarkably consistent across contexts. First, the introduction of short leaves is found to be beneficial, both in terms of health outcomes for mothers and their children, and in terms of mothers' labor market outcomes. Second, there are negligible benefits of leave beyond six months in terms of health outcomes. Longer leave often has an adverse effect on mothers' wages and employment, while appearing to have little effect on children's long-term outcomes. However, there is some evidence that there may be underlying heterogeneous effects for different socioeconomic groups.

The results on the effects of paternity leave are more mixed. While earmarked paternity leave proves effective in terms of increasing the uptake rate of fathers, the magnitude of its success in doing so varies tremendously across settings, and the evidence of its effect on labor market outcomes for both men and women is mixed. However, the majority of studies find that introduction of earmarked paternity leave increases paternal involvement in childcare, but to a lesser extent in other household tasks. If fathers are more involved in childcare, this might affect child outcomes, but very few studies investigate this possibility. However, those that do explore this idea find positive effects and highlight potential complementarities from paternal and maternal care. Moreover, there are important spillovers to other aspects of family life—most notably, to couple stability and fertility. However, the evidence remains scarce and somewhat mixed, underscoring the need for further research to understand the influence of paternity leave on family outcomes.

Finally, several recent studies focus on the consequences of parental leave on firms. The general finding from this literature is that firms are able to compensate for the lost labor input from having employees go on leave by hiring more employees or increasing work hours of other workers. These measures help firms to avoid incurring losses in their overall performance. However, firms may face certain barriers to replacing the lost labor input from leave-taking, resulting in negative effects on their performance.

The aim of this review is to provide a comprehensive overview of recent research on the impact of parental leave policies on key outcomes such as children's health and development, mothers' health, parents' labor market outcomes, societal norms, gender roles, gender equality, and their influence on firms' outcomes. This analysis will complement previous surveys found in the literature: First, Olivetti and Petrongolo (2017) share a detailed overview of the historical background of family policies in high income-countries with a focus on parental leave, childcare, and early childhood education. They survey the literature on the effects of these policies on fertility and women's labor market outcomes. Second, Rossin-Slater (2018) provides an excellent review of the literature on the influence of maternity leave and family policies on children's health and mothers' labor market outcomes. Third, Berlinski and Vera-Hernández (2019) summarize the literature on various family policies, including maternity leave, on child development.

In this review, we broaden the focus and bring in recent developments in the literature. In particular, we summarize the literature on (i) the impact of family leave policies on firm outcomes, (ii) the impact of policies that target fathers' leave-taking, and (iii) the role of norms, gender roles, and intra-household bargaining. The review starts with an overview of the history and motivation behind maternity leave policies. Next, we review the effects of maternity leave on maternal health, child health and development, fertility, couple stability, and mothers' labor market outcomes. We move on to present the more recent history of paternity leave and summarize how paternity leave schemes affect both fathers' and mothers' earnings and family outcomes. We then summarize the papers that evaluate the effect of leave policies on firms and employers. Lastly, we conclude by outlining suggestions for future research.

1.2 Maternity Leave

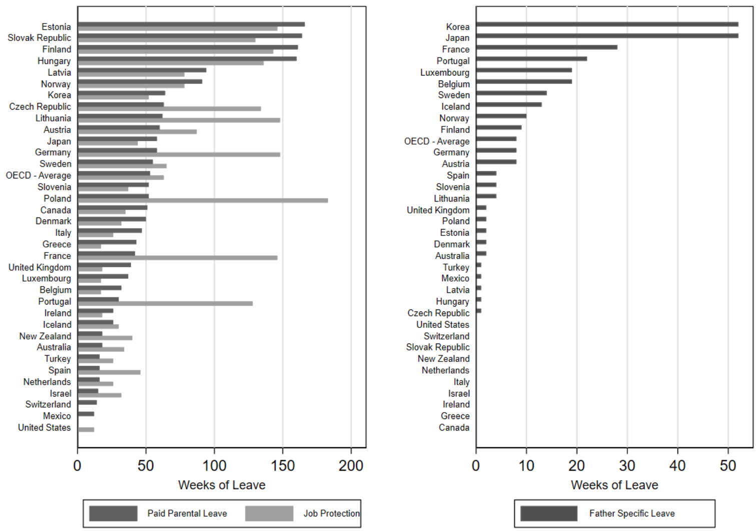
Labor market policies for expecting and new mothers emerged at the turn of the nineteenth century, following the Industrial Revolution and urbanization, when women increasingly began to work outside of their homes. The main purpose of these policies was to protect the health of mothers and their newborn children. In 1877, Switzerland was the first country to prohibit employment of pregnant women two weeks prior to and six weeks after childbirth, through the Swiss Factory Act of 1877. Similar laws were passed in Germany in 1878, Hungary in 1884, Austria in 1885, the Netherlands in 1889, Norway in 1892, Sweden in 1900, Denmark in 1901, and Greece in 1912 (Wikander et al., 1995).

In the beginning, these laws focused on employment prohibition around childbirth, but these policies evolved to enable voluntary leave from work, consisting of job protection and, in some cases, income support. Most of these policies were formulated with the underlying assumption that a male breadwinner was earning a sufficient family wage, which arguably underscored women's roles as wives and mothers (Wikander et al., 1995). By the mid-twentieth century, the focus of family leave policies shifted toward women's rights and gender equality. In light of this change, parental leave systems became a means for women to reconcile their jobs and family life. Sweden introduced three months of maternity leave in 1955, followed by Norway in 1956, Finland in 1964, and Denmark in 1967 with payment equivalent to unemployment insurance or sickness benefits (Datta Gupta et al., 2008).

The first proposal to introduce mandated parental leave from the EU Commission in 1983 was an effort to promote equal opportunity by ensuring that leave could be used by either parent. However, the proposal was rejected, and the EU directive on parental leave was not adopted until 1996 (Fusulier, 2011). When adopted, most EU member states had already implemented some form of shareable parental leave. In 1974, Sweden was the first country to introduce shared parental leave. Slovenia and France followed in the same year, and Norway did so in 1977 (Kamerman & Moss, 2011). However, mothers almost exclusively used parental leave rights. By 1990, all OECD countries except Australia, New Zealand, Switzerland, and the United States offered at least 12 weeks of paid leave. In 2013, 98 countries provided at least 14 weeks of employment protection, and 74 countries provided at least two-thirds of the women's pre-birth earnings for at least 14 weeks (Addati et al., 2014). Figure 1.1 shows the duration of provision of paid leave and job protection across the OECD countries in 2018. Givati and Troiano (2012) propose that parts of the variation across the globe stem from societal tolerance of gender-based discrimination. Measuring attitudes with a language-based measure, they show that societies with less tolerance provide longer leave.

The U.S. remains an outlier. It is the only high-income country in the world with no national provided paid parental leave system. During the 1970s, 23 U.S. states passed laws that prohibited health insurance companies from treating pregnancy differently from comparable illnesses, and federal law (PDA, 1978/9) outlawed employer discrimination of pregnant women more broadly. However, until 1993, there was no national employment protection during the weeks before and after childbirth. The Passage of the Family and Medical Leave Act (FMLA) in 1993 ensured any parent the right to unpaid family leave for up to 12 weeks per year. Over the last few years, several states have introduced paid family leave (PFL) programs. The first of these programs emerged in California in 2004, where employees became eligible for up to 6 weeks of leave with partial wage replacement. Similar programs were subsequently implemented in New Jersey in 2009, Rhode Island in 2014, and New York in 2018—where new parents could receive paid leave for up to 6, 4, and 8 weeks, respectively.

Figure 1.1. Leave Provision in the OECD in 2018



Source: OECD https://stats.oecd.org/index.aspx?queryid=54760

Generally, due to the timing of the introduction and development of parental leave schemes around the world, most modern studies evaluating the effects of introducing a short maternity leave are conducted with U.S. data, while studies on the extension of leave programs stem mainly from Europe.[1] Table 1 contains an overview of the reforms evaluated in the studies reviewed in this section, with details on leave duration, compensation, and eligibility.

1.2.1 Mothers' Health Outcomes

Although maternity leave policies initially stemmed from a desire to ensure the health of mothers and their newborn children, the evidence on the causal influence of leave on maternal health remains surprisingly scarce. Existing studies primarily focus on the effects of extending the current leave on mothers' mental health. Overall, studies indicate that the introduction of leave proves beneficial for mothers' health outcomes,

[1] A few papers have also focused on early reforms in European countries to study the effect of introducing a relatively short leave Bütikofer et al. (2021) study the influence of introducing paid parental leave in Norway in 1977, and Gregg et al. (2007) examine the introduction of job protection and 6 weeks of wage compensation in the UK in 1979.

TABLE 1.1: Maternity and parental leave reforms, by country and reform

Reform	Details	Paper
Norway (1977)	Paid leave extended from 12 to 18 weeks. Eligibility dependent on prior employment. 100 % replacement, increased from health insurance. Job protection extended from 3 to 12 months.	Bütikofer et al., 2021 Carneiro et al., 2015
Norway (1987-91)	Paid leave gradually extended from 18 to 35 weeks.	Kotsadam and Finseraas, 2013;
Norway (1992)	Extended paid leave from 42 to 52 weeks. 80 % replacement, 100 % if leave duration is shortened Eligibility depends on prior employment.	Corekcioglu et al., 2021; Dahl et al., 2016
United States (1978)	Expansion of the Temporary Disability Insurance covering birthgiving mothers receiving 6-12 weeks of leave with 50-66 % replacement.	Stearns, 2015
United States (1993)	Introduction of the Family Medical Leave Act that provides 12 weeks of job protection. Eligibility depends on employment time at the workplace and workplace size.	Rossin, 2011
California (2004)	Introduction of 16 weeks of paid family leave at 55% compensation rate.	Appelbaum and Milkman, 2011; Milkman and Appelbaum, 2013; Rossin-Slater et al., 2013; Lerner and Appelbaum, 2014; Baum and Ruhm, 2016; Bailey et al., 2019; Bullinger, 2019; Pihl and Basso, 2019; Bana et al., 2020
New Jersey (2009)	Six weeks of additional paid family leave on top off on top of TDI, at 2/3 wage compensation.	Appelbaum and Milkman, 2011; Lerner and Appelbaum, 2014
Rhode Island (2014)	Four weeks of additional paid family leave on top. on top of TDI, at 60 % wage compensation.	Bartel et al., 2016
New York (2018)	Eight weeks of paid family leave in 2018, 10 in 2019, and 12 in 2021, at wage compensation of 50 % in 2018 and 67 % in 2021.	Bartel et al., 2021
Sweden (1979)	Employment requirement relaxed for parity > 1.	Ginja, Jans, et al., 2020
Sweden (1989)	Paid job-protection extended to 15 months.	Liu and Skans, 2010
Germany (1979)	Paid job protection extended from 2 to 6 months. Benefits replacing income in the first three months and being provided at a flat rate for the remaining 3 months of 1/3 the average national income.	Dustmann and Schönberg, 2012 Guertzgen and Hank, 2018
Germany (1986-92)	Paid leave gradually extended from 6 to 24 months Job protection gradually extended to 36 months. No employment criteria for benefits.	Dustmann and Schönberg, 2012; Gangl and Ziefle, 2015; Ejrnæs and Kunze, 2013; Schönberg and Ludsteck, 2014
Germany (2007)	Paid leave reduced from 2 to 1 year at 67 % replacement of income. Flat rate for those without employment history. Introduction of 2 months paternity leave.	Raute, 2019; Kluve and Schmitz, 2018; Cygan-Rehm, 2016; Kluve and Tamm, 2013; Bergemann and Riphahn, 2015; Cygan-Rehm et al., 2018 Huebener et al., 2021

while extending parental leave beyond six months appears to have a negligible effect. However, the average treatment effect may conceal heterogeneity within the effects of extended leave, and some evidence points to health benefits of a longer leave for women in lower SES groups.

Bullinger (2019) examines the effects of introducing 6 weeks of paid leave in California in 2004, which effectively increased the average time that women stayed at home after

TABLE 1.1: Maternity and parental leave reforms, by country and reform (continued)

Reform	Details	Paper
United Kingdom (1979)	29 weeks of job protection and wages. 6 weeks at 90% wage compensation and 29 weeks at a flat rate.	Gregg et al., 2007
United Kingdom (1994)	Expanded eligibility. 90 % replacement for 6 weeks; flat rate for 12 weeks. Eligibility dependent on prior employment.	Stearns, 2018
United Kingdom (2000)	Paid leave extended to 6 months. Increased duration of the flat rate. Job protection increased to 1 year.	Stearns, 2018
Denmark (1984)	Extension from 14 to 20 weeks at 90 % replacement	Andersen, 2018; Rasmussen, 2010
Denmark (1994)	Paid leave extended from 24 to 76 weeks 90 % replacement for 24 weeks, then 60 % Eligibility depends on prior employment.	Datta Gupta et al., 2008; Friedrich and Hackmann, 2021; Andersen, 2018
Denmark (2002)	Increased compensation. 90 % replacement for 46 weeks. Job protection for 60 weeks.	Beuchert et al., 2016; Andersen, 2018; Gallen et al., 2019
Austria (1990)	Paid job-protection extended from 1 to 2 years. 100 % replacement for 8 weeks, then a flat rate Eligibility dependent on prior employment. Employment requirement relaxed for parity > 1	Danzer and Lavy, 2018; Lalive and Zweimüller, 2009; Danzer et al., 2020
Austria (1996)	Paid job-protected leave reduced to 18 months.	Lalive and Zweimüller, 2009
Canada (2000)	Paid leave extended from 6 to 12 months at 55 % replacement. Eligibility dependent on prior employment.	Baker and Milligan, 2008a; Baker and Milligan, 2008b; Baker and Milligan, 2010
France (1994)	Increased paid leave from 10 weeks to 3 years. Extended from second to third births. Flat rate benefit. Eligibility depends on prior employment.	Piketty, 2005; Lequien, 2012; Canaan, 2019
Czech Republic (1995)	Extension of flat rate benefit from 3 to 4 years. No job protection the fourth year. No eligibility requirement.	Mullerova, 2017; Bičáková and Kalíšková, 2019
Czech Republic (2008)	Possibility to shorten leave to 3 or 2 years. Total benefits kept constant.	Bičáková and Kalíšková, 2019

Note: This table is not meant to be an exhaustive list of reforms by country, but rather it contains the reforms utilized by the studies included in this review.

birth from 3 weeks to 6 weeks. She found that self-reported mental health and ability to cope with day-to-day demands improved among mothers after the reform. Chatterji and Markowitz (2005) and Chatterji and Markowitz (2012) also focus on leave duration in the U.S. They use variation in state leave policies as an instrument for maternity leave length (the average leave length in their sample is 9 weeks) and find that longer leave correlates with decreased depressive symptoms and improved self-reported health. Guertzgen and Hank (2018) study the expansion of paid leave in Germany from two to six months in 1979 and find that longer leave correlates with a higher incidence of long-term sickness and absence from work, while pointing out that this likely stems from the impact on selection into the labor market. Avendano et al. (2015) look at changes in the duration of paid maternity leave in several European countries between the 1960s and 1990s. In 1960, the duration of full-wage weeks ranged from 2 to 16 weeks, and by the end of their study period, it ranged from 8 to 16 weeks. They find that women who had access to more generous leave policies when they had their first child were less likely to experience depressive symptoms at age 50.

A recent paper by Bütikofer et al. (2021) significantly contributes to this literature by estimating the influence of both introducing and extending paid leave on a range of maternal health outcomes. In July of 1977, Norway introduced a 4-month paid leave along with 12 months of unpaid leave. Before the policy change, working mothers only had access to 12 weeks of leave. With the reform, benefits increased from sickness benefits to full wage replacement. Observing women at approximately age 40, the study found that the reform improved health outcomes such as BMI, blood pressure, pain, and mental health. The study also explores the impact of several later leave expansions, each of which increased the duration of paid leave by 2 weeks, and found no further improvements in the health outcomes.

Studies that focus on expanding leave length beyond 6 months suggest negligible benefits result in terms of mothers' health. Baker and Milligan (2008b) study the effects of extending paid maternity leave in Canada from 6 to 12 months. They look at mothers' outcomes 7–24 months after giving birth and find no effect on self-reported health, depression, or other postpartum problems. Dagher et al. (2014) use employer policies as an instrument for maternity leave duration and find a U-shaped relationship between leave duration and postpartum depressive symptoms, with minimal symptoms occurring for mothers who had around 6 months of leave. However, a study that examines a reform in Denmark (Beuchert et al., 2016) suggest that health benefits may result from a longer leave for some women. In 2002, the reform in Denmark increased the length of parental leave with full benefit compensation and effectively increased the average leave duration from 244 days to 276 days. The study finds that the increased length of maternity leave reduced hospital admissions and the probability that mothers would receive antidepressants in the first three years after giving birth and that these effects are driven by mothers with less than 10 years of schooling. Finally, Liu and Skans (2010) find no evidence that increasing parental leave in Sweden from 12 to 15 months has any effect on mothers being hospitalized due to mental disorders within 3, 6, or 16 years after giving birth.

1.2.2 Children's Health and Development

In the early days of parental leave policies, the central objective was to ensure infants' health and survival. More recent years have brought an increased focus on children's developmental outcomes. One argument that has been made for a longer parental leave is that increased parental time may have benefits in terms of cognitive development, which may influence later life outcomes such as schooling and income. The evidence suggests that introducing a short parental leave does indeed have significant benefits in terms of infant health outcomes and can bring health benefits later on by decreasing the risk of children being overweight or diagnosed with ADHD. The evidence on the impact of longer leave is more mixed. Overall, it appears to have no effect in terms of development and schooling outcomes. However, there is evidence that long leave may prove beneficial in terms of schooling outcomes for children born in higher SES families, while it adversely affects verbal development for children in low SES families.

Several studies that use cross-country comparisons suggest that longer maternity leave is associated with lower infant and child fatalities (see Heymann et al., 2011; Ruhm, 2000; Tanaka, 2005). Causal evidence indicates that the effect of introducing a short leave may differ from that of extending a longer leave. Studies from the U.S. show that ensuring that mothers can stay with their children during the first several weeks after birth has a significant and positive effect on infant health. Rossin (2011) evaluates the effects of the Family and Medical Leave Act (FMLA) in 1993 in the U.S., which mandated a minimum of 12 weeks of unpaid maternity leave for eligible women. She finds that the reform led to small increases in birth weight, decreased the likelihood of premature birth, and significantly decreased infant mortality among college-educated and married mothers, who were most able to take advantage of the unpaid leave. Stearns (2015) studies the effects of the temporary disability insurance (TDI) programs in the U.S. In 1978, these programs began providing wage replacement benefits to pregnant women for 6–12 weeks. This reform particularly benefitted women who could not have afforded to take leave before. The study find that the TDI benefits reduced incidences of low birth weight and early-term birth, and that it had the greatest impact among unmarried and black mothers. The California Paid Family Leave (PFL) program, implemented in 2004, allowed parents up to 6 weeks of paid leave with a newborn. This increased leave-taking by mothers by 3–6 weeks (from a baseline of approximately 3 weeks). Pihl and Basso (2019) find that this reform decreased hospital admissions, and Bullinger (2019) finds it improved overall child health.

There is less evidence on the long-term health benefits of introducing a relatively short maternity leave. Lichtman-Sadot and Bell (2017) investigate the effect of PFL on various health outcomes when children are between five and six years of age. They find that the reform reduced the risk of children being overweight or being diagnosed with ADHD, hearing problems, or communication problems. Parents who had children after the introduction of PFL were also more likely to assess their child's overall health more positively and less likely to report a history of frequent ear infections. The researchers find that these effects are driven by children from less advantaged backgrounds, consistent with the finding that PFL has the greatest effect on leave-taking among mothers who could not afford to take unpaid leave.

While the studies from the U.S. look at the impact of providing a short leave, research from Canada and Europe focuses on the effects of extending maternity leave beyond the first several weeks after birth. Baker and Milligan (2008b) examine the effects of extending paid maternity leave in Canada from 6 to 12 months and find no influence on children's overall health in the first 24 months after birth. In a follow-up study, Baker and Milligan (2010) find no significant effects on child development, specifically in measures of temperament and motor and social development. When studying the 2002 reform in Denmark, Beuchert et al. (2016) estimate the effect on children's inpatient hospital admissions and ER visits within one year and within three years from birth and find no significant effect. Danzer et al. (2020) estimate the effect of extending parental leave in Austria from one year on children's health outcomes. Importantly, they explore regional variation in the availability of formal childcare. When studying the heterogeneous effects of extending the duration of parental leave on children's outcomes, it is critical to consider what is being replaced with the increased time that children have with their parents,[2] yet this is rarely addressed. However, the findings of Danzer et al. (2020) highlight the importance of considering this factor, as they show that the extended leave had a positive effect on children's health outcomes only in regions where formal childcare was not readily available.

One mechanism through which longer maternity leave could affect infant health outcomes is by allowing mothers to breastfeed for longer periods. Huang and Yang (2015) look at the impact of PFL on breastfeeding practices in California and find that the reform led to a 10 to 20 percentage point increase in breastfeeding rates 3, 6, and 9 months after the birth of a child. Studying the same reform, Pac et al. (2019) also find a positive effect on breastfeeding, with a significantly larger effect for disadvantaged mothers. Baker and Milligan (2008b) find that the reform in Canada, which extended paid leave from 6 to 12 months, increased the duration of breastfeeding by more than one month. However, using this exogenous shock to assess the benefits of breastfeeding, their evidence suggests that, at least after six months, the benefits of increased duration of breastfeeding are trivial.

Several studies have explored the influence of parental leave on children's long-run outcomes. Carneiro et al. (2015) study the 1977 law change in Norway, which introduced 4 months of paid maternity leave (and 12 months of unpaid leave). They find that the reform led to a 2 percentage point reduction in high school dropout rates and a 5 percent increase in wages at age 30. This effect is driven by families with fewer resources, where the mother would have taken very little unpaid leave before the policy change. In contrast, Rasmussen (2010) investigates a policy change in Denmark in 1984 that increased parental leave from 14 to 20 weeks and find no significant effect on children's long-term educational outcomes. Furthermore, Dahl et al. (2016) evaluate the effect of increasing paid leave in Norway from 18 to 35 weeks and find no effect on children's schooling. Liu and Skans (2010) look at the effect of extending parental leave in Sweden from 12 to 15 months. They find no effect on hospitalizations within 3, 6, and 16 years after birth. Furthermore, they find no overall effect on children's school performance. However, they find positive effects on test scores among children of highly educated mothers. Dustmann and Schönberg (2012) assess the effects of three policy changes in Germany: The first expanded paid leave from 2 to 6 months in 1979, the second extended the leave further to 10 months in 1986, and finally, a reform in 1992 extended

[2]See e.g., Blanden and Rabe (2021) for an overview on the heteregoneity of the impact of childcare, depending on the family's SES, the quality of the childcare, and whether the childcare is replacing informal or parental care

the paid leave to 18 months. They find no evidence that any of these reforms improved children's schooling outcomes; in fact, they observe that the 1992 expansion may even have lowered children's educational attainment. Baker and Milligan (2015) estimate the effect of increasing maternity leave in Canada to 12 months and find no positive effect on cognitive and behavioral development of children when they reach ages four and five. Further, they uncover a small negative effect on PPVT (Peabody Picture Vocabulary Test) scores among boys. Similarly, Canaan (2019) investigates the effect of a French reform that extended leave duration from 16 weeks to 3 years and finds that it harmed children's verbal development at the age of six.

Danzer and Lavy (2018) study the influence of extending paid leave in Austria from 12 to 24 months. They find no significant overall effect on children's standardized test scores, but similar to Liu and Skans (2010) they find significant positive effects for children of highly educated mothers, especially for boys. Furthermore, they find a negative effect on the schooling outcomes of children whose mothers have lower levels of education—and in particular, for boys. In a forthcoming paper, Danzer et al. (2020) estimate the impact of the same reform on children's labor market outcomes and find no effects and no indication of a systematic pattern with respect to SES of the mother or the child's gender. Finally, Ginja, Jans, et al. (2020) investigate a policy change in Sweden that allows mothers higher benefits for a subsequent child without reestablishing eligibility through market work, if two births occur within a pre-specified interval. They find that this policy improves the schooling outcomes of the older child, likely due to increased maternal time.

Several countries have mandatory prenatal leave to protect the health of pregnant workers and their unborn children. Ahammer et al. (2020) study the effects of a policy reform in Austria that extended the mandatory prenatal leave from 6 to 8 weeks on children's short- and long-run outcomes. They find that the reform had no influence on children's health outcomes in the short- or long-run, and no impact on their future labor market outcomes. Furthermore, they find no effect on maternal health and subsequent fertility.

1.2.3 Fertility and Marriage

Parental leave policies affect the cost of having a child and might therefore affect family outcomes such as fertility and marriage decisions. Several studies have investigated the influence on fertility of a reform in Germany in 2007 that significantly increased the benefits for higher-earning women while decreasing the benefits for lower-earning women. This reform also reduced the total period of leave duration and implemented paternity leave. Disentangling the effects of these changes proves difficult. Raute (2019) documents a fertility increase of 23% among women with tertiary education, while Kluve and Schmitz (2018) observe that the policy reform reduced subsequent fertility among younger mothers. Cygan-Rehm (2016) find that the reform had a negative impact on fertility among low-income mothers.

Lalive and Zweimüller (2009) and Danzer et al. (2020) consider the effect of extending maternity leave in Austria from 12 to 24 months. While Lalive and Zweimüller (2009) find an increased probability that couples will have a second child within 10 years of the birth of their first child, Danzer et al. (2020) show no significant effects on completed fertility when they extend the horizon to 17 years after birth. They do not find that the reform had an overall influence on divorce probabilities, but document an increased probability that mothers who were unmarried at the time of birth would get married.[3] Liu and Skans (2010) observe no influence of extending the maternity leave in Sweden to 15 months on parental fertility or divorce rates, and similarly, Dahl et al. (2016) find no effect of extending the maternity leave in Norway from 18 to 35 weeks on completed fertility, marriage, or divorce.

1.2.4 Mothers' Labor Market Outcomes

Having children has an immense influence on women's labor market outcomes both in the short- and long-run (Angelov et al., 2016; Kleven et al., 2019; Lundborg et al., 2017). This reality has spurred an increased interest in reforms in high-income countries. Policymakers face difficult tradeoffs, as parental leave programs often aim to accommodate multiple concerns such as child and parental welfare, parental labor market outcomes, gender inequality, firm productivity, and governmental expenditures. Wage compensation is often combined with job protection schemes to ensure that parents can afford to take the leave and return to the labor market afterward. Proponents argue that generous parental leave policies promote gender equality and increase women's earnings by allowing mothers to retain valuable firm- or occupation-specific human capital and match-specific human capital after childbirth. However, it is precisely these longer spells of job absenteeism that opponents worry about. They argue that more time away from work lowers women's future labor market outcomes through human capital depreciation and possibly discrimination. In this section, we will review the literature on how a variety of parental leave policies affect mothers' and fathers' labor market outcomes.

A range of cross-country comparative studies, using variation in the availability and length of leave provision across countries, finds that paid leave is associated with somewhat higher female employment rates (Jaumotte, 2003; Pettit & Hook, 2005). In a prominent early study, Ruhm (1998) investigates the effect of parental leave on female employment and wages from 1969–1993 in nine European countries that experienced significant changes in their respective parental leave policies. He finds that entitlement to short periods of paid leave, totaling around three months, lead to an approximately 3–4% increase in female employment rates but little or no effect on wages. On the other hand, entitlement to longer parental leaves of more than nine months had no additional impact on employment but a significant negative impact of about 3% on female wages. Work covering additional countries and later years broadly confirms Ruhm's findings (Blau & Kahn, 2013; Ruhm & Teague, 1995; Thévenon & Solaz, 2013). Cross-country

[3] They only observe a significant effect on marriages in communities where nurseries are available.

studies also suggest that parental leave length affects women across SES levels differently. Long leave schemes increase labor market participation but decrease earnings for highly educated women relative to other women (Cipollone et al., 2014; Olivetti & Petrongolo, 2017).

Even though these rigorous studies provide strong comparisons of parental leave policies across countries, some concerns remain about the causal interpretation of cross-country studies. These studies are prone to overstating the true influence of parental leave since the extensions of this leave often happened over a period of time during which other family-friendly policies were implemented as well. Parental leave can vary in length, extent of job protection, income support, eligibility rules, and availability to either parent. The rules and costs governing preschool education and childcare also vary considerably across countries. Some countries have enabled direct family transfers and tax allowances to low-income working parents, differing in rules and magnitudes.

A large branch of the literature has addressed this challenge by focusing on one country and considering policy changes to elicit the causal effect of parental leave policies. Reviewing the results of various studies analyzing parental leave reforms in different countries confirms the overall findings from the cross-country studies. Overall, a concave relationship exists between the length of parental leave and mothers' labor market outcomes. Introducing and extending parental leave rights and wage compensation for up to six months improves mothers' labor market outcomes. Prolonging these rights for a year seems to have little effect, and extending them to a year or longer seems to have an adverse effect on women's wages and employment. Formal rights to maternity leave make it easier for mothers to maintain an attachment to their pre-birth job and employer, meaning that mothers do not have to start over when they return to the labor market after their childbirth and childrearing period. Where these rights already exist, however, extensions of the maternity leave period from a certain point on can have the opposite effect.

Introduction of Short Programs

Generally, introducing a short paid parental leave scheme has been shown to improve mothers' labor market outcomes. Several studies have examined the labor market consequences of the United States' first explicit paid parental leave policy implemented in California in 2004. Rossin-Slater et al. (2013) show that the implementation of this program doubled the use of parental leave by Californian women from 3 to 6 weeks on average, and that this change primarily resulted from the greater uptake by less privileged mothers. They estimate that this change increased the weekly work hours of employed mothers of one- to three-year-old children by 10–17%. Most studies on this reform confirm these findings and show an increase in the labor supply on both the intensive and extensive margins in the short-run, while highlighting that mothers' likelihood of returning to their pre-leave firm increased Bana et al. (2020) and Baum and Ruhm (2016). Conversely, a new study by Bailey et al. (2019) finds no evidence that this reform improved women's labor market outcomes and further claims that women making use of the improved leave provision experience a lower employment rate and wages 6–10 years after birth. Baker and Milligan (2008a) find similar effects when studying the introduction of 18 weeks of parental leave in Canada, which led to a sharp decrease in job separations. Gregg et al. (2007) show that the British introduction of job protection and 6 weeks of wage compensation in 1979 significantly increased mothers' labor market attachment in the years after childbirth, where many moved from unemployment into part-time positions. On the contrary, the German extension of paid leave and job protection from 2 to 6 months in 1979 led to a decrease in mothers' employment by 1–2% at 52 and 76 months after childbirth, although the effect does not persist in the long-run (Dustmann & Schönberg, 2012; Guertzgen & Hank, 2018).

Extending Parental Leave Up to 12 Months

Many countries have expanded their parental leave schemes over time, and the results suggest that little or no effect on mothers' labor market outcomes occurs until the duration of leave approaches one year in length. Focusing on the pre-1993 policy reforms in Norway, Dahl et al. (2016) find that expansions in government-funded maternity leave from 18 to 35 weeks had little effect on a wide variety of outcomes, including parental earnings and labor market participation in the short- or long-run. Datta Gupta et al. (2008) show that maternity leave approaching a year in length affects Danish mothers' wages negatively. Nielsen et al. (2004) suggest that the adverse effect is mainly driven by women employed in the private sector, while they find no negative effect of a longer leave for mothers in the public sector. They also find that the potentially higher wage compensation during pregnancy and better postpartum career opportunities in the public sector attract pregnant women to shift to the public sector.

In Norway, Corekcioglu et al. (2021) find that the extension of maternity leave from 30 to 52 weeks in 1993 did not help women reach top positions within their organization and indicates that it may even make them less likely to do so. Small but adverse effects on labor market attachment linked to a 52-week parental leave are also found in Germany (Schönberg & Ludsteck, 2014). Stearns (2018) is able to separately identify the effects of extending wage compensation and job protection to 52 weeks in Great Britain and finds that access to longer paid maternity leave increases the probability of returning to work in the short-run, but not in the long-run. In contrast, making job protection available to new mothers significantly increases maternal employment rates and job tenure five years after childbirth. Longer leave therefore seems to increase mothers' labor market attachment but decrease mothers' chances of career success in terms of promotions to managerial positions.

Extending Parental Leave Beyond 12 Months

In general, studies examining the effects of a parental leave that extends beyond a year find adverse effects on mothers' labor market outcomes. A range of studies examine the French 1994 reform that extended the period of paid leave for families with two children to three years and find that the reform induced women to exit the labor market and incur a wage penalty if returning to work, both in the short-run (Canaan, 2019; Piketty, 2005) and in the long-run (Lequien, 2012). Using German data, Ejrnæs and Kunze (2013) find that the increase in leave duration of up to 36 months led to detrimental effects on employment and wages for mothers. Using survey data, Gangl and Ziefle (2015) show that the expansion of leave duration changed German mothers' work-family preferences. The affected women reported lower levels of work commitment and fewer held a full-time position. In the same setting, Schönberg and Ludsteck (2014) find that the adverse effects on the labor market are mainly short-term effects. In particular, they find that increased leave duration reduced employment rates and earnings for up to 6 years after childbirth, but with smaller effects in the longer-run. The same has been found in Austria, where an extension of paid leave from one to two years reduced mothers' earnings in the short-term but had no longer-term effects (Lalive & Zweimüller, 2009).

Mullerova (2017) examines a parental benefit reform that took effect in the Czech Republic in 1995, extending the universal parental leave benefits from three to four years while keeping the job protection period at three years. She finds that mothers' probability of employment fell by 15–25% at the end of their parental leave and persisted at the same level more than two years later. Bičáková and Kalíšková (2019) evaluate the same reform as well as a later reform in 2008. The second reform allowed women to choose an alternative setup that shortens the paid leave from four to three or two years while keeping the overall amount of financial benefits received virtually unchanged. The job protection remained set at three years. Their findings demonstrate that the second reform had the opposite effect of the first, although with a much smaller impact.

A shortening of the parental leave also had a positive effect on German mothers. In 2007, Germany modernized its parental leave system, replacing the previous lengthy but low benefit leave—which specifically targeted low-income families—with a 12-month universal (in principle) leave offering much more generous coverage. The new benefits were dependent on pre-birth earnings, which meant that women with high labor market participation received a higher wage replacement rate. The empirical results indicate that the reform proved effective, leading to a 12% increase in mothers' employment probability after the end of the benefit period (Kluve & Tamm, 2013) and a positive influence on employment three to five years after childbirth for women with relatively high levels of education (Bergemann & Riphahn, 2015). However, these results do hide substantial heterogeneity, as women who were employed prior to giving birth increased their leave duration with the reform (Kluve & Tamm, 2013). Welteke and Wrohlich (2019) argue that the increase in benefits particularly encouraged high-income mothers to stay at home for the first 12 months following childbirth. By considering the increase in leave duration among working women and identifying female coworkers who had a child after the reform, they find substantial spillovers among the coworkers who took a longer leave themselves. The initial effect of the reform on new mothers' use of this leave and absenteeism from the labor market is therefore greater than what is identified when only looking at women in the reform window.

Summary

The surveyed research on the effects of maternity leave suggests significant benefits result from introducing a short leave, while the evidence of benefits for extending a longer leave is more mixed.

The literature on health outcomes provides compelling evidence for the beneficial effect on both maternal and child health of the introduction and expansion of a short maternity leave. The beneficial impact of leave extending beyond six months is more ambiguous, and some evidence suggests that policies implementing longer leave may increase inequality. In terms of health outcomes, low-income mothers benefit more from the provision and extension of paid leave. However, in terms of children's long run outcomes, such as test-scores, the benefits of leave extensions appear to be concentrated among those with highly educated mothers. Extending leave duration may therefore strengthen the relationship between maternal SES and child outcomes.

Introducing maternity and family leave entitlements generally appears to improve mothers' job continuity. The evidence shows that extending these provisions for up to six months improves mothers' labor market outcomes, but longer leave might have an adverse long-term effect on wages, employment, and career opportunities, especially when the leave extends for a year or more. The evidence also suggests that there are heterogeneous effects of different parental leave schemes. Offering universal paid leave increases use of leave by low-earning women, while longer paid leave and job protection periods may harm highly educated mothers' careers the most. In particular, women working in the private sector may experience diminished chances of reaching top positions when the paid leave duration increases. Expanding eligibility can also increase fertility, which might in turn lower mothers' long-term earnings due to the labor cost of additional children.[4]

1.3 Paternity Leave

As mothers remain the primary users of shareable leave, policymakers have to a greater extent started to target fathers. The primary goal of recent paternity leave policies has been to involve fathers more in childcare and other tasks in the household to alleviate some of the responsibility carried by mothers. Indeed, correlative studies show that leave-taking fathers are more involved in subsequent childcare (Boll et al., 2014; Nepomnyaschy & Waldfogel, 2007). If this relationship is causal, non-transferable paternity leave and equal sharing of parental leave should decrease household specialization. This could stem both from a direct effect on parents' labor supply and outcomes as well as a more indirect effect through changing norms and behaviors that can alter the division of labor within the household. Extensive causal evidence reveals the impact on earnings of both mothers and fathers, but perhaps due to data availability, the effect on time spent on childcare and housework has been less studied. Moreover, studies have shown an effect

[4]For more on this mechanism see the section on paternity leave and fertility

on fertility and couple stability. Paternal engagement has a positive association with child development (del Carmen Huerta et al., 2013; Sarkadi et al., 2008) and improves later father-child relationships (Petts et al., 2020). The causality and selection aspects of this finding have only been disentangled in a few papers.

1.3.1 Overview of History and Purpose

The recent focus on involving fathers in parental leave-taking stems mainly from gender equality concerns. According to the EU Commission, shared responsibility between parents should be an essential part of strategies to increase equality between men and women in the labor market and to ensure fathers' opportunity for time with their newborn child (Council of the European Union, 2019). A non-transferable (earmarked) paternity leave has been introduced sporadically since the 1990s but now often serves as a central element when modernizing the parental leave system in most OECD countries. A short paternity leave around the time of birth was introduced in Finland in 1978 and in Sweden in 1980.[5] Norway became the first country to introduce earmarked paternity leave in 1993, followed by Sweden in 1995. In 2000, Iceland passed a law that earmarked one-third of a 9-month-long parental leave to fathers. In 2021, the leave was extended to 12 months, earmarking 6 months to each parent, albeit with the possibility of transferring 6 weeks from one parent to the other. Figure 1.1 contains an overview of father-specific leaves in OECD countries in 2018.

While earmarked paternity leave has been praised for being an effective tool, uptake rates differ significantly by country. The Icelandic policy has proven most successful, bringing a more than 80%-point jump in uptake rates (Olafsson & Steingrimsdottir, 2020). Within Europe, the German and Danish introduction of parental leave has been the least effective. The German introduction of 2 months of paternity leave led to a jump of approximately 12 percentage points (Kluve & Tamm, 2013). The introduction of earmarked paternity leave took place in 2007 as a part of the modernization of the German parental leave system, which also shortened the leave duration and made benefits dependent on pre-birth earnings. The Danish introduction of 2 weeks of paternity leave in 1998 implied an increase of approximately two days of the average leave taken by fathers, and the abolishing of the earmarked leave in 2002 barely altered the average leave duration (Andersen, 2018).

[5] Very few papers have studied the effect of simultaneous leave. Persson and Rossin-Slater (2019) and Fontenay and Tojerow (2020) find that simultaneous leave improves maternal health in Sweden and Belgium, respectively. Andersen (2018) uses a series of reforms in Denmark, including the introduction of paternity leave around childbirth and find a positive effect on mothers' income.

TABLE 1.2: Fathers' take-up of earmarked paternity leave ('Daddy Quotas') upon reform implementation, by country and reform

Reform		Replacement rate	Prior take-up	Reform effect	Paper
Norway (1993)	Introduction (4 weeks)	80-100 % of former earnings*	3 %	32 %-point	Dahl et al., 2014;
				25 %-point	Cools et al., 2015; Johnsen et al., 2020
					Lappegård and Kornstad, 2020
Norway (2002)	Extension (2 weeks)		65 %		Lappegård and Kornstad, 2020
Norway (2009)	Extension (4 weeks)		75 %	3 weeks	Hart et al., 2019
					Lappegård and Kornstad, 2020
Sweden (1995)	Introduction (1 month)	80 % of former earnings*	30-38 days	50 %-point	Ekberg et al., 2013
				15 days	Avdic and Karimi, 2018
				10 days	Duvander and Johansson, 2012
Sweden (2002)	Extension (1 month)		37 days	5 days	Duvander and Johansson, 2012
Sweden (2008)	"Gender Equality" bonus	Tax credit per day used by father	48 days	No effect	Duvander and Johansson, 2012
Denmark (1998)	Introduction (2 weeks)	90 % of former earnings*	12-15 days	1-3 days	Andersen, 2018;
					Druedahl et al., 2019
Denmark (2002)	Removal (2 weeks)		14-22 days	Small reduction	Andersen, 2018; Beuchert et al., 2016
Iceland (2001)	Introduction (3 months)	80 % of former earnings*	~ 0	82.4-86.6 %-point	Olafsson and Steingrimsdottir, 2020
Canada (2006)	Introduction (5 weeks)	70 % of former earnings*	22 %	53.6 %-point	Patnaik, 2019; Wray, 2020;
					Margolis et al., 2021
Spain (2007)	Introduction (2 weeks)	100 % of former earnings*	~ 0	55 %-point	Farré and González, 2019
				6-8 days	González and Zoabi, 2021
Germany (2007)	Introduction (2 months)	67 per cent of net earnings*	4 %	12 %-point	Kluve and Tamm, 2013; Schober, 2014;
					Unterhofer and Wrohlich, 2017;
					Cygan-Rehm, 2016; Raute, 2019
					Tamm, 2019
United States (1993)	Introduction (12 weeks)	Unpaid leave w. job protection	7.2 %	3.9 %-point	Han et al., 2009
California (2003)	Introduction (6 weeks)	55 % wage replacement*	2 %	0.9 %-point	Bartel et al., 2018

*Benefits are capped at a ceilings.
Note: This table is not meant to be an exhaustive list of reforms by country, but contains the reforms utilized by the studies included in this review.

Analyzing the effect of the Californian Paid Family Leave Program—the first in the U.S.—Bartel et al. (2018) report an increase in paternity leave uptake of 0.9%-points. Using within-U.S. variation of employment protection covering fathers, Han et al. (2009) show that American men are insensitive to legislation enabling leave. As an arguably closer comparison to the US, Patnaik (2019) reports an increase in uptake rates of more than 50 percentage points after a reform in Quebec, which introduced 5 weeks of paternity leave. Moreover, important differences appear to exist between the introduction and the expansion of paternity leave. Evaluating two subsequent reforms in Sweden, Duvander and Johansson (2012) find the introduction of the first month of paternity leave in 1995 to have twice as great an effect as the expansion to two months in 2002. They also evaluated "a gender equality bonus" in 2006, which provided mothers with a tax credit if they shared leave equally, and find close to no effect on fathers' leave duration. Table 2 contains an overview of the reforms, fathers' use of leave prior to the reforms, and the reforms' effect across countries.

Many studies have explored which factors and characteristics make fathers use parental leave. Using Swedish data, Ma et al. (2019) find that men who are young, foreign-born, or earn a low income are less likely to take leave, explaining that this results from unstable labor market conditions. Descriptive evidence also highlights the importance of workplace characteristics (e.g., Bygren & Duvander, 2006; Geisler & Kreyenfeld, 2019; Kaufman & Petts, 2020; Naz, 2010) as well as relative income within couples, education levels, and number of previous children. Finally, the leave system itself plays an important role in uptake. Hook (2006) illustrate that paternity leave serves as an effective policy tool for increasing paternal involvement. Ray et al. (2010) emphasize how generosity and gender-egalitarian design of policy interrelate. Jørgensen and Søgaard (2021) document that uptake of paternity leave may be sluggish if benefits paid to fathers are low, highlighting the importance of wage replacement rates in influencing uptake of leave. Using data from 21 European countries, Castro-García and Pazos-Moran (2016) show that fathers take leave when it is non-transferable and payments are generous, while only a small minority take other types of leave. Mussino et al. (2019) compare the use of paternity leave among migrants in two culturally and economically similar countries—namely, Sweden, with a long paternity leave, and Finland, with a short paternity leave—and find that migrants' leave behavior is much more similar to the population in their country of residence than their country of birth, showing that policies enabling paternity leave are crucial for fathers' uptake of leave.

1.3.2 Gender Equality in Time Allocation and Labor Market Outcomes

Paternity leave policies might affect gender equality via two channels: first, by improving women's labor market earnings relative to men's, and second, by increasing the time fathers spend on childcare and other tasks in the home. In most settings, gender equality improves with the introduction of paternity leave. Importantly, this is rarely driven by a meaningful reduction in fathers' earnings, but rather, by a positive effect on mothers' earnings and labor supply combined with more paternal involvement at home.

Looking at the introduction of earmarked paternity leave in Norway in 1993, Cools et al. (2015) find no effect on Norwegian fathers' work hours and yearly earnings, and Kotsadam and Finseraas (2011) discover that paternity leave leads to a more equal division of specific tasks in the household. Rege and Solli (2013) find that Norwegian fathers' earnings are reduced with their uptake of leave, and by employing time-use data, they argue that this is driven by increased long-term paternal involvement, where fathers shift time and effort from the market to home production. Combined, these papers report that household specialization decreases with the introduction of paternity leave. Moreover, girls born immediately after the reform are less likely to do household work in adolescence (Kotsadam & Finseraas, 2013), showing that the equal sharing of household tasks persists into the next generation.

Dahl et al. (2014) document another type of social spillover. They find peer effects in workplace and family networks, as both brothers and coworkers of fathers initially affected by paternity leave reform take a longer leave themselves when they have a child. This effect depends on the strength of ties, with larger point estimates for brothers than coworkers. Moreover, the effect is transmitted in networks, creating a snowball effect that amplifies the initial impact of the reform and peer influence. Peer behavior likely provides fathers with relevant information about paternity leave, eventually leading to new norms of increased paternal involvement.

A related study by Johnsen et al. (2020) investigate the variations of relative leave induced by the reform and also find effects on coworkers caused by the leave-taking behavior of fathers. They observe that fathers' own leave-taking does not affect their labor market trajectory when controlling for their relative eligibility status within the firm. However, fathers have higher earnings if a larger share of their coworkers is eligible for paternity leave. This suggests that paternity leave may negatively affect fathers' earnings by causing them to lose out on high-wage positions to competing coworkers who do not take leave. Importantly, this effect is driven by the difference in eligibility and, in turn, leave-taking behavior. Dahl et al. (2014) show that the effect of the policy change might be greater than what is found when only comparing the couples with children born around the reform implementation period. Similarly, Johnsen et al. (2020) demonstrate that fathers other than those in the treatment group are affected by the reform. Norway further extended its paternity leave duration from 6 to 10 weeks in 2009, but Hart et al. (2019) find no effect on fathers' or mothers' subsequent earnings.

Sweden followed Norway's lead by introducing four weeks of paternity leave in 1995. Johansson (2010) investigates the influence of the reform and finds that it had a negative although statistically insignificant effect on fathers' earnings. Using the same reform, Avdic and Karimi (2018) also report a small reduction in fathers' earnings, along with a small reduction in mothers' earnings, which is mainly driven by mothers' increase the use of unpaid leave. By using a measure of absenteeism from work in order to care for sick children, Ekberg et al. (2013) find that the reform did not have a long-term influence on paternal involvement in childcare and uncover no effect on earnings of either mothers or fathers. Druedahl et al. (2019) use the Danish introduction of 2 weeks of earmarked paternity leave in 1998 and find that women's earnings increased significantly while men's dropped (albeit insignificantly). They explain that this effect is primarily driven by families wherein women are employed in the private sector.

Most studies on this topic have focused on the absolute duration of leave, whereas the relative difference in length of leave between the parents can act as an important driver of the gender wage gap, since it may determine the division of labor within the household. Andersen (2018) examines five separate Danish parental leave reforms and observes that an increase in paternity leave relative to maternity leave leads to higher earnings for mothers. Pylkkänen and Smith (2004) compare Sweden and Denmark, which are culturally and ideologically similar but differ remarkably in parental leave policies over time, Sweden has provided much longer maternity and paternity leave than Denmark. They conclude that longer fathers' leave shortens the mothers' period away from work.

While evidence from outside Scandinavia remains more limited, existing studies from Spain, Canada, and Germany all find evidence that lower gender specialization results from earmarked paternity leave. Couples affected by the reforms are more likely to move toward a dual-earner, dual-caregiver model. Farré and González (2019) use time-use data to investigate the effect of the Spanish introduction of two-week paternity leave on fathers' participation in childcare and demonstrate that eligible fathers increase their time spent on childcare compared to ineligible fathers. They find that fathers' earnings are unaffected and a positive effect on mothers' earnings occurs, driven by a reduction in unpaid leave.

Analyzing Quebec's introduction of five weeks paternity leave, Patnaik (2019) uses within-country variation in Canada. She documents that the time mothers spend on paid work and the time fathers spend on household responsibilities, including childcare, increased, with no effect on fathers' time spent on paid work. Using the same reform, Wray (2020) shows that fathers increased the time spent on solo parenting without their partner present.

A policy change in 2007 that introduced paternity leave in Germany simultaneously introduced changes in compensation rates and a shortening of total leave from 24 months to 14 months, making it difficult to separate the effects of the different changes. Tamm (2019) relies on within-father differences between first and subsequent children and reports that fathers' leave-taking increases the time allocated to childcare after their leave. Mothers' working hours increased and fathers' hours were reduced after a paternity leave, but these labor market effects are short-lived. Using a more standard reform evaluation framework to address the same reform, Kluve and Tamm (2013) find a small and insignificant effect on fathers' time allocated to housework. Using survey data from West Germany, Schober (2014) finds that fathers with children born just after the 2007 reform spend more time on childcare compared to those with children born before the reform, with no effect on housework. Similar to the evidence from Norway, spillovers to individuals in close proximity to the affected fathers seem to occur. Unterhofer and Wrohlich (2017) find that grandparents - in particular grandmothers - alter their view in support of working mothers when their son is given the opportunity to take paternity leave.

1.3.3 Fertility and Marriage

Since paternity leave can affect the household division of labor and shift the cost of childcare from mothers to fathers, it might also affect other family outcomes such as fertility and divorces. The transition to paternity leave can have mixed effects on fertility. On one hand, it can increase fertility, as having children becomes less costly for mothers' careers. However, if mothers' labor market attachment increases—and thus, their opportunity cost of subsequent children does too—fertility might decrease. Changes in costs for fathers should have symmetrical effects. The empirical evidence also shows that multiple effects are at play and findings of the effect of paternity leave on fertility are mixed. Several studies have investigated the effect on the risk of couple dissolution, and all but one study find that paternity leave has a stabilizing effect.

Doepke and Kindermann (2019) reveal that the distribution of the parental burden is a key determinant of fertility. Farré and González (2019) find that two weeks of paid paternity leave in Spain reduces fertility, driven by a postponement of subsequent childbirths. They suggest that higher opportunity costs for mothers reduce fertility desires, but also mention that increased paternal involvement might lower fathers' fertility desire as the costs related to childcare become more salient. Using Norwegian data covering a period of 25 years and exploring regional variation in uptake rates, Lappegård and

Kornstad (2020) find that higher uptake rates correlate with higher fertility. This effect is particularly strong for second births. Evaluating the introduction and extension of paternity leave in Norway, Cools et al. (2015) and Hart et al. (2019), respectively, do not find any effect on fertility. Using the Belgian introduction of a short paternity leave around the time of childbirth, Fontenay and Tojerow (2020) find that birth spacing increased as a result of this leave-taking. As mentioned, the German reform in 2007, which increased replacement rates of benefits but lowered total leave duration while earmarking two months to fathers, also affected fertility. Raute (2019) shows that the reform increased fertility particularly among highly educated mothers. Cygan-Rehm (2016) finds that spacing between births increased, driven by low-income mothers.

Olafsson and Steingrimsdottir (2020) investigate the effect of the 2001 Icelandic paternity leave reform and find that this reform reduces separations for up to 15 years following childbirth, with greater effects within the first 5 years. They find larger effects in households where the mother has a higher or similar educational attainment as the father. In households where the father is more educated than the mother, the long-term effect on marital stability is negative. Margolis et al. (2021) investigate the introduction of paid paternity leave in Quebec. This reform also expanded eligibility and increased compensation rates for both mothers and fathers. They report lower separation rates in the first five years after childbirth and no difference in the following three years. Proxying gender norms with household characteristics, they find that both paternity leave uptake and separation rates are greater among couples that are likely to hold more egalitarian views.

Farré and González (2019) also investigate divorce rates and report that up to three years after childbirth, paternity leave appears to have a stabilizing effect on marriages, but the effect is insignificant in the following three years. There is no effect on divorces in Norway from the implementation of paternity leave (Cools et al., 2015) or its extension from 6 to 10 weeks (Hart et al., 2019), although Kotsadam and Finseraas (2011) show lower levels of self-reported conflict after its introduction. Cygan-Rehm et al. (2018) evaluate the German introduction of paternal leave and find that the reform reduced the risk of single motherhood. The effects are driven by households where mothers are working. As paternity leave was introduced at the same time as other changes to the leave system, the researchers cannot disentangle its effect from that of related policies, but they conclude that their findings indicate mothers' improved financial situation and increased paternal involvement in childcare.

Contrasting these findings of either no or positive effects on marital stability is one paper with Swedish data. Avdic and Karimi (2018) investigate the introduction of parental leave and observe an increase in divorces within the first five years of the child's life among low-income mothers, showing that couples who would likely have split up later drive this result. They also investigate the extension of earmarked paternity leave from one to two months in 2002 and find no effect on divorces. When comparing the results across countries, the effects on female labor market outcomes and income might be important. Farré and González (2019) and Patnaik (2019) find a positive effect on labor supply of women in Spain and Quebec. The German reform increased benefits for a subset of households (e.g., Cygan-Rehm et al., 2018; Kluve & Tamm, 2013). However, Avdic and Karimi (2018) find an increase in unpaid maternity leave. While the reforms used in these studies all offered paid paternity leave, the different responses by the households and other details of the reforms led to opposite effects on household income in Sweden compared to other countries.

Gender norms provide another potential mechanism for reconciling the findings across countries. When children are born, most couples reorganize their lives toward more traditional family patterns, and this might cause conflict in couples that hold egalitarian views. Paternity leave might then have a stabilizing influence on these couples, but a destabilizing influence on couples who prefer a high degree of specialization. The results from Sweden are potentially driven by households that would have chosen a more "conservative" allocation of time, while the heterogeneous results reported by Cygan-Rehm et al. (2018), Margolis et al. (2021), and Olafsson and Steingrimsdottir (2020) show that stabilizing effects appear greater in couples that are more likely to hold egalitarian views. Proposing a framework wherein some couples specialize while others do not, González and Zoabi (2021) revisit the Spanish reform of 2007. They identify the part of the population wherein the reform had the greatest effect on decreased specialization and document decreased fertility and an increase risk of divorce.

1.3.4 Children's Health and Development

Involving fathers more in early childcare might affect child outcomes along several dimensions, such as health and educational outcomes. Only a few studies have investigated this possibility, but they suggest a complementary relationship between maternal and paternal care. Cools et al. (2015) study the introduction of one month of paid paternity leave in Norway in 1993 and find that children's school performance at age 16 improves as a result of this program. They observe the most concentrated effect in families in which the father is better educated than the mother and highlight the importance of the idea that the effect of increasing paternal care will depend on the relative quality of the care it is replacing. The size of their estimates is larger than that reported by Liu and Skans (2010), who study an expansion of maternity leave in Sweden from 12 to 15 months. The reform studied by Cools et al. (2015) introduced 4 weeks of paid paternity leave on top of an existing 12-month leave scheme almost solely used by mothers. They argue that the non-trivial effect is likely driven by the long-term effect of the reform on household specialization and paternal involvement found in other studies (e.g., Kotsadam & Finseraas, 2011; Rege & Solli, 2013).

Using the Swedish introduction of paternity leave, which simultaneously reduced the total shareable leave, Ekberg et al. (2013) find that both the male share of child sick days and the total number of sick days are unaffected by the reform. Also using Swedish data and the introduction of *double days* in 2012, Persson and Rossin-Slater (2019) investigate the effect of the reform on child and maternal health. The reform allowed parents to be on leave at the same time and also allowed them to take leave intermittently, implying that fathers could choose, on a day-to-day basis, to stay home with the mother and child. They find a positive effect on maternal health measured by decreased contact with health providers and a drop in usage of prescription drugs, but no effect on child health. The double days are therefore used when the mother is unavailable to care for the child due to being sick. Similar to the research of Cools et al. (2015), this finding speaks to the potential synergistic effect of maternal and paternal care.

Summary

Earmarked paternity leave can increase fathers' use of parental leave and family involvement, in turn ameliorating mothers' household burdens while increasing their labor market participation and work hours. The literature reviewed herein shows that the introduction of paternity leave entitlements increases fathers' usage of leave. Most studies have found that an increase in paternal involvement in childcare results from paternal leave-taking, but the evidence on labor market effects for both mothers and fathers is mixed. In most cases, mothers' labor supply and earnings rise with the increase in paternity leave. Studies of the earliest introduction of earmarked leave in Norway and Sweden have found a small reduction in fathers' earnings, but more recent introductions of earmarked leave found no effect on fathers' earnings. Moreover, there are important spillovers to other aspects of family life—most notably, couple stability and fertility. Few studies have investigated the effect of paternity leave on child outcomes, but the findings on paternity leave suggest that there are important complementarities between maternal and paternal care.

1.4 Parental Leave: Firms' Perspective

While a large body of work explores how parental leave affects households, less is known about their consequences for employers. In most countries, employers do not have to pay for the wages of workers on leave, as these are typically funded through the social insurance system. However, employers may bear more indirect costs. More specifically, a worker's absence due to parental leave leads to a decrease in the firm's labor input. The costs of parental leave for the firm thus depend on its ability to effectively replace this lost labor input. This in turn hinges on the availability of substitutes for the absent worker within the firm or in local labor markets. Recently, a number of studies have examined the implications of parental leave for employers in different settings. In general, studies have focused on how employers are affected by (i) the introduction of short periods of paid parental leave, (ii) reforms that extend the duration of paid leave, and (iii) employee leave-taking.

1.4.1 Introduction of Paid Leave

While the United States is the only OECD country with no national paid parental leave, several U.S. states have recently introduced paid family leave (PFL) — which provided researchers with the opportunity to evaluate how the introduction of leave affects employers. In 2004, California became the first state to give employees the right to take up to 6 weeks of partially paid leave. Other states followed, with New Jersey mandating up to 6 weeks of paid leave in 2009, and Rhode Island and New York introducing 4 and 8 weeks of leave in 2014 and 2018, respectively.

Using firm-level data from 2010 to 2018, Goldin et al. (2020) provide descriptive evidence on the type of U.S. firms that offer paid parental leave (PPL). They find that firms with generous PPL tend to hire more workers who invest in firm-specific human capital, and that they tend to be larger and have a younger workforce compared to other firms. Other studies focus on how employers are affected by the introduction of paid leave. Using surveys and in-depth interviews with employers, early descriptive evidence indicates that businesses in California and New Jersey saw either positive or no noticeable changes in profitability, turnover, employee productivity, and morale (Appelbaum & Milkman, 2011; Lerner & Appelbaum, 2014; Milkman & Appelbaum, 2013). These results align with studies that place more emphasis on identifying causal effects. Bedard and Rossin-Slater (2016) use an employer fixed effects model along with administrative panel data from California and find that employees' leave-taking slightly reduces firms' wage bill and increases turnover. Several other studies use a difference-in-differences design that compares the change in employer outcomes in a state where PFL was introduced with that of neighboring states, before and after PFL enactment. Bartel et al. (2016) surveyed small- and medium-sized food services and manufacturing businesses in Rhode Island, Connecticut, and Massachusetts in 2013 and 2015. They find that Rhode Island's PFL enactment had no significant effect on businesses' turnover, as well as employee productivity and morale. Bartel et al. (2021) also conducted a survey among employers in Pennsylvania and New York from 2016 to 2019. Their results show that New York's PFL did not change turnover, employee performance, or the characteristics of the firms' workforce. Furthermore, firms with more than 50 employees reported an increase in the ease of handling employee absences.

A recent study by Goodman et al. (2020) further shows that short periods of paid leave do not hurt employers, even when they have to pay for part of the wages of workers on leave. They focus on the 2017 introduction of the San Francisco Paid Parental Leave Ordinance, which requires employers to supplement California's 6-week partial wage replacement. This guarantees employees access to fully paid leave —the first such program in the United States. Despite an increase in availability of paid leave, San Francisco employers report no changes in their performance or employees' wellbeing.

Overall, these studies indicate that the *introduction* of short periods of paid leave does not significantly alter how businesses rate the performance and wellbeing of their workers.

1.4.2 Extensions in Duration of Paid Leave

Other work examines how employers are affected by reforms that increase the length of paid parental leave. Studies typically leverage reforms that unexpectedly extended the duration of paid leave. The unexpected nature of these reforms implies that firms are unable to plan in advance for worker absence, which can in turn limit their ability to efficiently compensate for lost labor input and can therefore hurt their performance. Ginja, Karimi, et al. (2020) focus on such a reform in Sweden, which extended the duration of paid leave from 12 to 15 months. The researchers find that the reform induces mothers to take an additional 2.5 months of leave, but it also raises their likelihood of switching to another firm. Firms then make costly adjustments to compensate for the sudden increases in turnover and employees' leave duration: They hire more temporary and permanent workers and raise work hours of coworkers of women on leave — resulting in a significant increase in the total wage bill. Following these adjustments, manufacturing firms experience drops in revenues, sales, and value added by labor input — suggesting that replacement workers are less productive than women on leave.

Another study by Gallen et al. (2019) looks at a 2002 unexpected expansion in the length of paid parental leave from 10 to 32 weeks in Denmark. She shows that small firms are more likely to shut down within five years of being exposed to the leave extension. Coworkers of women on leave see no significant changes in their earnings or employment rate, but they delay the timing of their own leave-taking and take more sick days due to the reform.

Finally, Huebener et al. (2021) evaluate a German reform that increased the amount of wage replacement for employees on leave from 3 to 12 months. They demonstrate that firms experience drops in their employment and wage bill while an employee is on leave—suggesting that they are unable to fully compensate for the lost labor input. Further, the researchers find suggestive evidence that firms are more likely to discriminate in their hiring against women of childbearing age. Taken together, these studies indicate that firms cannot effectively compensate for the lost labor input and experience a deterioration in their performance when exposed to unexpected extensions in the duration of parental leave.

1.4.3 Leave-Taking Events

While reforms that change parental leave entitlements can have significant effects on employer outcomes, the event of having an employee take leave (versus not take leave) is equally consequential. Furthermore, the influence of leave-taking on firms can be different than the effect of the introduction or extension in the duration of paid leave. In the absence of changes in parental leave regulations, firms anticipate the timing and length of employee leave-taking, which can allow them to better plan for worker absence.

Using Danish administrative data from 2001 to 2013, Brenøe et al. (2020) examine how small firms cope with having a female employee give birth and take parental leave. They use a dynamic difference-in-differences design that compares employers of women who give birth to employers of women who do not give birth over the next few years. They first document that employers of women who give birth are exposed to an average of nine and a half months of leave. Firms adjust to this leave-taking by hiring temporary workers and increasing retention and work hours of employees in the same occupation as the women on leave. As a result, firms' total work hours remain unchanged—which indicates that these adjustments were effective at compensating for employee absence. The costs of these adjustments appear minimal. Despite increasing earnings of coworkers of women on leave, firms see no significant changes in their total wage bill. They also do not experience significant changes in their overall performance as measured by their output, profits, and likelihood of survival.

1.4.4 Replaceability of Workers on Leave

The costs of parental leave for firms depend on how well they can replace the absent workers. This in turn could be determined by labor market conditions and constraints facing the firm at the time of leave-taking. A recent study from Denmark highlights how labor market conditions may affect firms' ability to adjust to leave-taking. Friedrich and Hackmann (2021) investigate how a one-year extension in the duration of paid leave in 1994 (from 28 weeks of paid leave) affected hospitals and nursing homes. They show that the program led to a significant increase in nurses' leave-taking. Because of stringent labor market regulations, employers were unable to replace nurses on leave—which led to a decrease in nurse employment. This in turn resulted in a significant drop in the quality of care provided by hospitals and nursing homes.

Another study suggest that the high costs of hiring and dismissing workers may limit firms' ability to replace leave-takers. Schmutte and Skira (2020) find that in Brazil—a country with rigid labor laws—firms exposed to leave-taking only slightly increase their hiring and could not replace the absent worker at a one-to-one rate. Ginja, Karimi, et al. (2020) also provide evidence that labor market conditions affect how firms replace workers on leave. They show that in thick local labor markets, employers mainly increase hiring of new workers and do not change their existing workers' work hours—with the opposite effects occurring in thin labor markets. citehueb2021 sshow that firms adjust by using both internal and external substitutes. Their findings reveal that firms use replacement hires more often when they have few internal substitutes (i.e., workers in the same occupation). They show that workers postpone their return from leave when internal substitutes are available. By exploiting an increase in the duration of paid leave, they demonstrate that this relationship between internal substitution and leave duration is greatly reduced, suggesting that coordination between workers and firms grows distorted by the increase in leave duration.

Finally, the substitutability of a firm's employees affects how they fare with a coworker's leave-taking. Ginja, Karimi, et al. (2020) show that firms with a high fraction of same-occupation employees primarily increase work hours of their employees in response to leave-taking, while other firms rely more heavily on new hires. Brenøe et al. (2020) further find that firms with no other workers in the same occupation as the absent employee cannot fully adjust to leave-taking—despite having anticipated the leave. More specifically, the researchers show that these firms experience declines in their total work hours, wage bill, sales, gross profits, and survival.

Summary

In general, firms are able to adjust to worker absence due to parental leave. They compensate for the lost labor input by hiring new employees and/or increasing the work hours of existing employees. These adjustments are not costly and prevent firms from incurring losses in terms of their overall performance. However, certain factors—such as unexpected leave-taking, lack of substitutes for the worker on leave within the firm or in local labor markets, and high costs of hiring or dismissing new workers—may limit employers' ability to replace workers on leave. This can result in high replacement costs and negative effects on firms' performance.

Much progress has recently been made in studying firms' response to parental leave, but some questions remain open. First, several U.S. studies show that the introduction of paid leave does not change employers' rating of their performance. However, the lack of administrative data in these settings prevents us from understanding how employers adjust to the introduction of leave, and how this affects the labor supply and wellbeing of other workers. Second, there remains no conclusive evidence on whether parental leave-taking results in statistical discrimination against women. Indeed, employers may limit the hiring and promotion of women of childbearing age in order to reduce their exposure to any consequences of parental leave-taking.

1.5 Conclusions and Future Research

Parental leave policies have evolved tremendously since the mid 20th century. Following women's entrance into the labor market, the focus of parental leave policies has changed from mother and child survival to parental labor market outcomes, family welfare, and child development. It is therefore important to evaluate various outcomes when examining parental leave policies.

Overall, parental leave policies prove highly important in helping parents to balance between job and family welfare responsibilities upon having children. Women are still the primary caregivers of newborn babies, and thus, most leave policies remain targeted toward mothers. In general, we observe an inverted U-shaped relationship between length of parental leave and most of the outcome variables. First, the introduction of short paid leave improves mothers' labor market outcomes as well as their own health and the health of their children. Second, the findings show that extending the leave beyond six months has negligible effects on child development and the health of both mothers and children, while long-term leave affects mothers' wages and employment negatively. Few studies focus on the heterogeneous effects of a long parental leave. Interestingly, the existing studies find that a parental leave that extends beyond six months negatively affects the income of highly educated women with specialized jobs the most. Long parental leave brings health benefits for women in lower SES groups but not for women in higher SES groups. Furthermore, it appears that a long parental leave may prove beneficial in terms of schooling outcomes for children born in higher SES families, while it adversely affects children in low SES families. The heterogeneous effects of family leave policies hold critical importance, and ample need remains for more studies on this topic.

The evidence on the effects of paternity leave is more mixed. Overall, studies show that introducing earmarked paternity leave proves effective in increasing fathers' uptake rates and childcare involvement. The evidence on mothers' and fathers' labor market outcomes varies across countries and policies. Findings show no to small positive effects of paternity leave on mothers' earnings and no to small negative effects on fathers' earnings. Paternity leave is also found to increase family stability and fertility, but again, the small amount of existing literature provides mixed findings on these topics. The non-monetary effects of the different types of earmarked paternity leave are in general understudied, and more studies are required to make an overall conclusion.

Economists have only recently begun to study the effects of parental leave on firm performance, and many effects remain vastly understudied. In general, recent studies find that firms are able to compensate for the lost labor input from leave-taking employees, usually through hiring and increasing the workload for the remaining workers. However, firms that need to replace leave-taking employees in highly educated and specialized positions face higher replacement costs, which in turn can negatively affect productivity and firm performance.

Most studies thus far focus on the length and inter-parental distribution of parental leave. We suggest that a demand exists for more studies focusing on the compensation rate and eligibility of the leave-taking workers. Over time, various reforms in different countries have changed the compensation rate for the entire duration of paid parental leave or parts thereof. It would be fruitful to gain a deeper understanding of how compensation rates affect uptake rates and the division of leave between the parents as well as the effects on parental and child welfare. Along those lines, it would also prove interesting to study leave-taking behavior and fertility when eligibility for receiving parental leave benefits changes. Eligibility rules have largely changed over time and vary across countries. While some countries have no eligibility requirements, in others, the parents need to have been in a full-time position for a year before becoming eligible for parental leave benefits. As future stories explore such research directions, decision-makers, firms, and individual workers will have a more well-rounded body of evidence to draw from in making decisions regarding parental leave.

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CHAPTER 2

Gender Norms and Specialization in Household Production: Evidence from a Danish Parental Leave Reform

The arrival of children implies a sharp reduction in mothers' earnings and labor supply while fathers' labor market trajectories are unaffected. To understand this specialization, I exploit a Danish parental leave reform. Upon reform implementation, mothers increase their leave by 5 weeks while the average leave duration of fathers remains unchanged, irrespectively of relative earnings. Consistent with the role of gender identity, women who had a working mother take a shorter leave than those with a stay-at-home mother. Moreover, I document peer effects among sisters who take a longer leave if exposed to the reform-induced change in leave duration.

2.1 Introduction

Parenthood and persistent gender inequalities are intricately linked. After their first child, women reduce their labor supply and instead spend more time on child care and household responsibilities. Meanwhile, men's labor market trajectories are largely unaffected. This pattern is the main explanation for the remaining gender wage gap (Ejrnæs and Kunze, 2013; Angelov et al., 2016; Kleven et al., 2019;). While this divergence is well documented, it is poorly understood. As standard economic incentives appear to play a muted role in time-allocation upon parenthood (Kleven et al., 2019; Daly and Groes, 2017), another potential explanation is that of gender identity. This concept has been used to explain women's labor supply (e.g. Fernandez et al., 2004; Fernández and Fogli, 2009; Olivetti et al., 2020) but less attention has been paid to the role gender identity plays upon parenthood. To improve the understanding of the striking gender gaps following parenthood, I investigate which factors influence time allocation within the household, in particular the use of parental leave.

To understand this specialization, I exploit an unexpected and rapid institutional change in Denmark. In 2002, a parental leave reform improved the economic compensation new parents received while on leave, effectively increasing the leave duration. At the same time, policy makers removed two weeks of earmarked leave specifically allocated to fathers. With the reform, the household could decide how to distribute the extended parental leave within the couple. I ask which factors influence take-up. My analysis is two-fold. First, I evaluate the reform effect on leave duration for the universe of eligible parents and investigate the role of relative earnings within couples in the 18-month window surrounding the implementation date. Second, I evaluate how leave behavior is affected by family behavior. I investigate inter-generational spillover in time allocation. I do this by linking historical information on maternal labor supply for women in the reform window. Further, I identify sisters who have a child after the reform and investigate peer effects on leave duration.

By implementing a Regression Discontinuity Design around the introduction of the reform, I investigate which factors influence households respond when they are given the opportunity to take an extended leave. Before turning to the empirical exercise, I briefly present a Becker (1981)-model on division on labor. Then, I turn to the role of identity and prescriptions as formulated by Akerlof and Kranton (2000). A set of testable predictions come out of this exercise and they are taken to the data. I find that mothers increase their leave by 5 weeks while the average leave duration of fathers is unchanged. Contrary to what a Becker (1981)-model predicts, estimates are largely unaffected by relative earnings in the household. Instead, the results are highly consistent with the notion of gender identity. I show that women who had a mother with a high labor supply take a shorter leave compared to those who had a stay-at-home mother. Moreover, I document peer effects among sisters. Women with a sister in the reform treatment group take on average take a 1.1 weeks longer leave compared to those with a sister in the reform control group. This corresponds to peer effects of 17 %. Importantly, all these women give birth at least 9 months after the reform was introduced and face identical institutional settings. The reform induces a change in behavior among mothers, and I argue that the extended leave duration creates new prescriptions associated with motherhood. These new prescriptions are transmitted via sisters and show up here as peer effects.

While both the reform effects and the peer effects are highly consistent with the notion of gender identity, it is possible to imagine other factors that could drive parts of these results. Such factors include biology, information transmission, and consumption externalities and I discuss each of these factors in detail below. While biology - including breastfeeding or unobservable differences across men and women - could drive the overall reform effect, it is difficult to see how this explains the heterogeneity by maternal labor supply and the peer effects. Information transmission and consumption externalities can explain the peer effects but fail to provide a useful explanation for the reform effects. Importantly, gender identity is the only explanation useful for understanding both the reform and peer effects.

This paper contributes to two strands of the literature. First and foremost, it contributes to the literature on gender inequality in the labor market.[1] In particular, this paper contributes to the literature on the divergence in labor market outcomes upon parenthood (Harkness and Waldfogel, 2003; Daniel et al., 2013; Ejrnæs and Kunze, 2013; Angelov et al., 2016; Lundborg et al., 2017; Kleven et al., 2019; Berniell et al., 2021). To elevate some of the costs associated with motherhood, most countries have some maternity leave[2]. Recently, the effectiveness of family policies for closing gender gaps in the labor market has been put into question (e.g. Canaan, 2019; Kleven et al., 2020; Ginja et al., 2020). I link the literature evaluating leave reforms to the literature showing the importance of gender identity for female labor force participation (e.g. Fernandez et al., 2004; Fernández and Fogli, 2009; Farré and Vella, 2013; Finseraas and Kotsadam, 2017).[3] The primary contribution of this paper is to show that when given the opportunity to take a longer parental leave, households behave in a way highly consistent with a model of gender identity. This speaks to why child penalties persist: gender differences in prescriptions upon parenthood are important for gender differences in time allocation.

Second, this paper contributes to the literature on peer effects. The literature on peer effects in labor market choices and gender goes back to Neumark and Postlewaite (1998). They show that labor market choices of women spur similar choices by close peers re-

[1] An enormous literature has focused on gender differences in participation rates, education, and occupation choices (see reviews by for example Bertrand (2011); Goldin (2014); Blau and Kahn (2017)).

[2] See Olivetti and Petrongolo (2017) and Rossin-Slater (2018) for reviews of the literature on the causal impact of these policies.

[3] While using family environment has been a prominent approach, another influential strategy to disentangle the role of gender norms from standard economic incentives has been to use shocks to gender norms such as the HIV/AIDS epidemic (Fortin, 2015), WWII (Fernandez et al., 2004; Goldin and Olivetti, 2013), settlers in Australian (Grosjean & Khattar, 2019), and the German reunification (Jessen, 2021; Lippmann et al., 2020; Boelmann et al., 2020; Beblo and Görges, 2018) or changes of economic incentives (Ichino, Olsson, Petrongolo, & Thoursie, 2019) to estimate effects from gender norms to female labor supply.

gardless of income effects. More recently, Nicoletti et al. (2018), and Olivetti et al. (2020) show peer effects on female labor force participation.[4] To circumvent threats to identification, quasi-experiments that only influence peer behavior are commonly used (e.g. Angrist and Lang, 2004; Kling et al., 2007; Brown and Laschever, 2012; Fadlon and Nielsen, 2019; Altmejd et al., 2021). Parental leave reforms often provide an ideal setting for such an approach, but only two other studies investigate peer effects. They find, similar to me, that leave duration causally affects peers' leave duration. Dahl, Løken, and Mogstad (2014) use the implementation of the "daddy quota" in Norway, and document peer effects on brothers and male co-workers. Welteke and Wrohlich (2019) find peer effects among female co-workers in Germany after prolonged maternity leave. This paper document peer effects following an extension of parental leave, rather than paternity or maternity leave. This is important as mothers remain the primary users of family policies. I show that a general extension of parental leave reinforces an existing gender gap in time allocation as a result of both the reform effect and subsequent peer effects. More general, my study also highlights how policies can interact with existing norms and prescriptions. This is important when considering the design and implementation of various programs.

Finally, this paper adds to a highly relevant policy area (e.g. The White House, 2021; Council of the European Union, 2019) and can inform the design of parental leave. Many countries have supplemented maternity leave with 'gender-neutral' parental leave-schemes and some countries have implemented leave for fathers.[5] To the best of my knowledge, Denmark is the only country that has *removed* paternity leave and this paper is the first to evaluate how households respond to this.[6] If the aim is to increase fathers' share of leave, my results show that a general extensions will not achieve this. To achieve greater gender equality, an explicitly targeting of fathers might be needed.

The structure of the paper is as follows. In Section 2.2, the relevant theories for intra-household specialization are presented along with predictions for the empirical investigation. Section 2.3 contains a presentation of the 2002-reform, the data set, and the empirical strategy. Graphical and regression-based results are reported in Section 2.4 together with robustness checks. Section 2.5 concludes.

[4]More broadly, this paper also contributes to a large literature on spillover effects within families (e.g. Bingley et al., 2019; Daysal et al., 2020; Lindquist et al., 2015; Brenøe, 2021; Cools and Patacchini, 2019; Black et al., 2020; Dahl et al., 2020; Altonji et al., 2017; Lundborg et al., 2014).

[5]While the use of these policies has been gradual (Dahl et al., 2014; Ma et al., 2019), studies report positive effects on women's wages (Druedahl et al., 2019; Farré and González, 2019). In some contexts, eligible fathers spend more time on housework (Patnaik, 2019; Kotsadam and Finseraas, 2011) and childcare (Kluve & Tamm, 2013). Studies also find effects on marital stability (Olafsson and Steingrimsdottir, 2020; Margolis et al., 2021; Avdic and Karimi, 2018).

[6]Other studies have utilized this reform for identification. They have been concerned with maternal and child health (Beuchert et al., 2016), firm performance (Gallen, 2019), and women's income (Andersen, 2018; Tô, 2018), and disregarded fathers' leave behavior. An exception to this is Lassen (2019), my Master's Thesis, which is directly concerned with (the lack of) fathers' response to this reform.

2.2 Household Behavior upon Parenthood

Before turning to the empirical investigation of households' responses to the reform, I outline two sets of theoretical framework that will guide the empirical investigation. I start with the standard Becker (1981)-model on division of labor. Then, I turn to the framework on identity developed by Akerlof and Kranton (2000). I arrive at two sets of testable predictions regarding the role of standard economic incentives and gender identity, respectively.

2.2.1 Financial Incentives and Comparative Advantages

In Becker's influential model of the household, intra-household specialization is determined by members' comparative advantages. Each member of the household can allocate time to each of the two sectors, the labor market and the home. Members are initially identical except for differences in human capital levels, broadly defined to include formal education, experience in both the labor market and with household specific tasks.[7] Members of the household cooperate to maximize joint production and specialize according to their comparative advantages.

If women on average have invested more heavily in human capital relevant for home production and men have invested more heavily in human capital relevant for market production, women should on average specialize in home production and men in market production. If high earning women are coupling with even higher earning men, this will should reinforce this pattern. On an aggregate level, this provides a compelling explanation for division of labor within families and why only women's earnings are affected by parenthood. However, in Denmark, as in most high and middle-income countries, there has been a rise in the educational level of women (Goldin, Katz, and Kuziemko (2006); Kleven and Landais, 2017; Houlberg and Larsen, 2011), and today young women are on average better educated than young men. In couples where the woman has the highest earnings, productivity of the household could benefit from the man allocating more time to home production.

2.2.2 Gender Identity and Prescriptions

Empirical evidence so far shows that educational level and relative earnings have very little predictive power over the size of the child penalty (Kleven et al., 2019) and time allocation to child-rearing (Daly & Groes, 2017). To understand this, I turn to the framework developed by Akerlof and Kranton (2000). In this framework, identity pay-off is

[7] I disregard any argument related to biological advantages, but expand on this in Section 2.4.3 'Alternative Explanations'.

derived from belonging to a social category, and for each category, a set of prescriptions is in place determining what is considered appropriate behavior. Akerlof and Kranton (2000) show that incorporation of identity and preferences for conforming to group behavior into a utility function yields equilibrium outcomes that are very different from what standard theory would otherwise predict. In this framework, prescriptions are defined locally as the average behavior among relevant peers such as school-mates (Akerlof & Kranton, 2002) and co-workers (Akerlof & Kranton, 2005). If relevant peers change their behavior, so does the optimal behavior of the individual.

Gender is a social category with great importance for individual choices. Recently, Bertrand (2020) described an insight from social psychology; gender stereotypes are not merely descriptive but serve a prescriptive role. They motivate men and women to adjust their self-view and choices to what is deemed appropriate for their gender and this result in gender identity. To improve on the understanding of how gender identity is constructed and enforced, the work by sociologists West and Zimmerman (1987) is useful. They view gender as "an emergent feature of social situations: both as outcome and as rationale for various social arrangements and as a means of legitimating one of the most fundamental divisions of society" (Ibid., p. 126). Gender differences are persistent and reinforced through everyday interactions where individuals adapt their behavior to align with what is expected of them based on their gender. In this context, prescriptions are those sets of behavioral norms expecting mothers to engage in care work and unpaid labor, while fathers are met with other expectations.[8] When an individual does not comply, they incur a cost (West and Zimmerman, 1987; Akerlof and Kranton, 2000).[9] By tradition, women have been given the vast responsibility for child-rearing and home production, and thus men and women face very different prescriptions upon parenthood. To comply, women allocate extensive time to home production and notions of the male breadwinner induce men to allocate less time to home production. In the framework outlined by Akerlof and Kranton (2000), the pay-off from identity and prescriptions implies that conformity is utility maximizing.[10]

Adding to the complexity, there might be substantial differences in prescriptions within gender categories. Complementing societal wide gender roles and stereotypes, the family has been highlighted as an important site for the formation of gender identity to take place and prescriptions are defined (see review by Bau and Fernández, 2021). A growing amount of evidence shows that parents' attitudes and behavior are transmitted to their children. Importantly, having a working mother increases own labor supply (e.g. Fernandez et al., 2004; Fernández and Fogli, 2009; Morrill and Morrill, 2013; Finseraas and Kotsadam, 2017; Olivetti et al., 2020) and is associated with a smaller child-penalty

[8]This is in the line with the influential definition of norms as empirical and normative expectations by Bicchieri (2005): an individual prefers to comply with a norm under the conditions that a) they believe a sufficiently large part of the population behaves in a certain way *and* b) they believe that they are expected to behave in the same way.

[9]Bicchieri (2005) allows for sanctions but does not require it.

[10]A large and growing literature shows penalties for both men and women when behaving in counter-stereotypical fashion (e.g. Rudman and Phelan, 2008; Moss-Racusin et al., 2010; Bertrand et al., 2015; Kuwabara and Thébaud, 2017; Folke and Rickne, 2020; Exley et al., 2020)

(Kleven et al., 2019). Women who had a working mother might hold a different set of prescriptions related to motherhood compared to those who had a stay-at-home mother. In turn, their optimal level of time allocated to child-rearing might be systematically different, regardless of standard economic incentives. Moreover, prescriptions are not static. If relevant peers (e.g. friends, co-workers, or siblings) change their behavior, the optimal behavior for the individual also changes.

2.2.3 Predictions

Based on the two frameworks, two different sets of predictions can be outlined. In a setting with improved opportunities for parental leave, a standard Becker model predicts that the parent with a comparative advantage in the household takes the longer leave. Mothers who have an advantage in the market should respond less to a reform that allows for a longer leave compared to those who have an advantage in the home. As I cannot observe comparative advantages, I use relative earnings.[11] Mothers who were primary earners prior to childbirth are expected to respond less to the reform than those who were not primary earners. Equivalently, fathers who are out-earned by their partner are expected to respond stronger to the reform than fathers who are primary earners.

However, if pay-off from gender identity and prescriptions determine time allocation, mothers would be the primary users of the extended leave, regardless of standard economic incentives. Instead, prescriptions for mothers and fathers influence the leave behavior. If prescriptions dictate that mothers should allocate more time to child-rearing than fathers, a large reform effect among mothers is expected. Fathers are not expected to use the opportunity to increase leave duration. In order to explore heterogeneity by gender identity, I use data on maternal labor supply of mothers (i.e. the grandmothers of the child) in the reform window. In line with the literature of inter-generational transmission of gender identity, those who had a mother with a high labor supply should respond less to the reform.

Subsequently, women who observe their sister taking a long leave - induced by the reform - observe a new set of prescriptions. Women with sisters in the control group observe their sister taking a relative short leave. These two groups of women are exposed to different prescriptions, and in turn, expected to behave differently even though they face the same institutional set-up. If the reform induced prescriptions of extended maternity leave, those with a sister in the reform treatment group experience these prescriptions of extended leave via their sister, while those in the control group do not. This should show up as peer effects in the empirical investigation.

[11] Leave is not fully compensated. This is explained in greater details in '2.3.1. Institutional Context'.

2.3 Identification and Empirical Strategy

This section provides an overview of the institutional setting before and after the reform, the data used, and the empirical strategy with an emphasis on the assumptions required for the identification of reform and peer effects.

2.3.1 Institutional Context

Denmark has, like the other Nordic countries, a long tradition of family-friendly policies enabling the vast majority of mothers to participate in the labor market (Datta Gupta et al., 2008). These policies include heavily subsidized day care for children, paid parental leave, and job protection while on leave. Moreover, couples in Denmark face individual taxation - rather than joint taxation as in Germany or the US - which creates a strong incentive for women, who often are secondary earners, to participate in the labor market (Selin, 2014). In the 1990s, 84 % of Danish mothers with children below the age of 10 worked outside the home and 2/3 worked full time (Leira, 2010). Since the '80s, the duration of parental leave with economic compensation has been gradually expanded (see Andersen (2018) for an overview), and childcare reached almost universal coverage in 2000 (Leira, 2010). While paternal leave formally is available to both parents, it is viewed as something relevant for mothers (Datta Gupta et al., 2008) who by tradition have been given the responsibility of childcare. In contrast to all other Nordic countries, where some version of a "daddy quota" is in place, Danish policy makers have refrained from implementing such policies and argued that parents – not the government – should decide the distribution of leave (Deding & Holt, 2012).[12]

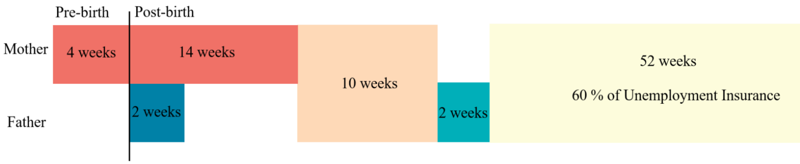
Figure 2.1 provides an overview of the 2002-reform which reorganized the parental leave system. In short, the duration of well-paid leave was extended by 22 weeks. Childcare leave, which was poorly compensated, was abandoned and shared parental leave was increased by 22 weeks. At the same time, the reform reduced the number of weeks allocated to the father by two weeks. This change implied better economic compensation from week 26 to 46 where new parents would receive compensation corresponding to full unemployment insurance. Parents could receive compensation of 90 % of former earnings up to a flat rate with an average compensation rate of 66 % (Datta Gupta et al., 2008). In addition to the public transfer, some employers pay additional compensation which is often determined through collective bargaining. While there are large sectorial differences in both level and duration, the vast majority of new parents would face a period after week 24 with compensation substantially lower than their labor market earnings.[13] Substantially flexibility was provided in terms of how and when to use the

[12]A two week daddy quota was in place from 1998 and abolished with the reform in 2002. As of August 2022, new fathers' are entitled to 2 months of leave. This is a results of the EU-directive on work-life balance for parents and carers (Council of the European Union, 2019).

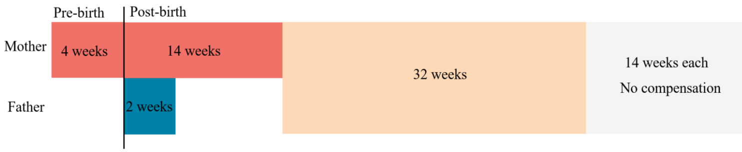
[13]The public sector has a longer history of generous leave schemes than the private sector. At the time of the reform, women in the public sector received full salary for 14 weeks after giving birth and then

Figure 2.1. Institutional Setting

(a) Duration of economic compensation (UI) before the reform of January 2002



(b) Duration of economic compensation (UI) after the reform of January 2002



shared leave, incl. simultaneous leave of both parents, part-time leave, and postponement of leave until the child turned 8. Ensuring leave beyond 48 weeks, each parent can extend their leave for up to 14 additional weeks with employment protection but without benefits. Prior to the reform, parents were entitled to a total period of 28 weeks with compensation after childbirth of which 4 were allocated to the father, 14 to the mother, and 10 could be shared. This period was followed by a period of 52 weeks at a reduced rate corresponding to 60 % of the previous benefit[14]. With the reduction in leave specifically allocated to fathers, I argue that the policy was conceived as primarily relevant for mothers. The empirical investigation supports this.

The reform was presented in Parliament on January 7 2002 and adopted on March 27 of the same year. For all parents of children born on or after January 2, the new rules apply. Parents with a child born between the January 2 and March 27 were given the option to choose between the two schemes. Results show a jump in average leave duration of mothers on January 2 and no change in the average leave duration of fathers. On March 27, the change in average leave is barely visible, implying that the vast majority of couples preferred the new scheme. With similar results, Beuchert, Humlum, and Vejlin (2016) argue that almost all parents choose the post-reform rules if given the option.[15] In further support of the unexpectedness of the reform, a parliament election took place in November 2001 leading to a change in government. The incumbent government campaigned on earmarked paternity leave, while the opposition's promises

up 10 weeks which could also be transferred to the father if he also worked in the public sector. This provides women with up to 24 weeks of fully paid leave. After the period of paid leave from the employer, new parents would be on benefits equivalent to unemployment insurance.

[14] If household consumption smooth, monetary benefits are only reduced in families that take more than 70 weeks of leave with the total maximum being 80 weeks before the reform and 76 after the reform.

[15] Beuchert et al. (2016) investigate health effects on mothers and children from the increase in leave duration of mothers. Nielsen (2009) and Tô (2018) also show substantial change in leave behavior among mothers at January 1 2002.

were less precise. There was no reason to suspect such a major change immediately after the new government took office. The rapid implementation of the reform implies that no self-selection can occur. The discontinuity that arises from the reform provides a close-to-ideal set-up for evaluating both the reform effect and peer effects.

2.3.2 Data

To evaluate the effects of the reform and subsequent peer effects on leave duration, I combine information from several administrative registers obtained via Statistics Denmark. This data contains individual records and covers the full Danish population with a high degree of precision and allows for the identification of all children and their parents. I use information on all parents who had a child between March 2001 and December 2005.[16] Family identifiers further allow for identification of mothers and sisters of the women in the reform window. The final data set includes rich covariates incl. education, labor market information, and historical labor supply of the maternal grandmother of the child. Details are reported in Appendix 2A. Labor market information for the parents is from the year prior to childbirth. This avoids any mechanical effects from income reduction while on leave or any confounders due to job changes (Nielsen, Simonsen, & Verner, 2004). For maternal labor supply of the previous generation (i.e. the maternal grandmother of the child), I follow Kleven et al. (2019) and obtain a historical measure of cumulated maternal labor supply that the new mother was exposed to as a child.

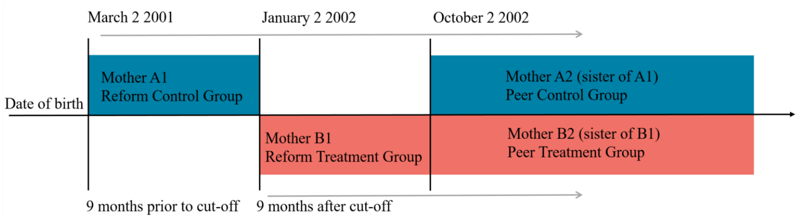
To measure the length of leave, I use information on weekly benefits from the DREAM-register. I construct a variable containing a count of weeks during which a parent receives compensation due to parental leave a year following the birth of their child. This measure includes the full compensation corresponding to unemployment insurance and the reduced rate that was in place before the reform. The measure does not include leave taken prior to childbirth (pregnancy leave). Potential top-ups from employers are not observed. To ensure that both parents are entitled to full compensation during leave, households where either parent is enrolled in education, self-employed, or loosely affiliated to the labor market are excluded. Similar to Beuchert et al., 2016, I impose a restriction so only mothers with at least 2 weeks of paid leave are included.[17] Mothers are required to take two weeks of leave after childbirth, so mothers without any leave registered are likely not entitled to paid leave (i.e. they are not participating in the labor market). It is not possible to impose the same restriction for fathers, as they are not required to take any leave. I only include the first child of parents who had multiple children between 2001 and 2005.

[16] In December 2005, a new law that required all private sector employers to pay contributions to a Parental Leave Fund was announced. In turn, employers would be reimbursed for salaries paid during parental leave. As this law changed the economic incentives for leave-taking for parts of the population, 2005 will be the end year of this analysis.

[17] I further restrict the sample and exclude twin births, same-sex parents, and households where at least one parent does not live with their child. The consequences of the restrictions for the sample size are reported in the Appendix 2B.

I divide the population of parents into four groups: reform control, reform treatment, peer effect control, and peer effect treatment. The reform control group consists of the parents who had a child prior to the reform. The reform treatment group consists of parents who had a child after the reform and could not know about the reform at the time of conception. These groups are used to evaluate the reform effects on both mothers and fathers. Both the peer effect control group and peer effect treatment groups contain mothers who had a child after the reform was implemented and knew about the new rules at the time of conception. The difference between these two groups is the date when their sister had a child. The four groups are depicted in Figure 2.2.

Figure 2.2. Reform group, peer group and peer effects



Mother A1 refers to a mother who had a child nine months prior to the reform, and Mother B1 refers to a mother who had a child in the nine months following the reform implementation. Both Mother A2 and Mother B2 had a child after October 1 2002. Mother A1 and Mother A2 are sisters and Mother A1 was in the reform control group. Mother B2's sister is Mother B1, who was in the reform treatment group.

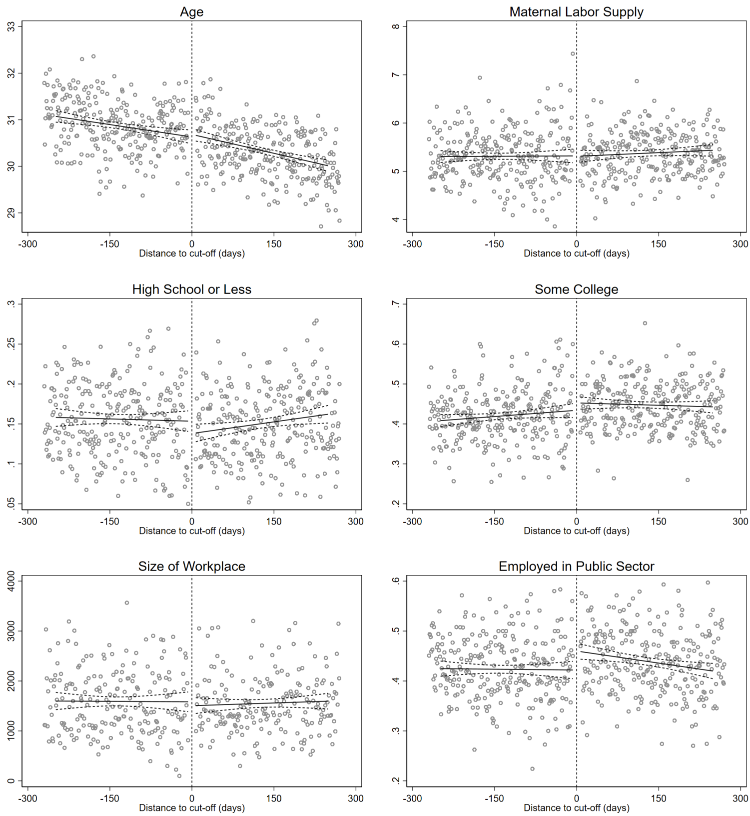
When the aim is to identify peer effects in naturally occurring peer groups, it raises a 'many-to-one' issue as multiple peers can affect the same individual. This problem arises if more than one peer became a parent around the reform date, particularly if there is a peer before implementation of the reform and another peer after. Dahl et al. (2014) only include networks where exactly one peer has a child in the reform window. Similarly, I drop mothers who have a child after October 1 2002 and have two or more sisters who give birth in the reform window. This also addresses the issue of using leave-out-means as measures of peer behavior raised by Angrist (2014) and Sacerdote (2014).

Formal checks show that the number of observations drops before cut-off. This is normally a sign of manipulation into treatment. However, inspection of the data shows that this occurs every year. Both formal checks and a graphical inspection of the drop in births around New Year is reported in the Appendix 2C. Why this happens is not obvious: It could be due to planned fertility, planned C-sections, and labor induction during the holidays. For this reason, observations 7 days before and after the cut-off are dropped.[18] The final sample used to investigate reform effects contains 21,475 mothers

[18]In '2.4.4 Robustness', I show that whether or not I include observations close to cut-off or I have larger 'donut', point estimates are extremely stable, providing reassurance that manipulation didn't take place.

in the control group and 22,481 mothers in the treatment group. The sample for investigating peer effects contains 1,915 mothers in the control group and 1,928 mothers in the treatment group.

Figure 2.3. Pre-determined covariates, Mothers in the reform window



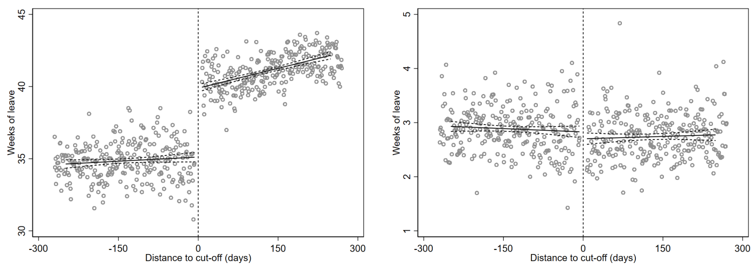
Notes: The figures show value of key covariates of mothers' in the reform window: age, her mothers' labor supply, a dummy indicating high school or less education, a dummy indicating some higher education, size of her workplace and a dummy indicating public sector employment. The running variable is date of child-birth of own child. Cut-off is 1st of Jan 2002 and the window on each side of cut-off is 9 months. The sample size is 44,316 couples. Each bin includes 50 observations and kernels are uniform.

To further rule out manipulation around the implementation date, I show the continuity in key covariates of the mothers over the implementation date. If certain groups manipulated the date of child birth, we would see a jump in these covariates at the cut-off date. This is depicted in Figure 2.3. I show age, maternal labor supply (of the grandmother of the child), a dummy indicating little formal education (high school or less), a dummy indicating some college (2-year degree or more), and characteristics of her workplace. There is a small jump in the likelihood of the mothers working in the public sector, but overall the two groups are very similar. For the remaining five variables there is no difference in the value at the reform implementation date, indicating a very valid research design.

Figure 2.4 shows the discontinuity in average leave duration at reform implementation for mothers and fathers, respectively. Mothers increase their leave with about 5 weeks at reform implementation, and there is no change in the average leave duration of fathers. The effects reported here are in line with results by other studies using this reform. Beuchert et al. (2016) focus on mothers' leave and report a 32-days increase in the leave duration of mothers. They consider a window of 60 days, where I use 9 months. Nielsen (2009) considers couples where both parties are employed in the public sector, and report larger estimates (approx. 50 days). In general, public sector employment is associated with longer leave of both parents. Gallen (2019) include sickness leave and find that women increase their time away from work by almost 7 weeks.

Figure 2.4. Leave Duration around the Reform Window

(a) Mothers' Leave (weeks) **(b)** Fathers' Leave (weeks)

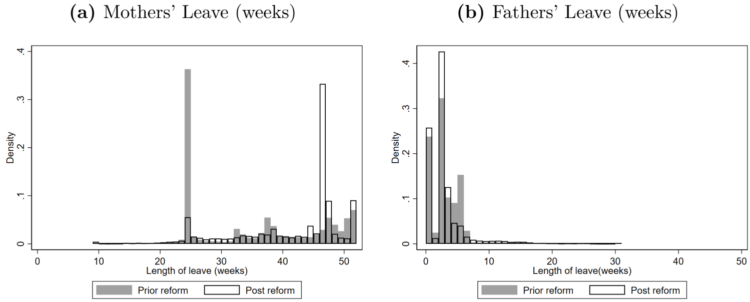


Notes: The figures show average leave duration measured in weeks of mothers and fathers with children born in the reform window. This measure does not include leave taken prior to child-birth. The running variable is date of child-birth of own child. Cut-off is 1st of Jan 2002 and the window on each side of cut-off is 9 months. The sample size is 44,316 couples. Each bin includes 50 observations and kernels are uniform. Using a quadratic fit barely makes a difference.

Figure 2.5 depicts two histograms of the leave duration for mothers and fathers, respectively. For mothers, there is a substantial shift to a longer leave. Before the reform, the mode leave duration was 24 weeks with 37 % of all mothers ending their leave at this point. At this point, the benefits equivalent to UI are exhausted. After the reform, only 5 % of all mothers end their leave at 24 weeks. The new mode is 46 weeks with 34 % of all mothers, which is the new maximum duration of compensated leave.[19]

[19]Longer leave than 46 weeks is taken at a low rate using left over leave from any child born when the old scheme were in place or without any compensation for up to 14 weeks where employment protection is in place.

Figure 2.5. Histogram of Reform Effect



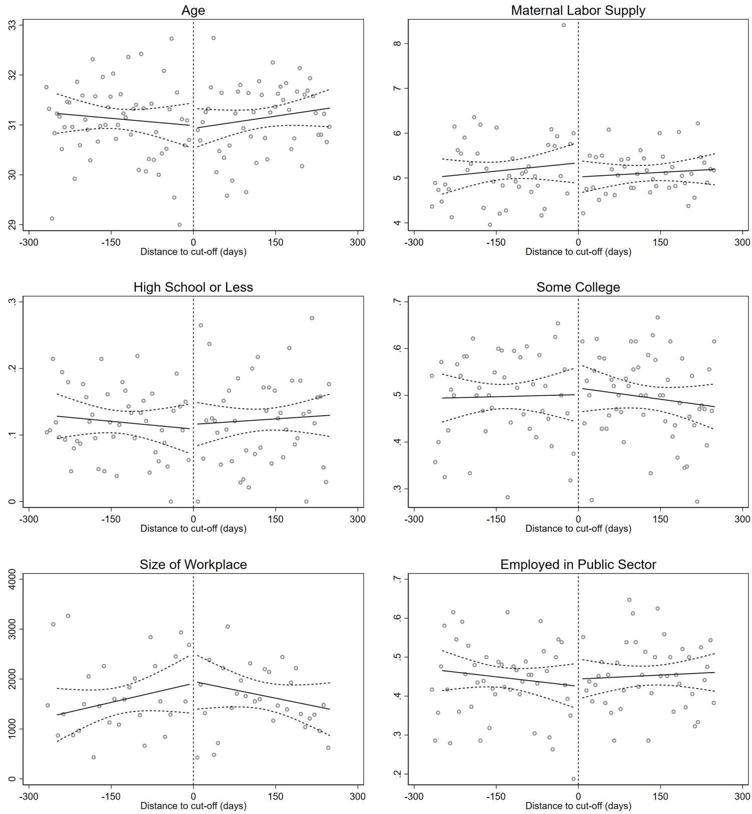
Notes: The figures show the distribution of weeks of parental leave prior to and after the reform for mothers and fathers. This does not include leave taken prior to child-birth. The Post reform group contains parents that have a child up to 9 months after 1st of Jan 2002 and the Prior Reform group contains parents that have a child up to 9 months before 1st of Jan 2002. The sample size is 44,316 couples.

However, for fathers, the mode leave duration both before and after the reform is 2 weeks, with 33 % of all fathers taking two weeks before the reform. With the reform, this share increases by 10 %-point. At reform implementation, the share of fathers who take 4 weeks of leave is reduced by 12 %-points. Moreover, 25 % of all fathers have no leave registered both before and after the reform. At first sight, this might seem like a registration issue, but upon closer inspection, this is also the case in the public sector where registration issues are believed to be of smaller concern. This is reported in Appendix 2C.

Meanwhile, a longer and more dense tail shows that some but few fathers increase their leave. In other words, the reform implied that most fathers reduced their leave, but a small share substantially increased their leave. The picture in Figure 2.4 showing no reform effect on fathers' leave duration hides substantial heterogeneity. This will have consequences for the empirical strategy.

Finally, I document that there is no signs of manipulation into treatment among the mothers who have a sister in the reform window. Manipulation would require them to control the birthday of their niece or nephew. Figure 2.6 contains the same variables as Figure 2.3. Unsurprisingly, there is no difference across treatment and control group.

Figure 2.6. Pre-determined covariates, Mothers with sisters in the reform window



Notes: The figures show value of key covariates of mothers' with a sister in the reform window: age, her mothers' labor supply, a dummy indicating high school or less education, a dummy indicating some higher education, size of her workplace and a dummy indicating public sector employment. The running variable is date of child-birth of the sister's child. Cut-off is 1st of Jan 2002 and the window on each side of cut-off is 9 months. The sample size is 2842 sister-pairs.

2.3.3 Empirical Strategy

The reform improved compensation for maternity leave with a duration beyond 24 weeks and paternity leave beyond 4 weeks. This creates a discontinuity in leave duration on January 1 2002. I use this to implement a sharp Regression Discontinuity Design (RD-design) to estimate the reform effect. Following the work by Dahl et al. (2014), I implement a two-stage-least-squared (2SLS) estimator to estimate the peer effects on mothers' leave behavior. As the reform implies that the probability of being exposed to a peer who takes a long leave increases drastically at cut-off, I can implement a fuzzy RD to estimate the peer effects. I also estimate the reduced-form.

The main identifying assumptions are that parents in the reform window are not able to control the day of birth of their own child. The fast announcement and implementation of the reform imply that this is close to impossible. The reform was implemented with retrospective effects: it was announced in the first week of January 2002, but policy makers allowed all couples with a child born on January 2nd or later to use the new scheme. The Parliament Election in November 2001 further supports the unexpectedness of the reform. For sisters exposed to the peer effects of extended leave, they should not be able able to control the day of birth of their peer's child. This seems even more unlikely to occur, especially taking the unexpectedness and rapid reform implementation into consideration.

When estimating peer effects, it is often an issue that peers affect each other and the researchers cannot observe the direction of this. This is what Manski (1993) refers to as 'the reflection problem'. I solve this with a time dimension that only allows the peer effect to operate in one direction. Manski (1993) also highlights the issues of endogenous group membership and correlation of unobservables due to contextual effects. By exploiting the fact that the reform is orthogonal to covariates and by defining group membership prior to treatment, the concerns voiced by Manski (1993) on the identification of peer effects should no longer be a concern. Treatment is then as good as randomly assigned.

The outcome of interest is a discrete variable counting the number of weeks that parents are receiving benefits due to parental leave. The assignment variable is the date of birth of the child, d_i. T_i is the treatment indicator for whether individual i (parent in the reform window) had a child prior to or after cut-off, d_0, January 1 2002:

$$T_i = 1[d_i \geq d_0] \tag{2.1}$$

where d_i is the distance (in days) from January 1 2002 to the birthday of the child of individual i. If the child is born on or after January 1, $T_i = 1$, and if the child is born before, $T_i = 0$. There is no jump of the treatment indicator, so any jump of the outcome at cut-off can be interpreted as the causal average effect of treatment (Imbens & Lemieux, 2008).

The reform effect for the full population with the outcome variable, L_i, indicating the length of leave of individual i is given by:

$$L_i = \beta_0 + \beta_1[d_i|d_i < d_0] + \beta_2 T_i + \beta_3[d_i|d_i \geq d_0] + X_i + \varepsilon_i \tag{2.2}$$

where β_2 can be interpreted as the reform effect. β_1 and β_3 can be interpreted as the slopes on either side of the cut-off. X_i is a vector that contains individual characteristics. Variables that potentially vary over time (e.g. earnings and sectorial occupation) are measured the year prior to childbirth.

To test the predictions regarding the reform effect outlined in Section 4.2, I interact the treatment indicator T_i with a dummy D_i indicating relative earnings and intergenerational maternal labor supply, respectively.

$$L_i = \beta_0 + \beta_1[d_i|d_i < d_0] + \beta_2 T_i + \beta_3[d_i|d_i \geq d_0] + \beta_4 T_i \times D_i + \beta_5 * D_i + X_i + \varepsilon_i \tag{2.3}$$

First, I interact the treatment indicator with a dummy taking the value 1 when the woman in the couple earns more than the man. I perform this exercise when evaluating the reform effect for both mothers and fathers. Then β_2 can be interpreted as the reform effect on those couples where the man is the primary earner. β_4 captures the additional reform effect on couples where the woman earns more than the man, and β_5 captures the initial difference in level across these two types of couples. This allows me to test the predictions from a Becker (1981)-model. When the outcome of interest is mothers' leave duration, we should expect $\beta_4 < 0$, and when investigating the effect of fathers' leave $\beta_4 > 0$. Second, I interact the treatment indicator with a dummy taking the value 1 in the case of low maternal labor supply. In this case, β_2 can be interpreted as the reform effect on the women that experienced a high maternal labor supply in childhood. Equivalently, β_4 captures the additional reform effect on women who grew up with a mother with a low labor supply, and $\beta_4 > 0$. β_5 captures any initial difference across these types of women. This allows me to test for the role of gender identity for mothers' take-up of leave. To investigate other types of heterogeneity, I implement models where I interact the treatment indicator with public sector employment and child parity.

Turning to my estimation of the peer effects, I adopt an 2SLS-estimator following the work by Dahl et al. (2014). The first-stage is equivalent to Equation 2.2:

$$L_i = \beta_0 + \beta_1[d_i|d_i < d_0] + \beta_2 T_i + \beta_3[d_i|d_i \geq d_0] + X_{ip} + \varepsilon_i \tag{2.4}$$

X_{ip} is a vector that contains individual and peer characteristics. For both the mother in the reform window and the sister, education is included, the relative education of both households, absolute and relative income in both households, sectorial dummies for occupation and whether or not they are first-time mothers. Again, variables that change over time are measured the year prior to childbirth. The fitted values from the first-stage, $\hat{L}_i$, are used to estimate the peer effects on individual p, δ_2, in the second-stage:

$$L_p = \delta_0 + \delta_1[d_i|d_i < d_0] + \delta_2 \hat{L}_i + \delta_3[d_i|d_i \geq d_0] + X_{ip} + d_p + \varepsilon_p \tag{2.5}$$

δ_1 and δ_3 are the slopes of either side of the cut-off. A control for date of birth of the mother p's own child is added to capture any general time trend.

An alternative empirical strategy is the reduced form:

$$L_p = \lambda_0 + \lambda_1[d_i|d_i < d_0] + \lambda_2 T_i + \lambda_3[d_i|d_i \geq d_0] + X_{ip} + d_p + \varepsilon_p \tag{2.6}$$

In this case, the parameter λ_2 can be given an Intension-To-Treat (ITT)-interpretation. This estimate is the difference in leave decision among mothers who had peers with children born prior to and after the cut-off. The advantage of the reduced form is that it requires fewer assumptions to estimate the peer effect. I also extend this model with interactions as in Equation 2.3 to investigate heterogeneity.

Three assumptions are needed to interpret the obtained estimates obtained as the Local Average Treatment Effects (LATE). These assumptions are the exclusion restriction, the independence assumption, and the monotonicity assumption.

For the reform effects, the exclusion restriction holds if the behavior is only affected through the institutional set-up. This implies that there would have been no change in leave behavior in the absence of the reform. The independence assumption implies that treatment is as good as randomly assigned. As mentioned above, the implementation of the reform was unexpected and rapid, implying no selection into treatment is possible. The graphical inspection reported in Figure 2.3 supports this. As the reform allowed for a longer leave with a better compensation rate but removed the duration with lower compensation, defiers among mothers could be a concern. However, as argued both here and by Beuchert et al. (2016), data inspection shows that most couples choose the new scheme when given the option. As depicted in Figure 2.5, 37 % of mothers previously took leave at the maximum duration with high benefits. After the reform, this share drops to 5 %, and the majority of mothers now take 46 weeks of leave, which is the new maximum. This suggests that the duration of leave with high benefits is an important factor. The monotonicity assumption for mothers in the reform window is then a small concern. However, monotonicity concerns arise regarding fathers' leave: the reform implied that a large share reduced their leave from 4 to 2 weeks, while a smaller share started to take a long leave. Therefore, I implement an alternative specification with the outcome variable being a dummy that takes the value 1, when the father takes a long leave (defined as 8 weeks or longer). This allows little room for defiers.[20] In this specification, the monotonicity assumption is met for fathers.

For the peer effects, the exclusion restriction implies that the only way that the birthday of the peer's child affects behavior is through the observed behavior of the sister in the reform window. This requires that there is no difference in leave decisions of mothers across the peer effect treatment group and control group in the absence of the reform. All the mothers experience the same institutional set-up and other changes (e.g. business cycles or changes in day care availability) should on average affect the two groups

[20] Changing this to 6 weeks provides virtually unchanged estimates. A lower threshold does not deal with the monotonicity concern.

in the same way. The assumption of independence requires that mothers are as good as randomly assigned to the peer treatment group. Selection into treatment is highly unlikely possible and correlation on unobservables among sisters should be dealt with as a result of the rapid implementation of the reform. The balanced observables across the two groups reported in Figure 2.6 suggest that this is indeed the case.[21] The monotonicity assumption requires that no mother reduces her leave after being exposed to a peer effect from the reform treatment group. Using the concept of prescriptions, I assume a preference for similar behavior to that of peers. That is, the reform-induced change in behavior implies that the women with a sister in the control group observe different prescriptions than women with a sister in the treatment group. These women are expected to behave accordingly when they have a child later in time. The monotonicity assumption is not possible to test. However, if this assumption is not met, the reduced form stated in Equation 2.6 will still consistently estimate the effect of having a peer mother exposed to the new versus the old institutional set-up.

Overall, it seems reasonable that all three required assumptions are met for mothers when evaluating both reform and peer effects. For fathers the monotonicity assumption is violated so I also implement an alternative specification with the outcome being a dummy indicating a long leave (8 weeks or more). Any differences in behavior among parents in the reform window can be attributed solely to the reform. Any differences in behavior among mothers exposed to peers with a child born on either side of the cut-off can be attributed solely to the influence of peer effects.

2.4 Results

2.4.1 Graphical Results

An RD-design provides a transparent and illustrative way of visualizing identification of the treatment effects (Thistlethwaite and Campbell, 1960; Imbens and Lemieux, 2008). The graphical results reported in this section are without individuals level controls.

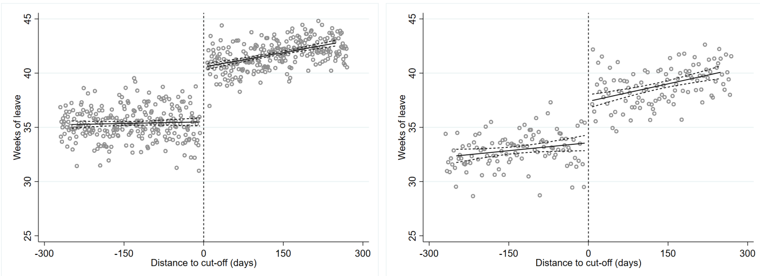
Figure 2.7 shows the average leave duration of the full population in the reform-window among mothers and fathers, split by relative earnings in the household. Theory of specialization predicts that mothers who are primary earners should respond less to the reform than mothers who are not primary earners. On the contrary, the reform affects the two groups similarly with a jump of 5 weeks. There is a difference in the initial duration of mothers' leave. In households where the mother earns less than the father, she takes 2 weeks longer leave compared to households where the mother earns the most. Among fathers who are primary earners, the reform leads to a small reduction

[21] A related concern is that some sisters coordinate fertility. Women in the reform treatment group and their sisters have children with closer spacing that those in the reform control group. I directly test for this in Section '4.3 Alternative Explanations' and rule out that my estimates of peer effects are driven by sisters with the closest spacing of births.

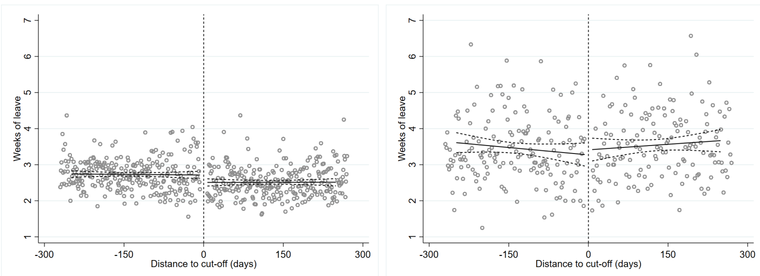
in average leave duration. In households where the mother is the primary earner, there is no change in the average leave duration of fathers. Again, a difference in leave duration across the two types of households remains largely unchanged. Both before and after the reform, fathers who are not primary earners take 1 week longer leave compared to fathers who are primary earners.

Figure 2.7. Graphical illustration of the reform effects split by relative earnings

(a) Mothers' Leave, Father earns most **(b)** Mothers' Leave, Mother earns most



(c) Fathers' Leave, Father earns most **(d)** Fathers' Leave, Mother earns most

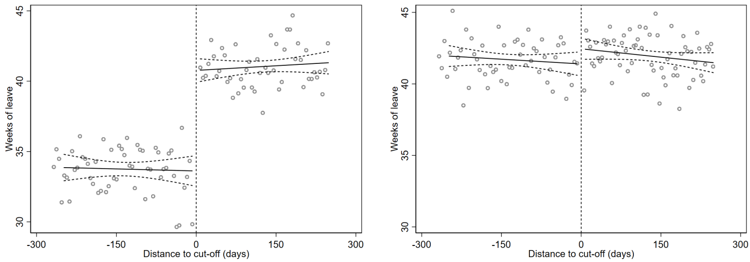


Notes: The figure shows average leave duration measured in weeks of mothers (top panel) and fathers (bottom panel) stratified by relative earnings in the household in the year prior to childbirth. The measure of leave does not include leave taken prior to child-birth. The running variable is date of birth of own child. Cut-off is 1st of Jan 2002 and the window on each side of cut-off is 9 months. The sample size is 44,316 couples. Each bin include 50 observations and kernels are uniforms. Using a quadratic fit barely makes a difference.

Figure 2.8 shows the average leave duration around reform introduction and the subsequent peer effects for sister-pairs. The reform window in Figure 2.8 illustrates the first-stage for mothers in the reform window who have a sister who gives birth between October 2002 and December 2005. There is a sharp jump in the average leave duration from 34 weeks to 41 weeks. The graphical depiction of the peer effects corresponds to the reduced form, showing that mothers with a sister in the reform treatment group do indeed take a longer leave than those with a sister in the reform control group. The difference is around 1 week and appears to be borderline significant even without individual and peer level controls.

Figure 2.8. Reform and Peer Effect, Sister-pairs

(a) Reform Effect **(b)** Peer Effect



Notes: The figure shows average leave duration measured in weeks of sets of sisters. On the right side, leave duration of the sister in the reform window is reported. On the left side, average leave duration of the sisters who themselves give birth between 1st of October 2002 and end of 2005 is reported. The measure of leave does not include leave taken prior to child-birth. The running variable is date of child-birth of the sister in the reform window. Cut-off is 1st of Jan 2002 and the window on each side of cut-off is 9 months. The sample size is 3,808 mothers with sisters in the reform window. Each bin includes 35 observations and kernels are uniform.

2.4.2 Regression-based Results

Table 2.1 presents the reform effects on mothers and fathers. The estimates for the baseline model for mothers are reported in column (1) and for fathers in column (3). Mirroring the results reported in Figure 2.4, the estimated reform effect on the mothers' leave is 4.9 weeks, and there is no effect on the average duration of fathers' leave. In column (2) and (4), I add an interaction term between relative earnings in the household and the treatment indicator, as outlined in Equation 2.3. Both before and after the reform, mothers in couples where the father is the primary earner take a longer leave compared to couples where she is the primary earner. Before the reform, the difference is 1.5 weeks. The interaction effect is far from being statistically significant. This corresponds to a longer initial duration among mothers who are not primary earners, but no additional reform effect. This is in contrast to what the theory of specialization predicts. As leave is not fully compensated, one parent will be on leave corresponding to UI while the other parent is earning their wage. Before the reform, fathers are outearned by their partner take a 0.6 weeks longer leave compared to fathers who are primary earners. In the baseline specification, the reform effect is insignificant. However, in the interacted specification, fathers who are primary earners are reducing their leave with 0.2 week upon the reform. Adding the reform effect and the interaction term together show that fathers who were not primary earners did not change their leave duration.[22]

[22]A alternative measure of productivity would be education. Using relative educational attainment provides very similar results, i.e. no meaningful heterogeneity. This is reported in Appendix 2D.

Table 2.1. Reform effects on leave duration, effect from relative earnings

	(1)	(2)	(3)	(4)	(5)	(6)
Outcome	Mothers' leave duration (weeks)		Fathers' leave duration (weeks)		Fathers' taking long leave (dummy=1 if leave $\geq$ 8 weeks)[a]	
	Baseline	Interaction	Baseline	Interaction	Baseline	Interaction
Reform effect	4.921***	4.715***	-0.136	-0.196**	0.0163***	0.0104***
	(0.219)	(0.288)	(0.0830)	(0.0830)	(0.00453)	(0.00711)
Reform X Mother primary earner		-0.262		0.278***		0.0279***
		(0.226)		(0.0925)		(0.00512)
Mother primary earner		-1.517***		0.593***		0.0374***
		(0.200)		(0.109)		(0.00635)
Observations	44,091	44,091	44,091	44,091	44,091	44,091
R-squared	0.128	0.130	0.028	0.032	0.025	0.041
Controls						
Household covariates	YES	YES	YES	YES	YES	YES
Time trend	YES	YES	YES	YES	YES	YES

Notes: The sample used for estimation includes parents who had a child in the 18-month reform window. All specifications include the running variable (d_i, date of birth) and the running variable interacted with an indicator for whether childbirth occurred before or after cut-off. Full regression reported in Appendix 2D.
Standard errors in parentheses are clustered on date of birth of child where *** p<0.01, ** p<0.05, and * p<0.1
[a]Changing this to 6 weeks provides virtually unchanged estimates. A lower threshold does not deal with the monotonicity issue as many fathers took 4 and 5 weeks of leave before the reform, as reported in Figure 2.5.

Column (5) and (6) present an alternative specification of the reform effects on fathers to shed light upon those fathers who take long leaves upon reform implementation. Defining the outcome as a dummy that takes the value 1 if the fathers take a leave of 8 weeks or longer, the reform implies an increase in 1.6 %-point probability of fathers taking a long leave. When adding an interaction term, we see that fathers who are not primary earners are more likely to take a long leave compared to those who are primary earners. With the reform, the size of this effect increases with 2.8 %-point, from 3.7 %. Overall, relative earnings have a very small impact on the reform effect. Regardless of relative earnings, mothers respond similarly to the reform while fathers' leave duration is largely unchanged. Among the fathers with partners who out-earn them, the reform increases the likelihood of a long leave by 2.8 %-point. While this is in the direction of what theory of specialization predicts, the magnitude is tiny compared to the 4.7 weeks increase in leave duration of the mothers in these households. Thus, the predictions provided by theory of specialization are not matched by the data.

The controls enter with the expected sign when they are significant (see Appendix 2D), but an interpretation of the controls should keep in mind that they are likely to correlate with unobservables. Notably, the estimates do not change whether the controls are included or not (see Section '2.4.4 Robustness' below).

While the homogeneous response across relative earnings is indicative of the importance of gender norms for mothers' leave behavior, Table 2.2 presents the estimates obtained from Equation 2.3 with interaction terms to shed light on the role of inter-generational female labor supply, sectorial occupation, and child parity. Column (2) contains an interaction term between the labor supply of the maternal grandmother (of the child born in the reform window) and the treatment indicator; Column (3) contains an interaction term between public sector employment of the mother and the treatment indicator and Column (4) contains an interaction term between first-time mothers and the treatment indicator. Lastly, Column (5) contains a model with all interaction terms. Women who grew up with a mother with a high labor supply respond less strong to the reform. This is in line with the notion that gender identity is strongly influenced by the childhood environment and the literature showing inter-generational transmission of gender identity and labor market choices. Before the reform, there was no difference in leave duration across those who had a mother with a high or low labor supply, arguably as the total leave with high compensation was too short. However, with the reform, those who had a mother with a low labor supply increase their leave by approx. half a week more than those with a mother with a high labor supply.

Across sectorial occupation, there were substantial differences in average leave duration across the public and private sectors before the reform. Mothers working in the public sector took a 2.4 week longer leave than those working in the private sector. After the reform, a gap remains but the size is reduced. This is driven by a smaller reform response among mothers working in the public sector, who increase their leave duration by -1.5 weeks less than women in the private sector. That mothers employed in the private sector respond strongly and increase their leave by 5.6 weeks suggests that preferences for family-friendly work arrangements are not driving the results. The public sector in Denmark is known for offering more family-oriented amenities, while the private sector penalizes absenteeism more (Nielsen et al., 2004). Thus, it is not surprising that publicly employed mothers take a longer leave than those employed in the private sector, but the reduction in this gap is remarkable. I find a similar pattern for child parity although the magnitude is smaller. Before the reform, first-time mothers took half a week longer leave compared to mothers of higher parity. However, with the reform, this gap is reduced. The heterogeneity across sector and child parity is greatly reduced with the reform implying that new mothers start to behave more similar. While there is a meaningful difference in the reform response across mothers with high and low maternal labor supply, the magnitude is only 1/6 of the difference across the private and public sectors prior to the reform. The more homogenous leave behavior in the population is driven by larger reform effects among mothers with characteristics that would have suggested a short leave in the absence of the reform.

Table 2.3 presents the estimates of the peer effects for the sisters. The first-stage is reported in column (1). The 2nd stage estimate is reported in column (2) and shows a 17

Table 2.2. Reform effect on mothers' leave duration, alternative specifications

Outcome: Weeks of leave	(1) Baseline	(2) Maternal labor supply	(3) Sector	(4) Child parity	(5) Full model
Reform effect	4.912***	4.708***	5.566***	5.063***	5.485***
	(0.220)	(0.240)	(0.234)	(0.228)	(0.349)
Reform X		0.384**			0.324*
Low maternal labor supply		(0.180)			(0.180)
Low maternal labor supply		0.0199			0.0389
		(0.140)			(0.140)
Reform X			-1.540***		-1.565***
Publicly employed			(0.191)		(0.192)
Publicly employed			2.414***		2.286***
			(0.157)		(0.157)
Reform X				-0.350*	-0.400**
First-time mother				(0.181)	(0.182)
First-time mother				0.555***	0.620***
				(0.146)	(0.147)
Reform X					-0.242
Mother earning most					(0.229)
Mother earning most					-1.527***
					(0.200)
Observations	44,091	40,249	44,091	44,091	40,249
R-squared	0.129	0.127	0.129	0.127	0.130
Controls					
Household covariates	YES	YES	YES	YES	YES
Time trend	YES	YES	YES	YES	YES

Notes: The sample used for estimation includes parents who had a child in the 18-month reform window. All specifications include the running variable (d_i, date of birth) and the running variable interacted with an indicator for whether childbirth occurred before or after cut-off. Maternal labor supply is defined as above or below the median in the sample. Sector and earnings are from the year prior to childbirth. Standard errors are clustered on date of birth of own child where *** p<0.01, ** p<0.05, * p<0.1

% increase in leave duration compared to the reform effect. The reform-induced change in behavior of mothers in the reform treatment group implies that the peer sisters observe different prescriptions, depending on when their niece/nephew was born. They change their behavior accordingly so that those exposed to the behavioral norm of long leave take a longer leave themselves, and this shows up here as peer effects. Column (3) reports the reduced form corresponding to Figure 2.8 with individual level and peer controls which improve precision. The point estimate corresponds to 1.1 weeks of additional leave among mothers with sisters who had a child after reform implementation.

TABLE 2.3: Peer effects on mothers' leave duration

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	1st	2nd			Reduced Form					
	stage	stage		Maternal	Own	Peer	Own	Peer	Own	Peer
			Baseline	labor supply	sector	sector	earnings	earnings	1st child	1st child
Peer effect	6.815***	0.168**	1.145**	1.423**	1.349**	0.986*	1.171***	1.255**	0.948	1.326**
	(0.709)	(0.0809)	(0.554)	(0.655)	(0.601)	(0.598)	(0.562)	(0.571)	(0.644)	(0.599)
Peer effect X				-0.524						
Low maternal labor supply				(0.536)						
Low maternal labor supply				0.766*						
				(0.392)						
Peer effect X					0.982***					
Publicly employed					(0.413)					
Publicly employed					1.446**					
					(0.429)					
Peer effect X						0.360				
Sister publicly employed						(0.572)				
Sister publicly employed						0.253				
						(0.421)				
Peer effect X							-0.119			
Mother primary earner							(0.627)			
Mother primary earner							-0.910			
							(0.627)			
Peer effect X								-0.498		
Sister primary earner								(0.702)		
Sister primary earner								0.152		
								(0.600)		
Peer effect X									0.386	
First-time mother									(0.551)	
First-time mother									0.146	
									(0.314)	
Peer effect X										-0.450
Sister first-time mother										(0.544)
Sister first-time mother										0.396
										(0.373)
Observations	3,154	3,154	3,154	3,154	3,154	3,154	3,154	2,996	3,154	3,154
R-squared	0.172	0.064	0.076	0.064	0.064	0.064	0.064	0.064	0.067	0.065
Controls										
Peer covariates	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Own coviartes	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Time trend	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES

Notes: The sample includes women who had a child which was conceived after the reform was announced *and* a sister with a child born in the 18-month reform window. All specifications include the running variable (d_i, date of birth) and the running variable interacted with the treatment indicator. Maternal labor supply is defined as above or below the median in the sample. Sector and earnings are from the year prior to childbirth. Standard errors in parentheses are clustered on date of birth of peer child where *** p<0.01, ** p<0.05, and * p<0.1.

Additional interaction terms are added to the reduced form. Column (4)-(10) show the reduced form model with interactions terms for both own and peer category. The point estimate increases in size when adding the interaction effect of the labor supply of the maternal grandmother, sectorial employment of the mother exposed to the peer effects from the reform, and child parity of the sister in the reform window.

Women who were exposed to low maternal labor supply in childhood take a longer leave prior to the reform. However, the reform-induced change in prescriptions strongly reduces inter-generational effects from maternal labor supply and the peer effect is larger for those exposed to high maternal labor supply. Mothers working in the private sector took a shorter leave compared to those working in the public sector irrespective of when their sister had a child. However, this difference across sectorial occupation is reduced if the sister was in the reform treatment group. This is driven by mothers in the private sector responding stronger upon their sister taking a long leave. This mirrors the investigation of heterogeneous effects across women in the reform window; with the reform and peer effects, variance in leave duration is reduced, and this is driven by larger responses among mothers with characteristics indicating that they would have taken a short leave in the absence of treatment.

2.4.3 Alternative Explanations

Women increase their leave behavior upon reform implementation while fathers' leave behavior is largely unchanged, regardless of relative earnings. Women who had a working mother themselves increase their leave with less than those who had a stay-at-home-mother. Subsequently, women with sisters in the reform treatment group take a longer leave compared to those who have a sister in the reform control group. These results are highly consistent with the notion of pay-off from gender identity. However, a number of alternative explanations are investigated. While arguments related to biology, in particular a wish for extended breastfeeding, could explain the gender gap in take-up after the reform. However, this can neither explain the heterogeneous effects by maternal labor supply nor the peer effects. Two channels are proposed to explain peer effects, but fail to provide a meaningful explanation for the reform effect: information and consumption externalities. I argue that the reform was public knowledge when the sisters went on leave. Moreover, studies that evaluate leave of this length overwhelmingly show that private benefits are zero to small. By investigating heterogeneity by geographical proximity and close spacing of births, I directly show that consumption externalities are not driving the peer effect. In sum, no other explanation than that of gender identity provide a compelling explanation of both the reform and peer effect.

Biology

Biological differences between men and women, in particular breastfeeding, are often proposed as a potential explanation for diverging labor market outcomes after parenthood. In this setting, the average leave duration prior to the reform well extended the recommended period of full breastfeeding (Sundhedsstyrelsen, 2008). In 2002, 13.2 % of Danish children were exclusively being breastfed when they turned 6 months, and this number has been fairly stable (Johansen et al., 2016). At 4 months, this number was 63.3 % in 2002. Thus, the extended leave is being held when the child is reaching an age where other types of food are becoming an increasingly important component of the diet. Moreover, earmarked leave for mothers ensures 'sick days' for 3 months after childbirth and this component of the leave system was unchanged by the reform.[23] Recent research has provided compelling evidence against the physiological aspects of motherhood as the main factor contributing to the unequal division of care work. Biological and adoptive mothers face almost identical post-childbirth labor market trajectories (Rosenbaum, 2021; Kleven et al., 2021). Moberg and van der Vleuten, 2021 show striking similarities in the division, length, and timing of parental leave for biological and adoptive children. Among same-sex couples, the drop in earnings is smaller for the birth parent compared to heterosexual couples (Moberg, 2016; Nix and Andresen, 2019; Moberg et al., 2021). In contrast to different-sex couples, Moberg et al. (2021) find that social parents in same-sex couples also experience a drop in earnings. Rosenbaum (2019) and Nix and Andresen (2019) find no meaningful within household differences in earnings trajectories following parenthood. This is also the case when accounting for intra-household earnings gaps prior to childbirth. Importantly, neither the heterogeneous effects by maternal labor supply nor the peer effects can be explained by any factors related to biology.

Information

One mechanism that could explain the peer effects is information transmission. In particular two types of information come to mind; information about the reform and information about the private benefits of extended leave. Regarding the former, it seems very unlikely that the mothers having a child later than in the fall of 2002 did not know about this reform. First, this reform was widely reported in Danish media. Second, pregnancy arguably provides couples with sufficient time to seek out the relevant information from government agencies, unions, and their employer. In order to directly test this, I drop the sisters bunching at the old cut-off of the high benefits which also translated into the mode leave duration before the reform, as shown in Figure 2.5. If lack of knowledge about the reform is the relevant mechanism, dropping the observations at the old threshold for paid leave should dramatically reduce the peer effect. However, this yield largely unchanged estimates. This is reported in Appendix 2E. Regarding benefits of staying at home with the child, it is extremely difficult to rule this channel out. However, research that evaluates this reform finds small effects on maternal health compressed among low-resource families and no effect on child health (Beuchert et al., 2016). A German expansion of leave coverage with a strong impact on mothers' leave behavior did not result in improved child outcomes (Dustmann & Schönberg, 2012). In Norway, Dahl et al. (2016) find that extended maternity leave does not affect child education, marital status or subsequent fertility. While these outcomes obviously don't capture all aspects of increased maternity leave, we might be willing to think of them as correlated with other types of private benefits. Evaluating the same reform as me, Tô, 2018 shows that a relative long leave negatively impacts the earnings of the mothers. This is in line with the literature showing that leave duration beyond 6-7 months hurts women's earnings (Ruhm, 1998; Olivetti and Petrongolo, 2017; Kyriacou, Rey, and Silva (2021)). Dahl et al., 2016 find no effect on Norwegian women's labor market earnings.

[23] See Persson and Rossin-Slater (2019) for a framework specifically on the different types of leave around childbirth

Overall, the evidence of non-monetary benefits of extended maternity leave of this length are zero to small and might even hurt women's labor market trajectories.

Consumption Externalities
Some women might enjoy being on maternity leave at the same time as their sister, and thus consumption externalities might arise. However, since births are spaced in time there is limited scope for the sisters to be on leave at the same time. To more directly investigate this, I add an interaction term between a dummy for living in the same municipal as one's sister and the treatment indicator. The results show that mothers in the control group who lived in the same municipal as their sister took a longer leave compared to those who did not live in the same municipal. This effect disappears with the reform. This could potentially be driven by those in the reform control group who used the leave at reduced rate and potentially experienced positive externalities of being on leave at the same time as their sister. This opportunity for consumption externalities is reduced with the reform. Moreover, we might be worried that some sisters coordinate fertility and those are the ones that particularly enjoy being on leave at the same time. To rule out that the peer effects are driven by such sisters, I exclude sister-pairs where the second child was born between October 2002 and January 2003. The sample is reduced, but the point estimate increases slightly. This is the opposite of what should be expected if the peer effects were driven by coordinated fertility. Arguably, the peer effects estimated here are not driven by consumption externalities. These estimates are reported in Appendix 2E.

2.4.4 Robustness

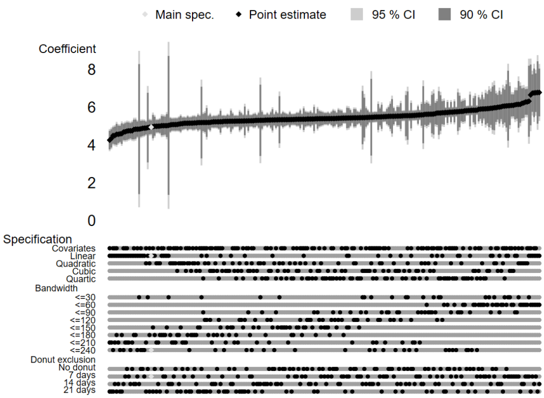
The robustness checks show a research design with very stable results. Estimated reform and peer effects from the preferred specification are very robust to standard checks. Running the model without controls, allowing for a quadratic, cubic, or quartic shape of the running variable, varying the bandwidth and the excluded number of days around cut-off around implementation provide virtually unchanged point estimates. For all specifications, the point estimate of the reform effect is between 4 and 6 weeks of leave. Due to a small sample size, precision decreases when bandwidth is set to 30. This is illustrated in Figure 2.9. The average reform effect that comes out of this exercise is 5.3 weeks.[24] Out of the 249 regressions, all estimates but two are significant on a 0.99 pct. level. The average t-statistics is 20.396.

A similar set of robustness checks are made for the reduced form estimate of the peer effects. Again, the point estimate is fairly stable. However, as the sample size is much smaller for peer effects than for the reform effect, precision decreases, especially when individual level controls are dropped. Having a bandwidth below 120 is not feasible as the number of observations drops too much. After running these 161 regression, the average reduced form effect is 0.87 weeks with an average t statistics of 1.58.

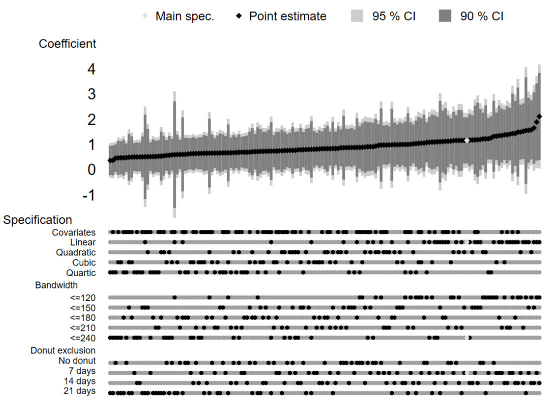
[24]Performing the equivalent exercise for the reform effect of the fathers yield an average reform effect of -0.13 weeks and far from being significant at conventional levels.

Figure 2.9. Alternative Specifications, Mothers' Leave Duration

(a) Reform Effect



(b) Peer Effect



Notes: The figure shows estimates of the reform effect when varying (i) whether or not to include covariates, (ii) the shape of the running variable, (iii) varying bandwidth and (iv) and excluded days around cut-off. The shaded 95 and 90 percent confidence intervals are based on standard errors clustered on date of birth. For each plotted coefficient, the dark dots below indicate the corresponding specification.

2.5 Concluding Remarks

This paper highlights the role of gender identity and different prescriptions faced by mothers and fathers as an important factor for intra-household specialization. By using the discontinuity that arises from the parental leave reform in Denmark in 2002, an RD-design provides robust estimates of 5 weeks increase in parental leave duration among mothers. Meanwhile, the average leave duration of fathers is unchanged. These estimates barely change across relative earnings. Instead, gender identity and prescriptions affect the distribution of leave within the household. In line with the growing literature on the role of inter-generational transmission of gender identity, those who were exposed to high maternal labor supply in childhood take a shorter leave upon reform implementation compared to those exposed to more traditional gender roles in childhood. Second, the reform-induced change in leave duration implies that women with a sister in the reform treatment group face prescription of extensive leave. This shows up here as peer effects and reaffirms gender-specific intra-household specialization.

In general, many family policies may have this effect. Too long maternity leave policies have the potential to have negative effects on women's labor market outcomes (Ruhm, 1998; Olivetti and Petrongolo, 2017; Kyriacou et al., 2021). While the introduction of the Norwegian 'daddy quota' evaluated by Dahl et al. (2014) increased men's involvement in childcare, the reform in Denmark is widening an already existing gender gap. This is arguably similar to the German reform evaluated by Welteke and Wrohlich (2019) in encouraging longer maternity leave and stressing the importance of staying home the year following childbirth. Women in West Germany respond stronger to the reform, consistent with West Germany having more traditional gender norms suppressing female labor supply (Boelmann et al., 2020) and increasing the size of the child penalty (Jessen, 2021). Instead of changing prescriptions regarding fathers' leave, Danish and German policies reinforced views regarding women's responsibility in childcare and home production, and extended leave is transmitted to close peers. The family-friendly policies in the Nordic countries have been characterized as a 'system-based glass-ceiling' (Datta Gupta et al., 2008) because they mainly affect the labor market outcomes of women. The results reported here support this reasoning. If family policies do not explicitly encourage fathers to use them, they will mainly be considered relevant for mothers and strengthen existing gender gaps in intra-household specialization.

This paper also provides new insight into empirical investigations of peer effects. As argued by Sacerdote (2014), studies of peer effects on social outcomes and labor market choices produce significant results more often than those on test scores. However, channels are rarely identified. Akerlof and Kranton (2000) highlights that prescriptions may be more influential than standard economic factors in many important choices. In empirical investigations, changes in prescriptions may show up as peer effects. The results reported here and interpretation of related studies can be explained as changes in prescriptions transmitted to close peers via social interactions.

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CHAPTER 3

Gender Gaps from Job Loss

with Ria Ivandić

Job loss leads to persistent adverse labor market outcomes, but assessments of gender differences in labor market recovery are lacking. We utilize plant closures in Denmark to estimate gender gaps in labor market outcomes and document that women face an increased risk of unemployment in the two years following job displacement. We decompose the gender gap and show that human capital explains half of women's increased risk of unemployment. In addition, childcare imposes an important barrier to women's labor market recovery regardless of individual characteristics. Gender differences in sorting across occupations and sectors prior to displacement play a very minor role.

3.1 Introduction

Throughout the twentieth century, earnings and labor market participation rates of men and women converged alongside economic development in many middle- and high-income countries (Goldin, 1995). A large share of women moved from unpaid production in the home or in family businesses to being wage-earners in the labor market. With the inflow into paid employment, women have also become directly exposed to labor market shocks, such as job loss. This has been particularly visible since the onset of the Covid- 19 pandemic. Women, especially those with care-giving responsibilities, have been disproportionately affected (Alon et al., 2021). While a large literature has established that job loss leads to persistently lower earnings and higher unemployment rates in the long run (e.g. Jacobson et al., 1993; Huttunen et al., 2011; Ichino et al., 2017; Lachowska et al., 2020), gender differences in labor market recovery following job loss remain unexplored.

This paper investigates what are the effects of *women's* and *men's* job loss on future labor market outcomes. The literature provides several potential explanations for why there may exist substantial gender gaps after job loss. One important factor is the constraint that childcare may impose on women's labor market recovery. Much evidence shows that the arrival of children drives a wedge between men's and women's labor market trajectories (Harkness and Waldfogel, 2003; Angelov et al., 2016; Lundborg et al., 2017; Kleven, Landais, and Søgaard, 2019). Women are likely to change jobs around the arrival of their first child (Nielsen et al., 2004; Hotz et al., 2017). This likely leads to gender differences in willingness to commute and search-behavior (Bütikofer et al., 2020; Borghorst et al., 2021; Le Barbanchon et al., 2021) which may affect labor market outcomes following job loss. Another important source of overall gender gaps is differences in human capital, broadly defined to include education, occupation, and other types of sorting in the labor market (Goldin, 2014; Goldin and Katz, 2016; Petersen and Morgan, 1995; Card et al., 2015; Gallen et al., 2019). Such differences might affect disparities in labor market recovery. In this paper, we will try to disentangle the roles these two channels play for recovery following job loss.

Our empirical analysis is based on employer-employee matched register data from Denmark. Denmark provides a good candidate country to study the gender gap in labor market outcomes following job displacement. First, the flexibility of the Danish labor market and generous education policies provide workers with ample opportunities to adjust to shocks. Second, the gender gap in labor force participation is small compared to international standards. Third, the Danish register data covers the full population and is linkable across domains. In addition to relevant worker and firm-level information, we have rich background information on each individual, such as their education and family characteristics.

To identify the effect of job loss on labor market outcomes, we use variation in job displacement from plant closures. As this is initiated by a firm-level shock, it makes the job loss and the timing plausibly exogenous to the individual. We defined the control group as workers matched on socio-demographic characteristics employed in a plant that is not closing. Our identifying assumption of the displacement effect is that the labor market outcomes of the individuals in the displacement and control groups would have evolved similarly over time in the absence of the displacement. We verify this parallel trends assumption by examining the leads to the event. The displaced men and women in our sample are very similar across family characteristics, but there are differences in sectorial occupation, education, and pre-displacement earnings. More importantly, their characteristics are representative of the universe of Danish workers in the private sector. We compute the gender gaps following displacement as the differences in labor market trajectories of men and women following the plant closure, which can be understood as the unconditional gender gap in displacement. To estimate the conditional gap, we account for gender differences in confounding factors by comparing men and women who are displaced from similar jobs and are similar on other observable characteristics. While the unconditional gap is the policy relevant estimate, the conditional gap is important for understanding the source of persistent gender gaps.

We find substantial gender gaps in the risk of unemployment for the first two years following job loss. For both men and women, job loss leads to a reduction in earnings and an increase in unemployment for at least 6 years. Women on average experience a 14.2 % point increase in the probability of unemployment over the first two years, while for men this is lower at a 9.8 % point. Relatively, this amounts to a 45 percent greater unemployment shock for women. While the absolute drop in earnings is larger for men, they have higher initial earnings and overall lose a smaller relative share of their income. Heterogeneity analysis shows that workers with little formal training face the most adverse labor market trajectories after job loss. As women are particularly over-represented in this group, they face a worse labor market trajectory. Meanwhile, there is no gender gap among workers with vocational training or higher education. While women are worse off across all age groups, older women face the greatest absolute risk of unemployment and the biggest drop in earnings. However, the gender gaps are greater among younger workers. When we compare similar men and women, the conditional gaps are reduced, but never fully closed. To disentangle why women are consistently worse off, we turn to the relative importance of human capital and the role childcare plays and perform a Kitagawa (1955)-Oaxaca (1973)-Blinder (1973) decomposition (hereafter KOB). Observable differences explain approximately half of the gender gap in unemployment. Pre-displacement earnings and education are particularly important characteristics. In addition, both the absolute and the unexplained part of the gender gap grow in the presence of children, suggesting that childcare imposes a barrier to women's recovery regardless of individual characteristics. Finally, we show that initial sorting across occupations and sectors barely affects the gender gap in unemployment following displacement.

The main contribution of this paper is to address a shortcoming in the existing literature on adverse outcomes following job loss: the almost complete absence of women. In this literature, it is common to purely focus on male workers (e.g. Oreopoulos et al., 2008; Sullivan and von Wachter, 2009; Huttunen et al., 2011; Davis and Wachter, 2011;

Browning and Heinesen, 2012; Seim, 2019; Halla et al., 2020).[1] Even among the studies that include women in their sample, they seldomly address gender differences (e.g. Eliason and Storrie, 2006; Rege et al., 2009; Lachowska et al., 2020; Jung and Kuhn, 2018). This tradition implies that conditions and constraints that are particularly important for women have not been identified and investigated. The paper closest to ours is the work by Illing et al. (2021) who use German data to compare similar men and women and find that women's earnings losses are about 35% greater than men's upon displacement. This is partly driven by women being more likely to take up part-time work and mini-jobs, but also by lower earnings in full-time jobs.[2] We contribute with an explicit analysis of gender gaps in labor market outcomes following displacement and explore the circumstances under which gender gaps are mitigated or exacerbated. We show that men are better off than women as a result of higher levels of human capital and by not being constrained by child care.

This paper also contributes to the literature on gender gaps and parenthood. It is well-established that women's labor market trajectories drop dramatically at the onset of parenthood (Harkness and Waldfogel, 2003; Ejrnæs and Kunze, 2013; Daniel et al., 2013; Angelov et al., 2016; Lundborg et al., 2017; Kleven, Landais, and Søgaard, 2019; Berniell et al., 2021; Delecourt and Fitzpatrick, 2021). This is partly attributed to reduced labor supply and employment in more flexible settings (Nielsen et al., 2004; Kleven, Landais, and Søgaard, 2019; Hotz et al., 2017). When the responsibility of childcare falls disproportionately on women, it likely imposes a barrier to labor market recovery.[3] We document that having children increases the gender gap following job loss, regardless of mothers' characteristics. This provides insights into the mechanisms of the child penalty. Even after going back to work post birth, mothers' ability to adjust to labor market shocks is constrained by childcare responsibilities.

Finally, our paper contributes to the literature on changes to skill demand and sectorial structures in developed economies. There is a large amount of evidence on the increase in labor market polarization from trade-pressure (Autor et al., 2015; Hummels et al., 2014; Utar, 2018; Gu et al., 2020) and technical changes (Autor and Dorn, 2013; Autor et al., 2006; Goos et al., 2014). The decline in employment in goods-producing industries has happened alongside a rise in the employment in service and has reduced gender gaps in labor market opportunities and outcomes (Petrongolo and Ronchi, 2020; Ngai and Petrongolo, 2017). However, there is little evidence of how this transition affects gender gaps among workers in declining sectors.[4] While men often are the mode worker, women have worked in goods production since the onset of the industrial revolution (Wikander

[1] See Appendix 3A for a comprehensive overview of the sex composition in this literature among papers that include estimates of labor market outcomes.

[2] Other examples of an explicit focus on women include the work by Bono et al., 2012 showing that women's job loss leads to reduced fertility. Several papers have investigated women's responses to their husband's job loss (Halla et al., 2020; Hardoy and Schøne, 2014; Skoufias and Parker, 2006).

[3] Mörk et al. (2020) and Ruiz-Valenzuela (2021) provide overviews of the literature on job loss and inter-generational spillovers. This literature stands out in the job loss literature more broadly by often including a comparison between maternal and paternal job loss.

[4] Exceptions to this include Aksoy et al. (2021), Ge and Zhou (2020) and Keller and Utar (2018).

et al., 1995). In our sample, women constitute 30 % of the exposed workers. We focus on closing plants in manufacturing and document that, within goods production, women are worse off.

The remainder of the paper is organized as follows. Section 3.2 describes the institutional background, data, and the definition of plant closures. Section 3.3 presents the research design. Section 3.4 contains the results along with robustness checks, and Section 3.5 discusses the mechanisms behind it. Section 3.6 concludes the paper.

3.2 Background and Data

In this section, we outline the main features of the Danish labor market and present a summary of Denmark's progress on gender equality. We describe the data and present the definition of plant closures and the displaced workers.

3.2.1 The Danish Labor Market

Danish firms can adjust employment with relative ease as a result of lax employment protection legislation. Wages are high, but indirect wage costs are among the lowest in the world (Eriksson & Westergaard-Nielsen, 2009). Employers in Denmark have among the highest freedom among OECD countries to dismiss permanent workers. This labor market model has led to job turnover rates that are similar to the UK and US rather than the rest of continental Europe (Hobijn and Sahin, 2009; Botero et al., 2004). Most employment spells are short (Andersen, 2021), and occupational mobility is high (Groes et al., 2015). Relatively generous unemployment insurance ensures that workers bear low costs of changing jobs. The majority of workers are members of voluntary unemployment insurance funds. During the period of this analysis, benefits were 90 % of former earnings with a cap on the higher bound of the benefits. Insured workers receive this benefit for up to four years. Uninsured workers may receive social assistance and means-tested housing allowance. Recipients are required to search for jobs and face both monitoring and sanctions. The combination of a flexible labor market, fairly generous unemployment insurance, and active labor market policies are often referred to as the 'flexicurity model' (Andersen, 2021). This labor market model has mitigated shocks from globalization and technological changes (A. Humlum and Munch, 2019; Andersen, 2021). However, workers with little formal education or occupation-specific human capital remain disproportionately affected by recent shocks (Hummels et al., 2014; Utar, 2018; Gu et al., 2020; M. K. Humlum et al., 2019).

3.2.2 Gender Equality in Denmark

Denmark has - alongside other Nordic countries - long been praised for social policies that enable high female labor force participation. Compared to international standards, there is a relatively small gender gap in labor force participation, and more than 80 % of Danish mothers with children below the age of 10 work outside the home, and 2/3 work full time (Leira, 2010). Women's paid work increased dramatically from the 1960s onwards alongside expansions of the public sector that institutionalized work that previously took place in the family (Datta Gupta et al., 2008). The gender gap in participation decreased until the early '90s and has remained fairly stable since. Couples in Denmark face individual taxation which creates a strong incentive for secondary earners, often women, to participate in the labor market (Selin, 2014). Women's labor market participation is further enabled by parental leave schemes and daycare with close to universal coverage (Leira, 2010). The majority of the remaining gender gap is driven by the child penalty (Kleven, Landais, & Søgaard, 2019). Upon parenthood, men's labor market trajectory is unaffected while women reduce hours and opt for jobs with more flexibility (Kleven, Landais, and Søgaard, 2019; Nielsen et al., 2004).

3.2.3 Data Sources and Descriptive Statistics

The starting point of our analysis is the Danish employer-employee matched register data covering the universe of Danish workplaces and all the corresponding workers. This register contains key labor market information such as wages, tenure, labor market status, and occupation. Information on unemployment insurance and social assistance allows us to construct a reliable measure for labor market participation (i.e. either working or actively searching). Mandatory pension payments are used to infer hours worked. We link this data with background information on sex, education, age, place of residence, marital status, and children.

We consider the period from 1995 to 2006 for two reasons. First, while the employer-employee matched data goes back to 1981, Danish women's labor market participation did not plateau until the early 1990s. Second, we purposely end our analysis before the financial crisis. The shocks induced by the crisis affected many dimensions of the Danish economy (Jensen and Johannesen, 2017; Renkin and Züllig, 2021; Bonin, 2020). More importantly, men's labor force participation decreased more during the crisis than women's labor force participation. In sum, we consider a period where labor force participation of Danish men and women moved in tandem.

For each private-sector workplace with at least 5 workers, we classify a workplace as closing if the number of workers in the workplace reduces by 90 pct. or more between year $t-3$ and t. Hence our definition of an event is stricter than that of a mass layoff, by describing a full plant closure and follow Bingley and Westergaard-Nielsen (2003) and Browning and Heinesen (2012).[5] With this definition of a plant closure as a shock to displacement, we plausibly estimate a shock that is more orthogonal to displaced workers' characteristics than a mass layoff, where a large, yet selected, share of workers within a

[5] Bingley and Westergaard-Nielsen (2003) investigate the role of firm-specific human capital in labor market trajectory following job loss. Browning and Heinesen (2012) document increased risk of mortality and hospitalization among displaced men. Both papers use Danish data.

plant lose their job. Most of the plants close down within a year.[6] 95 % of the plants close fully and retain zero workers. The remaining 5 % retain on average 2.4 workers (median: 1). This number likely signifies either administrative workers finalizing the closure or simply a registration issue, likely occurring in firms with multiple plants.[7] On average, the workers are displaced from plants with 185 workers (median: 53). Displaced workers are categorized as treated the year they separate from the closing plant. For 59 % of workers, the year of full closure is identical to the year of displacement, and 94 % leave the plant within 2 years of full closure. In the robustness section, we modify our definitions by only including plants closing over 1 year and by increasing the cut-off for the size of plants we consider. Results remain largely unchanged.

We follow the most recent literature (de Chaisemartin & D'Haultfœuille, 2020) and define the control group as only including workers that never experience a plant closure. Our identification strategy relies on choosing an appropriate control group of workers. Both displaced and control workers are identified from a sample of workers with at least 1 year tenure at a manufacturing plant with at least 5 workers. We exclude students, self-employed/managers, and those on (part-time) early retirement, but we do not impose restrictions on future labor market outcomes.[8] We apply coarsened exact matching to match one-to-one without replacement to find the most suitable control group. We perform the matching separately for men and women and match on pre-displacement earnings, marital status, age, educational groups, tenure at the firm, unemployment history, and labor market experience. In Appendix 3B, we report balancing tests of both these and other variables not used in the matching and find they, on average, balance. Our final sample consists of 1,492,791 observations, corresponding to 133,768 unique individuals, of which half of them are treated and 30 % are women. In Appendix 3C„ we report the evolution in unemployment rates for control and treated workers. Prior to displacement, the two groups have extremely similar labor market trajectories.

In Appendix 3D, we report covariates separately for men and women. For the sample of women, all covariates balance except plant size. Exposed women work in plants that are bigger than women in the control group. The exposed men work in both plants and firms that are bigger than those of the control group. The year prior to displacement, exposed men earn 2240 DKK (∼ €300 per year) more compared to the control group. While this difference is statistically significant at a 1 % significance level, this is hardly an economically meaningful amount. Displaced men are 1 % point less likely to have children and 0.8 % point less likely to work in 'Iron & Metal'. Comparing the men and the women, the most striking differences are on educational level and earnings. The women are much more likely to have little formal training, i.e. high school or less (53 % vs. 35 %). The year prior to displacement, the women earn 100,000 DKK (∼ €13,500)

[6]To ensure that we do not misclassify a workplace as closing due to a merger, administrative changes in legal structure, etc., we follow the displaced workers and calculate the share of workers that remain co-workers the following year. If this share is above 50 pct., we do not consider the plant to be closing.

[7]49 % of these plants are in firms with multiple plants.

[8]We only allow for workers to be treated once. While it is fairly rare for workers to be treated more than once, when we exclude these workers this leads to about 7.5 % reduction in the $person \times year$ number of observations.

less than the men. This corresponds to a gender gap of 26 %. The partners of the women earn a larger share of the household income than the partners of the men (49 % vs 33 %), implying that household income is higher for the men compared to the women. The largest sector for both sexes is 'Iron & Metal', followed by 'Food, Drinks & Tobacco'. For family characteristics, men and women are similar.

We focus exclusively on plant closures in the manufacturing sector. 70 % of all exposed workers in the sample period are in plants that are in the manufacturing sector. Every other sector has a share of exposed workers almost tenfold less, such as 'Retail & Service' (9 % of workers), 'Finance & Insurance' (6 % of workers) and 'Construction' (5 % of workers). Men are over-represented in construction, while women are over-represented in the service sector. Including this type of heterogeneity would make our gender gaps difficult to interpret. In Table Appendix 3D, we report a comparison between workers in the manufacturing sector and all other workers in the private sector. The workers in our sample are remarkably similar to all Danish private sector workers.

3.3 Empirical strategy

This paper assesses gender differences in labor market recovery following job displacement. However, with the aim of estimating the effect of job loss on future labor market outcomes, concerns related to endogeneity arise. The likelihood of a worker being displaced is likely to be correlated to individual unobservable characteristics. To overcome these issues of endogeneity, we exploit plant closures in the manufacturing sector make the timing of the job loss plausibly exogenous to the individual as it is initiated by a firm-level shock.

Our research design uses an event study specification that allows us to estimate the dynamic effects of job loss on displaced workers using the following baseline model separately for men and women:

$$Y_{i,j,t} = \alpha + \sum_{k=-6,t\neq-1}^{6} \beta_k PlantClosure_{i,j,t+k} + \sum_{k=-6,t\neq-1}^{6} \lambda_k Time_{i,j,t+k} + \theta_t + \theta_t \times \delta_j + u_{i,j,t} \tag{3.1}$$

where $Y_{i,j,t}$ is the dependent variable, $PlantClosure_{i,j,t+k}$ is a dummy variable equal to one in the year $t+k$ since the job displacement for individual i employed in plant p in the year of displacement, $Time_{i,j,t+k}$ identifies $t+k$ years since the event to capture cohort effects, θ_t captures year fixed effects, and $\theta_t \times \delta_j$ estimates municipality specific year fixed effects.[9] The dependent variables include: unemployment (whether the individual i is unemployed for at least 12 weeks in year t), labor earnings (the total labor income of

[9]Identifying the effect of plant closure on the exposed workers relies on the assumption that the plant closure does not affect the control group. If plant closures are large enough to affect the local labor market the control group will also be affected. Figure Appendix 3E shows the dispersion of exposed workers across Denmark. Workers live in all municipalities except for small islands. Within commuting

individual i in year t), log(labor earnings), and labor market participation (whether the individual i is employed or actively searching in year t). Our matching procedure implies that we compare displaced workers with similar non-displaced workers, so the inclusion of individual fixed effects or worker level covariates makes little difference.

This estimation strategy is a generalization of the Difference-in-Differences method and relies on the assumption that earnings and unemployment rates would have evolved similarly in the treated and control group in the absence of the plant closure, i.e. the assumption of parallel trends. If displacement is initiated by a firm level shock, the timing of the lay-off is arguably exogenous to the individual worker. In Appendix 3C, we show the evolution in unemployment rates for the displaced and control workers. The difference between these outcomes is what we estimate in Equation 3.1. The main independent variables are $\sum_{k=-6,k\neq-1}^{6} PlantClosure_{i,k}$ which are dummy variables equal to one for displaced workers in the k^{th} relative year. Our parameters of interest are β_k for $k = -6, -5, ..., 0, 1, ..., 6$, capturing the dynamic effects in 6 years before and after the plant closure of the workers exposed to the plant closure compared to similar workers. We interpret the significance of these coefficients as evidence of the causal relationship between job displacement and future labor market outcomes. Additionally, absence of meaningful effects in the pre-period can rule out anticipation effects.

The gender gap in labor market recovery is obtained by comparing displaced men to men in the control group and displaced women to women in the control group. This is the policy-relevant estimate. To estimate the conditional gender gap, we then compare similar men and women. To this end, we construct a new sample of men by matching all treated and control women to the men on the same set of variable as used in the initial matching procedure.[10] This provides us with a new 'matched' sample containing men similar to the women in our sample. We re-estimate Equation 3.1. This provides a gender gap where differences in observable characteristics are taken into account.

3.4 Gender Gaps Following Job Displacement

To measure the effect of women's and men's job loss on future labor market outcomes, we start by presenting results estimating Equation 3.1 for labor market outcomes for men and women respectively for up to 6 years following displacement. We investigate how sensitive our results are to the precise definition of the displaced group. We also show that our results are robust to recent advances regarding Difference-in-Differences applications with differential timing in treatment.

zones, the closures appear to be fairly spread out in the country. In the preferred specification, we include an interaction term between year and municipalities to capture local labor market effects. This makes little difference relative to the inclusion of municipality and year fixed effects separately.

[10]We choose to use women as the baseline because the sample of women is smaller (30 % of the sample). Had we used men as the baseline, we would not find a match for all men.

We then turn to the role of workers' characteristics to explore the circumstances under which gender gaps might be mitigated or exacerbated. Motivated by the existing literature, we investigate heterogeneity by age and educational attainment. Finally, we explore the role of childcare responsibility plays in labor market recovery.

3.4.1 Main Results

Figure 3.1 reports yearly labor market outcomes following displacement for men and women. Displaced men and women face an increased risk of entering long-term unemployment and experience substantial drops in earnings for up to 6 years. In the year of displacement and the following year, there is a substantial gender gap in the risk of entering unemployment (for 3 months or more). Women face an increased risk of 14.2 % point, while men experience an increase in risk by around 9.8 % point. Following the initial two years, the gender gap is greatly reduced and finally disappears. Women also experience a larger initial percentage drop in earnings. The second year following displacement, the gap has disappeared. Men lose a larger absolute amount of income. In the year of displacement and the following year, men lose 61,680 DKK (≈ €8,800) while women lose 57,210 DKK (≈ €7,700). This gap remains statistically significant throughout the period. Looking at non-participation rates (defined as the residual of time spent in employment and time spent being registered as unemployed), we don't find a gender gap following displacement. Both men and women face a 7.8 % point increase in the likelihood of being registered as non-participating.[11]

Subsequently using the matched sample of men with characteristics that are similar to the sample of women, we estimate the conditional increase in the risk of unemployment following job loss for men and women and compare the conditional and unconditional gender gap. Among the men matched on observables to women, the risk of unemployment stands at 11.6 % point and they lose DKK 61,680 (≈€8,300). This leads to a decrease in the magnitude of the gender gap, from the unconditional 45 % gender gap to the conditional 25 % gender gap.

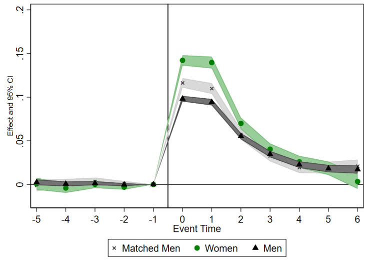
Across outcomes, the β_k for k < -1, i.e. before the displacement, allow us to investigate pre-trends and anticipation effects. We can test individual and joint significance for the years leading up to the displacement. For unemployment and earnings, none of the pre-periods are significantly different from zero, implying that our treated and control workers had similar earnings and unemployment rates. In general, we interpret this as the absence of dynamic selectivity into closing plants supporting the validity of our research design.[12] Our results are similar in magnitude to what Bingley and Westergaard-Nielsen (2003) and Bertheau et al. (2021) report for Denmark.

[11] Men are on average unemployed for 3.4 weeks while women are unemployed for 4.8 weeks. When men and women find a job, they are equally likely to find full time work.

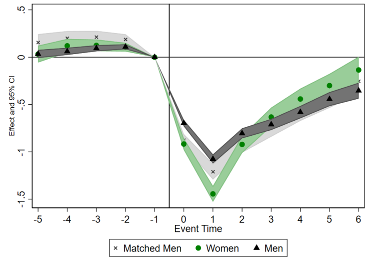
[12] For log(earnings), we observe an economically very small difference in pre-displacement earnings, suggesting that treated workers - both men and women - were on more favorable labor market trajectories than our control workers. If anything our estimated effect should then be biased towards zero as we are comparing treated workers with higher potential outcomes than their non-treated counterparts.

Figure 3.1. Labor Market Adjustment Following Displacement

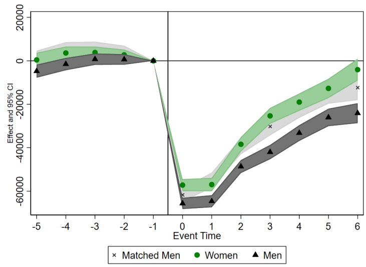
(a) Unemployment, By Gender



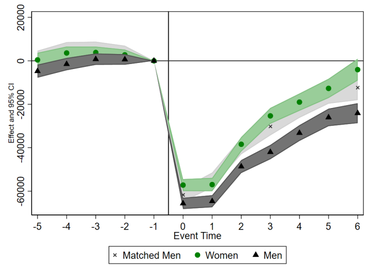
(b) Log(Earnings), By Gender



(c) Earnings, By Gender



(d) Non-participation, By Gender



Notes: Job displacement between -1 and 0. Black triangles denote displaced men, while green circles denote displaced women, relative to a control group of workers of their own gender who are not displaced. Grey crosses denote the men when they are compared to similar women. (a) shows the spike in risk of unemployment, defined as 3 months or more. Panel (b) and (c) show the drop in log(earnings) and earnings. Panel (d) shows the increase in non-participation, defined as neither working nor being registered as unemployed. Each panel shows the difference between the displaced workers and a matched control group with corresponding confidence intervals, obtained from estimating Equation 3.1.

Robustness: Intuitively, workers in smaller plants have more influence over the performance of the plant than workers in bigger plants. Approx. 12 % of the displaced workers were employed in plants with 5-10 workers, while more than 60 % of the workers are displaced from plants with 50+ workers. While the majority of plants in Denmark are fairly small, the workers in the sample are weighted by the plant size. Dropping workers displaced from plants with less than 10 workers hardly changes the point estimates. Only including plants with 50 or more workers reduces the sample by 35 % and estimates become less precise. The point estimates of the gender gaps in both unemployment and earnings shrink. This is driven by the men in the plants facing larger risk of unemployment, while the estimated risk for the women remains unchanged. This is reported in Appendix 3E.

We consider the event the year when the worker is no longer employed in the closing plant. For 59 % of the sample, the year of separation is the year of plant closure. In our definition of plant closures, we allow a three-year period for closure, yet 80 % of the workers are displaced from plants reducing the number of workers by 90 % in one year. To make sure our results are not driven by potential gender differences in timing of lay-off with respect to the closure, we limit our sample to only consider plants that close within 1 year. Importantly, the estimates for the gender gap in both unemployment and earnings remain unaffected. Among workers in plants who reduced the number of workers by 90 % in 1 year (i.e. closed within a year), the corresponding risk of unemployment is 14.8 % point and 10 % point. This is reported in Appendix 3E.[13]

Recent developments in the methodological literature have pointed out that in settings like this - with differential timing of treatment - the baseline specification might be biased towards zero. We consider plant closures over a 10 year period, and in Appendix 3D we show that the occurrence of plant closures is relatively evenly distributed across the years in our sample. We implement the estimator proposed by Sun and Abraham (2021). The obtained estimates and our baseline estimates are virtually identical. This is a result of the control group mirroring the cohort shares of the treatment group across years as well as the dynamic specification controlling for cohort fixed effects.

3.4.2 Heterogeneous Effects

In Figure 3.2, we report the risk of unemployment by age and educational attainment. Men older than 50 face a high risk of unemployment compared to younger men. For all three age brackets, gender gaps exist. This implies that women older than 50 face the highest risk of unemployment.

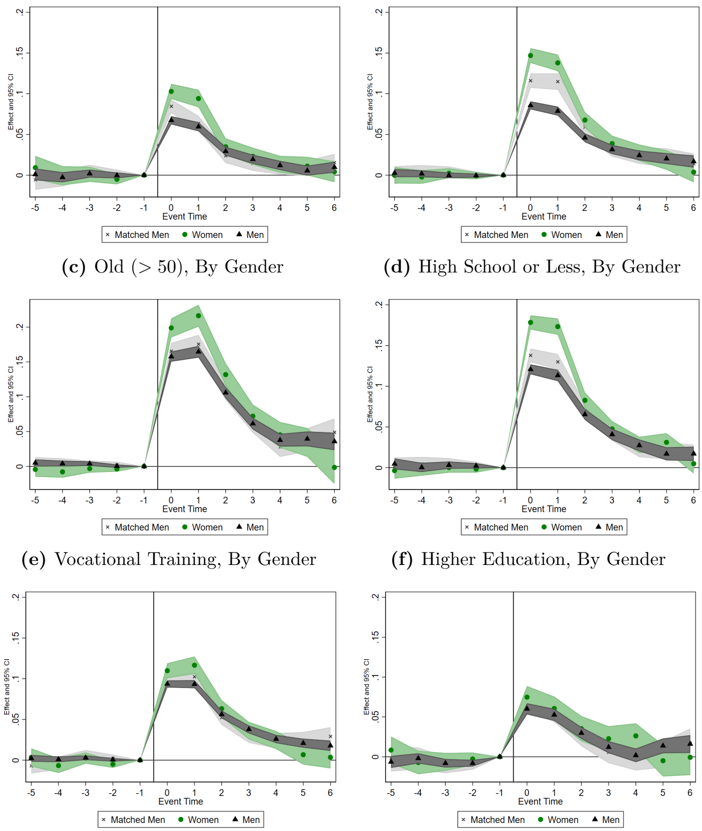
When we compare similar men and women using the matched sample, gender gaps among all three groups are reduced. Workers with a high school diploma or less education face the largest risk of unemployment and a large gender gap exists. These men face an increased risk of unemployment of 12.1 % points and the women facing a striking 17.8 % points risk of unemployment, relative to the control group. When comparing similar men and women, the gender gap remains largely unaffected.[14] As discussed before, women are over-represented in this group. Workers with vocational training face an increased unemployment risk of 10 % points. Those with at least some college face a risk of unemployment of 8 % points. There is no meaningful gender gap in these two groups.

These results mirror the existing literature on job displacement and labor market shocks more broadly, while our contribution highlights the gender differences across these. Less educated workers face adverse labor market outcomes while more educated workers are more likely to adapt (Gu et al., 2020; Utar, 2018; Hummels et al., 2014).

[13] When we consider the heterogeneity by timing of separation, 'early leavers' (i.e. those that leave an unstable plant 2 or 3 years prior to full closure) fare slightly worse in the labor market, regardless of gender.

[14] The results are similar for log(labor earnings), with the oldest and the least educated workers being worse off. This is reported in Appendix 3H.

Figure 3.2. Heterogeneity of Unemployment Rates, by age and education



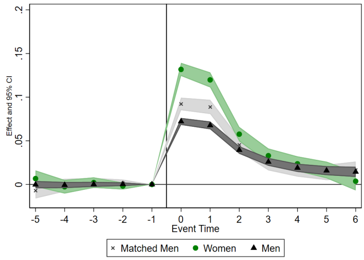
(a) Young (<= 35), By Gender
(b) Middle (> 35 & <= 50), By Gender
(c) Old (> 50), By Gender
(d) High School or Less, By Gender
(e) Vocational Training, By Gender
(f) Higher Education, By Gender

Notes: See Figure 3.1. Panel (a), (b) and (c) report the evolution in unemployment rates for workers in different age brackets. Panel (d) shows the unemployment rates for those with high school or less education, panel (e) reports the unemployment rates for workers with vocational training, and panel (f) reports unemployment for those with some higher education.

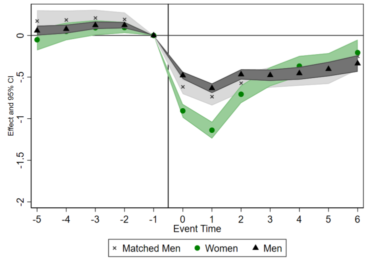
Specifically in the job closure literature, Ichino et al. (2017) document that older workers in Austria have lower re-employment probability after displacement and that women are worse off. Using Norwegian data, Salvanes et al. (2021) show that the probability of employment decreases with age. Related, Kunze and Troske (2012) document gender gaps in search-duration among displaced German workers between 20-35 and link this to fertility and childcare.

Figure 3.3. Children

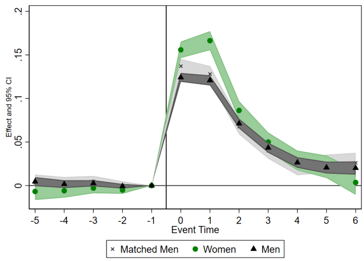
(a) Unemployment, Child in the HH



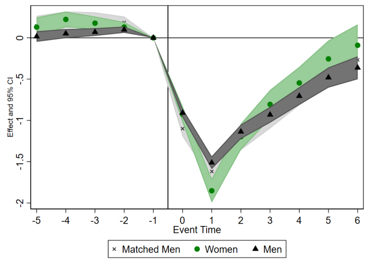
(b) Log(earnings), Child in the HH



(c) Unemployment, No Child in the HH



(d) Log(earnings), No Child in the HH



Notes: See Figure 3.1. Panels (a) and (b) reports the evolution in unemployment rate and log(earnings), respectively, for workers with children in the household and panels (c) and (d) plots the equivalent estimate for those without children.

To directly explore the role of childcare, we estimate Equation 3.1 separately for households with and without children and report this in Figure 3.3. In the presence of children, there are large gender gaps both for entering unemployment and in relative earnings losses. Fathers face an increased risk of entering unemployment of 7.3 % points, while this number for mothers is 12.1 % points. This gap is also mirrored in log(earnings). Comparing similar men to similar women in the matched sample, the gender gap with children present in both earnings and risk of unemployment is almost unaffected and remains as high as the unconditional gap. In households without children, the gender gaps are much smaller, but it is worthwhile to notice that these workers are worse off compared to those with children. Among men without children in the household, there is a 12 % point increase in the risk of entering unemployment while this number is 15 % points for women and a very small gap in log(earnings).

3.5 Explaining the Gender Gap

To further understand the mechanisms behind the gender gap in unemployment after job displacement following the results discussed in the previous sections, we focus on the potential role of three mechanisms. To understand which individual characteristics drive the difference between conditional and unconditional gaps, we perform a KOB-decomposition. Second, we test whether gender differences in pre-displacement sectors, occupations, firms, plants, or years explain the gender gap in unemployment that follows job loss. Finally, we perform a KOB-decomposition separately for parents and non-parents to understand the role gender differences in childcare responsibilities play in disparities in labor market recovery.

Human Capital: To understand the characteristics that explain the gender gaps in unemployment and log(earnings), we perform a KOB-decomposition of the year following job loss and report this in Table 3.1. Using tenure, pre-displacement earnings, education, age, and age squared, these observables explain 2.9 % points out of the 6.2 % points unconditional gender gap. These variables explain about 47 % of the gender gap in unemployment after job loss, but the majority remains unexplained. For log(earnings), 3.6 log-points out of 4.6 are explained by these characteristics. Higher pre-displacement earnings account for 40 % of the explained gap in unemployment and 75 % of the gap in log(earnings). Education accounts for 27 % of the gender gap in unemployment and the 6.4 % of the gap in log(earnings). Tenure at the closing firm and accumulated labor market experience matter less.

Pre-displacement Sorting: We investigate the role initial sorting across sectors, firms, and occupations play in gender gaps in unemployment. To account for this, we compare men and women displaced from the same occupations and sectors by adding pre-displacement fixed effects to the baseline regression. First, we add fixed effects at the sectorial level (with 7 different manufacturing sectors). We then add fixed effects at the occupation level (using 6-digit ISCO codes).[15] This is reported in Appendix 3I. These specifications have little implication for the gender gap. Finally, we report the distributions of fixed effects of year, and pre-displacement sector, occupation, firm, and plant, for displaced men and women, respectively. This is reported in Appendix 3J. Distributions of the obtained fixed effects across sexes are very similar. Combined, these exercises lead us to conclude that the gender gap in unemployment cannot be a result of initial differences in sorting, or because men and women are displaced in different years.

The Role of Childcare: We conduct the KOB-decomposition separately by parental status. This is reported in Table 3.1. In the presence of children, the gender gap in the risk of unemployment is 7.5 % points and observables account for 3.0 % points, about 40 %. However, in households without children, the gender gap is 5.6 % points and observables account for 1/2 of this gap. For log(earnings), gender gaps across the two

[15]As employer-specific fixed effects are conditioned on unemployment it is not meaningful to add fixed effects from the new job.

groups are 5.2 log-points. In the absence of children, this gap is fully accounted for by pre-displacement characteristics. With children, 27 % remains unexplained. Again, pre-displacement earnings is the most important variable, followed by education. Combined with the large gender gaps reported in Figure 3.3, this leads us to conclude that childcare imposes an important differential barrier for women's labor market and that cannot be explained by their pre-displacement characteristics.

Table 3.1. Kitagawa (1955)-Oaxaca (1973)-Blinder (1973)-decomposition

	Unemployment			Log(earnings)		
	All	W. children	W.o. children	All	W. children	W.o. children
Men	0.134***	0.0985***	0.168***	0.872***	0.918***	0.828***
	(0.00163)	(0.00205)	(0.00250)	(0.00155)	(0.00186)	(0.00245)
Women	0.196***	0.174***	0.223***	0.825***	0.866***	0.776***
	(0.00301)	(0.00387)	(0.00470)	(0.00281)	(0.00345)	(0.00459)
Difference	-0.0621***	-0.0752***	-0.0557***	0.0464***	0.0527***	0.0514***
	(0.00342)	(0.00438)	(0.00532)	(0.00321)	(0.00392)	(0.00520)
Explained	-0.0287***	-0.0297***	-0.0274***	0.0359***	0.0391***	0.0515***
	(0.00139)	(0.00213)	(0.00190)	(0.00177)	(0.00263)	(0.00276)
Unexplained	-0.0334***	-0.0455***	-0.0283***	0.0105***	0.0136***	-7.97e-05
	(0.00372)	(0.00496)	(0.00560)	(0.00351)	(0.00472)	(0.00546)
Explained						
Earnings	-0.0119***	-0.0122***	-0.00995***	0.0275***	0.0269***	0.0301***
	(0.00118)	(0.00183)	(0.00151)	(0.00153)	(0.00253)	(0.00205)
Tenure	-0.000187	0.000423**	-0.00167***	0.000122	-0.000207**	0.00133***
	(0.000174)	(0.000173)	(0.000385)	(0.000113)	(9.60e-05)	(0.000316)
Labor Market Experience	-0.0122***	-0.0170***	-0.00662***	0.0112***	0.0172***	0.00724***
	(0.000790)	(0.00128)	(0.000873)	(0.000735)	(0.00127)	(0.000864)
Higher Education (==1)	-0.00228***	-0.00290***	-0.00113**	0.000678***	0.00104***	0.000351**
	(0.000310)	(0.000407)	(0.000479)	(0.000140)	(0.000253)	(0.000168)
Vocational Training (==1)	-0.00550***	-0.00488***	-0.00556***	0.00354***	0.00271***	0.00530***
	(0.000526)	(0.000601)	(0.000934)	(0.000430)	(0.000513)	(0.000813)
Age	0.00316***	-0.000688	-0.0127***	0.0139***	0.00822***	-0.0194***
	(0.000881)	(0.00218)	(0.00361)	(0.00196)	(0.00187)	(0.00530)
Age squared	0.000201	0.00755***	0.0102***	-0.0211***	-0.0167***	0.0265***
	(0.000818)	(0.00241)	(0.00281)	(0.00254)	(0.00218)	(0.00668)
Unexplained						
Earnings	-0.00417	-0.00196	0.00195	-0.0942**	-0.294	-0.00174
	(0.00905)	(0.0122)	(0.0140)	(0.0431)	(0.412)	(0.115)
Tenure	0.000895	-0.00153	-0.00308	-0.00574	0.0304	-0.000159
	(0.00828)	(0.0104)	(0.0134)	(0.0129)	(0.0617)	(0.0106)
Labor Market Experience	-0.0484***	-0.0314***	-0.0617***	0.0297	-0.0118	0.00134
	(0.00930)	(0.0121)	(0.0149)	(0.0182)	(0.0567)	(0.0888)
Higher Education (==1)	0.00292*	0.00326	0.00241	-0.00617*	-0.0161	-0.000119
	(0.00171)	(0.00229)	(0.00256)	(0.00341)	(0.0236)	(0.00789)
Vocational Training (==1)	0.00593**	0.000502	0.00744*	-0.00722	0.00190	-0.000264
	(0.00281)	(0.00382)	(0.00409)	(0.00501)	(0.0152)	(0.0175)
Age	0.0265	-0.377**	0.384**	-0.130	0.705	-0.0111
	(0.103)	(0.153)	(0.167)	(0.159)	(1.069)	(0.737)
Age squared	0.0373	0.238***	-0.140	0.0105	-0.565	0.00460
	(0.0511)	(0.0773)	(0.0881)	(0.0735)	(0.775)	(0.305)
Observations	61,131	30,826	30,305	61,131	30,826	30,305

Standard errors clustered at the individual level in parentheses
This decomposition exercise is based on displaced workers the year following displacement
*** p<0.01, ** p<0.05, * p<0.1

3.6 Concluding Remarks

While women's and men's labor market outcomes have converged, substantial gender gaps remain. In this paper, we use administrative data from Denmark to show that displaced women following job loss are worse off than displaced men. While both men and women face adverse labor market outcomes for up to 6 years relative to non-displaced workers with similar characteristics, gender gaps exist in the first two years following job loss. Our analysis shows that men are shielded from larger adverse labor market outcomes by their higher levels of human capital, and by not being constrained by childcare. Women are particularly over-represented among workers with little formal education *and* they are worse off than their male counterparts. Moreover, we show that mothers are constrained by childcare, regardless of individual level characteristics.

When comparing displaced workers to non-displaced workers of their own gender, our results on earnings mirror those Illing et al., 2021 report for German workers. However, when comparing similar men and women, German women experience a larger drop in both absolute and relative earnings. The main results from Germany are estimated on a sample of married workers, where marital status is observed using residency and surnames. We do not impose any restrictions on marital status. Denmark and Germany also differ along dimensions that may contribute to these differences. While Danish couples face individual taxation, German couples are taxed jointly. Childcare is heavily institutionalized and child penalties are smaller in Denmark (Kleven, Landais, Posch, et al., 2019). While parenthood exacerbates gender gaps in both countries, the magnitude appears to be bigger in Germany.

Two implications follow. First, while the literature on the long-term negative effects following job displacement is large, systematic investigation of gender gaps is lacking. This striking gap in the literature implies that policy recommendations are perhaps not based on the most relevant estimates. For example, the existing evidence did not cover the most exposed workers during the Covid-19 pandemic (Alon et al., 2021) and conditions and constraints that are particularly important for women had not been identified. As the effects of job loss in Denmark are relatively muted as compared to other countries in Europe (Bertheau et al., 2021), we would expect these gender differences to be larger in other countries. The comparison between our results and those obtained by Illing et al., 2021 confirms this. Second, we show that childcare responsibility imposes an important barrier to women's labor market recovery. We document this in Denmark where publicly provided daycare has close to universal coverage. In other settings, this channel might be even more important. To reduce gender inequality following job loss, policymakers could alleviate childcare constraints.

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CHAPTER 4

Intergenerational Transmission of Labor Market Segregation

Many western economies have seen a fall in the employment share of the traditionally male-dominated, manufacturing sector, while demand is increasing in female-dominated jobs. Still, men appear reluctant to enter these occupations. To understand persistent labor market segregation, I exploit within-school-across-cohort variation in the gender composition of the occupations of schoolmates' parents, and document that gender segregation is transmitted from one generation to the next. Boys who were exposed to gender-stereotypical male role models enter male-dominated occupations, while those socialized in cohorts with peers whose fathers worked alongside women enter occupations with more women. This effect goes beyond the influence of their father. In general, mothers' labor market behavior has negligible effects on boys. In contrast, girls are mainly influenced by female role models, and compared to boys the effects are much smaller. However, when a larger share of mothers works full-time, gender segregation decreases in the next generation.

4.1 Introduction

Despite gender convergences in labor force participation and educational attainment, men and women still tend to work in different occupations. Gender segregation contributes substantially to the gender wage gap as women are over-represented in low-paid professions and under-presented in high-paying professions. Proposed explanations include gender differences in demand for amenities, human capital, discrimination, and gender norms (Cortes & Pan, 2017). However, male-dominated, manufacturing jobs are disappearing in many western economies, and female-dominated sectors are growing (Petrongolo & Ronchi, 2020). While women have entered and altered many previously male-dominated occupations (Goldin, 2014; Goldin, 2015; Pan, 2015), men appear more reluctant to enter feminized occupations. This highlights the importance of an improved understanding of persistent gender segregation in the labor market. In particular, the notion of masculinity and it's related gender norms may impose a potential source of adjustment frictions.

The goal of this paper is to assess whether the occupational choices of role models affect the gender composition of children's occupations as they become adults. Same-sex role models shape what is considered gender-appropriate behavior, and in turn, shape outcomes in adulthood. I explore the role of labor market behavior of relevant role models that children are exposed to during adolescence. Specifically, I look at the occupational choices of parents and parents of schoolmates. The underlying idea is that if a child observes role models working in a less gender-segregated occupation, they are more likely to view this as gender-appropriate compared to a child who observes role models in highly segregated occupations. By using the tools from both the literature on inter-generational mobility and the peer effects literature, I document the effect of labor market behavior of role models on the gender composition of occupations in adulthood.

Labor market outcomes, including occupational choice, are not only influenced by economic opportunities. A large literature is engaged with how the childhood environment affects labor market outcomes. Mothers' labor market participation influences their daughters' and daughter-in-laws' labor market participation (e.g. Goldin and Olivetti, 2013; Fernandez et al., 2004; Morrill and Morrill, 2013; Fogli and Veldkamp, 2011; Farré and Vella, 2013), and other women in proximity have a similar effect (Olivetti et al., 2018; Fernández, 2013; Fogli and Veldkamp, 2011). Father-son associations have received attention in the literature on inter-generational mobility. Both the family and the broader social environment as well as the interaction between the two are important determinants of outcomes (Cholli and Durlauf, 2022; Chetty et al., 2014). Beyond affecting resources, parents and other adults in proximity influence aspirations. If mothers and other women in proximity influence girls' outcomes via transmission of norms, the same may be true for fathers and sons. Importantly, inter-generational aspirations transferred to boys and girls are then likely to be very different.

To investigate how the gender composition of occupations transmits across generations, I use detailed Danish register data covering the cohort born between 1966 and 1974. I construct measures of gender-stereotypical behavior of parents, schoolmates' parents, and wider municipal measures of norms. These measures are intended to capture transmitted norms of the 'appropriate role' of men and women in society and within families. I measure exposure at age 15 and outcomes at age 45. First, I estimate the parent-child associations of the share of women in their occupations separately for boys and girls, and mothers and fathers. Second, I estimate the causal effect from schoolmates' parents' labor market behavior by exploiting quasi-random variation across cohorts within school. The source of variation used to identify effects from exposure to different types of role models is the difference between the child's cohort and the average composition of the school. This accounts for sorting into the school or school district. The main contextual effect of interest is the role of the gender composition of the occupations of schoolmates' fathers, but I also explore the role of the gender composition of the schoolmates' mothers' occupations as well as maternal labor supply.

Combined, my results show that labor market segregation in one generation transmits to the next, while role models acting in counter-stereotypical ways decrease gender segregation in the next generation. In particular, my results highlight the importance of same-sex role models. Within the family, the father-son association of the share of women in their occupations is extremely stable, while mothers have negligible effects on boys. The mother-daughter association is also stable, and 2/3 of the size of the father-son association. Moving on to the causal estimates, I show that boys who are socialized in cohorts where fathers work in occupations with a relatively high share of women also work in occupations with more women themselves. At baseline, peers' mothers matter less for the gender composition of occupations in the next generation. However, if a relatively high share of peers' mothers works full time, gender segregation decreases. This is driven both by boys entering occupations with more women and by girls entering occupations with fewer women. In this setting, mothers working full time is still uncommon. Thus, mothers behaving in counter-stereotypical ways reduce the labor market segregation of the next generation.

First and foremost, this paper contributes to the literature on gender norms and labor market outcomes, specifically gender segregation. The intergenerational transmission of female labor force participation is well-established. However, little attention has been paid to other aspects of gender stereotypical labor market behavior and to masculinity and related gender norms that may influence men's behavior (Lundberg, 2022; Nelson, 2016). Baranov, de Haas, and Grosjean (2021) show that historical rates of skewed sex ratios in Australia increase current-day excess male mortality, violence, and occupational segregation, and decrease tolerance towards sexual and gender minorities. Yet, decreasing segregation in the Norwegian military increases men's acceptance of feminine traits in themselves and gender equality (Dahl et al., 2020). Gender norms have been highlighted as important constraints that distort time allocation, both men's time spent on home production (Bertrand et al., 2015; Siminski and Yetsenga, 2022; Ichino et al., 2019) and men's support for women's labor force participation (Bursztyn et al., 2020). Moreover, men tend to avoid feminized occupations (Pan, 2015; Goldin, 2015). The paper contributes to the emerging literature on how gender norms influence men's labor market outcomes. I provide evidence of the transmission of a certain occupation-trait that has been linked to notions of masculinity, namely the absence of women.

Second, this paper adds to the literature on peer effects, specifically the literature that documents the persistence of peer influence on outcomes in the long run. For identification, it is common to exploit idiosyncratic variations across cohorts within the same schools. This approach was pioneered by Hoxby (2000) to study the effect of gender and racial composition in a classroom and has been widely used (e.g. Rivkin et al., 2005; Friesen and Krauth, 2007; Friesen and Krauth, 2010; Lavy and Schlosser, 2011; Anelli and Peri, 2017; Bifulco et al., 2011; Feld and Zölitz, 2017; Fruehwirth and Gagete-Miranda, 2019; Cools et al., 2021). This approach identifies a contextual effect (Manski, 1993). Two studies evaluate how same-sex role models and gender stereotypes transmit to the next generation. Olivetti et al. (2018) show that girls in cohorts with more working mothers are more likely to work once they have children themselves. Eble and Hu (2022) document the transmission of beliefs about gender differences in math ability. Exposure to peers' parents who believe that boys are better than girls increases the likelihood that the children hold this belief and harms girls' aspirations and test scores while improving boys' outcomes. Using this approach to exploit quasi-random variation within-schools-across-cohorts, and by constructing measures of gender stereotypical labor market behavior, I document causal transmission of gender segregation from one generation to the next.

Finally, this paper also adds to the literature on inter-generational transmission of occupation choice. Sociologists are the main contributors to this literature, while economists have focused more on the transmission of income (see e.g. reviews by Blanden (2011) and Cholli and Durlauf (2022)). Two papers have documented inter-generational correlations in the gender composition of occupations/education. My results largely resemble the evidence provided by Hederos (2017) who uses Swedish census data for an older cohort. Humlum et al. (2019) use Danish data and a similar period as me and focus on the gender composition of university programs. They focus on women' sluggish entry to male-dominated education programs and find that fathers hardly influence their daughters' occupational choices. I shift the focus to the boys and extend the analysis to all levels of educational attainment, and ask how role models influence boys' entry to female-dominated occupations.

The structure of the paper is as follows. In Section 4.2, I present the data, the measures of gender-stereotypical labor market behavior obtained from observational data, and some descriptive statistics. Section 4.3 contains descriptive evidence of the parent-child associations. Section 4.4 contains a presentation of the empirical approach and identifying assumptions as well as validity checks. Results are reported in Section 4.5. Section 4.6 concludes.

4.2 Background

This section contains a brief description of the Danish labor market and the schooling system, the data used in the analysis, and some descriptive statistics.

4.2.1 Setting

While the gender gap in labor market participation is small (approx. 5 %-point), the Danish labor market is still fairly segregated. This is a function of boys and girls choosing different types of education, and sectorial segregation where men tend to work in the private sector and women in the public sector, leading to horizontal segregation (Holt et al., 2006). Moreover, there is substantial vertical segregation with under-representation of women in the upper echelons of even female-dominated fields (Holt et al., 2006). The overall level of segregation in the Danish labor market is almost identical to the average segregation within the EU, and in most countries, segregation has decreased over the last decades (EU-Commission, 2009). To decrease segregation, policymakers have mainly proposed events and programs to increase girls' interest in male-dominated fields, such as STEM and IT, rather than encouraging boys to enter female-dominated fields, such as health care and social work (EU-Commission, 2009).

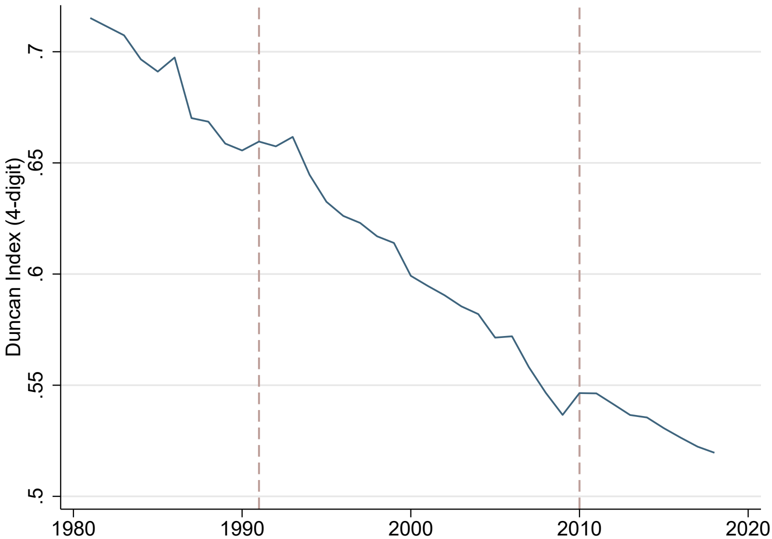
In the '80s — where I measure childhood exposure — the Danish labor market was highly segregated. Measured using a Duncan Segregation Index[1], Figure 4.1 shows that gender segregation in the '80s and early '90s was between 0.72 and 0.65, which corresponds to the proportion of individuals who would need to change occupation for the labor market to be completely integrated. I measure the outcomes once the focal individual turns 45 — in the 2010s – where the Duncan Index has declined by almost 1/3 to 0.52-0.55.

[1]The Duncan-Index identifies the percentage of women (or men) that would have to change occupations for the occupational distribution of the two genders to be equal:

$$DI_i = \frac{1}{2}\sum_{i=1}^{N} |m_i/M_i - f_i/F_i|$$

where m_i (f_i) is the male (female) population, in occupation i and M_i (F_i) are the total male (female) working population of the local labor market. It takes values between 0 (complete integration) and 1 (complete segregation).

Figure 4.1. Segregation of the Danish Labor Market



Notes: The figure shows the yearly Duncan Index for the Danish labor market. Dashed lines indicate two data breaks. The period from 1981-1991 based on NYSTGR at the 4-digit level, and the two latter periods are based on the Danish ISCO at the 4-digit level.

Table 4.1 contains a list of the most common occupations for men and women, respectively, across the two generations. Starting with the common occupations held by women, it is striking how similar the two lists are. The most common occupation for both generations is office worker, and various types of health and child care feature prominently on both lists. For both men and women in both generations, teacher is a large occupation. For men, transportation staff and various types of construction are very common for both generations. In the 2010s, retail work is the most common occupation for men, despite this being a female-dominated occupation. Self-employed farmer was the second most common occupation in the 80's for men, but this occupation is no longer in the top 10 of most common occupations. Similarly, assisting spouse was a common occupation for women in the '80s.

Another important feature of this study is the schooling system in Denmark. For the period of this analysis, nine years of schooling was mandatory. Importantly, there is no distinction between primary and secondary school in Denmark. Unless a family actively decides to change the child's school, the child stays in the same school from the age of six to the age of 15.[2] This implies that the cohort composition is fairly stable throughout nine years of schooling. School districts are linked to residential areas. Similar to many other settings, Danish families sort into neighborhoods along important socio-economic dimensions, such as income and education, influencing outcomes for the next generation (Heckman and Landersø, 2021; Damm, 2014).

Table 4.1. Most Common Occupations

Parental generation, 1981-1989

Men			Women		
Occupation	N	Share	Occupation	N	Share
Transportation staff	61007	0.94	Office worker	195985	0.85
Self-employed, farming	56230	0.95	Domestic help	77019	0.96
Marketing/finance	50081	0.99	Office worker in hospital	66996	0.88
Architect/engineer, manager	36989	0.96	Cleaning staff	50752	0.92
Teacher	35908	0.45	Nurse/x-ray assistant/therapist	49051	0.96
Office worker	34913	0.15	Retail worker	44657	0.65
Bricklayer/carpenter, trained	34818	0.98	Teacher	43889	0.55
Mechanic, trained	32257	0.99	Social worker, untrained	39429	0.91
Construction worker, untrained	29532	0.98	Assisting spouse	30345	0.98
Electrician, trained	29298	0.99	Social worker/pedagogue	25520	0.84

Child generation, 2010-2018

Men			Women		
Occupation	N	Share	Occupation	N	Share
Retail worker, sales	34630	0.41	Office worker	71475	0.77
Office worker, middle management	28304	0.71	Health care, private homes	67778	0.90
Warehouse and replenishment	27696	0.78	Child care, assistant	55276	0.76
Transport and warehouse	27026	0.80	Nurse	54601	0.95
Carpenter	26744	0.99	Waiters/bartenders	53507	0.86
Teacher	25717	0.33	Pedagogue	51744	0.82
Education, higher edu.	22369	0.33	Retail worker, excl. management	51158	0.59
Construction work	22235	0.92	Teacher	51110	0.67
Truck driver	21368	0.99	Health care, hospitals etc.	46326	0.85
Office worker	21349	0.23	Cleaning staff	45139	0.71

Notes: The table lists the biggest occupation for men and women, respectively, and the number of men and women working in these occupations together with the share of workers of the same sex, based on the full working population. For the parental generation, this is based on NYSTGR, and both the count and share refer to a mean over the period from 1981-1992. For the child generation, this is based on the Danish ISCO codes and covers the period from 2010-2018.

[2]There are exceptions to this. For example, in the rural parts of the country or on smaller islands it was not unusual to have a small school covering the first years of schooling. When the children reach a certain grade, they would start to commute to a larger school. Moreover, as of 1975, 1-year boarding schools ('Efterskoler') were allowed to offer final exams. I drop these schools from my analysis.

4.2.2 Data

The starting point is the population register containing all inhabitants in Denmark with family identifiers. I focus on the cohorts born between 1966 and 1974 and their parents. Educational registers with school identifiers allow me to identify the children that complete the mandatory schooling together. Having defined the population of interest, I link it to the registers that contain information on labor market outcomes for all workers in Denmark since 1981. In combination with a widely used measure of gender norms, namely maternal labor supply, I measure gender segregation of the parental generation to construct measures of gender norms that the focal individuals are exposed to in their adolescence (i.e. at age 15). I obtain these measures at the family level, at the school, and at the municipal level. At the municipal level, I use a Duncan Index, and for the parents and schoolmates' parents, I measure the share of women in the occupation cell.

I restrict the sample to exclude those with migrant background, defined as either born outside Denmark or with at least one parent born outside Denmark. Most of this is mechanical, as I need the children to reside with their parents in Denmark at age 15. Moreover, I do not want my results to be affected by discrimination in the labor market and other issues related to integration, so the remaining children with migrant background are also dropped.[3] I also exclude those where at least one parent is self-employed.[4] For self-employed, measures of labor supply (and potentially wages) are of poor quality, and if one parent is self-employed whether or not the other parent is assisting is unclear.

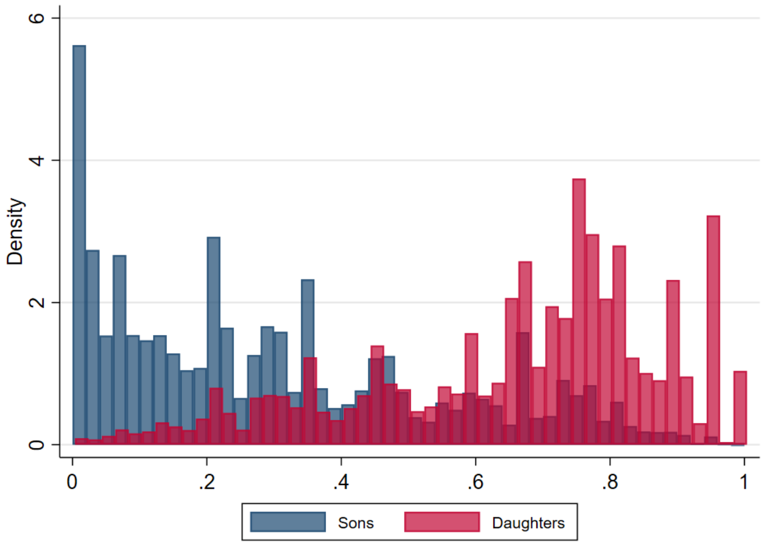
4.2.2.1 Outcome of Interest

The outcome of interest is obtained when the focal person is 45 years old. This age is chosen to ensure important labor market and family decisions have been made and to avoid any data breaks for measures of labor market outcomes. I measure the gender composition of the occupation of the focal person as the share of women in the occupation (similar to Humlum et al. (2019) for education and Hederos (2017) for occupation). I use the Danish ISCO-codes to identify the occupation and calculate the share of women at the four-digit level. In Figure 4.2, I show the distribution of the female share in the occupation that individuals work in at age 45 for both men and women. Confirming the picture from Figure 4.1, the Danish labor market is fairly segregated even for the most recent year. The median share of women in the son's occupation is 23.9 %, and for daughters this number is 72.2 %.

[3]In 2010, 9.5 % of the population have migrant background. In 2018, this number is 14 %.

[4]A literature has shown that entrepreneurial parents have entrepreneurial children (e.g. Lindquist et al., 2015; Dunn and Holtz-Eakin, 2000; Nicolaou and Shane, 2010), with stronger associations across same-sex parent-child pairs. In the early '80s, approx. 25 % of the children are living in households where at least one parent is self-employed. This number gradually decreases to 21 %.

Figure 4.2. Distribution of Female Share in Occupation at age 45, by Gender



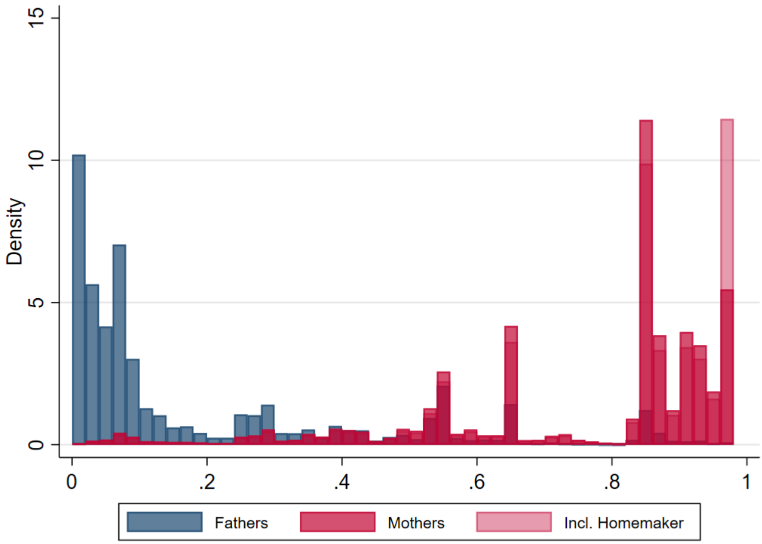
Notes: The figure shows the distribution of the share of women in the occupation of the focal individuals. Occupations are inferred from the Danish ISCO-codes and the gender share is calculated at the 4-digit level. Self-employed individuals and assisting spouses are excluded.

4.2.2.2 Gender Composition of Occupation

The gender composition of the parents' occupation is defined as the share of women in the occupation. The occupation cells are constructed by Statistics Denmark from a combination of union membership, education, and sectorial occupation. The occupational classification has a hierarchical structure, allowing for analyses at different levels of detail. I conduct the analyses at the most detailed level, using four-digit codes. At higher levels of aggregation, predominantly male or female occupations may be combined and appear as integrated. This approach follows Hederos (2017).

I modify the parents' occupational classifications in two ways. First, I ensure that the set of occupations is consistent over time. Some very small occupations cease to exist in the late '80s. Vice versa, new occupations arise in the latter part of the period. For these occupations, all with less than 80 workers each year, I collapse them with the one of their neighboring occupations, which has the most similar gender composition. Second, I classify homemakers (women with no labor supply and thus no occupation) as a separate occupation taking the same value as the most female-dominated occupation

Figure 4.3. Gender Composition of Parental Occupations



Notes: The figure shows the distribution of women in the fathers' and mothers' occupations, respectively. This is measured when the focal person is 15 years old. Occupation is inferred from the variable NYSTGR which Statistics Denmark construct from a combination of education, sectorial employment, and union membership, using the most detailed level (4-digits). Self-employed parents and assisting spouses are excluded. The fraction of women in the mothers' occupation is shown in red and pink (excluding and including homemakers, respectively), and the faction of women in the fathers' occupation is shown in blue..

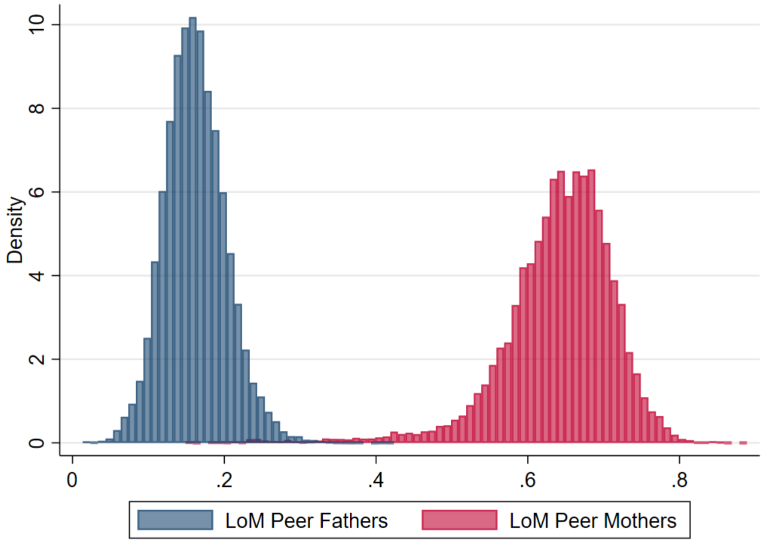
in a given year.[5]

The distribution of the measure of parents is depicted in Figure 4.3 and shows a highly segregated labor market. More men than women work in occupations without or with very few people of the opposite gender present. 10 % of fathers work in occupations with virtually no women, and 6 % of mothers work in occupations almost without any men. However, when including homemakers as occupation, this number increase to 12 % for women. The median father works in an occupation with 7.0 % women. Thus, it is fairly common to have a father in a heavily male-dominated occupation. The average share of women in the fathers' occupation is 19.3 %. The mothers also work in highly segregated occupations but are more likely to have at least some men present in their occupations. The median mother works in an occupation with 84.5 % women. While the median is unchanged when adding homemakers, the average share of women in mothers' occupations increases from 75.6 % to 77.3 %.

[5]Throughout the period, the share of mothers with a labor supply of zero decreases from 20 % to 12 %. The share of mothers that work full time increases from 22 % to 38 %.

Moving on to the school environment, I calculate the leave-out-mean of the gender composition of fathers and mothers of the cohort members at the school from which they finish 9th grade.[6] That is, for each student, this measure captures the gender composition of the occupation of the fathers and mothers, computed from the school-cohort distribution after eliminating the student from the distribution. The distribution of the leave-out-means is depicted in Figure 4.4. Fathers work in more segregated occupations than mothers. The average leave-out-mean of the share of women in the occupation of the schoolmates' fathers is 16.2 % and the corresponding number for mothers is 64.0 %, with very similar medians. Including homemakers returns a distribution that is shifted to the right and with a larger variance.

Figure 4.4. Gender Composition of Peers' Parents Occupations



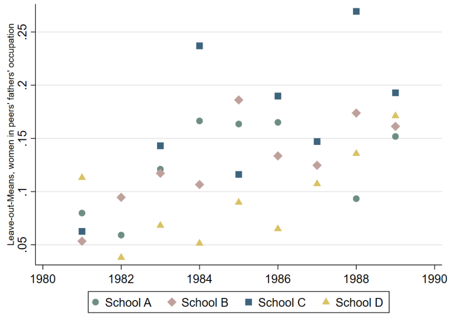
Notes: The figure shows the distribution of the Leave-out-Means of women in the occupation of the schoolmates' parents. This is measured when the focal person is 15 years old. Occupation is inferred from the variable NYSTGR which Statistics Denmark construct from a combination of education, sectorial employment, and union membership, using the most detailed level (4-digits). The fraction of women in the mothers' occupation is shown in red and the faction of women in the fathers' occupation is shown in blue.

There are two sources of variation in the gender composition of the parents' occupation, one, differences between schools, and two, differences within-school across-cohort,

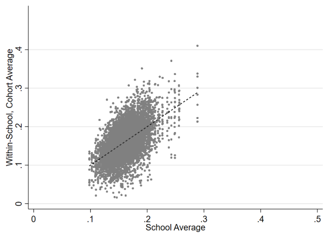
[6]I drop the 2 % most segregated schools (cut-off at 33 % and 75 % girls), boarding schools, cohorts with less than 22 students, and those with more than 90 students, corresponding to the 5th and 95th percentile. I create a balanced set of schools, dropping approx. 6.9 % of individuals who are attending schools that do not exist throughout the period.

Figure 4.5. Gender composition of the Occupations of Schoolmates' Parents: School average and within-school-year average

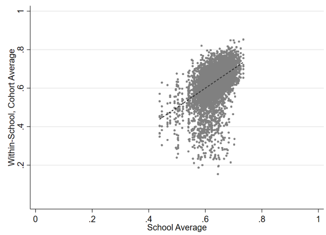
(a) Peers' fathers' Occupation within School, over time



(b) Father's Occupation



(c) Mother's Occupation



Notes: The upper figure shows the share of women in the schoolmates' fathers occupation four schools over time. The lower scatter plots show the school averages of the gender composition of the occupations of schoolmates' parents is plotted on the x-axis against the within-school-year average on the y-axis.

with the latter being the level of comparison used for identification. Figure 4.5 shows how the measure of the gender composition of the occupation of schoolmates' parents varies within a school. In panel (a), I plot 4 schools and show the yearly measure of gender segregation of the fathers' occupations. For all these schools, there is a positive trend, in line with a general decline in segregation in the labor market reported in Figure 4.1, but substantial variation around the trend. This is the variation that I use for identification. In panels (b) and (c), I plot the school average on the x-axis against the within-school-year average on the y-axis and overlay a 45-degree line. Each point's distance from the line shows the within-school-year variance in the gender composition of the occupation of fathers (panel b) and mothers (panel c). The within-school across-cohort corresponds to approx. 75 % of the variation in the gender composition of both mothers' and fathers' occupations. This is reassuring for the likelihood of obtaining useful estimates and for interpretation.[7]

4.2.2.3 Controls

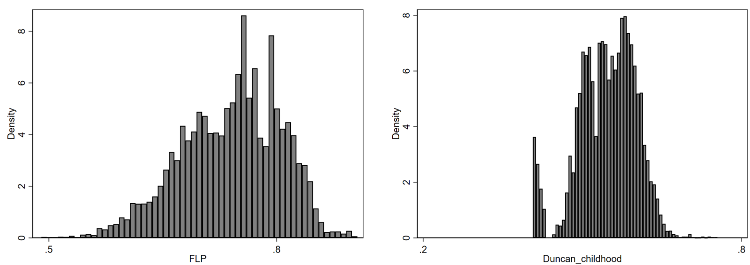
I obtain a wide range of control variables to capture cohort, region, and family characteristics that may jointly affect parental labor market behavior and the behavior of the focal person. As with the measures of gender norms, all family and parental controls are measured at age of 15 of the focal person and reported in Table 4.2.

A continuous measure of maternal labor supply is constructed from mandatory pension scheme contributions, ATP. From this, I can infer the fraction of full-time work. Throughout the period of analysis, non-participation among mothers fall from 20 to 12 %. The average labor supply increases from 56 % of full time to 69 % of full time and the share of mothers working full-time increases from 22 to 38 %. Moreover, I obtain measures of sibling parity, the number of siblings, sibling sex composition, both parents' age, parent's marital status, educational attainment of both parents, fathers' labor supply, and household income.

At the municipal level, I construct two measures. The first measure is a measure of female labor force participation, defined as the share of women between 18-65 who are working (excl. students). The second measure is a Duncan-Index. The Duncan-Index identifies the percentage of women (or men) that would have to change occupations for the occupational distribution of the two genders to be equal. It takes values between 0 (complete integration) and 1 (complete segregation).

Figure 4.6. Female Labor Force Participation and Duncan-Index, Childhood Municipal

(a) FLP, all years **(b)** Duncan-Index, all years



Notes: The left side of the panel shows the distribution of female labor force participation in the municipal at age 15 of the focal person. The right side of the panel figure shows the distribution of the Duncan-Index of the municipal measured at the same time. The municipal measure of female labor supply includes all women of working age, and the Duncan-Index is based on all workers, regardless of age.

[7]If this share had been smaller, i.e. a larger share coming from differences across schools, the interpretation would likely rely on out-of-sample predictions.

Table 4.2. Gender Norms and Controls, Boys and Girls

	Gender Norms				Controls		
	Girls	Boys	Dif		Girls	Boys	Dif
Own occupation	0.629	0.345	-0.284***	Birth year	1969.9	1969.9	-0.029***
	(0.212)	(0.231)	(0.001)		(2.612)	(2.607)	(0.010)
Leave-out-mean,	0.164	0.163	-0.000	Siblings	1.422	1.427	0.005
peers' fathers' occ.	(0.041)	(0.041)	(0.000)		(0.908)	(0.897)	(0.004)
Leave-out-mean,	0.640	0.641	0.001***	Same-sex sibling	0.545	0.544	-0.001
peers' mothers' occ.	(0.075)	(0.074)	(0.000)	=1	(0.498)	(0.497)	(0.002)
Leave-out-mean,	536.852	536.327	-0.525	Child parity	1.737	1.733	-0.004
maternal labor supply	(112.814)	(113.007)	(0.442)		(0.861)	(0.853)	(0.003)
Leave-out-mean,	0.733	0.733	0.000	Household inc.	277.615	278.383	768
working mother =1	(0.112)	(0.112)	(0.000)	(DKK)	(129134)	(128601)	(505)
Own father's occ.	0.194	0.193	0.001	Inc % earned by	0.658	0.660	0.003***
	(0.240)	(0.240)	(0.001)	Father	(0.227)	(0.227)	(0.001)
Own mother's occ.	0.755	0.755	-0.001	Fathers' edu., =1	0.322	0.315	-0.007***
	(0.225)	(0.224)	(0.001)	high school or less	(0.467)	(0.465)	(0.002)
Own mother's	614.584	613.756	-0.829	Father's edu., =1	0.458	0.459	0.000
labor supply	(365.795)	(365.389)	(1.432)	vocational edu.	(0.498)	(0.498)	(0.002)
Own mother	0.836	0.836	0.000	Father's edu., =1	0.205	0.212	0.007***
=1 if working	(0.370)	(0.370)	(0.001)	higher edu.	(0.404)	(0.409)	(0.002)
Duncan Index	0.700	0.700	0.000	Father's	1941.4	1941.3	-0.107***
in Childhood	(0.048)	(0.048)	(0.000)	birth year	(6.007)	(5.985)	(0.023)
Female LFP	0.830	0.830	-0.000	Mother's	1944.1	1944.0	-0.094***
in Childhood	(0.038)	(0.038)	(0.000)	birth year	(5.280)	(5.240)	(0.021)
Observations	132,779	128,128	260,907	Observations	132,779	128,128	260,907

Notes: The sample includes individuals of Danish ancestry born between 1966-1974, excl. self-employed parents and children. Measures are obtained at age 15 except for own occupation, which is measured at the age of 45. Leave-out-means are the measures from the schoolmates' parents. Occupation refers to the share of women in the occupation cell, using NYSTGR for the parents' occupation and 4-digit Danish ISCO for their own occupation. The municipal measure of female labor force participation includes all women between 18-65, excl. students. The Duncan-Index is based on all workers. *** p<0.01, ** p<0.05, * p<0.1

4.2.3 Descriptive Statistics

In Table 4.2, I report the mean and standard deviation of the proposed measures of gender norms and other family covariates, separately for boys and girls. The left side reports the measures of gender norms and shows that background measures are very similar across boys and girls. I also report the gender composition of the occupations these children work in once they reach adulthood. As expected, there is a large difference. At the age of 45, the men work in occupations with 34.5 % women, while women work in occupations with 62.9 % women. Gender segregation has decreased substantially compared to the parental generation, where mothers on average worked in occupations with 75 % women and fathers worked in occupations with on average 19.3 % women.[8] Humlum et al. (2019) report 42.6 % women in sons' university programs, and 70 % women in daughters' programs, returning an almost identical gap. They report 2/3 women in mothers' education and 1/3 women in fathers' education. Comparing my numbers to those reported by Humlum et al. (2019) suggests that in the parental generation, university-educated parents worked in more gender-segregated occupations than the rest of the population, but amongst the children, university-educated individuals are working in less gender segregated occupations.

For other control variables, there is a statistically significant difference across boys and girls. However, these numbers are almost all so small that they are economically meaningless. There is one exception to this - the boys have fathers with slightly more educational attainment, namely more likely to have a father with high education and less likely to have fathers with a high school diploma or less educational attainment.

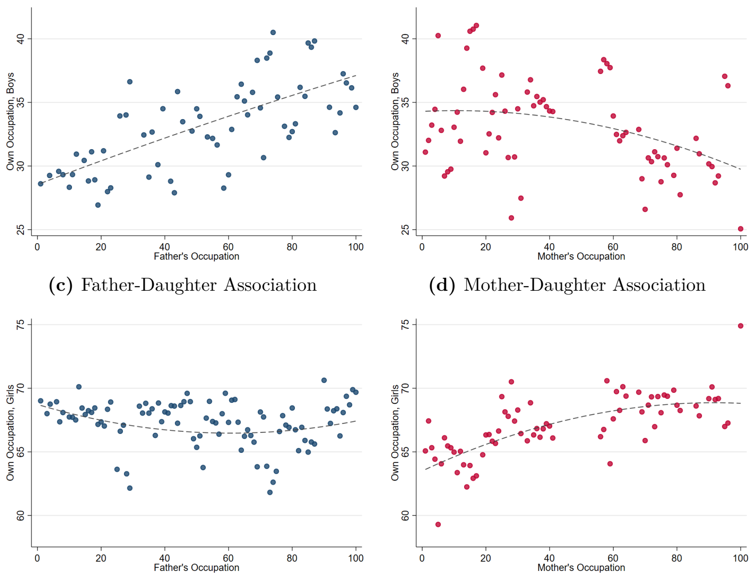
4.3 Inter-generational Correlations

Before moving on to the causal set-up, I document the father-child and mother-child associations in the gender composition of the occupations. I first provide graphical evidence, followed by regressions with rich family controls. These estimates should be interpreted as partial correlation. Keeping this in mind, the returned correlations are extremely stable to progressively adding control. The association is stronger for same-sex parents and generally confirms the idea of intergenerational transmission of labor market segregation. I directly show that they are not driven by those pairs that obtain the exact same education.

Figure 4.7 depicts the inter-generational correlation in the rank of the share of women in the occupation, going from the most to the least male-dominated. The higher rank (i.e. a higher share of women in the occupation) of occupation of the father, the higher rank of the occupation of the son. The association between mothers and sons is weaker and negative, i.e. having a mother who works in a female-dominated occupation is associated with working in an occupation with fewer women. There is a positive association between mothers and daughters, but no association between fathers and daughters.

[8] In Appendix 4A, I report the correlations across the family, school, and municipal measures of gender norms. There is little correlation across family and municipal levels as well as across family and school levels. School and municipal measures correlate substantially. This is not surprising as school districts are embedded within municipals.

Figure 4.7. Share of Women in Occupation, Rank-Rank Associations



(a) Father-Son Association **(b)** Mother-Son Association

(c) Father-Daughter Association **(d)** Mother-Daughter Association

Notes: This figure shows the unconditional parent-child correlation in the share of women in the occupation. For each percentile of bin of share of women in the parents' occupation, the expected rank of the share of women in the child's occupation is plotted.

To obtain the inter-generational correlations, I estimate the following equation separately for boys and girls:

$$GO_{i,t+1,m} = \beta_0 + \beta_1 GO^{Mom}_{i,t} + \beta_2 GO^{Dad}_{i,t} + \beta_3 X_{i,t} + \lambda_t + \lambda_m + \epsilon_{i,t,m} \tag{4.1}$$

The outcome $GO_{i,t+1,m}$ denotes the gender composition of the occupation, specifically the share of women in the occupation, for individual i measured in time $t+1$ (age 45) and grew up in municipal m. $GO^{Mom}_{i,t}$ and $GO^{Dad}_{i,t}$ denote the gender composition of the occupation of the mother and father, respectively, when the child is 15 years old. The coefficients β_1 and β_2 reflect the mother-child and the father-child inter-generational correlations in the gender composition of occupation, respectively. Following the literature, I have standardized these measures (i.e. subtracted the mean and divided by the standard deviation) to obtain coefficients that are easier to interpret as an increase of one standard deviation. $X_{i,t}$ is a vector that contains family co-variates (maternal labor supply, sibling composition, fathers' education, log(household income), parents' birth year, fathers' labor supply).

Table 4.3 reports the results from Model 4.1 with controls added progressively. The first panel contains the result for parent-son associations, and the second panel contains the parent-daughter associations. The first column reports the result for the same sex-parent-child correlation without any controls and thus the estimate corresponding to Figure 4.7.

Parent-child associations are reported in Table 4.3 and are remarkably stable across specifications. The associations confirm both the idea of intergenerational transmission of labor market segregation and the idea and that same-sex parents matter more. Moreover, the father-son association is hardly affected by adding mother-son measures. Adding maternal labor supply hardly influences the associations either. This suggests that fathers' labor market behavior influences boys independently of maternal labor market behavior.[9]

For all specifications, the father-son association in the share of women in the occupation is positive and highly significant, and the coefficient does not vary substantially when I progressively add controls. The point estimate of mother-son association is negative for all specifications. This is consistent with the hypothesis that boys growing up with fathers (mothers) in more feminized occupations are more (less) likely to enter feminized occupations themselves. However, the magnitude of the effect of the father is much larger than the effect of mothers. An increase in one standard deviation of the share of women in the father's occupation is associated with 2.2-1.7 % more women in the son's occupation from a baseline of 34.5 % women, an effect size of 5 %. In comparison, one standard deviation (correspond to 22.4 %) increase in the share of women in the mother's occupation has a tenfold smaller effect.

The mother-daughter association is positive. This is again consistent with the hypothesis that girls growing up with mothers in feminized occupations are more likely to enter feminized occupations. The size of this coefficient is approximately half of the size of the father-son association. An increase in one standard deviation (corresponding to 22.4 %) is associated with 1.2-0.9 % more women in the daughter's occupation, from a baseline of 63 %. Finally, the father-daughter association is also positive. One standard deviation increase in the share of women in the father's occupation increases the share of women in the daughter's occupation with 1/4 of the mother-daughter association.

The size of the parent-child coefficients decreases when I add family control in Column (3). This is driven by paternal educational attainment. Adding maternal labor supply (Columns (4) and (5)) hardly alters the coefficients. Column (6) adds control for sibling characteristics (number of siblings, parity, and a dummy for having a same-sex sibling) and municipal-wide measures of gender segregation and female labor supply.[10]

Overall, the pattern of my results mirrors those reported in the existing literature.

[9] In my preferred specification, I exclude mothers who are homemakers, but adding these families with the modification outlined above hardly influences the coefficients. This is reported in Appendix 4B.

[10] In an alternative set of specifications, I use ranks. This is reported in Appendix 4C. The results are qualitatively similar. One increase in the rank of the father (mother), increases the rank of the son by 0.0663 (-0.0148) while one increase in the rank of the mother (father) increases the rank of the daughter by 0.401 (0.0406).

Table 4.3. Intergenerational Correlation: Parent-Child Associations

Sons	(1)	(2)	(3)	(4)	(5)	(6)
GO^{Dad}	0.0224***	0.0225***	0.0171***	0.0.172***	0.0170***	0.0171***
	(0.00114)	(0.00115)	(0.000790)	(0.000796)	(0.000802)	(0.000810)
GO^{Mom}		-0.00590***	-0.00185**	-0.00191**	-0.00151**	-0.00189**
		(0.000841)	(0.000599)	(0.000611)	(0.000596)	(0.000586)
MLS, [1/2;full-time[					-0.00645**	
					(0.00223)	
MLS, <1/2 time					-0.000587	
					(0.00204)	
MLS, continous				-8.32e-06**		-7.90e-06**
				(2.87e-06)		(2.86e-06)
Constant	0.284***	0.284***	0.447***	0.416***	0.421***	0.420***
	(0.00299)	(0.00310)	(0.0583)	(0.0619)	(0.0621)	(0.0730)
Observations	145,793	125,649	125,649	125,649	125,649	125,229
R-squared	0.023	0.025	0.048	0.048	0.049	0.049
Daughters	(1)	(2)	(3)	(4)	(5)	(6)
GO^{Dad}		0.000319	0.00267***	0.00250***	0.00255***	0.00257***
		(0.000570)	(0.000584)	(0.000592)	(0.000588)	(0.000594)
GO^{Mom}	0.0123***	0.0125***	0.00980***	0.00988***	0.00966***	0.00990***
	(0.000585)	(0.000573)	(0.000682)	(0.000682)	(0.000669)	(0.000704)
MLS, [1/2;full-time[					1.10e-05	
					(0.00105)	
MLS, <1/2 time					-0.00590***	
					(0.00135)	
MLS, continous				1.17e-05***		1.14e-05***
				(2.36e-06)		(2.41e-06)
Constant	0.607***	0.607***	0.930***	0.974***	0.981***	0.943***
	(0.00329)	(0.00322)	(0.0352)	(0.0360)	(0.0371)	(0.0751)
Observations	148,227	124,444	124,444	124,444	124,444	124,057
R-squared	0.013	0.013	0.021	0.021	0.021	0.021
Municipal	Yes	Yes	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes	Yes	Yes
Family			Yes	Yes	Yes	Yes
Siblings						Yes
Municipal norms						Yes

Notes: The sample includes individuals of Danish ancestry born between 1966-1974. Estimates obtained from estimating Equation 4.1, separately for boys and girls. See above for a full list of covariates. Measures of maternal labor supply is obtained from mandatory pension contributions. Using a discrete measure of maternal labor supply, the baseline category is full-time work. Standard errors clustered at birth year, *** p<0.01, ** p<0.05, * p<0.1

Associations in labor market outcomes between father-son and mother-daughter are consistently stronger than father-daughter and mother-son associations, and effects are larger for sons than for daughters (Cholli & Durlauf, 2022). This is also true for the gender composition of education and occupation (Humlum et al., 2019; Hederos, 2017). The size of the coefficients reported here is very similar to what Hederos (2017) reports for a slightly older Swedish cohort. Using gender composition of university programs, Humlum et al. (2019) report larger estimates.

To understand the role of educational attainment, I estimate Equation 4.1 separately for families where the fathers have little formal education, vocational training, and those with at least a two-year college degree. In Table 4.4, I show that the association is stronger in families where the father has a college degree.

Table 4.4. Heterogeneity by Paternal Educational Attainment

	Boys	Girls	Boys	Girls	Boys	Girls
	High School or Less		Vocational		At Least Some College	
	(1)	(2)	(3)	(4)	(5)	(6)
GO^{Dad}	0.0123***	0.00127	0.0162***	0.00109	0.0218***	0.00680***
	(0.00142)	(0.00144)	(0.00127)	(0.000822)	(0.00186)	(0.00148)
GO^{Mom}	-0.000899	0.00842***	-0.000306	0.00827***	-0.00626***	0.0113***
	(0.00119)	(0.00144)	(0.00148)	(0.00113)	(0.00113)	(0.00108)
MLS, continous	-7.25e-06	9.24e-06***	-1.26e-05***	1.36e-05**	-5.73e-06	1.18e-06
	(5.44e-06)	(2.47e-06)	(3.71e-06)	(4.37e-06)	(6.23e-06)	(8.07e-06)
Constant	-0.259**	0.683***	0.0536	1.113**	0.683***	1.129***
	(0.0859)	(0.178)	(0.156)	(0.180)	(0.185)	(0.201)
Observations	35,168	34,647	58,924	58,571	29,236	28,945
R-squared	0.039	0.021	0.028	0.015	0.044	0.042
Municipal	Yes	Yes	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes	Yes	Yes
Family	Yes	Yes	Yes	Yes	Yes	Yes
Siblings	Yes	Yes	Yes	Yes	Yes	Yes
Municipal Norms	Yes	Yes	Yes	Yes	Yes	Yes

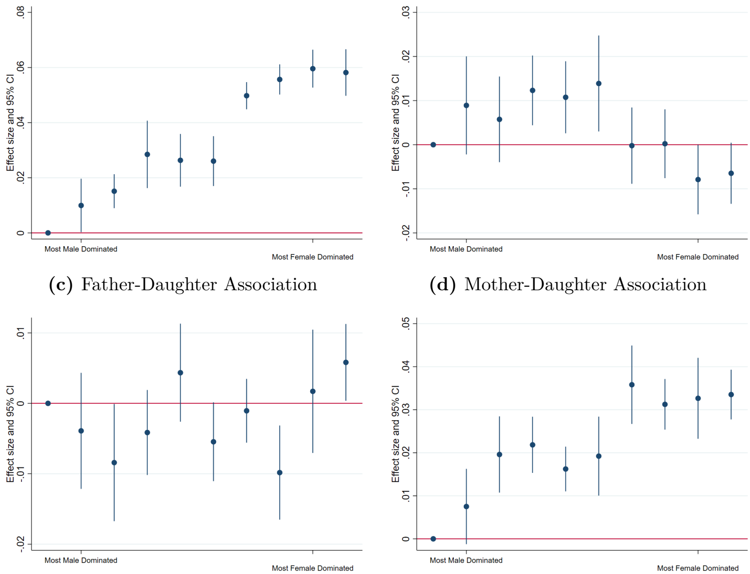
Notes: The sample includes individuals of Danish ancestry born between 1966-1974. Estimates obtained from estimating Equation 4.1, separately for boys and girls, split by paternal educational attainment. See above for a full list of covariates. Standard errors clustered at birth year. Standard errors clustered at birth year, *** p<0.01, ** p<0.05, * p<0.1

For sons with fathers who have at least some college, the association in the fully specified model is 0.0218, while the corresponding estimate for those with vocational training or less schooling is 0.0162 and 0.0123, respectively. For all groups, these effects are highly significant. For boys, the effects of the gender composition of mothers are insignificant across all groups. For girls, the father-daughter association is only significant in families where the father has a college degree, and the size is 1/3 of the father-son coefficient. The mother-daughter association is also strongest in families with highly educated fathers, with a coefficient of 0.0113, compared to a coefficient of 0.00827 and 0.00842 amongst those with fathers with less education.[11]

[11] In Appendix 4E, I report heterogeneity by maternal educational attainment. For girls, the association is strongest in families where mothers have at least some college with an association of 0.0145, compared to 0.00681 and 0.00927 for those with high school or less and vocational education, respectively.

A large literature in sociology documents the transmission of occupations from father to son. If fathers and sons work in the same field, and the gender composition hardly changes, this could drive my results. There is no consistent occupation code covering 30 years, so I use educational codes (indicating the school and the field/major) and drop the parent-child pairs that obtain the exact same education. This is reported in Appendix 4E. While the new point estimates decrease slightly, the overall pattern remains unchanged.[12]

Figure 4.8. Effects Along the Distribution



(a) Father-Son Association **(b)** Mother-Son Association **(c)** Father-Daughter Association **(d)** Mother-Daughter Association

Notes: The sample includes individuals of Danish ancestry born between 1966-1974. See above for the full list of covariates. Estimates were obtained by replacing the continuous measure of gender norms with dummies for each decile. Point estimates are relative to the baseline which is 1st decile - the most male-dominated occupations.

To understand the effects along the distribution of the main explanatory variables, I use dummies for each decile of $GO_{i,t}^{Mom}$ and $GO_{i,t}^{Dad}$ instead of the continuous measure. These coefficients are plotted in Figure 4.8. For fathers, the effects are fairly linear.

[12] The association decreases from 0.0171 to 0.0146 for father-sons and from 0.00990 to 0.00844 for mother-daughters in the fully specified model. The confidence intervals overlap with the baseline model.

The parent-son associations are plotted in panels (a) and (b) and the parent-daughter associations are plotted in (c) and (d). Starting with the sons, for each decile, the effect on the share of women in the sons' occupation increases. All estimates are different from the baseline (which is the most masculine occupations containing fathers that work in occupations with virtually no women). However, it is worthwhile to remember that the median in this sample is just 7 % women in the occupation. Only deciles 9 and 10 can be characterized as resembling gender balance with the cut-off at 36 and 55 % women, respectively. For the mother-son association, the pattern is less clear and estimates are less precise compared to the father-son association. Sons with mothers in occupations with less than 40 % women but more than 85 % enter occupations with more women. This is relative to the mothers in the most male-dominated occupations - those with less than 40 % women. The largest point estimate of the mother-son association is 1/4 of the largest point estimate of the father-son association.

For girls, the pattern is reversed. The father-daughter relationship is imprecisely estimated and no clear pattern emerges, and the mother-daughter relationship is almost linear. For mothers working in heavily feminized occupations, the mother-daughter association is 2/3 of the father-son association in the most feminized occupations.

4.4 Empirical Strategy

This section explains the empirical strategy and presents tests for the validity and relevance of the strategy.

4.4.1 Estimating Equation

The constructed data set contains information on students from multiple cohorts within the same school. This allows me to exploit across-cohort-within-school variation to estimate the effect of schoolmates' parents' labor market behavior. Identification relies on the quasi-randomness of the labor market behavior of schoolmates' parents. While parents may choose the school for their children based on the characteristics of the parents of the schoolmates, it is unlikely that they are aware of how the composition changes across cohort. The variance over time in the share of women in the schoolmates' parents' occupation in Figure 4.5 confirms this.

My empirical model can be written as:

$$\begin{aligned}GO_{i,t+1,s} = \delta_1 GO_{i,t,s}^{PeerDad} + \delta_2 GO_{i,t,s}^{PeerDad} * Girl + \delta_3 GO_{i,t,s}^{PeerMom} + \delta_4 GO_{i,t,s}^{PeerMom} * Girl \\ + \beta_1 GO_{i,t}^{Mom} + \beta_2 GO_{i,t}^{Mom} * Girl + \beta_3 GO_{i,t}^{Dad} + \beta_4 GO_{i,t}^{Dad} * Girl \\ + \lambda_t + \lambda_t * Girl + \lambda_s + X_{i,t} + \varepsilon_{i,t,s} \qquad (4.2)\end{aligned}$$

where i denotes the individual, t denotes the year of graduation, and s denotes the school. The outcome $GO_{i,t+1,s}$ is the share of women in the occupation of individual i who graduated from school s at time t. The main explanatory variables are listed in the first line of the equation: $GO_{i,t,s}^{PeerDad}$ and $GO_{i,t,s}^{PeerMom}$ which denote leave-out-means of the gender composition of the occupation of the fathers and mothers of the schoolmates. Moreover, I interact these variables with a dummy, taking the value 1 if the child is a girl, following Eble and Hu (2022).[13] For each individual i, $GO_{i,t,s}^{PeerDad}$ captures the average share of women in the paternal occupations of schoolmates computed from the school-cohort distribution after removing individual i from the distribution. Equivalently, $GO_{i,t,s}^{PeerMom}$ captures the average share of women in the maternal occupation of schoolmates at the school-cohort level after removing i. The coefficients δ_1 and δ_3 reflect the relationship between the gender composition of the occupation of the schoolmates' fathers and mothers, respectively, and the gender composition of the boys' occupation as adults. For girls, $\delta_1+ \delta_2$ reflect the effect from the gender composition of the occupation of schoolmates' fathers, while $\delta_3 + \delta_4$ reflect the effect from the gender composition of the occupation of schoolmates' mothers. Again, I standardized these measures. The estimates for the δ_i's are interpreted as the effect of an increase of one standard deviation. This corresponds to an increase of 4.1 % more women in schoolmates' fathers' occupations and 7.5 % more women in the mothers' occupations, respectively.

I include the gender composition of the child's own parents' occupations. Parent-child correlation is an object of interest in itself, and the main object of analysis in Hederos (2017) and Humlum et al. (2019). This correlation assists in benchmarking the relative importance of schoolmates' parents and own parents as role models, as custom in the literature (e.g. Olivetti et al., 2018; Bifulco et al., 2011; Eble and Hu, 2022). I add a set of family-level controls, $X_{i,t}$, incl. maternal labor supply, sibling composition, household income, and educational attainment. They are all measured when the focal person is 15 years old. Year fixed effects, λ_t, control for non-linear changes over time. I interact year with a gender dummy to capture a general decline in gender segregation. School fixed effects, λ_s, capture unobserved differences in average cohort member characteristics across schools (i.e. sorting) as well as other aspects of school quality that are constant across cohorts within a school.

4.4.2 The Credibility of the Identification Strategy

The idea is to treat the composition of students by cohort within a school as quasi-random and to use this to identify the social spillover effect from schoolmates' parents on adult outcomes. Before moving on to the main analysis, I follow Olivetti et al. (2018) and Bifulco et al. (2011) to document that the strategy is valid and that there is sufficient variation left after adding school and year fixed effects.

[13] An alternative approach would be to estimate separately for each sex. The interaction terms imply that these two approaches are equivalent. My preferred approach is to use the full sample in one regression to avoid issues of power.

Table 4.5. Balancing Test for Cohort Composition

Dependent variable	(1)	(2)	(3)
Leave-out-Mean of Peers' Father's Occupation			
Same-sex sibling	0.301***	0.0912***	0.0162
	(0.0244)	(0.0281)	(0.0293)
Number of siblings	-1.362***	-0.425***	-0.0491
	(0.0443)	(0.0499)	(0.0521)
Age, mother	1.824***	1.394***	-0.214
	(0.235)	(0.269)	(0.280)
Parents marital status	-0.428***	-0.162***	-0.00933
	(0.0272)	(0.0323)	(0.0346)
Vocational Training, father	0.139***	0.0572**	-0.0132
	(0.0244)	(0.0280)	(0.0291)
At least some college, father	0.574***	0.122***	0.00803
	(0.0200)	(0.0221)	(0.0229)
Leave-out-Mean of Peers' Mother's Occupation			
Same-sex sibling	0.0589***	0.0988***	0.0324
	(0.0185)	(0.0230)	(0.0241)
Number of siblings	-0.274***	-0.331***	-0.0260
	(0.0339)	(0.0407)	(0.0425)
Age, mother	1.030***	1.607***	-0.163
	(0.179)	(0.221)	(0.231)
Parents marital status	0.0643***	-0.0373**	0.00748
	(0.0159)	(0.0184)	(0.0188)
Vocational training, father	0.249***	0.0766***	0.0235
	(0.0184)	(0.0228)	(0.0238)
At least some college, father	-0.00541	0.134***	0.0238
	(0.0146)	(0.0176)	(0.0184)
Leave-out-Mean of Peers' Mother's Labor Supply			
Same-sex sibling	0.212***	0.0243*	-0.0309**
	(0.00957)	(0.0125)	(0.0138)
Number of siblings	-1.054***	-0.253***	0.0134
	(0.0186)	(0.0237)	(0.0259)
Age, mother	-0.351***	0.936***	-0.0440
	(0.0992)	(0.127)	(0.139)
Parents marital status	-0.168***	-0.0665***	0.0109
	(0.00670)	(0.00879)	(0.00986)
Vocational training, father	0.257***	0.0706***	0.0203
	(0.00948)	(0.0124)	(0.0137)
At least some college, father	0.260***	0.0868***	0.0101
	(0.00728)	(0.00939)	(0.0104)
School fixed effects	No	Yes	Yes
Year fixed effects	No	No	Yes

Notes: The table reports descriptive statistics for the composition of parents' labor market behavior, before and after removing year and school fixed effects, and school trends.

For identification, labor market outcomes of schoolmates' parents should be quasi-random. While the graphical inspection reported in 4.5, the labor market behavior of schoolmates' parents could be correlated with other characteristics that affect student outcomes. To directly test this, I check whether the schoolmates' parents' labor market characteristics are correlated with other characteristics of the student: sibling sex composition, number of siblings, educational attainment of the father, marital status of the parents, and age of the mother. Except for marital status, it is difficult to imagine how these characteristics should be affected by schoolmates' parents' characteristics, and are thus useful for investigating if sorting on pre-determined characteristics is taking place. The degree of sorting on observables likely provides a good indicator of the degree of sorting on unobservables (Altonji et al., 2005). Table 4.5 reports these "balancing tests" and supports the notion that the model specification identifies an exogenous source of variation and uncorrelated with other important characteristics. Column (1), without any controls, shows that family characteristics of student i correlate with the leave-out-mean from schoolmates' parents. This is expected if there is any sorting across school-districts. Importantly, these effects are greatly reduced by adding school fixed effects. When adding a time trend, only one of the estimated correlations is significantly different from zero - the sibling sex composition. This mitigates concerns about systematic sorting beyond the school level, and concerns about correlated factors that influence outcome. This supports the validity of the identification strategy.

In Table 4.6, I show the extent of variation over cohorts within school after taking out school fixed effects and school trends. Reassuringly, the results mirror Figure 4.5 and show sufficient variation in the main explanatory variables, that is the leave-out-means. The gender composition of the occupations of the focal person's parents are reported for comparison. Panel A and B report the variation in the gender composition of the occupation of the schoolmates' fathers and mothers, respectively.

At the school level, there is substantial variation in the gender share of occupations of fathers and mothers. On average, fathers work in occupations with 16.3 % female workers with a standard deviation of 4 %. This share ranges from 3.8 % to 42.2 %. Mirroring this, mothers work in occupations with on average 65 % women with a standard deviation of 6.2 %. This share ranges from 29.8 % to 87.9 %. Most importantly, after adding school fixed effects and a time trend, there is considerable residual variation left. The standard deviation after adding school and year fixed effects corresponds to approx. 75 % of the raw variation of both schoolmates' mothers' and fathers' occupations. Adding a school trend substantially decrease variation, and only 12 % of the variation is left. Panel C reports the variation in labor supply of the schoolmates' mothers. On average, the mothers work 53 % of full time (approx. 20 hours/week), ranging from 14.1 % to 89.4 %. Again, adding school and level fixed effects still leaves sufficient variation but when adding school trends, little variation is left.

Table 4.6. Raw and residual variation in peers' parents' labor market behavior

	Mean	SD	Min	Max
Panel A: Fathers' occupation				
Own father's occupation	0.176	0.225	0	1.000
Leave-out-Mean, cohort members' father	0.163	0.0394	0.0378	0.422
Residual:				
Net of year and school fixed effects	0	0.0294	-0.121	0.157
Net of grade and school fixed effects and school trends	0	0.00460	-0.0329	0.0152
Panel B: Mothers' Occupation				
Own mother's occupation	0.754	0.240	0.00233	1.000
Leave-out-Mean, cohort members' mother	0.647	0.0621	0.298	0.879
Residual:				
Net of year and school fixed effects	0	0.0475	-0.270	0.201
Net of grade and school fixed effects and school trends	0	0.00491	-0.0201	0.0305
Panel C: Mothers' Labor Supply				
Labor Supply, Mother	533.7	396.7	0	1,000
Leave-out-Mean, cohort members' mother	533.1	110.2	141.3	894.2
Residual:				
Net of year and school fixed effects	0	53.66	-307.7	227.4
Net of grade and school fixed effects and school trends	0	7.943	-33.61	33.37

Notes: The table reports descriptive statistics for the composition of parents' labor market behavior, before and after removing year and school fixed effects, and school trends.

Combined, these two exercises support the identification strategy as both valid and leaving enough variation to identify meaningful effects. First, it appears that selection into schooling is sufficiently dealt with when adding school fixed effects. Unobserved factors that influence within-school-variation in both cohort comparison and outcomes are unlikely to confound the estimates. Second, there is sufficient variation after taking out school and year fixed effects of the explanatory variable to obtain useful estimates. The coefficients can then be interpreted as what Manski (1993) refers to as a contextual effect. That is, I identify the effect of specific characteristics of the school environment: the effect of the gender composition of occupations of schoolmates' parents and the extent it transmits to the next generation.

4.5 Results

In this section, I present the impact from exposure to different male role models, obtained from estimating Equation 4.2. I report the father-son and mother-son association along with the estimates that can be given a causal interpretation. After reporting the main results, I conduct a series of horse race regressions by adding other sources of peer effects.

4.5.1 Main Results

The main results are presented in Table 4.7. In the first column, I only include fathers' occupations. I report the father-child correlation, as well as the main contextual effect, the gender composition of the occupation of the schoolmates' fathers. The parent-child association estimated here is very similar to the partial correlations obtained from Equation 4.1. Most importantly, I also find a positive relationship between the gender composition of the occupations of the fathers of the schoolmates and the boys' occupation in adulthood. An increase in one standard deviation (equivalent to 4 %) of the share of women in the occupation of schoolmates' fathers increases the share of women by 1 % on a baseline of 34.5 %. The effect on girls is negative and approximately 1/3 of the effect on boys (the sum of the baseline and the interaction). This is confirming that male role models are more important for boys than for girls. The effect from fathers' occupations hardly changes when the gender composition of mothers' occupations is added, and at baseline (Column (2)) the gender composition of mothers' occupations is neither influencing boys nor girls.

However, adding a leave-out-mean of maternal labor supply of the peers' mothers reduces the point estimates on the effect from peers' fathers, and increases the size of the point estimate for peers' mothers. The effect from the gender composition of the occupation of the schoolmates' mothers is significant for both boys and girls. For boys, a higher share of women in the occupation of schoolmates' mothers leads to a decrease of the share of women in their occupation as adults. For girls, more women in the occupations of the schoolmates' fathers' leads to a decrease in the share of women in their own occupations. The effect is approx. 1/3 of the size, compared to the boys. The share of women in the occupations of the peers' mothers increases the share of women in the girls' occupations. The role of maternal labor supply is further explored below.

Comparing the father-son and the causal estimate from peers' fathers shows considerable peer effects. Starting with Column (1), the effect from an one standard deviation increase in the share of women in peers' fathers is almost half of the size of a one standard-deviation increase in the share of women in the fathers' occupation. Using Column (5) provides a more conservative but still sizeable effect of roughly 1/4. It is worthwhile to remember that the standard deviation of the share of women in peers' fathers' occupation is 4.2 %, while the standard deviation of the share of women in the fathers' occupation is 24 %.

Table 4.7. Effects of Schoolmates' Parents on Gender Composition of own Occupation

	(1)	(2)	(3)	(4)	(5)
Peers' Parents					
$GO^{PeerDad}$	0.00959***	0.00962***	0.00669***	0.00667***	0.00542***
	(0.00108)	(0.00113)	(0.00111)	(0.00111)	(0.00111)
$GO^{PeerDad}$#Girl	-0.0161***	-0.0158***	-0.0102***	-0.0102***	-0.00802***
	(0.00147)	(0.00153)	(0.00145)	(0.00145)	(0.00147)
$GO^{PeerMom}$		-0.00159	-0.00578***	-0.00580***	-0.00434***
		(0.00110)	(0.00119)	(0.00118)	(0.00117)
$GO^{PeerMom}$ #Girl		0.00158	0.00891***	0.00887***	0.00632***
		(0.00148)	(0.00157)	(0.00156)	(0.00152)
$MLS^{PeerMom}$			0.0113***	0.0111***	0.00675***
			(0.00148)	(0.00148)	(0.00154)
$MLS^{PeerMom}$#Girl			-0.850***	-0.838***	-0.443***
			(0.108)	(0.108)	(0.103)
Own Parents					
GO^{Dad}	0.0214***	0.0216***	0.0213***	0.0207***	0.0207***
	(0.000885)	(0.000925)	(0.000927)	(0.000923)	(0.000926)
GO^{Dad} #Girl	-0.0219***	-0.0213***	-0.0210***	-0.0209***	-0.0209***
	(0.00113)	(0.00120)	(0.00120)	(0.00120)	(0.00120)
GO^{Mom}		-0.00500***	-0.00466***	-0.00424***	-0.00417***
		(0.000942)	(0.000945)	(0.000944)	(0.000942)
GO^{Dad}#Girl		0.0167***	0.0162***	0.0161***	0.0161***
		(0.00130)	(0.00130)	(0.00130)	(0.00130)
MLS, continuous			1.65e-05***	1.63e-05***	1.67e-05***
			(3.17e-06)	(3.31e-06)	(3.32e-06)
MLS, continuous#Girl			-2.32e-05***	-2.27e-05***	-2.31e-05***
			(4.24e-06)	(4.24e-06)	(4.25e-06)
Constant	0.485***	0.487***	0.476***	0.540***	0.582***
	(5.56e-06)	(8.03e-06)	(0.00655)	(0.0257)	(0.0600)
Observations	179,240	155,718	155,718	155,718	155,030
R-squared	0.380	0.378	0.378	0.379	0.379
Year	Yes	Yes	Yes	Yes	Yes
School FE	Yes	Yes	Yes	Yes	Yes
Family				Yes	Yes
Sibling					Yes
Municipal					Yes

Notes: This table shows results for estimating the effects of exposure to different role models, i.e. the gender composition of the occupation the peers' parents from estimating Equation 4.2. Individual controls are added progressively. The outcome of interest is the gender composition of own occupation at age 45. Standard errors clustered at the school level. *** p<0.01, ** p<0.05, * p<0.1

4.5.2 Mechanisms

To understand these effects in relation to other established sources of peer effects, I run a series of horse race regressions. I start with mothers' labor supply, which likely also captures gender norms. Then I move on to other known sources of peer effects. Again, I construct leave-out-mean at the school-year level. I focus on the extent to which the main estimates on $GO^{PeerDad}$ and $GO^{PeerMom}$ decrease when these measures are added. I construct leave-out-means for each of these characteristics of schoolmates' parents and progressively add them to my main specification and interact with gender.

To explore the role of mothers' labor market behavior, I interchange the continuous measure of labor supply with dummies. First, I add the share of schoolmates' mothers that work less than 50 % of full time, then I add the share that works part-time but at least 50 % of full-time, and finally the share that works full-time. This is reported in Table 4.8. For boys, any measure of maternal labor supply reduces the effects from peers' fathers with 40 %. For girls, adding maternal labor supply does not alter the influence from fathers. The effect from peers' mothers' occupations increases when accounting for maternal labor supply, regardless of measure, and this is true for both boys and girls. Looking at the direct effect from labor supply, being exposed to a larger share of mothers that work part-time - compared to other cohorts in the same school - decreases the share of women in the boys' occupation, and increases the share of women in the girls' occupation. Mothers working part-time are still argueably part-time home makers, and thus these children are still exposed to the norms of men and women having very different roles. On the other hand, when more mothers work full-time boys enter occupations with more women and girls enter occupations with fewer women. Somewhat surprisingly, maternal labor supply appears to matter more for boys than for girls. During this period, the share of mothers who work full time increased from 22 % to 38 %. Thus, women working full-time are uncommon and may challenge prevalent gender norms. Women working part-time is arguably still conforming to gender norms by being (part-time) homemakers, thus still exposing the children to the notion of the different role of women and men in the family. Being exposed to a relatively large share of mothers that work full time, reduces gender segregation in the next generation.

Using the same empirical approach and US Addhealth data, Olivetti et al. (2018) show that the extensive margin of peers' mother's labor force participation influences the likelihood of girls working once they reach adulthood. They find no effect on boys. In this setting — Denmark in the '80s — the vast majority of mothers work. The extensive margin is maybe less relevant here compare to the US setting in the '90s studied by Olivetti et al. (2018). Instead, whether peers' mothers work full or part-time is a more relevant margin for what influences the next generation, and I find larger effects for boys than girls. Moreover, I show that exposure to other types of gender norms than maternal labor supply are influencing both boys and girls.

Table 4.8. The Role of Maternal Labor Supply of Schoolmates' Mothers

	(1)	(2)	(3)	(4)	(5)
$GO^{PeerDad}$	0.00953***	0.00542***	0.00554***	0.00623***	0.00543***
	(0.00113)	(0.00111)	(0.00111)	(0.00111)	(0.00111)
$GO^{PeerDad}$#Girl	-0.0156***	-0.00802***	-0.00827***	-0.00940***	-0.00797***
	(0.00153)	(0.00147)	(0.00146)	(0.00147)	(0.00146)
$GO^{PeerMom}$	-0.00169	-0.00434***	-0.00448***	-0.00238**	-0.00300***
	(0.00109)	(0.00117)	(0.00116)	(0.00112)	(0.00107)
$GO^{PeerMom}$#Girl	0.00167	0.00632***	0.00622***	0.00275*	0.00439***
	(0.00147)	(0.00152)	(0.00151)	(0.00146)	(0.00141)
$MLS^{PeerMom}$		0.00675***			
		(0.00154)			
$MLS^{PeerMom}\#Girl$		-0.0103***			
		(0.00183)			
$< 1/2^{PeerMom}$			-0.00716***		
			(0.00141)		
$< 1/2^{PeerMom}$ #Girl			0.00973***		
			(0.00173)		
$[1/2; FullTime[^{PeerMom}$				0.000380	
				(0.00107)	
$[1/2; FullTime[^{PeerMom}$ #Girl				0.00131	
				(0.00144)	
$FullTime^{PeerMom}$					0.00589***
					(0.00139)
$FullTime^{PeerMom}$ #Girl					-0.00992***
					(0.00171)
Constant	0.527***	0.574***	0.571***	0.557***	0.590***
	(0.0235)	(0.0604)	(0.0587)	(0.0585)	(0.0596)
Observations	155,718	155,030	155,030	155,030	155,030
R-squared	0.371	0.373	0.373	0.373	0.374

Notes: This table shows a series of regression adding leave-out-means of maternal labor supply. Column (1) corresponds to column (2) in Table 4.7, and column (2) to the fully specified model from column (7) in Table 4.7. The leave-out-means reflect the share of mothers working less than half time, between half time and full time, and full time, respectively. Standard errors clustered at the school level. *** p<0.01, ** p<0.05, * p<0.1

Further, I add other characteristics of the schoolmates' parents that have been shown to generate peer effects in prior work.[14]. First, I add the gender composition of the classroom. Then, I progressively add the share of schoolmates' fathers' that have completed vocational training, and the share of fathers that have at least some college education. Finally, I add the share of households in the bottom and top 25 % of the income distribution for a given year. These leave-out-means are calculated the same way as the gender composition of the occupations' of parents, implying that the variation is within the school but across cohorts. Again, I interact these measures with the dummy taking the value 1 in the case where the child is a girl.

[14]Peer ability has produced peer effects in many dimensions. However, for this cohort grades are not available. Moreover, closer ties should arguably generate larger peer effects. However, I don't have information on classes within a cohort or more direct measures of friendships.

Table 4.9. Horse Race Regression - Other Sources of Peer Effects

	(1)	(2)	(3)	(4)	(5)
$GO^{PeerDad}$	0.00543***	0.00525***	0.00261**	0.00265**	0.00302***
	(0.00111)	(0.00110)	(0.00109)	(0.00109)	(0.00109)
$GO^{PeerDad}$#Girl	-0.00804***	-0.00750***	-0.00273*	-0.00277**	-0.00345**
	(0.00146)	(0.00144)	(0.00141)	(0.00140)	(0.00140)
$GO^{PeerMom}$	-0.00435***	-0.00397***	-0.00272**	-0.00265**	-0.00194*
	(0.00117)	(0.00116)	(0.00114)	(0.00114)	(0.00115)
$GO^{PeerMom}$#Girl	0.00632***	0.00544***	0.00308**	0.00303**	0.00185
	(0.00120)	(0.00119)	(0.00120)	(0.00119)	(0.00122)
Sex ratio, cohort	0.00853	0.00734	0.0113	0.0114	0.0122
	(0.0140)	(0.0139)	(0.0138)	(0.0138)	(0.0138)
Sex ratio, cohort#Girl	-0.00273	-0.00156	-0.00581	-0.00623	-0.00691
	(0.0183)	(0.0183)	(0.0183)	(0.0183)	(0.0183)
Vocational edu.		-0.0259**	0.0399***	0.0353***	0.0310**
		(0.0124)	(0.0126)	(0.0128)	(0.0129)
Vocational edu.#Girl		0.0765***	-0.0398***	-0.0288*	-0.0223
		(0.0150)	(0.0143)	(0.0150)	(0.0150)
At least some college			0.142***	0.137***	0.104***
			(0.0142)	(0.0144)	(0.0162)
At least some college#Girl			-0.237***	-0.224***	-0.171***
			(0.0142)	(0.0148)	(0.0179)
Low Income HH				-0.00923	0.00788
				(0.0158)	(0.0162)
Low Income HH#Girl				0.0475**	0.0199
				(0.0185)	(0.0194)
High Income HH					0.0591***
					(0.0152)
High Income HH#Girl					-0.0829***
					(0.0165)
Constant	0.576***	0.573***	0.571***	0.573***	0.573***
	(0.0600)	(0.0600)	(0.0603)	(0.0605)	(0.0605)
Observations	155,030	155,030	155,030	155,030	155,030
R-squared	0.373	0.373	0.375	0.375	0.375

Notes: This table shows results for a series of horse race regressions, including additional leave-out-means to assess the relative importance of gender composition of the occupation of parents and other known sources of peer effects. The starting point is the fully specified model from column (7) in Table 4.7. Household income and fathers' educational levels are coded as dummies so the leave-out-means reflects the share with this characteristic. Rich and poor households correspond to the top and bottom 25th percentiles in a given year. Standard errors clustered at the school level. *** p<0.01, ** p<0.05, * p<0.1

The results from this exercise are reported in Table 4.9. Column (1) adds the sex ratio of the cohort to the baseline specification reported in column (5) in Table 4.7 and this doesn't influence the estimates of $GO^{PeerDad}$ or $GO^{PeerMom}$. Adding the share of fathers that has at least some college education reduces the effect, while the share of fathers with vocational training has no effect. Additionally, adding measures of income don't influence the effect of the gender composition of fathers' or mothers' occupations. However, income and educational measures correlate, so adding income without education also reduces the effect. Boys who were socialized in cohorts where a large share of fathers hold at least some college enter occupations with a higher share of women compared to boys - in the same school - who are socialized in cohorts with fewer fathers with at least come college education. The opposite is true for girls: Those exposed to a higher share of fathers with a college degree enter occupations with a smaller share of women. A similar pattern emerges for those exposed to a higher share of schoolmates being from the richest 25 % of households: Boys in these cohorts enter occupations with a higher share of women, and girls enter occupations with fewer women.

4.6 Concluding Remarks

Labor market outcomes are not only affected by economic opportunities. A large literature shows how gender norms influence women's labor force participation and other economic outcomes (Fernandez et al., 2004; Fernández and Fogli, 2009; Goldin and Olivetti, 2013; Farré and Vella, 2013; Alesina et al., 2013; Olivetti et al., 2018). However, much less attention has been paid to how gender norms influence men's labor market outcomes.

This paper shows that labor market segregation in one generation transmits to the next, while role models acting in counter-stereotypical ways decrease gender segregation in the next generation. I extend on widely used measures of gender norms and use the gender composition of parents' occupations. These measures are intended to capture the transmitted norms of the 'appropriate role' of men and women in society and complement the use of maternal labor supply. First, I document stable father-son and mother-daughter associations in the gender composition of occupations. Second, I exploit within-school-across-cohort variation in the gender composition of schoolmates' parents to identify a causal impact from other role models. This exercise confirms the importance of same-sex role models for gender segregation in the next generation. The effects are stronger for boys than for girls. In general, mothers' labor market behavior hardly influences the gender composition of sons' occupations. However, in cohorts where mothers defy existing gender norms — by working full time — they influence outcomes of both boys and girls, and in particular, boys are making less gender stereotypical choices themselves.

In the last decades, many western economies have seen a steady fall in the employment share of traditionally male-dominated, manufacturing sectors. Men's roles both in society and within families are changing, and this is creating hardship, in particular at the left tail of the income distribution. Many studies have shown dire consequences, with the extreme case of increased male mortality (e.g. Autor et al., 2019; Coile and Duggan, 2019; Browning and Heinesen, 2012; Sullivan and von Wachter, 2009). Meanwhile, health care and the service sector is growing but men appear reluctant to enter these occupations. This paper highlights a need for an improved understanding of gender norms as a potential source of friction.

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Appendices

Appendix to Chapter 2

Appendix A: Data description

The measure of leave duration is calculated based on data from the Danish Ministry of Employment's DREAM-database.

This database contains a weekly measure of individual benefits from the government. This include unemployment benefit, sickness benefit, old age benefits, education benefit, among others. If multiple benefits is received the same week, the highest amount is recorded. The measure of parental leave is constructed as a count of number of weeks a parent receives parental leave benefits ('Barselsdagpenge') or receives childcare benefits ('Børnepasningsorlov') is included.

Background variables and labor market data

Using BEF (population), UDDA (education), FIRM (firm), and IDAN (employment), I obtain relvant background variables of all parents. The variables used include

Age	BEF
Gender	BEF
Family identifiers	BEF
Number of children in the family	BEF
Education	UDDA
Income and earnings	IDAN
Retirement contributions	IDAN
Sectorial occupation	FIRM
Occupation unit/firm	FIRM

Appendix B: Sample restrictions

Table B.1: Restriction on data

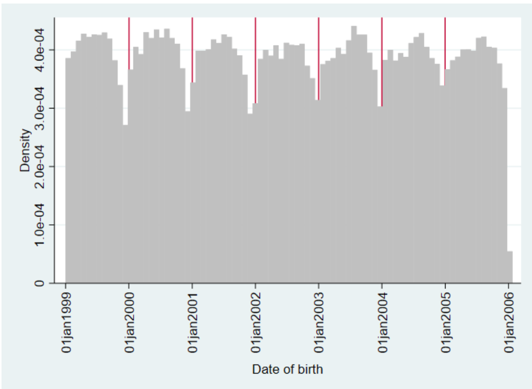
Year	Initial number of observations	Same-sex parents	Fathers co-habiting with child	Twin births	At least one parent enrolled in education	No ATP for for at least one parent	At east one parent is self-employed	Remaining number of observations
2001	58134	25	327	1135	6760	2730	3189	43968
2002	58385	25	302	1235	6953	3177	2655	44038
2003	59140	36	319	1255	7399	2852	3211	44068
2004	59093	39	298	1303	7594	2772	3211	43854
2004	58700	45	282	1296	7798	2697	3214	43368
%	100	0.06	0.52	2.12	12.44	4.85	5.28	74.74

Table B.2: Additional restrictions on the data

3*Year	No information on earnings available for at least one parent	Remaining number of observations	No leave records on mothers	Remaining number of observations
2001	10745	33223	1614	31609
2002	9766	34272	2049	32223
2003	8937	35131	1467	33664
2004	7854	36000	1735	34265
2005	6811	36557	2010	34547
%	15.03	59.70	3.02	56.67

Appendix C: Manipulation into Treatment

Drop in births at New Year



Notes: The figure shows the histogram of birth in the raw data before imposing any restrictions. Red lines mark New Year's. Every year there is a drop around the holidays.

TABLE C1: Formal check of bulking at cut-off, polynomial density estimation

Reform window			Peers		
No donut	Left of c	Right of c	No donut	Left of c	Right of c
Cut-off			Cut-off		
Number of obs	21763	23409	Number of obs	1615	1640
Efficient # of obs	2628	4184	Efficient # of obs	250	493
Order est (p)	2	2	Order est (p)	2	2
Order bias (q)	3	3	Order bias (q)	3	3
BW est	48.684	49.910	BW est	59.894	76.730
Running variable:	assign		Running variable:	assign	
Method	T	P>$\|T\|$	Method	T	P>$\|T\|$
Conventional	9.178	0.0000	Conventional	3.361	0.0008
Robust	7.396	0.0000	Robust	1.507	0.1319
7 days	Left of c	Right of c	7 days	Left of c	Right of c
Cut-off			Cut-off		
Number of obs	21475	22841	Number of obs	1593	1600
Efficient # of obs	3183	4629	Efficient # of obs	234	446
Order est (p)	2	2	Order est (p)	2	2
Order bias (q)	3	3	Order bias (q)	3	3
BW est	50.650	55.840	BW est	60.042	75.227
Running variable:	assign		Running variable:	assign	
Method	T	P>$\|T\|$	Method	T	P>$\|T\|$
Conventional	5.773	0.0000	Conventional	2.973	0.0030
Robust	3.972	0.0000	Robust	0.988	0.3234
14 days	Left of c	Right of c	14 days	Left of c	Right of c
Cut-off			Cut-off		
Number of obs	21159	22267	Number of obs	1572	1562
Efficient # of obs	2287	4408	Efficient # of obs	213	408
Order est (p)	2	2	Order est (p)	2	2
Order bias (q)	3	3	Order bias (q)	3	3
BW est	52.69	64.98	BW est	60.331	75.625
Running variable:	assign		Running variable:	assign	
Method	T	P>$\|T\|$	Method	T	P>$\|T\|$
Conventional	4.171	0.0000	Conventional	2.610	0.0091
Robust	-0.172	0.864	Robust	0.535	0.5924

Appendix D: Regression output

TABLE D1: Reform effects on leave duration, by relative earnings

	(1)	(2)	(3)	(4)	(5)	(6)
Outcome	Mothers' leave duration (weeks)		Fathers' leave duration (weeks)		Fathers' taking long leave (dummy) if leave $\geq$ 8 weeks)[a]	
VARIABLES	Baseline	Interaction	Baseline	Interaction	Baseline	Interaction
Reform effect	4.921***	4.715***	-0.136	-0.196**	0.0163***	0.0104***
	(0.219)	(0.288)	(0.0830)	(0.0830)	(0.00453)	(0.00711)
Interaction						
Reform X		-0.262		0.278***		0.0279***
Mother primary earner		(0.226)		(0.0925)		(0.00512)
Mother primary earner		-1.517***		0.593***		0.0374***
		(0.200)		(0.109)		(0.00635)
Running, before reform	0.00173*	0.00179*	-0.000454	-0.000480	2.75e-06	8.92e-07
	(0.000974)	(0.000974)	(0.000360)	(0.000361)	(1.86e-05)	(1.85e-05)
Running, after reform	0.00687***	0.00688***	0.000819	0.000810	7.39e-05***	7.30e-05***
	(0.00127)	(0.00127)	(0.000498)	(0.000498)	(2.82e-05)	(2.79e-05)
Co-variates (mother)						
Age	0.103***	0.113***	0.0307***	0.0264***	0.00179***	0.00148***
	(0.0134)	(0.0134)	(0.00533)	(0.00531)	(0.000282)	(0.000281)
High school education	-0.476**	-0.443*	0.332***	0.317***	0.0130***	0.0119***
	(0.238)	(0.238)	(0.0755)	(0.0756)	(0.00446)	(0.00445)
Vocational training	0.137	0.140	0.170***	0.168***	0.00331	0.00316
	(0.195)	(0.195)	(0.0545)	(0.0549)	(0.00317)	(0.00320)
Some college	-0.720***	-0.665**	0.626***	0.600***	0.0268***	0.0249***
	(0.277)	(0.276)	(0.0939)	(0.0935)	(0.00545)	(0.00542)
BA or equivalent	1.016***	1.120***	0.726***	0.679***	0.0393***	0.0359***
	(0.224)	(0.225)	(0.0673)	(0.0676)	(0.00411)	(0.00412)
MA or Phd	-2.146***	-1.939***	1.979***	1.885***	0.125***	0.118***
	(0.271)	(0.272)	(0.112)	(0.112)	(0.00713)	(0.00714)
Same edu level as partner	-1.027***	-1.044***	0.0966**	0.104**	0.00451	0.00498*
	(0.133)	(0.133)	(0.0485)	(0.0485)	(0.00274)	(0.00273)
More edu than partner	-1.517***	-1.493***	-0.0515	-0.0631	-0.00510	-0.00597*
	(0.143)	(0.144)	(0.0536)	(0.0535)	(0.00316)	(0.00315)
ln(household income)	-5.783**	-4.835*	12.17***	11.72***	0.346***	0.313***
	(2.705)	(2.706)	(1.031)	(1.033)	(0.0560)	(0.0557)
ln(household income)^2	0.163	0.115	-0.487***	-0.466***	-0.0140***	-0.0124***
	(0.106)	(0.106)	(0.0406)	(0.0407)	(0.00221)	(0.00220)
Share of hh income earned	-6.979***	-4.616***	0.867***	-0.188	0.0641***	-0.0101
	(0.262)	(0.346)	(0.114)	(0.142)	(0.00633)	(0.00804)
Public sector	1.622***	1.480***	-0.0753*	-0.0117	-0.00932***	-0.00483**
	(0.109)	(0.110)	(0.0401)	(0.0401)	(0.00240)	(0.00237)
First child, dummy	0.374***	0.412***	0.374***	0.357***	0.0193***	0.0182***
	(0.100)	(0.100)	(0.0393)	(0.0390)	(0.00239)	(0.00236)
Constant	82.56***	75.77***	-74.83***	-71.72***	-2.210***	-1.986***
	(17.25)	(17.27)	(6.588)	(6.608)	(0.357)	(0.354)
Observations	44,091	44,091	44,091	44,091	44,091	44,091
R-squared	0.127	0.130	0.028	0.032	0.035	0.041

Standard errors in parentheses are clustered on date of birth of child
*** p<0.01, ** p<0.05, * p<0.1

TABLE D2: Reform effects on leave duration, relative education

VARIABLES	(1) Baseline	(2) Mother's leave	(3) Father's leave	(4) Dummy
Reform Effect	4.921***	4.588***	-0.190*	0.00897
	(0.219)	(0.266)	(0.0990)	(0.00551)
Interaction				
Reform X		0.505**	0.0671	0.00828*
Same education		(0.233)	(0.0842)	(0.00501)
Same education	-1.027***	-1.285***	0.0624	0.000287
	(0.133)	(0.193)	(0.0594)	(0.00322)
Reform X		0.360	0.0748	0.0116**
Mother more educated		(0.243)	(0.0887)	(0.00522)
Mother more educated	-1.517***	-1.700***	-0.0898	-0.0111***
	(0.143)	(0.197)	(0.0657)	(0.00361)
Observations	44,091	44,091	44,091	44,091
R-squared	0.127	0.127	0.028	0.035
Controls				
Peer covariates	YES	YES	YES	YES
Own covariates	YES	YES	YES	YES
Time trend	YES	YES	YES	YES

The baseline category is 'mother less educated'.
Standard errors in parentheses are clustered on date of birth of child
*** p<0.01, ** p<0.05, * p<0.1

TABLE E1: Information on eligibility

	Excl. those at old cut-off			Excl. those around old cut-off		
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	First stage	ITT	2SLS	First stage	ITT	2SLS
Reform/peer effect	6.738***	1.091**	0.162**	6.773***	0.992**	0.146**
	(0.727)	(0.494)	(0.0736)	(0.732)	(0.487)	(0.0719)
Observations	3,059	3,059	3,059	3,002	3,002	3,002
R-squared	0.169	0.065	0.074	0.169	0.062	0.071
Controls						
Peer covariates	YES	YES	YES	YES	YES	YES
Own covariates	YES	YES	YES	YES	YES	YES
Time trend	YES	YES	YES	YES	YES	YES

Old cut-off is 24 weeks, the duration of benefits equivalent to UI prior to the reform and the mode leave duration observed in Figure 5. As an extension, those at 23 and 25 weeks is also excluded.

TABLE E2: Consumption Externalities

	Excl. children born in 2002			Interaction w. same municipal		
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	1st stage	ITT	2SLS	1st stage	ITT	2SLS
Reform/peer effect	6.421***	1.215**	0.189**	6.643***	1.375**	0.207**
	(0.757)	(0.596)	(0.0923)	(0.743)	(0.582)	(0.0876)
Reform X				0.785	-0.102	-0.264
Living in the same municipal				(0.552)	(0.419)	(0.435)
Living in the same municipal				0.199	0.770*	0.729*
				(0.594)	(0.425)	(0.399)
Observations	2,848	2,848	2,848	3,154	3,154	3,154
R-squared	0.168	0.065	0.073	0.172	0.065	0.069
Controls						
Peer covariates	YES	YES	YES	YES	YES	YES
Own covariates	YES	YES	YES	YES	YES	YES
Time trend	YES	YES	YES	YES	YES	YES

All specifications include the running variable (d_i, date of birth) and the running variable interacted with an indicator for whether childbirth occurred before or after cut-off.
Standard errors in parentheses are clustered on date of birth of peer child
*** p<0.01, ** p<0.05, * p<0.1

Appendix to Chapter 3

Appendix A: Literature Overview

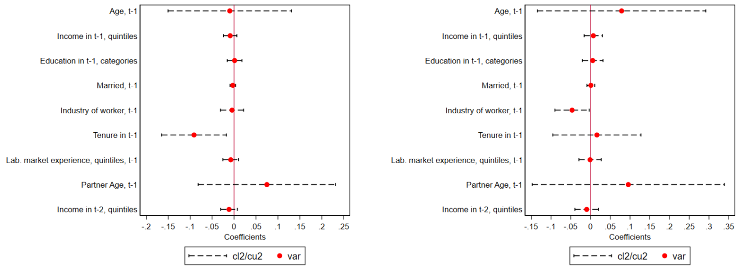
Literature on Job Loss and Earnings, Samples

Autor(s), year	**Setting**	**Sex**	**Comments on gender gap**
North America			
Jacobson et al., 1993	Pennsylvania	F, M	Women better of initially, but recover slower
Sullivan and von Wachter, 2009*	Pennsylvania	M	NA
Couch and Placzek, 2010	Connecticut	F, M	Larger % drop for women
Davis and Wachter, 2011	US	M	NA
Krolikowski, 2018	US	F, M	Not reported
Jung and Kuhn, 2018	US	F, M	Not reported
Lachowska et al., 2020	Washington	F, M	Sex only available for subset of data
Oreopoulos et al., 2008*	Canada	M	NA
Europe			
Bingley and Westergaard-Nielsen, 2003	Denmark	F, M	Not reported
Bennett and Ouazad, 2019**	Denmark	M	Women as robustness
Eliason and Storrie, 2006	Sweden	F, M	Not reported
Seim, 2019	Sweden	M	NA
Rege et al., 2009	Norway	F, M	Not reported
Hardoy and Schøne, 2014	Norway	M	NA
Huttunen et al., 2011	Norway	M	NA
Gathmann et al., 2020***	Finland	F, M	Women worse off
Hijzen et al., 2010	UK	F, M	Smaller % drop for women
Schmieder et al., 2020	West-Germany	M	Women as robustness
Illing et al., 2021	Germany	F, M	Women worse off
Ichino et al., 2017	Austria	F, M	Women worse off, no dynamics
Halla et al., 2020	Austria	M	NA
Raposo et al., 2021	Portugal	F, M	Not reported
Leombruni et al., 2013	Italy	F, M	Women worse off
Other			
Appleton et al., 2001	China	F, M	Women worse off, no dynamics
Bognanno and Delgado, 2005	Japan	F, M	No difference, no dynamics
Khanna et al., 2021**	Columbia	F, M	Women worse off
Bhalotra et al., 2021**	Brazil	F, M	No difference
Rucci et al., 2020	Chile/Brazil	F, M	Not reported

*spillover to children is in the main outcome, **crime is the main outcome, ***health is in the main outcome

Appendix B: Balancing After Matching

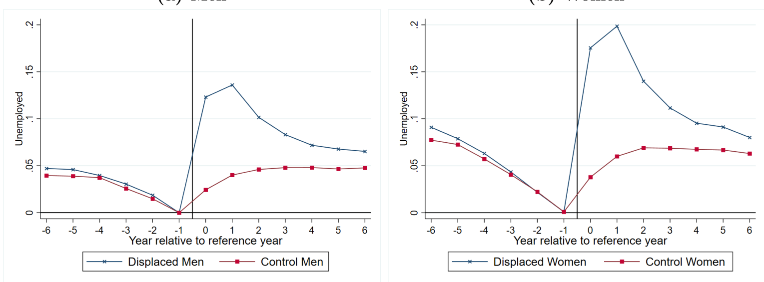
(a) Men **(b)** Women



Notes: We perform the matching separately for men and women and match on pre-displacement earnings, marital status, age, educational groups, tenure at the firm, unemployment history, and labor market experience. Continuous variables are discretized in deciles before matching. We do not match on patners' age or on income in year t-2.

Appendix C: Unemployment Rates, Treatment and Control

(a) Men **(b)** Women



Notes: Evolution of Unemployment (3 months or more) for the exposed and control workers. Panel (a) compares the probability to be unemployed (for 3 months or more) of women who are displaced (blue, X) to the control women (red, circles) based on estimation equation (1). Panel (b) shows the equivalent picture for men. The control group is a matched control group which resembles the displaced individual at the reference date.

Appendix D: Descriptive Statistics

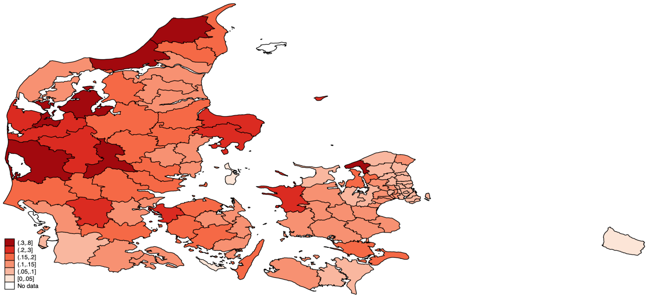
Table 1. By gender for the full population and for the estimating sample

	Male		Female	
	Full private sector	Sample	Full private sector	Sample
Age	41.02	41.22	40.59	40.25
	(11.21)	(10.94)	(10.82)	(10.53)
Age difference	2.07	2.14	-2.48	-2.60
	(4.14)	(4.11)	(4.41)	(4.40)
Children in HH (dummy)	0.48	0.49	0.54	0.56
	(0.50)	(0.50)	(0.50)	(0.50)
Number of children	0.98	0.96	0.87	0.88
	(1.035)	(1.022)	(1.051)	(1.048)
Married	0.58	0.60	0.54	0.56
	(0.49)	(0.49)	(0.50)	(0.49)
Cohabits	0.19	0.20	0.19	0.21
	(0.39)	(0.40)	(0.39)	(0.41)
Have vocational edu.	0.43	0.50	0.37	0.35
	(0.50)	(0.50)	(0.48)	(0.47)
Have university degree	0.08	0.04	0.07	0.05
	(0.27)	(0.20)	(0.25)	(0.21)
	Industry			
Food, Drinks & Tobacco	0.09	0.16	0.11	0.20
	(0.29)	(0.373)	(0.31)	(0.407)
Wood, Paper & Graphics	0.07	0.12	0.07	0.14
	(0.26)	(0.33)	(0.26)	(0.34)
Iron & Metal	0.28	0.43	0.20	0.32
	(0.29)	(0.50)	(0.20)	(0.47)
	Earnings (in DKK)			
Labor market earnings	392,426	382,655	299,452	280,413
	(228,932)	(186,555)	(155,584)	(147,417)
Labor market earnings, partner	236,884	221,193	376,614	349,434
	(161,557)	(149,971)	(279,293)	(237,321)
Own share of HH income	0.65	0.65	0.49	0.48
	(0.21)	(0.22)	(0.24)	(0.24)

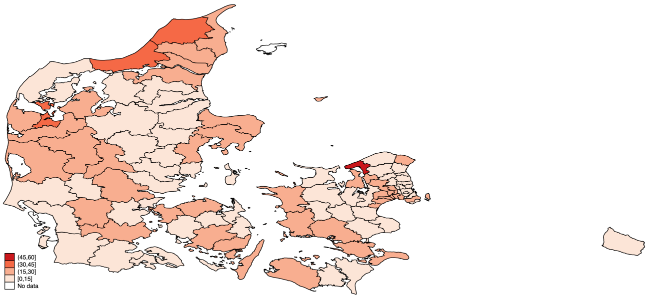
Notes: The table contains means and standard deviation (in parentheses) of key variables.
Earnings are adjusted for inflation and reported in 2019-levels.
Full population refer to all employees in the Danish private sector

Appendix E: Geographical Location of Exposed Workers

(a) % of Displaced Workers among Working Population across Municipalities



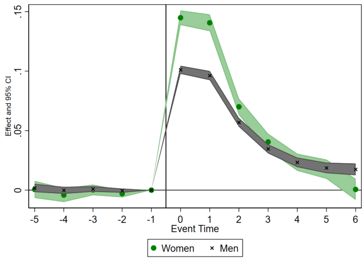
(b) % of Displaced Workers among Production Workers across Municipalities



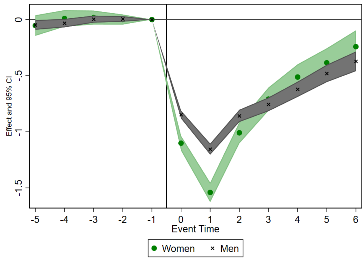
Notes: Data is missing for the small islands of Rømø and Læsø, where less than 5 displaced workers live.

Appendix F: Sensitivity to Plant Closure Definition

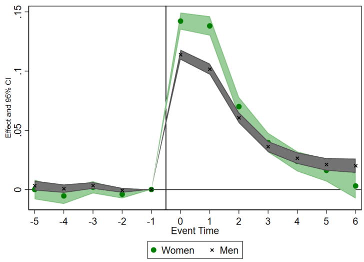
(a) 10+ workers, Unemployment



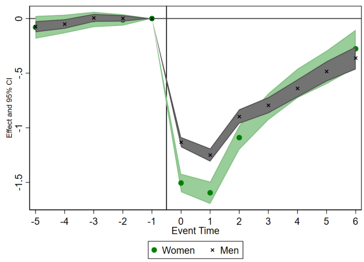
(b) 10+ workers, Log(Labor Earnings)



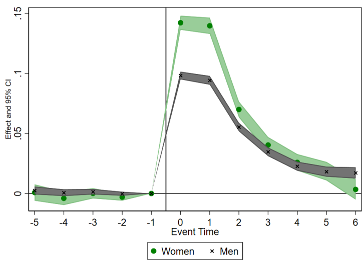
(c) 50+ workers, Unemployment



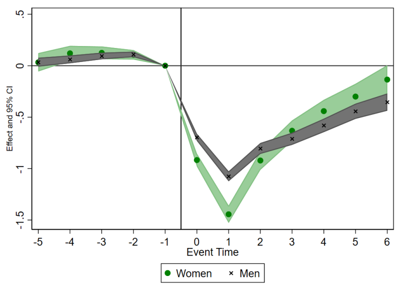
(d) 50+ workers, Log(Labor Earnings)



(e) 1 year closure, Unemployment



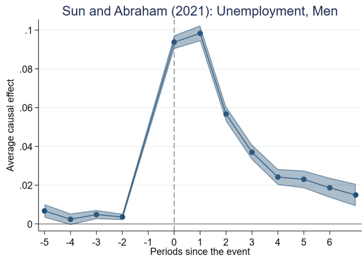
(f) 1 year closure, Log(Labor Earnings)



Notes: See Figure 3.1. Panel (a) and (b) shows displacement effects on workers in plants with at least 10 workers. Panel (c) and (d) show the effect on workers in plants with at least 50 workers. Panel (e) and (f) restrict the sample to only considering plants that close down over 1 year.

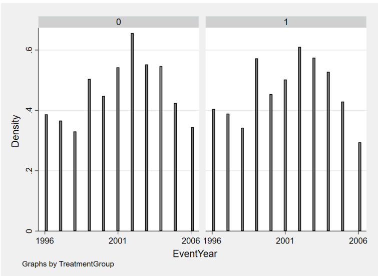
Appendix G: Robustness Estimators

(a) Sun and Abraham, 2021, Men

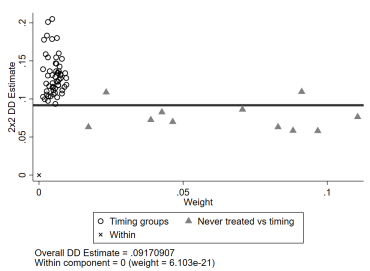


(b) Sun and Abraham, 2021, Women

(c) Histogram: Event Years



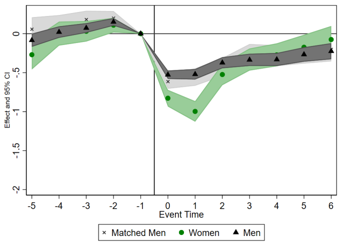
(d) Goodman-Bacon (2021) Decomp



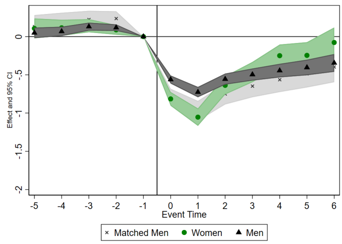
Notes: Top panel report estimates obtained using the estimator proposed by Sun and Abraham, 2021, specifying the control group to be the never-treated worker, for men and women, respectively. The bottom panel shows the distribution of event years and the decomposition proposed in Goodman-Bacon, 2021 showing our estimation does not contain negative weights and the average treatment effect reflects the comparison between the never-treated and timing of events in the treated group.

Appendix H: Heterogeneity, Earnings

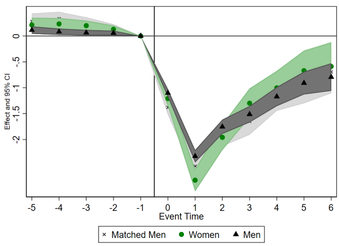
(a) Young (<= 35), By Gender



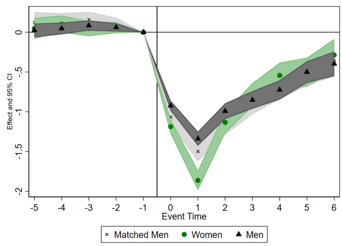
(b) Middle (> 35 & <= 50), By Gender



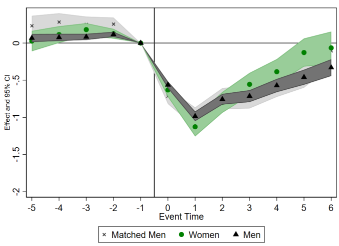
(c) Old (> 50), By Gender



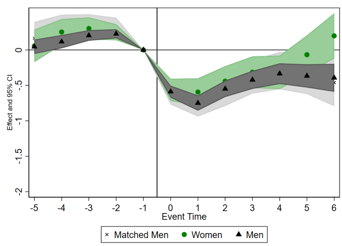
(d) High School or Less, By Gender



(e) Vocational Training, By Gender



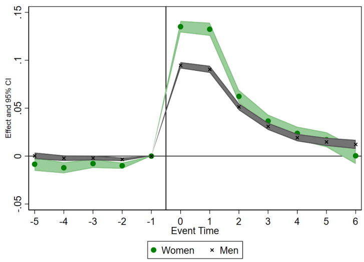
(f) Higher Education, By Gender



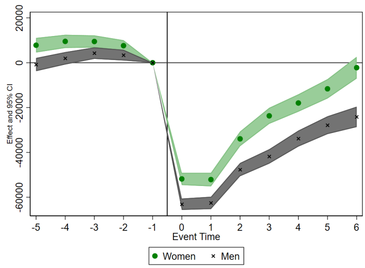
Notes: See Figure 3.1. Panel (a), (b) and (c) report the evolution in log(earnings) rates for workers in different age brackets. Panel (d) shows the log(earnings) for those with high school or less education, panel (e) reports the unemployment rates for workers with vocational training, and panel (f) reports unemployment for those with some higher education.

Appendix I: Sorting, Sectors and Occupations

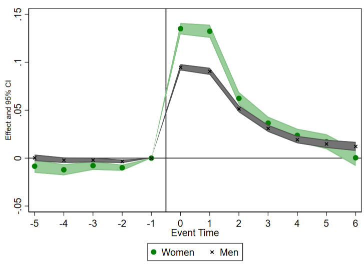
(a) Displacement Effect on Unemployment, Including Industry Fixed Effects



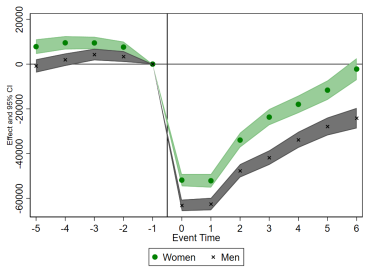
(b) Displacement Effect on Earnings, Including Industry Fixed Effects



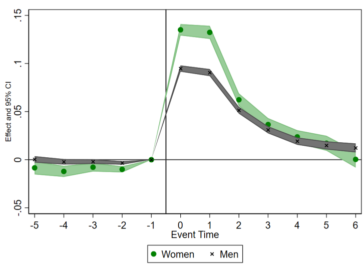
(c) Displacement Effect on Unemployment, Including Occupation Fixed Effects



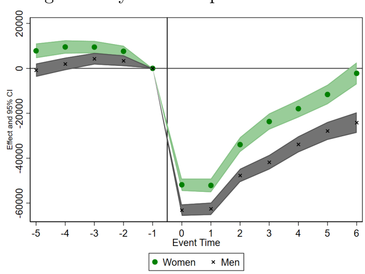
(d) Displacement Effect on Earnings, Including Industry Fixed Effects



(e) Displacement Effect on Unemployment, Including Industry and Occupation Fixed Effects

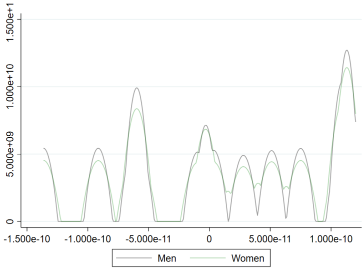


(f) Displacement Effect on Unemployment, Including Industry and Occupation Fixed Effects

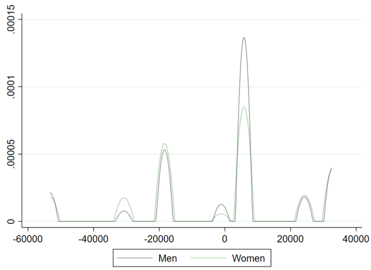


Appendix J: Fixed Effects, Men and Women

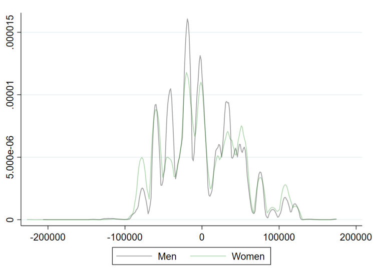
(a) Gender distribution across years



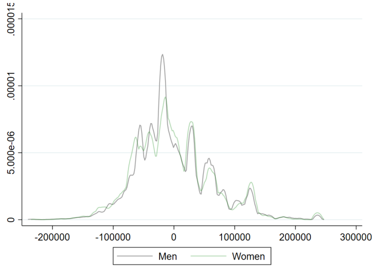
(b) Distribution across industries



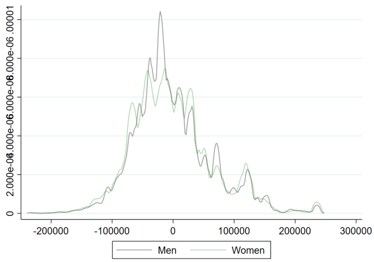
(c) Distribution across occupations



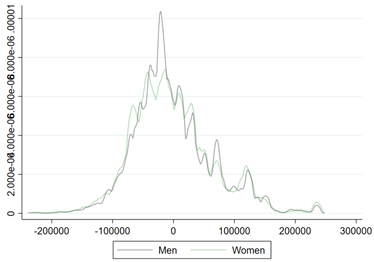
(d) Distribution across firms



(e) Distributions across plants



(f) Distributions across plants and occupations



Appendix to Chapter 4

Appendix A: Correlations Across Measures

	1)	2)	3)	4)	5)	6)	7)	8)	9)
1) Leave-out-Mean, peers' father's occ	1.00								
2) Leave out Mean, peers' mother's occ	0.2101	1.00							
3) Leave out Mean, maternal labor supply	0.4257	0.5164	1.00						
4) Leave-out-mean, working mother ==1	0.3958	0.6024	0.9068	1.00					
5) Own father's occupation	0.0494	0.0024	0.0299	0.0173	1.00				
6) Own mother's occupation	-0.0121	0.0205	-0.0127	-0.0021	-0.0405	1.00			
7) Own working mother==1	0.0239	0.0196	0.0462	0.0398	0.0129	0.0610	1.00		
8) Duncan Index in Childhood	-0.3717	-0.0218	-0.5022	-0.4114	-0.0523	0.0259	-0.0360	1.00	
9) Female LFP in Childhood	0.2034	0.2910	0.5165	0.3992	0.0262	-0.0236	0.0401	-0.2268	1.00

Correalation across parents, schools, and municipal measures of gender norms.

Appendix B: Inter-generational Correlation, Incl. Homemakers

			Sons			
	(1)	(2)	(3)	(4)	(5)	(6)
GO^{Dad}	0.0224***	0.0221***	0.0170***	0.0.171***	0.0171***	0.0170***
	(0.00114)	(0.00111)	(0.000739)	(0.000731)	(0.000740)	(0.000737)
GO^{Mom}		-0.00153*	-0.00153*	-0.00195**	-0.00183**	-0.00191**
		(0.000928)	(0.000680)	(0.000711)	(0.000655)	(0.000700)
MLS, [1/2;full time[					-0.00651**	
					(0.00228)	
MLS, <1/2 time					-0.000712	
					(0.00173)	
MLS, continous				-8.32e-06**		-7.90e-06**
				(2.87e-06)		(2.86e-06)
Constant	0.284***	0.284***	0.334***	0.308***	0.306***	0.228***
	(0.00299)	(0.00298)	(0.0333)	(0.0297)	(0.0312)	(0.0290)
Observations	145,793	125,649	125,649	125,649	125,649	125,229
R-squared	0.023	0.024	0.048	0.048	0.048	0.049
			Daughters			
	(1)	(2)	(3)	(4)	(5)	(6)
GO^{Dad}		8.90e-05	0.00241***	0.00211***	0.00218***	0.00217***
		(0.000513)	(0.000489)	(0.000511)	(0.000507)	(0.000522)
GO^{Mom}	0.0109***	0.0110***	0.00789***	0.00892***	0.00906***	0.00894***
	(0.000528)	(0.000542)	(0.000630)	(0.000661)	(0.000661)	(0.000678)
MLS, [1/2;full time[					0.000640	
					(0.00103)	
MLS, <1/2 time					-0.00590***	
					(0.00126)	
MLS, continous				1.71e-05***		1.67e-05***
				(1.78e-06)		(1.88e-06)
Constant	0.609***	0.609***	0.805***	0.878***	0.901***	0.855***
	(0.00295)	(0.00289)	(0.0195)	(0.0222)	(0.0245)	(0.0638)
Observations	148,227	124,444	124,444	124,444	124,444	124,057
R-squared	0.012	0.012	0.019	0.020	0.020	0.020
Municipal	Yes	Yes	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes	Yes	Yes
Family			Yes	Yes	Yes	Yes
Siblings						Yes
Municipal norms						Yes

Notes: The sample includes individuals of Danish ancestry born between 1966-1974. Estimates obtained from estimating Equation 4.1, separately for boys and girls, adding mothers who are not present in the labor market by assigning them a occupation equal to the most female dominated occupation of the year. See above for a full list of covariates. Standard errors clustered at birth year. Measures of maternal labor supply is obtained from mandatory pension contributions. Standard errors clustered at birth year, *** p<0.01, ** p<0.05, * p<0.1

Appendix C: Rank-Rank Associations

	Sons					
	(1)	(2)	(3)	(4)	(5)	(6)
GO^{Dad}	0.0826***	0.0825***	0.0663***	0.0664***	0.0661***	0.0663***
	(0.00309)	(0.00294)	(0.00169)	(0.00170)	(0.00171)	(0.00173)
GO^{Mom}		-0.0346***	-0.0155***	-0.0157***	-0.0143***	-0.0148***
		(0.00265)	(0.00206)	(0.00206)	(0.00205)	(0.00196)
MLS, [1/2;full time[					-0.577**	
					(0.219)	
MLS, <1/2 time					-0.122	
					(0.192)	
MLS, continous				-8.32e-06**		-7.90e-06**
				(2.87e-06)		(2.86e-06)
Constant	34.04***	35.74***	53.44***	51.06***	51.49***	47.09***
	(0.315)	(0.332)	(5.195)	(5.543)	(5.586)	(7.887)
Observations	145,793	125,649	125,649	125,649	125,649	125,229
R-squared	0.024	0.026	0.048	0.048	0.048	0.049
	Daughters					
	(1)	(2)	(3)	(4)	(5)	(6)
GO^{Dad}		-0.00814***	0.00448**	0.00422*	0.00432*	0.00453**
		(0.00133)	(0.00188)	(0.00190)	(0.00190)	(0.00187)
GO^{Mom}		0.0514***	0.0403***	0.0406***	0.0398***	0.0401***
		(0.00239)	(0.00266)	(0.00267)	(0.00263)	(0.00282)
MLS, [1/2;full time[					-0.0159	
					(0.0967)	
MLS, <1/2 time					-0.684***	
					(0.129)	
MLS, continous				0.00122***		0.00118***
				(0.000226)		(0.000230)
Constant		68.11***	98.75***	103.3***	104.1***	99.81***
		(0.253)	(4.046)	(4.125)	(4.234)	(7.243)
Observations	148,227	124,444	124,444	124,444	124,444	124,057
R-squared	0.013	0.013	0.021	0.021	0.021	0.021
Municipal	Yes	Yes	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes	Yes	Yes
Family			Yes	Yes	Yes	Yes
Siblings						Yes
Municipal norms						Yes

Appendix D: Parent-Child Associations, Excl. Same education

	(1)	(2)	(3)	(4)	(5)	(6)
			Sons			
GO^{Dad}	0.0192***	0.0192***	0.0146***	0.0147***	0.0146***	0.0146***
	(0.00104)	(0.00108)	(0.000784)	(0.000788)	(0.000791)	(0.000799)
GO^{Mom}		-0.00480***	-0.00141**	-0.00147**	-0.00109	-0.00144**
		(0.000780)	(0.000579)	(0.000587)	(0.000589)	(0.000558)
MLS, [1/2;full time[					-0.00566**	
					(0.00215)	
MLS, <1/2 time					0.000251	
					(0.00209)	
MLS, continous				-9.21e-06**		-8.83e-06**
				(3.13e-06)		(3.13e-06)
Constant	0.284***	0.284***	0.467***	0.432***	0.438***	0.458***
	(0.00282)	(0.00297)	(0.0580)	(0.0618)	(0.0621)	(0.0757)
Observations	141,010	121,368	121,368	121,368	121,368	120,967
R-squared	0.021	0.022	0.043	0.043	0.043	0.044
			Daughters			
	(1)	(2)	(3)	(4)	(5)	(6)
GO^{Dad}		6.81e-06	0.00273***	0.00257***	0.00261***	0.00263***
		(0.000610)	(0.000613)	(0.000620)	(0.000614)	(0.000614)
GO^{Mom}	0.0111***	0.0112***	0.00835***	0.00843***	0.00823***	0.00844***
	(0.000683)	(0.000686)	(0.000803)	(0.000803)	(0.000786)	(0.000823)
MLS, [1/2;full time[					-0.000471	
					(0.00102)	
MLS, <1/2 time					-0.00623***	
					(0.00126)	
MLS, continous				1.17e-05***		1.13e-05***
				(2.09e-06)		(2.12e-06)
Constant	0.607***	0.607***	0.933***	0.977***	0.985***	0.959***
	(0.00287)	(0.00279)	(0.0361)	(0.0366)	(0.0372)	(0.0710)
Observations	123,198	119,023	119,023	119,023	119,023	118,658
R-squared	0.012	0.012	0.022	0.022	0.022	0.022
Municipal	Yes	Yes	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes	Yes	Yes
Family			Yes	Yes	Yes	Yes
Siblings						Yes
Municipal norms						Yes

Notes: The sample includes individuals of Danish ancestry born between 1966-1974. Estimates obtained from estimating Equation 4.1, separately for boys and girls, exclude pairs with the same education (field and school). See above for a full list of covariates. Standard errors clustered at birth year. Measures of maternal labor supply is obtained from mandatory pension contributions. Standard errors clustered at birth year, *** p<0.01, ** p<0.05, * p<0.1

Appendix E: Heterogeneity by Maternal Education

	Boys	Girls	Boys	Girls	Boys	Girls
	High School or Less		Vocational		At Least Some College	
	(1)	(3)	(3)	(4)	(5)	(6)
GO^{Dad}	0.0138***	0.00127	0.00139	0.00109	0.00272*	0.00297
	(0.00101)	(0.000927)	(0.000911)	(0.00133)	(0.00120)	(0.00162)
GO^{Mom}	0.00243*	0.00681***	-0.00181	0.00927***	-0.0123***	0.0145***
	(0.00119)	(0.00131)	(0.00148)	(0.00132)	(0.00113)	(0.00139)
MLS, continous	-1.06e-05**	7.39e-06*	-1.39e-05**	9.60e-06*	6.85e-07	2.07e-05**
	(4.25e-06)	(3.57e-06)	(5.71e-06)	(5.10e-06)	(5.15e-06)	(8.45e-06)
Constant	0.160	0.805***	-0.645***	1.297***	0.572***	1.135***
	(0.145)	(0.0727)	(0.122)	(0.158)	(0.160)	(0.169)
Observations	50,267	49,559	47,792	47,556	25,774	25,587
R-squared	0.031	0.018	0.027	0.019	0.042	0.047
Municipal	Yes	Yes	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes	Yes	Yes
Family	Yes	Yes	Yes	Yes	Yes	Yes
Siblings	Yes	Yes	Yes	Yes	Yes	Yes
Municipal Norms	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The sample includes individuals of Danish ancestry born between 1966-1974. Estimates obtained from estimating Equation 4.1, separately for boys and girls, split by maternal educational attainment. See above for a full list of covariates. Standard errors clustered at birth year. Measures of maternal labor supply is obtained from mandatory pension contributions. Standard errors clustered at birth year, *** $p<0.01$, ** $p<0.05$, * $p<0.1$

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