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Essays on Gender and Skills in the Labour Market

Mathias Fjællegaard Jensen

A thesis presented for the degree of
Doctor of Philosophy

Primary supervisor: Fane Naja Groes
Secondary supervisor: Herdis Steingrimsdottir

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Contents

Preface

This thesis is a result of my PhD studies at the Department of Economics, Copenhagen Business School. I am grateful for the Department's support of my studies and for my always friendly, helpful, and encouraging colleagues.

I would like to express my sincere gratitude to my primary supervisor, Fane Groes, for your invaluable advice, patience, support, and friendship throughout my studies. Your guidance has greatly improved this thesis and my time as a PhD student. Thank you for introducing me to your friends and co-authors, some of whom I have also had the pleasure to work with during my studies. Thanks also to my secondary supervisor, Herdis Steingrimsdottir, for your insightful comments and feedback on my work.

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Lastly, I would like to thank my grandparents for helping me solve my first equations and for showing me how to persevere. You are sorely missed. I know that you would have been immensely proud of me. I would like to dedicate this thesis to you.

English Abstract

This thesis consists of three independent chapters on gender and skills in the labour market. Chapters 1 and 2 focus on gender differences in labour market outcomes, and Chapters 1 and 3 on the role of skills in the labour market. Thus, Chapter 1 binds the three chapters together.

In the first chapter, "Gender Differences in Returns to Skills: Evidence from Matched Vacancy-Employer-Employee Data", I show the advantages of individual-level matched vacancy-employer-employee data by estimating returns to skills and their heterogeneity across genders while controlling for firm and occupation FEs. Recently available data from online job vacancies have enabled analyses that move beyond across-occupation variation to also include within-occupation variation in workers' task-specific skills. However, existing analyses of job vacancy data are typically limited by the fact that information on the hired worker(s) is hidden. To overcome this issue, I develop a novel, pseudo-individual match between Danish job vacancy data and register data. With data on the hired worker(s) for each online job vacancy, I can test how the employment of skills and the returns to skills depend on the gender of the worker. I use the matched employer-employee-vacancy data to show that women face significantly lower returns to cognitive, character, customer service, financial, and specific computer skills when compared to men after controlling for both occupation and firm fixed effects. In other words, despite being employed in jobs that require the same task-specific skills, women generally face lower hourly wages than their male colleagues.

In the second chapter, "Income Effects and Labour Supply: Evidence from a Child Benefits Reform", co-authored with Jack Blundell, we look further into gender differences in labour market outcomes. We exploit a unique and unexpected reform to the child benefit system in Denmark to assess the effects of child benefits on parental labour supply. A cap on child benefit payments in 2011 led to a non-negligible reduction in child benefits for larger families with young children. The differential impact of this policy shift represents an opportunity to assess the causal impact of child benefit programmes on the labour

supply of mothers and fathers. As a new government was elected in late 2011, the reform was repealed after being in place for a single year, which allows us to assess long term effects of a temporary income shock that was perceived to be permanent. We find that a reduction in child benefits leads to a large increase in the labour supply of mothers; the effect on fathers is much smaller. Both mothers and fathers respond to the policy at the intensive margin, but the strongest response is from mothers at the extensive margin. The majority of the effects can be ascribed to fertility responses, but even after controlling for fertility-related family characteristics, we find significant increases in labour supply after the introduction of the reform. We confirm this result by using data on parents' consultations with doctors regarding sterilisation, a common procedure in Denmark. Lastly, the labour supply effects of the reform are generally sustained for at least 3 years after its repeal.

In the final chapter, "University Admission and the Similarity of Fields of Study", co-authored with Moira Daly and Daniel le Maire, we return to the theme of task-specific skills and education. We exploit discontinuities from the Danish university enrolment system and find that students who are marginally accepted into their preferred program in a broad field that is different from their next-best choice (e.g. business rather than science) experience significant and long-lasting rewards as a result. In contrast, students whose preferred and next-best program lie within the same broad field do not. Exploiting data from online job postings, we find that the estimated effects on skill usage similarly vary according to the degree of similarity between preferred and next-best choices.

Danish Abstract

Denne Ph.d.-afhandling indeholder tre uafhængige kapitler om køn og færdigheder på arbejdsmarkedet. Kapitel 1 og 2 fokuserer på kønsforskelle i arbejdsmarkedet, mens Kapitel 1 og 3 fokuserer på færdigheders rolle i arbejdsmarkedet. Dermed forbinder Kapitel 1 de tre kapitler emnemæssigt.

I det første kapitel, "Gender Differences in Returns to Skills: Evidence from Matched Vacancy-Employer-Employee Data", viser jeg en række fordele ved jobopslag-arbejdsgiver-arbejdstagerdata kombineret på individniveau, når jeg estimerer afkast på færdigheder og deres heterogenitet på tværs af køn, mens jeg kontrollerer for firma- og faggruppespecifikke effekter. Nyligt tilgængelige data fra jobopslag har gjort det muligt at foretage analyser, som ikke kun udnytter variation i opgavespecifikke færdigheder på tværs af faggrupper, men også variation indenfor faggrupper. Eksisterende analyser af jobopslagsdata er dog begrænset af det faktum, at data om de(n) nyansatte medarbejder(e) ikke er umiddelbart tilgængelige. For at overkomme denne begrænsning, udvikler jeg i dette kapitel et nyt pseudo-individuelt match mellem danske jobopslag og registerdata. Med disse data om de(n) nyansatte medarbejder(e) kan jeg således teste, om anvendelse af og afkast på opgavespecifikke færdigheder afhænger af den ansattes køn. Ved hjælp af de kombinerede jobopslag-arbejdsgiver-arbejdstagerdata finder jeg, at kvinder sammenlignet med mænd møder signifikant lavere afkast på kognitive, karakter, kundeservice, finansielle og specifikke computerrelaterede færdigheder, når jeg kontrollerer for både firma- og faggruppespecifikke effekter. Med andre ord: Selv når kvinder og mænd er ansat i job, som kræver de samme opgavespecifikke færdigheder, får kvinder generelt en lavere timeløn end deres mandlige kollegaer.

I det andet kapitel, "Income Effects and Labour Supply: Evidence from a Child Benefits Reform", med medforfatter Jack Blundell, kigger vi nærmere på kønsforskelle på arbejdsmarkedet. Vi udnytter en unik og uventet reform i børnepengesystemet i Danmark til at undersøge effekterne af børnepenge på forældres arbejdsudbud. Et loft over børnepengeudbetalinger i 2011 reducerede børnepengene for store familier med yngre børn.

Reformens differentielle påvirkning af familier gør det muligt at estimere den kausale effekt af børnepenge på arbejdsudbuddet for mødre og fædre. Da der blev valgt en ny regering i slutningen af 2011, blev reformen afskaffet igen efter kun at have været gældende i et år. Dette giver os mulighed for at undersøge de længerevarende konsekvenser af en midlertidig nedgang i indkomst, som ellers blev opfattet som værende permanent, da den trådte i kraft. Vores analyser viser, at en reduktion i børnepenge leder til en stor forøgelse af mødres arbejdsudbud, og at effekten på fædre er langt mindre. Både mødre og fædre reagerer på reformen på den intensive margin, men den største respons ses på den ekstensive margin. Størstedelen af effekterne kan tilskrives ændrede fertilitetsmønstre, men selv når vi kontrollerer for fertilitetsrelaterede familiekarakteristika, finder vi signifikante stigninger i arbejdsudbuddet efter introduktionen af reformen. Vi bekræfter dette resultat ved hjælp af data om forældres lægekonsultationer vedrørende sterilisering, et almindeligt indgreb i Danmark. Effekterne af reformen generelt opretholdt 3 år efter reformen bortfaldt.

I det sidste kapitel, "University Admission and the Similarity of Fields of Study", med medforfatterne Moira Daly og Daniel le Maire, vender vi tilbage til temaet vedrørende opgavespecifikke færdigheder og uddannelse. Vi udnytter diskontinuiteter fra det danske universitetsansøgningssystem og finder, at studerende, som er marginalt optaget på deres foretrukne studium i et bredt fagområde, der er forskelligt fra deres andetvalg (f.eks. erhvervsøkonomi i stedet for naturvidenskab), som resultat oplever signifikant og langvarig belønning i form af højere indkomst. I modsætning hertil, oplever studenterende, hvis fortrukne studium og deres andetvalg er i samme brede fagområde, ikke nogen belønning for marginalt optag på deres fortrukne studium. Ved hjælp af data fra jobopslag, finder vi, at de estimerede effekter på opgavespecifikke færdigheder på samme måde varierer afhængigt af ligheden mellem ansøgeres fortrukne studium og deres andetvalg.

Introduction

This thesis contributes to two bodies of literature on gender differences in earnings: First, the literature documenting that women and men may undertake similar work, but still be paid differently. Second, the literature emphasising that women and men may also undertake different work that is remunerated differently. The first category includes papers on various forms of discrimination, differences in negotiation of wages, etc., and the second category includes the many papers on segregation and sorting. Chapter 1 contributes to the literature in the first category by comparing the earnings of women and men that utilise similar skills. Chapter 2 is related to the literature in the second category as it shows how a policy intended to improve child outcomes may in fact reinforce gender differences in labour market participation, which again may contribute to gender differences in earnings. Chapter 3 returns to the theme of skills as we examine the effects of university admission on later skill utilisation and earnings on the job.

Family formation and parenthood are emphasised as key drivers of gender differences in earnings (e.g Kleven et al., 2019). On average, women undertake more childcare than fathers, which affects labour market participation in the form of career interruptions (maternity leave or stay-at-home moms) and in the form of part-time or family friendly employment (e.g Bertrand et al., 2010; Gupta & Smith, 2002; Nielsen et al., 2004; Joshi et al., 1999). Partly as a result of gendered division of childcare, segregation into different types of jobs (e.g. occupations and industries) and into different employers (e.g. private vs. public) is also pervasive and plays a central role in explaining the persistent gender inequalities in pay (Blau & Kahn, 2017; Levanon & Grusky, 2016; Olivetti & Petrongolo, 2014; Jarman et al., 2012; Nielsen et al., 2004; Card et al., 2016).

A number of studies have highlighted how numerous labour market and family policies were introduced in high-income countries to enable women to work while also having children (e.g. Olivetti & Petrongolo, 2017). However, such policies may also have the (unintended) consequence of reinforcing traditional gender roles. For example, generous parental leave policies may result in women taking even more parental leave than fathers, and thus, women may experience more and longer interruptions to their human capital accumulation (e.g. Phipps et al., 2001; Gupta & Smith, 2002).

In addition to parental leave policies, financial assistance to families with young children is a common policy adopted across many developed countries to encourage fertility, improve well-being and enhance the long-term opportunities of children. In Chapter 2, co-authored with Jack Blundell, we look further into an example of such a policy, namely child or family benefits, which are cash transfers to families with dependent children. Such benefits are often independent of income and labour market status, and unconditional child benefits thus represents an alternative to conditional or in-work benefits, such as the federal Earned Income Tax Credit (EITC) in the United States. Recently, the US child tax credit has been expanded to include monthly allowances to families with children, and thus, discussions of the effects of unconditional child benefits has reemerged (Financial Times, 2021).

Similar to the EITC, which has been argued to be *"effectively subsidizing married mothers to stay home"* (Eissa & Hoynes, 2004, p. 1931), unconditional child benefits can be viewed as a subsidy to parents enabling them to limit their labour supply. Limiting parental labour supply may be beneficial for certain (child) outcomes, but it may also reinforce less desirable outcomes, such as the child pay penalty and the gender pay gap if labour supply responses are more pronounced for mothers than for fathers (e.g. Kleven et al., 2019; Blau & Kahn, 2017).

Despite their prevalence across countries, only few studies have evaluated the effects of the introduction or expansion of unconditional child benefit policies. These studies generally find that mothers decrease their labour supply after an increase in child benefits while fathers' labour supply is unaffected (Hener, 2016; Tamm, 2010; González, 2013). Hener (2016) points out that the effectiveness of child benefits in improving families' financial situation is limited while the strain on public finances are amplified by behavioural responses to the increase in child benefits as the resulting decrease in maternal labour supply reduces tax payments.

In Chapter 2, we exploit a unique and unexpected reform to the child benefit system in Denmark to assess the effects of child benefits on parental labour supply. A cap on child benefit payments in 2011 led to reduced child benefits for larger families with young children. The differential impact of this policy shift represents an opportunity to assess the causal impact of child benefit programmes on the labour supply of mothers and fathers. As a new government was elected in late 2011, the reform was repealed after being in place for a single year. The unexpected repeal allows us to assess long term effects of a temporary income shock that was perceived to be permanent. We find that a reduction in child benefits leads to a large increase in the labour supply of mothers; the effect on fathers is much smaller. Both mothers and fathers respond to the policy at the intensive margin, but the strongest response is from mothers at the extensive margin. The majority of the effects can be ascribed to fertility responses, but even after controlling for fertility-related family characteristics, we find significant increases in labour supply after the introduction of the reform. We confirm this result by using data on parents' consultations with doctors regarding sterilisation, a common procedure in Denmark. Lastly, the labour supply effects of the reform are generally sustained for at least 3 years after its repeal.

Women and men often end up in different types of jobs and have different careers paths, largely due to parenthood. This type of sorting accounts for a large share of gender differences in earnings, but even if we condition on women and men being in similar jobs, it is still observed that women receive lower hourly wages than men. For example, Card et al. (2016) show that women in Portugal are less likely to work in high paying firms, but even if they do, they are likely to receive a smaller share of the firm specific pay premiums from which their male colleagues benefit. Similarly, numerous studies have found that conditional on working in the same occupations and industries, women still receive lower wages than their male colleagues (Blau & Kahn, 2017; Goldin, 2014; Lindley, 2016). However, it could be the case that women and men employ different task-specific skills, even within firms and occupations, and that these skills are remunerated differently. Task-specific skills refer to the type of skills that are associated with specific tasks, such as social skills, cognitive skills, and computer skills, and do not refer to education levels.

Exactly what individuals do at their jobs, however, has to a large degree remained a "black box" as data on the job-level composition of task-specific skills rarely are available. The few datasets that contain individual- or job-level information on task-specific skills are relatively small surveys, and thus, when using these data to estimate gender differences in returns to skills, one cannot control for sorting into firms either because data on firm affiliation is not available or because the samples are too small to include firm fixed effects in wage regressions (e.g. the OECD Survey of Adult Skills PIAAC, or the UK Skills Surveys as used by Lindley, 2012). From an economic perspective, it can be argued that firm fixed effects should be included in wage regressions when estimating gender differences in pay if workers face non-pecuniary firm-specific benefits or costs.

In Chapter 1, I develop a matched vacancy-employer-employee dataset from Danish online job posts covering the period 2007-2017. This dataset enables estimations of gender differences in returns to task-specific skills while controlling for both occupation and firms

FEs. Information on task-specific skills can be extracted from the text from each job post (using an approach similar to Deming & Kahn, 2018). Uniquely, the Danish job vacancy data can be matched with Danish register data at the firm*occupation*month-level. This exercise can only be undertaken because Danish register data include monthly information on employment, including earnings, occupation codes, and firm identifiers, for the universe of Danish employees. The resulting pseudo-individual match between vacancy data and register data make it possible to evaluate gender differences in returns to skills both *across* and *within* occupations and firms. Due to the match with Danish register data, I can control for factors that are usually highlighted as contributing to the gender pay gap in the literature, such as parental status and sorting into firms and occupations. By doing so, I can answer the question: Do women and men face equal returns to the same task-specific skills, e.g. social skills, cognitive skills, and computer skills?

When answering this question, I provide a validation and operationalisation of skills data derived from job vacancies matched with register data, and I provide a description of their complementarities. To my knowledge, this is the first paper that matches such data at an individual level and at a large scale. I also show the advantages of individual-level matched vacancy-employer-employee data by estimating returns to skills and their heterogeneity across genders while controlling for firm and occupation FEs. I find that task-specific skills do not yield particularly high returns to men beyond what can be explained by occupation and firm fixed effects with the exception of cognitive and financial skills. However, I find that there is significant heterogeneity in returns to skills across genders, even when controlling for firm and occupation FEs along with a long list of other controls. With these FEs and controls in the model, returns to 5 out of 9 task-specific skills are significantly lower for women when compared to men. Thus, even if women and men are in similar jobs and are using similar task-specific skills, women generally receive lower hourly wages when compared to their male colleagues.

In Chapter 3, co-authored with Moira Daly and Daniel le Maire, we look further into factors that affect individuals' use of task-specific skills. Specifically, we are interested in the effects of university admission on both earnings and task-specific skills use on the job. We use a regression discontinuity design to estimate the causal effect of admission to one's preferred field of study on earnings and subsequent skill use. When we consider students on the margin of admission between two broad fields (e.g. Humanities and Science), we find that students on average realize higher returns to studying their preferred field, consistent with the findings of Kirkeboen et al. (2016). On the other hand, when we consider students on the margin between two narrow fields within the same broad field (e.g. Archeology and History) earnings do not increase on average. The earnings results are mirrored when we consider instead the effects of field of study on skills sets required in subsequent jobs. When prospective students are on the margin between two broad fields, we find significant differences in the demanded skill sets, but when we consider those on the margin between two narrow fields within the same broad field, these effects disappear. To our knowledge, we are the first to compare the earnings effects of students on the margin between narrowly defined fields with those on the margin between two broadly defined fields. This is a useful exercise as it allows us to investigate the nature of comparative advantage in a larger portion of the applicant pool. Moreover, we are the first to show that the degree of similarity between preferred and next best fields has direct effects on the skill use in jobs for which students are subsequently hired. Our results suggest that different fields of study open doors to jobs that require different skill sets, but we are not able to say whether the effect of field of study is due to human capital accumulation or signaling.

Thus, Chapter 3 helps us understand some of the mechanisms behind why people end up in jobs requiring different task-specific skills. However , as shown in Chapter 1, even if women and men are employed in jobs requiring similar skills, women generally receive lower hourly wages than their male colleagues.

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Chapter 1

Gender Differences in Returns to Skills: Evidence from Matched Vacancy-Employer-Employee Data

Gender Differences in Returns to Skills

Evidence from Matched Vacancy-Employer-Employee Data

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Copenhagen Business School

August 2021

Abstract

Recently available data from online job vacancies have enabled analyses that move beyond across-occupation variation to also include within-occupation variation in workers' task-specific skills. However, analyses of job vacancy data are limited by the fact that information on the hired worker(s) is hidden. To overcome this issue, I develop a novel, pseudo-individual match between Danish job vacancy data and register data. With data on the hired worker(s) for each online job vacancy, I can test how the employment of skills and the returns to skills depend on the gender of the worker. I use the matched employer-employee-vacancy data to show that women face significantly lower returns to cognitive, character, customer service, financial, and specific computer skills when compared to men while controlling for both occupation and firm fixed effects.

JEL classifications: J16, J24, J31, J71

Keywords: returns to skills, tasks, wage differentials, gender pay gap.

*Department of Economics, Copenhagen Business School, mfj.eco@cbs.dk, +4538155620. Thanks to Fane Groes and Moira Daly for data access, advice, and support. Thanks to Herdis Steingrimsdottir for helpful discussions. Thanks to Oliver-Alexander Press, Tim Schurig, and Peter Sundquist for their research assistance with the job vacancy data. This paper is partly based on my master's thesis submitted as part of the 4+4 PhD-programme at Copenhagen Business School. This work was supported by the Novo Nordisk Foundation grant number NNF16OC0021056.

1. Introduction

Already in early work on gender inequalities in the labour market, both researchers and feminists focused on whether or not women and men received equal pay for equal work (e.g. Edgeworth, 1922; Fawcett, 1918). As a result, the ambition of securing equal pay for equal work for women and men also received political attention, e.g. in the US Equal Pay Act of 1963. Despite these efforts, a multitude of modern economic studies show that women continue to receive substantially lower hourly wages when compared to men, although some convergence between labour market outcomes of women and men has been observed internationally over the last few decades, both in terms of hours worked, earnings, and educational attainment (Blau and Kahn, 2017; Goldin, 2014; Lindley and Machin, 2012; Olivetti and Petrongolo, 2016).

Family formation and parenthood are emphasised as key drivers of the persistent gender inequalities in the labour market (e.g Kleven, Landais, and Søgaard, 2019). Mothers typically undertake more childcare than fathers, which affects labour market participation in the form of career interruptions (maternity leave or stay-at-home moms) and in the form of part-time or family friendly employment (e.g Bertrand, Goldin, and Katz, 2010; Gupta and Smith, 2002; Nielsen, Simonsen, and Verner, 2004; Joshi, Paci, and Waldfogel, 1999). Partly as a result of the gendered division of childcare, gender segregation into different types of jobs (e.g. occupations and industries) and into different employers (e.g. private vs. public) is also pervasive and plays a central role in explaining the persistent gender inequalities in pay (Blau and Kahn, 2017; Levanon and Grusky, 2016; Olivetti and Petrongolo, 2014; Jarman, Blackburn, and Racko, 2012; Nielsen et al., 2004; Card, Cardoso, and Kline, 2016).

For example, Card et al. (2016) show that women in Portugal are less likely to work in high paying firms, but even if they do, they are likely to receive a smaller share of the firm specific pay premiums from which their male colleagues benefit. Similarly, numerous studies have found that conditional on working in the same occupations and industries, women still receive lower wages than their male colleagues (Blau and Kahn, 2017; Goldin, 2014; Lindley, 2016). Depending on the definition of "equal work," however, these findings do not necessarily imply that women do not receive equal pay for equal work. It could be the case that women and men employ different task-specific skills, even within firms and occupations, and that these skills are renumerated differently. Task-specific skills refer to the type of skills that are associated with specific tasks, such as social skills, cognitive skills, and computer skills, but not education levels.

Exactly what individuals do at their jobs, however, has to a large degree remained a "black box" as data on the job-level composition of task-specific skills rarely are available. The few datasets that contain individual- or job-level information on task-specific skills are relatively small surveys, and thus, when using these data to estimate returns to skills, one cannot control for sorting into firms, either because data on firm affiliation is not available or because the samples are too small to include firm fixed effects in wage regressions (e.g. the OECD Survey of Adult Skills PIAAC, or the UK Skills Surveys as used by Lindley, 2012). At the same time, the US Equal Pay Act of 1963 defines equal work as requiring *"substantially equal skill ... within the same establishment"* (U.S. Equal Employment Opportunity Commission, 1997). Thus, if the aim is to estimate whether or not women and men face equal pay for equal work by the definition in the US Equal Pay Act, firm fixed effects should be included in wage regressions. Also from an economic perspective, when estimating gender differences in pay, it can be argued that one should control for sorting into firms by including firm fixed effects in wage regressions if workers face non-pecuniary firm-specific benefits or costs.

To be able to control for sorting into both firms and occupations when estimating gender differences in returns task-specific skills, I develop a matched vacancy-employer-employee dataset from Danish online job posts covering the period 2007-2017. Information on task-specific skills can be extracted from the text from each job post (using an approach similar to Deming and Kahn, 2018). Uniquely, the Danish job vacancy data can be matched with Danish register data at the firm*occupation*month-level. This exercise can only be undertaken because Danish register data include monthly information on employment, including earnings, occupation codes, and firm identifiers, for the universe of Danish employees. The resulting pseudo-individual match between vacancy data and register data make it possible to evaluate gender differences in returns to skills both *across* and *within* occupations and firms. Due to the match with Danish register data, I can control for factors that are usually highlighted as contributing to the gender pay gap in the literature, such as parental status and sorting into firms and occupations. By doing so, I return to the traditional question: Do women and men receive equal pay for equal work? Or in my terminology: Do women and men face equal returns to the same task-specific skills, e.g. social skills, cognitive skills, and computer skills?

When answering this question, I contribute to the literature as follows: 1) I provide a validation and operationalisation of skills data derived from job vacancies matched with register data, and I provide a description of their complementarities. To my knowledge, this is the first paper that matches such data at an individual level and at a large scale. 2) I show the advantages of individual-level matched vacancy-employer-employee data by estimating returns to skills and their heterogeneity across genders while controlling for firm and occupation FEs. 3) I provide individual-level tests of various hypotheses on returns to interactions of skills from the existing literature, including that on technological change.

I find that task-specific skills do not yield particularly high returns to men beyond what can be explained by occupation and firm fixed effects with the exception of cognitive and financial skills. However, I find that there is significant heterogeneity in returns to skills across genders, even when controlling for firm and occupation FEs along with a long list of other controls. With these FEs and controls in the model, returns to 5 out of 9 task-specific skills are significantly lower for women when compared to men. Importantly, the gender differences in returns to task-specific skills are pronounced for cognitive and specific computer skills; skills that have been emphasised as being technology-complementing in the existing literature. In contrast to Deming and Kahn (2018), I do not find any positive significant effects of the interaction between social and cognitive skills.

The paper is outlined as follows. In Section 2, I provide more details on the existing literature on task-specific skills and job vacancy data. In Section 3, I describe the Danish vacancy data and register data which are utilised in the analyses that follow. Furthermore, this section includes some details on the data pre-processing. Section 4 includes descriptive analyses of the data. In section 5, I present regression models and results, as well as robustness checks. Section 6 concludes.

2. Task-specific skills and job vacancy data

Since the seminal work of Autor, Levy, and Murnane (2003) emphasised the link between task-specific skills, technological change, and job polarisation, research on the demand for certain skills and returns to these skills has become increasingly prevalent. Interactions between certain task-specific skills, e.g. cognitive and computer skills, have been highlighted as complementing new technologies, and thus, the demand and returns to these skills should increase with technological change. Recently, Deming (2017) and Weinberger (2014) have emphasised the growing employment and wages in jobs requiring both social and cognitive skills, rather than cognitive skills alone. Although Black and Spitz-Oener (2010) and Cerina, Moro, and Rendall (2020) find that the job polarization patterns noted by Autor et al. (2003) are pronounced for women than for men, little research has looked into differences in skills and in returns to skills between women and men. An exception is Bacolod and Blum (2010) who show that women are particularly well-endowed with people skills and cognitive skills and that increasing returns to these two skills can explain up to 20 % of the decline in the gender pay gap. Similarly, Beaudry and Lewis (2014) find that gender pay gap narrowed with the adoption of PCs from 1980 to 2000, because women are well-endowed with cognitive skills, which complement PC adoption. However, studies on the interaction between technological change and gender typically use skills and task data at the occupation level, i.e. they do not observe within-occupation variation in skills. Before the availability of job vacancy data, researchers typically relied on skills and tasks data from relatively small surveys or from the DOT- and O*NET-databases, which were infrequently updated and provided job characteristics that only varied at the occupation level. Hence, gender differences in skills and in returns to skills could only be inferred from the fact the occupational composition of workers differs between women and men.

With the internet's omnipresence in the Global North, online posting of job vacancies is now an integrated part of firms' recruitment of new employees. The text of each job post is highly informative when studying modern labour markets: Typically, job posts state expected skills, education, and experience of potential applicants, as well as certain characteristics of the job itself, e.g. its occupation, industry, and region. Crucially, the text of digital job posts can easily be scraped from various sources on the web. Many of the studies that utilise job vacancy data also exploit that job vacancies typically include information on skills requirements, and importantly, the derived skill measures vary within occupations. However, these studies do not tend to point at gender differences in outcomes. For example, Hershbein and Kahn (2018) show that during the Great Recession, skill requirements in job posts increase more in areas that were hit harder by the recession. Modestino, Shoag, and Ballance (2016a,b) find a similar relationship between skill requirements and the availability of workers, i.e. that skill requirements increased during the recession and decreased again through the recovery. Cortes, Jaimovich, and Siu (2018) utilise new measures of tasks from job ads in a range of US newspapers from 1960 to 2000 together with DOT data. They find that when social skills become more important within an occupation, the occupation's female share of employment also increases. After merging their skills measure to a sample of US census data, they also indicate that returns to social skills have increased over time, which is consistent with Deming's (2017) findings. Bagger, Fontaine, Galenianos, and Trapeznikova (2021) match Danish job vacancy data from the period 2003-2009 with Danish register data to document the relationship between vacancy posting and a number of firm outcomes. For example, they show that vacancy posting is associated with increasing hiring rates at the firm level. However, they do not extract data on skills from the job posts.

Of the papers utilising job vacancy data, Deming and Kahn's (2018) is the one closest related to my analysis. They use Burning Glass Technologies' online job vacancy data from 2010 to 2015 to extract 10 general skill measures at the firm*occupation level. Next, they match these skill measures to data on individual firms and to wage data from metropolitan statistical areas (MSA). Thus, they can estimate the relationships between skills and wages, as well as between skills and firm performance. Deming and Kahn (2018) find that their skills measures generally correlate positively with both wages and firm performance. High paying and high performing firms require higher levels of social and cognitive skills. When a job requires both social and cognitive skills, they find a particularly high level of wages.

However, job vacancy data are typically constrained by the fact that information on the hired worker is hidden, including information on the worker's gender and earnings. Vacancy data is often matched with firm-level data, for example by using firm names in Deming and Kahn (2018), but matching at the individual level is impossible in settings where only datasets with subsets of workers are available. Although Deming and Kahn (2018) explore variation in returns to skills both across and within occupations, they cannot say whether or not their results hold at the individual level. This follows from the fact that they cannot match their skills and firm data with employees, but only with wage data at the MSA level. With my matched employer-employee-vacancy data, I can check if the findings from the existing literature hold at the individual level, and I can test for gender differences in returns to skills while controlling for firm and occupation FEs.

3. Data sources and pre-processing

The analysis that follows relies on two sources of data. Firstly, Statistics Denmark provides register data on employment, education, demographics, firm characteristics etc. Crucially, these registers include the entire population of both employees and firms in Denmark. Furthermore, it is possible to match the different registers at both the firm level and individual level. Most importantly, monthly employment data are available, and they include a firm identifier and an occupational code for each employment relation. Secondly, Danish online job vacancy data from 2007-2017 are supplied by the Danish consultancy firm, Højbjerre Brauer Schultz (HBS). These data also include a firm identifier and an occupational code for each job post as well as a posting date. Thus, it is possible to match data from the two sources using firm identifiers and occupational codes, and by exploiting the data's time dimension. In the following subsections, I separately describe the register and job vacancy data, and next, the data match.

3.1. Register data

Danish register data contain detailed monthly employment information for the entire Danish population of employees. Monthly wages, job start and end dates, monthly hours, a firm identifier, and an occupational code are provided for each monthly observation.[1] In

[1] The data provider, Statistics Denmark, uses 6-digit Danish versions of the International Labour Organization's ISCO88 and ISCO08 occupational codes, called DISCO88 and DISCO08 codes. The occupational codes have a break in 2009/2010 as Statistics Denmark move from DISCO88 to DISCO08 codes. In order to

what follows, I define a job spell as the period over which a worker remains within the same firm*occupation cell. Thus, a new job spell starts when a worker enters a new role in the same firm (new occupation code), or when a worker gets a job in another firm (new firm identifier). However, in order to avoid possible bias from firm specific human capital accumulation, I only keep new job spells of individuals that switch to a new firm. From this definition, I construct my main dataset as follows. First, I identify new jobs in the employment register, i.e. jobs where workers are registered with a new firm identifier in a given month.[2] I construct a sample of those new jobs with the first 12 months of observations in the employment register (or fewer, if the job spell ends before). Next, I aggregate to get the 12-months-averages of hourly wages, full-time equivalents, and other relevant variables. [3] Thus, this dataset contains all new jobs and information on the first 12 months of employment. I have access to the monthly data from January 2008 until June 2018, and since I need 12 months of observations, the latest job spells included start in July 2017. The constructed dataset yield information on workers only during their first year of employment in a certain job. I impose a number of restrictions on the sample, see Appendix B.1. To complement the employment data, I extract data on demographics, years of education, student status, employment experience etc. from other registers, which completes my register-based dataset. In the following subsection briefly outline the Danish job vacancy data before moving on to describe the match between the vacancy and register data.

3.2. Job vacancy data

The vacancy data is supplied by HBS, who also have provided the initial cleaning of the data. They believe that their data contains the near universe of publicly accessible Danish online job posts from 2007 to 2017.[4,5] They remove duplicates and clean the data before

get consistent codes occupational codes over time, I convert them into 228 consistent occupational groups, which gives a level of detail somewhere between 3- and 4-digit ISCO-codes (see Appendix B.3.1 and Online Appendix D.2 for more details on occupational codes).

[2]"New" in the sense that the worker was not observed in same firm in the previous month. Furthermore, I detect gaps between spells of work in the same firm*occupation cell. If the gap between two spells is less than 6 months, I do not code reoccurring work in a firm*occupation cell as a new job, but include both of them in the same job. I also correct for changing firms identifiers.

[3]A full-time job is defined as 1923.96 hours per year by Statistics Denmark. Hence, full-time equivalents = total number of hours per year / 1923.96 (see https://www.dst.dk/da/Statistik/dokumentation/Times/moduldata-for-arbejdsmarked/fuldtid). This measure of full-time equivalents will be used as weights in the analyses that follows.

[4]For more details, see: https://hbseconomics.com/wp-content/uploads/2017/09/Eftersp%C3%B8rgslen-efter-sproglige-kompetencer.pdf

[5]Due to data collection issues at the data provider, keywords information from the latter half of October, all of November and December 2011 as well as April 2017 are not available, although metadata for these months is available. This represents a very small fraction (2.1%) of the total number of job vacancies.

machine reading the job posts. HBS extracts the date on which a given job vacancy was posted online, a firm ID, and an occupation code.[6] If the firm identifier is not listed directly in the job post, HBS imputes it from publicly accessible registers using the firm name listed in the job post. Importantly, HBS also extract keywords from the raw text in the job post. In many ways, the resulting data is similar to the US job vacancy data supplied by Burning Glass Technologies. In order to be able to match with the register datasets, the vacancy data sample is restricted to include job posts with non-missing firm identifiers and occupational codes only.

When extracting skill requirements from the job vacancy data, I initially follow the method of Deming and Kahn (2018) and map a selection of keywords into skills categories. For example, the keyword "teamwork" is indicative of a job requiring social skills. The nine skill categories as well as the categories' mapping to a selection of keywords can be found in Table 1. Unlike Deming and Kahn (2018) who only map a selection of keywords into skill categories, I assign all keywords either a skill category or a noise tag. This is done as follows: 1) The most frequent keywords (approx. 2000) are assigned a skill category or noise tag manually. These words amount to the vast majority of keyword-observations. 2) Using online dictionary APIs each word's synonyms are obtained.[7] Each word's synonyms are assigned the same category as the word itself. 3) Using online dictionary APIs each word's definition is obtained. 4) Using the definition of the words, the remaining non-categorised words are assigned a category using machine-learning methods (see Appendix B.2 for more details). After these steps, all keywords are assigned either a skill category or a noise tag. The categorised keywords undergo further pre-processing, but only after the vacancy and register data are matched. The matching procedure is described in Section 3.3.

3.3. Data match

As unique firm identifiers and occupational codes are included in both the register data and job vacancy data, the two datasets can be matched along those dimensions.[8] Furthermore, I exploit the time dimension of the data.

In order to match the Danish register data with the job vacancy data, I first assume that vacancies are posted in same month as the vacancy is filled or maximum four months

[6] HBS extracts 6-digit DISCO-codes, which I also convert to the consistent 228 occupational groups as described above, see Appendix B.3 for further details.

[7] Many thanks to the Society for Danish Language and Literature for providing access to these ressources.

[8] For the match on DISCO-codes to be reliable, the codes must be consistently coded across the register data and job vacancy data. In Appendix B.3, I briefly outline how DISCO-codes are coded in the two data sources.

Table 1: Skills categories and examples of their corresponding keywords

Skill	Examples of keywords
Cognitive	problem solving, research, analytical, critical thinking, math, statistics, systematic
Social	communication, teamwork, collaboration, negotiation, presentation, social, extrovert, network, relations
Character	organised, detail-orientated, multi-tasking, time management, meeting deadlines, energetic, busy, engaged, overview
Writing/ language	writing, language, English, German, Swedish, Norwegian
Customer Service	customer, sales, client, patient
Management	management, supervisory, leadership, mentoring, staff, control, planning, implementing
Financial	budgeting, accounting, finance, cost, tender/bids
Computer (general)	computer, spreadsheets, common software, (e.g. Microsoft Excel, PowerPoint)
Computer (specific)	programming, java, python, computer science

Note: Categories and their corresponding keywords are based on Deming and Kahn (2018), Table 1.

prior.[9] For example, if a job spell starts in May, the corresponding vacancy would be posted any time from the beginning of January to the end of May in the same year. With this assumption, I use the job vacancy data to construct a rolling sum of job vacancies for each firm*occupation cell. If a new job spell appears in the employment register, I match it with job vacancies summed over the relevant 5 months. For example, if a firm posts two job vacancies in the same firm*occupation cell, one in January and one in February, a job spell starting in January will only be matched with first vacancy, whereas a job spell starting in February will be matched with both vacancies. Because only 4 months of job vacancy data before job start is needed, my matched data is only limited by the availability of the monthly employment register data, and thus, job spells commencing any time during the period January 2008 to July 2017 are included the final dataset [10]. This matching strategy gives a pseudo-individual-level match between new employees and their corresponding job post. Table 2 shows match rates aggregated to the yearly level for the dataset using the 228 occupational groups.

[9] In job posts, job start dates are often reported as an interval or not reported at all, but the posting date is accurately measured. Considering both the time between the posting date and the application deadline as well as the time from the application deadline to job start, a 5 months rolling window should capture most matches.

[10] Job spells commencing on 1 January 2008 are excluded, as I cannot check if a person was employed in the same firm*occupation cell in December 2007. However, spells commencing 2-31 January 2008 remain included

Table 2: Match rates

Year	New jobs	Matched new jobs	% new jobs matched	Job posts	Matched job posts	% job posts matched
2009	410 850	114 855	28.0	101 241	46 709	46.0
2010	430 916	105 559	24.5	90 232	41 856	46.4
2011	413 976	93 868	22.7	74 623	32 658	43.7
2012	397 809	101 757	25.6	101 114	49 940	49.4
2013	411 235	119 602	29.1	109 475	57 242	52.2
2014	422 277	117 994	27.8	115 159	59 130	51.2
2015	462 099	124 529	26.8	126 497	67 684	53.5
2016	497 670	133 270	26.7	130 171	69 465	53.4
Total	3 446 832	911 434	26.3	848 512	424 684	50.0

Note: As job spells commencing on 1 January 2008 or after July 2017 are excluded, the counts and match rates are not comparable to those reported here and are therefore excluded.

It is not surprising that only 26.3 % of new jobs from the employment register can be matched with a job post. Many of the new jobs are likely to be informal hires (the job is not publicly posted), or hires in a job that does not correspond with the job title in the job posts. This will, of course, result in an occupational mismatch. However, 50 % of job posts are matched to new jobs in employment register. This is a very high match rate when compared to, for example, Kettemann, Mueller, and Zweimüller (2018) who undertakes a similar exercise using Austrian data.

It is necessary to assume that new employees' skills levels are reflected in the job posts in their firm*occupation cell just around the start of their job spell. Furthermore, focusing on the first 12 months of wages in a job spell should limit bias from additional human capital accumulation in the firm*occupation cell. Since only few workers tend to start in the same firm*occupation cell in a given month, the level of aggregation is low. However, aggregating the job vacancy/skills data at the firm*occupation*start-month levels is a potential drawback of my data: I cannot separate women and men in the job vacancy data, and thus, I assume that everyone has the same skills at the firm*occupation*start-month level. In other words, the same skills are assigned to women and men in the same cells; I do not observe any gender variation in skills at the firm*occupation*start-month levels. If women and men tend to work in the same cells, this would restrict my analysis. However, as pointed out above, women and men tend to work in different occupations in the Danish labour market, i.e. high levels of occupational segregation are observed (Jarman et al., 2012). Due to the smaller cell sizes, gender segregation is likely to be even more pronounced at the firm*occupation*start-month levels. To explore gender segregation at these levels, I first calculate the female share of hours in each firm*occupation*start-month cell. Next, I graph the cumulative distribution of hours worked for women and men respectively on the cell's female share of hours. Figure 1 shows that women and men rarely get employed at the same time in the same firm*occupation*start-month cell. So, despite the fact that I cannot observe any gender variation in skills within firm*occupation*start-month cells, I still observe considerable gender variation in skills across these cells. Furthermore, I do observe gender differences in wages and in all other characteristics within a cell; these variables vary at the individual level.

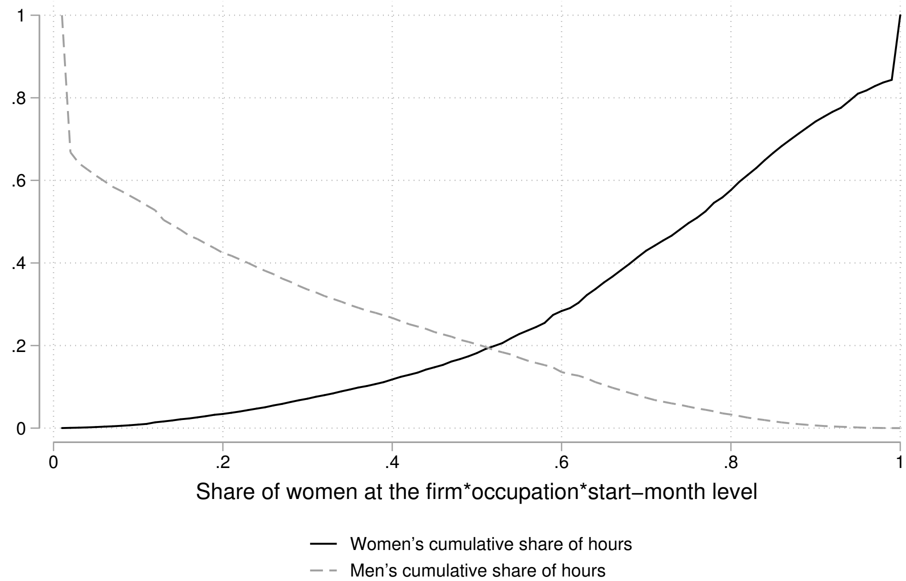
An average match rate of 26.3 % of employment register jobs can be problematic if the matched jobs spell are not representative of the population of new job spells. To check whether or not all occupations and industries are represented in the matched data, I compare the occupational and industrial distribution in the complete employment register data and in the matched subsample. Figures showing the distributions are included in Appendix B.3. The significant overrepresentation of public employees in the matched sample follows from the fact that all permanent public sector jobs by law must be publicly advertised. Thus, public sector job vacancies are also overrepresented in the vacancy data. Importantly, all industries are represented in the matched data. The data analyses includes a control variable indicating whether a job is in the public or private sector. Figure B.2 shows the representation of the 228 occupational groups in the matched data. If a data point lies to the left of the 45 degree line, it indicates that an occupation is underrepresented in the matched data, and if it lies to the right of the 45 degree line, it is overrepresented. Thus, the figure shows that smaller occupations generally are underrepresented and that larger occupations generally are overrepresented in the matched sample. Also for this reason, occupation fixed effects are included in the analyses that follows.

3.4. Skill measures

After matching job spells and job posts, the categorised keywords are revisited. If a job spell is matched with more than one job post, keywords from all the relevant job posts are aggregated. Next, the number of (aggregated) keywords belonging to the nine skill categories as well as noise words are counted for each job spell. Using these counts, the fraction of keywords indicating a certain skill are calculated for each job spell. For example, a job spell may be matched with one job post, which contains 4 % "character" words. Or a job spell may be matched with two job posts, which in total contain 8 % "character" words. However, these skill fractions are hard to interpret, and particularly in regressions analyses.

A more easily interpretable alternative would be to classify each job spell as either "character" or "not character", i.e. to create an indicator variable for each skill category. Indicator variables are easy to interpret, particularly in regression analyses with interaction terms.

Figure 1: Cumulative distribution of hours worked



Source: BFL 2008-2017, excluding observations with missing CVR- or DISCO-codes. *Notes:* Cumulative distribution of hours worked by women and men on the share of women in firm*occupation*start-month cells. Notice that hours worked by men is concentrated in cells with a low share of women and vice versa.

However, almost all job posts include one or more “character” keywords. Hence, there would be little variation in the skill measure if all job posts that include a single “character” keyword were classified as “character” rather than “not character”. At the same time, other skill keywords are relatively rare, e.g. keywords indicating “computer (specific)” skills. Therefore, a simple data-driven approach is used to classify each job post as either “character” or “not character”, and analogously for the remaining eight skills.

First, I consider the non-zero fractions of “charater” keywords for each job spell: At which point in distribution does the fraction of “character” keywords predict anything about wage levels? In order to determine this, I do the following: 1) Calculate each percentile in the distribution of non-zero “character” fractions. 2) Construct percentile-dummies indicating whether or not a job spell's “character” fraction is above or below each percentile. 3) Separately regress ln(hourly wages) on each of the percentile-dummies and a constant, but no control variables. 4) Choose the percentile-dummy which yields the most predictive power (the highest r^2). 5) Classify each job spell as “character” if the fraction of “character” keywords equals or exceeds that of the percentile determined by the percentile-dummy. This exercise is repeated for the remaining eight skill measures, giving nine binary skill measures.

As an alternative to the binary skill categorisation, I also develop continuous skill measures by standardising the skill fractions separately for each of the nine skills. Since keywords indicative of some skills are much more common than others, standardisation eases the interpretation and comparison of the effects of different skills on wages. The results using the binary skill indicators are reported in Section 5. In addition, all results using the continuous skills measures are reported in Appendix C.

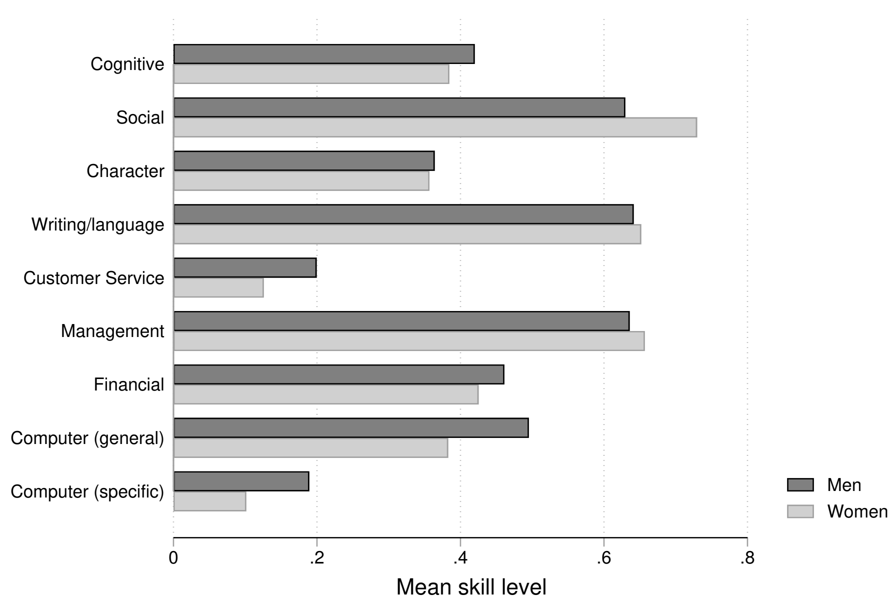
To confirm that the skill measures derived from job posts in fact reflect skill use in the corresponding jobs, I check their correlation with occupation level skill use data from PIAAC, 2011-2012. Generally, the skill measures derived from the job postings data correlate with the relevant measures from PIAAC as one would expect. More details on this validation exercise are available in Appendix D.1.

4. Descriptive statistics

4.1. Gender differences in skills

The vacancy-register data match enables analyses of skills together with the rich sets of variables provided by the Danish registers. In the context of this paper, an essential piece of information to exploit is the gender of workers. Figure 2 maps the average of jobs categorised as requiring each skill by the gender of the hired worker.

Figure 2: Mean skill levels by gender



Notes: Observations weighted by full-time equivalents. For version using continuous, standardised skill measures, see Figure C.1.

Figure 2 shows that women are overrepresented in jobs that are categorised as requiring "social", "writing/language", and "management" skills when compared to men. The opposite is the case for the remaining six skills. Despite some small gender differences, jobs are largely similarly categorised for women and men. The largest relative gender difference observed is in “computer (specific)” skills, where men are more likely to be employed in a firm*occupation*start-month cell that is categorised as requiring “computer (specific)” skills.

4.2. Correlations

Table 3 shows simple correlation coefficients between the skill measures, wages, and gender are important to consider for at least two reasons. Firstly, the skill measures should not be too highly correlated, as that could result in multicollinearity issues in regressions. Second, the correlations themselves may give us some idea of whether or not the skill measures make sense to include in wage regressions. For example, one would expect that high wage workers tend to work in cells with more skill requirements, i.e. that skills measures and wages are positively correlated.

Table 3 includes correlations between ln(hourly wages), a female dummy variable (=1 for women), and finally, all nine skill measures. All skill measures are positively correlated with wages, with the exception of “character” and “customer service” skills. Most skills are positively correlated with each other, although there are a couple of exceptions: “character” and “customer service” are negatively correlated with a few skills. This is an early indication of “character” and “customer service” skills being common in low wage jobs and in jobs with few other skills. Importantly, no skill measures are correlated to a degree that should cause problems of multicollinearity in regression models.

4.3. Variance

Although the correlation coefficients indicate that my skill measures are not correlated to a degree that would cause multicollinearity issues in regression models, the variance of the skill measures should also be explored. Before moving on to regression analyses it must be established that skill requirements cannot be entirely predicted by potential covariates. If so, the skill measures would not add any explanatory to a regression model. Thus, I regress the nine skill measures on various sets of control variables, and plot the adjusted R^2 from each regression. Figure 3 shows that between approx. 35 % and 62 % of the variance in the skill measures can be explained by the most extensive set of covariates. Notice that occupation and firm fixed-effects explain particularly large fractions of the variance in skill requirements. Still, a significant share of the variance in skill demands cannot be explained by even the most extensive set of covariates. Thus, the skill measures appear as suitable regressors in regressions in which similar sets of covariates are included, and the skill measures yield

Table 3: Correlation tables of skills, wages and gender

	ln(wage)	Female	Cognitive	Social	Character	Writing/language	Customer Service	Management	Financial	Computer (general)	Computer (specific)
ln(wage)	1										
Female	-0.150***	1									
Cognitive	0.276***	-0.0356***	1								
Social	0.0933***	0.106***	0.0953***	1							
Character	-0.150***	-0.00747***	-0.194***	-0.0248***	1						
Writing/language	0.108***	0.0108***	0.179***	0.110***	-0.112***	1					
Customer Service	-0.114***	-0.0998***	-0.114***	-0.115***	0.177***	-0.124***	1				
Management	0.148***	0.0217***	0.243***	0.203***	-0.156***	0.175***	-0.134***	1			
Financial	0.122***	-0.0357***	0.224***	0.0387***	-0.123***	0.155***	-0.0323***	0.275***	1		
Computer (general)	0.178***	-0.112***	0.289***	0.0252***	-0.170***	0.197***	-0.0548***	0.183***	0.239***	1	
Computer (specific)	0.172***	-0.126***	0.167***	-0.0107***	-0.0820***	0.0904***	-0.00112	0.0832***	0.0987***	0.218***	1

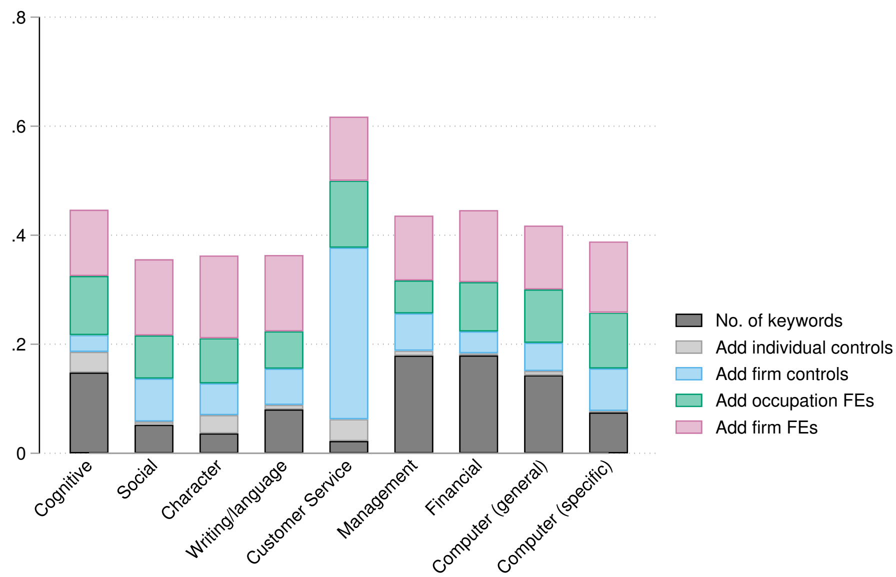
Notes: Observations weighted by full-time equivalents.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

For version using continuous, standardised skill measures, see Table C.1

explanatory power beyond that of standard labour market data (cf. Deming and Kahn, 2018).

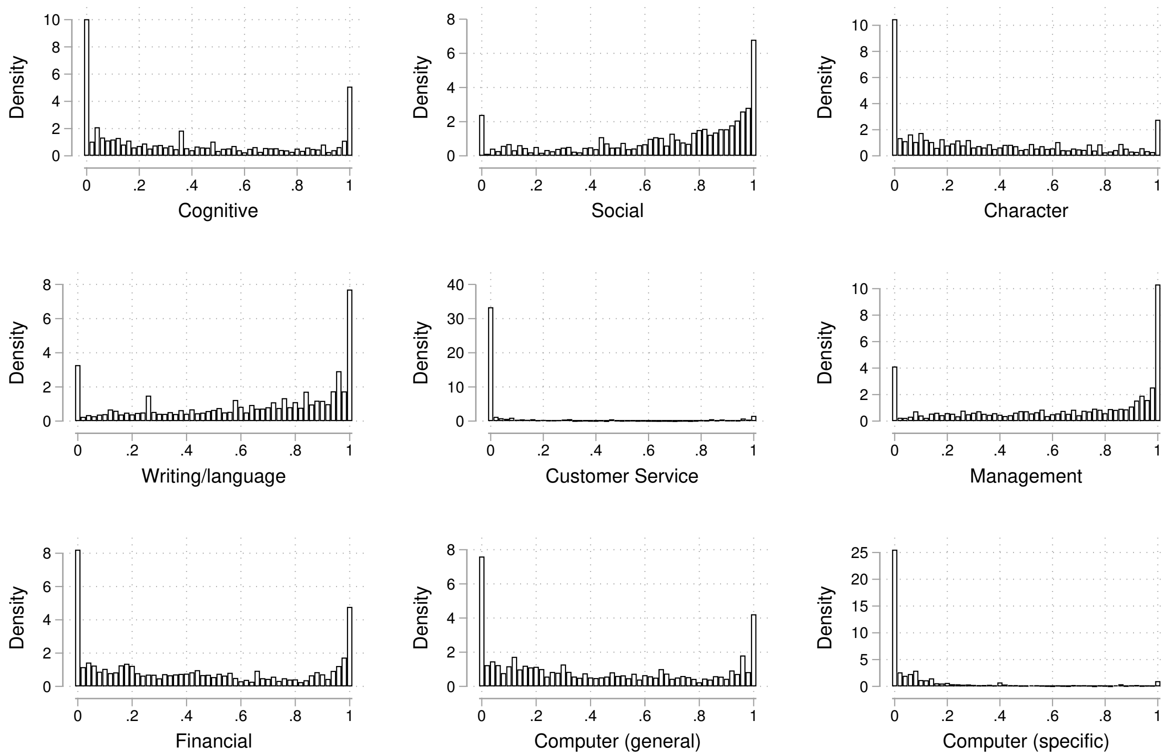
Figure 3: Adjusted R^2 from regressions of skills level on various controls



Notes: Skills are regressed on various sets of controls. Individual controls and number of keywords: *parent dummy, parent*female interaction, age, age*^{2}*, years of experience, years of education, immigrant dummy, marriage dummy, part-time dummy, year FEs, start-month FEs, number of keywords.* Firm controls: *1-letter industry dummies, firm location, number of employees, a private sector dummy.* Occupation fixed effects: *See Appendix B.3.1 for details.* Firm fixed effects: *4682 firms in total.* Observations weighted by full-time equivalents. For version using continuous, standardised skill measures, see Figure C.2.

The next section introduces the regression analyses used to estimate gender differences in returns to skills. An important aspect of the analyses is the inclusion of occupation and firm fixed effects. Hence, variation in the skills measures at these levels is crucial. Figure 4 shows the distributions of the mean skill levels within each firm*occupation cell. One possible limitation of the data could be that all job posts within the same firm*occupation cell would include the same keywords, and thus, consistently be coded as requiring the same skills. However, from Figure 4 it is evident that even at this very detailed level, the mean skill levels vary, and far from all mean skill levels are exactly equal to zero or one. A job is most rarely categorised as requiring "Customer Service" and "Computer (specfic)" skills. Many firm*occupation cells do not include jobs with these requirements, and consequently, the mean in Figure 4 is equal to zero for those jobs. Importantly, Figure 4 reveals that firm*occupation fixed effects do not sufficiently account for the variation in skill requirements across jobs.

Figure 4: Distributions of mean skill requirements within firm*occupation cells



Note: For version using continuous, standardised skill measures, see Figure C.3.

5. Regression analyses

In this section, I first outline the regression models used to estimate gender differences in returns to skills. Next, results are presented, and finally, robustness checks are reported.

5.1. Models

Regressing hourly wages on skills and their gender interactions will indicate whether women and men with same skill requirements also receive the same wage. I regress ln(hourly wages) on skills and female*skills interactions with extensive sets of control variables and fixed effects. Before writing out the relevant regression models, I outline the four sets of control variables and fixed effects that are used:

1. Individual controls and number of keywords: *parent dummy, parent*female interaction, age, age*^{2}*, years of experience, years of education, immigrant dummy, marriage dummy, part-time dummy, year FEs, start-month FEs, number of keywords*

2. Firm controls: *1-letter industry dummies, firm location, number of employees, a private sector dummy*
3. Occupation fixed effects (see Appendix B.3.1 for details)
4. Firm fixed effects

The parent dummy equals one if an individual has a child less than 18 years old. The parent*female interaction term is included to control for the gender specific effects of parenthood (cf. Kleven et al., 2019), which may otherwise affect the estimates of gender differences in returns to skills. Controls are additively included in the regression models, so it suffices to write out the full model including all controls and fixed effects:

$$w_{iofym} = \beta_0 + I_{iym}\beta_1 + F_{fy}\beta_2 + S_{ofym}\gamma_1 + S_{ofym} \times g_i \times \gamma_2 + \lambda_o + \phi_f + \theta_y + \delta_m + \varepsilon_{iofym}$$

Where the subscript i indicates variation at the individual level, f at the firm level, o at the occupation level, m at the *start*-month level, and y at the year level. w_{iofym} is ln(hourly wage). I_{iym} is a matrix of individual start-month-varying characteristics and includes a female dummy, F_{fy} a matrix of firm year-varying characteristics, S_{ofym} is a matrix of the nine skill measures that vary at the firm*occupation*year*start-month-level. g_i is a female dummy variable, which equals 1 for women only. λ_o are occupation FEs, ϕ_f firm FEs. Finally, δ_m are start-month FEs and θ_y are year FEs.[11] The vector γ_1 gives the coefficients on the nine skill measures for men and $\gamma_1 + \gamma_2$ for women, i.e. γ_2 is the gender differences in the skill measures' coefficients. Standard errors are clustered at the firm*occupation*start-month level.[12]

5.2. Identification

The full specification with both occupation and firm FEs targets the issue that workers with certain skill compositions may sort into high/low paying occupations and firms. Note that including both occupation and firm FEs is not analogue to including firm*occupation FEs, and thus, variation specific to the firm*occupation interactions remains. The estimated coefficients can be interpreted as *within*-firm*occupation returns to skills and *within*-firm*occupation gender differences in returns.

[11] Note that the start-month FEs and year FEs are *not* interacted, and thus, they are not year*start-month FEs.

[12] As the nine skill measures only vary at the firm*occupation*start-month levels, the errors ε_{iofym} are correlated within these cells. Thus, I follow the approach taken by Hersch (1998) and cluster my standard errors at these levels. Such an approach is also recommended by Cameron and Miller (2015). The nine skill measures are perfectly correlated within clusters (they do not vary within), and thus, applying cluster-robust standard errors significantly inflate the estimated errors.

Although the full specification with various controls, occupation, and firm FEs may identify returns to skills, omitted variable/ability bias at the individual level should also be considered. However, not many individuals start more than one job spell in a new firm in the relatively short sample period. Furthermore, there is little variation in skills *within* individuals that do (again due to the short sample period), and thus, a specification including individual FEs is not feasible, although estimates from such a specification would warrant a more causal interpretation.

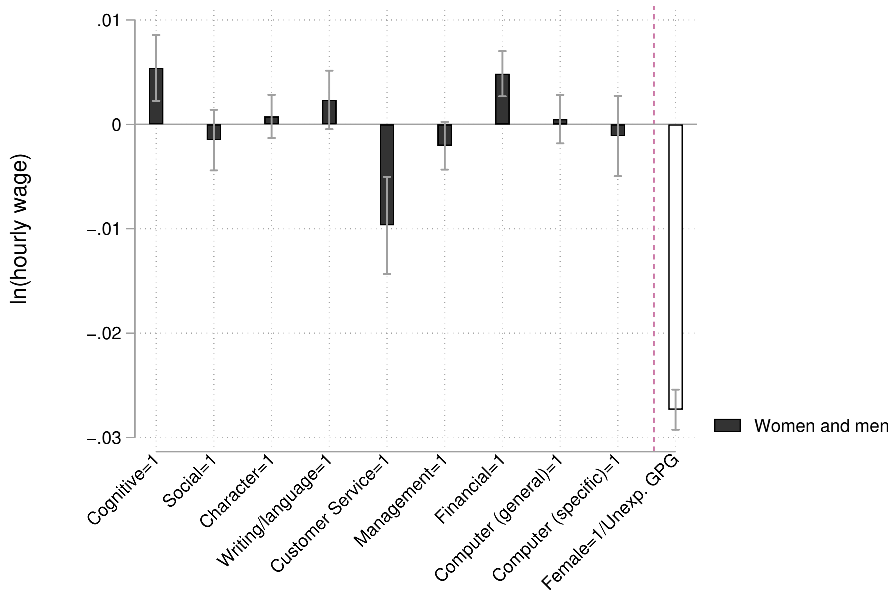
5.3. Results

First, estimation results from the linear regression model outlined above are reported, but *excluding* the gender interactions with the nine skill measures. These results are graphically presented in Figure 5 (see Table A.1 in Appendix A for point estimates and for results with varying sets of controls/FEs). The estimates show that when both occupation and firm FEs are included in the model, the coefficients on the skill measures tend to be insignificant. Exceptions are jobs categorised as requiring "customer service" skills, which are associated with lower wages. In contrast, jobs categorised as requiring "cognitive" or "financial" are associated with higher wages. Although the effects are significant, they are rather small. Thus, one could jump to the premature conclusion that task-specific skills generally do not matter much for wage formation beyond what can be explained by occupation and firm FEs.

However, after including gender interactions with the nine skills measures in the model, a different picture emerges. The results are presented in Figure 6 (see Table A.2 in Appendix A for point estimates and for results with varying sets of controls/FEs). For the model including both occupation and firm FEs, consider the coefficients on the skill measures that are not interacted with the female dummy, γ_1 (dark grey bars in Figure 6). These coefficients can be interpreted as *within*-firm*occupation returns to skills for men. The coefficients on the "cognitive", "characer" and "financial" skill measures are positive and significant even after introducing occupation and firm FEs. However, the coefficients on the "social" and "management" measures are negative and marginally significant, but also small.

In the model including both occupation and firm FEs, the coefficients on the female*skill interactions, γ_2, can be interpreted as gender differences in returns *within*-firm*occupation cells (black bars in Figure 6). Notice the coefficient on the interaction terms with the "cognitive", "character", "customer service", "financial", and "computer(specific)" all are negative and highly significant. Thus, for five out of nine skill measures, women face lower returns. The interaction term with the "social" skill measure is positive and significant at the 0.01-level. The interaction term with the "management" skill measure is also positive,

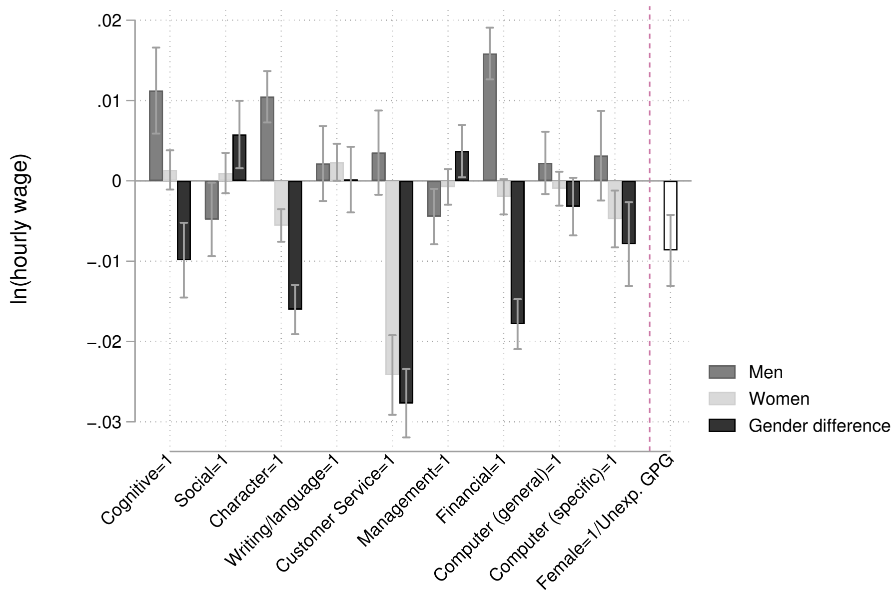
Figure 5: Coefficients on skills *without* gender interaction



Notes: Individual controls and number of keywords: *parent dummy, parent*female interaction, age, age^2, years of experience, years of education, immigrant dummy, marriage dummy, part-time dummy, year FEs, start-month FEs, number of keywords.* Firm controls: *1-letter industry dummies, firm location, number of employees, a private sector dummy.* Occupation fixed effects: *See Appendix B.3.1 for details.* Firm fixed effects: *4682 firms in total.* Observations weighted by full-time equivalents. Standard errors clustered at the firm*occupation*start-month level, 95 % confidence intervals indicated. See Column (4) in Table A.1 in Appendix A for point estimates. For version using continuous, standardised skill measures, see Figure C.4.

but the coefficient is small and only marginally significant. These estimates are the main take away from this paper. If the gender dimension of returns to skills was ignored, I could have concluded that skill generally do not yield returns beyond what can be explained by occupation and firm FEs. Instead, this specification sets the stage for the conclusion that men face positive returns to a number of skills, even within firm*occupation cells, and that women face lower returns than men to most skills, and women's returns to skills $(\gamma_1 + \gamma_2)$ are generally near zero (light grey bars in Figure 6). However, before jumping to this conclusion, a couple of robustness checks are considered.

Figure 6: Coefficients on skills for women and men, and their gender difference



Notes: Individual controls and number of keywords: *parent dummy, parent*female interaction, age, age^2, years of experience, years of education, immigrant dummy, marriage dummy, part-time dummy, year FEs, start-month FEs, number of keywords.* Firm controls: *1-letter industry dummies, firm location, number of employees, a private sector dummy.* Occupation fixed effects: *See Appendix B.3.1 for details.* Firm fixed effects: *4682 firms in total.* Observations weighted by full-time equivalents. Standard errors clustered at the firm*occupation*start-month level, 95 % confidence intervals indicated. See Column (4) in Table A.2 in Appendix A for point estimates. For version using continuous, standardised skill measures, see Figure C.5.

5.4. Robustness

A couple of robustness checks are performed. First, the full specification with both occupation and firm FEs is re-estimated for a number of subpopulations in order to check whether or not the results from the previous section are driven by a certain group of workers. Next, potential interactions between skill measures are explored. In the existing literature, computer and cognitive skills have been highlighted as complementing technology and technological change, and thus, yielding positive returns. However, the results from the previous section indicate that this is only the case for men, since cognitive skills are not associated with higher wages for women. Social skills have also been emphasised as complementing technological change, but together with cognitive skills (Deming, 2017; Deming and Kahn, 2018). Lastly, the results using continuous, standardised skill measures are discussed and compared to the results using binary skill measures.

5.4.1. Subpopulations

The first robustness check focuses on the full specification, which include all occupation and firm FEs as well as various other controls. The model is estimated again on the following subpopulations:

a. Professionals [13]
b. Workers in large firms (with 100 or more employees)
c. Workers in small firms (with fewer than 100 employees)
d. Private sector workers
e. Full-time workers
f. Workers that remain employed for at least 12 months after commencing a job spell

Results for each subpopulation are reported in Table 4. The results from the entire sample generally hold for all the selected subpopulations. However, the results for relatively small sample of job spells of workers at smaller firms differ to some degree. For example, the coefficient on "character" skill measure is insignificant, and this is also the case for the coefficient on its interaction term with the female dummy. The number of job spells per firms is naturally lower for small firms, and thus, the firm FEs may account for more of the variation in wages at these firms. For the comparison with Deming and Kahn (2018), the subsample of professionals is particularly important. The results generally hold for professionals, although the coefficient on "computer (general)" skills measure is positive and significant, and the coeffecients for its interaction term between with the female dummy is negative and significant. On the other hand, the coefficient on the "character" skill measure is insignificant for the subsample of professionals. For the subsample of employees in the private sector, the coefficients on the "social" and "management" skills measures are now positive for men, although insignificant. The coefficients on the interactions between the female dummy and the skills measures also differ slightly. For private sector employees, the coefficients on the interaction terms with the "cognitive", "social", and "computer (specific)" skill measures are insignificant, whereas the coefficient on "management" skills is negative and significant.

5.4.2. Interactions

In the existing literature, interactions between certain skills are often emphasised in the context of technological change. Thus, four sets of interaction terms are explored. First, an interaction term between the "social" and "cognitive" skill measures is included. This interaction term is of particular interest after the recent work by Deming (2017), and Deming and Kahn (2018). Next, interaction terms between "cognitive" and "computer (general)", as well as "cognitive" and "computer (specific)" are included respectively. Lastly, both of the

[13] Here, professionals are crudely defined as workers with more than 16 years of education.

Table 4: ln(hourly wage) regressed on skills with gender interaction for subpopulations

	Dependent variable: ln(hourly wages)						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	All	Professionals	Large firms	Small firms	Private sector	Full-time	Whole year
Female=1	-0.00866***	0.0180***	-0.00665**	-0.0243***	-0.0339***	-0.0154***	-0.00919***
	[0.00225]	[0.00307]	[0.00243]	[0.00489]	[0.00305]	[0.00272]	[0.00232]
Cognitive=1	0.0112***	0.00873***	0.0108***	0.0132**	0.00786**	0.00999**	0.00814***
	[0.00273]	[0.00210]	[0.00289]	[0.00423]	[0.00282]	[0.00310]	[0.00207]
Social=1	-0.00480*	-0.00843***	-0.00543*	0.00328	0.000142	-0.00343	-0.00649***
	[0.00234]	[0.00220]	[0.00249]	[0.00359]	[0.00225]	[0.00270]	[0.00194]
Character=1	0.0105***	-0.000340	0.0120***	-0.00500	0.00385	0.00985***	0.0102***
	[0.00163]	[0.00215]	[0.00173]	[0.00361]	[0.00243]	[0.00183]	[0.00178]
Writing/language=1	0.00216	0.00219	0.00254	-0.00103	0.00397	0.00247	0.00762***
	[0.00238]	[0.00231]	[0.00252]	[0.00387]	[0.00267]	[0.00270]	[0.00202]
Customer Service=1	0.00351	0.00995	0.00347	0.00311	-0.00665*	0.00600*	0.00443
	[0.00268]	[0.00559]	[0.00295]	[0.00412]	[0.00271]	[0.00303]	[0.00295]
Management=1	-0.00445*	0.00254	-0.00384*	-0.000546	0.000269	-0.00663***	-0.00405*
	[0.00176]	[0.00229]	[0.00184]	[0.00429]	[0.00262]	[0.00199]	[0.00192]
Financial=1	0.0159***	0.0166***	0.0158***	0.0276***	0.0214***	0.0186***	0.0165***
	[0.00164]	[0.00195]	[0.00171]	[0.00458]	[0.00248]	[0.00178]	[0.00175]
Computer (general)=1	0.00223	0.00719***	0.00251	0.00414	-0.00154	0.00286	0.00158
	[0.00198]	[0.00209]	[0.00206]	[0.00485]	[0.00293]	[0.00213]	[0.00195]
Computer (specific)=1	0.00313	0.00312	0.00273	0.00849	-0.00218	0.00297	0.00219
	[0.00285]	[0.00251]	[0.00302]	[0.00552]	[0.00307]	[0.00320]	[0.00241]
Female=1 × Cognitive=1	-0.00987***	-0.00865***	-0.00954***	-0.0135**	-0.00513	-0.00566*	-0.00752***
	[0.00237]	[0.00202]	[0.00251]	[0.00497]	[0.00267]	[0.00267]	[0.00192]
Female=1 × Social=1	0.00577**	0.00768***	0.00603**	0.000717	-0.00197	0.00760**	0.00754***
	[0.00214]	[0.00215]	[0.00229]	[0.00412]	[0.00233]	[0.00244]	[0.00195]
Female=1 × Character=1	-0.0160***	-0.00946***	-0.0172***	-0.00227	-0.00702**	-0.0178***	-0.0144***
	[0.00157]	[0.00225]	[0.00166]	[0.00422]	[0.00233]	[0.00184]	[0.00174]
Female=1 × Writing/language=1	0.000152	-0.00405	-0.000409	0.00110	0.00815**	0.000318	-0.00365
	[0.00208]	[0.00221]	[0.00221]	[0.00438]	[0.00257]	[0.00240]	[0.00187]
Female=1 × Customer Service=1	-0.0277***	-0.0499***	-0.0282***	-0.0176***	-0.00754**	-0.0408***	-0.0282***
	[0.00217]	[0.00486]	[0.00233]	[0.00494]	[0.00254]	[0.00251]	[0.00242]
Female=1 × Management=1	0.00370*	-0.00266	0.00306	-0.00602	-0.00842***	0.00617**	0.00378*
	[0.00166]	[0.00225]	[0.00175]	[0.00462]	[0.00250]	[0.00196]	[0.00182]
Female=1 × Financial=1	-0.0178***	-0.0202***	-0.0176***	-0.0242***	-0.0214***	-0.0200***	-0.0198***
	[0.00159]	[0.00192]	[0.00165]	[0.00551]	[0.00262]	[0.00175]	[0.00172]
Female=1 × Computer (general)=1	-0.00321	-0.00741***	-0.00322	-0.0152**	-0.00254	-0.00312	-0.00317
	[0.00183]	[0.00204]	[0.00191]	[0.00542]	[0.00265]	[0.00201]	[0.00187]
Female=1 × Computer (specific)=1	-0.00788**	-0.00579*	-0.00693*	-0.0124	-0.00489	-0.00809**	-0.00671**
	[0.00266]	[0.00246]	[0.00282]	[0.00650]	[0.00316]	[0.00302]	[0.00243]
Parent=1	0.0527***	0.0531***	0.0536***	0.0399***	0.0565***	0.0505***	0.0535***
	[0.00104]	[0.00167]	[0.00110]	[0.00278]	[0.00142]	[0.00117]	[0.00116]
Parent=1 × Female=1	-0.0554***	-0.0596***	-0.0564***	-0.0367***	-0.0496***	-0.0510***	-0.0542***
	[0.00129]	[0.00184]	[0.00136]	[0.00364]	[0.00172]	[0.00150]	[0.00137]
R^2	0.672	0.647	0.674	0.675	0.665	0.716	0.672
N	850063	294971	791761	58188	362655	479577	512184
Individual controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Occupation FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Individual controls and number of keywords: *parent dummy, parent*female interaction, age, age^2, years of experience, years of education, immigrant dummy, marriage dummy, part-time dummy, year FEs, start-month FEs, number of keywords.* Firm controls: *1-letter industry dummies, firm location, number of employees, a private sector dummy.* Occupation fixed effects: *See Appendix B.3.1 for details.* Firm fixed effects: *4682 firms in total.* Observations weighted by full-time equivalents. Cluster-robust standard errors in brackets, clustered at the firm*occupation*start-month level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.
For version using continuous, standardised skill measures, see Table C.4

computer skills are interacted with the "cognitive" skill measure. In the earlier literature on the technological, cognitive and computer skills were emphasised as complementing technology during recent periods of technological change. The four sets of interaction terms are also interacted with the female dummy to check for possible gender differences in returns to these. The estimates are reported in Table 5.

The coefficients on the "social"*"cognitive" term and its interaction is negative and marginally significant, but its interaction with the female dummy is insignificant, and after including these terms, the coefficient on the "cognitive" skill measure and its interaction remain significant. Thus, at the individual level, I find no support for results in Deming and Kahn (2018). However, the interaction terms with the computer skill measures are also interesting. Returns to "computer (general)" skills do not only appear to be gender specific, but they also depend on the interaction with "cognitive" skills. When including the interaction term between "computer (general)" and "cognitive" skills, the coefficients on two skills alone are insignificant. Women also face lower returns to this interaction when compared to men.

The coefficient on the interaction between "computer (specific)" skills and "cognitive" skills is negative, but note that the coefficients on "computer (specific)" and "cognitive" skills alone are larger in this specification, and the overall effect of the combination of "computer (specific)" and "cognitive" skills remains positive. The gender difference in returns to this interaction is insignificant.

When including interaction terms between "cognitive" skills and both computer skills in the same specification (see column 5), the results from the two previous columns generally hold.

5.4.3. Continuous, standardised skill measures

The results using continuous, standardised skill measures are discussed in Appendix C.2. Figure C.4 and Table C.2 show the estimated coefficients on the continuous, standardised skill measures *without* gender interactions. Figure C.5 and Table C.3 show the analogous results for the model *including* gender interactions with the continuous skill measures. The standardised continuous skill measures generally confirm the previous results of women facing lower returns to most skills.

The negative coefficient on the binary "management" skill measure, see Figure 6, appears particularly controversial, but note that in the version using continuous, standardised skill measures, Figure C.5, the coefficient on the "management" skill measure is positive and significant for men. This difference can be explained by linear or increasing returns to management skills, rather than constant returns to management skills after surpassing a

Table 5: ln(hourly wage) regressed on skills, skill interactions, and gender interactions

	Dependent variable: ln(hourly wages)				
	(1) No interactions	(2) Cognitive and social	(3) Cognitive and computer(specific)	(4) Cognitive and computer(general)	(5) Cognitive and computer(both)
Female=1	-0.00866***	-0.00739**	-0.00821***	-0.0103***	-0.00990***
	[0.00225]	[0.00227]	[0.00226]	[0.00238]	[0.00237]
Cognitive=1	0.0112***	0.0190***	0.0135***	0.00451	0.00622*
	[0.00273]	[0.00495]	[0.00299]	[0.00270]	[0.00277]
Social=1	-0.00480*	-0.000226	-0.00489*	-0.00477*	-0.00488*
	[0.00234]	[0.00261]	[0.00232]	[0.00232]	[0.00230]
Character=1	0.0105***	0.0103***	0.0106***	0.0106***	0.0107***
	[0.00163]	[0.00163]	[0.00163]	[0.00163]	[0.00163]
Writing/language=1	0.00216	0.00218	0.00214	0.00212	0.00208
	[0.00238]	[0.00235]	[0.00238]	[0.00236]	[0.00234]
Customer Service=1	0.00351	0.00349	0.00331	0.00356	0.00332
	[0.00268]	[0.00268]	[0.00268]	[0.00268]	[0.00268]
Management=1	-0.00445*	-0.00469**	-0.00461**	-0.00436*	-0.00455**
	[0.00176]	[0.00175]	[0.00176]	[0.00177]	[0.00176]
Financial=1	0.0159***	0.0159***	0.0159***	0.0160***	0.0161***
	[0.00164]	[0.00164]	[0.00164]	[0.00165]	[0.00164]
Computer (general)=1	0.00223	0.00228	0.00215	-0.00310	-0.00416
	[0.00198]	[0.00197]	[0.00197]	[0.00268]	[0.00273]
Computer (specific)=1	0.00313	0.00295	0.00977*	0.00296	0.0115**
	[0.00285]	[0.00284]	[0.00443]	[0.00287]	[0.00447]
Female=1 × Cognitive=1	-0.00987***	-0.0130**	-0.0116***	-0.00337	-0.00464
	[0.00237]	[0.00430]	[0.00257]	[0.00244]	[0.00249]
Female=1 × Social=1	0.00577**	0.00334	0.00588**	0.00565**	0.00578**
	[0.00214]	[0.00240]	[0.00213]	[0.00211]	[0.00210]
Female=1 × Character=1	-0.0160***	-0.0159***	-0.0161***	-0.0162***	-0.0163***
	[0.00157]	[0.00157]	[0.00157]	[0.00157]	[0.00157]
Female=1 × Writing/language=1	0.000152	0.000158	0.000194	0.000196	0.000256
	[0.00208]	[0.00206]	[0.00208]	[0.00206]	[0.00205]
Female=1 × Customer Service=1	-0.0277***	-0.0278***	-0.0276***	-0.0278***	-0.0278***
	[0.00217]	[0.00217]	[0.00217]	[0.00217]	[0.00217]
Female=1 × Management=1	0.00370*	0.00385*	0.00387*	0.00365*	0.00386*
	[0.00166]	[0.00166]	[0.00166]	[0.00166]	[0.00166]
Female=1 × Financial=1	-0.0178***	-0.0179***	-0.0179***	-0.0180***	-0.0181***
	[0.00159]	[0.00158]	[0.00158]	[0.00159]	[0.00159]
Female=1 × Computer (general)=1	-0.00321	-0.00320	-0.00313	0.00182	0.00277
	[0.00183]	[0.00183]	[0.00182]	[0.00251]	[0.00254]
Female=1 × Computer (specific)=1	-0.00788**	-0.00777**	-0.0126**	-0.00755**	-0.0143**
	[0.00266]	[0.00266]	[0.00448]	[0.00271]	[0.00451]
Cognitive=1 × Social=1		-0.0113*			
		[0.00500]			
Female=1 × Cognitive=1 × Social=1		0.00527			
		[0.00454]			
Cognitive=1 × Computer (specific)=1			-0.0122*		-0.0157**
			[0.00501]		[0.00529]
Female=1 × Cognitive=1 × Computer (specific)=1			0.00812		0.0118*
			[0.00518]		[0.00543]
Cognitive=1 × Computer (general)=1				0.0128**	0.0151**
				[0.00452]	[0.00472]
Female=1 × Cognitive=1 × Computer (general)=1				-0.0121**	-0.0142**
				[0.00418]	[0.00435]
R^2	0.672	0.672	0.672	0.672	0.672
N	850063	850063	850063	850063	850063
Individual controls	Yes	Yes	Yes	Yes	Yes
Firm controls	Yes	Yes	Yes	Yes	Yes
Occupation FEs	Yes	Yes	Yes	Yes	Yes
Firm FEs	Yes	Yes	Yes	Yes	Yes

Individual controls and number of keywords: *parent dummy, parent*female interaction, age, age^2, years of experience, years of education, immigrant dummy, marriage dummy, part-time dummy, year FEs, start-month FEs, number of keywords.* Firm controls: *1-letter industry dummies, firm location, number of employees, a private sector dummy.* Occupation fixed effects: *See Appendix B.3.1 for details.* Firm fixed effects: *4682 firms in total.* Observations weighted by full-time equivalents. Cluster-robust standard errors in brackets, clustered at the firm*occupation*start-month level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. For version using continuous, standardised skill measures, see Table C.5

certain skill level threshold, which is the underlying assumption for the binary skill measures. When using the continuous, standardised skill measures, the coefficient on the interaction between the female indicator and the "management" skill measure is negative and significant.

For the remaining skills measures, however, the results using the binary skill measures align well with those using continuous, standardised skill measures, and the coefficients on the binary measures remain easier to interpret.

6. Conclusions

In this paper, I return to the question of whether or not women and men face equal pay for equal work, which is defined by US Equal Pay Act of 1963 as requiring *"substantially equal skill … within the same establishment"* (U.S. Equal Employment Opportunity Commission, 1997). Previously, data on task-specific skills have either been derived from small surveys or from occupation-level databases such as O*NET, and thus, estimating gender differences in returns to skills was unfeasible while also controlling for occupation and firm FEs.

In order to determine whether or not women and men face different returns to the same task-specific skills, a combination of Danish job vacancy and Danish matched employer-employee data is operationalised for the first time. Internationally, only few studies has merged job vacancy data with individual-level data, but with much lower match rates (e.g. Kettemann et al., 2018). Thus, this paper is one of the first to utilise individual-level variation in characteristics and wages together with skills data from job vacancies. I derive nine task-specific skill measures from job posts by a reading of keywords. The vacancy and register data are matched on the firm*occupation*start-month-level, which involves some aggregation. However, a high degree of gender segregation at these levels preserves variation in skills across genders. With the matched data it is possible to show that variation in skills cannot be entirely explained by an extensive set of control variables, occupation FEs, and firm FEs. Keeping this in mind, the skills measures are included in wage regressions. With control variables, and firm and occupation FEs included in wage regressions, the coefficients on the nine skill measures are largely insignificant, but only when gender interactions with the skill measures are excluded. When including interactions between gender and skills, a different story emerges.

Even after including occupation FEs, firm FEs, and the extensive set of control variables, the coefficients on the female interactions with "cognitive", "character", "customer service", "financial", and "computer(specific)" skills are negative and significant. "Social" skills are associated with lower wages for men, but not for women. Thus, ignoring the gendered dimension of returns to skills would generally lead to (gender-)biased results and conclusions, overestimating the returns to skills for women and underestimating them for men.

For the subsample of workers in the private sector, I find no significant gender differences in returns to "social" and "cognitive" skills, but instead, the female interaction with "management" skills is negative and significant.

Additionally, interactions between the skill measures are considered. After including controls and FEs, the interaction between the "cognitive" and "social" skill measures is not associated with higher wages at the individual level, which contrasts a number of studies on returns to skills, which highlight the interaction between social and cognitive skills as particularly important (Deming, 2017; Deming and Kahn, 2018; Weinberger, 2014). This may be due to differences between the US and Danish labour market, but it may also be due to the fact that individual-level data on job skill requirements and wages have not been utilised in a US context. Particularly, Deming and Kahn (2018) find positive correlations between the dual requirement of social and cognitive skills in job posts and MSA-occupation level wages as well as firm performance. A possible explanation of the difference between my findings and those of Deming and Kahn (2018) is that the individual team member providing social skills may not be fully rewarded for the positive spillovers on team members. An individual's social skills may increase own marginal productivity slightly, but also increase the overall productivity of a team or firm (cf. Deming, 2017). Thus, individual returns to social skills (and the interaction with cognitive skills) may be low, while the total returns to teams and firms may be high.

Furthermore, the interaction between the skill measures "cognitive" and "computer (general)" is associated with higher wages for men, but not for women. The interaction between cognitive and computer skills was highlighted as particularly important in the early literature on technological change. My results indicate that this skill interaction remains important for wage formation as it is associated with higher wages for men, but not for women when controlling for occupation and firm FEs.

How do the findings of gender differences in returns to task-specific skills relate to the existing literature on gender differences in earnings? Firstly, the aim of this paper is to compare the earnings of women and men that, by a very narrow definition, undertake "equal work" and not to provide a decomposition of the gender pay gap. Therefore, I control for factors that may affect earnings either because they have an effect on the nature of work itself, or because they are related to non-pecuniary benefits and cost at the job. Thus, when estimating gender differences in returns to skills, I control for sorting into firms and occupations, parental status, years of education, part-time status etc.; factors that are all highlighted as drivers of gender differences in earnings (e.g. Blau and Kahn, 2017). While controlling for those factors, I compare the earnings of women and men who are employed in jobs that require the same task-specific skills, and I find that women face lower return to most of these skills. In this setting, women and men do not face equal pay for equal skills, and thus, not for equal work. Because of the relatively short sample period, I cannot confirm whether or not changing skills prices has caused a narrowing of the gender pay gap (as pointed out by Bacolod and Blum, 2010; Beaudry and Lewis, 2014; Rendall, 2010; Yamaguchi, 2018). However, my results confirm that differences in returns to skills contribute to the gender pay gap (cf. Lindley, 2012).

How can the findings of gender differences in returns to skills be explained? Although I control for many contributors to gender differences in pay, some of the factors that are highlighted in the existing literature as contributing to the general gender pay gap may also explain gender differences in returns to skills.

For example, negotiation of wages at the firm-level contribute to gender differences in pay (Card et al., 2016; Bertrand, 2011). Similarly, negotiation of wages may also cause differences in returns to skills if women and men with same skills negotiate different wages within the same firm. Blau and Kahn (2017) emphasise that negotiation of wages is a form of bargaining, and if women face discrimination in the rest of the labour market, their outside options are also relatively worse, and thus, their bargaining power is – on average – also lower than that of men. This points to another explanation of gender differences in returns to skills, namely taste-based and statistical discrimination (Guryan and Charles, 2013). Employers may require the same task-specific skills of their female and male workers, but incorrectly assume that one of them is more productive than the other.

Finally, in line with Babcock, Recalde, and Vesterlund (2017a), Babcock, Recalde, Vesterlund, and Weingart (2017b), and Niederle and Vesterlund (2007), it may be the case that women are more likely to be assigned tasks with low rewards or are more likely to avoid competition to get out of these tasks. Despite the detailed task-specific skill measures used in this paper, even within the nine categories of task-specific skills, it may be case that women end up undertaking the less rewarding tasks.

Bargaining, discrimination, and gender-specific task assignments are all potential explanations of gender differences in returns to task-specific skills. However, gender-specific task assignment within the nine categories of task-specific skills is the only explanation that points to the fact that women and men may be paid differently due to differences in the work they undertake. If this is the dominant explanation, could it still be argued that women and men face equal pay for equal work? Recall that the US Equal Pay Act of 1963 defines equal work as requiring *"substantially equal skill"* and not necessarily as an identical set of tasks (U.S. Equal Employment Opportunity Commission, 1997). In this paper, I have utilised data on required task-specific skills in job posts to show that women face lower returns to five out of nine skills, and thus, by the definition in the US Equal Pay Act of 1963, the ambition of equal pay for equal work appears to remain far from reality, at least in Denmark.

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Appendix A. Additional results

Table A.1: ln(hourly wage) regressed on skills *without* gender interaction

	Dependent variable: ln(hourly wages)			
	(1)	(2)	(3)	(4)
	Individual controls	Occupation FEs	Firm FEs	Both FEs
Female=1	-0.0236***	-0.0277***	-0.0339***	-0.0273***
	[0.00424]	[0.00111]	[0.00122]	[0.000979]
Cognitive=1	0.0581***	0.0115***	0.0430***	0.00540***
	[0.00555]	[0.00185]	[0.00269]	[0.00161]
Social=1	0.0243***	0.00000872	0.00391	-0.00151
	[0.00601]	[0.00177]	[0.00275]	[0.00148]
Character=1	-0.0236***	-0.00350**	-0.0243***	0.000757
	[0.00213]	[0.00126]	[0.00132]	[0.00105]
Writing/language=1	0.01000**	0.00639***	0.00275	0.00234
	[0.00318]	[0.00166]	[0.00180]	[0.00143]
Customer Service=1	-0.0393***	-0.0150***	-0.0199***	-0.00967***
	[0.00295]	[0.00285]	[0.00241]	[0.00237]
Management=1	0.0194***	0.00108	0.0184***	-0.00205
	[0.00230]	[0.00152]	[0.00165]	[0.00116]
Financial=1	-0.00593*	0.00343*	-0.00450**	0.00485***
	[0.00235]	[0.00141]	[0.00162]	[0.00111]
Computer (general)=1	-0.00572	0.00208	-0.00134	0.000500
	[0.00345]	[0.00146]	[0.00189]	[0.00118]
Computer (specific)=1	0.0112	0.00653*	-0.00354	-0.00112
	[0.00635]	[0.00318]	[0.00277]	[0.00196]
Parent=1	0.0781***	0.0574***	0.0654***	0.0526***
	[0.00192]	[0.00110]	[0.00121]	[0.00103]
Parent=1 × Female=1	-0.0975***	-0.0602***	-0.0775***	-0.0550***
	[0.00378]	[0.00134]	[0.00157]	[0.00127]
R^2	0.478	0.635	0.565	0.672
N	850068	850068	850063	850063
Individual controls	Yes	Yes	Yes	Yes
Firm controls	Yes	Yes	Yes	Yes
Occupation FEs	No	Yes	No	Yes
Firm FEs	No	No	Yes	Yes

Individual controls and number of keywords: *parent dummy, parent*female interaction, age, age^2, years of experience, years of education, immigrant dummy, marriage dummy, part-time dummy, year FEs, start-month FEs, number of keywords.* Firm controls: *1-letter industry dummies, firm location, number of employees, a private sector dummy.* Occupation fixed effects: *See Appendix B.3.1 for details.* Firm fixed effects: *4682 firms in total.* Observations weighted by full-time equivalents. Cluster-robust standard errors in brackets, clustered at the firm*occupation*start-month level. *$*$ $p < 0.05$, $**$ $p < 0.01$, $***$ $p < 0.001$.* For version using continuous, standardised skill measures, see Table C.2

Table A.2: ln(hourly wage) regressed on skills *with* gender interaction

	Dependent variable: ln(hourly wages)			
	(1)	(2)	(3)	(4)
	Individual controls	Occupation FEs	Firm FEs	Both FEs
Female=1	-0.00219	-0.00867***	-0.000758	-0.00866***
	[0.00605]	[0.00254]	[0.00297]	[0.00225]
Cognitive=1	0.0766***	0.0181***	0.0573***	0.0112***
	[0.0102]	[0.00311]	[0.00466]	[0.00273]
Social=1	0.0262**	-0.00316	0.00301	-0.00480*
	[0.00964]	[0.00279]	[0.00437]	[0.00234]
Character=1	-0.00349	0.00413*	-0.00855***	0.0105***
	[0.00386]	[0.00204]	[0.00194]	[0.00163]
Writing/language=1	0.00476	0.00579*	0.00160	0.00216
	[0.00640]	[0.00292]	[0.00293]	[0.00238]
Customer Service=1	-0.0130***	-0.00392	-0.00214	0.00351
	[0.00393]	[0.00334]	[0.00276]	[0.00268]
Management=1	0.00780*	-0.00119	0.0158***	-0.00445*
	[0.00357]	[0.00232]	[0.00224]	[0.00176]
Financial=1	-0.00300	0.0159***	0.0111***	0.0159***
	[0.00477]	[0.00233]	[0.00224]	[0.00164]
Computer (general)=1	-0.0157*	0.00385	-0.00201	0.00223
	[0.00616]	[0.00244]	[0.00281]	[0.00198]
Computer (specific)=1	0.0277**	0.0117**	0.00172	0.00313
	[0.00949]	[0.00439]	[0.00381]	[0.00285]
Female=1 × Cognitive=1	-0.0328***	-0.0110***	-0.0242***	-0.00987***
	[0.00854]	[0.00270]	[0.00375]	[0.00237]
Female=1 × Social=1	-0.00560	0.00596*	0.00134	0.00577**
	[0.00760]	[0.00244]	[0.00356]	[0.00214]
Female=1 × Character=1	-0.0341***	-0.0131***	-0.0257***	-0.0160***
	[0.00358]	[0.00189]	[0.00191]	[0.00157]
Female=1 × Writing/language=1	0.00820	0.000766	0.00199	0.000152
	[0.00590]	[0.00259]	[0.00257]	[0.00208]
Female=1 × Customer Service=1	-0.0579***	-0.0243***	-0.0371***	-0.0277***
	[0.00397]	[0.00284]	[0.00245]	[0.00217]
Female=1 × Management=1	0.0190***	0.00355	0.00375	0.00370*
	[0.00369]	[0.00203]	[0.00199]	[0.00166]
Female=1 × Financial=1	-0.00409	-0.0206***	-0.0251***	-0.0178***
	[0.00486]	[0.00216]	[0.00202]	[0.00159]
Female=1 × Computer (general)=1	0.0162**	-0.00346	0.000714	-0.00321
	[0.00520]	[0.00218]	[0.00238]	[0.00183]
Female=1 × Computer (specific)=1	-0.0309***	-0.0100**	-0.0102**	-0.00788**
	[0.00753]	[0.00344]	[0.00320]	[0.00266]
Parent=1	0.0787***	0.0574***	0.0653***	0.0527***
	[0.00196]	[0.00111]	[0.00122]	[0.00104]
Parent=1 × Female=1	-0.0987***	-0.0604***	-0.0776***	-0.0554***
	[0.00376]	[0.00136]	[0.00160]	[0.00129]
R^2	0.481	0.636	0.566	0.672
N	850068	850068	850063	850063
Individual controls	Yes	Yes	Yes	Yes
Firm controls	Yes	Yes	Yes	Yes
Occupation FEs	No	Yes	No	Yes
Firm FEs	No	No	Yes	Yes

Individual controls and number of keywords: *parent dummy, parent*female interaction, age, age^2, years of experience, years of education, immigrant dummy, marriage dummy, part-time dummy, year FEs, start-month FEs, number of keywords.* Firm controls: *1-letter industry dummies, firm location, number of employees, a private sector dummy.* Occupation fixed effects: *See Appendix B.3.1 for details.* Firm fixed effects: *4682 firms in total.* Observations weighted by full-time equivalents. Cluster-robust standard errors in brackets, clustered at the firm*occupation*start-month level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.
For version using continuous, standardised skill measures, see Table C.3

Appendix B. Data details

Some of the data details are also described in Daly, Jensen, and le Maire (2021), where we utilise a subset of the data described here.

B.1. Register data

Observations with the following characteristics are sequentially excluded from the sample:

a) With a missing DISCO-code (or a DISCO-code that cannot be converted to one of the 228 occupational groups, see Appendix D.2) or firm identifier
b) With a total number of hours for a given year below the equivalent of a full-time month (1923.96/12 as defined by Statistics Denmark) or above 3,500 hours. Part-time workers remain included.
c) Aged under 20, or over 65 when their job spell commences.
d) With an hourly wage below 30 DKK or above 5,000 DKK (in 2016-levels)
e) With total wages exceeding 10,000,000 DKK (in 2016-levels)
f) Enrolled at an educational institution when their job spell commences.
g) At firms with less than 5 full-time equivalents in the matched sample (see below).
h) In an occupational group with less than 50 full-time equivalents in matched sample (see below).

Criterion a) and b) are the most restrictive. Criterion a) is necessary to construct job spells at the firm*occupation level, missing DISCO-codes are mostly observed in the private sector. I describe the DISCO-coding in detail below. Criterion b) is imposed to avoid observations where hours of work may be misreported, e.g. freelance work. In addition, I believe that jobs spells with fewer hours than the equivalent of a full-time month are less likely to appear in the job vacancy data due to fixed costs of hiring.

B.2. Vacancy data

As pointed out in the section 3.2, all keywords are assigned either a skill category or a noise tag. This is done the following way: 1) The most frequent keywords (approx. 2000) are assigned a skill category or noise tag manually. These words amount to the vast majority of keyword-observations. 2) Using online dictionary APIs each word's synonyms are obtained. Each word's synonyms are assigned the same category. 3) Using online dictionary APIs each word's definition is obtained. 4) Using the definition of the words, the remaining non-categorised words are assigned a category using machine-learning methods. The machine-learning methods are described in more detail here.

The training set consists of both the more than 2000 manually categorised words and their categorised synonyms. In order to categorise the remaining words, the dictionary definition of each keyword obtained from two dictionaries, one Danish dictionary and one English dictionary. To use the English dictionary, the keywords are translated beforehand. Although the translation step may seem tedious, it involves some regularisation of the keywords, which again helps when looking up definitions of the words. Next, the classification exercise is undertaken.

Two approaches to the classification problem is repeated for both Danish and English versions of the keywords' definitions. The first approach is a one-step categorisation, where each keywords is assigned one of 10 categories, i.e. either one of the nine skills or a noise tag. A Random Forest Predictor is used for this exercise. The second approach is a two-step categorisation. In the first step, each keyword is classified as either noise or non-noise. In the second step each non-noise word is assigned to one of the nine skill categories. For both steps a Random Forest Predictor is applied.

Thus, four predicted categorisations are available for each keyword that was not a part of the training set: a one-step and a two-step version for both the Danish and English definitions. If predictions from all four approaches agree on a category, the keyword is assigned to this category. The same step is undertaken if predictions from three out of four approaches agree. Some words' definitions are only available in either the Danish or English dictionary. These words are categorised if the two approaches in the same language agree and if the probability of the predicted class is relatively high. For the few words that have not been categorised after these steps, English predictions with very high probabilities are considered and assigned to keywords. The predictions based on the English definitions are typically more reliable due to longer definitions of the keywords. If keywords are not categorised after this procedure, they are assigned a noise tag.

B.3. Data match

B.3.1. DISCO-codes

Statistics Denmark has adopted 6-digit versions of the International Labour Organization's ISCO88 and ISCO08 occupational codes, namely DISCO88 and DISCO08. The two extra digits are added to provide additional detail. Prior to 2010, all occupation codes in Statistics Denmark's datasets are coded using the 6-digit DISCO88 codes (although for some early years, only 4-digit codes are reported). From 2010 and onwards, all occupation codes in Statistics Denmark's datasets are coded using the 6-digit DISCO08 codes. Unfortunately, the (D)ISCO88 and (D)ISCO08 codes do not map consistently one-to-one, one-to-many or many-to-one, and thus, a crosswalk cannot be straightforwardly produced, and crosswalks are not provided by either ILO or Statistics Denmark. Since I consider labor market outcomes from 2008-2017, I need consistent occupational codes over this period. Therefore, I produce a revealed crosswalk, using occupational information on people that remain in the same job during the break in occupational codes from December 2009 to January 2010. I aggregate 4-digit DISCO88 and 6-digit DISCO08 codes into 228 mutually exclusive occupational groups. In comparison, there are 145 unique 3-digit and 479 unique 4-digit valid DISCO88 codes reported in the monthly employment register (BFL) from 2008. Thus, my grouping gives a level of detail somewhere between the 3- and 4-digit level. The crosswalk is included in Online Appendix D.2.

Although variables on wages and hours in the employment register are automatically imputed from the Danish tax authorities' data, the DISCO-codes are not. As they require some "manual" coding, i.e. placing a worker in a category, they do not appear in the Danish tax authorities' data. Hence, Statistics Denmark collects the DISCO-codes in a separate procedure. For public employees, Statistics Denmark impute DISCO-codes directly from the public wage data where every employee's job title/position is recorded. In the private sector, Statistics Denmark collect data on employees from firms with 100 or more employees every year.[14] Smaller firms are sampled to report DISCO-codes on their employees from year to year. Private employers are supplied with a correspondence table between job titles/positions and DISCO-codes in order to secure consistent reporting.[15] If a private firm is not sampled, Statistics Denmark impute an individual's DISCO-code from the previous year given that changes no in the individual's employment are observed. Otherwise, they estimate a DISCO-code from register data on each individual's education, the industry of the individual's employer, and the individual's membership of an unemployment insurance fund (these funds are often occupation-specific).[16]

In the case of the job vacancy data, HBS first extract a job title from each job post. Using a correspondence table between job titles/positions and DISCO-codes similar to that supplied by Statistics Denmark to DISCO-reporting firms, HBS can then identify the *6-digit* DISCO-code which corresponds to the extracted job title.[17] Thus, both the register data's and the

[14] For more details, see:
https://www.dst.dk/ext/loen/Vejl_Lon_ligeaar–pdf

[15] For more details, see:
https://www.dst.dk/da/Indberet/oplysningssider/loenstatistik/stillingsbetegnelser-disco-08-i-loenstatistikken

[16] For more details, see:
https://www.dst.dk/Site/Dst/SingleFiles/hojkvalbilag.aspx?varid=107187&bilagid=183191

[17] For more details, see:
http://www.hbseconomics.dk/wp-content/uploads/2017/09/Eftersp%C3%B8rgslen-efter-sproglige-kompetencer.pdf

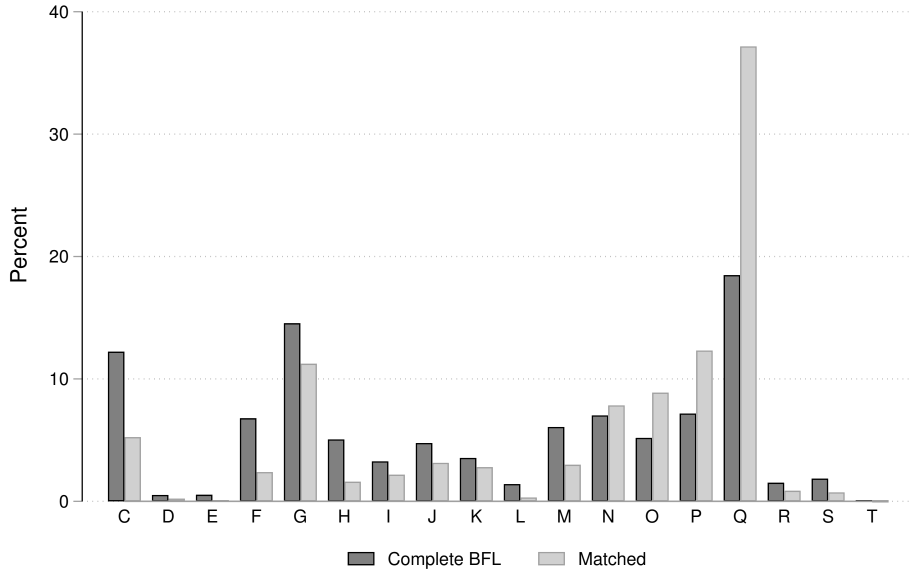
job vacancy data's *6-digit* DISCO-codes are imputed from detailed job titles/positions.

Though DISCO-codes are generally imputed in a similar manner in both the register data and the job vacancy data, some inconsistencies are to be expected at the very detailed *6-digit* level. For example, there are three subdivisions of school teachers at the *6-digit* level and only one at the *4-digit* and *3-digit* levels. For more details, see: https://www.dst.dk/da/Statistik/dokumentation/nomenklaturer/disco-08 However, due the 2009/2010 break in occupational codes, I convert the DISCO-codes from both data sources into the 228 consistent occupational groups. Another advantage of this strategy is that the aggregation resulting from the conversion also eliminates these potential inconsistencies at the very detailed 6-digit level.

B.3.2. Occupational and industrial distributions

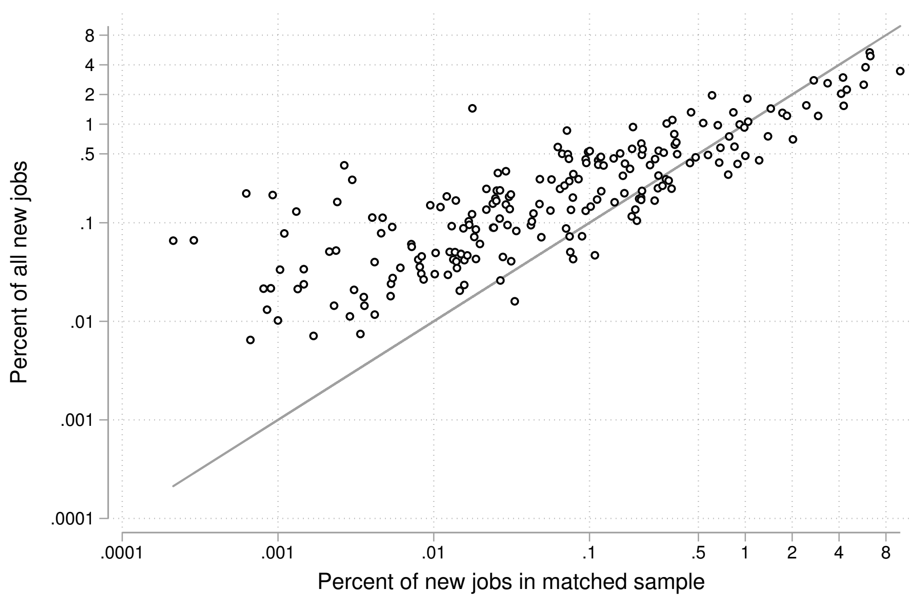
A lists of d 1-letter industries with their titles are included in Table B.1. Figure B.2 and Figure B.1 shows the occupational and industrial distribution of workers in the matched and the full sample respectively. Notice the overrepresentation of the industries "O Public administration and defence; compulsory social security", "P Education", and "Q Human health and social work activities". These occupations are dominated by large groups of public employees, namely teachers, nurses and care assistant. Figure B.2 shows the representation of the 228 occupational groups in the matched data. If a data point lies to the left of the 45 degree line, it indicates that an occupation is underrepresented in the matched data, and if to the right of the 45 degree line, it is overrepresented. Thus, the figure shows that smaller occupations generally are underrepresented and that larger occupations generally are overrepresented in the matched sample. Some occupations are dropped entirely as they are very small, and in the final estimation sample 160 occupations are represented, see exclusion criteria in Appendix B.1.

Figure B.1: Distribution of industries in the monthly employment register (BFL) and matched data



Source: BFL, 2008-2018, HBS-Jobindex 2007-2017.
Note: Observations weighted by full-time equivalents.

Figure B.2: Distribution of occupations in the monthly employment register (BFL) and matched data



Source: BFL, 2008-2018, HBS-Jobindex 2007-2017.
Note: Observations weighted by full-time equivalents.

Table B.1: 1-letter industries

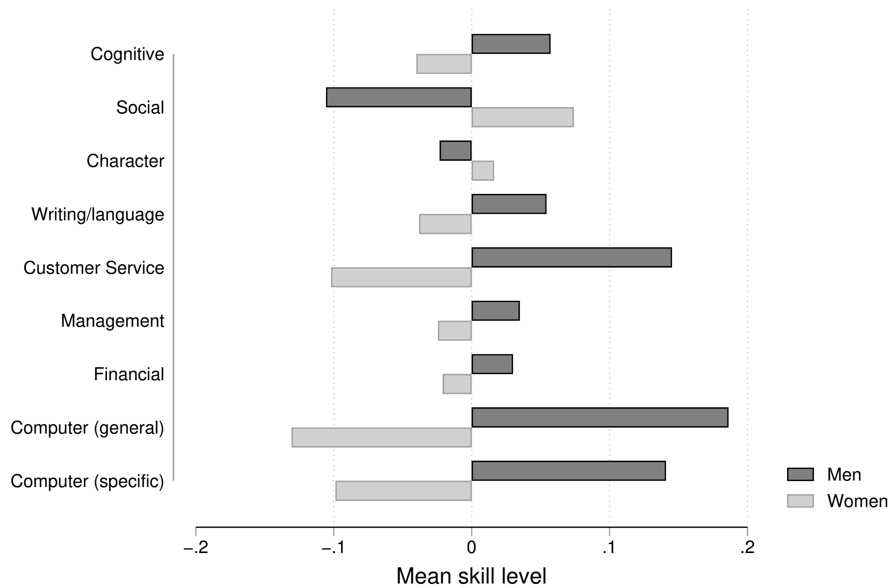
1-letter code	Industry title
A	Agriculture, forestry and fishing
B	Mining and quarrying
C	Manufacturing
D	Electricity, gas, steam and air conditioning supply
E	Water supply; sewerage; waste management and remediation activities
F	Construction
G	Wholesale and retail trade; repair of motor vehicles and motorcycles
H	Transporting and storage
I	Accommodation and food service activities
J	Information and communication
K	Financial and insurance activities
L	Real estate activities
M	Professional, scientific and technical activities
N	Administrative and support service activities
O	Public administration and defence; compulsory social security
P	Education
Q	Human health and social work activities
R	Arts, entertainment and recreation
S	Other services activities

Source: http://ec.europa.eu/competition/mergers/cases/index/nace_all.html

Appendix C. Results using continuous skills measures

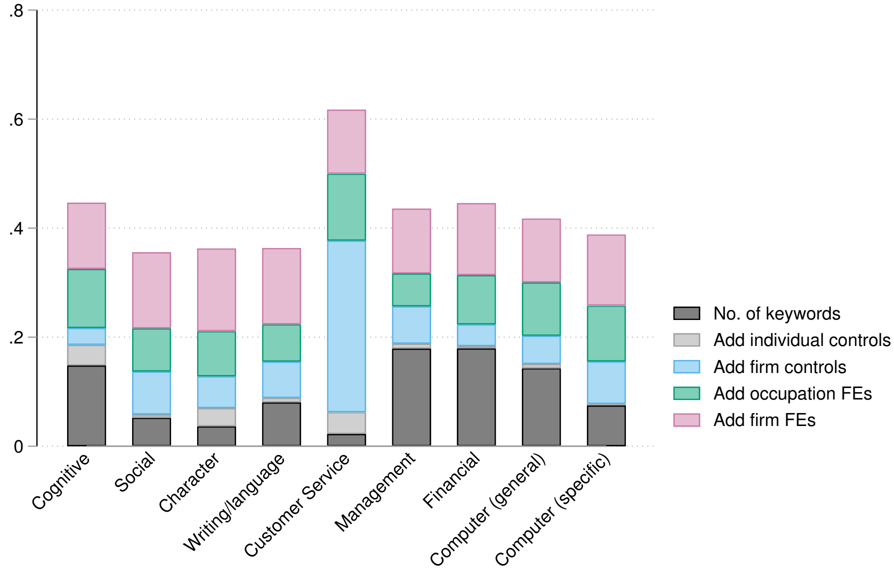
C.1. Descriptive results

Figure C.1: Continuous, standardised skill measures: Mean skill levels by gender



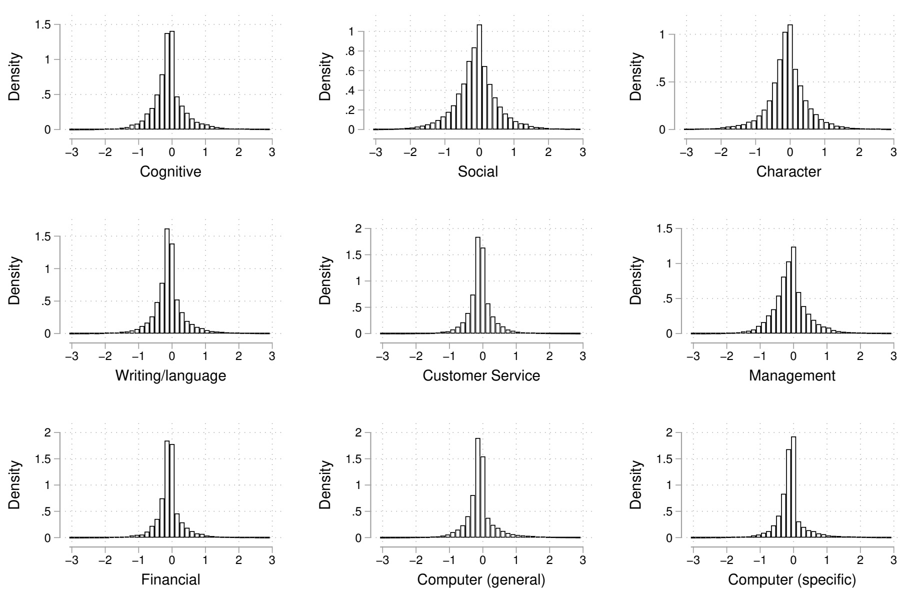
Source: BFL, 2008-2018, HBS-Jobindex 2007-2017.
Note: Observations weighted by full-time equivalents.

Figure C.2: Continuous, standardised skill measures: Adjusted R^2 from regressions of skills level on various controls



Notes: Skills are regressed on various sets of controls. Individual controls and number of keywords: *parent dummy, parent*female interaction, age, age*^{2}*, years of experience, years of education, immigrant dummy, marriage dummy, part-time dummy, year FEs, start-month FEs, number of keywords.* Firm controls: *1-letter industry dummies, firm location, number of employees, a private sector dummy.* Occupation fixed effects: *See Appendix B.3.1 for details.* Firm fixed effects: *4682 firms in total.* Observations weighted by full-time equivalents.

Figure C.3: Continuous, standardised skill measures: Distributions of de-meaned skill requirements within firm*occupation cells



Note: The figure shows individual skill level minus the mean skill level within each firm*occupation cells.

Table C.1: Continuous, standardised skill measures: Correlation tables of skills, wages and gender

	ln(wage)	Female	Cognitive	Social	Character	Writing/language	Customer Service	Management	Financial	Computer (general)	Computer (specific)
ln(wage)	1										
Female	-0.150***	1									
Cognitive	0.248***	-0.0479***	1								
Social	0.0760***	0.0885***	0.0447***	1							
Character	-0.155***	0.0194***	-0.181***	0.0752***	1						
Writing/language	0.0411***	-0.0455***	0.0380***	-0.0843***	-0.0819***	1					
Customer Service	-0.0866***	-0.122***	-0.0906***	-0.0658***	0.148***	0.0383***	1				
Management	0.103***	-0.0292***	0.187***	0.158***	-0.112***	-0.0588***	-0.0721***	1			
Financial	0.0936***	-0.0251***	0.143***	0.0169***	-0.0711***	-0.00244*	0.0375***	0.203***	1		
Computer (general)	0.153***	-0.156***	0.181***	-0.0455***	-0.109***	0.0404***	0.0547***	0.0672***	0.0407***	1	
Computer (specific)	0.152***	-0.118***	0.171***	-0.00310**	-0.0942***	0.0506***	0.00715***	0.0989***	0.0365***	0.265***	1

Notes: Observations weighted by full-time equivalents.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

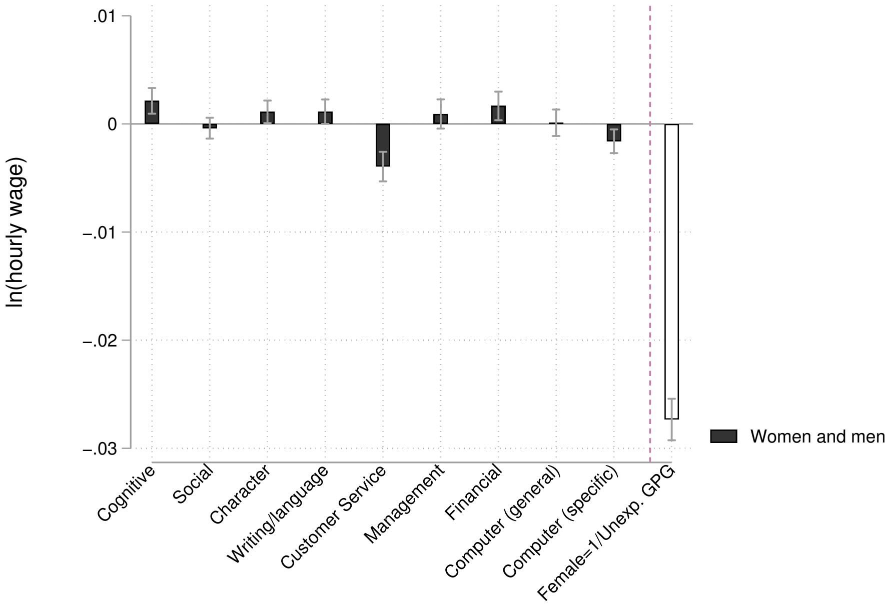
C.2. Regression estimates

Figure C.4 and Table C.2 show the estimated coefficients on the continuous, standardised skill measures *without* gender interactions. The estimates show that when both occupation and firm FEs are included in the model, the coefficients on the skill measures tend to be insignificant or only marginally significant. Exceptions are jobs with higher levels of "customer service" skills, which appear is associated with lower wages. Jobs categorised with higher level of "cognitive" skill are also found to be associated with higher wages here, and the effect is highly significant. Small, positive effects of "character", "writing/language" and "financial" skills are found, but the effects are only marginally significant. Somewhat surprisingly, "computer (specific)" are correlated with lower wages within occupation*firm cells.

Figure C.5 and Table C.3 show the analogous results for the model *including* gender interactions with the continuous skill measures. In the model including both occupation and firm FEs, consider the coefficients on the skill measures that are not interacted with the female dummy, γ_1. As proviously mentioned, these coefficients can be interpreted as *within*-firm*occupation returns to skills for men. The coefficients on the "cognitive", "character" and "financial" skill measures are large, positive and significant even after introducing occupation and firm FEs, which confirms the findings from the analysis using the binary skill measures. In addition, when using the continuous skill measures, higher level of "writing/language" skills are also correlated with higher wages. The coefficients on the "social" measure remains negative and is highly significant. The main contrast to the findings using the binary skills measures is the changed sign for the coefficient on "management" skills, which is positive for the standardised, continuous skills measures. This difference can be explained by linear or increasing returns to management skills, rather than constant returns to management skills after surpassing a certain skill level threshold, which is the underlying assumption for the binary skill measures.

In the model including both occupation and firm FEs, the coefficients on the female*skill interactions, γ_2, can be interpreted as *within*-firm*occupation gender differences in returns. Notice the coefficient on the interaction terms with all skill measures, but "social" skills, are negative, and for most of the skills also highly significant. Thus, the standardised continuous skill measures confirm the previous results of women facing lower returns to most skills. The interaction term with the "social" skill measure is positive and significant at the 0.01-level. Also for the interaction term, a change of sign on the "management" skill measure is observed when using the standardised continuous skill measure. This again implies linear or increasing returns to management skill as well as gender differences in those.

Figure C.4: Continuous, standardised skill measures: Coefficients on skills *without* gender interaction



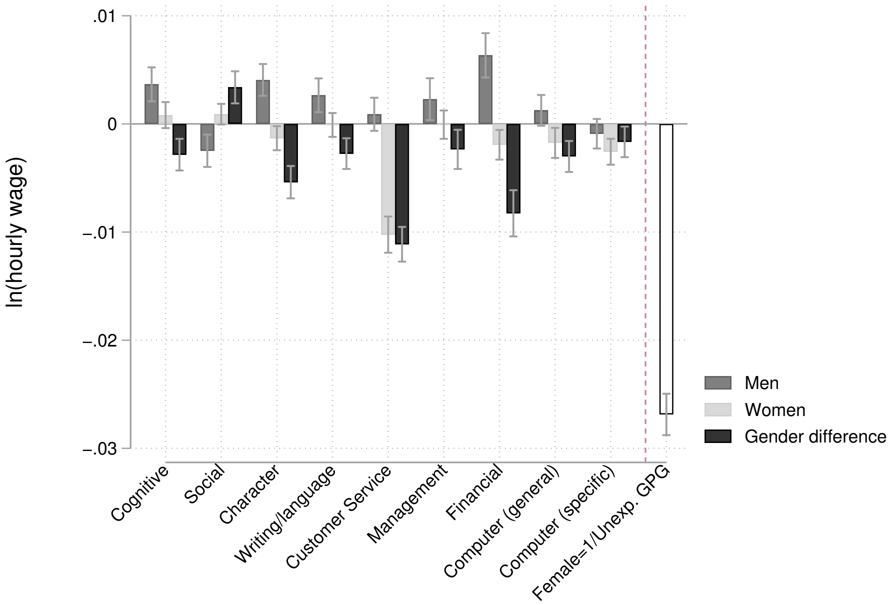
Notes: Individual controls and number of keywords: *parent dummy, parent*female interaction, age, age*^{2}*, years of experience, years of education, immigrant dummy, marriage dummy, part-time dummy, year FEs, start-month FEs, number of keywords.* Firm controls: *1-letter industry dummies, firm location, number of employees, a private sector dummy.* Occupation fixed effects: *See Appendix B.3.1 for details.* Firm fixed effects: *4682 firms in total.* Observations weighted by full-time equivalents. Standard errors clustered at the firm*occupation*start-month level, 95 % confidence intervals indicated.

Table C.2: Continuous, standardised skill measures: ln(hourly wage) regressed on skills *without* gender interaction

	Dependent variable: ln(hourly wages)			
	(1)	(2)	(3)	(4)
	Individual controls	Occupation FEs	Firm FEs	Both FEs
Female=1	-0.0208***	-0.0276***	-0.0324***	-0.0273***
	[0.00444]	[0.00110]	[0.00123]	[0.000978]
Cognitive	0.0210***	0.00578***	0.0170***	0.00213***
	[0.00135]	[0.000816]	[0.000874]	[0.000606]
Social	0.00943***	0.00136	0.00408***	-0.000397
	[0.00143]	[0.000723]	[0.000773]	[0.000492]
Character	-0.0133***	-0.00244***	-0.0177***	0.00112*
	[0.00120]	[0.000641]	[0.000794]	[0.000531]
Writing/language	0.00218*	0.00256***	0.000571	0.00113*
	[0.00110]	[0.000624]	[0.000788]	[0.000576]
Customer Service	-0.0137***	-0.00390***	-0.0105***	-0.00395***
	[0.00129]	[0.000954]	[0.000971]	[0.000696]
Management	0.0111***	0.00467***	0.0169***	0.000918
	[0.00254]	[0.000994]	[0.00118]	[0.000689]
Financial	0.00126	0.00316***	-0.00322***	0.00166*
	[0.00120]	[0.000816]	[0.000965]	[0.000673]
Computer (general)	-0.000832	0.00117	-0.00282***	0.000106
	[0.00121]	[0.00115]	[0.000740]	[0.000624]
Computer (specific)	0.00410**	0.00136	-0.00126	-0.00160**
	[0.00131]	[0.00101]	[0.000693]	[0.000560]
Parent=1	0.0783***	0.0574***	0.0652***	0.0526***
	[0.00203]	[0.00110]	[0.00121]	[0.00103]
Parent=1 × Female=1	-0.0986***	-0.0603***	-0.0774***	-0.0550***
	[0.00399]	[0.00134]	[0.00157]	[0.00127]
R^2	0.476	0.635	0.566	0.672
N	850068	850068	850063	850063
Individual controls	Yes	Yes	Yes	Yes
Firm controls	Yes	Yes	Yes	Yes
Occupation FEs	No	Yes	No	Yes
Firm FEs	No	No	Yes	Yes

Notes: Individual controls and number of keywords: *parent dummy, parent*female interaction, age, age*^{2}*, years of experience, years of education, immigrant dummy, marriage dummy, part-time dummy, year FEs, start-month FEs, number of keywords.* Firm controls: *1-letter industry dummies, firm location, number of employees, a private sector dummy.* Occupation fixed effects: *See Appendix B.3.1 for details.* Firm fixed effects: *4682 firms in total.* Observations weighted by full-time equivalents. Cluster-robust standard errors in brackets, clustered at the firm*occupation*start-month level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Figure C.5: Continuous, standardised skill measures: Coefficients on skills for women and men, and their gender difference



Notes: Individual controls and number of keywords: *parent dummy, parent*female interaction, age, age^2, years of experience, years of education, immigrant dummy, marriage dummy, part-time dummy, year FEs, start-month FEs, number of keywords.* Firm controls: *1-letter industry dummies, firm location, number of employees, a private sector dummy.* Occupation fixed effects: *See Appendix B.3.1 for details.* Firm fixed effects: *4682 firms in total.* Observations weighted by full-time equivalents. Standard errors clustered at the firm*occupation*start-month level, 95 % confidence intervals indicated.

Table C.3: Continuous, standardised skill measures: ln(hourly wage) regressed on skills *with* gender interaction

	Dependent variable: ln(hourly wages)			
	(1)	(2)	(3)	(4)
	Individual controls	Occupation FEs	Firm FEs	Both FEs
Female=1	-0.0183***	-0.0268***	-0.0314***	-0.0269***
	[0.00420]	[0.00108]	[0.00121]	[0.000976]
Cognitive	0.0250***	0.00715***	0.0200***	0.00366***
	[0.00196]	[0.00105]	[0.00119]	[0.000802]
Social	0.00909***	-0.000133	0.00172	-0.00249**
	[0.00237]	[0.00115]	[0.00123]	[0.000764]
Character	-0.00442*	-0.000743	-0.0106***	0.00407***
	[0.00185]	[0.000974]	[0.00102]	[0.000750]
Writing/language	0.00502**	0.00419***	0.00332**	0.00265***
	[0.00163]	[0.000871]	[0.00113]	[0.000796]
Customer Service	-0.00258	0.000512	-0.00220*	0.000889
	[0.00139]	[0.00113]	[0.000971]	[0.000781]
Management	0.000825	0.00684***	0.0152***	0.00229*
	[0.00440]	[0.00140]	[0.00156]	[0.000988]
Financial	0.00978***	0.00869***	0.00701***	0.00634***
	[0.00206]	[0.00128]	[0.00145]	[0.00105]
Computer (general)	0.00182	0.00173	-0.000354	0.00125
	[0.00135]	[0.00125]	[0.000847]	[0.000725]
Computer (specific)	0.00634***	0.00200	0.000222	-0.000911
	[0.00157]	[0.00123]	[0.000828]	[0.000697]
Female=1 × Cognitive	-0.00751***	-0.00254**	-0.00555***	-0.00284***
	[0.00156]	[0.000895]	[0.00104]	[0.000747]
Female=1 × Social	-0.000515	0.00247*	0.00346**	0.00338***
	[0.00205]	[0.00101]	[0.00112]	[0.000756]
Female=1 × Character	-0.0171***	-0.00327***	-0.0127***	-0.00539***
	[0.00160]	[0.000917]	[0.000950]	[0.000763]
Female=1 × Writing/language	-0.00502***	-0.00294***	-0.00453***	-0.00275***
	[0.00140]	[0.000829]	[0.000972]	[0.000725]
Female=1 × Customer Service	-0.0271***	-0.0103***	-0.0185***	-0.0111***
	[0.00144]	[0.00106]	[0.000986]	[0.000820]
Female=1 × Management	0.0207***	-0.00403***	0.00320*	-0.00235*
	[0.00421]	[0.00117]	[0.00136]	[0.000924]
Female=1 × Financial	-0.0142***	-0.00991***	-0.0168***	-0.00827***
	[0.00206]	[0.00134]	[0.00195]	[0.00109]
Female=1 × Computer (general)	-0.00760***	-0.00181	-0.00607***	-0.00301***
	[0.00121]	[0.000928]	[0.000934]	[0.000731]
Female=1 × Computer (specific)	-0.00521***	-0.00149	-0.00365***	-0.00166*
	[0.00123]	[0.000935]	[0.000838]	[0.000722]
Parent=1	0.0791***	0.0571***	0.0651***	0.0525***
	[0.00207]	[0.00111]	[0.00122]	[0.00104]
Parent=1 × Female=1	-0.100***	-0.0601***	-0.0773***	-0.0550***
	[0.00395]	[0.00136]	[0.00158]	[0.00129]
R^2	0.480	0.636	0.568	0.672
N	850068	850068	850063	850063
Individual controls	Yes	Yes	Yes	Yes
Firm controls	Yes	Yes	Yes	Yes
Occupation FEs	No	Yes	No	Yes
Firm FEs	No	No	Yes	Yes

Notes: Individual controls and number of keywords: *parent dummy, parent*female interaction, age, age^2, years of experience, years of education, immigrant dummy, marriage dummy, part-time dummy, year FEs, start-month FEs, number of keywords.* Firm controls: *1-letter industry dummies, firm location, number of employees, a private sector dummy.* Occupation fixed effects: *See Appendix B.3.1 for details.* Firm fixed effects: *4682 firms in total.* Observations weighted by full-time equivalents. Cluster-robust standard errors in brackets, clustered at the firm*occupation*start-month level.

$^* p < 0.05$, $^{**} p < 0.01$, $^{***} p < 0.001$.

Table C.4: Continuous, standardised skill measures: ln(hourly wage) regressed on skills *with* gender interaction for subpopulations

	Dependent variable: ln(hourly wages)						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	All	Professionals	Large firms	Small firms	Private sector	Full-time	Whole year
Female=1	-0.0269***	-0.00995***	-0.0257***	-0.0513***	-0.0508***	-0.0324***	-0.0280***
	[0.000976]	[0.00139]	[0.00102]	[0.00316]	[0.00156]	[0.00118]	[0.00114]
Cognitive	0.00366***	0.00394***	0.00354***	0.00534***	0.00385***	0.00349***	0.00313***
	[0.000802]	[0.000962]	[0.000866]	[0.00137]	[0.00106]	[0.000872]	[0.000829]
Social	-0.00249**	-0.00369***	-0.00338***	0.00222*	0.000979	-0.00227**	-0.00248**
	[0.000764]	[0.00107]	[0.000857]	[0.00106]	[0.000963]	[0.000876]	[0.000803]
Character	0.00407***	-0.00173	0.00513***	-0.00129	0.00235*	0.00452***	0.00435***
	[0.000750]	[0.00117]	[0.000861]	[0.000998]	[0.000919]	[0.000831]	[0.000825]
Writing/language	0.00265***	0.00131	0.00305***	-0.00177	0.00313***	0.00335***	0.00412***
	[0.000796]	[0.00103]	[0.000888]	[0.00107]	[0.000908]	[0.000919]	[0.000873]
Customer Service	0.000889	0.00339*	0.000712	0.00211	-0.00104	0.00221**	0.000889
	[0.000781]	[0.00168]	[0.000858]	[0.00139]	[0.000804]	[0.000851]	[0.000853]
Management	0.00229*	0.00381***	0.00232*	0.00675***	0.00401***	0.00237*	0.00252**
	[0.000988]	[0.00103]	[0.00106]	[0.00180]	[0.00111]	[0.00107]	[0.000901]
Financial	0.00634***	0.00378**	0.00662***	0.00568*	0.00596***	0.00640***	0.00498***
	[0.00105]	[0.00116]	[0.00113]	[0.00228]	[0.00106]	[0.00107]	[0.000941]
Computer (general)	0.00125	0.00147	0.00123	0.00170	0.00102	0.00123	0.000740
	[0.000725]	[0.000926]	[0.000835]	[0.000876]	[0.000725]	[0.000758]	[0.000771]
Computer (specific)	-0.000911	0.000241	-0.00104	0.00142	-0.00165*	-0.000983	-0.000619
	[0.000697]	[0.000838]	[0.000768]	[0.00121]	[0.000800]	[0.000743]	[0.000705]
Female=1 × Cognitive	-0.00284***	-0.00465***	-0.00289***	-0.00395*	-0.00109	-0.00160	-0.00255**
	[0.000747]	[0.000891]	[0.000804]	[0.00160]	[0.00104]	[0.000818]	[0.000807]
Female=1 × Social	0.00338***	0.00460***	0.00395***	0.000749	-0.00237*	0.00477***	0.00331***
	[0.000756]	[0.00105]	[0.000843]	[0.00138]	[0.00101]	[0.000878]	[0.000819]
Female=1 × Character	-0.00539***	-0.000486	-0.00614***	-0.00118	-0.00128	-0.00693***	-0.00500***
	[0.000763]	[0.00119]	[0.000861]	[0.00129]	[0.000975]	[0.000908]	[0.000874]
Female=1 × Writing/language	-0.00275***	-0.00194*	-0.00292***	0.000787	-0.000324	-0.00360***	-0.00368***
	[0.000725]	[0.000940]	[0.000807]	[0.00123]	[0.000888]	[0.000891]	[0.000828]
Female=1 × Customer Service	-0.0111***	-0.0203***	-0.0110***	-0.00881***	-0.00584***	-0.0158***	-0.0120***
	[0.000820]	[0.00168]	[0.000891]	[0.00160]	[0.00100]	[0.000985]	[0.000944]
Female=1 × Management	-0.00235*	-0.00379***	-0.00225*	-0.00973***	-0.00628***	-0.00210*	-0.00265**
	[0.000924]	[0.00102]	[0.000988]	[0.00214]	[0.00114]	[0.00105]	[0.000949]
Female=1 × Financial	-0.00827***	-0.00709***	-0.00847***	-0.00691**	-0.00771***	-0.00811***	-0.00687***
	[0.00109]	[0.00109]	[0.00117]	[0.00254]	[0.00140]	[0.00113]	[0.00105]
Female=1 × Computer (general)	-0.00301***	-0.00376***	-0.00277***	-0.00564**	-0.00207*	-0.00329***	-0.00304***
	[0.000731]	[0.000958]	[0.000797]	[0.00192]	[0.000985]	[0.000801]	[0.000817]
Female=1 × Computer (specific)	-0.00166*	-0.00218*	-0.00148	-0.00225	-0.00180	-0.00155	-0.00158*
	[0.000722]	[0.000986]	[0.000788]	[0.00164]	[0.000928]	[0.000790]	[0.000785]
Parent=1	0.0525***	0.0529***	0.0533***	0.0403***	0.0565***	0.0503***	0.0533***
	[0.00104]	[0.00165]	[0.00110]	[0.00278]	[0.00142]	[0.00117]	[0.00116]
Parent=1 × Female=1	-0.0550***	-0.0592***	-0.0560***	-0.0373***	-0.0495***	-0.0507***	-0.0539***
	[0.00129]	[0.00182]	[0.00136]	[0.00365]	[0.00172]	[0.00150]	[0.00136]
R^2	0.672	0.647	0.674	0.675	0.665	0.716	0.672
N	850063	294971	791761	58188	362655	479577	512184
Individual controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Occupation FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Individual controls and number of keywords: *parent dummy, parent*female interaction, age, age^2, years of experience, years of education, immigrant dummy, marriage dummy, part-time dummy, year FEs, start-month FEs, number of keywords.* Firm controls: *1-letter industry dummies, firm location, number of employees, a private sector dummy.* Occupation fixed effects: *See Appendix B.3.1 for details.* Firm fixed effects: *4682 firms in total.* Observations weighted by full-time equivalents. Cluster-robust standard errors in brackets, clustered at the firm*occupation*start-month level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table C.5: Continuous, standardised skill measures: ln(hourly wage) regressed on skills, skill interactions, and gender interactions

	Dependent variable: ln(hourly wages)				
	(1) No interactions	(2) Cognitive and social	(3) Cognitive and computer(specific)	(4) Cognitive and computer(general)	(5) Cognitive and computer(both)
Female=1	-0.0269***	-0.0268***	-0.0268***	-0.0267***	-0.0267***
	[0.000976]	[0.000980]	[0.000981]	[0.000984]	[0.000986]
Cognitive	0.00366***	0.00379***	0.00370***	0.00388***	0.00386***
	[0.000802]	[0.000838]	[0.000827]	[0.000835]	[0.000846]
Social	-0.00249**	-0.00246**	-0.00248**	-0.00243**	-0.00243**
	[0.000764]	[0.000768]	[0.000764]	[0.000763]	[0.000763]
Character	0.00407***	0.00406***	0.00407***	0.00411***	0.00411***
	[0.000750]	[0.000751]	[0.000751]	[0.000751]	[0.000751]
Writing/language	0.00265***	0.00263***	0.00266***	0.00267***	0.00267***
	[0.000796]	[0.000794]	[0.000796]	[0.000797]	[0.000797]
Customer Service	0.000889	0.000885	0.000886	0.000899	0.000900
	[0.000781]	[0.000780]	[0.000781]	[0.000780]	[0.000780]
Management	0.00229*	0.00223*	0.00229*	0.00229*	0.00229*
	[0.000988]	[0.000988]	[0.000988]	[0.000988]	[0.000988]
Financial	0.00634***	0.00632***	0.00634***	0.00634***	0.00634***
	[0.00105]	[0.00105]	[0.00105]	[0.00105]	[0.00105]
Computer (general)	0.00125	0.00123	0.00126	0.00115	0.00115
	[0.000725]	[0.000725]	[0.000725]	[0.000724]	[0.000723]
Computer (specific)	-0.000911	-0.000915	-0.000885	-0.000848	-0.000864
	[0.000697]	[0.000697]	[0.000703]	[0.000700]	[0.000705]
Female=1 × Cognitive	-0.00284***	-0.00302***	-0.00284***	-0.00292***	-0.00291***
	[0.000747]	[0.000764]	[0.000760]	[0.000771]	[0.000777]
Female=1 × Social	0.00338***	0.00332***	0.00338***	0.00340***	0.00340***
	[0.000756]	[0.000761]	[0.000756]	[0.000756]	[0.000757]
Female=1 × Character	-0.00539***	-0.00537***	-0.00540***	-0.00540***	-0.00540***
	[0.000763]	[0.000763]	[0.000762]	[0.000763]	[0.000763]
Female=1 × Writing/language	-0.00275***	-0.00273***	-0.00275***	-0.00276***	-0.00276***
	[0.000725]	[0.000724]	[0.000725]	[0.000726]	[0.000726]
Female=1 × Customer Service	-0.0111***	-0.0111***	-0.0111***	-0.0111***	-0.0111***
	[0.000820]	[0.000820]	[0.000820]	[0.000819]	[0.000819]
Female=1 × Management	-0.00235*	-0.00227*	-0.00235*	-0.00233*	-0.00233*
	[0.000924]	[0.000927]	[0.000924]	[0.000925]	[0.000925]
Female=1 × Financial	-0.00827***	-0.00823***	-0.00827***	-0.00828***	-0.00828***
	[0.00109]	[0.00109]	[0.00109]	[0.00109]	[0.00109]
Female=1 × Computer (general)	-0.00301***	-0.00297***	-0.00298***	-0.00291***	-0.00291***
	[0.000731]	[0.000732]	[0.000731]	[0.000725]	[0.000727]
Female=1 × Computer (specific)	-0.00166*	-0.00166*	-0.00161*	-0.00153*	-0.00152*
	[0.000722]	[0.000722]	[0.000731]	[0.000728]	[0.000734]
Cognitive × Social		0.000509			
		[0.000505]			
Female=1 × Cognitive × Social		-0.000797			
		[0.000510]			
Cognitive × Computer (specific)			-0.000130		0.0000672
			[0.000320]		[0.000334]
Female=1 × Cognitive × Computer (specific)			-0.000252		-0.0000250
			[0.000413]		[0.000425]
Cognitive× Computer (general)				-0.000745	-0.000769
				[0.000396]	[0.000413]
Female=1 × Cognitive × Computer (general)				-0.00111*	-0.00110
				[0.000553]	[0.000577]
R^2	0.672	0.672	0.672	0.672	0.672
N	850063	850063	850063	850063	850063
Individual controls	Yes	Yes	Yes	Yes	Yes
Firm controls	Yes	Yes	Yes	Yes	Yes
Occupation FEs	Yes	Yes	Yes	Yes	Yes
Firm FEs	Yes	Yes	Yes	Yes	Yes

Notes: Individual controls and number of keywords: *parent dummy, parent*female interaction, age, age^2, years of experience, years of education, immigrant dummy, marriage dummy, part-time dummy, year FEs, start-month FEs, number of keywords.* Firm controls: *1-letter industry dummies, firm location, number of employees, a private sector dummy.* Occupation fixed effects: *See Appendix B.3.1 for details.* Firm fixed effects: *4682 firms in total.* Observations weighted by full-time equivalents. Cluster-robust standard errors in brackets, clustered at the firm*occupation*start-month level. ** $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.*

Appendix D. Online Appendix

D.1. Validation of skill measures

PIAAC skill use data from Denmark is used to validate the skill measures derived from the job vacancy data.[18] However, PIAAC data is only available from interviews conducted in 2011 and 2012, and with 4-digit DISCO08 codes. Since 6-digit DISCO08-codes are not available, the above-mentioned conversion to 228 time consistent occupational groups cannot be undertaken. Thus, for the validation exercise described below, I first instead limit my sample to job spells commencing in January 2010 or thereafter.

In PIAAC, skill use is measured in five discrete values, for example: Never=1, Less than once a month=2, Less than once a week but at least once a month=3, At least once a week but not every day=4, Every day=5. I rescale this to a measure between 0 and 1, and collapse to get mean skill use at the 4-digit occupational level.

Next, the PIAAC skill use measures are merged onto the estimation sample from above, and the correlations with the binary skill measures and standardised, continuous skill measures are reported in Table D.1 and Table D.2 respectively.

Generally, the correlation coefficients confirm that the skills measures derived from the job postings data are indicative of reported skill usage in PIAAC at the occupational level. For example, see the large correlation coefficient between "Problem solving - Complex problems" skill use in PIAAC and the "cognitive" skill measures derived from keywords in job posts. However, notice that there are some inconsistencies between Table D.1 and Table D.2. For example, for "writing/language" skills, the sign of the correlation coefficients with the various "Literacy" skill use measures from PIAAC is generally positive when consider the binary skill measures (Table D.1), but not when considering the continuous skill measures (Table D.2). Thus, the binary skill measures seem to capture skill use better, possibly because of the skill measures are suffer from noise when the fraction of keywords indicative of certain skills is very low. At the same time, coefficients on the binary skill measures are also easier to interpret in regression analyses, and thus, the main parts of this paper report results using the binary skill measures, although the results generally are robust to the use of the continuous skills measures, see Appendix C

[18] For more details on PIAAC, see https://www.oecd.org/skills/piaac/ or, for example, Allen, Levels, and Van Der Velden (2013)

Table D.1: Correlations between binary skill measures and PIAAC skill use measures

	(1) Cognitive	(2) Social	(3) Character	(4) Writing/language	(5) Customer Service	(6) Management	(7) Financial	(8) Computer (general)	(9) Computer (specific)
Time cooperating with co-workers	0.0262***	0.0725***	-0.000626	-0.0311***	-0.111***	0.0859***	0.00400**	0.0571***	-0.0248***
How often - Sharing work-related info	0.139***	0.149***	-0.113***	0.0331***	-0.136***	0.137***	0.0769***	0.156***	0.0415***
How often - Teaching people	0.216***	0.180***	-0.190***	0.0903***	-0.229***	0.188***	0.0420***	0.135***	0.0614***
How often - Selling	-0.0487***	-0.0800***	0.159***	-0.149***	0.579***	-0.0682***	0.0339***	-0.00533***	0.00256*
How often - Advising people	0.157***	0.207***	-0.112***	-0.0117***	0.0999***	0.176***	0.101***	0.0908***	-0.00202
How often - Planning own activities	0.229***	0.224***	-0.201***	0.146***	-0.231***	0.224***	0.179***	0.121***	0.130***
How often - Planning others activities	0.120***	0.103***	-0.144***	0.0599***	-0.171***	0.181***	0.0751***	0.0999***	0.0252***
How often - Organising own time	0.282***	0.171***	-0.199***	0.123***	-0.153***	0.266***	0.270***	0.190***	0.181***
How often - Influencing people	0.165***	0.207***	-0.112***	0.0230***	0.0223***	0.166***	0.0805***	0.0974***	0.0326***
How often - Negotiating with people	0.188***	0.161***	-0.0978***	0.0614***	0.106***	0.190***	0.193***	0.103***	0.0713***
Problem solving - Simple problems	0.212***	0.195***	-0.168***	0.0419***	-0.106***	0.174***	0.111***	0.193***	0.102***
Problem solving - Complex problems	0.343***	0.196***	-0.240***	0.141***	-0.170***	0.271***	0.242***	0.272***	0.232***
How often - Working physically for long	-0.333***	-0.119***	0.199***	-0.190***	0.0592***	-0.271***	-0.348***	-0.292***	-0.252***
How often - Using hands or fingers	-0.165***	-0.0128***	0.0802***	-0.115***	-0.106***	-0.105***	-0.203***	-0.171***	-0.171***
Literacy - Read directions or instructions	0.238***	0.165***	-0.212***	0.0466***	-0.152***	0.178***	0.146***	0.182***	0.0674***
Literacy - Read letters memos or mails	0.329***	0.243***	-0.253***	0.154***	-0.166***	0.297***	0.269***	0.254***	0.155***
Literacy - Read newspapers or magazines	0.312***	0.164***	-0.206***	0.116***	-0.0308***	0.248***	0.251***	0.261***	0.182***
Literacy - Read professional journals or publications	0.353***	0.157***	-0.264***	0.140***	-0.187***	0.282***	0.243***	0.244***	0.143***
Literacy - Read books	0.214***	0.120***	-0.163***	0.128***	-0.283***	0.107***	0.0126***	0.127***	0.0560***
Literacy - Read manuals or reference materials	0.288***	0.121***	-0.217***	0.0427***	-0.115***	0.209***	0.154***	0.260***	0.129***
Literacy - Read financial statements	0.0870***	0.0155***	0.0449***	-0.00703***	0.288***	0.0998***	0.254***	0.0921***	0.0925***
Literacy - Read diagrams maps or schematics	0.407***	0.0436***	-0.236***	0.113***	-0.0403***	0.269***	0.324***	0.370***	0.266***
Literacy - Write letters memos or mails	0.316***	0.246***	-0.264***	0.165***	-0.207***	0.299***	0.285***	0.254***	0.158***
Literacy - Write articles	0.209***	0.0737***	-0.179***	0.137***	-0.100***	0.203***	0.199***	0.207***	0.154***
Literacy - Write reports	0.198***	0.225***	-0.205***	0.0394***	-0.246***	0.182***	0.0803***	0.0738***	0.0161***
Literacy - Fill in forms	0.159***	0.0940***	-0.118***	-0.0363***	0.0247***	0.188***	0.166***	0.119***	-0.0208***
Numeracy - How often - Calculating costs or budgets	0.0610***	-0.0502***	0.103***	-0.0537***	0.439***	0.0266***	0.187***	0.0934***	0.0896***
Numeracy - How often - Use or calculate fractions or percentage	0.320***	-0.00366**	-0.0907***	0.0334***	0.201***	0.189***	0.309***	0.294***	0.220***
Numeracy - How often - Use a calculator	0.267***	-0.0155***	-0.0575***	0.0000838	0.276***	0.176***	0.313***	0.278***	0.190***
Numeracy - How often - Prepare charts graphs or tables	0.307***	0.0660***	-0.184***	0.138***	-0.113***	0.235***	0.250***	0.269***	0.256***
Numeracy - How often - Use simple algebra or formulas	0.368***	-0.0102***	-0.157***	0.0688***	0.0209***	0.213***	0.289***	0.329***	0.266***
Numeracy - How often - Use advanced math or statistics	0.326***	-0.0604***	-0.165***	0.120***	-0.0870***	0.165***	0.234***	0.323***	0.280***
ICT - Internet - How often - For mail	0.351***	0.224***	-0.257***	0.180***	-0.172***	0.324***	0.303***	0.278***	0.187***
ICT - Internet - How often - Work related info	0.376***	0.211***	-0.255***	0.167***	-0.131***	0.324***	0.298***	0.291***	0.208***
ICT - Internet - How often - Conduct transactions	0.218***	0.0452***	-0.0663***	0.0659***	0.152***	0.157***	0.279***	0.226***	0.223***
ICT - Computer - How often - Spreadsheets	0.293***	0.0710***	-0.146***	0.160***	0.0153***	0.247***	0.345***	0.292***	0.281***
ICT - Computer - How often - Word	0.343***	0.202***	-0.258***	0.202***	-0.177***	0.314***	0.305***	0.266***	0.196***
ICT - Computer - How often - Programming language	0.257***	0.00397**	-0.112***	0.0952***	-0.0272***	0.123***	0.164***	0.301***	0.404***
ICT - Computer - How often - Real-time discussions	0.289***	0.0611***	-0.150***	0.163***	-0.0106***	0.168***	0.249***	0.313***	0.398***
Observations	636845	636845	636845	636845	636845	636845	636845	636845	636845

Note: Observations weighted by full-time equivalents.

Table D.2: Correlations between standardised, continuous skill measures and PIAAC skill use measures

	(1) Cognitive	(2) Social	(3) Character	(4) Writing/language	(5) Customer Service	(6) Management	(7) Financial	(8) Computer (general)	(9) Computer (specific)
Time cooperating with co-workers	-0.0107***	0.0305***	0.00824***	-0.133***	-0.138***	0.0921***	-0.0690***	0.0176***	0.0145***
How often - Sharing work-related info	0.117***	0.125***	-0.106***	-0.124***	-0.179***	0.139***	-0.00218	0.0718***	0.0749***
How often - Teaching people	0.197***	0.178***	-0.192***	-0.0826***	-0.286***	0.131***	-0.0240***	0.0372***	0.0762***
How often - Selling	-0.0820***	-0.0308***	0.125***	-0.0434***	0.549***	-0.0725***	0.0621***	0.0242***	-0.0204***
How often - Advising people	0.106***	0.217***	-0.111***	-0.152***	0.0224***	0.143***	0.0465***	0.000614	0.0103***
How often - Planning own activities	0.226***	0.252***	-0.164***	0.000796	-0.215***	0.199***	0.171***	0.0707***	0.117***
How often - Planning others activities	0.110***	0.124***	-0.140***	-0.0472***	-0.192***	0.175***	0.00614***	0.0212***	0.0278***
How often - Organising own time	0.280***	0.191***	-0.183***	-0.0316***	-0.112***	0.262***	0.239***	0.138***	0.168***
How often - Influencing people	0.126***	0.216***	-0.122***	-0.142***	-0.0598***	0.127***	0.0122***	0.00980***	0.0427***
How often - Negotiating with people	0.161***	0.191***	-0.0978***	-0.0595***	0.0869***	0.186***	0.150***	0.0250***	0.0581***
Problem solving - Simple problems	0.181***	0.188***	-0.182***	-0.136***	-0.166***	0.162***	0.0493***	0.114***	0.121***
Problem solving - Complex problems	0.342***	0.208***	-0.243***	-0.0525***	-0.170***	0.272***	0.166***	0.194***	0.227***
How often - Working physically for long	-0.341***	-0.145***	0.212***	-0.0749***	0.00254*	-0.297***	-0.334***	-0.191***	-0.232***
How often - Using hands or fingers	-0.202***	-0.0536***	0.111***	-0.0911***	-0.128***	-0.160***	-0.197***	-0.100***	-0.137***
Literacy - Read directions or instructions	0.218***	0.145***	-0.193***	-0.114***	-0.172***	0.162***	0.0629***	0.0965***	0.0943***
Literacy - Read letters memos or mails	0.307***	0.251***	-0.236***	-0.0436***	-0.166***	0.273***	0.218***	0.138***	0.159***
Literacy - Read newspapers or magazines	0.294***	0.185***	-0.219***	-0.0377***	-0.0430***	0.247***	0.213***	0.148***	0.173***
Literacy - Read professional journals or publications	0.347***	0.173***	-0.292***	-0.0704***	-0.213***	0.249***	0.167***	0.108***	0.134***
Literacy - Read books	0.213***	0.104***	-0.209***	-0.0484***	-0.353***	0.0576***	-0.0322***	0.00568***	0.0623***
Literacy - Read manuals or reference materials	0.273***	0.105***	-0.219***	-0.105***	-0.136***	0.181***	0.0419***	0.187***	0.155***
Literacy - Read financial statements	0.0550***	0.0600***	0.0390***	0.0317***	0.338***	0.135***	0.346***	0.0763***	0.0568***
Literacy - Read diagrams maps or schematics	0.415***	0.0542***	-0.266***	0.00388**	-0.00636***	0.285***	0.254***	0.287***	0.249***
Literacy - Write letters memos or mails	0.299***	0.252***	-0.245***	-0.0377***	-0.201***	0.287***	0.230***	0.134***	0.162***
Literacy - Write articles	0.207***	0.105***	-0.208***	0.0122***	-0.0982***	0.224***	0.134***	0.117***	0.135***
Literacy - Write reports	0.170***	0.205***	-0.186***	-0.157***	-0.284***	0.130***	0.00431***	0.0226***	0.0329***
Literacy - Fill in forms	0.111***	0.0725***	-0.107***	-0.138***	0.0190***	0.157***	0.105***	0.0506***	0.00549***
Numeracy - How often - Calculating costs or budgets	0.0415***	0.00255*	0.0750***	0.0262***	0.458***	0.0544***	0.256***	0.0936***	0.0536***
Numeracy - How often - Use or calculate fractions or percentage	0.319***	0.0137***	-0.120***	-0.00712***	0.228***	0.193***	0.320***	0.226***	0.197***
Numeracy - How often - Use a calculator	0.259***	0.0160***	-0.0798***	-0.00900***	0.309***	0.197***	0.327***	0.233***	0.170***
Numeracy - How often - Prepare charts graphs or tables	0.332***	0.0936***	-0.199***	0.0288***	-0.0783***	0.227***	0.202***	0.223***	0.234***
Numeracy - How often - Use simple algebra or formulas	0.381***	-0.000416	-0.185***	0.00376**	0.0605***	0.210***	0.272***	0.270***	0.250***
Numeracy - How often - Use advanced math or statistics	0.390***	-0.0315***	-0.220***	0.0375***	-0.0645***	0.147***	0.168***	0.257***	0.251***
ICT - Internet - How often - For mail	0.332***	0.233***	-0.239***	-0.00506***	-0.150***	0.306***	0.254***	0.161***	0.187***
ICT - Internet - How often - Work related info	0.348***	0.215***	-0.247***	-0.0185***	-0.115***	0.298***	0.239***	0.172***	0.206***
ICT - Internet - How often - Conduct transactions	0.188***	0.0699***	-0.0697***	0.0548***	0.207***	0.183***	0.331***	0.192***	0.199***
ICT - Computer - How often - Spreadsheets	0.302***	0.106***	-0.148***	0.0898***	0.0859***	0.273***	0.330***	0.244***	0.254***
ICT - Computer - How often - Word	0.337***	0.226***	-0.249***	0.0241***	-0.153***	0.307***	0.265***	0.149***	0.187***
ICT - Computer - How often - Programming language	0.283***	0.00551***	-0.121***	0.0594***	0.0273***	0.124***	0.105***	0.334***	0.401***
ICT - Computer - How often - Real-time discussions	0.308***	0.0866***	-0.162***	0.105***	0.0410***	0.198***	0.220***	0.300***	0.375***
Observations	636845	636845	636845	636845	636845	636845	636845	636845	636845

Note: Observations weighted by full-time equivalents.

D.2. DISCO-crosswalk

The crosswalk between 4-digit DISCO88 codes and 6-digit DISCO08 codes can be found here: https://www.dropbox.com/s/xmvzwpmwvqnntft/disco-crosswalk.pdf?dl=0. The results of applying the crosswalk is 228 time consistent occupational groups.

Chapter 2

Income Effects and Labour Supply: Evidence from a Child Benefits Reform

Income Effects and Labour Supply: Evidence from a Child Benefits Reform*

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This version August 30, 2021

Abstract

In this paper, we exploit a unique and unexpected reform to the child benefit system in Denmark to assess the effects of child benefits on parental labour supply. A cap on child benefit payments in 2011 led to a non-negligible reduction in child benefits for larger families with young children. The differential impact of this policy shift represents an opportunity to assess the causal impact of child benefit programmes on the labour supply of mothers and fathers. As a new government was elected in late 2011, the reform was repealed after being in place for a single year, which allows us to assess long term effects of a temporary income shock that was perceived to be permanent.

We find that a reduction in child benefits leads to a large increase in the labour supply of mothers; the effect on fathers is much smaller. Both mothers and fathers respond to the policy at the intensive margin, but the strongest response is from mothers at the extensive margin. The majority of the effects can be ascribed to fertility responses, but even after controlling for fertility-related family characteristics, we find significant increases in labour supply after the introduction of the reform. We confirm this result by using data on parents' consultations with doctors regarding sterilisation, a common procedure in Denmark. Lastly, the labour supply effects of the reform are generally sustained for at least 3 years after its repeal.

*This paper has benefited substantially from discussions with Fane Groes, Herdis Steingrimsdottir, and Caroline Hoxby.

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1 Introduction

Financial assistance to families with young children is a common policy adopted across many developed countries to encourage fertility, improve well-being and enhance the long-term opportunities of children. Child or family benefits are cash transfers to families with dependent children and are often independent of income and labour market status. Child benefits thus represents an alternative to conditional or in-work benefits, such as the federal Earned Income Tax Credit (EITC) in the United States. Child benefits are a major part of public spending in most European countries, and spending on family benefits amounts on average to 1.16% of GDP across OECD countries in 2017.[1] There are many potential motivations for government spending on child benefits, such as the well-being of families, opportunities for children, and effects on fertility. Recently, the US child tax credit has been expanded to include monthly allowances to families with children, and thus, discussions of the effects of unconditional child benefits has reemerged (Financial Times, 2021).

Child benefits can be viewed as a subsidy to parents enabling them to limit their labour supply. We are particularly interested in this effect. Limiting parental labour supply may be beneficial for certain (child) outcomes, but it may also reinforce less desirable outcomes, such as the child pay penalty and the gender pay gap if labour supply responses are more pronounced for mothers than for fathers (see e.g. Kleven et al., 2019; Blau & Kahn, 2017).

A simple economic analysis would predict that higher levels of child benefits would increase fertility and decrease labour supply among parents. But despite their prevalence, relatively little is known about the impact of unconditional child benefits on key margins of fertility, and particularly, of labour supply. Most of the current evidence on the effects of child benefits rely on difference-in-differences analyses comparing families with and without children, although these families may differ along many other unobservable dimensions.

We exploit a unique and unexpected reform to the child benefit system in Denmark to assess the effects on maternal and paternal labour supply. A cap on child benefit payments in 2011 led to a non-negligible reduction in child benefits for larger families with young children, but did not directly affect families with one or two children. The differential impact of this policy shift represents an opportunity to assess the causal impact of child benefit programmes on the labour supply of mothers and fathers respectively. Importantly, the cap on child benefits was not directly related to families' levels of income, but only to the number and age composition of their children. Thus, we can compare families with different compositions of children and not rely on a comparison between families with and without children. Furthermore, we can control for individual fixed effects due to the population-level Danish employment registers. Although the cap on child benefits was announced as and intended to be permanent, a new government was elected in November 2011, and the cap was repealed from 2012 onward.

[1] https://data.oecd.org/socialexp/family-benefits-public-spending.htm

We find that both women and men respond to a reduction in child benefits at the intensive margin and increase the number of hours worked per month if employed. However, the strongest response to the reduction in child benefits is at the extensive margin for mothers for whom we find large increases in participation rates. A large share of the estimated effects can be ascribed to fertility responses, but even after controlling for fertility related family characteristics and limiting our sample to sterilised parents, we find relatively large and significant labour supply responses, most noticeably at the extensive margin for mothers. Finally, the labour supply effects of the reform generally remain three years after its repeal. Thus, we find evidence of long term effects of the temporary income shock that was perceived to be permanent, which can be explained by labour market entry/switching costs or by increased uncertainty about future child benefit payments.

The paper is structured as follows: In the next section, we consider related literature. In section 3, we describe the Danish child benefits system and the 2011-cap in detail. In section 4, we outline the hypotheses, we test in the following sections. In section 5, we describe our data and define our treatment and control groups. In section 6, we outline our empirical strategy. In section 7, we report our estimated effects on aggregate labour supply, and in section 8 we split up the responses to the reform into adjustments at the extensive and intensive margins of labour supply. Finally, in section 9, we conclude.

2 Literature

Our analysis of the Danish 2011-cap on child benefits contributes to at least two large literatures. Firstly, our analysis relates to the specific literature on the effects of child benefits, family cash transfers, and family tax credits on labour market outcomes, e.g. the many papers on the EITC in the US and the 1996-reform of child benefits in Germany. Secondly, the unexpected income loss for the affected families relate to the large and general body of studies on income effects on, e.g., labour supply. Importantly, the income shock we study in this paper is not directly related to labour market status and labour market income. In the following, we focus on the literature specifically on child benefits.

Labour supply effects of conditional family or child benefits are well-documented. For example, evaluations of the effects of the US EITC show that single mothers' labour supply increase with the introduction and expansions of the tax credits (Kleven, 2019; Eissa & Hoynes, 2006; Eissa & Liebman, 1996). For example, by comparing single mothers with single women without children, Eissa & Liebman (1996) find that single mothers increase their labour supply at the extensive margin in response to expansions of the EITC in the late 1980s, but they find no effects on the intensive margin. Evaluations of Working Families Tax Credit (WFTC) in the UK show similar results, see e.g. Francesconi & van der Klaauw (2007). Thus, it appears well-established that the in-work child subsidies positively affect labour supply of mothers, or at least, single mothers. Eissa & Hoynes (2004) consider the labour supply responses of married couples to EITC expansions between 1984 and 1996. They find that women decrease their labour supply at the extensive margin, and the labour supply responses of their spouses do not offset this effect. Thus, Eissa & Hoynes (2004, p. 1931) conclude that *"the EITC is effectively subsidizing married mothers to stay home..."* Using detailed administrative data from California, Hotz & Scholz (2006) again consider the labour supply effects of the EITC. Hotz & Scholz (2006) exploit variation in the EITC by the number of children, and they control for family fixed effects. They find that expansions of the EITC increase family labour supply.

In addition to the in-work child benefits or tax credits, many countries also pay unconditional child benefits to families with children. Only few studies have evaluated the effects of these – very expensive – policies. For example, Hener (2016) and Tamm (2010) consider the effects of the 1996-increase in child benefit payments in Germany. Using a difference-in-differences setup, Hener (2016) and Tamm (2010) compare couples with and without children. Tamm (2010) finds that mothers decrease their labour supply on the intensive margin after the increase in child benefits while fathers' labour supply is unaffected. Hener (2016) points out that the policy's effectiveness in improving families' financial situation is limited while the strain on public finances are amplified by the behavioural response to the increase in child benefits as the resulting decrease in maternal labour supply reduces tax payments. Nevertheless, Raschke (2016) exploits the same reform to show that families' expenditures on food increase after the increase in child benefits. In addition to the German reform, González (2013) study the introduction of an unconditional child benefit in Spain, namely one-time payment of €2,500 after birth. González (2013) find positive fertility effects, no effects on child expenditures, and negative labour supply responses for mothers.

In comparison to the existing papers on the effects of unconditional child benefits, our analysis differs in at least the following ways: 1) We study a cap on child benefit payments, not an increase in or an introduction of child benefits; 2) The cap on child benefits only affect a subgroup of families with children. Therefore, we can use the non-affected families with children as a control group, rather than childless couples, and we can control for family characteristics, such as number of children and the age of youngest child; 3) We are able to isolate the income effect and shut down the fertility response by looking only at families where at least one parent have consulted a doctor regarding sterilisation prior to the reform; 4) The 2011-cap on child benefits was repealed after being in place for a single year, which allows us to assess long term effects of a temporary income shock that was perceived to be permanent; 5) More detailed Danish data allow us to control for individual fixed effects, and we can further analyse mechanisms, heterogeneity, and timing of policy responses.

In an unpublished note, Almlund (2018) discusses fertility effects of the 2011-reform in child

Table 1: Yearly per-child benefit levels in 2011

Age of child:	0-2	3-6	7-17
Annual payment (DKK)	16,992	13,452	10,584

benefits in Denmark. Almlund (2018) finds that the reduction in child benefits significantly decreases the probability of having a third or higher order child. Our focus is on the labour supply effects of the reform, but fertility is, of course, a crucial determinant of labour supply. Therefore, we also separate the labour supply responses from the fertility response by controlling for family characteristics and by exploiting data on medical consultations regarding sterilisation, a common procedure in Denmark.

3 Child benefits in Denmark

In the following sections, we briefly outline the Danish child benefits system, the 2011 reform and its repeal, although further details are reserved to Appendix A.

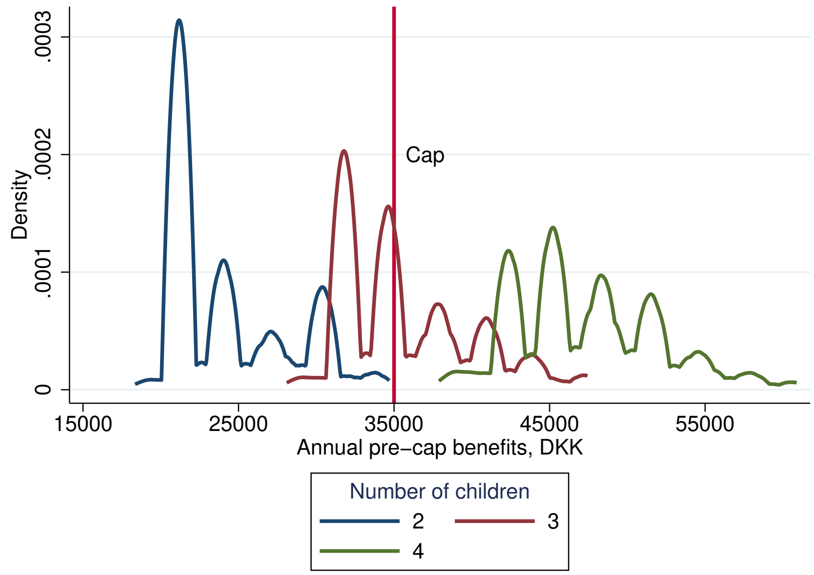
Since 1986, families with dependent children have received child benefits from the government in Denmark. The amount of child benefits paid to each family depend on the number of children as well as on the age of each child, with younger children allocated the greatest benefit level. Child benefits are paid quarterly directly to the child's mother, or father if no mother present. The first payment is made at the beginning of the next quarter following a child's birth.[2] 2011 per-child benefits are listed in Table 1. For families with multiple children, the per-child benefits are simply added together. For example, a family with three children of ages 1, 5, and 8 would receive $16,992 + 13,452 + 10,584$ DKK $= 41,028$ DKK in annual child benefits in 2011. Importantly, child benefits are not subject to income taxation in Denmark, where tax rates otherwise are relatively high. Figure 1 shows the distributions of child benefit payments in 2011 if the cap on child benefits described below had not been introduced.

3.1 Reform

In May 2010, in response to strong pressure on public finances after the global financial crisis, it was announced that there would be introduced a cap of 35,000 DKK (2011-level, approx 5,000 USD) on total child benefits received by each family. The policy affected child benefit payments from January 2011 and onwards. The policy is estimated to have affected 50,000 families. To smooth the income shock, child benefits were reduced evenly over 3 years. Furthermore, the policy included a maximum cut of approximately 12,000 DKK per year; a maximum which was

[2]Specifically, all child benefit payments are made on 20 January, 20 April, 20 July, and 20 October every year.

Figure 1: Distributions of yearly child benefit payments

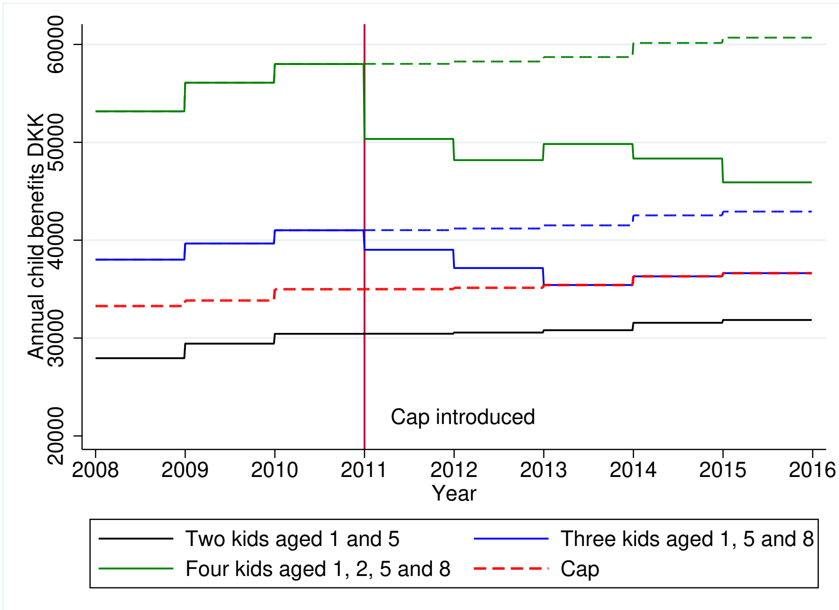


Notes: Annual pre-cap benefits in 2011. Cap at DKK 35,000 indicated. Although child benefits generally are paid in discrete amounts (see Table 1), in the quarter a child turns 18, child benefits are cut proportionally to the number of days under 18, which smooths the distribution. Observations below/above 7th/99th percentiles dropped due confidentiality restrictions on Danish register data. Epanechnikov kernel density, bandwidth = 500.

gradually increased over the coming years (see details in Appendix A). Finally, the reduction of child benefits in 2011 is generally proportional to the total expected future loss of benefits for each family in the following years.

The 35,000 DKK-cap on child benefits did not directly affect one-child families. Even though the reform did not directly affect the financial situation of two child families, it did change the marginal child benefit received for having an additional child for two-child families and above. In other words, two child families would receive a lower amount of child benefits for a potential third child post reform. The 35,000 DKK-cap on child benefits changed the unearned income for three-child families with young kids, specifically for families with at least one child under the age of 3 or at least two children under the age of 7 years old. The reform changed the unearned income for all families with four or more kids. Figure 2 shows the total child benefits received for families of different structures from 2008 to 2016 after the introduction of the reform. Figure C.11 in Appendix C shows the distribution of child benefits losses for families with different numbers of children.

Figure 2: Introduction of reform

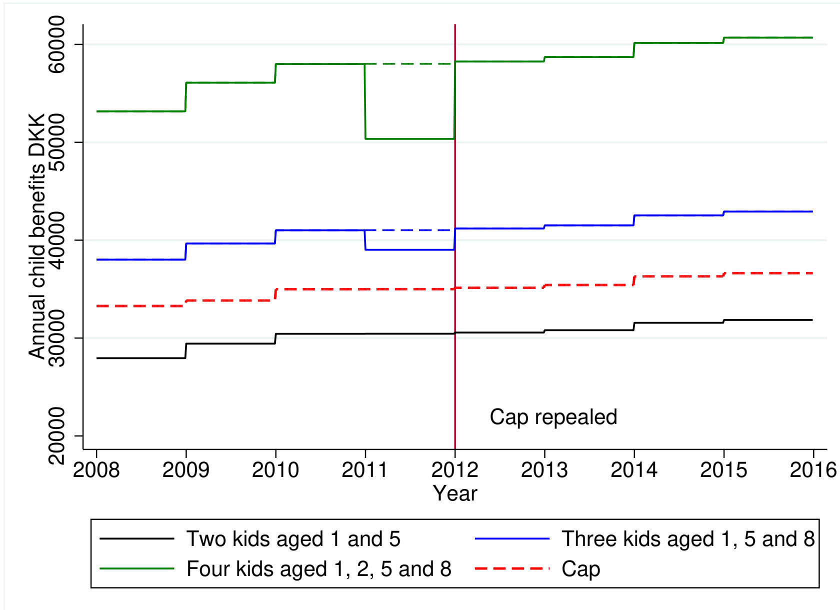


Notes: DKK 35,000 threshold introduced in 2011 in dashed red, counterfactuals are dashed. Child benefits level are inflation-adjusted yearly. Policy given as announced in 2010. Notice the gradual (and non-linear) phase-in of the reform; it would be fully phased-in by 2020 (see details in Appendix A). Child age composition is fixed across years - we compare the effect of the reform on families with children of similar ages across years.

3.2 Repeal

At the end of November 2011, a newly elected government announced that from 2012, the cap would be abolished; it was highlighted as damaging to large families. Therefore, the loss in child benefits for those with large families was ultimately restricted to the year 2011, with 2012 payments returning approximately to 2010 levels. Figure 3 illustrates both the introduction and repeal of the reform for various family compositions. See further details on the Danish child benefits system, and the 2011-reform and its repeal in Appendix A.

Figure 3: Repeal of reform



Notes: DKK 35,000 threshold introduced in 2011 in dashed red, counterfactuals are dashed. Child benefits level are inflation-adjusted yearly. Policy given as announced in 2010, and repeal as announced in 2011. Child age composition is fixed across years - we compare the effect of the reform on families with children of similar ages across years.

4 Hypotheses

In Appendix B, we outline a simple, static model in which parents derive utility from having children, from consumption, and from leisure. Children are associated with a financial cost, which is in part mitigated by the child benefit system.

The simple model has a number of key implications. The introduction of a cap on child benefit is predicted to:

1. Increase labour supply at the intensive margin for individuals in work with large numbers of children.

2. Increase labour supply at the extensive margin for individuals not working with large numbers of children. These effects may persist after the repeal of the reform due to switching costs.

3. Not affect individuals with few children.

4. Reduce fertility among individuals with large numbers of children.

In the sections that follow below, we will test the predictions of the model set out here. We are particularly interested in predictions 1. to 3.

Although the cap on child benefits was intended to be permanent when it was announced and introduced, it was repealed after being in place for just a year as a new government was elected. Despite the repeal of the cap, however, there are at least two reasons for expecting permanent effects of the reform.

Firstly, if we consider policy responses on the extensive margin, we may find that parents enter the labour market and carry the costs of entry when their non-market income decreases as child benefits are capped (Cogan, 1981). If parents also face a cost when exiting the labour market, the parents are likely to remain in the labour market, even after the repeal of the reform. We model this as a switching cost in the model outlined in Appendix B.

Secondly, the reform introduced uncertainty about the future levels of child benefits. Prior to the 2011-cap, child benefits in Denmark had not been subject to any substantial cuts since their introduction, only increases. With the 2011-cap, child benefits suddenly attracted (negative) political attention, and parents could likely be subject to another cut in benefits if the balance of power in the parliament shifted again. If parents are risk averse, uncertainty about future levels of child benefits are likely to affect labour supply at both the intensive and extensive margins in the long run for the families that were subject to the 2011-cap.

5 Data, sample selection, treatment and control groups

5.1 Data and sample selection

We construct a balanced panel with monthly observations of women and men covering the period September 2008 to December 2014. We exclude observations prior to September 2008 as a small change in the amounts of child benefits was implemented earlier in 2008. From the Danish register data (BEF, BFL, OF), we construct a monthly dataset with individual-level data on number of children, age of children, age, gender, earnings, immigrant background, parental leave, hours worked etc. Note that we exclude immigrants from our estimation sample. In addition, we have annual data on marital status and cohabiting partners (from BEF).

From Danish register data (BOBO/BOTI), we also have data on child benefits payments, but these registers only include data on payouts from the fourth quarter of the years 2009-2014. From the population registers, however, we can construct child benefits payments based on each family's number of children and the children's ages. Importantly, we can validate our measure of child benefits with the fourth quarter register data, and we can exclude families who receive irregular child benefits payments, e.g. because one or more children are in foster care.

Lastly, we obtain data on consultations with doctors regarding sterilisation procedures (from 1994 and onwards from LPRADM, SYSI, SSSY). We construct monthly dummies that equal one if ever having seen a doctor regarding sterilisation. Sterilisation is a relatively common procedure to undergo in Denmark after finalising fertility (see Figure C.21 in Appendix C).

We impose a number of sample selection criteria, and exclude people who are:

- Less than 25 years old by September 2008
- 60 years old or older by December 2014
- Paid irregular child benefits (e.g. because a child is in foster care)
- Immigrants
- Self-employed (as we do not observe hours worked for this group)

After defining treatment and control groups in the following section, we comment on the relevant summary statistics of the various samples.

5.2 Defining treatment and control groups

We group our families into those who were affected by the policy (treatment group) in the first two quarters of 2011, i.e. the families that experienced a strictly positive reduction in child benefits due the policy, and those who were not affected by the policy (control group) in the first two quarters of 2011. We define the treatment/control groups based on the first two quarters of 2011 to avoid selection in and out of the treatment group. Child benefits payments in a given quarter is based on the number and ages of the children in a family in the *previous* quarter. As the child benefit reform was announced in late May 2010, children conceived before the announcement of the policy may have been born until March 2011, and thus, affect the child benefits payments in the second quarter of 2011. In other words, children born later than March 2011 will almost definitely have been conceived after the announcement of the reform, and they will affect child benefits payments from the third quarter of 2011 and thereafter.

In choosing our treatment and control groups, we want to select groups that are as similar as possible in all ways except for their treatment under the policy. In general, the policy affected families with more children and with younger children. Families with 3 children under 18 are affected by the policy if they have at least one infant (0-2 years old) or two children in the age bracket 3-6 years. All families with 4 children under 18 are affected by the policy. In order to have young children in our control group after the policy introduction, we can therefore not limit our sample to families with 3 children or more under 18. Instead, we limit our analysis to the individuals that in January 2011:

- Had at least 2 children under 18 [3]
- Had no more than 4 children in total

[3] We also require that both children were registered in the Danish population register BEF in 2011 (or 2012 for children born in 2011) with the relevant parent ID.

- Were married or in a cohabiting couple

Thus, we have young children in both control and treatments groups, and 4-child families are also present in the control group if at least one child is older than 18.

After we impose these additional selection criteria, we balance the panel, and exclude people who are not present in the Danish population registers and in our sample for the entire sample period, September 2008 to December 2014. We are left with 28,968 treated women and 22,477 treated men. The difference in sample size between women and men is a result of the sample selection criteria: Compared to women, men are more likely to be self-employed, and men are typically older when they have children.

We report summary statistics for the various samples in Tables C.8 to C.11 in Appendix C. Generally, both the treated women and men are younger, have less labour market experience, are better educated, and are more likely to have twins or triplets when compared to the control groups. On average, the youngest child is also younger for the treatment group. These differences in characteristics can be explained by the fact that our treatment is related to the age composition of a family's children – a family is more likely to be affected by the cap on child benefits if the have young children. In our empirical strategy, which we outline in the following section, we specifically address these differences in characteristics between our treatment and control group, and next, we compare pre-trends in outcomes for the different groups. Notice that the "Benefits lost in 2011" is not equal to zero for the control group. This is because we define our treatment group as those who were affected by the policy in the first two quarters of 2011 in order avoid selection into the treatment group, which is further detailed above.

6 Empirical strategy

We would like to test whether the temporary introduction of the child benefit cap affected labour supply, and how the labour supply responses were affected by fertility.

Our first set of specifications will simply group our families into those who had their child benefits reduced due to the reform (treatment group) and those who were not affected by the reform (control group) as described above. By classifying families in this way, we can estimate the effect of the reform by using a binary treatment indicator. This is a simplification of the reform, but it is one which is particularly useful for a graphical analysis and provide some first evidence of the direction of a potential effect. We will then extend our analysis and consider the treatment as continuous.For all the analyses below, we run the analyses separately for women and men and compare the results.

6.1 Binary treatment

We undertake two sets of analyses using our binary treatment indicator. First, we analyse treatment dynamics using a Generalised Difference-in-Differences setup, and determine whether or not parallel trends are a reasonable assumption for our further analyses. Second, we apply a standard Difference-in-Differences model, but with three time periods, namely: 1) before introduction of reform 2) after introduction of reform 3) after repeal of reform.

6.1.1 Generalised Difference-in-Differences

$$Y_{it} = \beta_0 + \sum_{j=0}^{T} \beta_1^j 1[j=t] + \sum_{j=0}^{T} \beta_2^j 1[j=t] * W_i + \beta_3 X_{it} + m_t + \alpha_i + \epsilon_{it} \tag{1}$$

Where:

- Y_{it} is an outcome of interest for person i in time t (e.g. hours worked or employment)
- W_i indicates whether i is in the treatment group (affected by benefit cut in Q1 and/or Q2 2011)
- X_{it} is a set of time-varying family controls
- m_t are 12 calendar month fixed effects (January to December, i.e. not year*month fixed effects)
- α_i are individual fixed effects

Note that a treatment group indicator without a time interaction is not included, as treatment group membership is fixed over time, and thus, absorbed by the individual fixed effects. We cluster standard errors at the individual level.

6.1.2 Difference-in-Differences

After inspecting pre-trends both in the actual levels of hours worked of our treatment and control group as well as in our Generalised Difference-in-Differences results from the specification above, we conclude that the parallel trends assumption is full-filled (see results below). Thus, we can estimate our next specification with a binary treatment indicator. Our main specification is:

$$Y_{it} = \beta_0 + \beta_1 Intro_t + \beta_2 W_i * Intro_t + \beta_3 Repeal_t + \beta_4 W_i * Repeal_t + \beta_6 X_{it} + \beta_7 m_t + \alpha_i + \epsilon_{it} \tag{2}$$

Where:

- Y_{it} is an outcome of interest for person i in time t (e.g. hours worked or employment)
- $Intro_t$ is an indicator which equals 1 in all periods after policy introduction
- $Repeal_t$ is an indicator which equals 1 in all periods after policy repeal
- W_i indicates whether i is in the treatment group (affected by benefit cut in Q1 and/or Q2 2011)
- X_{it} is a set of time-varying family controls
- m_t is time in months (giving a common linear time trend)
- α_i are individual fixed effects

Our main hypothesis is therefore that $\beta_2 > 0$. Again, we cluster standard errors at the individual level.

6.2 Continuous treatment

In practice, the magnitude of the policy treatment is continuous. Therefore we also estimate the following specification including a continuous treatment measure:

$$Y_{it} = \beta_0 + \beta_1 Intro_t + \beta_2 V_i * Intro_t + \beta_3 Repeal_t + \beta_4 V_i * Repeal_t + \beta_5 X_{it} + \beta_6 m_t + a_i + \epsilon_{it} \quad (3)$$

Where:

- Y_{it} is an outcome of interest for person i in time t (e.g. hours worked or employment)
- $Intro_t$ is an indicator which equals 1 in all periods after policy introduction
- $Repeal_t$ is an indicator which equals 1 in all periods after policy repeal
- V_i is the reduction in child benefits in thousand DKK in 2011 due to our reform for i's family if i is in the treatment group (affected by benefit cut in Q1 and/or Q2 2011) and zero otherwise
- X_{it} is a set of time-varying family controls
- m_t is time in months (giving common a linear time trend)
- α_i are individual fixed effects

Again, we cluster standard errors at the individual level. We do not consider the actual level of child benefits, but only the reduction in child benefits due to the reform. Individual fixed effects and family controls capture the factors that otherwise determine the total level of child benefits.

6.3 Fertility and treatment effects

Due to the fertility effects of reform described above, we can attribute a significant share of the treatment effect to decreased fertility. In order to disentangle the effects of changed fertility on labour supply from the income effect, we run three versions of the analyses described above, where we add different control variables and additional sample selection criteria:

1) *Parental controls:*
   Only individual FEs and parental age FEs. Parental age FEs include fixed effects for each year of age in order to control nonparametrically for age effects of parents.

2) *Parental controls + Family controls:*
   Same as above, but also age of youngest child FEs and total number of children FEs.[4] Age of youngest child FEs include fixed effects for each year of age in order to control nonparametrically for age effects children.

3) *Parental controls + Family controls + Sterilisation - Young:*
   Same as above, but only families where at least one parent has consulted a doctor regarding sterilisation prior to the announcement of the policy. Also excluding families with young children (below the age of 3 throughout the sample period).

In model 1), we estimate the combined effects of decreased fertility and the income effect of loosing child benefits due to the reform. In model 2), we control for a changing composition of children, and in model 3) we shut down fertility response completely by only considering families where one or both parents have consulted a doctor regarding sterilisation prior to the announcement of the reform and who do not have young children. Models 2) and 3) should therefore allow us to separately estimate the income effects of lost child benefits on parental labour supply.

6.4 Alternative strategies

Existing papers have estimated the effects of child/family benefits using various empirical strategies. Many papers use a DiD strategy similar to ours, but importantly Dahl & Lochner (2012) have suggested an IV approach when they exploit changes in the EITC over time to estimate the impact of family income on children's math and reading achievement. Dahl & Lochner

[4] Of children registered in the Danish population register BEF in each year from 2008-2014

(2012, p. 1932) introduce a simulated instrument variable approach that *"eliminates omitted variable biases due to both permanent and temporary shocks correlated with family income and alleviates bias due to measurement error in income."* First, they use lagged pre-tax income to predict changes in EITC income, and next, they use the predicted change in income as an IV. Although Dahl & Lochner's (2012) IV approach targets potential biases when considering the effect of family income on childrens achievement, their strategy is neither feasible, nor applicable when considering effects on parental labour supply due the cap on child benefits in Denmark. Firstly, and most importantly, the Danish child benefits were unconditional and not a function of income during the period we consider. Therefore, we cannot apply an IV strategy analogous to that of Dahl & Lochner (2012). Sticking to their terminology, the predicted income change and the actual income change due to the cap on child benefits in Denmark would be identical as it is independent of income levels. Secondly, the EITC schedule changed repeatedly in 1980s and 1990s, which yields lots of variation in predicted EITC income. Therefore, when generating their instrument, Dahl & Lochner (2012, p. 1935) only *"exploit variation in predicted EITC income due to government changes in EITC schedules over time and not due to changes in family structure."* In the context of child benefits in Denmark, we observe much less variation in the payment schedules of child benefits over time. In fact, the reform we consider is the first major cut in child benefits in Denmark since their introduction in the 1980s. Again, this makes a similar IV estimation strategy infeasible in the context of Danish child benefits. Furthermore, these two differences between the EITC and the Danish child benefits also explain why we do not apply a strategy similar to Hoynes & Patel (2018) who use a "simulated" EITC to estimate the effect of multiple changes to the EITC on the income of single mothers with children. Thirdly, the omitted variable biases Dahl & Lochner (2012) aim to alleviate are less likely to affect our results. We can precisely measure the income shock from the cap on child benefits due to the detailed information in the Danish register data. We also correct for permanent income effects using individual FEs. However, as discussed above, our estimates may be biased as our control group may respond to the cap on child benefits by delaying or changing fertility decisions, even though their incomes were not directly affected by the reform at the time of introduction. For example, a family with two children may respond to the policy by not having a third or fourth child as they realise that they will receive a lower level of child benefits for the additional children. Rather than pursuing an IV strategy to eliminate this source bias, we first control for fertility-related family characteristics, and next, we exploit data on sterilisation and focus on families that have finalised fertility prior to the announcement of the reform.

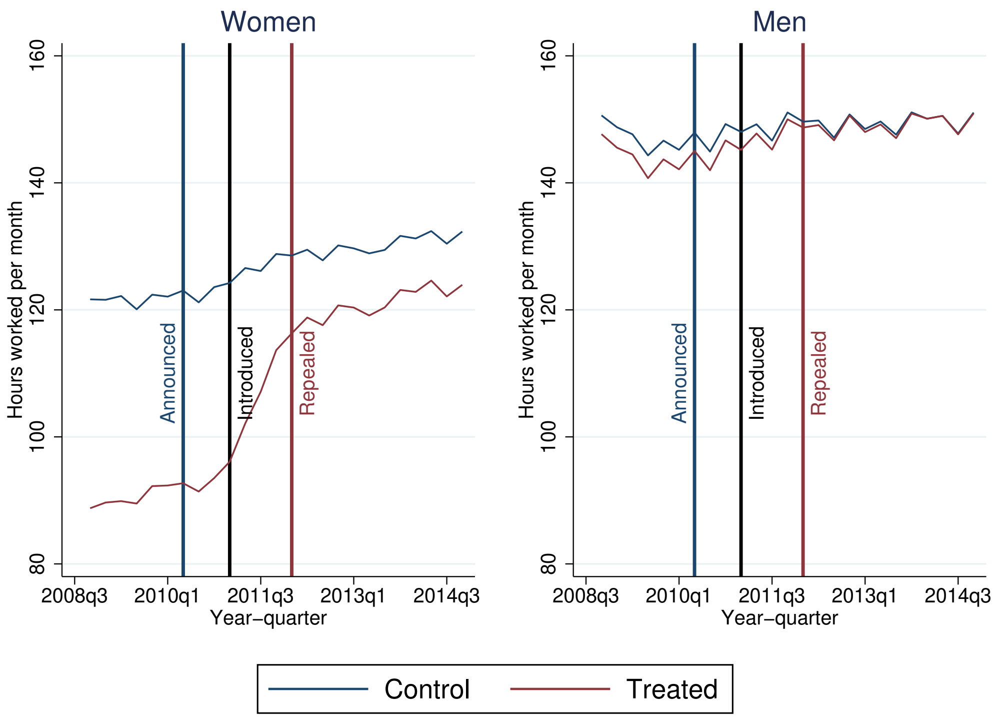
7 Results

7.1 Descriptives

Firstly, we inspect the raw trends in hours worked per month for our treatment and control groups. In Figure 4, we see that the trends in hours worked are very similar for the two groups before the introduction of the reform as well as after its repeal. This serves as a first indication of the parallel trends assumption being full-filled for our treatment and control groups. However, we see a large increase in hours worked per month for our treatment group during the reform period, roughly 20 hours per month. Notice that the response to the reform is gradual after its introduction; mothers do not respond immediately after the announcement of the reform. This is in line with Hotz & Scholz (2006, p. 42) who find that *"the employment responses to EITC policy changes occur with a lag of one or two years."*

We also see a very small increase in hours for our control group after the introduction of the reform. Although the control group is not directly affected by the cap on child benefits, families with two children can be indirectly affected if they were planning to have a third child before the introduction of the reform. If they have a third child, their child benefits for this child will be reduced after the introduction of the reform. Therefore, families may delay or reconsider having a third child due to the reform. Therefore, if fertility is fixed, we should not observe this effect. We use our sterilisation subsample to confirm this, see Figure C.12 in Appendix C.

Figure 4: Hours worked per month for women and men



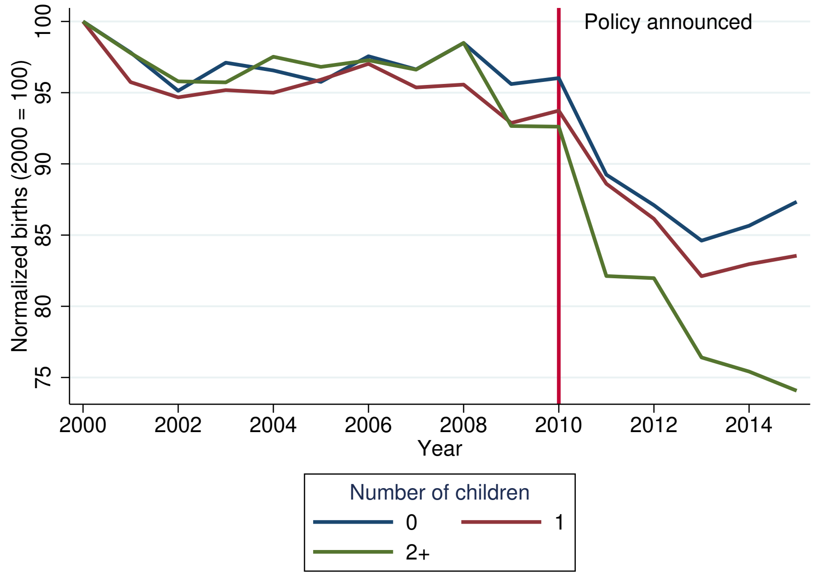
Notes: Average hours worked per month for women and men respectively, including individuals working zero hours.

Secondly, in Figure 5 we see a general decrease in fertility rates, but we also see that the number of births disproportionally decreases in families with two or more children after the introduction of the policy. This is in line with the results of Almlund (2018). We keep this in mind throughout our analysis by estimating the effects of the reform both with and without controls for fertility related characteristics.

7.2 Generalised Difference-in-Differences

After concluding that the raw trends in hours worked per month are very similar for our treatment and control groups, we further inspect pre- and post-trends using our generalised difference-in-differences model. In Figure 6, we show the results from our model only including individual fixed effects and parent age fixed effects. Again, we observe parallel trends for our treatment and control groups. In addition, for women, we find a large increase in labour supply for our treatment group relative to our control group, but only a small effect on men. Similar to the raw trends in hours worked per month reported above, we see that the effect increases through the reform period and remain stable after the repeal. The effects shown in Figure 6 can be ascribed to both reduced fertility as well as a general income effect caused by the reduction in child benefits for our treatment group.

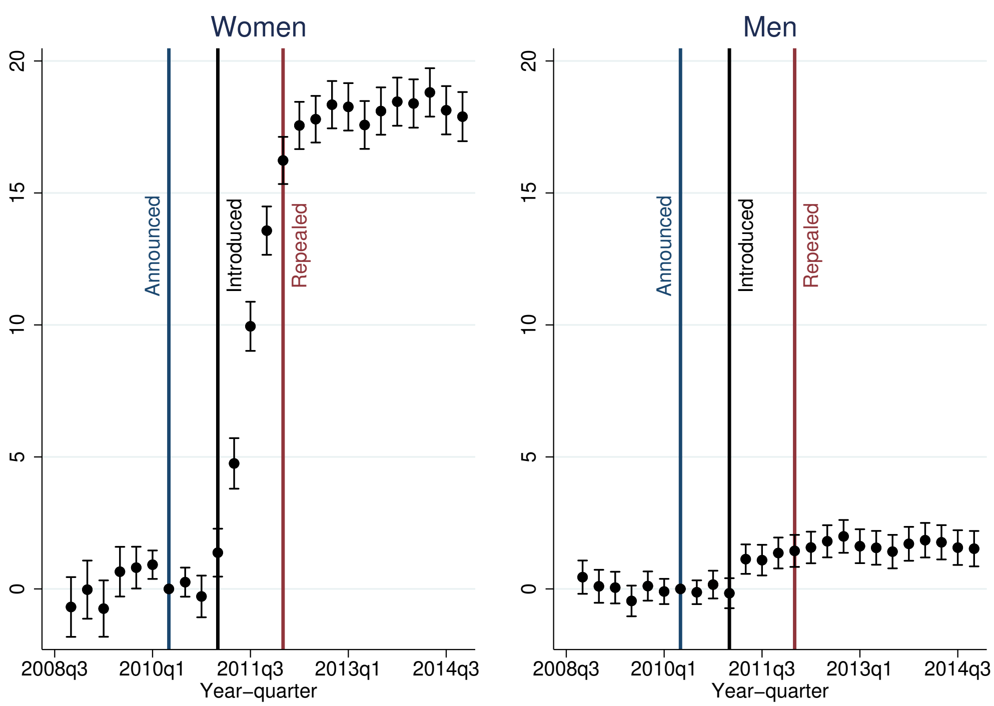
Figure 5: Total number of births by pre-birth number of children



Notes: Normalised, 2000 = 100. Numbers for the full population and not just the estimation sample from which 1-child families are excluded.

Figure 6: G-DiD: Hours worked per month

Parental Controls

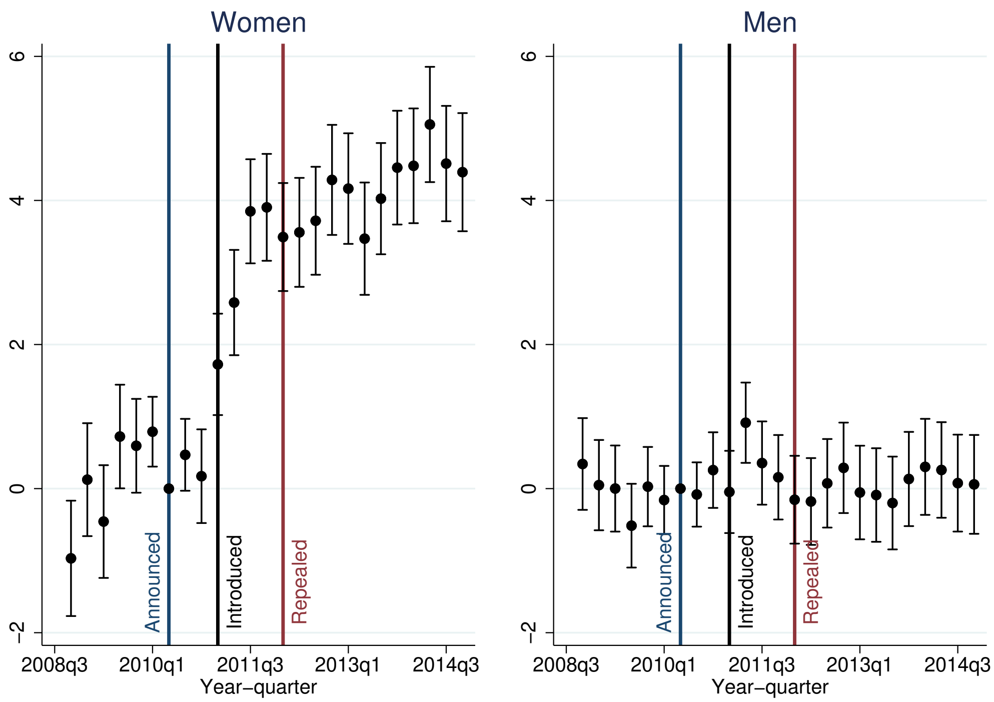


Notes: Dependent variable (y-axis): hours worked per month. Parental controls include: individual FEs and parental age FEs.

In order to separate the effect of reduced fertility from the general income effect of the reform, we control for fertility-related family characteristics in Figure 7. We see a smaller, but still significant effect on women's labour supply both during the reform period and after its repeal. For men, we observe a small and only marginally significant effect and only during the reform period, not after its repeal.

Figure 7: G-DiD: Hours worked per month

Parental Controls + Family Controls

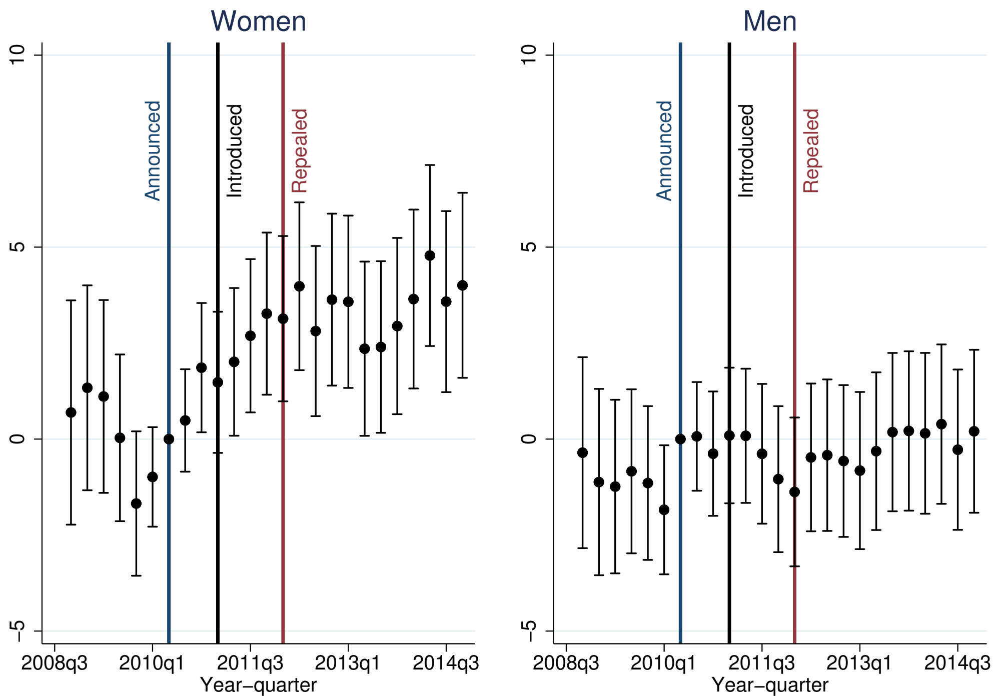


Notes: Dependent variable (y-axis): hours worked per month. Parental controls include: individual FEs and parental age FEs. Family controls include: age of youngest child FEs and number of children FEs.

In order to fully shut down the effect of the fertility response to the policy, we now limit the sample to families where at least one parent had consulted a doctor regarding sterilisation prior to the announcement of the policy. Furthermore, we exclude families with young children throughout the sample period and unbalance the panel in this sub-analysis. In Figure 8, we see similar effect sizes for women for this subgroup of families, but with the smaller sub-sample the coefficients are also less precisely estimated.

Figure 8: G-DiD: Hours worked per month

Parental Controls + Family Controls + Sterilisation - Young



Notes: Dependent variable (y-axis): hours worked per month. Parental controls include: individual FEs and parental age FEs. Family controls include: age of youngest child FEs and number of children FEs. Sterilisation - Young include: only families where at least one parent has consulted a doctor regarding sterilisation prior to the announcement of the policy. Also excluding families with young children (below the age of 3 throughout the sample period).

7.3 DiD

We now move on to report our difference-in-differences estimates. In Table 2, the results using our binary treatment indicator confirm our previous findings. For women, we see a treatment effect of $7.334 + 10.57 = 17.904$ hours worked per month, when we include the fertility response. After controlling for fertility-related family characteristics, we find an effect of $2.820 + 1.130 = 3.95$ hours worked per month. The estimates from the subgroup of sterilised families without young children confirm these results, although the coefficient on the interaction between the repeal and treatment indicators is insignificant.

For men, we find a small, but significant effect of $0.862 + 0.830 = 1.692$ hours worked per month, but only when the fertility response is included.

Table 2: Binary treatment

	Dependent variable: hours worked per month					
	Women			Men		
	(1)	(2)	(3)	(4)	(5)	(6)
1.Intro × 1.Treat	7.334***	2.820***	2.242**	0.862***	0.362	0.578
	[0.327]	[0.249]	[0.776]	[0.187]	[0.192]	[0.633]
1.Repeal × 1.Treat	10.57***	1.130***	0.998	0.830***	-0.265	0.0169
	[0.291]	[0.241]	[0.621]	[0.186]	[0.188]	[0.551]
1.Intro	1.247***	-0.271***	-1.046***	0.952***	0.757***	0.545**
	[0.0958]	[0.0814]	[0.173]	[0.0725]	[0.0726]	[0.167]
1.Repeal	1.114***	0.0190	-0.171	0.510***	0.290***	-0.0822
	[0.0942]	[0.0778]	[0.161]	[0.0693]	[0.0690]	[0.156]
R^2	0.516	0.610	0.662	0.455	0.459	0.508
N	18153075	18153075	3205408	14655450	14655450	2575379
Female	1	1	1	0	0	0
Parental controls	1	1	1	1	1	1
Family controls	0	1	1	0	1	1
Sterilisation	0	0	1	0	0	1
Ex. young	0	0	1	0	0	1

Notes: Standard errors in brackets, clustered at the individual level, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Parental controls: individual FEs and parental age FEs. Family controls: age of youngest child FEs and number of children FEs. Sterilisation: only families where at least one parent has consulted a doctor regarding sterilisation prior to the announcement of the policy. Ex. young: excluding families with young children (below the age of 3 throughout the sample period).

In the next set of analyses, we replace our binary treatment indicator with a continuous measure of child benefits lost due the reform in DKK divided by 1000. 1000 DKK corresponds to approximately 160 USD. The estimates are reported in Table 3. Keep in mind that an average family in our treatment group experiences in a reduction in child benefits of approx. 2300 DKK (see Appendix C), but within our treatment group there is substantial variation in the amount of benefits lost, e.g. four-child families are particularly affected by the reform. Child benefits are tax free, and with the relatively high Danish taxes on labour market income, $\approx$ 50%, this corresponds to about 4600 DKK in labour market income. We find large and significant effects on the treated women. For every 1000 DKK lost due to the reform, women work $2.071 + 3.862 = 5.933$ hours more per month on average. Excluding the fertility response, we find an effect between 1.088 and 0.804 hours per month for every 1000 DKK lost. With a tax rate around 50% and hourly wages of 200 DKK, the estimated effect indicates that women offset the income loss due to the reform roughly 1-to-1 when excluding the fertility response by earning an additional $(12 \times 1.088 \times 200) \times 0.5 = 1305.6$ for 1000 DKK lost in child benefits.

For men, the estimated effects are smaller, but highly significant when including the fertility response. A small effect remains significant even after including family controls, but not when limiting the sample to the sterilised subgroup.

Table 3: Continuous treatment: Reduction in 1000 DKK

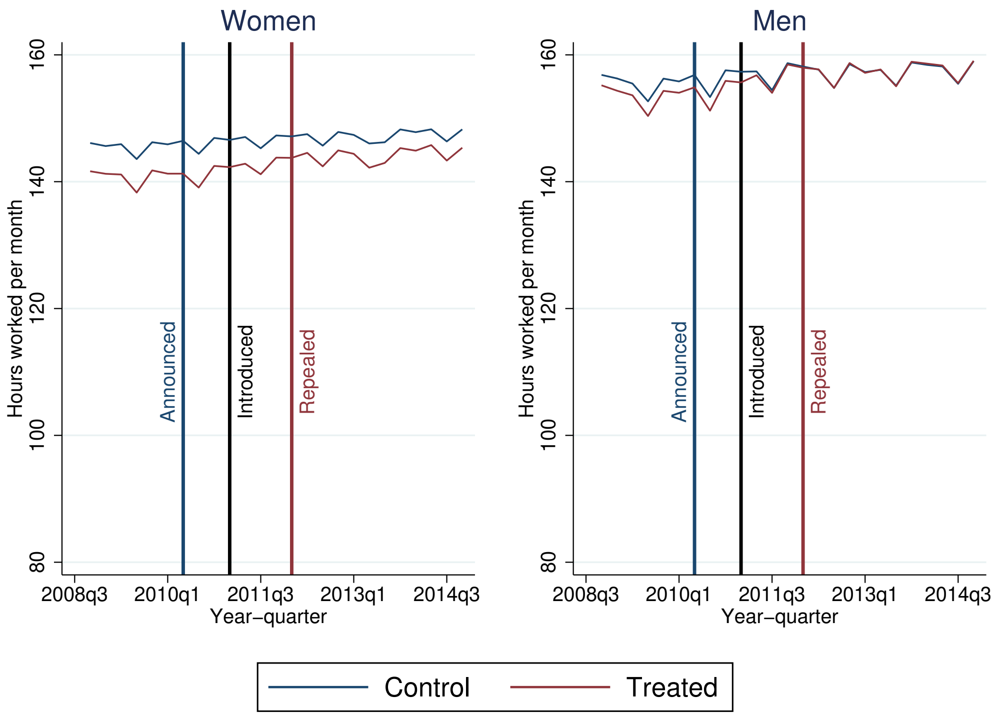
	Dependent variable: hours worked per month					
	Women			Men		
	(1)	(2)	(3)	(4)	(5)	(6)
1.Intro × c.Treat	2.071***	1.088***	0.804**	0.304***	0.230***	0.203
	[0.118]	[0.0920]	[0.299]	[0.0668]	[0.0692]	[0.230]
1.Repeal × c.Treat	3.862***	0.117	0.433	0.339***	-0.0884	-0.0616
	[0.107]	[0.0901]	[0.241]	[0.0657]	[0.0660]	[0.205]
1.Intro	1.535***	-0.236**	-1.027***	0.969***	0.739***	0.549***
	[0.0953]	[0.0807]	[0.172]	[0.0714]	[0.0716]	[0.166]
1.Repeal	1.306***	0.115	-0.160	0.513***	0.284***	-0.0702
	[0.0939]	[0.0771]	[0.160]	[0.0684]	[0.0680]	[0.155]
R^2	0.516	0.610	0.662	0.455	0.459	0.508
N	18153075	18153075	3205408	14655450	14655450	2575379
Female	1	1	1	0	0	0
Parental controls	1	1	1	1	1	1
Family controls	0	1	1	0	1	1
Sterilisation	0	0	1	0	0	1
Ex. young	0	0	1	0	0	1

Notes: Standard errors in brackets, clustered at the individual level, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Parental controls: individual FEs and parental age FEs. Family controls: age of youngest child FEs and number of children FEs. Sterilisation: only families where at least one parent has consulted a doctor regarding sterilisation prior to the announcement of the policy. Ex. young: excluding families with young children (below the age of 3 throughout the sample period).

8 Extensive and intensive margins

In this section, we further analyse the increase in average hours worked for the treatment group. Particularly, we want to separate adjustments along the intensive and extensive margins of labour supply. As a first step, in Figure 9, we map out the average hours of work per month for employed people only, i.e. we exclude people working zero hours.

Figure 9: Hours worked per month, excluding people working zero hours



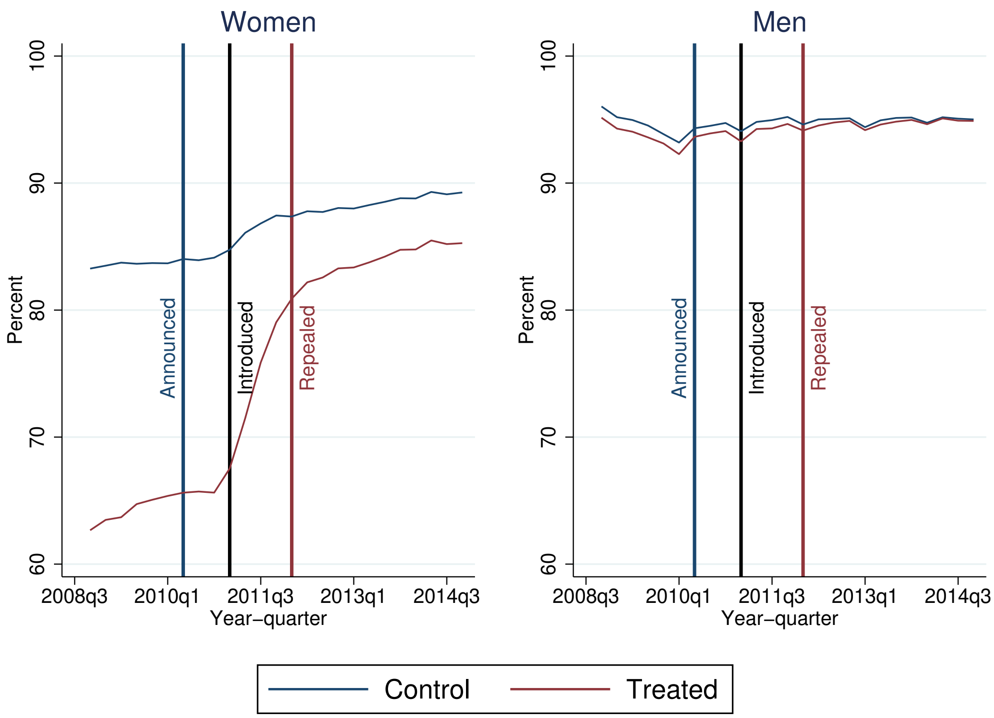
Notes: Average number of hours worked per month, excluding people working zero hours, for women and men respectively. This illustrates changes in labour supply at the intensive margin.

From Figure 9, it appears that there is only a small break in the trend of average hours worked per month for women in the treatment group after the reduction in child benefits. For men, the gap in hours worked between the treatment and control groups closes during the treatment period. Thus, we have a first indication of men's labour supply being affected at the intensive margin during the treatment period.

Before moving onto a casual setting, another descriptive static is important to emphasise, namely participation rates. In Figure 10, we map out participation rates for women and men respectively. We see a large jump in the participation rate for the treated women, but not for men. From Figures C.13 and C.14 in Appendix C, we see that the jump in the participation rate for treated women appears both in part-time and full-time jobs. We also see a small increase the participation rate for women in our control group after the introduction of the reform. Although the control group is not directly affected by the cap on child benefits, families with two children would be affected if they had a third child. This may delay or change the decision to have a third child. Again, we use our sterilisation sub-sample to exclude this effect.

Thus, the descriptive results suggest that women's response to the reform in child benefits happen through increased participation, whereas men may respond at the intensive margin. In the following subsection, we explore the responses at the extensive and intensive margins respectively in the casual setting from the previous section.

Figure 10: Percent of women and men currently employed (i.e. working non-zero hours)



Notes: Percent of women and men currently employed (i.e. working non-zero hours). This illustrates changes in labour supply at the extensive margin.

8.1 Extensive margin

We report the graphical Generalised Difference-in-Differences results in Appendix C, see Figures C.15 to C.17. The graphical analyses again confirm that the parallel trends assumption is full-filled. In Table 4, the effects of the reform are reported using our binary treatment indicator. The results confirm that men do not respond to the reduction in child benefits at the extensive margin. For women, however, we observe a large response to the reform at the extensive margin. Including the fertility response, we find an effect of $5.01 + 6.95 = 11.96$ percentage points (column 1). Excluding the fertility response, we estimate an effect ranging between 1.27 (column 3) and $2.0 + 0.6 = 2.6$ percentage points (column 2).

Table 4: Binary treatment

	Dependent variable: Employment indicator					
	Women			Men		
	(1)	(2)	(3)	(4)	(5)	(6)
1.Intro × 1.Treat	0.0501***	0.0200***	0.0127**	-0.0000148	-0.00251*	0.000577
	[0.00212]	[0.00158]	[0.00473]	[0.00100]	[0.00104]	[0.00342]
1.Repeal × 1.Treat	0.0695***	0.00600***	0.00394	0.00210*	-0.000817	0.00144
	[0.00187]	[0.00152]	[0.00389]	[0.000990]	[0.00101]	[0.00291]
1.Intro	0.00695***	-0.00250***	-0.00792***	0.00212***	0.00170***	0.00214*
	[0.000609]	[0.000508]	[0.00108]	[0.000397]	[0.000398]	[0.000899]
1.Repeal	0.00769***	-0.000113	0.000873	0.00377***	0.00337***	0.00137
	[0.000596]	[0.000485]	[0.000998]	[0.000377]	[0.000377]	[0.000847]
R^2	0.466	0.588	0.638	0.466	0.467	0.524
N	18153075	18153075	3205408	14655450	14655450	2575379
Female	1	1	1	0	0	0
Parental controls	1	1	1	1	1	1
Family controls	0	1	1	0	1	1
Sterilisation	0	0	1	0	0	1
Ex. young	0	0	1	0	0	1

Notes: Standard errors in brackets, clustered at the individual level, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Parental controls: individual FEs and parental age FEs. Family controls: age of youngest child FEs and number of children FEs. Sterilisation: only families where at least one parent has consulted a doctor regarding sterilisation prior to the announcement of the policy. Ex. young: excluding families with young children (below the age of 3 throughout the sample period).

In Table 5, the effects of the reform are reported using our continuous treatment measure. Again, these results confirm that men do not respond to the reduction in child benefits at the extensive margin. Again, we observe that women respond at this margin. Including the fertility response, we find a positive effect on participation of $1.43 + 2.62 = 4.5$ percentage points for every 1000 DKK lost in child benefits (column 1). Excluding the fertility response, we estimate an effect ranging between 0 (column 3) and $0.823 + 0.119 = 0.942$ percentage points (column 2).

Table 5: Continuous treatment: Reduction in 1000 DKK

	Dependent variable: Employment indicator					
	Women			Men		
	(1)	(2)	(3)	(4)	(5)	(6)
1.Intro × c.Treat	0.0143***	0.00823***	0.00363	-0.000129	-0.000856*	0.000228
	[0.000778]	[0.000591]	[0.00186]	[0.000355]	[0.000371]	[0.00119]
1.Repeal × c.Treat	0.0262***	0.00119*	0.00251	0.000960**	-0.000124	0.000634
	[0.000693]	[0.000574]	[0.00153]	[0.000353]	[0.000358]	[0.00110]
1.Intro	0.00890***	-0.00236***	-0.00769***	0.00215***	0.00164***	0.00214*
	[0.000606]	[0.000505]	[0.00107]	[0.000391]	[0.000392]	[0.000893]
1.Repeal	0.00872***	0.000253	0.000811	0.00375***	0.00332***	0.00138
	[0.000595]	[0.000481]	[0.000988]	[0.000371]	[0.000371]	[0.000837]
R^2	0.466	0.588	0.638	0.466	0.467	0.524
N	18153075	18153075	3205408	14655450	14655450	2575379
Female	1	1	1	0	0	0
Parental controls	1	1	1	1	1	1
Family controls	0	1	1	0	1	1
Sterilisation	0	0	1	0	0	1
Ex. young	0	0	1	0	0	1

Notes: Standard errors in brackets, clustered at the individual level, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Parental controls: individual FEs and parental age FEs. Family controls: age of youngest child FEs and number of children FEs. Sterilisation: only families where at least one parent has consulted a doctor regarding sterilisation prior to the announcement of the policy. Ex. young: excluding families with young children (below the age of 3 throughout the sample period).

8.2 Intensive margin

In order to assess labour supply responses at the intensive margin, we unbalance the panel and delete all monthly observations with zero hours of work. Again, we report the graphical Generalised Difference-in-Differences results in Appendix C, see Figures C.18 to C.20. The pre-trends for women are less convincing for this sub-analysis, due to the changing composition of the sample as participation increases through the sample period, but keep in mind the parallel trends in Figure 9. For men, the pre-trends are also parallel in Figure 9, as well as in the Generalised Difference-in-Differences setup.

Using our binary treatment indicator, the effects of the reform on the number of hours worked for people in employment are reported in Table 6. Interestingly, both women and men respond at the intensive margin by increasing their number of hours worked. The estimates are consistent across all specifications, although less significant in columns 3 and 6. Notice that in column 5, we have a first significant indication of a partial reversal of the effects of the reform after its repeal for men.

Table 6: Binary treatment

	Dependent variable: hours worked per month, ex. zero hours					
	Women			Men		
	(1)	(2)	(3)	(4)	(5)	(6)
1.Intro × 1.Treat	0.604***	0.724***	0.754	0.950***	0.802***	0.816*
	[0.139]	[0.147]	[0.480]	[0.114]	[0.116]	[0.373]
1.Repeal × 1.Treat	1.355***	0.917***	0.694	0.477***	-0.226*	-0.431
	[0.133]	[0.135]	[0.369]	[0.112]	[0.112]	[0.319]
1.Intro	-0.165***	-0.338***	-0.475***	0.504***	0.370***	0.0281
	[0.0447]	[0.0452]	[0.103]	[0.0418]	[0.0419]	[0.102]
1.Repeal	0.0161	-0.0664	-0.310**	-0.00372	-0.174***	-0.297**
	[0.0434]	[0.0431]	[0.102]	[0.0411]	[0.0408]	[0.0965]
R^2	0.474	0.478	0.543	0.324	0.328	0.370
N	15440331	15440331	2839834	13883537	13883537	2441788
Female	1	1	1	0	0	0
Parental controls	1	1	1	1	1	1
Family controls	0	1	1	0	1	1
Sterilisation	0	0	1	0	0	1
Ex. young	0	0	1	0	0	1
Ex. zero hours	1	1	1	1	1	1

Notes: Standard errors in brackets, clustered at the individual level, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Parental controls: individual FEs and parental age FEs. Family controls: age of youngest child FEs and number of children FEs. Sterilisation: only families where at least one parent has consulted a doctor regarding sterilisation prior to the announcement of the policy. Ex. young: excluding families with young children (below the age of 3 throughout the sample period). Ex. zero hours: drop monthly observations in which an individual works zero hours.

In Table 7, we again report the effects of the reform on the number of hours worked for people in employment are reported, but this time using our continuous treatment measure. These results confirm that both women and men respond to the reduction in child benefits at the intensive margin by increasing their number of hours worked. Notice that the estimates again are consistent across all specifications, although less significant in column 3. However, using our continuous treatment measure, we find that men respond to the reform almost as strongly as women at the intensive margin. Interestingly, we also see a partial reversal of the increase in hours worked of men after the repeal of the reform after controlling for fertility responses (columns 5 and 6).

Table 7: Continuous treatment: Reduction in 1000 DKK

	Dependent variable: hours worked per month, ex. zero hours					
	Women			Men		
	(1)	(2)	(3)	(4)	(5)	(6)
1.Intro × c.Treat	0.167**	0.223***	0.425*	0.365***	0.401***	0.292*
	[0.0542]	[0.0584]	[0.192]	[0.0419]	[0.0433]	[0.143]
1.Repeal × c.Treat	0.450***	0.225***	0.222	0.185***	-0.101*	-0.254*
	[0.0508]	[0.0511]	[0.142]	[0.0398]	[0.0400]	[0.118]
1.Intro	-0.143**	-0.314***	-0.487***	0.515***	0.357***	0.0353
	[0.0443]	[0.0448]	[0.103]	[0.0412]	[0.0413]	[0.101]
1.Repeal	0.0574	-0.0224	-0.293**	0.00132	-0.172***	-0.288**
	[0.0429]	[0.0426]	[0.101]	[0.0406]	[0.0403]	[0.0954]
R^2	0.474	0.478	0.543	0.324	0.328	0.370
N	15440331	15440331	2839834	13883537	13883537	2441788
Female	1	1	1	0	0	0
Parental controls	1	1	1	1	1	1
Family controls	0	1	1	0	1	1
Sterilisation	0	0	1	0	0	1
Ex. young	0	0	1	0	0	1
Ex. zero hours	1	1	1	1	1	1

Notes: Standard errors in brackets, clustered at the individual level, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Parental controls: individual FEs and parental age FEs. Family controls: age of youngest child FEs and number of children FEs. Sterilisation: only families where at least one parent has consulted a doctor regarding sterilisation prior to the announcement of the policy. Ex. young: excluding families with young children (below the age of 3 throughout the sample period). Ex. zero hours: drop monthly observations in which an individual works zero hours.

9 Conclusions

A temporary cap on child benefit payments in 2011 led to a non-negligible reduction in child benefits for 3-child families with young children as well as larger families. The differential impact of this policy shift represents an opportunity to assess the causal impact of child benefit programmes on the labour supply of mothers and fathers. We find that a reduction in child benefits increase the labour supply of mothers, but only marginally of fathers. The majority of the effect on mothers can be ascribed to reduced fertility, but even after controlling for fertility-related family characteristics, we find a significant increase in mothers' labour supply after the introduction of the reform. This is in line with the existing literature which highlights that women's labour supply relatively more elastic when compare to men, e.g Evers et al. (2008).

We find that both mothers and fathers in affected families respond to the reform at the intensive margin, and that the effect on fathers partially reverses after the repeal of the reform. The strongest response to the reform, however, is at the extensive margin, but only for mothers.

Furthermore, the effects on mothers' labour supply is persistent, even after the repeal of the reform. There are at least two explanations of the persistence of the effects: 1) The costs of entry to labour market / the costs of increasing work hours have already been borne by the mothers, e.g. by enrolling children in daycare and kindergarten. 2) The reform introduced uncertainty about future child benefit payments, even after its repeal, as the generosity of the payments received increased political and public awareness.

We confirm our results by using data on parents' consultations with doctors about sterilisation, a common procedure in Denmark. The advantage of the data on sterilisation is that we can limit our analyses to families who had finalised their fertility decisions prior to the announcement of the reform.

In terms of policy implications, our results complement existing evidence on the EITC from the US, e.g. Kleven (2019), which generally find positive employment effects of in-work benefits on single mothers. The universal child benefit system in Denmark appears to have the opposite effect for women in two-parent families. Thus, our results support the conclusion by Eissa & Hoynes (2004, p. 1931) who found that in the US, *"the EITC is effectively subsidizing married mothers to stay home..."* Depending on policymakers' objectives, alternative policies could be developed, e.g. to target the child pay penalty or part-time pay penalty of mothers. However, other outcomes, such as the well-being and poverty of children and mothers, should of course also be considered.

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A Policy background

A.1 Child benefits in Denmark

The introduction of the the income-independent child benefits in 1986 (Act no. 147 of 19 March 1986) was a part of reform of the Danish tax system intended to improve the economic conditions for families with children in Denmark, cf. the remarks to the proposed legislation, Bill no. 110 of 20 February 1986. Hence, only children of parents who were fully subject to income taxation in Denmark qualified for the new benefits/tax relief. In the remarks to original piece of legislation, the government notes that although the intention is provide a tax relief for child families, it is largely similar to a benefit. This also explains why the Ministry of Taxation is still responsible for child benefit payments today (and not the Ministry for Children and Social Affairs). The remarks to the proposed legislation, Bill no. 110 of 20 February 1986, does not mention any considerations of effects on labour supply.

Prior to the 2011-cap on benefits, numerous changes to the original law from 1986 were implemented. Importantly, changes prior to the 2011-cap included the introduction of child age-dependent benefits so that younger children would qualify for higher levels of benefits. The first two-tier system came into effect in 1990, and it became three-tier in 1995. Furthermore, benefit levels have been increased a few times.

The 2011-cap on child benefits was, however, the first cut in child benefits. The only exception is the introduction of proportional benefits to children in quarter they turn 18 from 2006 and thereafter. The cap 2011-cap was repealed already in the year of its introduction. Non-capped benefits were paid out again from Q1 2012 and thereafter. However, another cut in child benefits was introduced with the means-testing of child benefits in 2014, although it only affected couples with very high earnings. We generally limit our sample period to September 2008 to December 2014.

The remaining changes to 1986-child benefits law were either due to updates in references to other laws, rules for the withholdment of benefits, changes in rules on repayment of benefits received by non-eligible families, and lastly, residency requirements of eligible children.

A.2 2011 Reform

In light of the Great Recession, The Government and The Danish Peoples Party agreed on an austerity package on 25 May 2010 with the intention of limiting Denmark's budget deficit.[5]

The austerity package included a 30,000 DKK-cap on child benefits from 2011 and onwards, which was later changed to 35,000 DKK. Details on the cap are outlined below.

[5] https://www.fm.dk/Nyheder/Pressemeddelelser/2010/05/20100525%20Aftale%20om%20genopretning%20af%20dansk%20oekonomi.aspx

A.2.1 Introduction

The initial Bill no. 221 of 27 May 2010 included a yearly cap on child benefits of 30,000 DKK with effect from 1 January 2011, but with a gradual implementation over three years. Families would be cut 1/3 of benefits exceeding 30,000 DKK in 2011, 2/3 in in 2012, and the full amount in 2013. No maximum cut was mentioned in this first proposal, and no general cut in benefits.

Less than a week after their agreement of 25 May 2010, the Government and The Danish Peoples Party agreed to update their proposal to secure a fairer distribution of the cuts in child benefits on 31 May 2010. The yearly cap on child benefits was increased from 30,000 DKK to 35,000 DKK. This proposal also included a maximum cut of 12,000 DKK per year in 2011-2013 (see details below). From 2014 and onwards, the maximum cut would be increased by 3,000 DKK every year till 2019 where it would amount to 30,000 DKK. In 2020, the maximum cut was to be abolished. The gradual implementation over three years where families would be cut 1/3 of benefits exceeding 35,000 DKK in 2012, 2/3 in in 2013, and the full amount in 2013 remained in the updated proposal.

An additional rule modifies the rule of a maximum cut of 12,000 DKK (2011-level), making the cut slightly smaller for the families with benefits exceeding 68,335 DKK. It states that in 2011, the level of paid out benefits must be at least equal to: non-capped benefits * 1.013 - 12,000 DKK.

The maximum cut of 12,000 DKK is therefore not imposed on actual non-capped benefits, but on the non-capped benefits with a 1.3 percentage addition. This means that maximum cut in practice was 11112 DKK, affecting families with non-capped benefits of 68,335 DKK and above. It also means that maximum cut in benefits is decreasing in the level of benefits above 68,335 DKK.

Lastly, the updated proposal from the Government and The Danish Peoples Party proposal included a general cut in child benefits of 5 percent from 2011 to 2013. Despite the more lenient 35,000 DKK-cap on child benefits, the general 5 percent cut secured savings for the government that exceeded those of the original proposal.

After approval in parliament, the final law, Law no. 725 of 25 June 2010, came into force on 27 June 2010, but it only affected benefits payouts from Q1 2011 and thereafter.

A.2.2 Repeal

The repeal of 2011-cap on child benefits was included in the 2012 Finance Act. A complete Finance Act proposal published by the newly elected left-wing government on 3 November 2011 included the repeal of the 2011-cap on child benefits.[6] However, the repeal was already announced in the press on 30 October 2011. The proposal simply repealed the 35,000 DKK cap

[6] https://fm.dk/media/15064/ansvaroghandling.pdf

on child benefits and its gradual implementation. However, it did not contain a repeal of the general 5 % cut in child benefits.

The final law as proposed by the government was approved in parliament on 21 December 2011 with the support of the RedGreen Alliance (DA: Enhedslisten). After approval in parliament, the final law, Law no. 1382 of 28 December 2011, came into force on 1 January 2012, and affected benefits payouts from Q1 2012 and onwards.

B Conceptual framework

In this appendix, we introduce a theoretical framework which generates tight predictions over individual behavior under different child benefit regimes. This provides hypotheses which can be tested empirically. The static model below that individuals do not consider utility in the future, and thus, it assumes very high discount rates.

Utility U_i is a function of leisure l, consumption C and number of children K. We assume that leisure, consumption and number of children are additively separable:

$$U_i(C, l, K) = f_i(C) + g_i(l) + j_i(K) \tag{4}$$

Note the subscript i: We consider agents to be heterogeneous, although we do make some common assumptions about the structure of their preferences. Here j_i is the function determining utility generated from having children. This could capture expectations of future family earnings or simply the consumption value of having children. We assume that utility is increasing and concave in number of children, so $j_i'(K) > 0$ and $j_i''(K) < 0$. We assume the same of f_i and g_i, and we assume that both consumption and leisure are normal goods. To simplify exposition, we assume log functions for each component of utility, giving us a Cobb-Douglas utility function:

$$U_i(C, l, K) = \lambda_{c,i} log(C) + \lambda_{l,i} log(l) + \lambda_{k,i} log(K) \tag{5}$$

In order to consider a specific monotonic transformation of the utility function for each individual, i, we also assume that:

$$\lambda_{c,i} + \lambda_{l,i} + \lambda_{k,i} = 1 \tag{6}$$

$$\lambda_{c,i}, \lambda_{l,i}, \lambda_{k,i} > 0 \tag{7}$$

B.1 Linear child benefits

Before choosing the number of children, an individual can choose between two states. First, we consider the state in which an individual works and receives the wage w_i, which varies across individuals. Next, we consider the state in which an individual does not work, but instead receives a fixed transfer from the government.

B.1.1 Working

If working, an individual splits their total time, which is normalized to 1, between leisure l and work h:

$$h = 1 - l \tag{8}$$

The budget constraint is as follows, with wage w_i taken as given:

$$C + \alpha K = w_i(1 - l) + P(K) \tag{9}$$

Here α represents the cost of having a child, which we assume to be linear. $P(K)$ represents the amount of child benefits received, which is a function of the number of children. This could be a linear or a non-linear function, and could be zero in the case of no child benefits. When child benefits are linear, $P(K) = \delta K$, where δ is the per-child benefit amount. The budget constraint then becomes:

$$C + \alpha K = w_i(1 - l) + \delta K \tag{10}$$

We assume that the financial cost of having a child exceeds the per-child benefit, so $\alpha > \delta$. In this setup, where children are viewed as a consumption good with price $\alpha - \delta$, we obtain the usual optimality condition for consumption / leisure choices and an additional one for consumption / number of children. The superscript W, indicates that this is the optimal choice after deciding to work:

$$C^W = \frac{\lambda_{c,i}}{\lambda_{l,i}} w_i l^W \tag{11}$$

$$C^W = \frac{\lambda_{c,i}}{\lambda_{k,i}} (\alpha - \delta) K^W \tag{12}$$

Combining these with the budget constraint gives the following optimal allocations in terms of factors external to the model:

$$l^W = \frac{\lambda_{l,i}}{\lambda_{c,i} + \lambda_{l,i} + \lambda_{k,i}} = \lambda_{l,i} \tag{13}$$

$$C^W = \frac{\lambda_{c,i}}{\lambda_{c,i} + \lambda_{l,i} + \lambda_{k,i}} w_i = \lambda_{c,i} w_i \tag{14}$$

$$K^W = \frac{\lambda_{k,i}}{\lambda_{c,i} + \lambda_{l,i} + \lambda_{k,i}} \frac{w_i}{(\alpha - \delta)} = \lambda_{k,i} \frac{w_i}{(\alpha - \delta)} \tag{15}$$

B.1.2 Not working

If not working, individuals do not split their total time between leisure and working, and we have that:

$$l = 1 \tag{16}$$

Again assuming that child benefits are linear, the budget constraint is as follows, where T is a fixed transfer from the government:

$$C + \alpha K = T + \delta K \tag{17}$$

We obtain the following optimality condition for consumption / number of children where the superscript NW, indicates that this is the optimal choice after deciding not to work:

$$C^{NW} = \frac{\lambda_{c,i}}{\lambda_{k,i}}(\alpha - \delta)K^{NW} \tag{18}$$

Combining these with the budget constraint gives the following optimal allocations in terms of factors external to the model:

$$C^{NW} = \frac{\lambda_{c,i}}{\lambda_{c,i} + \lambda_{k,i}}T \tag{19}$$

$$K^{NW} = \frac{\lambda_{k,i}}{\lambda_{c,i} + \lambda_{k,i}}\frac{T}{(\alpha - \delta)} \tag{20}$$

We see that an individual chooses to work if:

$$\lambda_{c,i}log(\lambda_{c,i}w_i) + \lambda_{l,i}log(\lambda_{l,i}) + \lambda_{k,i}log(\lambda_{k,i}\frac{w_i}{\alpha - \delta}) > \lambda_{c,i}log(\frac{\lambda_{c,i}}{\lambda_{c,i} + \lambda_{k,i}}T) + \lambda_{k,i}log(\frac{\lambda_{k,i}}{\lambda_{c,i} + \lambda_{k,i}}\frac{T}{\alpha - \delta}) \tag{21}$$

B.2 Non-linear child benefits: The introduction of a benefits cap

Now let us consider a case where individuals have already chosen l, C and K subject to the linear benefits system, but then a new non-linear system is introduced. We first consider the case of an individual that decided to work under the previous benefits system, and decides to do so again. The new optimal choices will be denoted $l^{W,W}$, $C^{W,W}$ and $K^{W,W}$.

Under the new system, $P(K) = \min(\delta K, \phi)$ where ϕ is the maximum amount of child benefits. This also means that $\frac{\phi}{\delta}$ is a positive number which can be interpreted as the maximum number of kids for which you can receive benefits.

Clearly, if $K^W < \frac{\phi}{\delta}$, individuals are unaffected by the cap. However, if $K^W > \frac{\phi}{\delta}$ individuals will be away from the optimum. We assume that the parameters are such that it is not optimal to have children above the capped level, so $K^{W,W} \leq \frac{\phi}{\delta}$.

It is not in general possible to reduce your number of children, so individuals are subject to the constraint the $K^{W,W} \geq K^W$. For these individuals "stuck" with a sub-optimally high number of children, what do we expect to happen to consumption and labor supply in the model?

B.2.1 Working before cap

If working already before the cap on child benefits were introduced, individuals re-optimize to choose consumption and leisure subject to the new budget constraint:

$$C + \alpha K^W = w_i(1-l) + \phi \tag{22}$$

Once again, the optimal consumption-leisure trade-off is determined by:

$$C^{W,W} = \frac{\lambda_{c,i}}{\lambda_{l,i}} w_i l^{W,W} \tag{23}$$

This gives us:

$$l^{W,W} = \frac{\lambda_{l,i}}{\lambda_{c,i} + \lambda_{l,i}} \frac{w_i - \alpha K^W + \phi}{w_i} \tag{24}$$

Substituting for K^W gives the following:

$$l^{W,W} = \frac{\lambda_{l,i}}{\lambda_{c,i} + \lambda_{l,i}} + \frac{\lambda_{l,i}}{\lambda_{c,i} + \lambda_{l,i}} \frac{\phi}{w_i} - \frac{\lambda_{l,i}}{\lambda_{c,i} + \lambda_{l,i}} \frac{\alpha \lambda_{k,i}}{\alpha - \delta} \tag{25}$$

How does this relate to $l^W = \lambda_{l,i}$, the leisure choice under linear child benefits? We have that individuals increase their labour supply if the following inequality holds:

$$\begin{align} \lambda_{l,i} <& \frac{\lambda_{l,i}}{\lambda_{c,i} + \lambda_{l,i}} + \frac{\lambda_{l,i}}{\lambda_{c,i} + \lambda_{l,i}} \frac{\phi}{w_i} - \frac{\lambda_{l,i}}{\lambda_{c,i} + \lambda_{l,i}} \frac{\alpha \lambda_{k,i}}{\alpha - \delta} \tag{26} \\ \lambda_{l,i}(\lambda_{c,i} + \lambda_{l,i}) <& \lambda_{l,i}(1 + \frac{\phi}{w_i} - \frac{\alpha \lambda_{k,i}}{\alpha - \delta}) \tag{27} \\ \lambda_{c,i} + \lambda_{l,i} <& 1 + \frac{\phi}{w_i} - \frac{\alpha \lambda_{k,i}}{\alpha - \delta} \tag{28} \\ & \tag{29} \end{align}$$

Now, remember that $\lambda_{c,i} + \lambda_{l,i} + \lambda_{k,i} = 1$, and we have that:

$$\lambda_{c,i} + \lambda_{l,i} < 1 + \frac{\phi}{w_i} - \frac{\alpha\lambda_{k,i}}{\alpha - \delta} \tag{30}$$

$$1 - \lambda_{k,i} < 1 + \frac{\phi}{w_i} - \frac{\alpha\lambda_{k,i}}{\alpha - \delta} \tag{31}$$

$$\frac{\alpha\lambda_{k,i}}{\alpha - \delta} - \lambda_{k,i} < \frac{\phi}{w_i} \tag{32}$$

$$-\frac{\delta\lambda_{k,i}}{\alpha - \delta} < \frac{\phi}{w_i} \tag{33}$$

$$0 < \frac{\phi}{w_i} + \frac{\delta\lambda_{k,i}}{\alpha - \delta} \tag{34}$$

$$\tag{35}$$

Since $\phi > 0$, $\delta\lambda_{k,i} > 0$ and $\alpha > \delta$ by definition, the inequality holds. Thus, labour supply increases for people who decided to work already prior to the cap on child benefits.

B.2.2 Not working before cap

For the group of individuals not working before the introduction of the cap on child benefits, the choice of C is now given by the following budget constraint if they continue to stay out of work:

$$C^{NW,NW} + \alpha K^{NW} = T + \phi \tag{36}$$

$$C^{NW,NW} = T + \phi - \alpha K^{NW} \tag{37}$$

$$C^{NW,NW} = T - X \tag{38}$$

$$\tag{39}$$

Where $X = \alpha K^{NW} - \phi$ is a constant and are the expenses to children not covered by the capped child benefits.

Next, we consider the case where an individual switches from not working to working after the cap on child benefits is introduced. Then they face the budget constraint:

$$C = w_i(1 - l) - X \tag{40}$$

The optimal consumption-leisure trade-off is determined by $C^{NW,W} = \frac{\lambda_{c,i}}{\lambda_{l,i}} w_i l^{NW,W}$. This gives us:

$$l^{NW,W} = \frac{\lambda_{l,i}}{\lambda_{c,i}+\lambda_{l,i}} \frac{w_i - X}{w_i} \tag{41}$$

$$C^{NW,W} = \frac{\lambda_{c,i}}{\lambda_{c,i}+\lambda_{l,i}} (w_i - X) \tag{42}$$

Thus, an individual switches from not working to working after the introduction of the cap if the following inequality holds:

$$\lambda_{c,i} log(\frac{\lambda_{c,i}}{\lambda_{c,i}+\lambda_{l,i}}(w_i - X)) + \lambda_{l,i} log(\frac{\lambda_{l,i}}{\lambda_{c,i}+\lambda_{l,i}} \frac{w_i - X}{w_i}) + \lambda_{k,i} log(K^{NW}) > \lambda_{c,i} log(T - X) + \lambda_{l,i} log(1) + \lambda_{k,i} log(K^{NW}) \tag{43}$$

$$\lambda_{c,i} log(\frac{\lambda_{c,i}}{\lambda_{c,i}+\lambda_{l,i}}(w_i - X)) + \lambda_{l,i} log(\frac{\lambda_{l,i}}{\lambda_{c,i}+\lambda_{l,i}} \frac{w_i - X}{w_i}) > \lambda_{c,i} log(T - X) \tag{44}$$

B.3 An example

We now turn to an example of an agent, j, with the following parameters:

$$w_j = 400 \tag{45}$$

$$T = 0.35 w_j = 140 \tag{46}$$

$$\alpha = 20 \tag{47}$$

$$\delta = 13 \tag{48}$$

$$\phi = 35 \tag{49}$$

$$\lambda_{l,j} = \frac{3}{8} \tag{50}$$

$$\lambda_{c,j} = \frac{1}{2} \tag{51}$$

$$\lambda_{k,j} = \frac{1}{8} \tag{52}$$

$$\tag{53}$$

B.3.1 Before introduction of cap

First, the agent chooses between working and not working. If working, the agent would choose:

$$
\begin{aligned}
l^W =& \lambda_{l,j} = \frac{3}{8} && (54)\\
C^W =& \lambda_{c,j} w_j = \frac{1}{2} 400 = 200 && (55)\\
K^W =& \lambda_{k,j} \frac{w}{(\alpha - \delta)} = \frac{1}{8} \frac{400}{(20-13)} \approx 7.14 && (56)
\end{aligned}
$$

If not working, the agent would choose:

$$
\begin{aligned}
l^{NW} =& 1 && (57)\\
C^{NW}, =& \frac{\lambda_{c,j}}{\lambda_{c,j} + \lambda_{k,j}} T = \frac{\frac{1}{2}}{\frac{1}{2} + \frac{1}{8}} 140 = 112 && (58)\\
K^{NW} =& \frac{\lambda_{k,j}}{\lambda_{c,j} + \lambda_{k,j}} \frac{T}{(\alpha - \delta)} = \frac{\frac{1}{8}}{\frac{1}{2} + \frac{1}{8}} \frac{140}{(20-13)} = 4 && (59)
\end{aligned}
$$

We can now compare the levels of utility from working and not working:

$$
\begin{aligned}
U_j^W(200, 3/8, 7.14) =& \frac{1}{2} log(200) + \frac{3}{8} log(\frac{3}{8}) + \frac{1}{8} log(7.14) \approx 1.0975 && (60)\\
U_j^{NW}(112, 1, 4) =& \frac{1}{2} log(112) + \frac{1}{8} log(4) \approx 1.0998 && (61)
\end{aligned}
$$

As $U_j^{NW}(112, 1, 4) > U_j^W(200, 3/8, 7.14)$, the individual chooses not to work and to have 4 children before the introduction of the cap on child benefits.

B.3.2 After introduction of cap

Now consider the choices of the same individual after the introduction of the cap on child benefits. We assume that the individual has finalised fertility decisions and cannot change the number of children from 4. First, we calculate:

$$
X = \alpha K^{NW} - \phi = 20 * 4 - 35 = 45 \tag{62}
$$

If the individual is still not working after the introduction of cap on benefits, the individual's consumption is given by:

$$
C^{NW,NW} = T - X = 140 - 45 = 95 \tag{63}
$$

If the individual starts to work after the introduction of the cap, the optimal choices of consumption and leisure are:

$$
\begin{aligned}
l^{NW,W} =& \frac{\lambda_{l,j}}{\lambda_{c,j}+\lambda_{l,j}}\frac{w_j - X}{w_j} = \frac{\frac{3}{8}}{\frac{1}{2}+\frac{3}{8}}\frac{400-45}{400} = \frac{213}{560} \approx 0.38 && (64)\\
C^{NW,W} =& \frac{\lambda_{c,j}}{\lambda_{c,j}+\lambda_{l,j}}(w_j - X) = \frac{\frac{1}{2}}{\frac{1}{2}+\frac{3}{8}}(400-45) = \frac{1420}{7} = \approx 202.86 && (65)
\end{aligned}
$$

We can now compare the levels of utility from working and not working after the introduction of the cap on benefits:

$$
\begin{aligned}
U_j^{NW,W}(202.86, 0.38, 4) =& \frac{1}{2}log(202.86) + \frac{3}{8}log(0.38) + \frac{1}{8}log(4) \approx 1.0714 && (66)\\
U_j^{NW,NW}(95, 1, 4) =& \frac{1}{2}log(95) + \frac{1}{8}log(4) \approx 1.0641 && (67)
\end{aligned}
$$

As $U_j^{NW,W}(202.86, 0.38, 4) > U_j^{NW,NW}(95, 1, 4)$ the individual starts working after the introduction of the cap.

B.3.3 Switching costs

Until now, we have assumed that individuals are able to switch freely between working and not working. Let us now consider the case where an individuals face a switching cost, S, when switching from working to not working or vice versa. How much would the individual be willing to pay to switch from not working to working when the cap on child benefits are introduced?

If the individual starts to work after the introduction of the cap, and pays the switching cost, the optimal choices of consumption and leisure are:

$$
\begin{aligned}
l^{NW,W} =& \frac{\lambda_{l,j}}{\lambda_{c,j}+\lambda_{l,j}}\frac{w_j - X - S}{w_j} = \frac{\frac{3}{8}}{\frac{1}{2}+\frac{3}{8}}\frac{400-45-S}{400} = \frac{1065-3S}{2800} && (68)\\
C^{NW,W} =& \frac{\lambda_{c,j}}{\lambda_{c,j}+\lambda_{l,j}}(w_j - X - S) = \frac{\frac{1}{2}}{\frac{1}{2}+\frac{3}{8}}(400-45-S) = \frac{1420-4S}{7} && (69)
\end{aligned}
$$

Thus, the maximum switching cost the individual is willing to pay is given by the equation:

$$
\begin{aligned}
U^{NW,W}(\frac{1420-4S}{7}, \frac{1065-3S}{2800}, 4) =& U^{NW,NW}(95, 1, 4) && (70)\\
\frac{1}{2}log(\frac{1420-4S}{7}) + \frac{3}{8}log(\frac{1065-3S}{2800}) + \frac{1}{8}log(4) =& 1.0641 && (71)\\
S \approx& 6.7593 && (72)
\end{aligned}
$$

We now want to consider the case where the cap on benefits is repealed again. How much would the individual be willing to pay to stop working? If the individual stop working, the level of consumption is given by:

$$C^{NW,W,NW} = T - (\alpha - \delta)4 - S = 140 - 7 * 4 - S = 112 - S \tag{73}$$

If the individual keeps working after the repeal of the cap on benefits, the level of consumption and leisure are given by:

$$l^{NW,W,W} = \frac{\lambda_{l,j}}{\lambda_{c,j} + \lambda_{l,j}} \frac{w_j - (\alpha - \delta) \times 4}{w_j} = \frac{\frac{3}{8}}{\frac{1}{2} + \frac{3}{8}} \frac{400 - (20 - 13) \times 4}{400} = \frac{279}{700} \approx 0.40 \tag{74}$$

$$C^{NW,W,W} = \frac{\lambda_{c,j}}{\lambda_{c,j} + \lambda_{l,j}} (w_j - (\alpha - \delta) \times 4) = \frac{\frac{1}{2}}{\frac{1}{2} + \frac{3}{8}} (400 - (20 - 13) \times 4) = \frac{1488}{7} =\approx 212.57 \tag{75}$$

Thus, the maximum switching cost the individual is willing to pay to stop working again is given by the equation:

$$U_j^{NW,W,W}(\frac{1488}{7}, \frac{279}{700}, 4) = U_j^{NW,W,NW}(112 - S, 1, 4) \tag{76}$$

$$1.0891 = \frac{1}{2} log(112 - S) + \frac{1}{8} log(4) \tag{77}$$

$$S \approx 5.3687 \tag{78}$$

This example shows that if, for example, $S = 6$ then at least some individuals will switch to from not working to working when the cap on benefits is introduced, but then not switch back after the repeal of the cap.

B.4 Fertility responses

While the above model assumes the number of children is already chosen, fertility is a dynamic process and will also be affected by a change from linear to non-linear child benefits. If optimal number of children in the linear child benefits case is under the level of the child benefits cap, so $K^W < \phi/\delta$ or $K^{NW} < \phi/\delta$. For individuals with $K < K^W$ or $K < K^{NW}$ who are still accumulating children, the fertility decision is unchanged upon the introduction of the cap.

Next, we consider the case where the optimal number of children in the linear case is above the level of the child benefits cap, so $K^W > \phi/\delta$ or $K^{NW} > \phi/\delta$. Maintaining the assumptions above, upon the switch to the non-linear child benefits system the optimal number of children is $K^W = \phi/\delta$ or $K^{NW} = \phi/\delta$. If individuals accumulating children currently have $K < \phi/\delta$, they will continue accumulating until $K = \phi/\delta$. If individuals have already accumulated $K > \phi/\delta$, but not reached previous optimum $K^W > \phi/\delta$ or $K^{NW} > \phi/\delta$, they will stop accumulating children.

C Data

C.1 Summary statistics, full sample

Table C.8: Summary statistics, full sample: Women

	Treatment Group	Control	Diff.	Std. Error
Age	36.0308	39.1069	3.0761***	0.0331
Benefits lost in 2011	2319.8836	3.4718	-2316.4118***	3.5557
Family has twins or triplets	0.1364	0.0460	-0.0905***	0.0014
Age of youngest child	2.1035	6.7948	4.6913***	0.0259
Annual disposable income	255901.0676	259745.9958	3844.9282***	635.4222
Hours (BFL)	94.5865	123.4527	28.8662***	0.3930
Hours > 0 (BFL)	0.6651	0.8421	0.1770***	0.0024
Years of education	15.0313	14.8576	-0.1737***	0.0138
Potential experience, years	11.6676	14.7797	3.1121***	0.0441
Register experience, years	11.3949	15.5240	4.1292***	0.0454
Number of individuals	28968	213073		

Notes: * p < 0.1, ** p < 0.05, *** p < 0.01. Data from January 2011. Potential experience equals years since finishing education. Notice that the "Benefits lost in 2011" is not equal to zero for the control group. This is because we define our treatment group as those who were affected by the policy in the first two quarters of 2011 in order avoid selection into the treatment group.

Table C.9: Summary statistics, full sample: Men

	Treatment Group	Control	Diff.	Std. Error
Age	38.0318	40.9809	2.9491***	0.0398
Benefits lost in 2011	2357.1098	3.0487	-2354.0610***	4.0960
Family has twins or triplets	0.1423	0.0453	-0.0970***	0.0016
Age of youngest child	2.1249	6.6513	4.5264***	0.0293
Annual disposable income	325148.4529	324784.1813	-364.2716	4363.0986
Hours (BFL)	144.9328	147.5132	2.5804***	0.3378
Hours > 0 (BFL)	0.9302	0.9393	0.0091***	0.0017
Years of education	15.0767	14.7984	-0.2783***	0.0162
Potential experience, years	14.1452	17.5651	3.4198***	0.0538
Number of individuals	22477	172929		

Notes: * p < 0.1, ** p < 0.05, *** p < 0.01. Data from January 2011. Potential experience equals years since finishing education. Notice that the "Benefits lost in 2011" is not equal to zero for the control group. This is because we define our treatment group as those who were affected by the policy in the first two quarters of 2011 in order avoid selection into the treatment group.

C.2 Summary statistics, sterilisation subsample

Table C.10: Summary statistics, sterilisation subsample: Women

	Treatment Group	Control	Diff.	Std. Error
Age	38.3770	41.6980	3.3209***	0.0906
Benefits lost in 2011	2103.1343	0.1192	-2103.0151***	7.0791
Family has twins or triplets	0.1770	0.0443	-0.1327***	0.0046
Age of youngest child	5.0734	9.4886	4.4152***	0.0681
Annual disposable income	252710.5064	252411.8932	-298.6131	1831.8352
Hours (BFL)	119.3680	130.3181	10.9501***	1.1795
Hours > 0 (BFL)	0.8373	0.8865	0.0492***	0.0067
Years of education	14.5484	14.3308	-0.2176***	0.0443
Potential experience, years	14.3822	17.6526	3.2704***	0.1451
Register experience, years	13.0508	18.0290	4.9782***	0.1533
Number of individuals	2440	39552		

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Data from January 2011. Potential experience equals years since finishing education. Notice that the "Benefits lost in 2011" is not equal to zero for the control group. This is because we define our treatment group as those who were affected by the policy in the first two quarters of 2011 in order avoid selection into the treatment group.

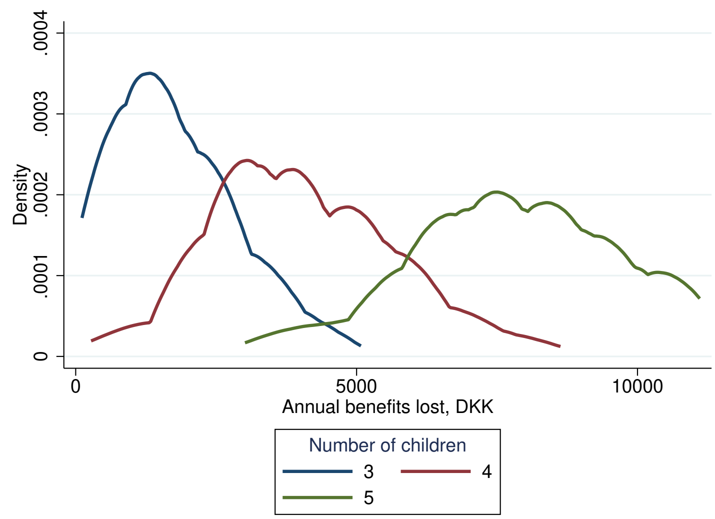
Table C.11: Summary statistics, sterilisation subsample: Men

	Treatment Group	Control	Diff.	Std. Error
Age	40.5988	43.8436	3.2448***	0.1070
Benefits lost in 2011	2124.0295	0.0042	-2124.0253***	8.5254
Family has twins or triplets	0.1768	0.0459	-0.1309***	0.0052
Age of youngest child	5.0208	9.4544	4.4337***	0.0757
Annual disposable income	318245.2932	315419.3384	-2825.9548	5583.2587
Hours (BFL)	147.3259	148.3792	1.0533	1.1070
Hours > 0 (BFL)	0.9357	0.9399	0.0042	0.0055
Years of education	14.4667	14.3157	-0.1510***	0.0508
Potential experience, years	17.6218	21.1897	3.5679***	0.1622
Register experience, years	19.9189	23.4733	3.5544***	0.1608
Number of individuals	1974	31764		

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Data from January 2011. Potential experience equals years since finishing education. Notice that the "Benefits lost in 2011" is not equal to zero for the control group. This is because we define our treatment group as those who were affected by the policy in the first two quarters of 2011 in order avoid selection into the treatment group.

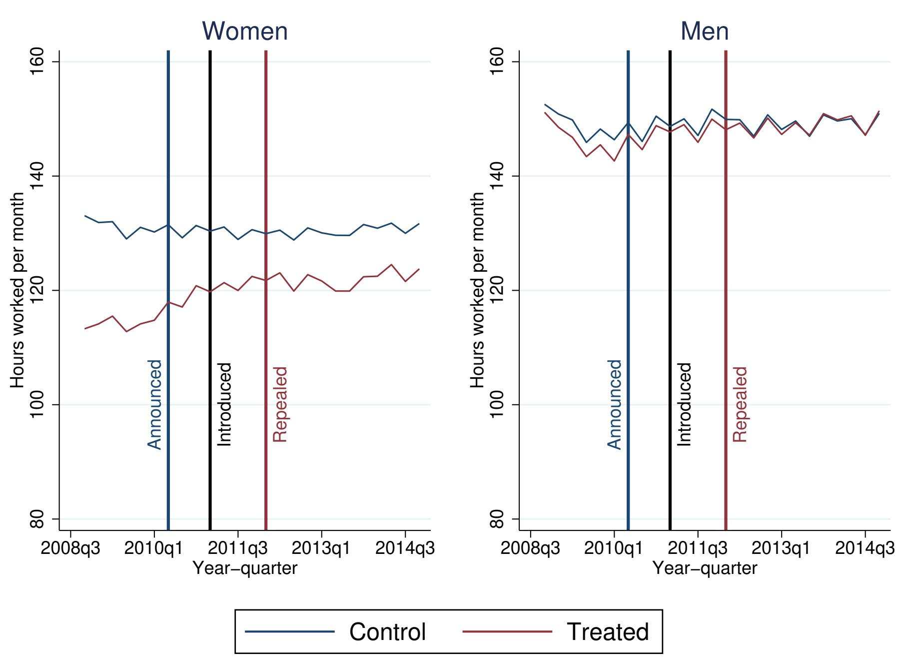
C.3 Descriptives

Figure C.11: Benefits lost in 2011 by number of children



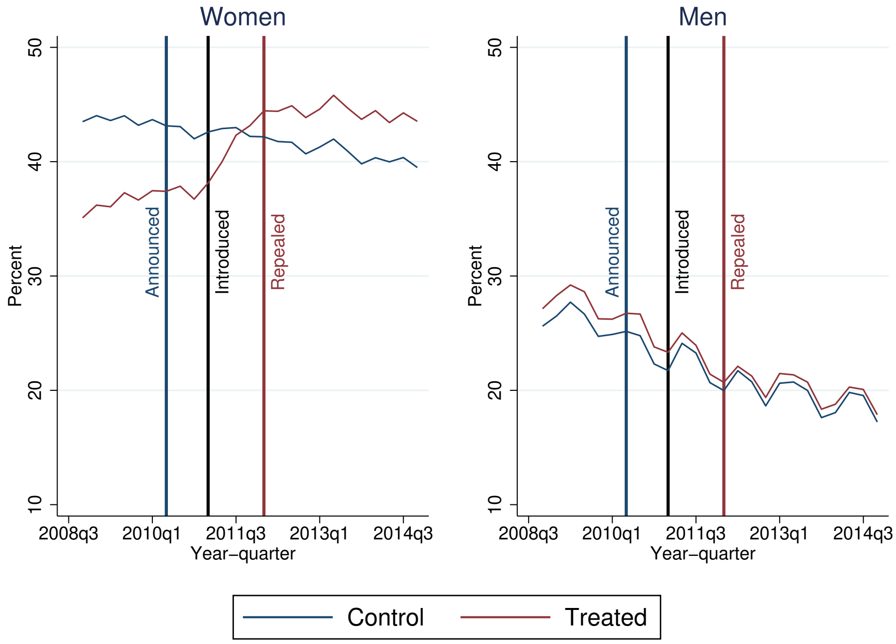
Notes: Benefits lost as a consequence of the cap on child benefits. Observations below/above 1st/99th percentiles dropped. Epanechnikov kernel density, bandwidth = 500.

Figure C.12: Hours worked per month for women and men, sterilisation subsample



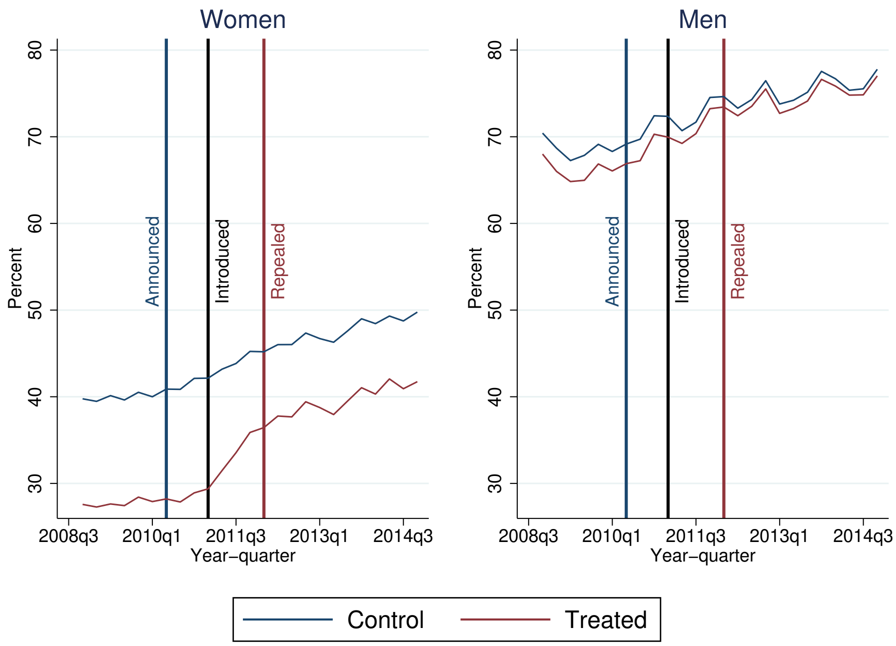
Notes: Average hours worked per month for women and men respectively, including individuals working zero hours. Sterilisation subsample only.

Figure C.13: Percent of women and men employed in part-time jobs



Notes: Percent of women and men currently employed in part-time jobs. Part-time work is defined as working 160 hours or less per month.

Figure C.14: Percent of women and men employed in full-time jobs

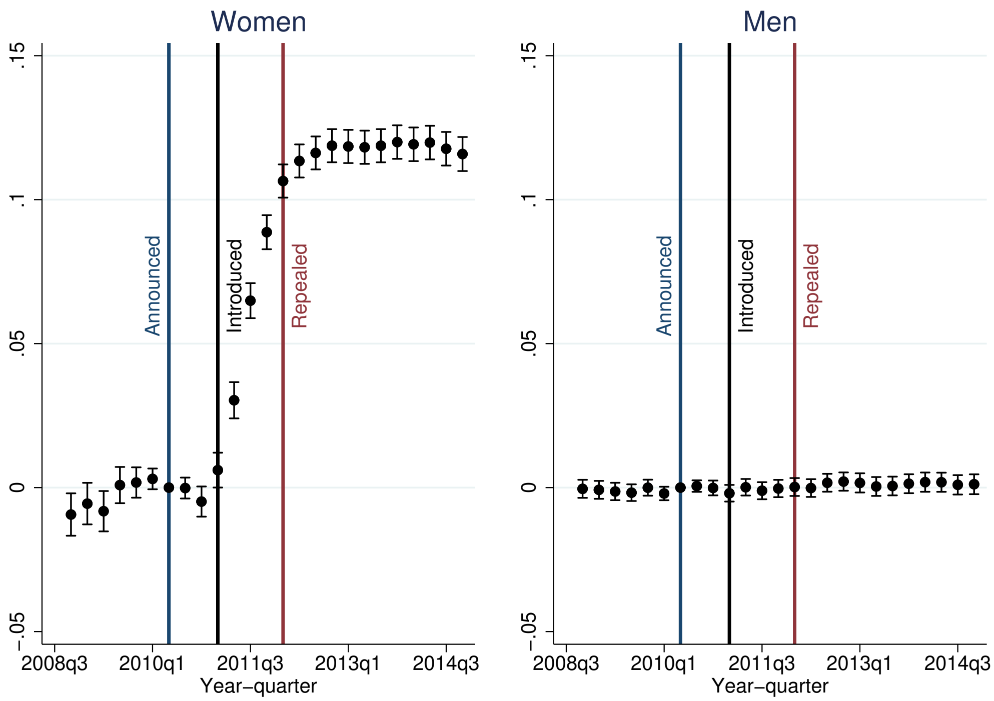


Notes: Percent of women and men currently employed in full-time jobs. Full-time work is defined as working more than 160 hours per month.

C.4 Extensive margin

Figure C.15: G-DiD: Employment indicator (working non-zero hours)

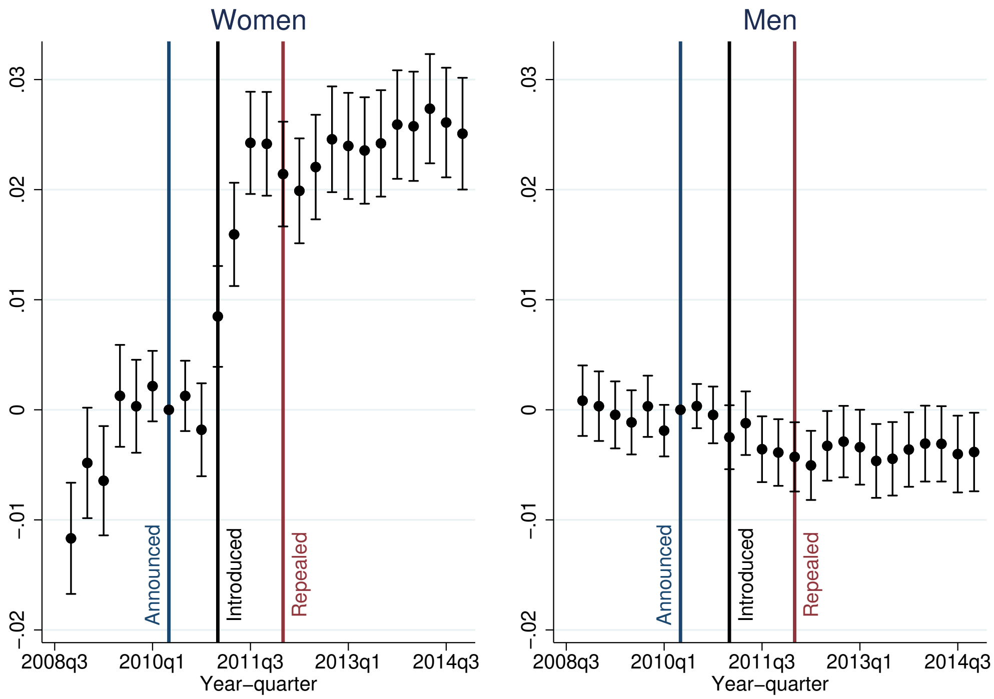
Parental Controls



Notes: Dependent variable (y-axis): employment indicator (working non-zero hours). Parental controls: individual FEs and parental age FEs.

Figure C.16: G-DiD: Employment indicator (working non-zero hours)

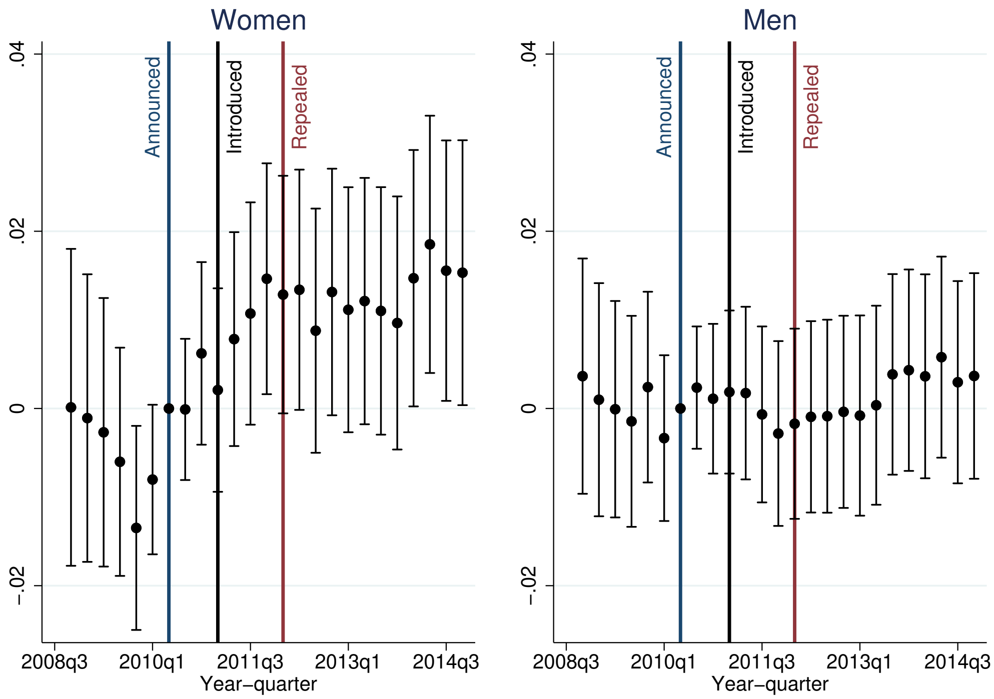
Parental Controls + Family Controls



Notes: Dependent variable (y-axis): employment indicator (working non-zero hours). Parental controls: individual FEs and parental age FEs. Family controls: age of youngest child FEs and number of children FEs.

Figure C.17: G-DiD: Employment indicator (working non-zero hours)

Parental Controls + Family Controls + Sterilisation - Young

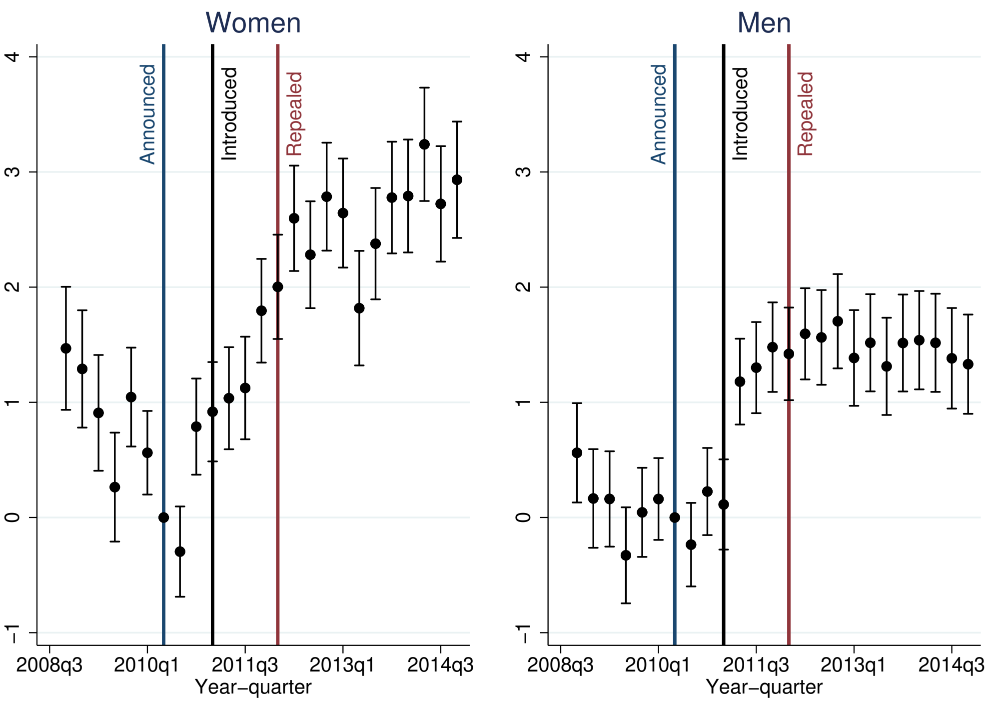


Notes: Dependent variable (y-axis): employment indicator (working non-zero hours). Parental controls: individual FEs and parental age FEs. Family controls: age of youngest child FEs and number of children FEs. Sterilisation - Young: only families where at least one parent has consulted a doctor regarding sterilisation prior to the announcement of the policy. Also excluding families with young children (below the age of 3 throughout the sample period).

C.5 Intensive margin

Figure C.18: G-DiD: Hours worked per month, excluding people working zero hours

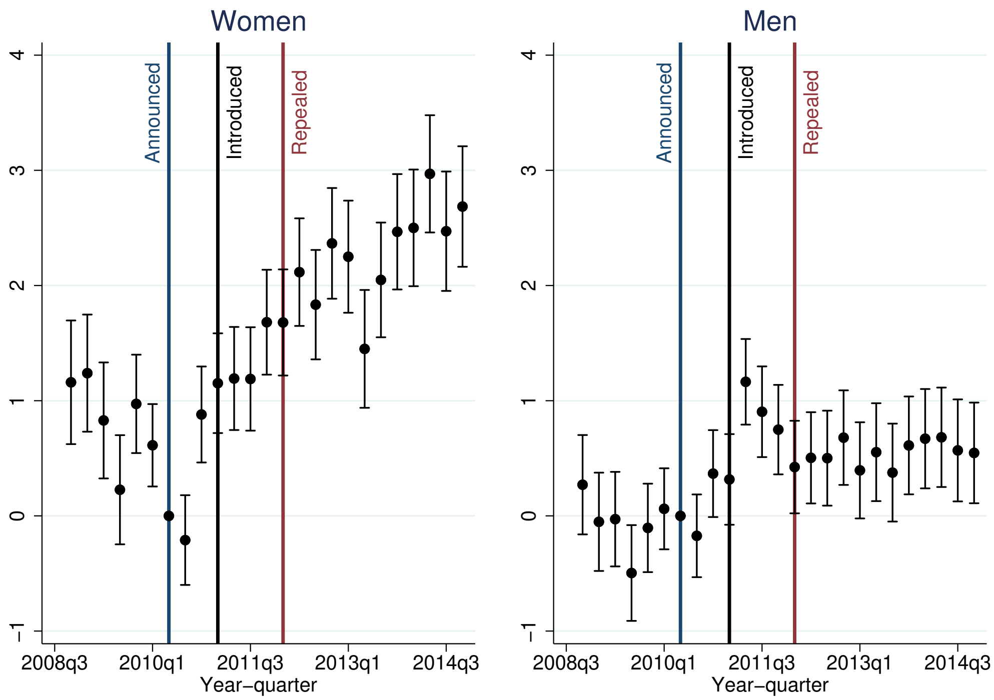
Parental Controls



Notes: Dependent variable (y-axis): hours worked per month. Excluding people working zero hours. Parental controls: individual FEs and parental age FEs.

Figure C.19: G-DiD: Hours worked per month, excluding people working zero hours

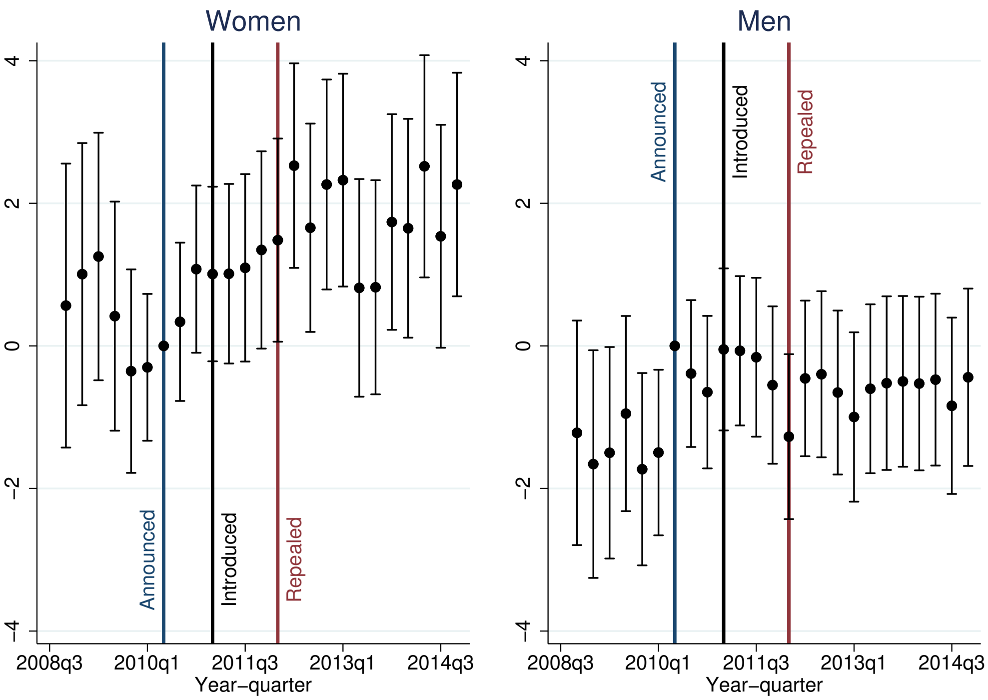
Parental Controls + Family Controls



Notes: Dependent variable (y-axis): hours worked per month. Excluding people working zero hours. Parental controls: individual FEs and parental age FEs. Family controls: age of youngest child FEs and number of children FEs.

Figure C.20: G-DiD: Hours worked per month, excluding people working zero hours

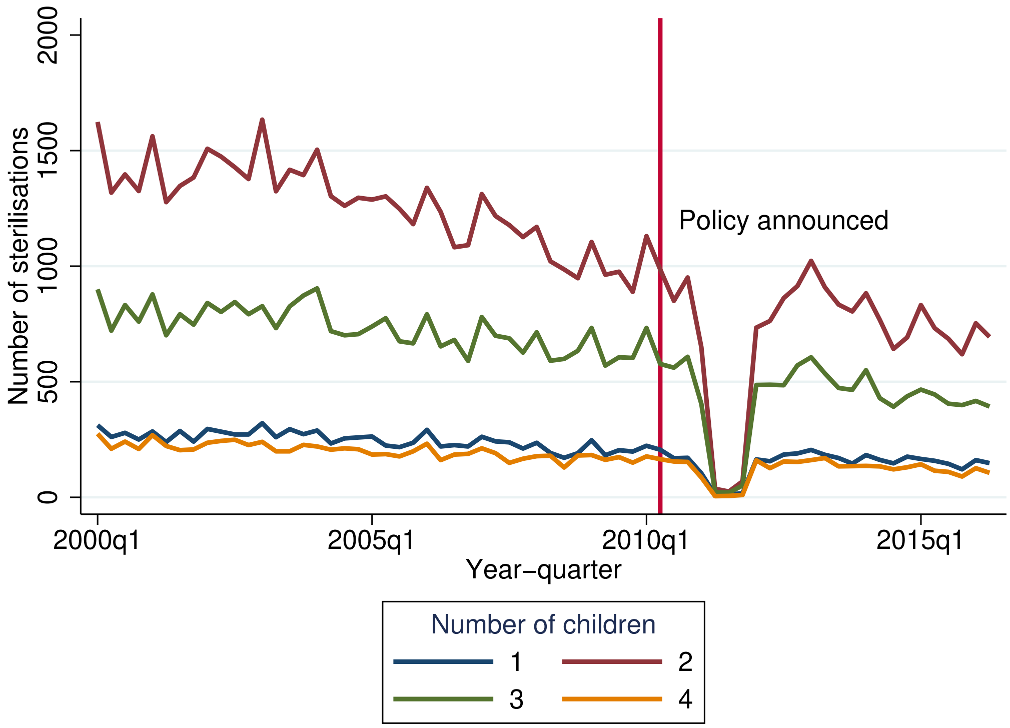
Parental Controls + Family Controls + Sterilisation - Young



Notes: Dependent variable (y-axis): hours worked per month. Excluding people working zero hours. Parental controls: individual FEs and parental age FEs. Family controls: age of youngest ch ild FEs and number of children FEs. Sterilisation - Young: only families where at least one parent has consulted a doctor regarding sterilisation prior to the announcement of the policy. Also excluding families with young children (below the age of 3 throughout the sample period).

C.6 Sterilisation

Figure C.21: Quarterly number of consultations on sterilisation by number of children



Notes: If multiple consultations per person, only the first is included here. In 2011, a co-payment for sterilisation procedures was temporarily introduced, hence the large decrease in consultations. Note that in our sterilisation subsample, we only include families where at least one parent that have consulted a doctor regarding sterilisation no later than May 2010. The the cap on child benefits was announced in late May 2010, and therefore the 2011-drop in sterilisations does not affect our subsample. These are number from the full Danish population, not just our estimation sample.

Chapter 3

University Admission and the Similarity of Fields of Study

University admission and the similarity of fields of study

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Using discontinuities from the Danish college enrollment system, we find that students who are marginally accepted into their preferred program in a broad field that is different from their next-best choice (e.g., business rather than science) experience significant and long-lasting rewards as a result. In contrast, students whose preferred and next-best program lie within the same broad field do not. Exploiting data from online job postings, we find that the estimated effects on skill usage similarly vary according to the degree of similarity between preferred and next-best choices.

Keywords: Field of study, earnings, skills, online job postings, regression discontinuity
JEL classifications: H52, I23, I26, J24, J31

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1. Introduction

Choosing the right education is one of the most important choices facing many young adults as it has lasting effects on both one's future job and earnings. Although the returns to different college majors vary considerably (see for example Altonji et al., 2016, and Altonji & Zhong, 2021), most of the education literature has focused on estimating the effect of an additional year of schooling while ignoring different fields of study.

In a seminal contribution, Kirkeboen et al. (2016) argue that one can only identify the returns to one field of study relative to another. They focus on estimating the return to field of study for students on the margin between two broad fields (e.g. social sciences vs. science) and conclude that students sort into broad fields based on comparative advantage. We also study one's chosen specialization in terms of field of study, namely its impacts on earnings and how students sort into different fields of study. In addition to earnings, we consider the effect of one's chosen field of study on skill utilization in one's future job. Unlike Kirkeboen et al. (2016), our focus is on how the similarity of the applicant's preferred and next-best alternatives affects these outcomes. Specifically, we ask whether exceeding the admission requirements of one's preferred field has an effect on one's future earnings and subsequent skill usage, and next, whether this effect is a function of the similarity of the applicant's preferred and next-best fields of study.

Our hypothesis is that individuals' field choices affect future job opportunities such that candidates in two relatively similar fields will tend to work in jobs requiring more similar skill sets and have more similar earnings. In other words, comparative advantage should play a larger role and generate larger earnings differences when the preferred and next-best fields are less alike. Our goal is thus not to estimate the effects of particular fields of study relative to particular alternatives. Rather, we seek to understand how the degree of similarity between the preferred and next best fields affects the relative returns of admission.

We use a regression discontinuity design to estimate the causal effect of admission to one's preferred field of study on earnings and subsequent skill use. Students in Denmark rank up to eight programs and are admitted into their highest ranked program for which they are qualified according to whether their high school grade point average (GPA) exceeds the program's GPA admission criteria. We use data from the Danish centralized college enrollment system that captures these rankings and combine it with earnings data, baseline characteristics and data on high school GPA from the Danish administrative registers. Finally, we use data from the near population of Danish online job postings. Inspired by Deming and Kahn (2018), we identify keywords indicative of nine different skill categories and construct measures of skills intensity for each occupation category.

Our results support our hypothesis: When we consider students on the margin between two broad fields, we find that students on average realize higher returns to studying their preferred field, consistent with the findings of Kirkeboen et al. (2016).[1] On the other hand, when we consider students on the margin between two narrow fields within the same broad field (e.g. Archeology and History) earnings do no increase on average. The earnings results are mirrored when we consider instead the effects of field of study on skills sets required in subsequent jobs. When prospective students are on the margin between two broad fields, we find significant differences in the demanded skill sets, but when we consider those on the margin between two narrow fields within the same broad field, these effects disappear. To our knowledge, we are the first to compare the earnings effects of students on the margin between narrowly defined fields with those on the margin between two broadly defined fields. This is a useful exercise as it allows us to investigate the nature of comparative advantage in a larger portion of the applicant pool. Moreover, we are the first to show that the degree of similarity

[1] In this paper, we consider broad fields to be Humanities, Social Sciences, Business, Science, Medicine, Law, Technology, Engineering and Life Sciences.

between preferred and next best fields has direct effects on the skills for which students are subsequently hired.

We also contribute to the literature by showing that the effect of threshold crossing on earnings for those on the margin between different broad fields are persistent for at least 14 years after admission. We find suggestive evidence that skill differences also persist as skills and earnings differences appear to go hand in hand. Our results suggest that different fields of study open doors to jobs that require different skill sets, but we are not able to say whether the effect of field of study is due to human capital accumulation or signaling.

Finally, we consider a simple cost-benefit analysis using applicants on the margin between two broad fields, between two narrow fields, and those who just list one narrow field. Our results imply that a *marginal* loosening of the GPA requirements will increase subsequent labor market earnings by improving the allocation of students across fields and by allowing more students to begin studying earlier.[2] An additional policy implication of our results is that students should be encouraged to apply to several programs within their preferred broad field to increase the probability of immediate acceptance.[3]

The literature using a regression discontinuity design to study the effect of post-secondary education generally finds significant positive effects on graduation of being admitted into one's preferred field. However, evidence is mixed concerning the findings on earnings. For example, Öckert (2010) finds low or no earnings effects for Sweden, whereas studies examining the effect of being admitted to higher-quality institutions find substantial effects for the U.S. (Hoekstra, 2009), for Colombia (Saavedra, 2008), and Italy (Anelli, 2016).

[2] For this cost-benefit analysis, we use earnings to capture the benefits of studying a program and measure the costs as based on the fixed rates the universities are paid when an additional student graduates. These fixed rates differ between fields of studies.

[3] Note that we do not suggest that a university admission policy should directly target applicant types as this would induce the strategic behavior of applicants, if, say, the chance of admission in the preferred program is higher when the preferred and next-best programs are in different broad fields.

Kirkeboen et al. (2016) and Hastings et al. (2013) have used college admission data from Norway and Chile, respectively, to examine whether students sort into fields of study based on comparative advantage.[4] Hastings et al. (2013) find small earnings gains from being admitted to one's preferred field of study, but do not find strong support for students sorting in to fields of study by comparative advantage. In fact, Hastings et al. (2013) find that a significant share of prospective students sort into programs with zero or negative returns. In contrast, Kirkeboen et al. (2016) find that students, on average, have considerably larger future earnings in their preferred field of study and that students sort according to comparative advantage.

Our results can rationalize the mixed evidence on the gains of admission to one's preferred field of study provided in the literature, e.g., Kirkeboen et al. (2016) and Hastings et al. (2013).[5] When we follow Kirkeboen et al. (2016) and distinguish between each student's preferred and next-best broad fields and only consider applicants whose preferred and next-best programs lie in *different* broad fields, we also find fairly large earnings gains from being admitted to one's preferred field. However, when we consider students whose preferred and next-best fields lie within the *same* broad field, we do not find significant earnings gains from admission. The analysis of Hastings et al. (2013) includes both types of applicants, and therefore based only on this difference in inclusion criteria, their earnings effects ought to be smaller than those found by Kirkeboen et al. (2016).

Four recent studies use the regression discontinuity design in the Danish college enrollment system. Humlum et al. (2014) estimate the causal effects of exceeding the admission requirements of one's first-choice program on the timing of university enrollment, educational outcomes, and how these factors relate to family formation. They find that threshold-crossing increases the speed with which students enroll in and complete university, enter the labor

[4] See Altonji et al. (2016) for a thorough discussion of Hastings et al. (2013) and Kirkeboen et al. (2016).

[5] Another cause for the divergence is that Hastings et al. (2013) estimate the intent to treat, that is, the effect of marginally exceeding university admission requirements, whereas Kirkeboen et al. (2016) estimate the effect of completing a degree into which one was marginally admitted.

market, and begin a family.[6] In independent work, Andersen et al.(2020) , Heinesen (2018) and Heinesen and Hvid (2019) study the effect of crossing the admission threshold of one's first-choice program on completion, earnings and gender gaps. The study of Heinesen and Hvid (2019) is closest to our paper as they use the set-up in Kirkeboen et al. (2016). Their results are broadly consistent with our earnings results for individuals who are the margin of two different broad fields, the subgroup of applicants that they consider.

The paper is organized as follows. In section 2, we discuss the institutional details of higher education in Denmark. In section 3, we consider the econometric framework employed in this paper. We describe the data in section 4 and provide simple descriptive statistics. In the following section, we provide graphical checks of the research design, while we present our results in section 6. Section 7 concludes the paper.

2. Institutional Details

In Denmark, children must attend compulsory schooling for 9 years, usually from the age of 7 to 16. After completing compulsory schooling, more than half of a cohort completes a 3-year high school program, a precondition for admittance into a university or professional bachelor's degree program. Studying in Denmark is comparatively cheap: University programs are publicly provided and are free of charge. In addition, the government provides generous student grants and optional student loans with favorable terms. The vast majority of university students study a two-year master's degree immediately after their bachelor's degree.

Danish students make their choice of study program when they apply for admission to university. A program identifies a field (e.g., economics) and an institution (e.g., University of Copenhagen) combination. All applications to university programs are handled by a centralized admission system. Once an applicant has been admitted to a program, generally speaking, the only way for her to change her program is to apply through the centralized application system

[6] Their results agree with our finding that those applicants who apply to only one field and exceed the GPA requirement for admittance into that program are able to complete their programs earlier.

in the following year. However, if she would like to change to a program that has vacant slots, she may do so in the current year without reapplying.

During the period we are studying, universities determined the maximum number of students who could be admitted into each program.[7] The number of available slots in a program is determined prior to the period of application. Some programs also have a course-specific admission requirement, which naturally is determined prior to the period of application.[8]

If, after the application deadline, the number of applications exceeds the number of available slots, admission to a program is restricted and only applicants whose high school GPA exceeds a cutoff will be admitted. Programs with fewer applicants than available slots will have no GPA cutoff. About half of all programs have a GPA admission restriction. In practice, this cutoff is the binding constraint facing the vast majority of applicants who are not admitted to a program.

An applicant may list up to eight preferences in her application each year. If her GPA exceeds the GPA cutoff for her first-choice program, she is admitted and will not be considered for lower priorities. If she does not meet the GPA requirement for her first choice, she will be considered for her second-choice program. This process will continue down her preference rankings until she is admitted to one of her listed programs or receives no offer of admittance. In this way, the best students will be offered their preferred education. The application system is strategy-proof such that students' ranking of fields truthfully reflects their preferences.

The GPA admission requirement changes from year to year, mainly due to variation in applicant pools and, to a lesser extent, because of changes in the number of available slots. As a consequence, applicants do not know the exact GPA cutoff when applying. In particular, an applicant with a high school GPA close to the previous year's GPA requirement will not be

[7] There a few exceptions to this practice. A small number of university programs have a maximum number of students set at a central level. For example, as medicine involves mandatory practice, universities cannot alone determine the size of the incoming class.

[8] An example of a course-specific admission requirement is that the applicant has passed (A-level) math in high school.

able to predict whether or not she will be accepted.[9]

There are two mechanisms in place which allow admission for students with lower GPAs (see Online Appendix C for more details). Students admitted through these mechanisms make up less than a quarter of the admitted students.

3. Econometric Specification

Does the similarity of an applicant's preferred and next-best fields affect the earnings consequences of meeting one's preferred degree admission requirement? To answer this question, we classify applicants into three types: those who apply to *only one* field; those whose preferred and next-best fields belong to two different broad fields, that is, are on the margin of acceptance *between* two different fields (e.g., have a preferred broad field of Social Science and a next-best broad field of Medicine); and those applicants who are on the margin of acceptance between two fields *within* a broad field (e.g., have a preferred narrow field of Anthropology and next-best narrow field of Sociology, both of which lie within the broad field of Social Science). *Between* applicants are the subject of Kirkeboen et al. (2016), but, as discussed in the data section, comprise only about a quarter of the Danish applicant pool.

We will estimate the causal effect of exceeding the primary GPA requirement of one's preferred field of study (hereafter cutoff) on earnings and skills:

$$(1) \qquad y_{it} = \sum_{j \in O,W,B} \left[\beta_j \mathbb{I}(r_i > 0) * \mathbb{I}(type = j) \right] + f(r_i) + \alpha x_{it} + \varepsilon_{it}$$

where y_{it} is individual i's outcome of interest in year t, and r_i is the application score defined as the difference between an individual's high school GPA and the GPA cutoff of her preferred program, normalized by the standard deviation of GPA. We standardize in order to facilitate the interpretation of the running variable. The index j corresponds to the applicant type: applicants who list only one program are denoted by O, those whose preferred and next-best

[9] Humlum et al. (2018) provide a valuable discussion of time variation in the Danish admission requirements. They demonstrate a high degree of time variation during the application years that we consider.

fields are within the same broad field are denoted as W, those who are on the margin between two different broad fields are denoted by B. Our interest lies in estimating the β_j, the intention to treat effect (ITT) of admittance into one's preferred program, for each applicant type rather than pooling these effects as is often done in the literature. $\mathbb{I}(r_i > 0)$ is an indicator function taking the value of one if an individual's GPA exceeds the cutoff of the preferred program.

Subject to an appropriately specified running variable (application score), we can interpret any discontinuous jumps in earnings as the effect of meeting the GPA admissions requirement. We estimate various specifications of $f(r_i)$ including linear, quadratic, and an interaction between $f(r_i)$ and $\mathbb{I}(r_i > 0)$. Finally, we control for a set of predetermined variables, x_{it} including sex, the earnings of the applicant's father at age 16, an indicator for whether or not the father's earnings is missing, age, age squared. Also included in x_{it} are a set of calendar year indicators, a set of application year indicators, a set of preferred narrow field indicators, a set of next-best narrow field indicators, a set of preferred institution indicators and a set of next-best institution indicators as well as indicators for applicant type.

GPA is recorded to the first decimal place only, and in this sense, our running variable is discrete. Thus, we have applicants with an application score of zero. Whether all such individuals are considered as passing the admission criteria or not varies at the program level and is not observable to us. To address this issue we drop applicants whose application score is zero prior to estimation, that is, we employ the so-called "donut-RD" estimator as used by Barreca, et al. (2011).[10] In the case of a discrete running variable, Lee and Card (2008) recommend clustering the standard error on the discrete values of the running variable. Earnings may also be correlated within program type. Following Heinesen (2018) and Humlum et al. (2017), we cluster on preferred program in the earnings regressions as this appears to be more

[10] Tables B4 and B5 in the Online Appendix present the results when those whose application score is zero are included either as controls or as treated. Estimated earnings effects remain significant and positive, but the estimates are dampened due to the measurement error in the dummy for threshold crossing.

conservative in the earnings regressions.[11]

After carefully examining evidence that the requirements for the regression discontinuity design are met in the Danish enrollment system, we estimate equation (1) with OLS using application score windows of 2, 1 and 0.5.

The average effect on earnings, over individuals and time, of meeting one's preferred GPA requirement is captured by the β's. We would also like to understand how these effects evolve over time. To do so, we will also estimate equation (1) separately for each year after application.

4. Data and Descriptive Statistics

4.1 Data

4.1.1 Coordinated Enrollment System and Danish Register Data

Fundamental to our analysis is the availability of detailed information on student preferences over programs of study at the time of application, where program identifies a field (e.g. economics) and institution (e.g. University of Copenhagen) combination. The Coordinated Enrollment System (CES), by which all college applications are processed, has provided this data from 1993 to 2014 for all applicants along with each applicant's personal identifier, a key by which additional register data can be merged. Statistics Denmark maintains several high quality administrative registers that cover virtually the entire population of Denmark. Demographic characteristics are taken from the population registers which are available from 1980 onward. Earnings and income histories are taken from the income registers which are available from 1980-2014 and high school GPA is taken from the education registers. Our measure of total earnings includes wages and self-employment income. Monetary figures are shown in 1000s of 2014 DKK.[12]

[11] Estimation results from using the running variable to cluster standard errors are presented in Online Appendix Table B3.
[12] An approximate exchange rate of 1USD to 6.5DKK can be used.

4.1.2 Job Posting Data

Danish online job vacancy data from 2007-2014 are supplied by the Danish consultancy firm, Højbjerre Brauer Schultz (HBS). HBS collects online job vacancy data from numerous Danish online jobs boards, and thus, they believe that their data contains the near universe of publicly accessible Danish online job posts.[13] Most job postings have a firm identifier that can be linked with the Danish register data and a 6-digit occupational code.[14] The data set contains raw keywords from the job post.

In order to be able to match with the Danish register data, only job posts with non-missing firm identifiers and occupational codes are considered.[15] Largely following Deming and Kahn (2018), we map a selection of keywords into nine skill categories: character, cognitive, computer (general), computer (specific), customer service, financial, management, social, writing/language. For example, a job posting containing the raw keyword "teamwork" would be a job posting that requires social skill. We similarly assign the top 2000 or so most frequent keywords (corresponding to the vast majority of keyword observations) to one of the nine skill categories or a noise category. We then use this as a training sample when applying supervised machine learning methods to assign the remaining keywords to a skill or noise category. Once all keywords have been assigned a category, we calculate the fraction of non-noise words indicative of a certain skill for each job post. To ease interpretation, each skill measure is standardized by subtracting the average fraction of words indicative of the skill in the job posting data and dividing by the standard deviation using job posting observations for the entire Danish labor market.

[13] http://www.hbseconomics.dk/wp-content/uploads/2017/09/Eftersp%C3%B8rgslen-efter-sproglige- kompetencer.pdf

[14] If the firm identifier is not listed directly in the job post, HBS imputes it from publicly accessible registers using the firm name and address listed in the job post. Occupation is imputed from the job title. See Online Appendix C for more detail.

[15] See the Online Appendix C for a detailed description of the skills data, the construction of skill variables and the creation of the skills estimation data set.

To understand how skills on the job are shaped by just exceeding the admission threshold of one's preferred degree, we first look at those individuals in our estimation sample that we can match at the firm-occupation level with a job posting. We are able to match about 35% of the individuals and almost 10% of the observations in our estimation sample from 2008-2014.

Unlike earnings, there is no natural way to rank a skill mix. Since we want to study whether skills change when crossing the admission threshold, we calculate the distance between an individual's skill use and a relevant benchmark of skill use in their preferred narrow field, henceforth "skill distance". The benchmark that we consider is the average skill use of those who have completed a master's degree in a particular narrow field. We calculate such a distance for each skill category (analytic, social, etc.). These skill distance measures will reveal how threshold crossing affects skills, and to what extent this is a function of the degree of similarity between preferred and next best field. If skill sets within a broad field are more similar as compared to skills across broad fields, we expect to see larger reductions in skill distances as a result of threshold crossing for *Between* applicants when compared to *Within* applicants.

As we are only able to match a relatively small fraction of job spells at the individual level, we next impute skills by assigning skill usages to individuals based on the average skill usage in the occupation in which they work and again compute skill distances relative to the benchmark.[16] Using these methods, we are able to assign skills to about 93% of individuals and about 79% of yearly observations.

4.2 Preferred and Next-best Fields

We use the notion of *preferred field*, defined from the local course ranking around an applicant's GPA rather than the first-choice field (i.e. the field which is given first priority). Changing the focus from preferred field to first-choice field (as well as implied sample selection

[16] There are 228 occupational groups. We calculate average skill use for each skill category for each of the 228 occupations. All those working in an occupation are then assigned the average skills for that occupation.

criteria discussed below) does not significantly alter our results, a consequence of the fact that the vast majority of Danish applicants list few programs.

From the *program* level priority ranking we aggregate preferences to the narrow *field* level and assign the minimum GPA requirement. For example, if an individual applies to the University of Copenhagen's Sociology program with GPA admission requirement of 9.0 and Aarhus University's Sociology program with GPA admission requirement of 8.7 these two individual preferences would be aggregated to a narrow field of Sociology with minimum GPA requirement 8.7. We aggregate at 50 rather narrow definitions of field (e.g. Sociology) whereas Kirkeboen et al. (2016) aggregate at a rather broad definition of field (e.g. Social Sciences).

4.3 Sample Selection

We consider Danish first-time applicants aged 17-25 with non-missing high school GPAs who applied to CES between 1996 and 2006.[17] We use the years 1993-1995 to determine whether individuals in 1996 and later are indeed first-time applicants. We do not consider applicants after 2006 because of a large change that was made to the Danish grade scale. We focus on applicants whose preferred and next-best fields are for university programs.[18] We drop applicants whose most preferred field does not have a GPA requirement for admission.[19]

Only One applicants constitute the majority of the estimation sample for the following reasons. First, we aggregate fields of study at the 6-level, resulting in 50 narrow fields as described above. Some students may have listed multiple preferences within the narrow field, but they will still be categorized as *Only One* applicants. Second, we drop *Between* and *Within* applicants whose preferred and next-best fields have non-descending GPA admission

[17] We remove immigrants as information on GPA or other demographics are not available, i.e. earnings of parents at age 16. See Table D4 for a detailed description of the basic cleaning performed.
[18] There are 9 institutions at the university level. See Online Appendix Figure D2.
[19] We follow Kirkeboen et al.'s (2016) construction of an estimation sample suitable for regression discontinuity analysis.

thresholds and also those whose GPA never exceeds any an admission threshold.[20] Third, we drop a large number of individuals who list preferences over study programs in which we are not interested because they are not undertaken at a university (see Table D5 in Online Appendix D).

4.4 Descriptive Statistics

About half of all individuals who applied through CES between 1996 and 2006 listed just one preferred program, about 22% listed two programs and 18% listed three programs. Only 6% listed more than four programs despite the ability to list up to eight.

Panel (A) of Table I presents descriptive statistics, by type of applicant, calculated from the *main estimation sample*. Comparing these columns with the equivalent figures for the *full sample* in panel (B) reveals that the two samples are rather similar, i.e. earnings 8 years after application are effectively identical, though the estimation sample is perhaps positively selected: applicants in the estimation sample tend to have higher application scores, slightly more educated parents with higher earning fathers.

Comparing summary statistics across applicant type in the first six columns of panel (A) reveals noticeable differences. We see that almost 75% of the individuals in the *estimation sample* apply to *only one* field, as opposed to 55% of the data set before imposing sample selection criterion; this is primarily driven by the fact that *Between* and *Within* applicants whose preferred and next-best fields have non-descending GPA admission thresholds are dropped. Relative to *Only One* or *Between* applicants, *Within* applicants make almost 50,000 DKK less 8 years after application, are more likely to be women, and have fathers who tend to earn less. Whereas one in two *Between* and *Within* applicants will be offered their first priority and about 10% will receive no offer at all, 66% of *Only One* applicants will be offered their first (and

[20] We remove 88 individuals who completed a master's degree in less than 3 years after being admitted to a bachelor, these are likely transfer students from abroad. We replace negative earnings with zero and earnings above the 99.9[th] percentile with the 99.9[th] percentile value. For most of the analysis, we use an estimation window of 2.0 application score points (i.e. standardized GPA), but also show results with a narrower window of 1.0 and 0.5.

only) priority.[21]

Immediately noticeable from Figure I is the large concentration of *Within* applicants, almost 60%, who have Humanities as a preferred field in the full and estimation samples. The large share of *Within* applicants with preferences for Humanities reconciles well with the lower subsequent earnings displayed for these applicants in Table I.

The estimation sample contains larger shares of applicants with preferred fields of Social Science, Medicine and Law and generally lower shares of applicants with preferred fields of Science, Technology and Engineering.[22] Online Appendix Table D3 contains estimation results from our main specification excluding humanities, and various other fields. Reassuringly, the results do not change substantially.

As discussed earlier, we will also consider the effects of threshold crossing on various measures of skill usage. Figure II presents statistics of the standardized skill usage, defined at the occupational level, for each skill category by applicant type. Bars depict mean values whereas dots show standard deviations. Mean values above (below) 0 indicate that the skill usage is higher (lower) than that found in the entire job posting data set. For instance, the individuals in our estimation data set have much higher levels of cognitive skill levels in their jobs, a finding that makes sense given that we are selecting those who apply for university education. Interestingly, despite the different distribution by broad field type shown in Figure I, the skill distribution look relatively stable across applicant type.

[21] Columns (3) and (4) in Panel (A), describing *Between* applicants, is the sub-sample that most closely mimics the sample used by Kirkeboen et al. (2016). Our estimation sample contains almost 7,000 *Between* applicants, noticeably less than the 50,000 used in Kirkeboen et al. (2016). There are three main reasons for this difference in sample size. First, we focus on university educations, more than halving our sample, whereas Kirkeboen et al. (2016) includes non-university educations. Second, on average, Danish applicants list fewer preferences relative to Norwegians. Third, many STEM programs have no admission requirements and are consequently dropped from our analysis.

[22] Also, notice that broad fields of Business or Law each consist of only one narrow field.

TABLE I: SUMMARY STATISTICS

	(A) Estimation Sample								(B) Full Sample	
	Only One		Between		Within		All		First time applicants	
	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
Age at application	20.78	(1.35)	20.83	(1.31)	21.04	(1.34)	20.82	(1.35)	20.83	(1.35)
Female	0.58		0.55		0.61		0.58		0.54	
Earnings 8 years after application	258.35	(188.04)	263.81	(181.78)	215.78	(162.29)	254.68	(185.04)	259.24	(191.55)
Application Score	0.27	(0.85)	-0.05	(0.76)	-0.05	(0.78)	0.19	(0.84)	-0.18	(1.11)
Mother has higher education	0.51		0.52		0.51		0.51		0.48	
Father has higher education	0.47		0.48		0.46		0.47		0.44	
Father's earnings	493.09	(581.02)	484.78	(451.51)	440.07	(356.34)	486.26	(543.61)	481.46	(805.81)
Fields ranked	1.00	(0.00)	2.56	(0.90)	2.62	(0.93)	1.41	(0.83)	1.51	(0.87)
Institutions ranked	1.21	(0.52)	1.84	(0.90)	1.76	(0.89)	1.36	(0.69)	1.38	(0.69)
Rank of best offer	1.05	(0.23)	1.64	(0.88)	1.61	(0.83)	1.23	(0.58)	1.16	(0.52)
Offered first priority narrow field	0.69		0.51		0.52		0.64		0.72	
Offered second priority narrow field	0.00		0.34		0.33		0.09		0.08	
Offered third priority narrow field	0.00		0.04		0.04		0.01		0.02	
No offer	0.31		0.10		0.09		0.26		0.19	
Individuals	34383		6961		4869		46213		104383	

The column titled “Only One” refers to applicants who applied for one narrow field of study. The column titled “Between” refers to applicants whose preferred and next-best fields are in different broad fields whereas the column titled “Within” refers to those applicants whose preferred and second-best fields are within the same broad fields. About 3% and 6% of mother and father education is missing, respectively. About 3% of father's earnings is missing. Monetary figures shown in 1000s 2014 DKK. The full sample first time applicants sample corresponds to the sample after basic cleaning and the removal of individual who just have preferences for non-university. About 1% of observations don't have earnings in the 8th year after application in the estimation sample: About 12% of observations don't have earnings in the 8th year after application in the full sample of first time applicants mainly because all years are considered (not just more than 7 years after application). Offer refers to not receiving an offer in the current year. The row titled ‘No offer’ in the Table I refers to the percent of individuals who did not receive an offer for admission in the current year.

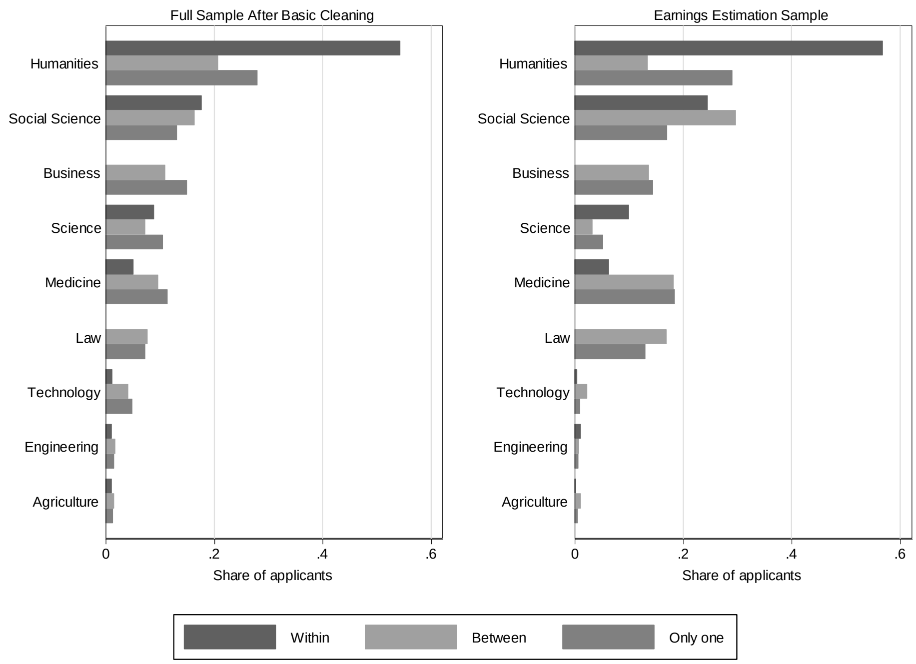

FIGURE I: DISTRIBUTION OF PREFERRED FIELD BY APPLICANT TYPE

"Only one" refers to applicants who applied for one study field. "Between" refers to applicants whose preferred and next-best fields are in different broad fields. "Within" refers to those applicants whose preferred and second-best fields are within the same broad fields. The term "preferred" has a different meaning in the two pictures: as the full sample is created prior to the sample selections necessary to define preferred and next-best fields, "preferred" in the full sample actually corresponds to first choice.

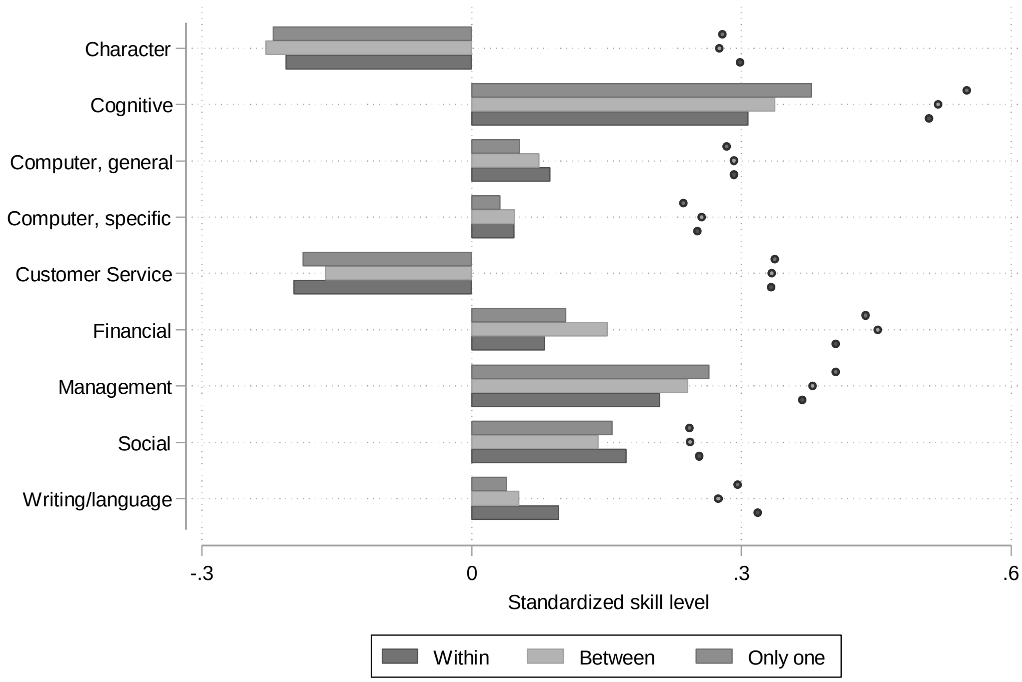

FIGURE II: DISTRIBUTION OF STANDARDIZED SKILL LEVEL BY APPLICANT TYPE

Bars correspond to means whereas dots correspond to standard deviations. "Only one" refers to applicants who applied for one study program. "Between" refers to applicants whose preferred and next-best fields are in different broad fields. "Within" refers to those applicants whose preferred and second-best fields are within the same broad fields.

5. Graphical Illustration of Research Design

Our identification strategy relies on the discontinuity at the GPA admission giving us exogenous variation. Before proceeding to the estimation results, we will examine the graphical evidence to verify the validity of the research design and to provide a sense of the effects we expect to see. We used the naturally occurring (discrete) values of the application score rather than collapsing the data further into broader bins. All figures include a local linear regression line estimated on either side of 0 using a triangular kernel and a bandwidth of 1.

Figure III plots the share of applicants who are offered admission (in the current year) and complete their preferred field against their application score for each applicant type. There is a clear and large discontinuous jump in the share of applicants who receive an offer below and above 0, regardless of type. Applicants with application scores above 0 almost always receive an offer. Those whose application score falls below the required GPA admission threshold are much less likely to receive an offer. The positive admission probability to the left of the cutoff results from the existence of alternate admission mechanisms, see Online Appendix C for more institutional details.

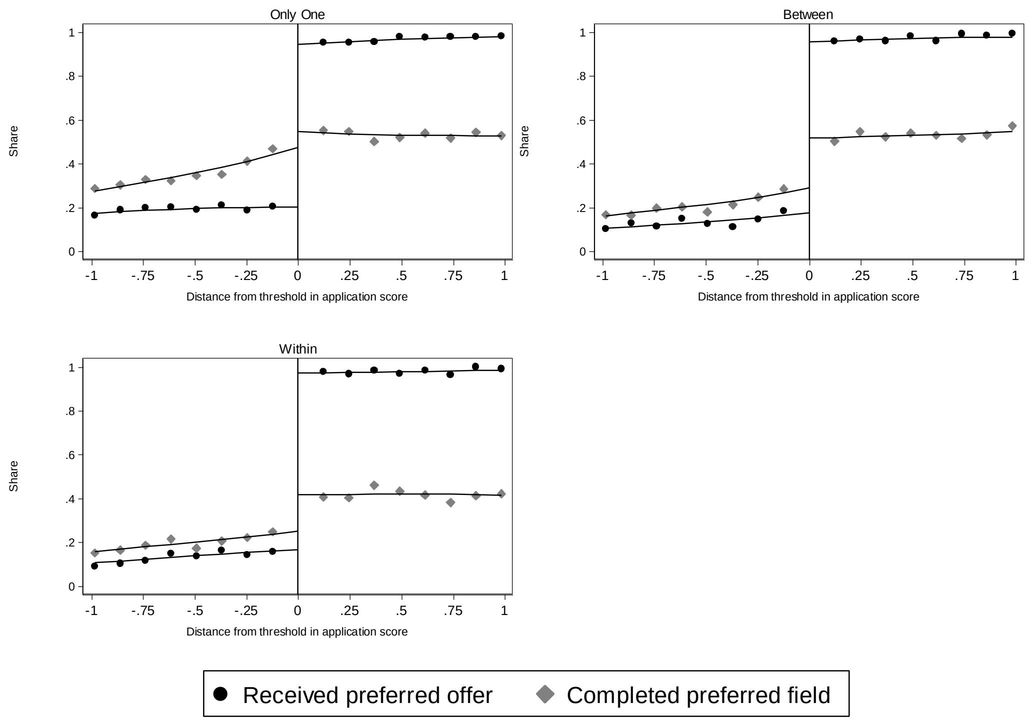

FIGURE III: ADMISSION THRESHOLDS AND PREFERRED FIELD OFFER AND COMPLETION

"Only One" refers to applicants who applied to one field of study. "Between" refers to applicants whose preferred and next-best fields are in different broad disciplines. "Within" refers to those applicants whose preferred and second-best fields are within the same broad discipline. "Offer" here refers to an offer of admission in the current year.

The size of the discontinuous jump decreases when we look at the share of applicants completing their preferred degree, but it remains strong in the case of *Between* and *Within* applicants. *Only One* applicants are clearly more committed to studying a particular program and are likely to seek admission to this program in a subsequent year if not admitted immediately. Interestingly, the share of applicants above 0 who complete their degrees is more or less constant with respect to the application score, regardless of type.[23]

Regression discontinuity design is only valid if individuals have imprecise control over their application score relative to the GPA cutoff of their preferred program. If we detect discontinuities in the density of the application score, we may suspect that applicants are sorting, placing the validity of the identification strategy in question. Figure IV displays the log density of application scores by type, and there is no evidence of bunching at the GPA cutoff.

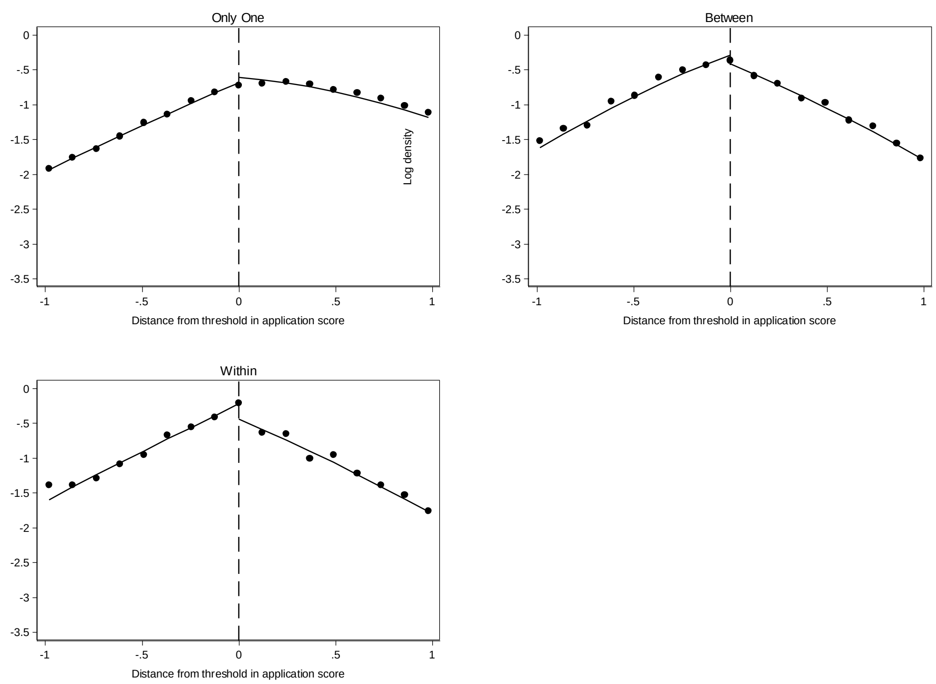

FIGURE IV: BUNCHING CHECK AROUND THE ADMISSIONS CUTOFFS

"Only One" refers to applicants who applied to one field of study. "Between" refers to applicants whose preferred and next-best fields are in different broad fields. "Within" refers to those applicants whose preferred and second-best fields are within the same broad fields.

Figure V plots the pooled average earnings by applicant type for years 7–18 after application. The interpretation of this effect is somewhat difficult: It is the average effect over

[23] Online Appendix A contains plots analogous to Figure III for our control variables. No significant discontinuities are detected.

all observable years after graduation and across all program types. Rather than determining the precise magnitude of the effect, here we seek to verify a non-negligible discontinuous jump in earnings, acknowledging that the magnitude is likely to change once we control for factors such as specific program indicators and year of application indicators. The figure does show jumps in average earnings for applicants whose application score exceeds the admission criteria. For *Only One* and *Between* applicants, we see a jump in earnings of almost 30,000 DKK per year, or around 4,500 USD. In contrast, *Within* applicants experience a very small reward for meeting the admission criteria.

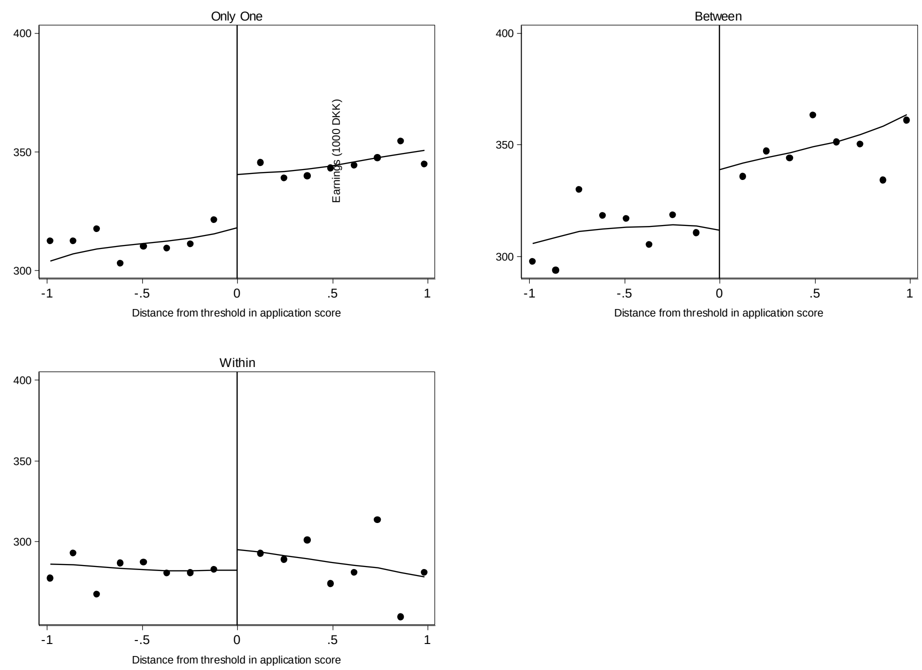

FIGURE V: ADMISSION THRESHOLDS AND AVERAGE POST-GRADUATE EARNINGS

"Only One" refers to applicants who applied to one field of study. "Between" refers to applicants whose preferred and next-best fields are in different broad disciplines. "Within" refers to those applicants whose preferred and second-best fields are within the same broad discipline.

Next, we look at the effects of threshold crossing on skill formation for those applicants on the margin between two different broad fields and for those on the margin between two fields within the same broad field. Specifically, we compare the intensity of skills usage of the applicant to the average skills usage of graduates with the same preferred narrow field. If admission has an effect on skills usage in subsequent jobs, this distance should be reduced in a discontinuous manner at the GPA cutoff of the preferred field.

If we only use job spells which can be linked to an online job ad, we have too few observations to graphically show the effect of threshold crossing. However, we can use simple t-tests on the difference in skill distances just above and below the admission threshold for each skill category. For this exercise, we use a window of 1.0 standard deviation of the GPA score.

Table II shows these differences in skill distances along with the t-statistics in parentheses. Columns (1) and (2) present these results for *Between* and *Within* applicants, respectively.[24] Considering first column (1), we see that crossing the admission threshold for one's preferred degree on average reduces the distance between an individuals' subsequent skills use in a job and the average skill use for graduates with the same preferred degree. This decrease in distance is present in all nine skill categories. In four of these nine skill categories the reduction in skill distance is significant. We see particularly large and significant effects on cognitive, social, and computer skills.

The second column of Table II shows the equivalent t-test for the *Within* applicants. We see that none of the t-tests for the nine skill categories are significant. Moreover, the values of the differences are less negative for *Within* applicants when compared to *Between* applicants. This simple exercise suggests that skills used in subsequent jobs are affected by admission into one's preferred field, and that these skills vary more across broad fields than within broad fields.

[24] For the skills data, we are focusing on *Between* and *Within* applicants. For *Only One* applicants, we find that the skills distance decreases at the cutoff for three out of the nine skills when using all observations from 7 years after application and beyond. However, when only using observations from 8 years after application and beyond, we only find one significant negative effect. This reflects that persons just applying to one program usually re-apply in the following year if not admitted. Thus, differences in skills for *Only One* applicants should be expected to be of a more temporary nature.

Since we only have individual skills measures for a small share of our estimation sample, we focus on the measure of skills defined at the occupation level for the rest of the analysis. Figures B2 & B3 in Online Appendix B plot the pooled average skill usages for each of the nine skill categories for *Between* and *Within* applicants respectively, for years 7–18 after application. Clear non-negligible discontinuous drops in skill distances are present for *Between* applicants in all categories except Writing/language. The analogous plots for *Within* applicants are much noisier and suggest no effect of threshold crossing.

TABLE II: T-TESTS ON SKILL DIFFERENCES

Skill Category	(1) *Between*	(2) *Within*
Character	-0.0346 (-1.18)	-0.0263 (-0.67)
Cognitive	-0.0827*** (-2.83)	-0.0110 (-0.36)
Computer, general	-0.0545** (-2.00)	-0.0392 (-0.92)
Computer, specific	-0.104*** (-3.17)	0.00887 (0.21)
Customer Service	-0.0527 (-1.59)	-0.0318 (-0.83)
Financial	-0.00364 (-0.12)	0.0630 (1.62)
Management	-0.0123 (-0.42)	-0.0421 (-1.30)
Social	-0.0560* (-1.77)	0.0284 (0.69)
Writing/language	-0.00990 (-0.30)	-0.0150 (-0.37)
Observations	1948	1252

t statistics in parentheses. "*Between*" refers to applicants whose preferred and next-best fields are in different broad fields. "*Within*" refers to those applicants whose preferred and second-best fields are within the same broad fields. These differences were calculated from applicants close (a window of 1) to the admission threshold. *** p<0.01, ** p<0.05, * p<0.1

6. Results

Table III presents the results from estimating equation (1). The effects of marginally surpassing the GPA requirement of one's preferred field are quite robust to the particular specification used. *Between* applicants realize gains of about 20,000-30,000 DKK per year on average or from 8%-11% of annual earnings 8 years after application. Likewise, the average benefit of threshold crossing of *Only One* applicants is 7%-11% of annual earnings. On the contrary, *Within* applicants see effectively zero and insignificant effects of surpassing the GPA requirement of their preferred degree.

To understand how these effects evolve over time, we estimate equations (1) for each year, starting in the year of application, these results are shown in Figure VI. In addition, we estimate a regression that includes a quadratic in actual work experience. Although experience is likely endogenous, we include it only to explore the degree to which our results are sensitive to time differences in the timing of labor market entry.

Figure VI verifies that rewards begin to accumulate for *Between* applicants around the time that most students have completed their studies, 6 to 7 years after application. In the year of application and the year following, applicants admitted on the margin realize earnings losses as they study rather than work. While studying, these applicants realize no significant effects on earnings. The figure reveals that the rewards realized by *Between* applicants are not just concentrated early in the work life. In fact, these applicants receive a rather constant, and predominantly significant, bonus that hovers around 20,000-25,000 DKK per year, from 7-16 years after application. This is true regardless of whether or not experience is included. *Within* applicants see no large positive statistically significant effects, again regardless of whether or not experience is included.

The time profile of effects for *Only One* applicants shows substantial time variation. In the year of application and one year later, *Only One* applicants who just exceed the admission

TABLE III: EFFECTS OF PREFERRED DEGREE ON EARNINGS, BY APPLICANT TYPE

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Only One * (Application Score>Cutoff)	19.87***	16.34***	28.98***	28.95***	18.90***	25.39***	20.99***
	(5.13)	(4.15)	(4.28)	(3.88)	(3.93)	(3.64)	(4.28)
Within * (Application Score>Cutoff)	-16.58	-6.87	5.52	-1.53	-4.46	4.61	-2.14
	(10.46)	(6.44)	(5.99)	(13.39)	(5.72)	(5.69)	(7.06)
Between * (Application Score>Cutoff)	26.70***	17.84***	29.64***	34.00***	19.47***	22.42***	22.18***
	(6.85)	(5.66)	(6.60)	(10.53)	(4.78)	(7.73)	(7.21)
Observations	300,233	300,233	300,233	300,233	300,233	228,233	141,532
Individuals	46213	46213	46213	46213	46213	34936	21490
Window	2.0	2.0	2.0	2.0	2.0	1.0	0.5
Preferred and second-best narrow field indicators	NO	YES	YES	YES	YES	YES	YES
Control variables	NO	NO	YES	YES	YES	YES	YES
Different slopes	NO	NO	YES	YES	YES	YES	YES
Quadratic terms	NO	NO	YES	YES	NO	NO	NO
Functions of running variables by type	NO	NO	NO	YES	NO	NO	NO
Preferred and second-best institution indicators	NO	NO	NO	YES	NO	NO	NO

"Only One" refers to applicants who applied for one study program. "Between" refers to applicants whose preferred and next-best fields are in different broad fields. "Within" refers to those applicants whose preferred and second-best fields are within the same broad fields. The set of control variables includes sex, a quadratic in age, father's earnings, an indicator for whether father's earnings are missing, calendar year indicators and indicators for year of application. All regressions include application score and indicators for *Within* type and *Between* type. Standard errors, clustered at the narrow field level (there are 50), shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

requirement experience large negative statistically significant effects as these applicants presumably study rather than work. However, by the second year after application these effects disappear suggesting that many of the *Only One* applicants who did not meet the entry requirement initially re-apply the subsequent year.

The positive threshold crossing effects for these applicants become apparent around the time that students start to complete their studies 6 to 7 years after applications, reaches a maximum at 8 years when the vast majority have completed their studies, then starts to trend downward. The level of the effects also drops early in the profile once experience is included. These facts reconcile well with *Only One* applicants realizing gains to threshold crossing that are due to entering the labor market earlier. In Online Appendix Figures B1 and B4, we explore this possibility further. First, in Online Appendix Figure B1, we plot the running variable against the number of years to MA graduation, measured from the application year, for each applicant type. No discontinuities are detected for the *Between* or *Within* applicants, but a clear and significant discontinuous drop of about a quarter of a year is present at the admission threshold for *Only One* applicants. Second, in Online Appendix Figure B4, we show the raw earnings differences over time for all applicant types. In each panel, we split applicants in two groups depending on whether or not their GPA exceeded the admission threshold for their preferred field of study. We see that the earnings differences for *Only One* applicants (lower left panel) are almost eliminated when we lag earnings by one year for applicants with GPA below the admission threshold (lower right panel). This confirms that earnings gains to admission for *Only One* applicants to a large extent can be explained by earlier labor market entry. However, we cannot rule out that motivational effects may also partially explain the earnings gains to admission for *Only One* applicants. This would be the case if *Only One* applicants are more motivated due to admission compared to *Within* applicants for whom we do not find any earnings gains from admission, and thus, no motivation effects from being admitted to one's preferred field.

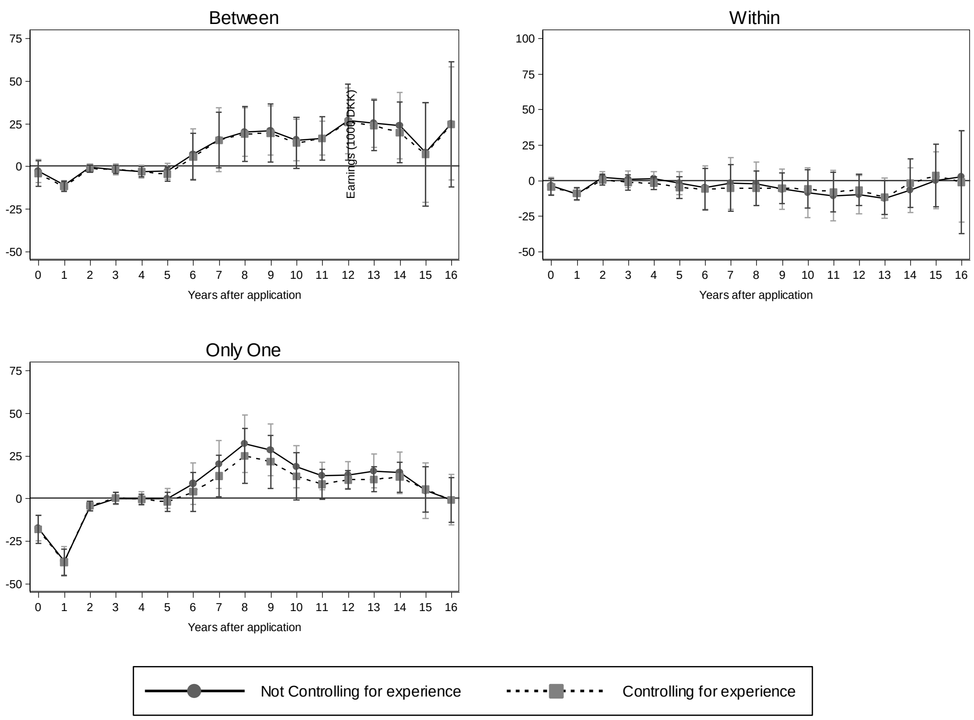

FIGURE VI: ITT PAYOFFS TO PREFERRED DEGREE OVER TIME (DKK YEAR)

"Only One" refers to applicants who applied for one study program. "Between" refers to applicants whose preferred and next-best fields are in different broad fields. "Within" refers to those applicants whose preferred and second-best fields are within the same broad fields. This figures plots the results from estimating equation (2) by year.

6.3 Comparative Advantage

In this section, we investigate the degree to which comparative advantage can explain our main result that individuals whose preferred and next-best fields lie in different broad fields generally obtain higher earnings, whereas applicants whose preferred and next-best fields lie within the same broad field generally do not. In other words, to what extent can our main results be explained by individuals preferring fields in which they have a comparative advantage?

We try to answer this question in three ways. First, we examine if skills play a role in generating earnings differences between fields. If the hypothesis of comparative advantage is true, we would expect to observe larger skill differences across broad fields in which we also observe larger differences in earnings. Second, following Kirkeboen et al. (2016), we examine whether it is the case that students obtain higher earnings in their preferred broad field by essentially comparing the effect of threshold-crossing for applicants with preferred field of *j* and next-best field *k* with the threshold-crossing of applicants with the preferred and next-best field interchanged, i.e. preferred field *k* and next-best field *j*. With this analysis, we want to rule out that the only reason that students obtain higher earnings in preferred fields is that there is a common ranking of broad fields in terms of income, which applies for all prospective students. Third, it is possible that being admitted to one's preferred field leads to a motivation effect that increases performance. However, if there is such a general motivation effect, we would also expect to find earnings gains resulting from being admitted into one's preferred degrees for *Within* applicants.

We begin by exploring the degree to which the skill sets are causally affected by how similar one's preferred and next best degrees are. If we see that skill sets are more distinct across broad fields as compared to fields within a broad field, the potential for a comparative advantage explanation is clearer. For this analysis, we first assign skill usages to individuals based on the average skill usage in the occupation in which they work. We then compare these to benchmark skill usages defined from those who completed an education in the same narrow field as the applicant's preferred narrow field. Specifically, for each skill category, we calculate the distance between the individual's skill use and the benchmark skill use, which we refer to as the skill distance.

We then use this skill distance as a dependent variable for each of the nine skill categories in regressions similar to equation (2). Table IV displays the results of these estimations. Negative coefficients imply that the effect of surpassing the GPA requirement for admission leads to a skill set that is closer to the skill set used by individuals who have graduated in the same narrow field. In seven of the nine skill regressions, the effect of threshold crossing for *Between* applicants on skill distance is significant and negative. The magnitudes of these effects are between -0.02 and -0.06 standard deviations. For instance, as shown in column (2), *Between* applicants who just exceed the admission threshold of their preferred field reduce the distance between their cognitive skill usage and the average cognitive skill usage of those who graduated in their preferred field by 0.06 standard deviation in the cognitive skill measure. *Within* applicants generally see zero effects from just exceeding the admission threshold. Our regression-based findings confirm both our earlier descriptive results presented in Table II with individual jobs that could be matched to job ads and the graphical results in the Online Appendix Figures B2 and B3, which use the skill distances imputed at the occupational level.

Figure VII displays the results of estimating the effects on skill usage separately for each year, analogous to Figure VI. We can see from Figure VII that the effects on skill usage are generally negative, stable and persistent at least to 15 years for *Between* applicants. As expected, the estimated effects for *Within* applicants are consistently near zero. These results suggest that earnings differences between broad fields could be due to differences in subsequent jobs' skill content. Such an interpretation is necessary for our results to imply that prospective students sort into broad field according to comparative advantage. However, our skills results alone do not rule out a pure signaling story. In principle, graduation in a field may merely signal that graduates are productive in performing certain tasks and, hence, open the doors to better paid jobs requiring a particular set of skills. This could be the case without studies in a particular field leading to the demanded skills.

TABLE IV: EFFECTS OF PREFERRED DEGREE ON SKILL DISTANCE, BY APPLICANT TYPE

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Character	Cognitive	Computer, general	Computer, specific	Customer service	Financial	Management	Social	Writing/ language
Within * (Application Score>Cutoff)	-0.00	-0.00	-0.01	0.00	0.01	-0.02	-0.00	-0.00	0.01
	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Between * (Application Score>Cutoff)	-0.02*	-0.06***	-0.03***	-0.02*	-0.00	-0.04***	-0.04*	-0.02**	-0.01
	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)
Observations	57,409	57,409	57,409	57,409	57,409	57,409	57,409	57,409	57,409
Individuals	10,614	10,614	10,614	10,614	10,614	10,614	10,614	10,614	10,614

"Between" refers to applicants whose preferred and next-best fields are in different broad fields. "Within" refers to those applicants whose preferred and second-best fields are within the same broad fields. The specification and included control variables are equivalent to that of column (3) in Table III. The set of control variables includes sex, a quadratic in age, father's earnings, an indicator for whether father's earnings are missing, calendar year indicators, indicators for year of application, and indicators for preferred and next-best narrow fields. All regressions include application score and indicators for *Within* type and *Between* type. Standard errors, clustered at the narrow field level (there are 45), shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1

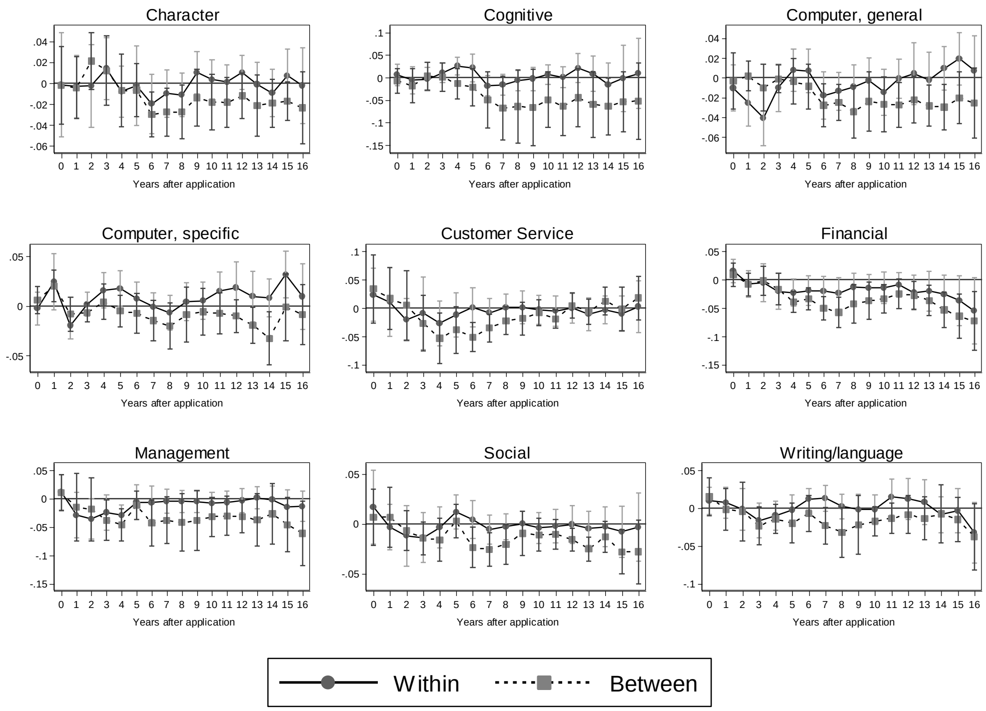

FIGURE VII: ITT SKILL EFFECTS TO PREFERRED DEGREE OVER TIME

"Between" refers to applicants whose preferred and next-best fields are in different broad fields. "Within" refers to those applicants whose preferred and second-best fields are within the same broad fields. This figure plots the results from estimating equation (2) by year.

Next, following Kirkeboen et al. (2016) we examine if the earnings gains across broad fields are simply due to students tending to prefer high-paying broad fields. The starting point is Sattinger's (1978, 1993) definition of comparative advantage: person 1 has comparative advantage over person 2 in field j, while person 2 has comparative advantage over person 1 in field k if the following inequality is true

$$(2) \qquad \frac{y_1^j}{y_2^j} > \frac{y_1^k}{y_2^k} \Leftrightarrow \left(logy_1^j - logy_1^k\right) - \left(logy_2^j - logy_2^k\right) > 0$$

where y_i^j denotes the productivity – or in our case earnings – for individual i in field j. By estimating log earnings equations we can estimate the differences in log earnings in equation (2).[25] Denoting β_{jk} as the return to crossing the GPA requirement of the preferred field j ($d_1 = j$) when the individual's next-best field is k ($d_2 = k$), we estimate the following regression:

(3) $$logy_{it} = \sum_{j,k \in B, j \neq k} \left[\beta_{jk} \mathbb{I}(r_i > 0) * \mathbb{I}(d_1 = j) * \mathbb{I}(d_2 = k) \right] + \sum_{l,m \in W, l \neq m} [\beta_{lm} \mathbb{I}(r_i > 0) * \mathbb{I}(d_1 = l) * \mathbb{I}(d_2 = m)]$$

$$+ \beta_O \mathbb{I}(r_i > 0) * \mathbb{I}(type = O) + f(r_i) + \alpha x_{it} + \varepsilon_{it}$$

where the first summation has all the combinations of different preferred and next-best broad fields (B is the set of *Between* applicants) and the second summation include all the combinations of different preferred and next-best narrow fields within each broad field (W is the set of *Within* applicants). Because we do not decompose the effect of crossing the GPA threshold for applicants with only one preferred narrow field, we capture their effect by β_O. As in the main regressions, we include narrow field dummies for preferred and next-best fields, year dummies and dummies for year of admission, indicators if an applicant is a *Within* or *Between* applicant, as well as a few socioeconomic controls.

Figure VIII shows the distribution of the estimated relative differences $\mathrm{E}\left(logy^j - logy^k | d_1 = j, d_2 = k\right) - \mathrm{E}\left(logy^j - logy^k | d_1 = k, d_2 = j\right)$ weighted by the number of persons with the combinations of either field j preferred over field k or field k preferred over field j. The distribution of pairwise relative differences of *Between* field estimates stochastically dominates the distribution of relative differences of *Within* field estimates. This result is intuitive as we would

[25] We perform this test in the OLS setting, not in the IV setting of Kirkeboen et al. (2016). This implies that our estimates also could reflect comparative advantage in the degree completion time in addition to the comparative advantage in earnings when having completed the preferred field. However, this does not seem to be much of a concern as Appendix Figure B1 shows, that there is essentially no effect of threshold crossing on degree completion time for Between and Within applicants.

expect that comparative advantage would be smaller within a broad field than across broad fields. In fact, 82% of the relative differences of *Between* estimates are positive whereas this is only true for 47% of the relative differences of *Within* estimates.

As in Kirkeboen et al. (2016), we conclude that *Between* field applicants tend to sort into fields in which they have a comparative advantage. However, for the choice of program within a broad field, non-pecuniary benefits seem to play a larger role than pecuniary benefits. The distribution of relative differences within broad fields and the insignificant skills results within a field suggest that broad fields tend to reward similar skills and that comparative advantage tends to be small within a broad field.

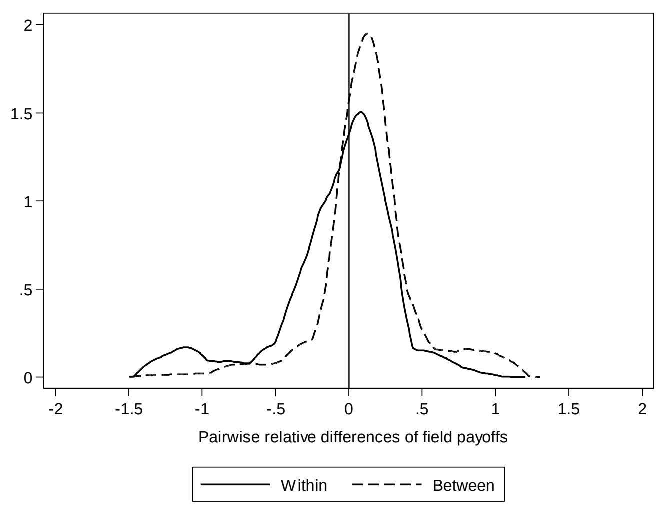

FIGURE VIII: TESTABLE IMPLICATION OF SORTING BASED ON COMPARATIVE ADVANTAGE

Our finding that less than half of the pairwise relative differences of *Within* estimates are positive suggests that there is not just a general long-lasting positive effect of being admitted to one's preferred field. Hence, based on the *Within* applicants, we do not seem to find evidence of a general positive effect due to enhanced passion and motivation (see Stoeber et al., 2011). To the extent that there is a general motivation effect, we would expect that effect to be present for both

Within and *Between* applicants. As the effect for *Within* applicants is insignificant, we conclude that comparative advantage is driving our causal earnings estimates for *Between* applicants. We must note that a motivation effect that is specific to the combination of preferred and second-best choices cannot be ruled out. However, in this case, a motivation effect so defined and a comparative advantage effect are observationally equivalent and arguably the same conceptually.

6.4 Results by broad field

We would like to verify that the results are general and are not just driven by one or two broad fields. To examine this, we estimate the effects of threshold crossing separately for each broad field and applicant type. We will estimate:

$$(4)\ y_{it} = \sum_j \sum_k \left[\beta_{jk} \mathbb{I}(\, r_i > 0) * \mathbb{I}(\, type = j) * \mathbb{I}(\, field = k)\right] + f(r_i) + \alpha x_{it} + \varepsilon_{it}$$

where j indexes applicant type *(Only One, Within* or *Between)* and k indexes broad field type (Humanities, Science, Social Science, Technology, Life Sciences, Medicine, Business, Law). As Kirkeboen et al. (2016) discuss, the distribution of next-best fields is likely to be different between the two fields, implying that estimated effects of broad fields from equation (4) should not be used to understand the earnings effects of one broad field relative to another. Nonetheless, the intention to treat parameters, i.e. the βs, are informative for policy decisions, such as marginally expanding or contracting degree programs because the relevant counterfactual is the actual distribution of second-best fields.

Table V presents the results for one representative specification. Turning first to the effects for *Between* applicants shown in column (1), we see insignificant, and sometimes negative, effects on future earning from exceeding the GPA requirement if one's preferred broad field is Humanities, Science or Life Science. The effect of threshold crossing for those whose preferred broad field is Business is about 18,000 DKK, but insignificant. The effects of threshold crossing in all other broad fields are positive and significant, ranging from about 30,000 DKK (about 4,500 USD) in the case of those whose preferred field is Social Science to more than 50,000 DKK (about 8,000 USD) for those whose preferred field is Law or Technology.

TABLE V: EFFECTS OF PREFERRED DEGREE ON EARNINGS, BY BROAD FIELD

	(1) Between	(2) Only One	(3) Within
Humanities	0.96	0.03	2.98
	(9.01)	(8.89)	(8.17)
Science	9.94	-5.09	-9.37
	(27.46)	(12.60)	(10.92)
Social Science	30.44***	14.49	11.32
	(10.76)	(10.29)	(9.10)
Technology	18.02	44.92***	60.28***
	(11.54)	(7.23)	(20.27)
Life Sciences	58.82***	74.32***	-0.48
	(7.93)	(6.87)	(21.46)
Medicine	48.01***	53.77***	2.44
	(12.80)	(19.07)	(19.75)
Business	-4.97	20.19***	
	(43.05)	(6.86)	
Law	35.34***	48.35***	
	(9.98)	(12.71)	
Observations		300,233	
Individuals		46213	

"Only One" refers to applicants who applied for one study program. "Between" refers to applicants whose preferred and next-best fields are in different broad fields. "Within" refers to those applicants whose preferred and second-best fields are within the same broad fields. The set of control variables includes sex, a quadratic in age, father's earnings at applicant age 16, an indicator for whether father's earnings are missing, calendar year indicators, indicators for year of application, and indicators for Within and Between type. A window of 2.0 is used. All regressions include a quadratic in the application score whose affect may change above and below the threshold. Standard errors, clustered at the six-digit education level (50 of them), shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1

A similar pattern is seen for the *Only One* applicants except that the estimated earnings effects for those applicants whose preferred field was Social Science is halved and insignificant and those whose preferred field was Life Science and Business now realize positive and significant effects. Otherwise, the effects by broad field are generally larger for the *Only One* applicant group relative to the *Between* group, notably so for those applicants whose preferred field is Law: they receive an approximately 75,000 DKK benefit for threshold crossing. Finally, with the exception of

Technology, no *Within* applicant realized significant positive gains on future earnings from threshold crossing on average.

6.5 Robustness Checks

In order to further examine the validity of our estimation strategy, we estimate the equivalent of equation (1)

$$(5) \qquad y_{it} = \sum_{j \in O,W,B} [\beta_j \, \mathbb{I}(\, r_i > p) * \mathbb{I}(\, type = j)] \; + f(\, r_i\,) + \alpha \, x_{it} + \varepsilon_{it}$$

separately for different values of p. In the estimation above, p was always set to 0, the true admission cutoff. If our estimation strategy is sound, we expect to see the largest effects when p is zero and that the effects die away as we look at pseudo cutoffs farther from zero. Figure IX plots the estimated effects from equation (5) for values of p between -0.5 and 0.5 for both the *Between* and *Only One* applicants. Reassuringly, we do indeed see this pattern.[26]

An explanation of the insignificant returns to threshold crossing for *Within* applicants could be that they transfer more frequently between narrow fields *after* admission, and thus, graduate in their preferred field, despite not being admitted at the time of application. Students are able to transfer between fields of study either by getting an administrative transfer through their university, or by reapplying (and potentially get credits for already completed courses transferred after admission). We rule out this explanation by checking transfers between narrow fields for *Within* applicants in comparison to *Between* and *Only One* applicants, see Table D6 in Online Appendix D. We find that *Within* and *Between* applicants transfer between narrow fields at similar rates (11.15 % vs. 9.34 %), and therefore, more frequent transfers for *Within* applicants do not seem to drive the result of insignificant returns to threshold crossing for this group of applicants. As one

[26] As within applicants realize no significant effects in the main results (p=0) we do not include them in this exercise.

would expect, *Only One* applicants transfer to a different narrow field after admission less often (4.85 %). As a final check of our results' sensitivity to transfers between narrow fields *after* admission, we use threshold crossing as an IV for completing a Master's degree in one's preferred field (results available upon request). Intuitively, one would expect that the ITT estimate of admission is a lower bound on the effect of completing a Master's degree in one's preferred field. However, when using the IV approach, we also find insignificant returns to completing a Master's degree in one's preferred field vs. another field for *Within* applicants.

Next, we investigate whether our results hold using an alternative measure for similarity of preferred and next best narrow fields as educational categories are to some extent created for administrative purposes and may not necessarily reflect the degree of similarity between various educational programs. We proceed by grouping applicants by the distance in skills between their preferred and next-best narrow fields rather than grouping applicants according to whether their preferred and next-best fields belong to the same broad field.

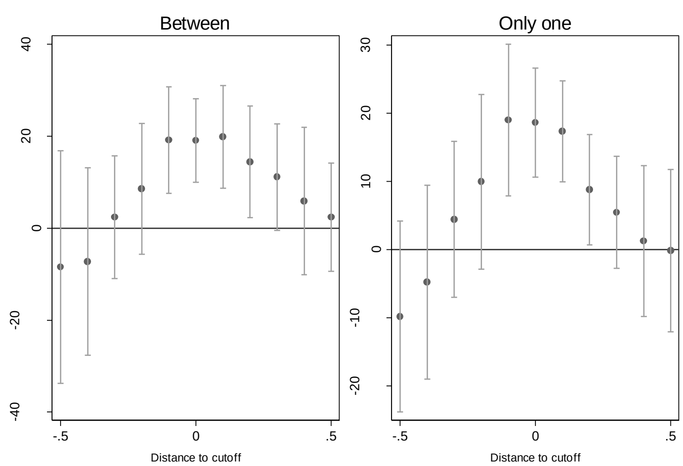

FIGURE IX: ESTIMATED PSEUDO EFFECTS OF PREFERRED DEGREE ON EARNINGS

To categorize applicants according to the skills distance between their preferred and next-best fields, we first compute the average skills of graduates in each narrow field. Next, for each student, we use these narrow field level averages to calculate the skill distance between their preferred and next best field. We do this for each of the nine skills categories. To obtain a one-dimensional skills distance, we simply sum the resulting nine skill distances. This is obviously a crude measure of the aggregate skills distance as we impose that each of the nine skill distances carries the same weight and enters linearly. To facilitate comparison with our *Between* and *Within* categorization, we classify students as having large or small distances by splitting applicants at the median of this aggregate skill distance. In Table VI, we split applicants based on aggregate skill distance and we obtain very similar estimation results compared to those based on our categorizing of applicants as *Between* and *Within* broad field, see Table III.[27]

In Appendix Figure B6, we also examine the time profile of the payoffs to preferred field when we categorize applicants based on whether the skills distance is below and above the median. The figure looks very similar to Figure VI, where we instead grouped individuals as *Between* and *Within* applicants. Appendix Figure B5 demonstrates that comparative advantage in the preferred field plays a larger role when the skills distance between the preferred and next-best fields is above the median, although not as large a role as when applicants are categorized according to being *Between* and *Within* applicants (Figure VIII).

Our results using skills distances to group applicants suggest that the categorization of broad fields adequately captures the required skills needed in subsequent jobs and that earnings differences between fields are related to differences in skill requirements.

[27] For most of the specifications in Table VI, we can reject that the coefficients of being below and above the median of the skills distance are the same. However, if we use more than two categories based on the aggregate skills distance, the coefficients of adjacent skills distance groups are almost always insignificantly different.

TABLE VI: EFFECTS OF PREFERRED DEGREE ON EARNINGS, ALTERNATIVE DEFINITION OF APPLICANT TYPE

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Only One * (Application Score>Cutoff)	16.03***	14.37***	24.06***	23.26***	18.17***	22.98***	16.81***
	(5.49)	(3.65)	(3.16)	(3.17)	(3.08)	(2.60)	(4.63)
Small skill distance * (Application Score>Cutoff)	-15.66**	-9.47	3.01	2.97	-2.78	2.67	-2.03
	(7.46)	(6.87)	(7.55)	(13.29)	(6.17)	(7.35)	(8.14)
Large skill distance * (Application Score>Cutoff)	29.10***	19.94***	24.00***	27.73**	18.08***	17.43***	14.71**
	(8.65)	(6.80)	(5.73)	(12.63)	(5.82)	(5.72)	(7.03)
Observations	224,561	224,561	224,561	224,561	224,561	171,529	106,556
Individuals	39988	39988	39988	39988	39988	30365	18732
Window	2.0	2.0	2.0	2.0	2.0	1.0	0.5
Preferred and second-best narrow field indicators	NO	YES	YES	YES	YES	YES	YES
Control variables	NO	NO	YES	YES	YES	YES	YES
Different slopes	NO	NO	YES	YES	YES	YES	YES
Quadratic terms	NO	NO	YES	YES	NO	NO	NO
Functions of running variables by type	NO	NO	NO	YES	NO	NO	NO
Preferred and second-best institution indicators	NO	NO	NO	YES	NO	NO	NO

"Only One" refers to applicants who applied for one study program. "Small skill distance" refers to applicants whose aggregate skill distance falls below the median. "Large skill distance" refers to applicants whose aggregate skill distance falls above the median. The set of control variables includes sex, a quadratic in age, father's earnings at applicant age 16, an indicator for whether father's earnings are missing, calendar year indicators, indicators for year of application, and indicators for *Within* and *Between* type. All regressions include the function of the application score indicated in the table. Standard errors, clustered at the six-digit education level, shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1

6.6 Present Value and Costs of Studying

Understanding the effect of marginally decreasing the GPA admission requirements on student lifetime earnings vis-à-vis the cost of reshuffling applicants across programs is central to any redesign of the college admission process. Although data limitations prevent us from considering lifetime earnings, we can consider the effects on total earnings up to 16 years after application. Table VII presents the results of estimating equation (1) using a simple sum of annual earnings over different post-application time periods as the dependent variable. For instance, in column (1) we see the effects of just exceeding the GPA requirement for one's preferred degree on the total amount earned from the year of application to 6 years afterward.[28] In this column, we see that *Within* and *Between* applicants who just exceed the admission criteria for their preferred degree realize no significant gains from doing so within 6 years of application. On the other hand, those who applied to one field and marginally surpassed the admission requirements for that field earned significantly less (40,000 DKK) during the first 6 years after application. The intuition here is clear: Those applicants who were not admitted to their preferred field postponed becoming a student and worked in the meantime.

Turning to column (2), we see that effects change when we consider the sum of earnings 7-11 years after application, by which point the majority of students have completed their education. Considering first *Only One,* we see that those who exceed the admission threshold now realize approximately 140,000 DKK more during these 5 years, no doubt at least in part reflecting their earlier entry into the labor market. As shown in column (3), these gains drop in the following 5 years by about 40%. Columns (4) and (5) present the effects on total earnings from the year of

[28] For this exercise, we use balanced samples. For instance, for column (4) labeled "0–10," individuals were only included in the estimation if earnings were not missing for each of the years from 0 through 10 after application.

application to 11 and 16 years after application, respectively. Despite their foregone earnings while in school, *Only One* applicants still realize a significant 180,000 DKK (a bit more than 27,000 USD) bonus to their total earnings within the first 16 years of application. This bonus is non-negligible: It is close to 70% of their average earnings 8 years after application.

Focusing now on the second row of Table VII, we see that *Within* applicants realize no significant effects from surpassing the admission requirements of their preferred field, regardless of the time horizon. This finding contrasts sharply with the effects realized by *Between* applicants shown in the third column of Table VII. Those *Between* applicants who just exceed the admission requirement for their preferred degree are significantly rewarded, receiving more than 110,000 DKK more within 7-11 years after application and 170,000 DKK more within 11-16 years after application. The total gains within the first 16 years after application that equals approximately 300,000 DKK, more than 45,000 USD, close to an entire year's worth of earnings.

If the marginal cost of reallocating applicants into their preferred fields is less than the marginal benefit of doing so, policymakers ought to marginally lower admission criteria. Our best estimate of the marginal benefit of exceeding the admission requirement of one's preferred degree is provided in column (5) of Table VII. Of course, this measure captures only the pecuniary effects. In reality, the benefits received by applicants who meet the admission requirement of their preferred degree may be higher (lower) if they also enjoy more (less) nonpecuniary benefits from pursuing their preferred degree.

TABLE VII: EFFECTS OF PREFERRED DEGREE ON PRESENT VALUE OF EARNINGS, BY APPLICANT TYPE

	(1) 0-6	(2) 7-11	(3) 12-16	(4) 0-11	(5) 0-16
Only One * (Application Score>Cutoff)	-40.63***	141.65***	104.78**	109.84***	182.52***
	(12.22)	(22.78)	(38.74)	(25.68)	(59.47)
Within * (Application Score>Cutoff)	-2.86	10.57	76.97	27.17	101.24
	(22.64)	(35.44)	(74.51)	(49.38)	(125.99)
Between * (Application Score>Cutoff)	-0.88	113.79***	170.04*	113.00***	300.13**
	(11.39)	(27.89)	(94.11)	(30.05)	(122.90)
Individuals	46111	30762	10016	30681	9894

"Only One" refers to applicants who applied for one study program. "Between" refers to applicants whose preferred and next-best fields are in different broad fields. "Within" refers to those applicants whose preferred and second-best fields are within the same broad fields. The set of control variables included in these regressions includes sex, a quadratic in age at application, father's earnings at applicant age 16, an indicator for whether father's earnings are missing, indicators for year of application, and indicators for *Within* and *Between* type. All regressions include the application score and allow for different slopes on either side of zero and a quadratic in the running variable. Preferred and second-best education indicators are also included. Standard errors, clustered at the narrow field level, shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1

As discussed earlier, the benefits received by *Only One* applicants mainly stem from earlier enrollment. The small discontinuity for this applicant type shown in Figure III suggests that individuals who are not accepted in the first year of application reapply for the same program in the following year, implying that the marginal cost of admitting slightly more individuals immediately ought to be close to zero. Similarly, *Within* applicants affected by a marginal loosening of the GPA admission requirement would be reallocated to another narrow field within the same broad area. As narrow fields within the same broad discipline tend to have similar per student costs, the relevant marginal-cost measure of these applicants is also zero.

The possibility of a non-zero marginal cost comes into play when considering a marginal loosening of GPA requirements for *Between* applicants. Several scenarios are possible. First, if there is a symmetrical reallocation of the applicants across broad fields, then the aggregate marginal cost would remain zero. To the extent that this is not true, we can place some bounds on the level of the marginal cost. Generally speaking, the most expensive students to educate are those in

Science and Medicine, whereas the least expensive students to educate are those in Social Science and Humanities. The cost of shifting a student from Humanities or Social Science to Science or Medicine, an upward bound on the cost of switching any applicant, would be around 250,000 DKK, still less than the marginal benefit at 16 years.[29] Regardless of applicant type, marginally lowering the GPA admission requirements is beneficial.

7. Conclusion

We have examined the effect of being admitted to one's preferred field of study on future earnings and skill usage and find evidence that some types of students sort based on comparative advantage. As a consequence of this sorting, potential exists for improving the allocation of students across fields by marginally lowering GPA admission requirements. Such a marginal lowering of admission requirements would enable individuals whose preferred and next-best fields are within different broad fields to earn more by allowing them to exploit their comparative advantage. We find no evidence that lowering the admission cutoff will increase earnings for individuals whose preferred and next-best fields are within the same broad field.

Our result that earnings effects vary by applicant type may help explain differences across existing studies. For example, while Hastings et al. (2013) find that a significant share of prospective students sort into programs with zero or negative returns, Kirkeboen et al. (2016) find that students, on average, have considerably larger future earnings in their preferred field of study and that students sort according to comparative advantage. Furthermore, using job posting data, we find evidence supporting that these earning effects reflect that fields within the same broad discipline require similar skills. Consequently, comparative advantage within a broad field, on

[29] We calculate the costs using the so-called taximeter funding, which increases at a fixed rate in the number of students. Different rates exist, depending on the field of study. In addition, universities are financed by research funding that is not directly related to the number of students in a field. We thank Fane Groes for sharing this data.

average, plays only a small role. Lowering the GPA admission thresholds will also allow more individuals who only apply for one field to start their studies immediately and consequently complete their studies earlier.

Our findings imply that a marginal loosening of admission criteria could prove beneficial, but at least for the Danish case, the GPA requirements in Humanities and Science need not be loosened. In addition to this policy recommendation, the small role played by comparative advantage within a broad discipline suggests that prospective students ought to be encouraged to apply to multiple fields within the same broad field rather than just applying to a single narrow field within a particular discipline. The result that comparative advantage generally plays only a small role in fields within the same broad discipline leads to two possible interpretations. The first interpretation is that specialization within a broad field may not be necessary, at least at the start of an individual's university career. The second, and alternative, explanation is that perhaps specialization *is* important, but the particular type of specialization an individual selects within a broad field holds less importance. Our results do not allow us to discriminate between these two possible interpretations. To the extent that the former interpretation holds, there may be benefits from increased coordination across courses in different programs within a broad field. A relevant avenue for future research would be to investigate economies of scale within broad fields.

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Online Appendices: University admission and preferred field of study

Online Appendix A: Admission Thresholds Plots for Covariates

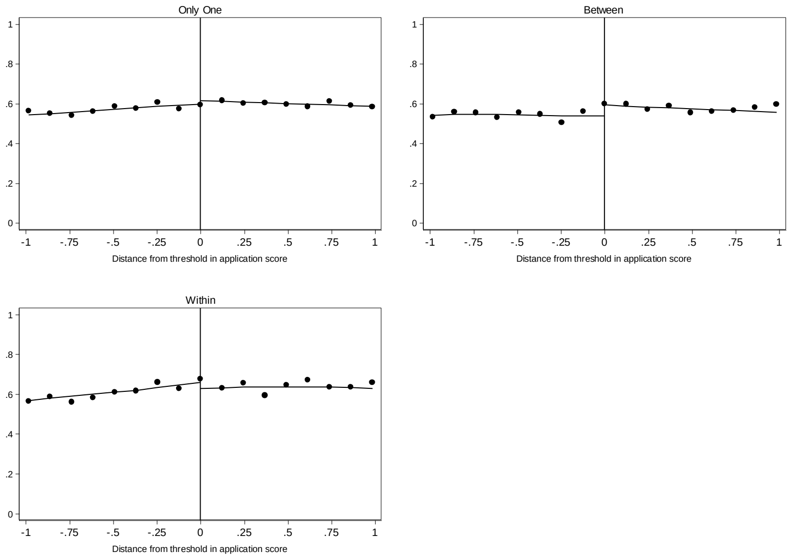


FIGURE A1: ADMISSION THRESHOLDS BY SEX

"Only One" refers to applicants who applied for one study program. "Between" refers to applicants whose preferred and next-best fields are in different broad fields. "Within" refers to those applicants whose preferred and second-best fields are within the same broad fields.

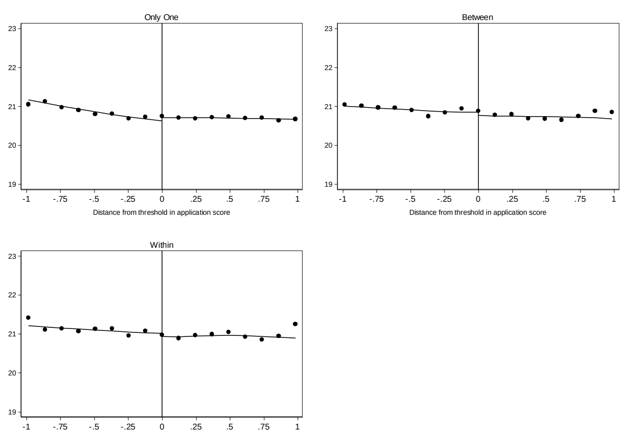


FIGURE A2: ADMISSION THRESHOLDS BY AGE

"Only One" refers to applicants who applied for one study program. "Between" refers to applicants whose preferred and next-best fields are in different broad fields. "Within" refers to those applicants whose preferred and second-best fields are within the same broad fields.

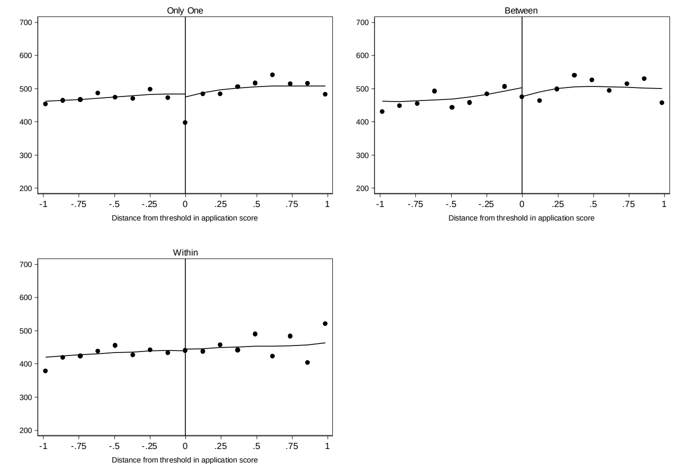


FIGURE A3: ADMISSION THRESHOLDS BY EARNINGS OF FATHER WHEN THE APPLICANT IS AGE 16
"Only One" refers to applicants who applied for one study program. "Between" refers to applicants whose preferred and next-best fields are in different broad fields. "Within" refers to those applicants whose preferred and second-best fields are within the same broad fields.

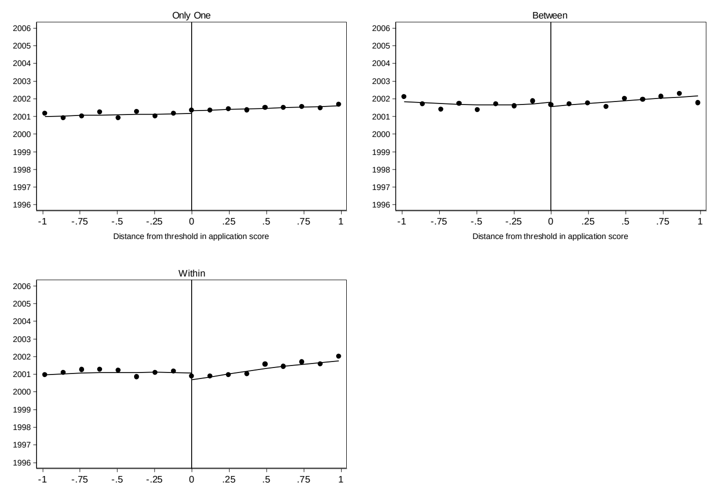


FIGURE A4: ADMISSION THRESHOLDS BY YEAR OF APPLICATION
"Only One" refers to applicants who applied for one study program. "Between" refers to applicants whose preferred and next-best fields are in different broad fields. "Within" refers to those applicants whose preferred and second-best fields are within the same broad fields.

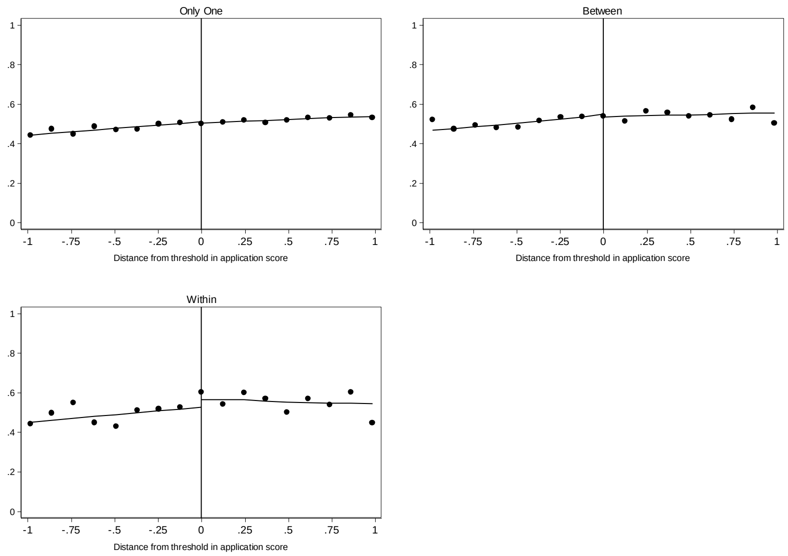


FIGURE A5: ADMISSION THRESHOLDS BY WHETHER MOTHER HAS A HIGHER EDUCATION WHEN THE APPLICANT IS AGE 16
"Only One" refers to applicants who applied for one study program. "Between" refers to applicants whose preferred and next-best fields are in different broad fields. "Within" refers to those applicants whose preferred and second-best fields are within the same broad fields.

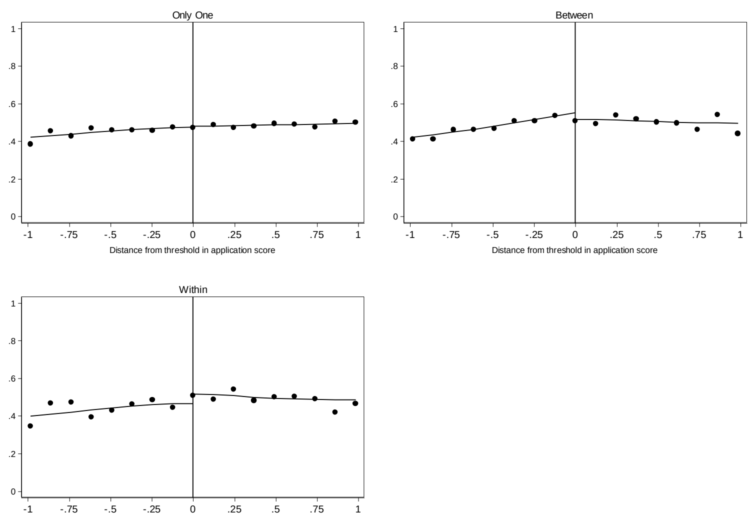


FIGURE A6: ADMISSION THRESHOLDS BY WHETHER FATHER HAS A HIGHER EDUCATION WHEN THE APPLICANT IS AGE 16
"Only One" refers to applicants who applied for one study program. "Between" refers to applicants whose preferred and next-best fields are in different broad fields. "Within" refers to those applicants whose preferred and second-best fields are within the same broad fields.

Online Appendix B: Additional Results

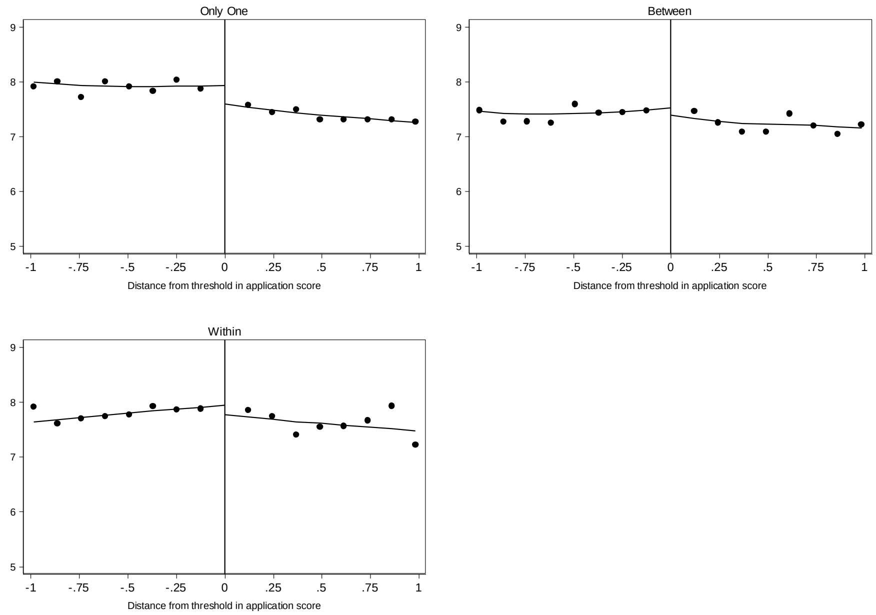


FIGURE B1: ADMISSION THRESHOLDS AND TIME TO GRADUATION

"Only One" refers to applicants who applied for one study program. "Between" refers to applicants whose preferred and next-best fields are in different broad fields. "Within" refers to those applicants whose preferred and second-best fields are within the same broad fields.

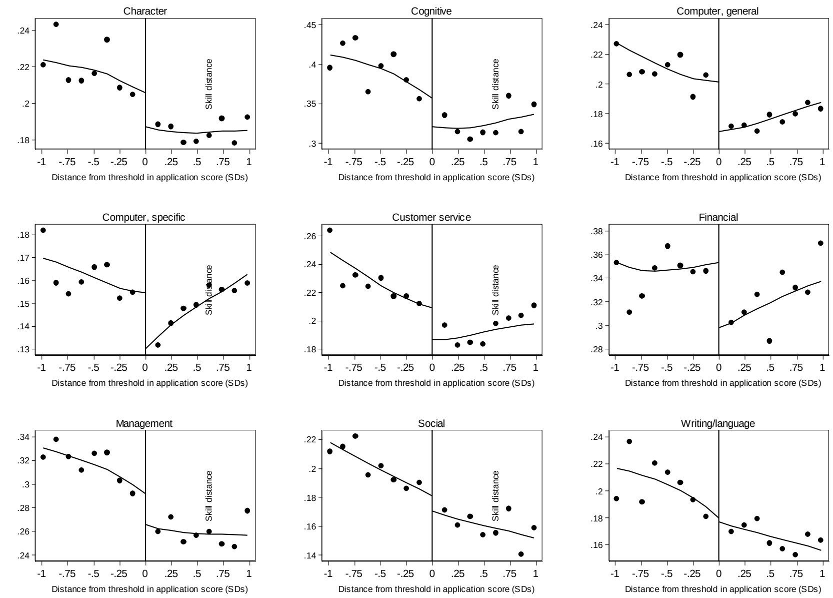


FIGURE B2: ADMISSION THRESHOLDS AND SKILL DISTANCE, THOSE ON THE MARGIN BETWEEN TWO DIFFERENT BROAD FIELDS, ***BETWEEN*** TYPE

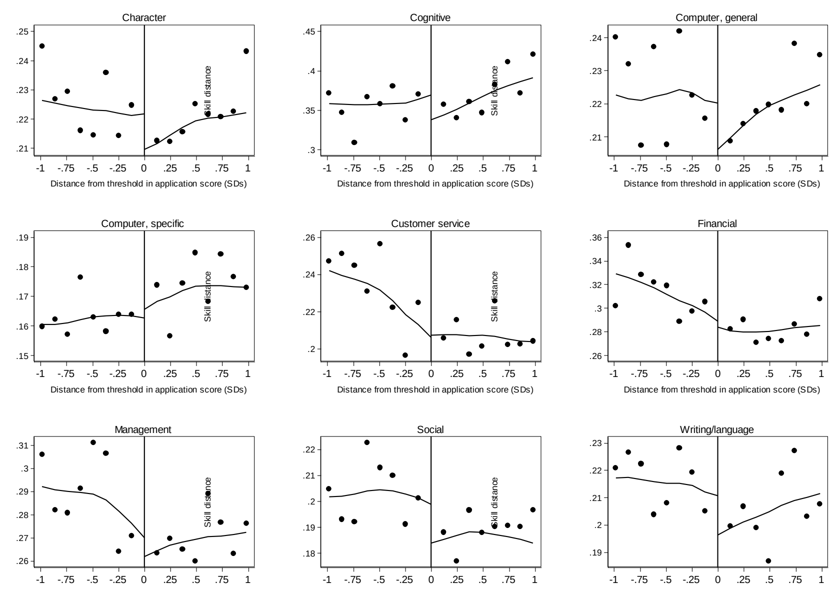


FIGURE B3: ADMISSION THRESHOLDS AND SKILL DISTANCE, THOSE ON THE MARGIN BETWEEN TWO DIFFERENT BROAD FIELDS, ***WITHIN*** TYPE

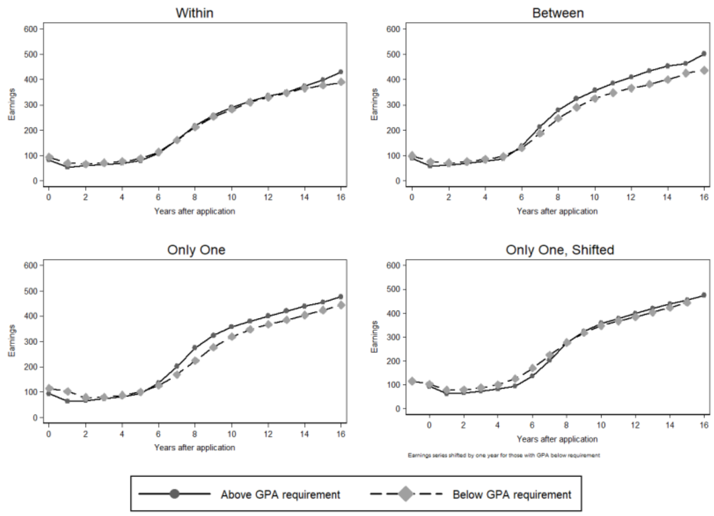


FIGURE B4: EARNINGS SINCE APPLICATION BY APPLICANT TYPE

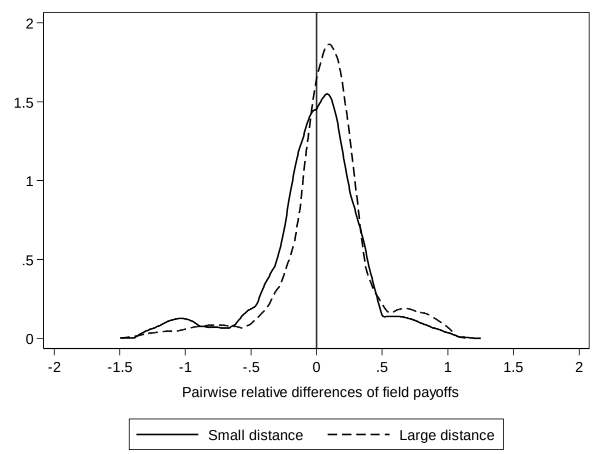


FIGURE B5: TESTABLE IMPLICATION OF SORTING BASED ON COMPARATIVE ADVANTAGE BY DEGREE OF SKILL DISTANCE

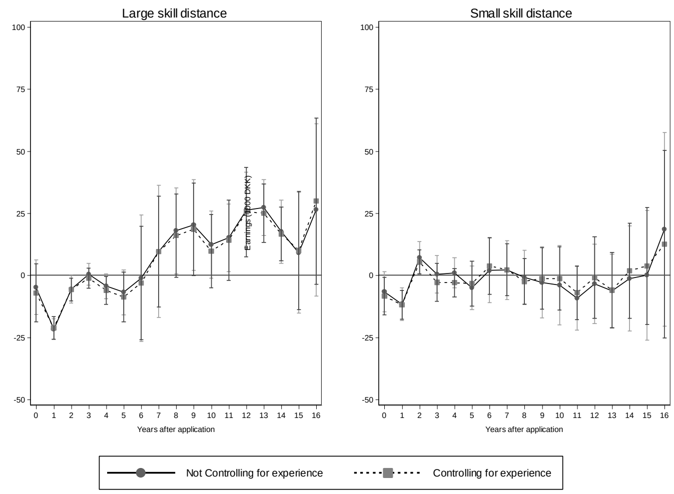


FIGURE B6: ITT PAYOFFS TO PREFERRED DEGREE OVER TIME (DKK YEAR) BY DEGREE OF SKILL DISTANCE

TABLE B1: MULTIPLE SPECIFICATIONS

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Only One * (Application Score>Cutoff)	19.87***	16.34***	18.63***	18.90***	28.98***	29.41***	28.95***
	(5.13)	(4.15)	(4.10)	(3.93)	(4.28)	(3.95)	(3.88)
Within * (Application Score>Cutoff)	-16.58	-6.87	-4.88	-4.46	5.52	-2.68	-1.53
	(10.46)	(6.44)	(6.29)	(5.72)	(5.99)	(13.66)	(13.39)
Between * (Application Score>Cutoff)	26.70***	17.84***	19.08***	19.47***	29.64***	33.99***	34.00***
	(6.85)	(5.66)	(4.63)	(4.78)	(6.60)	(10.63)	(10.53)
Observations	300,233	300,233	300,233	300,233	300,233	300,233	300,233
Individuals	46213	46213	46213	46213	46213	46213	46213
Window	2.0	2.0	2.0	2.0	2.0	2.0	2.0
Preferred and second-best education indicators	NO	YES	YES	YES	YES	YES	YES
Control variables	NO	NO	YES	YES	YES	YES	YES
Different slopes	NO	NO	NO	YES	YES	YES	YES
Quadratic terms	NO	NO	NO	NO	YES	YES	YES
Functions of running variables by type	NO	NO	NO	NO	NO	YES	YES
Preferred and second-best institution indicators	NO	NO	NO	NO	NO	NO	YES

“Only One” refers to applicants who applied for one study program. “Between” refers to applicants whose preferred and next-best fields are in different broad fields. “Within” refers to those applicants whose preferred and second-best fields are within the same broad fields. The set of control variables includes sex, a quadratic in age, father's earnings at applicant age 16, an indicator for whether father's earnings are missing, calendar year indicators, indicators for year of application, and indicators for *Within* and *Between* type. All regressions include the function of the application score indicated in the table. Standard errors, clustered at the six-digit education level, shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1

TABLE B2: MULTIPLE SPECIFICATIONS

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Between * 1(Application Score>Cutoff)*							
Humanities	-47.18***	-9.57	-9.81	-9.54	0.96	6.14	6.57
	(17.14)	(8.85)	(7.54)	(7.14)	(9.01)	(10.78)	(10.64)
Science	-17.92	-2.29	-0.50	-0.26	9.94	12.94	13.12
	(36.91)	(27.61)	(27.13)	(26.97)	(27.46)	(27.43)	(27.17)
Social Science	-3.71	17.06	19.63**	19.93**	30.44***	36.94**	36.64***
	(21.47)	(10.18)	(8.69)	(8.75)	(10.76)	(13.90)	(13.61)
Business	60.84***	-4.54	6.92	7.18	18.02	21.46	21.85
	(15.13)	(10.38)	(9.17)	(8.86)	(11.54)	(13.24)	(13.61)
Law	69.19***	42.61***	48.21***	48.40***	58.82***	64.21***	63.92***
	(14.56)	(5.18)	(5.37)	(5.21)	(7.93)	(11.14)	(10.82)
Technology	8.95	41.16***	36.92***	36.94***	48.01***	50.85***	50.06***
	(23.56)	(12.50)	(11.08)	(11.07)	(12.80)	(14.45)	(14.59)
Life Science	-96.98**	-12.68	-16.13	-15.40	-4.97	2.81	2.95
	(36.73)	(37.18)	(41.79)	(41.50)	(43.05)	(43.31)	(44.73)
Medicine	63.87***	37.26***	24.47***	24.79***	35.34***	41.84***	42.03***
	(16.06)	(7.27)	(8.54)	(8.59)	(9.98)	(13.84)	(13.56)
Within * 1(Application Score>Cutoff)*							
Humanities	-59.46***	-8.66	-7.66	-7.44	2.98	-3.74	-2.80
	(21.31)	(7.68)	(7.63)	(7.37)	(8.17)	(12.64)	(12.40)
Science	-6.99	-24.01*	-20.11*	-19.64*	-9.37	-14.69	-12.96
	(20.77)	(12.66)	(11.44)	(11.12)	(10.92)	(16.21)	(16.16)
Social Science	51.80***	-8.41	0.60	1.01	11.32	1.71	2.98
	(14.86)	(11.19)	(9.70)	(9.10)	(9.10)	(16.00)	(16.02)
Technology	44.32*	66.15***	49.48**	49.56**	60.28***	55.06**	52.77**
	(25.93)	(22.45)	(20.80)	(20.75)	(20.27)	(22.20)	(21.76)
Life Sciences	-3.17	11.48	-9.93	-9.06	-0.48	-12.74	-17.86
	(18.89)	(24.65)	(21.43)	(22.01)	(21.46)	(22.17)	(19.74)
Medicine	92.36***	5.15	-7.78	-7.61	2.44	-5.45	-2.41
	(27.90)	(22.21)	(19.36)	(19.15)	(19.75)	(23.45)	(23.50)
Only One * 1(Application Score>Cutoff)*							
Humanities	-69.78***	-14.83	-10.51	-10.37	0.03	-0.09	-0.83
	(20.76)	(9.62)	(9.04)	(8.81)	(8.89)	(10.07)	(9.99)
Science	-30.95	-13.98	-15.61	-15.35	-5.09	-5.17	-6.07
	(19.90)	(17.40)	(13.35)	(12.93)	(12.60)	(13.59)	(13.21)
Social Science	-8.85	-5.36	3.74	4.02	14.49	14.42	13.62
	(27.38)	(11.37)	(10.92)	(10.77)	(10.29)	(9.68)	(9.48)
Business	94.93***	30.14***	34.16***	34.27***	44.92***	44.75***	46.98***
	(19.62)	(9.19)	(8.31)	(8.11)	(7.23)	(8.36)	(8.40)
Law	82.50***	62.64***	63.97***	64.04***	74.32***	74.13***	73.22***
	(19.02)	(8.48)	(7.75)	(7.60)	(6.87)	(8.02)	(7.81)
Technology	29.96	51.49**	43.13**	43.04**	53.77***	54.55***	55.13***
	(31.77)	(23.11)	(19.08)	(19.15)	(19.07)	(19.70)	(19.70)
Life Sciences	-59.04***	9.32	9.18	9.84	20.19***	19.80**	21.40**

	(21.17)	(9.35)	(7.50)	(6.97)	(6.86)	(7.63)	(8.41)
Medicine	82.41***	41.04***	37.44***	37.72***	48.35***	48.25***	47.59***
	(24.10)	(13.53)	(12.86)	(12.91)	(12.71)	(12.40)	(12.40)
Observations	300,233	300,233	300,233	300,233	300,233	300,233	300,233
Individuals	46213	46213	46213	46213	46213	46213	46213
Window	2.0	2.0	2.0	2.0	2.0	2.0	2.0
Preferred and second-best education indicators	NO	YES	YES	YES	YES	YES	YES
Control variables	NO	NO	YES	YES	YES	YES	YES
Different slopes	NO	NO	NO	YES	YES	YES	YES
Quadratic terms	NO	NO	NO	NO	YES	YES	YES
Functions of running variables by type	NO	NO	NO	NO	NO	YES	YES
Preferred and second-best institution indicators	NO	NO	NO	NO	NO	NO	YES

"Only One" refers to applicants who applied for one study program. "Between" refers to applicants whose preferred and next-best fields are in different broad fields. "Within" refers to those applicants whose preferred and second-best fields are within the same broad fields. The set of control variables includes sex, a quadratic in age, father's earnings at applicant age 16, an indicator for whether father's earnings are missing, calendar year indicators, indicators for year of application, and indicators for *Within* and *Between* type. All regressions include the function of the application score indicated in the table. Standard errors, clustered at the six-digit education level, shown in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

TABLE B3: STANDARD ERRORS CLUSTERED ON THE RUNNING VARIABLE

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Only One * (Application Score>Cutoff)	19.87***	16.34***	18.63***	18.90***	28.98***	29.41***	28.95***
	(5.13)	(4.15)	(4.10)	(3.93)	(4.28)	(3.95)	(3.88)
Within * (Application Score>Cutoff)	-16.58	-6.87	-4.88	-4.46	5.52	-2.68	-1.53
	(10.46)	(6.44)	(6.29)	(5.72)	(5.99)	(13.66)	(13.39)
Between * (Application Score>Cutoff)	26.70***	17.84***	19.08***	19.47***	29.64***	33.99***	34.00***
	(6.85)	(5.66)	(4.63)	(4.78)	(6.60)	(10.63)	(10.53)
Observations	300,233	300,233	300,233	300,233	300,233	300,233	300,233
Individuals	46213	46213	46213	46213	46213	46213	46213
Window	2.0	2.0	2.0	2.0	2.0	2.0	2.0
Preferred and second-best education indicators	NO	YES	YES	YES	YES	YES	YES
Control variables	NO	NO	YES	YES	YES	YES	YES
Different slopes	NO	NO	NO	YES	YES	YES	YES
Quadratic terms	NO	NO	NO	NO	YES	YES	YES
Functions of running variables by type	NO	NO	NO	NO	NO	YES	YES
Preferred and second-best institution indicators	NO	NO	NO	NO	NO	NO	YES

"Only One" refers to applicants who applied for one study program. "Between" refers to applicants whose preferred and next-best fields are in different broad fields. "Within" refers to those applicants whose preferred and second-best fields are within the same broad fields. The set of control variables includes sex, a quadratic in age, father's earnings at applicant age 16, an indicator for whether father's earnings are missing, calendar year indicators, indicators for year of application, and indicators for *Within* and *Between* type. All regressions include the function of the application score indicated in the table. Standard errors, clustered on the running variable, shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table B4: Effects of Preferred Degree on Earnings, including those with an application score of 0 in the control group

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Only One * (Application Score>Cutoff)	17.66***	13.78***	20.00***	19.50***	16.03***	19.48***	15.68***
	(3.68)	(3.49)	(4.26)	(4.25)	(3.57)	(3.64)	(4.58)
Within * (Application Score>Cutoff)	-14.53*	-8.16	-1.58	-0.08	-5.39	0.72	-3.64
	(8.63)	(6.06)	(6.65)	(12.01)	(6.03)	(5.93)	(7.35)
Between * (Application Score>Cutoff)	25.45***	15.50***	21.04***	20.88**	17.06***	17.23**	17.18**
	(5.86)	(5.31)	(5.79)	(9.19)	(4.24)	(6.69)	(6.48)
Observations	322,405	322,405	322,405	322,405	322,405	250,405	163,704
Individuals	49564	49564	49564	49564	49564	38287	24841
Window	2.0	2.0	2.0	2.0	2.0	1.0	0.5
Preferred and second-best education indicators	NO	YES	YES	YES	YES	YES	YES
Control variables	NO	NO	YES	YES	YES	YES	YES
Different slopes	NO	NO	YES	YES	YES	YES	YES
Quadratic terms	NO	NO	YES	YES	NO	NO	NO
Functions of running variables by type	NO	NO	NO	YES	NO	NO	NO
Preferred and second-best institution indicators	NO	NO	NO	YES	NO	NO	NO

The set of control variables includes sex, a quadratic in age, father's earnings, an indicator for whether father's earnings are missing, calendar year indicators and indicators for year of application. Standard errors, clustered at the 6-digit education level, shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table B5: Effects of Preferred Degree on Earnings, including those with an application score of 0 in the treatment group

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Only One * (Application Score>Cutoff)	16.27***	14.02***	24.04***	23.85***	16.92***	21.34***	14.42***
	(5.44)	(3.93)	(3.31)	(2.98)	(3.44)	(2.93)	(3.46)
Within * (Application Score>Cutoff)	-18.64*	-6.66	2.67	-2.12	-4.55	2.17	-5.98
	(10.36)	(5.93)	(4.96)	(9.53)	(4.73)	(5.03)	(6.05)
Between * (Application Score>Cutoff)	20.46***	14.87***	24.24***	28.27***	16.89***	18.18**	14.93*
	(6.68)	(5.20)	(6.51)	(9.91)	(5.02)	(7.56)	(7.50)
Observations	322,405	322,405	322,405	322,405	322,405	250,405	163,704
Individuals	49564	49564	49564	49564	49564	38287	24841
Window	2.0	2.0	2.0	2.0	2.0	1.0	0.5
Preferred and second-best education indicators	NO	YES	YES	YES	YES	YES	YES
Control variables	NO	NO	YES	YES	YES	YES	YES
Different slopes	NO	NO	YES	YES	YES	YES	YES
Quadratic terms	NO	NO	YES	YES	NO	NO	NO
Functions of running variables by type	NO	NO	NO	YES	NO	NO	NO
Preferred and second-best institution indicators	NO	NO	NO	YES	NO	NO	NO

The set of control variables includes sex, a quadratic in age, father's earnings, an indicator for whether father's earnings are missing, calendar year indicators and indicators for year of application. Standard errors, clustered at the 6-digit education level, shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table B6: Effects of Preferred Degree on Earnings, including additional parental education variables

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Only One * (Application Score>Cutoff)	19.87***	16.34***	29.13***	29.20***	18.98***	25.57***	21.39***
	(5.13)	(4.15)	(4.33)	(3.92)	(3.92)	(3.66)	(4.34)
Within * (Application Score>Cutoff)	-16.58	-6.87	5.40	-1.75	-4.66	4.52	-2.34
	(10.46)	(6.44)	(6.02)	(13.21)	(5.69)	(5.68)	(6.93)
Between * (Application Score>Cutoff)	26.70***	17.84***	29.67***	33.81***	19.42***	22.46***	22.41***
	(6.85)	(5.66)	(6.58)	(10.69)	(4.78)	(7.76)	(7.28)
Observations	300,233	300,233	300,233	300,233	300,233	228,233	141,532
Individuals	46213	46213	46213	46213	46213	34936	21490
Window	2.0	2.0	2.0	2.0	2.0	1.0	0.5
Preferred and second-best education indicators	NO	YES	YES	YES	YES	YES	YES
Control variables	NO	NO	YES	YES	YES	YES	YES
Different slopes	NO	NO	YES	YES	YES	YES	YES
Quadratic terms	NO	NO	YES	YES	NO	NO	NO
Functions of running variables by type	NO	NO	NO	YES	NO	NO	NO
Preferred and second-best institution indicators	NO	NO	NO	YES	NO	NO	NO

The set of control variables includes sex, a quadratic in age, father's earnings, indicators for Within and Between type, an indicator for whether father's earnings are missing, an indicator for whether the mother had a higher education when the student was 16 years old, whether the mother's education was missing when the student was 16, an indicator for whether the student's father had a higher education when the student was 16, an indicator for whether the father had a missing education, and calendar year indicators and indicators for year of application. Standard errors, clustered at the 6-digit education level, shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Online Appendix C

Additional Institutional Details

The admission system just described is usually referred to as the Quota 1 application system. Some programs also offer slots through Quota 2. To be eligible to apply through Quota 2, the applicant must have collected a sufficient number of points in a point-based system in which non-academic activities, such as work experience, organization and political work, military service, and living abroad, are rewarded. The specific criteria and the share of slots available in Quota 2 are decided at a decentralized level. If a Quota 2 applicant has a higher GPA than the Quota 1 GPA cutoff, she will be admitted through Quota 1. Thus, the benefit of applying through the Quota 2 system is that the high school GPA requirement is lower than the Quota 1 GPA requirement.

As mentioned, if an applicant is not admitted to any of her listed priorities, it is still possible to enroll in programs with vacant slots. In addition, some programs also offer standby slots. Standby slots require a lower GPA cutoff than the Quota 1 slots and offer admittance into the program in the following year.

More than three-quarters of applicants are admitted through Quota 1, which seems to suggest that this is the most relevant cutoff to consider. Nevertheless, our main argument for using the Quota 1 cutoff and not the standby cutoff is that lowering the latter would imply that more students would have to wait for a year before beginning university studies. From a policy perspective, it is clearly more attractive for both society and the individual to admit students in the current year rather than postponing their studies.

The existence of standby slots, the Quota 2 system, and the applicant's ability to reject an offered slot imply that we do not have a sharp regression discontinuity design, even for enrollment.

However, from a policy point of view, the main question of interest is the effect of changing the GPA cutoffs on earnings.

Skill data

In the following sections, we describe the construction of our datasets with information on skills of applicants. Some of this is also described in the working paper by Jensen (2020). In order to obtain information on skills, we first need consistent occupational codes across for the period in which we consider labor market outcomes, namely 2003-2014. How we obtain consistent occupational codes is described in the first subsection below, and next, we describe our skills data in more detail.

DISCO-codes

DST has adopted 6-digit versions of the International Labour Organization's ISCO88 and ISCO08 occupational codes, namely DISCO88 and DISCO08. The two extra digits are added to provide additional detail. Prior to 2010, all occupation codes in DST's datasets are coded using the 6-digit DISCO88 codes (although for some years, only 4-digit codes are reported). From 2010 and onwards, all occupation codes in DST's datasets are coded using the 6-digit DISCO08 codes. Unfortunately, the (D)ISCO88 and (D)ISCO08 codes do not map consistently one-to-one, one-to-many or many-to-one, and thus, a crosswalk cannot be straightforwardly produced, and crosswalks are not provided by either ILO or DST. Since we are looking at labor market outcomes from 2003-2014, we need consistent occupational codes over this period. Therefore, we produce a revealed crosswalk, using occupational information on people that remain in the same job during the break in occupational codes from December 2009 to January 2010. We aggregate 4-digit DISCO88 and

6-digit DISCO08 codes into 228 mutually exclusive occupational groups. In comparison, there are 150 unique 3-digit and 492 unique 4-digit DISCO88 codes reported in AKM from 2003-2009. Thus, our grouping gives a level of detail somewhere between the 3- and 4-digit level.

Skill datasets

Danish online job vacancy data from 2007-2014 are supplied by the Danish consultancy firm, Højbjerre Brauer Schultz (HBS). These data also include a firm identifier and an occupational code for each job post as well as a posting date. Thus, it is possible to match these data with register data along those dimensions. HBS collects online job vacancy data from numerous Danish online jobs boards, and thus, they believe that their data contains the near universe of publicly accessible Danish online job posts.[30] They remove duplicates and clean the data before machine reading the job posts. HBS extracts the date on which a given job vacancy was posted online, the identification number (CVR-number) of the posting firm, and a 6-digit DISCO-code. If the firm identifier is not listed directly in the job post, HBS imputes it from publicly accessible registers using the firm name and/or address listed in the job post. Importantly, HBS also extract keywords from the raw text in the job post. In many ways, the resulting data is similar to the US job vacancy data supplied by Burning Glass Technologies. In order to be able to match with the register datasets, the vacancy data sample is restricted to include job posts with non-missing firm identifiers and occupational codes. The basis of our skills datasets is 1,928,972 unique job posts with at least one keyword, posted from 2007-2014. When we drop job posts with missing occupation codes, we have 1,232,920 posts, and after dropping those with missing firm identifiers, we are left with 1,020,294

[30] http://www.hbseconomics.dk/wp-content/uploads/2017/09/Eftersp%C3%B8rgslen-efter-sproglige- kompetencer.pdf

job posts. From these 1,020,294 job posts, we derive two datasets with information on skills of university applicants. The first dataset gives us information on individual variation in skills for people who start a new job that was recently posted, and thus, observed in our job vacancy data. Using this data, we can verify how skill usage change across the cut-off for *Between* applicants, but not for *Within* applicants, in t-tests. However, since people do not change jobs very often, we are left with a relatively low number of observations in this dataset, and thus, this dataset does not yield the statistical power we need in regression analyses. Thus, we also construct a dataset with occupation-level information skills and match the occupational information to all people working in a given occupation, and not only people who recently started a new job. More details on these datasets are in available in Table C1.

TABLE C1: SKILLS DATASETS

	Individual-level skills	**Occupation-level skills**
Initial skill data	Obtain data from the first 12 months of new jobs (either new firm identifier or new occupational code) from BFL from 2008-2014, and merge with job posts at the firm*occupation*time-level. We assume that a vacancy is filled in the same month it is posted or maximum 4 months later. This gives a 5 months matching window. Merge onto KOT-sample.	Collapse skills data from 2007-2014 at the occupation-level. Obtain occupation codes at individual-level from AKM and merge these and occupational-level skills onto KOT-sample.
For analyses, *all years* after KOT	Keep only if GPA falls within of 2.0 application score points (i.e. standardized GPA) around cut-off. Keep only first observed job after graduation (and drop if this is 8 years or more after graduation)	Keep only if GPA falls within of 2.0 application score points (i.e. standardized GPA) around cut-off.
For *calculation of mean skill usage of graduates* within narrow fields	Keep only graduates of master's programs Keep only first observed job after graduation (and drop if this is 8 years or more after graduation)	Keep only graduates of master's programs Keep only if job is observed 8 years after application.

Skill categorization

In order to extract skill requirements from the job vacancy data, we initially follow the method of Deming and Kahn (2018). They map a selection of keywords into skills categories. For example, the keyword “teamwork” is indicative of a job requiring social skills. The nine skill categories, which we use, can be seen in Figure II. Unlike Deming and Kahn (2018) who only map a selection of keywords into skill categories, we assign all keywords either a skill category or a noise tag. This is done as follows: 1) The most frequent keywords (approx. 2000) are assigned a skill category or noise tag manually. These words amount to the vast majority of keyword-observations. 2) Using online dictionary APIs each word’s synonyms are obtained. Each word’s synonyms are assigned the same category. 3) Using online dictionary APIs each word’s definitions are obtained. 4) Using the definition of the words, the remaining non- categorized words are assigned a category using machine-learning methods. The machine learning methods are described in more detail here.

The training set consists of both the more than 2000 manually categorized words and their categorized synonyms. In order to categories the remaining words, the dictionary definition of each keyword obtained from two dictionaries, one Danish dictionary and one English dictionary. To use the English dictionary, the keywords are translated beforehand. Although the translation step may seem tedious, it involves some regularization of the keywords, which again helps when looking up definitions of the words. Next, the classification exercise is undertaken.

Two approaches to the classification problem is repeated for both Danish and English versions of the keywords’ definitions. The first approach is a one-step categorization, where each keyword is assigned one of 10 categories, i.e. either one of the nine skills or a noise tag. A Random Forest Predictor is used for this exercise. The second approach is a two-step categorization. In the first step, each keyword is classified as either noise or non-noise. In the second step each non-noise word is assigned to one of the nine skill categories. For both steps a Random Forest Predictor is applied.

Thus, four predicted categorizations are available for each keyword that was not a part of the training set: a one-step and a two-step version for both the Danish and English definitions. If predictions from all four approaches agree on a category, the keyword is assigned to this category. The same step is undertaken if predictions from three out of four approaches agree. Some words' definitions are only available in either the Danish or English dictionary. These words are categorized if the two approaches in the same language agree and if the probability of the predicted class is relatively high. For the few words that have not been categorized after these steps, English predictions with very high probabilities are considered and assigned to keywords. The predictions based on the English definitions are typically more reliable due to longer definitions of the keywords. If keywords are not categorized after this procedure, they are assigned a noise tag.

Skill measures

In the following two subsections, we describe how we derive the skill measures for the datasets with individual- and occupation-level information on skills respectively.

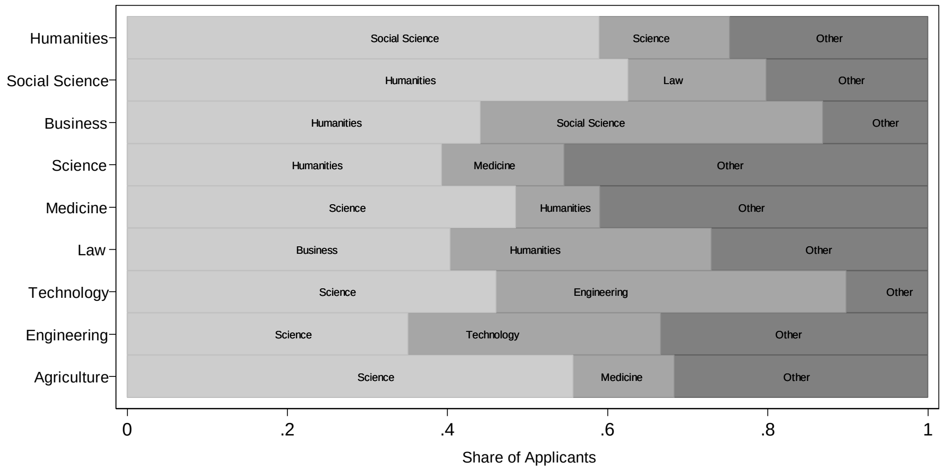
Individual-level skills

In the individual-level data, we match an individual who starts a new job in a given firm*occupation cell with job posts that have occurred within the same firm*occupation cell in same month the individual starts the job or maximum 4 months prior. An individual may therefore be matched with one or more job posts. We calculate the fraction of words indicative of a certain skill across the matched job post(s). We interpret these skill fractions as intensive measures of skill usage of workers. Thus, we are left with intensive measures of skills usage that vary at the individual level. Next, we standardize the skill measures by subtracting the average skill usage across all individuals and divide by the standard deviation. We do this because certain skills are rare, e.g. specific computer skills, and others are very common, e.g. character skills. The standardization makes the measures more comparable. Since we want to know more about the (dis)similarities of the skill usage of applicants that are just above/below the cut-off of their preferred choice of BA program, we calculate the distances between the average Master's graduate of a narrow field and a given applicant in that field as follows: 1) We consider the first observed job of each graduate of a Master's degree in a given narrow field, no more than 7 years after application, and calculate their average skills use based on the skills measure derived above; 2) We calculate the absolute distances between these averages and the skills usage of the applicant who has listed the corresponding narrow field BA program as their preferred choice.

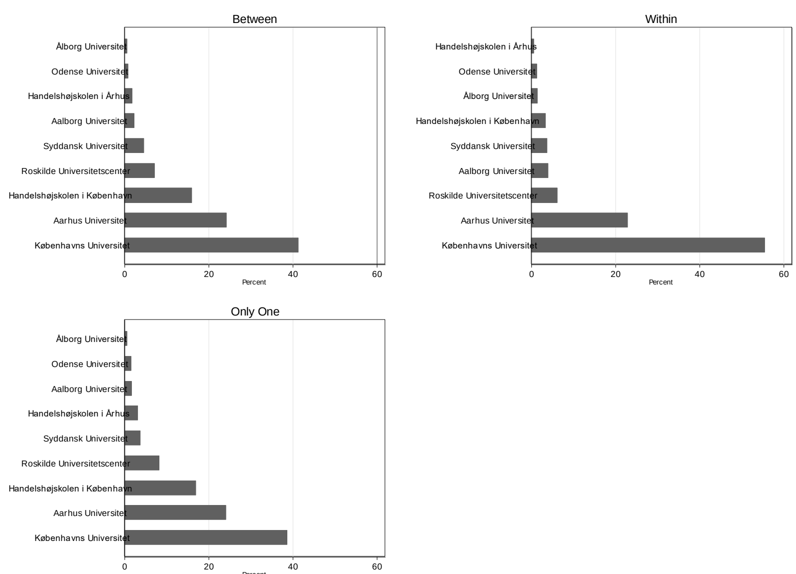
Occupation-level skills

For each job post, we calculate the fraction of words indicative of a certain skill. We interpret these skill fractions as intensive measures of skill usage of workers. Next, we standardize the skill measures by subtracting the average skill usage across all job posts and divide by the standard deviation. After calculating the fraction of words indicative of a certain skill for each job post and standardizing them, we average these measures within our 228 occupational groups. Thus, we are left with intensive measures of skills usage that vary at the occupation level. We merge these skills measures onto our estimation sample after converting DISCO88/DISCO08 codes into our 228 occupational groups. Again, we calculate the distances between the average Master's graduate of a narrow field and a given applicant as follows: 1) We consider all graduates of a Master's degree in a given narrow field 8 years after application, and calculate their average skills use based on the skills measure derived above; 2) We calculate the absolute distances between these averages and the skills usage of the applicant who has listed the corresponding narrow field BA program as their preferred choice.

Online Appendix D: Additional descriptive information, results and data cleaning



F IGURE D1: MOST COMMON NEXT-BEST FIELDS BY PREFERRED FIELDS FOR BETWEEN APPLICANTS



F IGURE D2: MOST PREFERREDINSITITUTIONS BY TYPE

"Only One" refers to applicants who applied for one study program. "Between" refers to applicants whose preferred and next-best fields are in different broad fields. "Within" refers to those applicants whose preferred and second-best fields are within the same broad fields.

TABLE D3: MAIN RESULTS DROPPING VARIOUS FIELDS

	(1) Baseline	(2) No Humanities	(3) No Tech or Medicine	(4) No Law or Business
Only One * (Application Score>Cutoff)	18.90***	23.88***	15.60***	19.52***
	(3.93)	(4.01)	(5.09)	(4.88)
Within * (Application Score>Cutoff)	-4.46	-4.80	-3.38	3.14
	(5.72)	(6.25)	(6.34)	(4.45)
Between * (Application Score>Cutoff)	19.47***	18.21***	18.19***	15.33***
	(4.78)	(5.23)	(6.23)	(5.63)
Observations	300,233	207,912	244,583	229,114
Individuals	46213	32493	37709	34605
Window	2.0	2.0	2.0	2.0
Preferred and second-best education indicators	YES	YES	YES	YES
Control variables	YES	YES	YES	YES
Different slopes	YES	YES	YES	YES
Quadratic terms	NO	NO	NO	NO
Functions of running variables by type	NO	NO	NO	NO
Preferred and second-best institution indicators	NO	NO	NO	NO

Only One refers to applicants who applied for one study program. "Between" refers to applicants whose preferred and next-best fields are in different broad fields. "Within" refers to those applicants whose preferred and second-best fields are within the same broad fields. The set of control variables includes sex, a quadratic in age, father's earnings at applicant age 16, an indicator for whether father's earnings are missing, calendar year indicators, indicators for year of application, and indicators for Within and Between type. All regressions include the function of the application score indicated in the table. Standard errors, clustered at the 6-digit education level, shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Data Cleaning

TABLE D4: BASIC CLEANING

	Drop	Individual-Application Year-Preferences	Individual-Application Years	Individuals
1	Raw, 1993-2006	1,680,225	816,683	555,369
2	Drop observations with KOT priority errors	1,658,363	808,190	550,157
3	Drop those who can't be matched to education registers (educ spells)	1,650,803	804,989	547,462
4	Drop years before 1996 (after flagging first year applied)	1,246,057	625,389	441,803
5	Drop those who do not appear on the population registers	1,245,723	625,244	441,684
6	Keep first time applicants, using a minimum of a year observation history	788,982	402,535	402,535
7	Keep those whose age at application is between the ages of 17 and 25	636,848	313,787	313,787
8	Drop immigrants (ie_type==2)	587,053	294,054	294,054
9	Drop individuals with no HS GPA	512,759	254,299	254,299
10	Drop those had a KOT application year before graduation year	490,521	242,929	242,929

TABLE D5: SAMPLE SELECTION

A	B	C	D	E	F	G
	Drop	**Observation Type in column D**	**Observations**	**Individual**	**Individuals with Multiple Preferences**	**Individuals with Only One Preference**
	After Basic Cleaning	**Individual-Raw Preferences**	**490,521**	**242,929**	**120,458**	**122,471**
1	Dropping those who only have preferences for KOT>=30000 or other fields that we don't consider	Individual-Raw Preferences	267,029	125,310	70,212	55,098
2	Aggregating at the 6-digit level	Individual-Aggregated Preferences	219,138	125,310	56,570	68,740
3	Dropping those with a non-binding cut-off in most preferred broad field	Individual-Aggregated Preferences	163,569	88,442	44,423	44,019
	Drops made on those with multiple preferences					
4a	Dropping those who are most interested in High KOT before preferred and next best	--	--	--	31,949	--
4b	Dropping those whose next best field is for a program we are not interest in (High KOT)	--	--	--	24,151	--
4c	Drop people whose GPA never exceed a threshold or who have local priorities for High KOT	--	--	--	19,334	--
4d	Drops individuals whose preferred and next best have non-descending GPA cutoffs	--	--	--	14,694	--
4e	Again, need to drop those who have a High KOT preference before their preferred and next best. Above we did this for KOTs that appeared before ANY other GPA in the preference ranking. But we need to drop individuals for instance who have low KOT preferences, followed by High KOT preferences, followed by their preferred and next best	--	--	--	14,646	--
4f	For those who have a preferred field of 3 and next best of 4, we need that the GPA cutoffs of 1&2 be higher than the cutoff of 3 and 4, but we don't care if 1 is less than 2	--	--	--	13,822	--
	Drops made to those with just one preference					
5	Dropping those who have a non-binding cut-off or only apply for High KOT	--	--	--	--	39,838
6	Bringing sample together after drops 4 & 5	Individual-Aggregated Preferences	77,227	53,660	13,822	39,838

7	Dropping preferences other than preferred and next best. Bringing together those with one preference (one record per individual) and those with multiple preference (2 records per individual)	Individual-Aggregated Preferences	67,482	53,660	13,822	39,838
8	Horizontal wrt. aggregated preferences	Individuals	53,660	53,660	13,822	39,838
9	Merge to all years in Demographic /Income Files	Individual-Earnings Years	971,969	53,656	13,821	39,835
11	Drop individuals if educBA1 or educBA2 <600000	Individual-Earnings Years	958,669	52,887	13,542	39,345
10	Drop observations that are less than 7 years after KOT application	Individual-Earnings Years	341,598	52,600	13,465	39,135
12	Drop if individual has max grad time less than 3 years (these are likely transfer from abroad)	Individual-Earnings Years	340,845	52,512	13,451	39,061
13	Dropping those whose standardized GPA greater than 2 in absolute value	Individual-Earnings Years	322,405	49,564	13,012	36,552
14	Dropping those whose GPA is equal to the cutoff- **Estimation Sample**	Individual-Earnings Years	300,233	46,213	11,830	34,383

TABLE D6: TRANSFERS BETWEEN NARROW FIELDS AFTER ADMISSION

Change narrow field	Type			
	Only One	Between	Within	Total
No	32,714	6,311	4,326	43,351
	95.15	*90.66*	*88.85*	*93.81*
Yes	1,669	650	543	2,862
	4.85	*9.34*	*11.15*	*6.19*
Total	34,383	6,961	4,869	46,213
	100.00	*100.00*	*100.00*	*100.00*

First row has *frequencies* and second row has *column percentages.*

Conclusion

In the first chapter, I developed a novel, pseudo-individual match between Danish job vacancy data and register data. With data on the hired worker(s) for each online job vacancy, I tested how the employment of skills and the returns to these skills depend on the gender of the worker. I found that women face significantly lower returns to cognitive, character, customer service, financial, and specific computer skills when compared to men after controlling for both occupation and firm fixed effects. I concluded that there are least three potential explanations of these gender differences in skills. Firstly, negotiation of wages may cause differences in returns to skills if women and men with same skills negotiate different wages within the same firm. Blau & Kahn (2017) emphasise that negotiation of wages is a form of bargaining, and if women face discrimination in the rest of the labour market, their outside options are also relatively worse, and thus, their bargaining power is – on average – also lower than that of men. This points to the second explanation of gender differences in returns to skills, namely taste-based and statistical discrimination (Guryan & Charles, 2013). Employers may require the same task-specific skills of their female and male workers, but incorrectly assume that one of them is more productive than the other. Finally, in line with Babcock et al. (2017b,a) and Niederle & Vesterlund (2007), it may be the case that women are more likely to be assigned tasks with low rewards or more likely to avoid competition to get out of these tasks. Although the task-specific skill measures used in this paper are relatively detailed, even within the nine categories of task-specific skills, it may be case that women end up undertaking the less rewarding tasks.

In the second chapter, co-authored with Jack Blundell, we exploited a unique and unexpected reform to the child benefit system in Denmark to assess the effects of child benefits on parental labour supply. A cap on child benefit payments in 2011 led to a non-negligible reduction in child benefits for larger families with young children. We found that a reduction in child benefits led to a large increase in the labour supply of mothers; the effect on fathers was much smaller. Both mothers and fathers responded to the policy at the intensive margin, but the strongest response was from mothers at the extensive margin. The majority of the effects can be ascribed to fertility responses, but even after controlling for fertility-related family characteristics, we found significant increases in labour supply after the introduction of the reform. We confirmed this result by using data on parents' consultations with doctors regarding sterilisation. In terms of policy implications, our results complement existing evidence on the EITC from the US, e.g. Kleven (2019), which generally finds positive employment effects of in-work benefits on single mothers. The universal child benefit system in Denmark appears to have the opposite effect for women in two-parent families. Thus, our results support the conclusion by Eissa & Hoynes (2004, p. 1931) who found that in the US, *"the EITC is effectively subsidizing married mothers to stay home..."* Depending on policymakers' objectives, alternative policies could be developed, e.g. to target the child pay penalty or part-time pay penalty of mothers. However, other outcomes, such as the well-being and poverty of children and mothers, should of course also be considered

In the final chapter, co-authored with Moira Daly and Daniel le Maire, we returned to the theme of task-specific skills and education. We exploited discontinuities from the Danish university enrollment system and found that students who are marginally accepted into their preferred program in a broad field that is different from their next-best choice experience significant and long-lasting rewards as a result. In contrast, students whose preferred and next-best program lie within the same broad field do not. Exploiting data from online job postings, we found that the estimated effects on skill usage similarly vary according to the degree of similarity between preferred and next-best choices. We examined the effect of being admitted to one's preferred field of study on future earnings and skill usage and found evidence that some types of students sort based on comparative advantage. As a consequence of this sorting, potential exists for improving the allocation of students across fields by marginally lowering GPA admission requirements. Such a marginal lowering of admission requirements would enable individuals whose preferred and next-best fields are within different broad fields to earn more by allowing them to exploit their comparative advantage. We found no evidence that lowering the admission cutoff will increase earnings for individuals whose preferred and next-best fields are within the same broad field.

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