IFAU – INSTITUTE FOR
LABOUR MARKET POLICY
EVALUATION

The effects of markets, managers and peers on worker outcomes

Lena Hensvik

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The Institute for Labour Market Policy Evaluation (IFAU) is a research institute under the Swedish Ministry of Employment, situated in Uppsala. IFAU's objective is to promote, support and carry out scientific evaluations. The assignment includes: the effects of labour market policies, studies of the functioning of the labour market, the labour market effects of educational policies and the labour market effects of social insurance policies. IFAU shall also disseminate its results so that they become accessible to different interested parties in Sweden and abroad.

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Abstract

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This thesis consists of four self-contained essays.

Essay 1: This essay exploits the entry of private independent high schools in Sweden to examine how local school competition affects the wages and the mobility of teachers in a market with individual wage bargaining. Using rich matched employer-employee panel data covering all high school teachers over a period of 16 years, I show that the entry of private schools is associated with higher teacher salaries, including higher salaries for teachers in public schools. The wage returns from competition are highest for teachers entering the profession and for teachers trained in math and science. Private school entry has also increased wage dispersion between high- and low-skilled teachers within the same field. Several robustness checks support a causal interpretation of the results, which draw attention to the potential effects of school competition on teacher supply, through the more differentiated wage setting of teachers.

Essay 2: (with Olof Åslund and Oskar Nordström Skans) We investigate how manager origin affects hiring patterns, job separations, and entry wages. The analysis, draws on a longitudinal matched employer-employee data including more than 100,000 workplaces during a nine year period. Immigrant managers are substantially more likely to hire immigrants, a result robust to comparisons within 5-digit industry and location as well as within firms across establishments. The finding holds also when we follow establishments that change management over time, even accounting for trends. Origin dissimilarity increases separations within the first year of employment, but there is no impact on entry wages. Several results point to information asymmetries as an important explanation to the patterns.

Essay 3: The third essay examines whether women benefit from working under female management. I use matched employer-employee panel data for Sweden, which enables me to account for unobserved heterogeneity among both workers and firms. In line with existing work, I document a substantial negative correlation between the proportion of female managers and the establishment's gender wage gap. However, most of this relationship reflects worker heterogeneity, suggesting that sorting is an important explanation for the lower gender wage difference in female-led firms. Further analysis supports this conclusion by showing that while female managers are not more likely to hire same-sex workers per se, they do indeed hire women with higher portable earnings capacity.

Essay 4: (with Peter Nilsson) We analyze how peer effects among co-workers affect fertility using population-wide matched employer-employee panel data. We provide evidence on if, when, why and for whom co-workers' fertility decisions matter. Overall the impact of co-workers on own fertility is of the same magnitude as the effect of being one year older in the age span 20 to 30. "Same-type" co-workers are particularly influential, although social status and own previous childbearing experiences modify the influence of peers in distinct ways. Peers' fertility decisions matter most when the uncertainty about job-related costs of childbearing is low. The results provide insights to the sharp fluctuations in fertility rates observed in many countries, and give an indication of how social interactions affect important career related decisions.

To my family

Acknowledgments

Without the help from all my colleagues and friends, writing this dissertation would not have been possible. First and foremost, I would like to thank my advisors, Oskar Nordström Skans and Olof Åslund. You have guided and supported me from the first day, and steered me in the right direction when going off track. I also appreciate your insightful comments and continuous support, always delivered with optimism. Of course, I am also thankful and impressed by your perfect co-ordination of childbearing, as to ensure an uninterrupted supervision of my work.

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It has been a privilege to be part of the great research environment provided by the Department of Economics at Uppsala University and the Institute for Labor Market Policy Evaluation (IFAU). This is an inspiring place, not only because of all the skilled people in these buildings, but also because of the warm collegial atmosphere. I wish to express my appreciation to all my colleagues at both places, and in particular to Eva Mörk and Anna Sjögren for being supportive along the way. I am also endlessly grateful to the administrative staff at the Department and IFAU. Your excellent organization allowed me to focus on the thesis instead of practical issues. Of all people I would especially like to thank Jörgen Moen at IFAU for excellent just-in-time computer assistance.

Part of my doctoral studies was conducted abroad, when I visited Harvard University during the academic year 2009-2010 and Stanford University during 2010-2011. Taking part of academic life at these universities has been a fantastic opportunity and learning experience. I would therefore like to thank Claudia Goldin at Harvard and Jon Shoven at Stanford for making these stays possible. I further would like to express special gratitude to Per-Anders Edin, who has helped me in various ways throughout my studies and in particular in making the visits at Harvard and Stanford possible. Thank you also Moa Bursell for sharing the experiences in Boston and Magne Krogstad Asphjell for great company in Palo Alto.

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Under the California sun, Palo Alto, February, 2011
Lena Hensvik

Contents

Introduction

One of the key questions in labor economics is why similar workers receive different earnings in the labor market and why similar firms pay different wages. For example, a large literature documents that wages differ for observably identical workers across establishments and industries as well as by race and gender. This thesis consists of four self-contained belonging to the field of labor economics, with a common objective to examine how employers contribute to differences in worker outcomes.

There are two main approaches to explain the observed wage variability in the labor market. The first approach is based on the predictions of the standard textbook model, which relies on the supply-side determinants of wages (i.e. worker characteristics). The model states that employers have little influence over earnings inequality, since employment and wages are determined by the overall demand and supply in the labor market. Wage differentials across similar workers must therefore reflect unobserved supply-side differences, related to e.g. worker skills or preferences for non-pecuniary aspects of work (Dickens and Katz, 1987). In other words, identical workers should receive identical wages even if they work in different firms.

The competitive model has motivated a vast amount of empirical research trying to explain individual wage variation with factors like age, education and labor market experience. The main problem is that large and persistent wage differentials remain even after conditioning on the observed skills of workers. In fact, labor economists are typically happy if they can explain about one third of the total wage variation using the most detailed information on worker education, experience and job tenure (Mortensen, 2003). Even if these models probably fail to account for *all* the skills that are relevant in the labor market, the remaining wage differences seem too large to be readily explained by workers' unobserved characteristics, such as their cognitive and social skills or motivation.

In light of these findings, models emphasizing the role of the demand side of the labor market have received increasing interest. These models state that workers' opportunities in the labor market may depend not only on their supply-side characteristics but also on the employers. Similar individuals employed in different firms may be offered different wages if markets are less than perfectly competitive, since firm level differences in market power generate differences in their ability to compensate workers. The intensity of competition in the labor market could also impact firms' willingness to pay. If employers want to retain workers they may offer high wages in order to guarantee a low quit rate. Conversely, if there is little competition over workers in a labor market, for example due to high specialisation or discrimination, employers may take advantage of the situation and pay lower wages than what the employees would have received in a fully competitive labor market (Manning, 2003).

Models relaxing the assumption about fully competitive markets hence state that wages may depend on firms' productivity, profits, degree of competition, turnover costs and the bargaining strength of workers – and that the wages of workers from different groups of occupations, education and seniority could differ even if these are equally qualified.

Empirical studies play an important role in testing the relevance of the supply and demand side as determinants of wage differentials. This is of crucial policy interest, as better knowledge about the sources of economic inequality can facilitate the choices among and design of different policies. If a large portion of the observed wage inequality is attributable to the supply-side, then policies changing the skills that workers bring to the labor market will be effective if the goal is to change workers labor market outcomes. If outcomes in contrast depend heavily on employer practices and how workers are matched to different firms, other policy interventions may be more effective.

Until recently, economists' ability to distinguish between these mechanisms has been hampered by the lack of appropriate data that contain information on both workers and firms. Naturally, the relevance of the supply and demand side of the labor market in explaining worker outcomes can only be assessed empirically if the characteristics of the workers and firms are simultaneously taken into account. Recent developments of matched employer-employee datasets have, however, opened up new possibilities to analyze the importance of firm characteristics for a range of labor market phenomena. A key feature of such data is that individuals and employing firms are both identified and followed over time, which makes it possible to study differences in employee outcomes across firms as well as employers responses to various policy interventions.

The four essays in this thesis use longitudinal matched employer-employee data from Sweden to examine questions of how labor market competition (*markets*), firms' decision-makers (*managers*) and co-workers (*peers*) affect individual outcomes in the labor market. Below, I put each of the essays into context and provide a brief description of the questions analyzed, the empirical strategies and main findings.

Market power and the wage setting of teachers

Essay 1 is interested in how (the lack of) labor market competition affects the wage structure. As indicated above, this is a question of large interest among labor economists, although it is far from fully understood whether differences in market power held by employers is an important factor behind the observed wage differences among similar workers in different segments of the labor market. Yet, the question has a long history in economics; Joan Robinson stated already in 1933 that *monopsony* –when a limited number of employers provide the only source of jobs for a class of workers– may give employers enough market power to lower wages. Lately, monopsony has received renewed interest in economics as more recent theoretical work has demonstrated that firms may have some wage-setting power even in the presence of many competitors, due to imperfect information or high levels of differentiation (see Ashenfelter et al, 2010, for a survey).

This essay focuses on the labor market for teachers. This market is often put forth as a classic example of monopsony as the limited number of teaching jobs within close distance and the restricted set of outside options reduce teacher mobility, which may generate substantial market power in schools' wage setting. Despite that the teachers' labor market is one of the textbook examples of monopsony there is scarce convincing evidence of whether schools act as monopsonists when setting teachers' wages. Early investigations found a negative relationship between employer concentration and wages in teaching (Luizer and Thornton, 1986). These results were later contested by studies showing that employer concentration had little effect on wages after controlling for important characteristics such as city size and the general wage level (Hirsch and Schumacher, 1995). This clearly illustrates the difficulties of isolating the true impact of competition on wages, as strongly competitive labor markets may differ from less competitive labor markets in many aspects that may also affect the wage.

The labor market for teachers is naturally also of large interest as it is well understood that effective education systems require high quality teachers. However, even if recent evidence clearly shows that teacher skills have a well-measured impact on student achievement, teacher compensation has continued to be low (c.f. Hanushek et. al., 2005). Figure 1 shows the annual earnings ratio between male and female teachers in Sweden relative to workers with the same educational attainment 1968-2000. There is a clear negative trend in teacher wages relative to other professions. While male teachers have received lower wages throughout the period, female teachers have also started to fall behind their female counterparts in other segments of the labor market.

These and similar trends in other western countries have increased researchers and policy markers interest to the teachers' labor market. The main concern is that the wage structure limits the overall supply of potential teachers and pushes the most highly skilled teachers into other professions (Ballou and Podgursky, 1997; Hoxby and Leigh, 2004). Thus, a better knowledge about the determinants and implications of teacher wage setting is an important and urgent issue in order to stimulate the supply of talented teachers and maintain a school system of high quality.

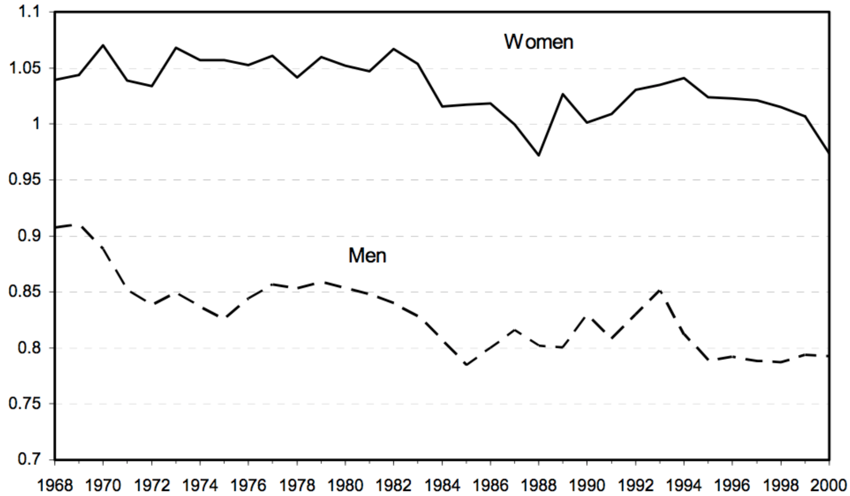

Figure 1 Teacher earnings relative to those with the same educational attainment, 1968-2000.

Source: Björklund et al (2005)

In the essay, I use the rapid increase in the number of private employers following a voucher reform in Sweden to examine whether employer competition is an important aspect of teachers' wages. Until the 1990s, teacher pay was strictly regulated and largely determined in absence of competitive forces. However, in the 1990s the Swedish government implemented a number of radical changes aimed at enhancing the competitiveness of the school system. The financial responsibility for primary and secondary schools shifted from the central to local governments, teacher pay was individualized and there was a move towards increased school choice and privatization. In many respects, Sweden went further than most other western countries with these market-oriented reforms, creating one of the most competitive education systems in the world (see Björklund et al., 2005 for a more detailed overview of these reforms).

One of the most radical changes was the voucher program, which allowed primary and secondary private schools to enter the market with public funding under weak restrictions. The reform gave rise to a rapid increase in the number of private schools. From a situation with less than one percent of the student enrolled in private schools, in 2009 about 10% of compulsory education students and 20% of upper-secondary school students attended independent schools (The Swedish National Agency of Education).

Comparing wages for teachers over time in areas with more and less private school entry, I find that increased school competition is associated with higher wages, in particular for high skilled teachers in math and science and for teachers entering the teaching profession. The finding suggests that competitive forces are important determinants of teacher pay and hence that increased competition in this, and similar labor markets, leads to higher more market based wages. This result is also relevant for the debate on school competition and student achievement. Policy makers in many countries have experimented with various ways of increasing competition in the schooling system through school choice and private school reforms. There is no consensus in the literature, however, on the effects of competition on school performance. Because school performance is likely to depend heavily on the ability of teachers, these results highlight the potential gains from school competition, as a more market-based wage setting could help to overcome problems of teacher shortages and enhance the quality of the overall teaching pool.

Hiring and wages, do managers matter?

The recognition that employers might have some discretion over individual earnings suggests that they may also be an important factor behind the large and persistent labor market disparities by race, ethnicity and gender. Essay 2 and Essay 3 investigate this in more detail by examining the role of *managers* for individual employment and wages.

Models stating that firms are important aspects of worker outcomes in the labor market inevitably give a paramount role to managers. As key decision-makers on the demand side, managers have a large influence over firms' recruitment, wage and promotion practices. A clear example of this is Becker's (1957) discrimination theory, which states that employers with a "taste" for discrimination against a particular group will hire less of that group and pay them lower wages.

More recent empirical evidence suggests that firms use different recruitment strategies and that managers differ in "styles" in ways related to firm and individual outcomes (c.f. Shaefer and Oyer, 2010, Bertrand and Schoar, 2003).

In these essays we study a particular feature of the manager-employee relationship, namely whether the origin (**Essay 2**) and gender (**Essay 3**) of the manager are important determinants of worker outcomes. A common feature of the essays is that both use population wide data on employers and employees, which is an important contribution to the literature focusing on the influence of managers, which has mainly been confined to empirical investigations of limited size and generalizability. The use of representative population data allows us to examine these questions in dept and increase the possibilities to extrapolate the results to other settings.

The Swedish labor market displays systematic and persistent inequality both by origin and gender but its nature and sources are also potentially different, which motivates separate analyses. It is a well-documented fact that immigrants are less likely to succeed in the labor market than natives in most OECD countries. About 14 percent of the Swedish working-age population is foreign born, composed of labor market migrants, refugees and family reunification migrants. Recent immigrants from non-OECD countries face the largest difficulties in the labor market; they are less likely to be employed and earn significantly lower wages compared to natives and all other immigrant groups. However, even if immigrants receive lower wages than natives in the Swedish labor market, the main socio-economic divider is the immigrant-native employment gap.

In **Essay 2** (joint with Olof Åslund and Oskar Nordström Skans) we therefore examine whether the origin of the hiring manager is a factor determining the origin of recruited workers. The immigrant-native earnings-gap has been extensively studied in the past with focus on what portion of this gap that can be explained by supply side factors such as the role of formal education and host country language skills (Bleakly and Chin, 2004). In Sweden, evidence suggests that the immigrant-native earnings gap cannot be explained by lower education levels among immigrants (le Grand and Szulkin, 2000). Moreover, even if employment chances do improve with residence in Sweden, certain immigrant groups continue to show a significant employment gap even after over 20 years (Nekby, 2002).

The size and the persistence of the immigrant native earnings gap have motivated social scientists to look for other explanations. For example, recent studies using firm-level data document strong segregation patterns at the workplace level in several countries in terms of race and immigrant origin (Hellerstein and Neumark, 2008, Åslund and Nordström Skans, 2010). Workplace segregation is moreover associated with lower wages, which calls for an increased understanding of the factors determining firms hiring and wage practices.

A less recognized fact is that immigrant workers rarely belong to the group of managers. This could be a disadvantage, if managers favour workers of their own background when taking hiring, wage and promotion decisions. Using matched employer-employee data covering managers and hires in Swedish establishments over nine years, we demonstrate that immigrants are severely underrepresented in managerial positions; 7.2 percent of hires are non-western immigrants compared to 3.7 percent of managers. We then continue to investigate the importance of sharing background with the manager on hirings, quits and entry wages.

We find a very strong correlation between the immigrant composition of managers and hires in different industries. However, since factors like workplace location and the goods and services provided by the firm may affect both the managerial staff and the composition of employees, it is not straightforward to conclude that managers are more likely to hire workers who share their own background.

To provide more convincing evidence, we compare hiring patterns in very similar establishments in the same localities with managers of different origin. We also examine whether immigrants hiring prospects change when establishments switch from a native (immigrant) to an immigrant (native) manager. Our results suggest that managers are more likely to hire workers sharing their origin; even after adjusting for location, industry and establishment characteristics, immigrants are almost twice as likely to be hired by an immigrant compared to a native manager. These findings draw attention to the underrepresentation of managers with immigrant origin as one explanation behind the slow convergence of immigrants and natives in the labor market.

There are several possible reasons for *why* managers favour similar workers, and the interpretation of the results naturally depends on the underlying explanations. More similar manager-worker relationships may be more productive, for example because a common language increases productivity. In-group bias could also be a result of prejudice, higher uncertainty about the productivity of dissimilar workers or prior beliefs that less similar workers have lower productivity on average.[1]

Our findings suggest that access to job-related networks is an important mechanism for why managers hire workers of their origin. This result is consistent with a growing line of research highlighting that networks are important determinants of individual labor market success (see e.g. Montgomery, 1991, Ioannides and Datcher Loury, 2004).[2] Promoting the careers of already employed immigrants as well as increasing the number of social ties between immigrants and natives could thus be effective tools in reducing the immigrant-native employment gap.

The public debate on managerial composition has otherwise mainly evolved around the scarcity of women in top ranks and management positions. In 1986, two U.S. journalists coined the term "glass ceiling" to describe the barriers for women to assume top ranks and management jobs (Meyersson and Trond Petersen, 2006). Since then, this term has been used extensively and numerous studies in many countries document strong occu-

[1]

[2] Three out of four newly employed workers in Sweden state that informal contacts were the primary source of information about the job (Arbetsmarknadsrapport, 2010)

pational segregation by men and women as well as a dearth of women in management. Even in Sweden, which is typically perceived as a country with high gender equality there is clear evidence of a "glass-ceiling" keeping women from moving up the career ladder (Albrecht et al, 2003).

Increasing the share of women into influential positions is a possible way of breaking the glass ceiling. Female managers might serve as positive role models and help other women through hiring and promotions. Yet, despite a vast interest in the possible benefits increasing female management representation in the public debate, there is little empirical evidence documenting the existence and size of such effects.

Essay 3 investigates whether women benefit from working under female management. I follow male and female careers for 13 years and assess how their outcomes vary with the gender composition of their managers. The high labor force participation of women in Sweden and the persistent gender wage gap mentioned above motivate the focus on female managers' potential to narrow the wage differences between male and female employees rather than getting women into employment.

The analysis suggests that women receive relatively higher wages in women-led compared to male-led firms. However, this result mainly reflects the fact that more productive women sort into female led firms, rather than women receiving favourable treatment by female managers. Female managers are found to hire women of higher portable earnings capacity compared to male managers, which fully explains the lower gender wage gap in female-led firms. Women are also found to recruit other women to higher paying positions within the firm.

The finding that female managers seem to recruit women with higher earnings potential either suggests that more productive women self-select into female-led firms or that female managers have better information about other women's productivity. In any case, these results lend support to theories stating that information is important for the matching between workers and firms. Because it is difficult – both for employers and workers – to observe all relevant factors about the employment relationship, employers may use informal networks or statistical discrimination to overcome uncertainty. This conjecture is also supported by the findings in Essay 2, as immigrants have higher employment probabilities if they belong to the managers' networks. Further knowledge about how different firms use and invest in such strategies and how this in turn affects the outcomes in the labor market for different workers is important both from a research and public policy perspective.

Co-worker interactions and women's fertility decisions

One of the most frequently stated reasons for the observed glass-ceiling for women is the prevalence of workplace interruptions due to childbearing. This is particularly true in Sweden as the generosity of the social security system allows parents to stay at home relatively long with their new born children.

In **Essay 4**, (joint with Peter Nilsson) we examine whether the timing of childbearing depends on women's co-workers' fertility decisions. The construction of the Swedish parental leave system has been highlighted to influence the timing of childbearing since the benefits are earnings related. This could partly explain the pro-cyclical variation in fertility rates displayed in Figure 2, since women's fertility decision will be heavily influenced by the time the couple has a permanent income.

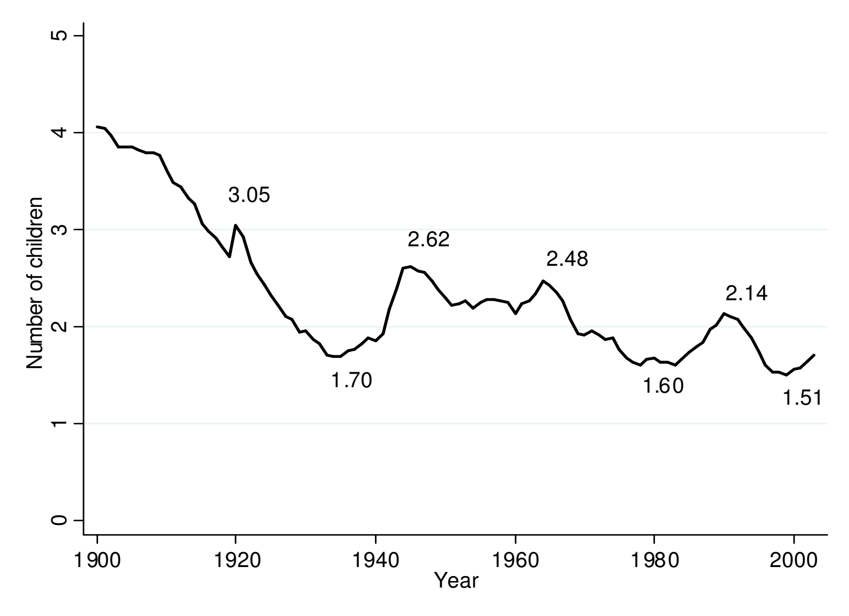

Figure 2 Total fertility rate, 1900-2003

Source: Socialstyrelsen (2005)

However, it has also been argued that much of the swings displayed above seem to occur so dramatically and rapidly that they are unlikely to be explained by changes in economic conditions alone. Social scientists have therefore been trying to get a better understanding of the determinants and consequences of the sharp fluctuations in fertility rates observed in Sweden during the past century. This is important since large swings in fertility rates may complicate social planning such as the need for day-care, schooling and housing etc. as well as affect individuals labor market prospects through the size of the labor supply. Thus, the swings are potentially costly, both from a public policy and from an individual perspective. The matter seems to be a question of timing of childbearing rather than cohort-fluctuations in family size, as women's completed fertility is remarkably constant around two children per women for most of the 20th century.

We examine whether the sharp fluctuations in fertility rates are reinforced by social influences. This view has flourished among sociologists for long, and economists have more recently started to incorporate social considerations in models of individual behavior (Akerlof and Kranton, 2000). There is still however, very little empirical evidence on whether and how peers influence women's fertility decisions. The existing studies have mainly used small and non-representative samples, either focusing on social interactions within developing countries, among teens or within families.

We consider social interactions among co-workers, a group that may be of particular interest when it concerns fertility-timing decision in developed countries. We believe that co–workers childbearing experiences could be important first because these may convey information about job related consequences of childbearing that is difficult to obtain from other social networks or sources. Second, the similarity between co–workers and the day-to-day interactions also suggest that social influences could be important within this peer group. A few recent studies suggest that co-workers have influential over a range of individual behaviors and outcomes, such as work effort, sickness absence (Mas and Moretti, 2009; Hesselius et. al., 2009) retirement decisions and job satisfaction (Duflo and Saez, 2003; Card et. al., 2010).

Peer effects in the timing of fertility could arise, for example, if co-workers value joint parental leave or because of social concerns motivated by e.g. peer pressure or desires to "fit in" within the workplace. These mechanisms could all generate an increased likelihood of own childbearing after the birth of a co-workers child.

We test the relevance of fertility peer effects among co-workers using representative data on 150,000 women in childbearing ages in Sweden within workplaces with less than 50 employees. Overall, we find that the birth of a co-workers child increases the probability of women's own childbearing with ten percent.[3] A nice feature with the rich data is that we can examine how the strength of the peer effects varies with the characteristics of the co-workers, and how these match the characteristics of the focal worker. Figure 3 (from the paper) summarizes the main results from this analysis. As we can see, the social influence is strongest when co-workers

[3] The effect is comparable to estimates of job displacement on fertility or to the impact of increasing a woman's age with one year on in the age interval 20 through 30 on the probability of childbearing.

are of the same gender, age and have the same number of previous children. The peer effect in addition seems to be motivated by social status concerns; only births to co-workers of higher education have a significant influence.

Figure 3 Monthly increase in the probability that an employee give birth 13-24 months after co-workers of different types had a child
Source: Hensvik and Nilsson (2011)

The focus on the timing of childbearing allows us to rule out alternative explanations for the observed effect, for example that similar individuals may be more likely to work together and that workers in the same workplace may be subject to similar chocks affecting childbearing, such as changes in firm policy or management. We also find results suggesting that social concerns are likely to be the driving mechanism behind our results rather than individuals using their peers' experiences to reduce uncertainty about own career-related consequences.

From a policy perspective, these results highlight that policies designed to affect the fertility rate can be reinforced by social interactions. However, the importance of same type peers and job security suggests that the strength of these peer effects may depend on the context of the targeted group. In addition, the existence of peer effects in such important decision as the timing of childbearing also indicates that social influences may be relevant also in other types of career related decisions and the organization of work and family.

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Essay 1: Competition, Wages and Teacher Sorting: Four Lessons Learned from a Voucher Reform♣

1 Introduction

The teacher labor market has received growing attention among social scientists and policy markers as recent evidence suggests that teacher quality is one of the key inputs in improving school performance (see e.g. Hanushek et al., 2005, Rockoff, 2004). Despite this, teacher pay remains low and compressed in many countries compared to other occupations with similar qualification requirements. In addition, the factors that determine teacher salaries often bear little relation to student achievement.

The main concern with this pay structure is that it may limit the supply of potential teachers and push the most highly skilled teachers into other segments of the labor market (Ballou and Podgursky, 1997; Hoxby and Leigh, 2004). In order to create a school system of high quality it is therefore important to understand how the particular features of the teacher labor market affect teachers' wages.

This paper provides evidence on whether introducing private school competition in the school system has an impact on teachers' wages and how this effect may operate. I investigate the consequences of a Swedish policy reform that allowed publicly funded private schools to operate in the market for education. The reform initiated a rapid expansion in the number of private schools (Figure 1) and large temporal and regional variation in private school entry. Because almost all Swedish schools were run by local governments prior to the reform the new sector of private employers clearly increased employer competition in the teacher labor market.

Reforms that increase school competition can have a significant impact on teacher's wages through both reductions in the monopsonistic power of in-

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cumbent schools and increased competition over students (Boal and Ransom, 1997, Manning, 2003, Hoxby, 2002). Monopsony has long been an issue of concern in the teacher labor market due to the limited geographic and occupational mobility options for teachers, which may generate significant market power for schools when setting teachers' wages. Recent estimates of teacher mobility provide indirect evidence of monopsony power in the teacher labor market (Falch, 2010, Ransom and Sims, 2010), although few empirical studies have convincingly demonstrated its actual impact on teacher wages.[1]

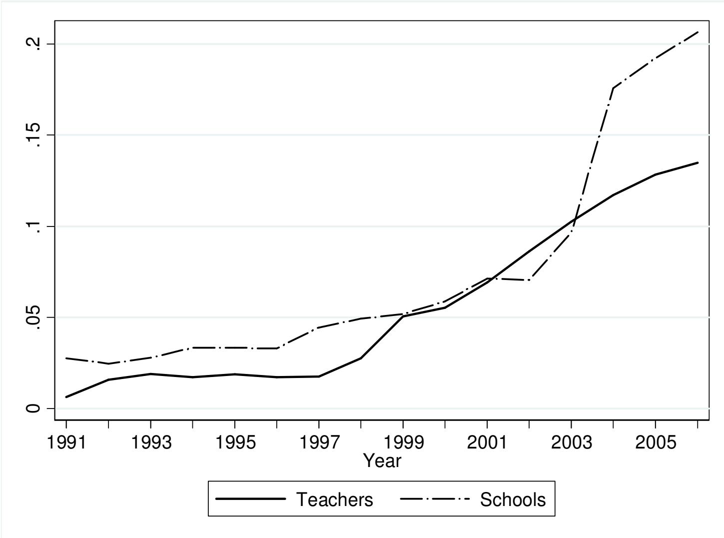

Figure 1 Trends in private share 1991-2006.

The paper adds to the literature in several ways. First, it is the first study to investigate competition effects in a teacher market characterized by a decentralized wage setting that allows schools to respond to increased competition in the local wage negotiations.[2] Second, the variation in private high school openings over time and across regions, together with the rich data at hand enables me to address many of the limitations in existing work, which has

[1] Several studies document large and systematic wage differences between observably identical workers, both across industries (Krueger and Summers, 1988, Katz and Summers, 1989 and Murphy and Topel, 1990) and across local labor markets (Moretti, 2011). One theoretical explanation behind such differences is that they reflect variations in the competitiveness of markets that arise from, for example, search frictions or entry barriers (cf. Manning, 2003). However, the economic relevance of imperfect competition is far from clear in the empirical literature. A few studies examines the impact of competition and entry regulations in the private sector.

[2] Few studies have investigated the consequences of increased competition in any labor market on workers wages. For evidence from the private sector, see Bertrand and Kramarz (2002) and Black and Strahan (2001).

mainly relied on cross-sectional data (Medcalfe and Thornton, 2006, Vedder and Hall, 2000). An important feature of the reform is that the local governments who run the public schools have little influence over the inflow of private schools, since the approval is decided at the national level.

Finally, the paper also contributes to the literature on the possible effects of school competition on school quality. Many countries have recently adopted or considered reforms aimed at increasing competition between schools, and these reforms have spurred considerable debate on whether market reforms are effective in raising student achievement. However, there is no consensus in the literature on the effects of competition on school performance. Focusing on the outcomes of teachers can provide important insights into this debate, as the potential effects of competition on the wage structure may also affect the selection of teachers and in turn student achievement.[3]

I begin by documenting public and private schools' hiring patterns. Because private schools have a strong incentive to attract students, it is interesting to assess whether they hire different kinds of teachers than public schools. The data used for this analysis contain longitudinal information on the universe of teachers and annual information on monthly full-time wages between 1991 and 2006. In addition to wages, the data also include standard background characteristics as well as information on certification status and field of specialization. For a large sample of male teachers there are also measures of social and cognitive skills (from the enlistment). For most of the study period, the data also contain unique identifiers for schools in which individual teachers are employed.

My results suggest that private schools differ significantly from public schools in their recruitment behavior; they hire from a broader array of occupations and recruit more from the private sector than public schools. In addition, by comparing public school teachers who remain in their school to those who move to other schools, I find that private schools are more likely than public schools to attract teachers with subject area skills and high cognitive ability (rather than formal qualifications).

I then turn to the effect of private school competition on wages. Using changes in private high school entry within and across local labor markets I find that private school expansion has a significant impact on teacher wages, particularly for teachers with high mobility and teachers in subjects characterized by teacher shortages; new teachers in the most competitive areas as well as math and science teachers receive 2 to 3 percent higher wages than

[3]A number of papers have estimated the relationship between private school penetration on test scores, grades and university attendance, finding only weak and inconsistent evidence of such student achievement gains both in Sweden and elsewhere (cf. Ahlin, 2003, Sandström and Bergström, 2005 and Böhlmark and Lindahl, 2009 for evidence on Sweden; Clark, 2009 and Gibbons et al., 2008 for the UK, and Hoxby, 2003 and Figlio and Hart, 2010 for the US).

comparable teachers in areas without competition from private schools.[4] The effects persist once individual heterogeneity is controlled and when restricting the sample to public school teachers, suggesting that public schools respond to private school entry by raising the wages for the teachers most valuable to them.

The empirical strategy accounts for many of the potential confounders that could generate a spurious relationship between school competition and wages, such as time-invariant differences between local labor markets and local linear trends in unobserved determinants of wages. Still, it is possible that the effects capture changes in time-varying unobserved characteristics of local labor markets rather than competition. I provide a range of robustness tests addressing this concern; all contradict that the results are simply driven by spurious correlations.

In the final part of the paper, I examine whether the effects of competition vary with teachers' cognitive and social skills. As previously mentioned, one of the leading hypotheses for the declining trends in teacher aptitude observed in many countries is that the pay compression pushes the most high-skilled teachers out of the profession (cf. Hoxby and Leigh, 2004, Lazear, 2003). It is thus interesting to investigate whether private school competition has created winners and losers among incumbent teachers in terms of their teaching skills.

Notably, the results suggest that the magnitude of the competition effect varies substantially depending on the skill level of the teachers. The entire effect is concentrated among teachers in math and science with *high cognitive* skills and among social science teachers with *high social skills*. In contrast, there is no effect on teachers below the median in the cohort-specific skill distribution. Consistent with these results, I also document a clear association between the local labor market-specific wage returns to cognitive ability and the competition from private schools.[5]

The rest of the paper outlines as follows: Section 2 provides a theoretical motivation and related literature, Section 3 describes the data, and Section 4 describes the reform generating the variation exploited in the paper and the setting of teacher wages in Sweden. Section 5 analyzes hiring patterns and investigates the impact of school competition on teachers' wages. Section 6 provides results on the differential effects with respect to teacher skills, and Section 7 concludes.

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[4] Given the substantial wage compression in the teaching profession, these effects can be considered to be non-trivial. For comparison, in the most recent wage negotiations, the employers and the teachers' labor unions agreed upon a wage increase of 3.5 percent averaged across all teachers in the municipality over two years.

[5] Using engineers as a comparison group, I show that these findings do not seem to be driven by cross-market differences in general trends in the returns to cognitive traits.

2 Background and related literature

In labor markets with few employers and low worker mobility, employers may act as monopsonists and set wages below the competitive level (see Ashenfelter et al., 2010 or Boal and Ransom, 1997 for reviews on the literature on monopsony). Monopsony has long been a concern in the teachers market, since the number of available schools is often limited in a given geographic area. Teachers, moreover, have high occupation-specific skills, which restrict the set of non-teaching jobs available to them.[6] Increasing the number of employers available in the local labor market could therefore lead to an upward pressure on wages through a reduction in existing schools' market power, even if workers are identical.

Recent estimates of teachers' labor supply elasticity support that teachers have low mobility, although these studies do not provide evidence of whether or to what extent schools actually exploit their market power to lower wages (Falch, 2010, Ransom and Sims, 2010).[7] Another strand of the literature based on cross-sectional evidence shows that areas with more private schools have higher public school teacher salaries (Vedder and Hall, 2000, Medcalfe and Thornton, 2006). However, given the inherent difficulties of isolating the impact of competition from other sources of regional wage differentials mentioned earlier, it is unclear whether these studies render the true association between school competition and wages.

Recent market-oriented reforms implemented in several countries improve the scope for credible identification of competition effects in the teacher labor market. The most closely related paper to my study is that of Kirabo Jackson and Cowan (2009), who study the effects of charter school entry on public school teacher hiring, turnover and wages. Exploiting the entry of nearby charter schools in North Carolina, they provide evidence that private competition leads to higher public school teacher salaries. Though this evidence is compelling, one limitation of their setting is that fixed teacher credentials determine teacher pay to a large extent, which limits schools' ability to respond to local competition. An important contribution of this paper is that I examine the wage effects of competition in a context where wages are set via local negotiations between the school and the teacher.

In addition, this paper provides a more detailed analysis of how the competition effect operates with respect to teacher characteristics. In theory,

[6] Moreover, teachers are often secondary wage earners in a family, which may further limit their mobility. Examining teacher mobility in the US, Boyd et al. (2005) find that teachers delineate their job searches to relatively small geographic areas close to where they grew up.

[7] Ransom and Sims (2010) find firm labor supply elasticities of 3.65 using data for school districts in Missouri, and Falch (2010) studies the impact on the supply of teachers in Norway in response to an increase in wages in some schools with past recruitment difficulties, finding an individual wage elasticity in the region of 1.0-1.9.

school competition could not only contribute to higher teacher wages overall but also lead to a more differentiated wage setting. Workers who bear low costs of switching jobs should, for example, require a higher wage to stay with the current employer as competition increases. In a dynamic framework, schools may furthermore pay attention to the costs associated with teacher turnover and should therefore be more eager to keep teachers in fields with supply shortages, as these must be replaced by on-the-job workers with higher reservation wages (Manning, 2003).

Because funding is tied to student enrolment, competition may moreover increase schools incentives to retain and attract the teachers most valuable to them. Although it has been proven difficult to pinpoint the characteristics associated with teacher quality, previous literature finds that higher school competition is associated with schools valuing teachers' effort, independence, math and science skills and the quality of their college education (Hoxby, 2002). These findings suggest that more competition increases the demand for certain teacher characteristics, but they do not say whether public schools respond to competition from private schools by raising the salaries for certain teachers. In this paper, I examine this question in greater detail and provide results on the heterogeneous impact of private competition on teacher mobility and wages.

3 Data

The data used in this study come from population-wide registers collected by Statistics Sweden. The analysis is based on two main sources. The first of these, the teacher register (Lärarregistret), contains all teachers employed in Swedish schools as well as information about where they are employed (region, public/private), whether the individual is certified to be a teacher and his/her individual field of specialization. The information can be linked to standard demographic characteristics and aggregated regional statistics, such as the number of high school students. From 1995 onward, the data also contain unique school identifiers for the school in which the teacher is employed. The main sample consists of all high school teachers in Sweden in the years 1991 to 2006.[8] Individuals with non-teaching appointments, such as study counselors, are excluded from the sample.

The second register, Strukturlönestatistiken, has annual information on monthly full-time wages for all individuals employed in the public sector and for a sample of individuals in the private sector. I retain one wage obser-

[8] 1991 is the first year for which the data contain wage information for all teachers in the public sector.

vation per teacher and year, determined by the teacher's main source of income.[9]

Wages are measured in November each year, which means that teachers in the academic year 1991–1992 are assigned to the 1991 wage observation. The sampling is stratified by firm size and industry, and the register holds weights that can be used to obtain aggregated regional statistics that are nationally representative. Because part of the empirical strategy relies on within-teacher variation in competitive pressure from private schools, only teachers who appear in the sample for two or more years will help to identify the coefficient of interest. The sampling implies that the probability of observing the same privately employed teacher more than once during the study period is fairly low. For this reason, I impute the log monthly wage for all teachers in private schools who are not sampled in a given year. This is possible because the data contain annual income for all workers, which can be used to recover information on wages for teachers in the private sector.[10] I will check the sensitivity of the results using the weights contained in the data. However, it should be emphasized that for public school teachers, who constitute the great majority of the teaching pool, wages are available for the full working population.

3.1 School competition and local labor markets

I use Statistics Sweden's definition of local labor market regions (LLMs) to define the market in which schools compete for labor. These are based on commuting distance and seem to capture the teacher's true labor market quite well; 88 percent of all teachers in the sample work in their residential local labor market.[11] As a sensitivity check, I also consider alternative geographical boundaries of the local labor market.

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[9] Unfortunately, there is no correspondence between the school identifier and workplace identifier in the data, and I thus run the risk of incorrectly specifying the workplace for teachers with multiple employment spells in a given year. Because this is a potential measurement error in the dependent variable, I regard this as a minor concern. However, to mitigate this problem, I use the detailed (5-digit) industry codes contained in the data to construct a dummy variable for whether the workplace operates in the high school industry. When estimating the models described in Section 5.2, I pool all workers, but I have also tried interacting all variables with this dummy variable, which does not change any of the results. Including all workers in the estimation sample implies that all workers are used to estimate the local labor market fixed effects, leading to more precise estimates.

[10] To impute the monthly wage for private teachers who are not sampled in a given year, I use the predicted monthly wage obtained from the estimation of a traditional Mincerian wage regression, which, apart from standard wage controls (sex, education and the age earning profile), includes a dummy for whether the teacher worked in a private school, details on the type of teaching position and a measure of the approximated wage on the right-hand side derived by dividing the total annual earnings by the number of months adjusted for hours worked.

[11] There are 109 local labor markets in Sweden, which has 2.6 municipalities on average. Figures A1 and A2 provide a map.

Because the aim of this paper is to measure the effects of private school entry on teachers' wages, the main competition measure will simply be the share of private high school teachers in a given local labor market and year.[12] An alternative available measure would be to use the share of private high schools in the local area. However, because private schools are systematically smaller than public schools (see Figure 1), this definition would lead me to understand the impact of competition. For this reason, I focus on the private share of teachers as my preferred measure of competition throughout the analysis.[13]

3.2 Teachers' cognitive and social skills

Apart from standard demographic characteristics, the teacher register can also be linked to information on the cognitive and non-cognitive skills for a large part of the male population. The measures are obtained from the military enlistment, where comparable data are available for cohorts born between 1951 and 1980. In these cohorts almost all males went through the draft procedure at age 18 or 19.[14]

The cognitive tests provide an evaluation of cognitive ability based on several subtests of logical, verbal and spatial abilities and are similar to the AFQT in the US. Individuals are graded on a 1-9 scale, which I use to construct a percentile ranking within each cohort of teachers.

The non-cognitive test scores are based on a standardized interview with a certified psychologist, with the objective to evaluate the conscript's ability to succeed in the military. The personality traits evaluated in the draft procedure are psychological endurance, emotional stability, the ability to take initiative, social outgoingness, sense of responsibility and ease of adjusting to a military environment. The motivation for doing the military service is not a factor to be evaluated. Just as for the cognitive tests, the individuals are scored on a scale from 1-9 and ranked by percentile within each cohort.[15]

Are the skill measures relevant measures of teacher quality? An advantage compared to, for example, value-added measures of teacher quality is

[12] This paper focuses on the effects of increased competition due to the inflow of private independent schools, which is different from competition between public education providers. The ability of teachers and students to choose another public school in a neighboring municipality within the local labor market may potentially impose some competitive pressure on public schools. Although such competition effects are interesting in their own right, they are not the primary focus of this paper.

[13] A common measure is to use the share of students attending private schools in the local education market. Unfortunately, these data are not available for the study period of interest in this paper.

[14] During this time period, it was not possible to avoid the military service by scoring low on the enlistment tests. In contrast, there were incentives to obtain a high score, as the decision about the type of military service was based on the conscripts' performance.

[15] For a more detailed description of these test scores, see Lindqvist and Vestman (*forthcoming*).

that the tests are taken *before* individuals select into the teaching profession and thus do not rely on assumptions about the matching process of students to teachers.[16] Moreover, previous research shows that both the cognitive and non-cognitive ability measures are strongly related to labor market outcomes, such as future wages and earnings (Lindqvist and Vestman, *forthcoming*). In the population of high school teachers used in this paper, the estimated wage-test score relationship appears to be approximately linear (not in paper). Teachers' results on the military tests have furthermore been associated with student outcomes at the compulsory level (Grönqvist and Vlachos, 2008). In sum, these findings suggest that the test scores capture teaching skills that parents and students care about.[17]

Table A1 presents the average cognitive and social ability test scores as well as the correlation between cognitive and social skills for the full sample of male teachers and separately by field. The table shows that there is variation in the average skills across teachers in different fields; math and science teachers have higher cognitive and non-cognitive test scores than the rest of the teachers. Moreover, comparing teachers to the college-educated population in non-teaching professions confirms the notion that teachers are disproportionally drawn from the lower parts of the skill distribution.[18]

It is not clear a priori whether the cognitive or non-cognitive skills are most important. Grönqvist and Vlachos (2008) show that high-performing students benefit from having teachers with high cognitive ability, whereas low-aptitude students are better off with teachers with non-cognitive skills. Moreover, being a good teacher in one field may not require the same skills as being a good teacher in another. In fact, the results will later suggest that the returns to cognitive and social skills differ depending on a teacher's field of specialization.

4 Institutional framework

4.1 The voucher reform

The voucher reform passed in 1994, allows publicly funded private schools to operate in the market for high school education.[19] In practice, this means

[16] Rothstein (2010) provides a critical evaluation of the assumptions underlying commonly used value-added models.

[17] I am not aware of any study that estimates the impact of teachers' test scores on students' high school achievement.

[18] The college-educated population outside teaching score on average 6.65 on the cognitive tests and 5.78 on the non-cognitive tests.

[19] A similar reform was implemented in 1992 at the compulsory school level. The reason for focusing on the high school level is, first of all, that the expansion of private schools is larger here and second because the teachers' field of specialization is well defined, which enables a deeper analysis of how the competition effect operates. The reforms were implemented by the

that local governments, who run the public schools, are required to provide private schools with funding on a per-student basis. Municipalities receive block grants from the central government to be spent on schooling. However, there is no ear-marked money for schools; consequently, there is scope for differences in expenditures on public schools across municipalities (Björklund et al., 2005).

To qualify for public funding, private schools must follow the same rules for enrollment as public schools, which means that they must be tuition-free and admit students based on grades.[20] Besides this, the requirements to receive funding are fairly lax. There are, for example, no regulations on ownership structure and schools are operated by religious, non-profit cooperatives and for-profit corporations.[21] Importantly, local governments have very limited possibilities to influence the entry of private schools in their municipality, as entry is approved at the national level by the National Agency of Education.

4.2 Wage setting in the teacher's market

A key feature of the Swedish context is that teachers' wages are determined at the local level, through negotiations between the teacher and the principal.[22] The individualized pay regime came into place in 1996 through an agreement between the employer's organization and the teacher labor unions. Prior to this, salaries were largely determined by fixed credentials based on the type of work and the number of years of experience, although local deviations were common when faced with, for instance, teacher shortages.

The intention of the reform was to give employers more discretion over wages to reward teacher quality and effort, although quantitative evidence suggests that the move to individualized pay had limited impact on the overall wage dispersion among high school teachers (Söderström, 2006). There are several possible explanations for this. First, there were already deviations from the wage scales before 1996; the labor union of the majority of high

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Conservative-led coalition government that assumed governing power from the Social Democrats in 1991. When the Social Democrats returned to power in 1994, they did not alter the development but continued in the same vein as the previous government.

[20] Top-up funding is not allowed over and above the voucher. Initially, private schools were allowed to charge a tuition restricted to an amount considered reasonable by the NAE, but since 1997, charging tuition is prohibited.

[21] In the immediate aftermath of the voucher reform, private schools were mainly run by non-profit organizations offering special profiles. After this initial stage, the growth in private schooling has mainly been driven by independent schools with a general profile, often run by for-profit companies (Skolverket).

[22] The involvement of a local union representative is also possible in endorsing the proposed salary.

school teachers (Lärarnas Riksförbund) had, in fact, already accepted individualized wage setting in 1992 (Söderström, 2006). Because the wage scales had a steep age-earnings profile in the old regime, wage increases in the lower parts of the age distribution could produce a *more* compressed wage structure than before. Interviews with single principals indeed highlight that teachers entering the profession have benefitted most from the market-based wages (Skolledningsnytt 06/2004). It is also possible that schools' incentives to introduce individualized pay were too weak in a non-competitive environment. The enforcement of individualized wages could therefore be an important mechanism through which the competition effect operates.[23]

It should finally be noted that the strong labor unions could mitigate the scope for employers to capture monosponistic rents by strengthening the bargaining power of workers. Moreover, changes in market competition could in itself affect union power, as the costs of organizing employees should be higher in industries with a larger number of employers (Peoples, 1998). Because both of these mechanisms would lead me to understate the effect of competition, the effects found in the Swedish context could be even larger in markets with weaker labor unions or in those that lack them.

5 Results

This section presents the main results. Section 5.1 looks at the hiring patterns in public and private schools, Section 5.2 focuses the effects of private school competition on wages and Section 5.3 tests the validity of my findings. The empirical strategy is presented in conjunction with the results.

5.1 Hiring patterns

Table 1 shows the characteristics of teachers hired by public and private schools respectively.[24] The data are longitudinal with unique identification numbers for workers and firms, hence, all teachers can be followed over time as well as across schools and alternative employers. New hires are defined as those not observed in the same school in the preceding three years.

In line with previous work, I find that private schools hire younger and fewer certified teachers than public schools. Private schools also hire more frequently from other private schools, other industries and from non-

[23] To understand the full impact of individualized wage setting, it thus seems important to examine how the local wage setting has changed the dispersion within age groups and to account for the possible interaction effects between localized wages and the competitive environment.

[24] Worker flows are studied from 1995, as this is the first year for which the data contain school identifiers.

employment.[25] When recruiting individuals from non-teaching professions, private schools mainly attract workers from the business and retail industries. Public schools in contrast, mainly hire workers from other public sector industries. [26]

Table 1 Public and private school hires (1998-2006).

Hiring school is:	Private	Public
Hire characteristics		
Age	38.6	41.7
Certified	0.43	0.58
Female	0.49	0.51
Cognitive Ability (males)	0.42	0.39
Social Ability (males)	0.42	0.40
Fraction hired from:		
Public high schools	0.14	0.24
Private high schools	0.08	0.01
Other education levels	0.36	0.39
Other industries	0.24	0.20
Non-employment	0.18	0.16
Fractions from other industries:		
Manufacturing	0.10	0.09
Construction	0.02	0.05
Wholesale and retail sale	0.13	0.11
Hotels and restaurants	0.05	0.06
Transport, storage and communication	0.05	0.05
Financial intermediation	0.01	0.01
Real estate, renting and business activity	0.23	0.15
Public administration and defense	0.06	0.09
Health and social services	0.15	0.20
Other community, social and personal services	0.18	0.17
Observations	8,994	44,650
Observations (males)	2,916	12,256

Notes: New hires are defined as workers not receiving compensation from their current school within the three preceding years, which restricts the sample period to 1998-2006. Industries that employ less than one percent of the total hires ("Agriculture, hunting and forestry", "mining and quarrying" and "Electricity, gas and water supply") are not shown in the table.

Next, I look at the characteristics of the teachers who *leave* public for private schools. Björklund et al., (2005) show that the probability to leave a public school for a private one increased proportionally with the expansion of the private schools during the 1990s (Table 1 suggested that, on average, 14 percent of the private school teachers come from public high schools). An

[25] Among teachers hired from other levels, most hires come from primary schools and universities. Only a very low fraction (0.8 percent), are hired from preschools. Other sectors include, e.g., adult education and labor market education.

[26] According to the Swedish Education Act, public and private schools are subject to the same rules regarding recruitments in that all schools – both public and private - are required to hire teachers with appropriate instructional training (1985:1100). Exceptions can be made if people with the required training are not available.

advantage of the data used for this study is that they contain all teachers employed in each school. This enables me to examine whether teachers with certain characteristics are more likely to leave a public school for a private in comparison to all his/her co-workers. In practice, I estimate models of the following type:

$$H_{ipt} = \alpha + X_{ipt}\beta + \theta_{pt} + \varepsilon_{ipt} \qquad (1)$$

where H_{ipt} is a dummy taking a value of one if teacher *i* in public school *p* in year *t* switched to a private school, X_{ipt} a vector of teacher characteristics (age, gender, certification status and field of education) and θ_{pt} a vector of *school*$\times$*year* dummies (i.e., a fixed effect for each set of co-workers for public teacher *i*). Because the model includes *school* $\times$ *year* fixed effects, it accounts for all school characteristics that could influence the decision to leave a public school in a given year, such as teacher and student composition and the regional location of the public school.[27]

The estimated βs presented in column (1) in Table 2 suggest that teacher mobility from public to private schools decreases with age and is significantly lower for certified teachers. However, teachers certified in math and science and social science are more likely to leave than teachers in, for example, vocational subjects. Column (2) includes the teachers' cognitive and social skills. Notably, these results indicate that teachers moving from public to private schools have higher average cognitive skills than those who remain in public schools. This result is not driven by systematic skill differences between teachers in different fields, as the effect looks very similar when including *school* $\times$ *year* $\times$ *field* fixed effects (column 3).

For comparison, columns (4)-(9) display the same results for teachers who leave a public school for another public school. I distinguish between destination schools located in the same municipality (columns 4-6) and those located in a different municipality (columns 7-9), as schools located in different municipalities are more likely to compete over teachers than public schools run by the same local government.

The results suggest that within a municipality, mobility is substantially higher for certified teachers, which may partly be explained by the involuntary reshuffling of teachers between public schools. As expected, cross-municipality mobility is more similar to teacher mobility between public and private schools. However, one distinct difference can be noted; whereas private schools hire teachers from the upper part of public school teachers' skill distribution, public schools appear to recruit teachers with formal qualifications but lower cognitive and social skills.

[27] I restrict the sample to fixed effects groups, where there is variation in the dependent variable, i.e., to schools from which someone was actually hired. A similar method is applied in, e.g., Bayer, Ross and Topa (2008) and Kramarz and Skans (2007).

Table 2 Teacher mobility from public schools.

	Private			Public (same employer)			Public (different employer)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	All	Males	Males	All	Males	Males	All	Males	Males
Age	-0.117***	-0.180***	-0.171***	-0.093***	-0.025	-0.032	-0.132***	-0.166***	-0.147***
	(0.007)	(0.022)	(0.023)	(0.010)	(0.029)	(0.036)	(0.006)	(0.018)	(0.019)
Female	0.019			-0.330			-0.306***		
	(0.120)			(0.201)			(0.086)		
Certified	-0.484**	-0.406	-0.497	1.411***	1.464**	1.311**	-0.196	0.184	0.017
	(0.205)	(0.454)	(0.484)	(0.348)	(0.605)	(0.665)	(0.142)	(0.312)	(0.337)
× Math & Science	0.666**	0.602		-0.648*	-0.431		0.484***	0.771	
	(0.290)	(0.672)		(0.333)	(0.838)		(0.181)	(0.470)	
× Social Science	0.602***	0.745		0.059	0.796		0.483***	0.553	
	(0.206)	(0.570)		(0.265)	(0.645)		(0.144)	(0.412)	
× Vocational subjects	-0.394***	-0.518		-0.450*	-0.633		0.068	0.166	
	(0.120)	(0.356)		(0.256)	(0.570)		(0.101)	(0.283)	
Ability:									
Cognitive ability		1.138**	1.142**		-0.460	-0.677		-0.440	-0.149
		(0.512)	(0.546)		(0.658)	(0.741)		(0.380)	(0.431)
Social Ability		0.048	0.214		-0.245	0.030		-0.596*	-0.462
		(0.544)	(0.625)		(0.698)	(0.774)		(0.347)	(0.407)
Mean of dependent variable	1.93	2.72	2.72	15.25	14.46	14.46	2.40	2.46	2.46
Observations	53,360	12,029	12,029	91,409	18,949	18,949	151,225	33,582	33,582
R^2	0.088	0.141	0.323	0.404	0.430	0.560	0.039	0.102	0.286
School × year dummies	yes	yes	no	yes	yes	no	yes	yes	no
School × year × field dummies	yes	yes	yes	yes	yes	yes	yes	yes	yes

Notes: Each column represents a separate regression. *,** and *** denote statistical significance at the 10/5/1 percent levels, respectively. Standard errors robust for clustering at the school level are shown in parentheses. The dependent variable is an indicator variable taking the value one if the teacher left the public school for a private/public destination school. The sample includes all individuals in public schools where at least one teacher who switched from a public school to a) a private school (columns 1-3), b) to a public school within the municipality (columns 4-6) and c) to a public school in a different municipality (columns 7-9). Columns (3), (6) and (9) display the results when estimating the differential hiring probabilities for teachers within the same school, year and field. The dependent variable has been scaled by 100; hence, the mean probability for a public school teacher to leave for a private school is approximately 2 percent. [A] Because this model includes ability measures from the military enlistment, the sample is restricted to the cohorts for whom data are available, i.e., males born between 1951 and 1981.

5.2 Wages

This section examines the relationship between private school competition and teachers' wages. Before turning to the econometric specification and the estimation results, I provide a brief description of the variation in school competition exploited in the empirical analysis and summary statistics for the sample used in the estimations.

5.2.1 Local school competition and descriptive patterns

As previously mentioned, the reform gave rise to large regional variation in private school openings. Figure 2 shows the kernel density plot of the local labor market specific changes in privatization between 1991 and 2006, and Figure A1 and A2 in the Appendix display how the private high school teachers were geographically distributed across Sweden in 1991 and 2006, respectively. From these figures, it is clear that local labor markets had very different levels of private school penetration during the study period. Whereas some labor markets experienced increases in the share of private school teachers with up to 30 percentage points; in some locations, there had still been no entry of private schools in 2006. Geographically, private schools opened in all parts of Sweden, although we can see that most of the expansion took place in the population dense areas in the south. The empirical strategy uses the within- and cross-regional variation in private school penetration along with several robustness checks to identify the effect of school competition on teachers' wages.[28]

Table A2 in the appendix presents descriptive statistics for the teachers included in the estimations in the pre- and post-reform period respectively. As seen in the table, the share of private teachers was close to zero before the reform and increased to 7 percent on average in the post reform years. Consistent with the results in Table 1, the entry of private schools in the post-reform period is associated with a shift towards fewer certified teachers. Columns (2) and (3) compare teachers in more or less competitive markets. There are substantially fewer teachers in regions without any future expansion of private schools due to the higher frequency of private schools in urban areas. However, whereas teachers received similar wages in the pre-reform period irrespectively of future expansion, wages are somewhat higher among teachers in competitive markets in the post-reform period.

Figure 3 displays the evolution of the median wage difference between regions with and without any private school expansion during the study period. There is no clear trend prior to 1994, whereas wages start to diverge after the reform in favor of teachers in more competitive labor markets.

[28] The few private schools existing prior to the reform were boarding schools, schools for students with special needs or religious schools. A few of these received state funding, although not a per-student basis.

Unless this pattern is explained by unobserved time-varying differences between more or less competitive markets the figure clearly suggests that private competition has a positive effect on teachers' wages.

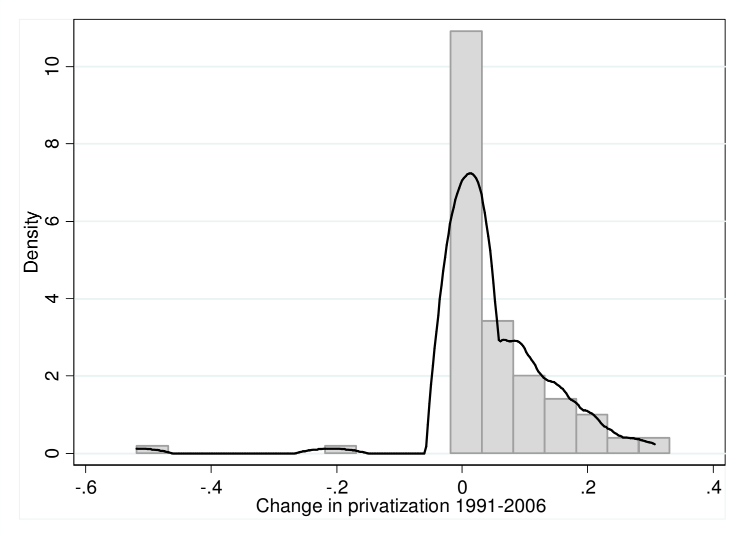

Figure 2 Kernel density distribution of local labor market-specific changes in the share of private high school teachers 1991-2006.

Figure 3 Median wage ratio between teachers employed in local labor markets with (treated) and without (comparison) any post-reform private school expansion 1991-2006.

5.2.2. Empirical Strategy

To estimate the impact of school competition on teacher wages, I exploit the local variation in private school expansion induced by the voucher reform using individual data. The empirical specification is given by:

$$\log w_{ilt} = \beta_1(\overline{P}\times\text{Post})_{lt} + \mu_l + \mu_t + \mu_l \times Year + \beta_3 X_{ilt} + \varepsilon_{ilt} \quad (2)$$

where w is the wage for teacher i in local labor market l in time period t; $\overline{P}$ is the continuous measure of the degree of competition in the local labor market, Post is a dummy taking the value one after the private school reform (=1 if after 1994)[29], X_{ijt} is a vector of observable teacher characteristics (gender, age, educational attainment and certification status) as well as the number of pupils of high school age, μ_t and μ_l are year and local labor market dummies, $\mu_l \times Year$ are local labor market-specific time trends and ε_{ilt} is the error term.

This baseline specification takes into account many of the confounding factors that could generate a spurious relationship between competition and wages; the covariates in X account for compositional changes in the observed characteristics of the teaching pool and for changes in the local demand for schooling due to cohort size fluctuations; the year dummies control for smoothly evolving factors such as business-cycle effects and long-term national trends and the local labor market dummies account for permanent spatial differences in economic outcomes. Importantly, the long time period allows me to eliminate local linear labor market-specific trends, which implies that the parameter of interest is identified from the residual variation in each labor market around its own linear time trend.

A potential concern is that teachers may sort into labor markets with more or less competition based on unobserved characteristics. If this is the case, β_1 may capture both direct effects of competition for incumbent teachers as well as compositional changes in the teaching pool. An advantage of using longitudinal data is that I am able to control for such compositional changes by including teacher fixed effects. Therefore, I augment equation (2) with a vector of teacher-specific indicators, μ_i:

$$\log w_{ilt} = \mu_i + \beta_1(\overline{P}\times\text{Post})_{lt} + \mu_l + \mu_t + \mu_l \times Year + \beta_3 X_{ilt} + \varepsilon_{ilt} \quad (3)$$

The model relies on variation in teachers' exposure to local school competition and it accounts for all unobserved teacher characteristics that are fixed

[29] This is because all variation comes from the post-reform period. Exploiting the variation in the entire period (1991–2006) yields similar results.

over time. Consequently, the effect can only be identified for incumbent teachers.

The parameter of interest is β_1, which captures the full impact of competition in the local labor market averaged across *all* teachers, both public and private. The fixed effects specification also allows for the separate estimation of the wage effects for public and private teachers.[30]

Apart from being associated with teacher mobility, privatization may also affect the composition of students remaining in public schools. For example, if private schools cream-skim, it is possible that the estimated effects capture wage compensation for increased segregation in public schools rather than changes in market power (Epple and Romano, 1998). Because the private schools cannot charge tuition and must follow the same admission rules as public schools, there is probably less room for such selectivity in the Swedish system than in other settings.[31] Moreover, the reshuffling of students between public and private schools would only affect average wages in the local labor market if teacher wages increase disproportionally to the share of low-ability students. Although it is impossible to fully rule this explanation, it is difficult to reconcile with the heterogeneous effects across different teachers shown later in the study.

The assumption maintained for identification is always that the regional private school expansion is uncorrelated with the error term once I have conditioned on all covariates included in (2) and (3). The main source of heterogeneity that is not controlled for and that may generate a spurious relationship between school competition and wages is the presence of local and non-linear trends in unobserved determinants of wages that are correlated with the degree of private school competition. Higher economic growth in a region could, for example, attract parents with a higher demand for private schooling, in which case regions with private school openings may even in its absence have experienced increasing wages. I discuss this and similar threats to identification in greater detail in Section 5.2.5, where I also present a number of robustness checks to validate my findings.

5.2.3 Estimation results

The estimate in the first column in Table 3 shows the baseline effect from estimation of equation (2), which relates teacher wages to the private school share in the assigned local labor market. The dependent variable is the individual log monthly wage and all specifications include individual wage con-

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[30] This cannot be achieved in (2), as the decision to move between schools may be endogenous to the wage. To see this, if private schools attract teachers of high ability (as was suggested by the mobility patterns described in Section 5.1 then looking at public school teachers separately in (2) would produce estimates that are negatively biased by the outflow of teachers from the upper part of the ability distribution.

[31] MacLeod and Urquiola (2009) show that competition via non-selective, for-profit schools leads to less stratification compared to a system with selective schools.

trols, the number of individuals in high school age, year dummies, local labor market fixed effects and local labor market linear trends.[32]

The estimated effect suggests that a one percent increase in the private high school share raises teacher salaries by 0.03 percent on average. This effect, significant at the ten percent level, is rather small; it implies that teachers in areas with the highest levels of competition (at most 30 percent) that occurred in the post-reform period received around 1 percent higher wages than teachers in areas without any private school competition.

Columns (2) and (3) continue to show the differential impact between entering and incumbent teachers, where entering teachers are defined as those who are not observed in the teacher register in any of the five preceding years. As previously discussed, the impact of competition is likely to be higher among teachers who are entering the profession than among incumbents, due to the higher mobility in this group. Consistent with this, I find that the effect is twice as large for new teachers; those who enter the most competitive areas receive 2 percent higher wages than those who enter labor markets without any competition from private schools.[33] Evaluated at the mean entry wage this effect corresponds to a monthly wage difference of roughly 400 SEK/ € 40/USD 50.

Columns (4) and (5) present the results from teacher fixed effects models. As argued above, it is possible that the main effects capture both the direct impact of competition and compositional changes in the teachers' labor pool. However, sorting of teachers does not seem to constitute any large issue of concern.[34] Finally, the last column shows that the estimated effect remains approximately the same when the sample is restricted to public school teachers only, suggesting that public schools respond to private school competition by raising the wages for incumbent teachers.

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[32] To conserve space, I do not report the estimates of the control variables, but it should be noted that these have the expected signs; wages are higher for males than for females, increase with age and level of education and are higher for certified teachers. Weighting the sample instead of using imputed wages does not alter any of the results, although the estimates are less precise. These results are available upon request.

[33] To arrive at 2 percent, I scale the estimate with the highest realized levels of competition, i.e., 0.068*0.3=0.0204.

[34] Note that the identification in the teacher fixed effects specification comes from both within and between local labor market variation in school competition. However, including teacher by LLM fixed effects (i.e., looking at wage changes for the same teacher within the same local labor market) produces the estimate 0.038 (0.017). Therefore, changes in the competition measure are not driven by teacher mobility between more/less competitive labor markets.

Table 3 Baseline estimates.

	Dependent variable: log(monthly wage)				
Sample:	All	Entering	Incumbent		
				Teacher fixed effects	
	(1)	(2)	(3)	(4)	(5)
			All	All	Public
Private share × Post	0.032*	0.068*	0.031*	0.037**	0.032*
	(0.017)	(0.036)	(0.017)	(0.017)	(0.018)
Observations	408,731	47,169	361,562	361,562	341,689
R^2	0.716	0.632	0.723	0.900	0.901
LLM fixed effects	yes	yes	yes	yes	yes
LLM linear trends	yes	yes	yes	yes	yes
Teacher fixed effects	no	no	no	yes	yes

Notes: *,** and *** denote statistical significance at the 10/5/1 percent levels, respectively. Standard errors robust for clustering at the local labor market level (109 LLM:s) are shown in parentheses. In addition to the fixed effects indicated by the table, all regressions control for year fixed effects, a dummy indicating whether the individual wage is imputed or not and the number of students in high school age in the given labor market and year. The individual controls include gender, age, age^2 and education dummies (6 bins).

5.2.4 Heterogeneity in the competition effect

Although the estimates in Table 3 were positive and significant, the average effects suggested a rather small economic impact from competition on incumbent teachers' wages. Whereas the baseline model assumes that the wage effect is the same for all teachers, we know from the theoretical discussion in Section 2 that the effects could differ between teachers with different characteristics.

The first panel in Table 4 reports the results of the estimation of fully interacted versions of model (3) with respect to teachers' field of specialization (defined by their field of education). Notably, there is substantial heterogeneity across different teachers; the entire effect of private competition is concentrated among teachers specialized in math and science.[35] Restricting the sample to public school teachers produces similar estimates, suggesting that public school teachers in math and science get higher wages in local labor markets with more private alternatives.

There are at least two possible explanations for this finding. First, if parents and students place higher value on math and science skills, schools have stronger incentives to retain and attract math and science teachers than teachers in other subjects. Second, the heterogeneity could also reflect dif-

[35] Because the model controls for local labor market fixed effects and local labor market linear time trends, differences between regions, such as a higher demand for math teachers in metropolitan areas with more employment in high-technology industries, is unlikely to explain the result. I address this and similar concerns further below.

ferences in teacher supply in the context of a greater need for teachers in math and science.

The second and third panel of Table 4 reports the estimates separately by gender. These results support the explanation that competition matters more for teachers in areas of needs; the effects are concentrated to male teachers in math and science and female vocational teachers. The latter group is mainly specialized in “health and social work”, a field suffering from great shortages, according to the Swedish National Agency of Education.[36]

Table 4 Heterogeneity in the competition effect by teachers’ field.

	Incumbent teachers			
	(1)	(2)	(3)	(4)
	All	Math and Science	Social Science	Vocational subjects
Sample: All				
Private share × Post	0.037**	0.079**	0.020	0.031
	(0.017)	(0.037)	(0.019)	(0.025)
Observations	361,562	22,135	45,401	113,846
Sample: Males				
Private share × Post	0.040*	0.104**	0.053*	0.005
	(0.020)	(0.049)	(0.027)	(0.030)
Observations	187,521	14,364	17,236	63,534
Sample: Females				
Private share × Post	0.029	0.022	0.001	0.062**
	(0.018)	(0.039)	(0.022)	(0.027)
Observations	174,041	7,771	28,165	50,312
LLM fixed effects	yes	yes	yes	yes
LLM linear trends	yes	yes	yes	yes
Teacher fixed effects	yes	yes	yes	yes

Notes: *,** and *** denote statistical significance at the 10/5/1 percent level respectively. Standard errors robust for clustering at the local labor market level are shown in parentheses. The dependent variable is the individual log monthly wage. In addition to the fixed effects indicated by the table, all regressions control for year fixed effects, a dummy indicating whether the individual wage is imputed or not and the number of pupils in the given labor market and year. The individual controls include gender, age, age^2 and education dummies (6 bins). Because the model includes teacher fixed effects it estimates the effect for incumbent teachers only. Column (1) includes all teachers employed in Swedish high schools. Besides those specialized in the fields mentioned in the table, a large fraction are non-certified teachers and teachers defined as having “other” as their field of specialization.

[36] Manning (2003) proposes to use the fraction of new hires from non-employment to proxy for labor market tightness. Figure A3 illustrates this measure calculated for teachers in different fields. The figure is largely consistent with other descriptions of the teachers’ labor market; whereas there is an ample supply of teachers in the social sciences, the shortages are most pronounced among teachers in vocational subjects (SACO, 2009). Among female vocational teachers, the largest shares are found in Health and Social Care (34.5 percent), Business and Administration (13 percent) and Sports (11.1 percent). Males are most often found in Manufacturing (60 percent), Sports (12.4 percent) and Music (8.7 percent).

5.2.5 Robustness checks

As previously discussed, the main concern is that the expansion of private schools is correlated with trends in unobserved determinants of wages not captured by the local linear trends. In particular, if the fixed salary schemes were binding prior to the wage bargaining legislation in 1996, the results may simply capture a spurious relationship between private school competition and wages showing only after the removal of the wage scales.

Table 5 presents results from several robustness checks addressing these concerns. The first column reproduces the estimate for the baseline specification in column (4) of Table 3. The remaining columns present estimates of variants of the baseline model. Because the strongest effects were found for teachers in math and science, Panel B presents estimates for this group separately.

Local wage trends in non-teaching professions

As a first test, I use information on wages for workers in other segments of the local labor market to test whether my results are sensitive to general local wage trends. The wages of other workers should capture trends in unobserved factors driving higher wages for everyone in the local labor market. Therefore, as long as there is no direct effect of high school entry on other workers' wages, this can be considered a strong test.[37]

I first include the log median wages for all college-educated workers in the same local labor market and year (column 2). The college-educated should be more comparable to teachers than workers with lower education levels. I also interact wages with an indicator of whether wages were measured after 1996, the year of the wage bargaining legislation, to pick up any effects of differential wage trends that may show up after the abolition of the wage scales.

Second, in column (3) I use the synthetic control method proposed by Abadie et al. (2009) to construct median wages for those workers who were most similar to teachers in the pre-reform period.[38] As we can see, the estimates do not change when including these wage measures, which clearly strengthen the conclusion that general trends are unlikely to explain the main results.

[37] I exclude workers in education, health and social work and other public services because these could potentially be affected by the entry of private schools.

[38] The method delivers the best counterfactual to the treated unit (teachers in this case) based on the outcome variable (wages) and observable characteristics (age and gender) in the pre-reform period. In practice, I use 2-digit industry codes and construct weights based on how similar workers in different industries were to teachers in the pre-reform period using the synth software package in STATA. The weights obtained for each industry are used when calculating the median wage in each local labor market and year included in the regression. I also tried calculating the weights separately for each local labor market and received very similar results.

Table 5 Specification checks.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Baseline effect	+ Wage controls	+ Wage controls	Falsification test:	+ Political majority	- Trends	+ Region × Year	- Stockholm
		College educated	*Synthetic control group*	*Pre-school teachers*				
Sample: All teachers								
Share private×Post	0.037**	0.039***	0.033*	-0.007	0.037**	0.104***	0.060***	0.048**
	(0.017)	(0.016)	(0.017)	(0.016)	(0.017)	(0.029)	(0.023)	(0.022)
Observations	361,562	361,562	361,562	1,410,021	361,562	361,562	361,562	298,995
Sample: Math & Science								
Share private×Post	0.079**	0.089**	0.077**	-	0.084**	0.109***	0.073*	0.059
	(0.037)	(0.035)	(0.034)	-	(0.035)	(0.035)	(0.042)	(0.043)
Observations	24,235	22,135	22,135	-	24,235	24,235	24,235	22,584
LLM fixed effects	yes	yes	yes	yes	yes	yes	yes	yes
LLM linear trends	yes	yes	yes	yes	yes	no	no	yes
Teacher fixed effects	yes	yes	yes	yes	yes	yes	no	yes

Notes: *,** and *** denote statistical significance at the 10/5/1 percent levels, respectively. Standard errors robust for clustering at the local labor market level are shown in parentheses. The dependent variable is the individual log monthly wage. All specifications in the table control for year fixed effects, a dummy indicating whether the individual wage is imputed or not, gender, age, age^2, education dummies (6 bins) and the number of pupils in the given labor market and year. Column (1) restates the baseline estimate presented in Table 3, column (4) and Table 4, column (1). Column (4) estimates the association between private high school expansion and wages among preschool teachers obtained from estimation of model (2) described in Section 5.2. Because there is no reliable information on hours worked by preschool teachers, observations for those working in the private sector have been weighted according to the individual sampling probabilities.

Finally, I examine whether wages of preschool teachers are also affected when the share of private high schools increases (column 4). The results provided earlier showed that the inflow of private high schools did not impose any increased competition for preschool teachers (only 0.8 percent of the total hires come from preschools).[39] These teachers should therefore be unaffected by the variation generated by the voucher reform, unless high school entry is correlated with trends in other factors that influence public sector wages, such as public spending, demands for private schooling, area amenities or labor quality. Reassuringly, I find no relationship between private high school expansion and wages among preschool teachers, which supports the conclusion that the main effect is not driven by local trends in omitted factors, at least to the extent that these are common to teachers at the preschool and high school levels.

Additional robustness results

It is likely that private schools choose to locate in areas with higher demand for private schooling. Interviews with private secondary schools indicate that attitudes toward privatization are an important factor for the location decision (Böhlmark and Lindahl, 2008). Because changes in the demand for privatization may be correlated with changes in other factors determining wages, we would like to control for local preferences for privatization in the model. In Column (5), I use information on the local political majority (left/right) to capture changes in the demand for privatization. Although local politicians cannot directly influence the approval of private schools, their attitudes toward privatization should reflect local preferences for private schooling. A conservative local authority (which is known to be friendlier toward privatization) could also indirectly affect private schools location decisions through its influence on, for example, the supply of buildings. Including a political majority in the model, however, does not change the results.

Column (6) presents results without the linear trends, and column (7) includes *region*×*year* fixed effects instead.[40] Although the estimates are somewhat sensitive to the omission of trends, the estimates are not statistically different from the main effect.[41] Importantly, the results also suggest that time-varying differences at the region level are not driving the main

[39] Preschool teachers also experienced an increase in private alternatives although much less dramatic. Hanspers and Hensvik (2011) show that this increase did not affect wages among preschool teachers. In addition, because the funding of private schools is based on the number of pupils enrolled, any potential negative spill-over effects of wage increases among high school teachers are likely to be small.

[40] There are 21 regions (or counties) in Sweden, each containing 7 (sd 3.2) local labor markets on average.

[41] Including quadratic trends in the model yields an estimate very close to the baseline (0.031 (0.017)).

effect. As an additional test for local trends, I run regressions exploring the relationship between future privatization (t+2) and current wages (Table 6). Unless the baseline model is picking up spurious effects, future privatization should not affect current wages, conditional on the current level of privatization. As we can see, the estimate of future privatizations is small and not statistically significant.

The last column of Table 5 reports the estimate based on a sample excluding the Stockholm local labor market. As the capital and largest metropolitan area of Sweden, Stockholm constitutes an important labor market for teachers, and it is therefore plausible that differences between Stockholm and other local labor markets in Sweden could have a large influence on the main effect. Additionally, the city of Stockholm implemented an additional reform in 2000, enlarging the catchment area at the high school level.[42] Excluding the Stockholm area from the sample does not significantly change the baseline estimate.

Table 6 Specification checks (cont.).

	(1)	(2)
Sample:	All	Math and Science
Private share × Post	0.051*	0.121**
	(0.028)	(0.051)
Private share$_{t+2}$ × Post	0.007	0.010
	(0.038)	(0.045)
Observations	307,640	18,702
LLM fixed effects	yes	yes
LLM linear trends	yes	yes
Teacher fixed effects	yes	yes

Notes: see Table 5

Alternative definitions of the local labor market

Table B1 in Appendix displays the results using other definitions of the local labor market. Failure in defining the correct labor market may lead to downward-biased estimates due to measurement error. I use two alternative measures of competition, the municipality and the county. Salaries appear to increase irrespectively of the definition of the local labor market and most of the estimates are in the vicinity of the main effects. However, using municipality level variation in private school competition produces smaller estimates, suggesting that this is a too narrow definition of the local labor market. The effects are in contrast somewhat larger when looking at the county

[42] Before this reform, students in the city of Stockholm, as in the rest of Sweden, could choose high school but were assigned to their neighborhood schools in the case of space limitations. After the reform, student admission is based solely on grades, which means that the scope for competition over students is larger compared to the rest of Sweden.

level, although some of the estimates are less precise, as there is less variation in school entry at the county level.[43]

6 Differential effects: teachers' cognitive and social skills

Having concluded that the results seem not to be driven by trends in omitted factors, I now turn to the association between school competition and the link between teacher wages and teacher ability.

I estimate models similar to those in Table 3 but allow the effect of school competition to vary by the teachers' ability. For simplicity, I distinguish between teachers with *high* and *low* cognitive and social ability, separated by the median percentile rank in the distribution of cohort-specific military test scores. Specifically, I estimate models of the form:

$$\log w_{ilt} = \mu_i + \beta_1(\overline{P}\times\text{Post})_{lt} + \lambda((\overline{P}\times\text{Post})_{lt} H_i + \mu_l + \mu_t + \mu_l \times Year + \beta_3 X_{ilt} + \varepsilon_{ilt} \quad (4)$$

where H_i indicates whether the teacher is above the median in the cognitive and social skill distributions, respectively. I estimate the model separately by teachers' field of specialization, and positive estimates of λ imply that high-ability teachers have higher returns to competition than low-ability teachers within the same field.

Estimates of β and λ are reported in Table 7. The sample consists of all male teachers belonging to the cohorts born between 1951 and 1980. Because the model includes teacher fixed effects, beginning teachers are automatically excluded. The results suggest that there are significant differences in the wage impact of competition depending on the skill level of the teacher. In particular, the effects are concentrated among teachers in math and science with *high cognitive skills* (column 2) and among teachers in social science with *high social skills* (column 3).[44] These results are interest-

[43] I have also tried interacting the variable of interest, i.e., the share of private high school teachers with the absolute number of private high schools in the local area. If there are differences in the competitive pressure in markets where many small private schools enter as opposed to one or two large private schools entering, I could potentially miss variation in competition from private school entry. I found, however, no significant difference depending on the number of schools entering. In addition, using the absolute number of private high school teachers instead of the local private share produced very similar results to the baseline. Both of these results are available upon request.

[44] The test score ranges from 1-9, and the median scores are 6.2 and 5.1 for cognitive and social skills, respectively, in the overall sample of male teachers. Separate regressions for high- and low-skilled teachers yield similar results as in Table 6. Instead of using the median, I have also tried dividing the teachers into three groups based on their position in the test score distribution of teachers within their field. This reinforces the pattern in Table 6, in par-

ing for several reasons. First, they suggest that competition increases wage dispersion between high- and low-skilled teachers within the same field, which can have important implications for the selection of individuals who decide to become a teacher. Second, the results also indicate that the market returns to teacher skills are not the same for all teachers, which highlights the difficulties of using measurable teacher characteristics as general proxies for teacher quality.

Table 7 Estimates by field and teacher skills.

	Incumbent teachers			
	(1)	(2)	(3)	(4)
Sample:	All	Math and Science	Social Science	Vocational subjects
Private share×Post	0.036	-0.082	0.038	0.032
	(0.026)	(0.067)	(0.052)	(0.038)
×***High cognitive ability***	0.014	0.238**	-0.054	0.049
	(0.029)	(0.092)	(0.037)	(0.041)
×***High social ability***	0.001	-0.036	0.049*	-0.026
	(0.019)	(0.044)	(0.028)	(0.027)
Observations	68,321	5,514	8,707	22,899
LLM fixed effects	yes	yes	yes	yes
LLM linear trends	yes	yes	yes	yes
Teacher fixed effects	yes	yes	yes	yes

Notes: *,** and *** denote statistical significance at the 10/5/1 percent levels, respectively. Standard errors robust for clustering at the local labor market level are shown in parentheses. In addition to the fixed effects indicated by the table, all regressions control for year fixed effects, a dummy indicating whether the individual wage is imputed or not and the number of pupils in the given labor market and year. The individual controls include gender, age, age^2 and education dummies (6 bins). An individual is recorded to be of high ability when that individual has a military test score above the 50th percentile within each cohort. Column (1) includes all teachers employed in Swedish high schools that belong to the sample. Aside from those specialized in the fields mentioned in the table, representing a large fraction are the non-certified teachers and teachers with "other" as their field of specialization.

Finally, to complement the results in Table 7, I also estimated the returns to cognitive and social skills separately for each local labor market. The upper-left graph of Figure 4 plots the relationship between the returns to cognitive skills and the degree of competition in each local labor market. There is substantial variation in the returns to cognitive skills across locations; they are high in some labor markets, and low – even negative – in other.[45] Consistent

ticular for math and science teachers, whereas the differences are less clear for the other groups.

[45] The returns are estimated using traditional wage regressions, where the coefficient of interest is the continuous measure of the individual ability test score and the included controls are age, age^2 and field of education. The restrictions of the sample (male teachers born between 1951 and 1981) require me to focus the analysis on 1/3 of the largest local labor markets. Furthermore, I choose the base year in the middle of the sample period (2000), as this allows

with the findings above, the figure suggests that there is a positive relationship between the returns to teacher skills and local school competition.

The association appears to be even stronger when relating the changes in local school competition to the changes in the returns to cognitive skills and thus when time-invariant differences across localities are taken into account (upper right). Performing the same analysis in a regression framework yields similar findings (Table B2 in Appendix), although the conclusions only hold for the cognitive skills.

As previously discussed, one worry is that the relationship reflects general regional trends in the returns to ability that are correlated with private high school expansion. Therefore, I estimated the returns to ability for workers in a non-teaching profession in the same region. If there is an association between school competition and returns to ability for these workers, one should probably worry that the relationship between school competition and returns to skills is spurious. The lower graphs of Figure 4 plot the relationship for engineers, an occupation that should be a potential alternative for teachers educated in math and science.[46]

As expected, engineers have higher average returns to ability compared to teachers. There is also a positive relationship between the level of returns and private school expansion suggesting that private schools tend to locate in regions where the returns to ability are higher in general. Importantly, however, there seems to be no association between the *changes* in high school competition and returns to ability for these individuals (lower right). This is reassuring, as it suggests that the increase in the returns to ability among teachers is at least not driven by trends that are common for these two groups of workers.

me to include younger cohorts, thereby increasing the number of teachers in the estimations. Apart from lowering precision, choosing an earlier base year does not alter the main conclusions.

[46] Interestingly, engineers score on average 7.10 (5.85) on the cognitive (non-cognitive) test, which is very similar to the average test scores among math and science teachers in Table A1.

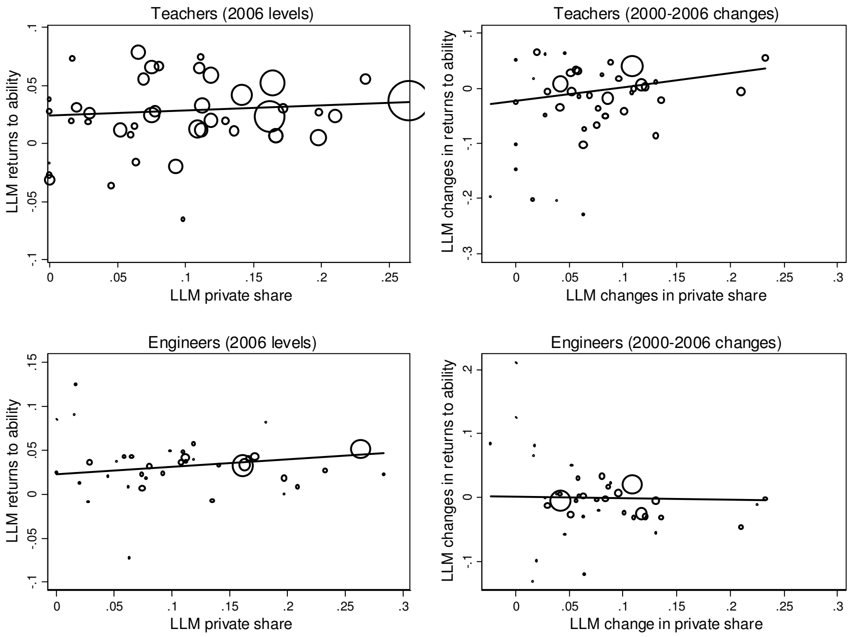

Figure 4 Relationship between school competition and the local labor market specific wage returns to cognitive ability.

Notes: The estimated returns to cognitive test scores are obtained from traditional wage regressions estimated in 2000 and 2006. Apart from the test scores, controls include the age-earnings profile and a detailed field of education. The estimations are based on a sample of male teachers belonging to cohorts born between 1951 and 1980. Each observation is weighted according to its inverted sampling variance of the estimated returns in 2006.

7. Conclusions

This paper has examined the impact of local private school competition in Sweden, introduced by a voucher reform, on teacher flows and wages in a decentralized wage setting. The findings can be summarized in four main conclusions:

(1) Private schools deviate substantially from public schools in their recruitment behavior. Whereas private schools do not necessarily hire the most qualified teachers in terms of formal certification they do seem to attract younger teachers, teachers specialized in math and science and social science, and teachers in the upper part of the skill distribution. In line with previous research, these findings suggest that the private schools, with presumably stronger incentives to attract students, value different teacher characteristics from those valued by traditional public schools.

(2) The increase in local school competition is also associated with higher teacher salaries. This effect cannot be explained by compositional changes in the teaching pool, and it remains when the sample is limited to public school teachers. The finding suggests that the absence of competitive forces in the teachers market has contributed to the depressed wages in the profession, leading to higher teacher salaries in more competitive labor markets.

(3) Whereas the average effects are modest, there are substantial differences in the effect of private competition with respect to teacher characteristics. The effect is, first of all, twice as large for teachers who are entering the profession. One potential explanation is that these teachers have higher wage elasticity than incumbent teachers, which allows them to exploit the increased number of employers when negotiating over the entry wage. Second, there are also differences with respect to teachers' field of specialization; the effects are concentrated among teachers in areas of needs, such as male teachers in math and science and female teachers in vocational subjects.

(4) Finally, there is evidence that school competition increases wage dispersion between high- and low-skilled teachers within the same field. Specifically, I find that competition has introduced higher returns to cognitive skills for math and science teachers and higher returns to social skills for social science teachers.

To summarize, this paper shows that abolishing the local monopoly in the Swedish educational market has increased teacher salaries and introduced a wage setting more closely related to teacher mobility, the current market conditions and teachers' skills. Given the literature highlighting the relationship between the pay structure and the secular declines in teacher quality observed in many countries, these findings are promising, as more competition between schools can generate a wage setting that may enhance the selection of teachers as well as the effort and output levels in a given teaching pool. These conclusions may extend to other markets that share common attributes with teacher labor markets, such as nursing and other social services. According to my findings, increased competition can help push up wages in these labor markets and create a more incentive based wage structure.

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Appendix A Descriptive analysis

Table A1 Teacher characteristics (by field).

	(1)	(2)	(3)	(4)
	All	Math and Science	Social Science	Vocational subjects
Log(wage)	9.95	9.98	9.96	9.92
Age	47	46.9	45	48
Female	0.48	0.36	0.63	0.45
Observations	408,731	24,607	50,481	123,552
Skill measures (*males*):				
Cognitive ability (1-9)	6.15	7.12	6.27	5.80
Social ability (1-9)	5.61	5.96	5.59	5.64
Corr. cognitive-social ability	0.18	0.17	0.15	0.15
Observations	93,501	6,800	10,764	28,094

Notes: The table shows summary statistics for the sample of teachers 1991-2006. The teachers' field of specialization is defined by the field of education. Apart from the subject area teachers in columns (2)-(4), column (1) contains all other teachers' i.e. non-certified teachers and teachers specialized in other fields.

Table A2 Descriptive statistics.

	(1)	(2)	(3)
	All	Treated	Comparison
Period 1991-1993			
Share private	0.02	0.02	0.01
Log(wage)	9.76	9.76	9.75
Sd	(0.14)	(0.14)	(0.15)
Age	47	47	47
Female	0.47	0.48	0.40
Certified	0.91	0.92	0.86
Observations	76,642	67,415	9,227
Period 1994-2006			
Share private	0.07	0.08	0.01
Log(wage)	9.98	9.99	9.97
Sd	(0.17)	(0.17)	(0.18)
Age	47	47	47
Female	0.49	0.49	0.43
Certified	0.80	0.79	0.76
Observations	390,075	342,488	47,587

Notes: The table compares local labor markets with positive expansion (Treated) and no expansion (Comparison) of private school teachers during the entire study period (1991–2006).

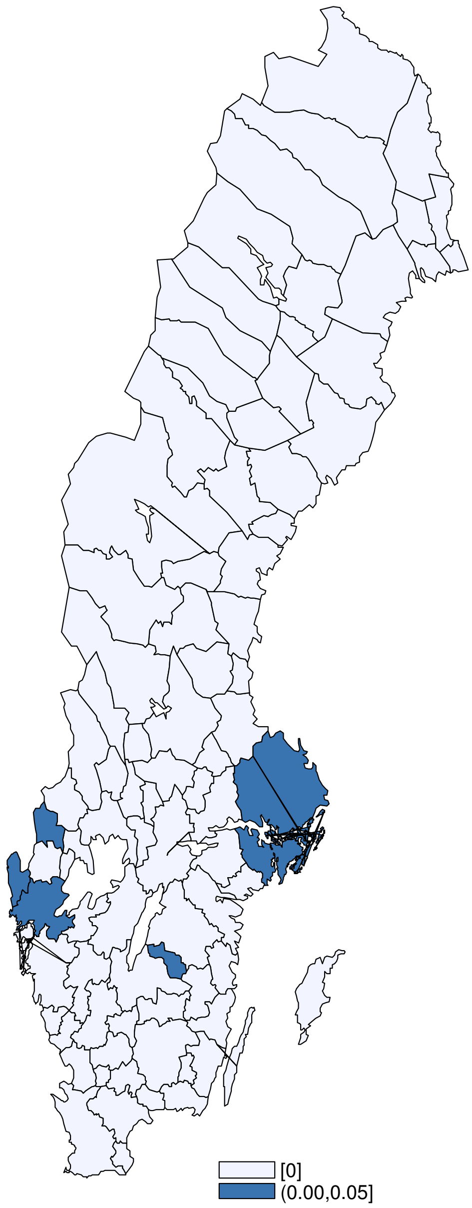

Figure A1 Privatization in Swedish local labor markets 1991.

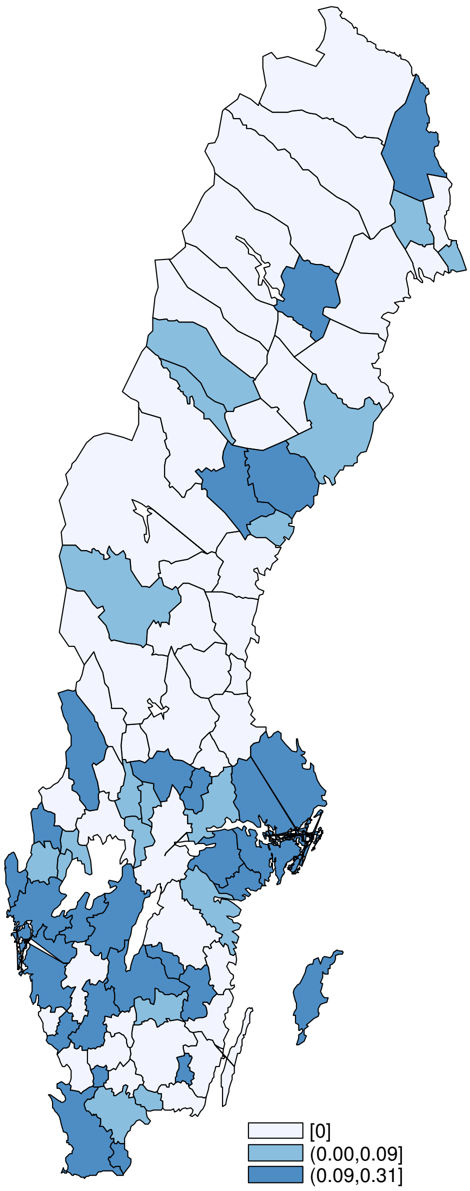

Figure A2 Privatization in Swedish local labor markets 2006.

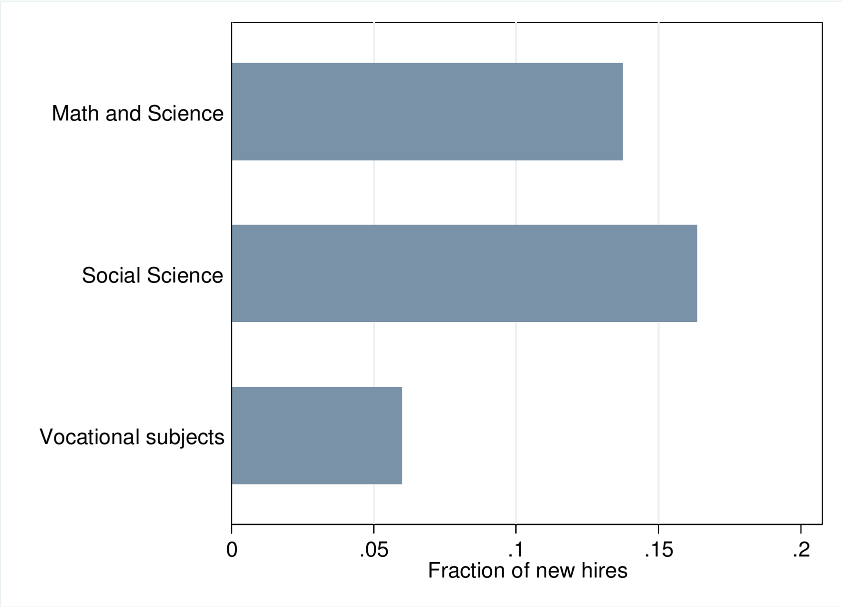

Figure A3 Fraction of recruits from non-employment.

Notes: The fraction of recruits from non-employment is calculated by taking the number of newly recruited teachers who were observed as being non-employed in the previous year.

Appendix B Additional results

Table B1 Alternative labor market definitions.

	All	Entering	Incumbent		
				Teacher fixed effects	
	(1)	(2)	(3)	(4)	(5)
			All	All	Public
A: Local labor market					
Private share× Post	0.032*	0.068*	0.031*	0.037**	0.032*
	(0.017)	(0.036)	(0.017)	(0.017)	(0.018)
B: County					
Private share× Post	0.077**	0.143**	0.071**	0.062	0.059
	(0.033)	(0.060)	(0.033)	(0.036)	(0.035)
C: Municipality					
Private share× Post	0.009*	-0.002	0.012**	0.014	0.021*
	(0.005)	(0.006)	(0.005)	(0.009)	(0.011)
Observations	408,731	47,169	361,562	361,562	341,689
LLM fixed effects	yes	yes	yes	yes	yes
LLM linear trends	yes	yes	yes	yes	yes
Teacher fixed effects	no	no	no	yes	yes

Notes: *,** and *** denote statistical significance at 10/5/1 percent level respectively. Standard errors robust for clustering at the local labor market/county/municipality level are shown in parenthesis. In addition to the fixed effects indicated by the table all regressions control for year fixed effects and a dummy indicating whether the individual wage is imputed or not. The individual controls include gender, age, age^2, education dummies (6 bins) and the number of pupils in the given labor market and year.

Table B2 OLS estimates of privatization on returns to ability (males).

	Not weighted	Weighted
A: Dep. var.: estimated LLM returns:	Cognitive skills	Cognitive skills
Private share	0.063*	0.030
	(0.034)	(0.027)
B: Dep. var.: estimated LLM returns:	Social skills	Social skills
Private share	0.020	-0.011
	(0.024)	(0.017)
Observations	86	86
Year dummies	yes	yes
LLM fixed effects	yes	yes

Notes: The table shows the relationship between the share of private high school teachers and the estimated returns to cognitive and social skills in the assigned local labor market obtained from traditional wage regressions that apart from the test scores include age, age^2 and detailed field of education. The sample consists of all male teachers in the cohorts born between 1951 and 1980. Column (2) weights each observation with its inverted sampling variance of the estimated return to ability.

Essay 2: Seeking Similarity: How Immigrants and Natives Manage at the Labor Market♣

Co-authored with Olof Åslund and Oskar Nordström Skans

1 Introduction

Managers are key players in the labor market. Their practices matter for firm performance, for the overall wage distribution, and for the allocation of skills across firms and industries. The impact of managerial decisions is of first order importance for the individual workers whose potential job offers and wages are determined by actual managers. Motivated by the theoretical (e.g. Currarini et al., 2009) and experimental (e.g. Fershtman and Gneezy, 2001) evidence on in-group biases in decision making in general, this paper investigates how manager and worker origin affect hiring patterns, job separations, and entry wages.

Although the poor economic integration of migrant workers in many countries has received a lot of attention, a rarely analyzed fact is that groups with poor labor market performance also tend to be underrepresented among managers. In our data, 7.2 percent of recruited workers but only 3.7 percent of the managers are foreign born. If similarity in background matters, this unbalance offers a potential contributing explanation to the immigrant-native performance gap as well as an explanation for the strong ethnic segregation across workplaces found in many countries.[1]

Studies relying on cross-sectional data have documented correlations between manager race and the race of hires (Carrington and Troske, 1998; Stoll

♣ We are grateful for comments by Mikael Lindahl, Lena Magnusson-Turner, Eva Mörk, Peter Skogman-Thoursie and seminar participants at IFAU, IBF, the 2008 Cost Conference in Paris, the ELE Immigration Conference in Helsinki, the labor lunch workshop at Harvard University, the Nordic Labor Economists meeting in Bergen, the 2009 Swedish Integration Research Network Conference in Lund, and the 2009 SOLE and EEA conferences. Financial support from FAS is gratefully acknowledged. The order of the authors is in accordance with the English alphabet and not related to contribution.

[1] See Hellerstein and Neumark (2008) for the US, Åslund and Skans (2010) for Sweden and Dustmann et al (2010) for Germany.

et al., 2004). But it is difficult to distinguish the causal impact of manager characteristics on hiring patterns from spurious relationships generated by non-random sorting on other (potentially unobserved) characteristics of firms and workers. To facilitate more reliable identification, a number of recent papers have relied on single-firm data with detailed accounts of many aspects of the process. Bandiera et al. (2009) study the importance of nationality and social connections between the manager and employees for the allocation of jobs within a British fruit-picking farm where workers are allocated to jobs on a day to day basis. Two papers by Giuliano et al. (2009, 2011) document substantial ethnic biases in hiring and firing in a large US retail chain.

These recent studies provide compelling evidence of a causal effect of manager race/ethnicity on hiring patterns in the studied firms. The main advantage of these datasets is the detailed longitudinal information on workers and managers combined with high worker turnover. Bandiera et al. (2009) also include precise accounts of worker productivity.[2] However, whether the findings form the studied firms can be generalized to other parts of the labor market where jobs may be rationed and turnover is low remains an open issue.

Our study complements the earlier studies by analyzing hiring patterns in a very broad set of firms. We use a longitudinal matched employer-employee data set with more than 100,000 Swedish workplaces across the entire economy during a nine-year period. This allows us to implement various strategies to account for unobserved heterogeneity among workers, managers and firms, as well as to document the effect of manager origin across industries, firm size and type of ownership. In addition, we also provide evidence on the role played by the managers' networks of former co-workers.

Our analysis shows that immigrant and native managers differ dramatically in their hiring patterns. Native managers hire on average 6 percent immigrant workers; the corresponding figure for immigrant managers is 43 percent. A strong association remains when comparing different establishments in the same 5-digit industry and location (e.g. different pharmacies in the same village), different establishments within the same firm and location,

[2] A closely related literature studies managers from a gender perspective and shows a positive correlation between female management and female wages (Carrington and Troske 1995; Hultin and Szulkin 2003). Using a matched employer-employee dataset for Portugal, Cardoso and Winter-Ebmer (2010) estimate the effect of within-establishment manager changes and find that female-led firms pay a premium to female workers of almost 5 percent. Using Swedish data, Hensvik (2011) documents a similar correlation between the share of female managers and the establishment gender wage gap but finds that most of this relationship is attributable to female mangers hiring women with higher (unobserved) skills compared to male managers. She finds no evidence of increased hiring prospects for women in women-led establishments.

and when studying establishments that change management over time. The estimates are economically and statistically significant throughout these specifications and remain if the composition among incumbent workers is controlled for.

We also find that similarity matters for both high- and low-skilled workers, in workplaces of different sizes, in both the private and public sector, and in most industries. But the effects tend to be larger for low-skilled workers and males, in small and owner-managed companies and in service industries (where the manager has a higher financial stake and/or higher need to interact directly with the new employee). We find no entry wage premium from sharing origin with the manager. Productivity gains can therefore not be the sole underlying mechanism unless starting wages are independent of match-specific productivity. Separations are more frequent when workers and managers are of dissimilar origin, but only within the first year of employment.

Notably, the probability that an immigrant worker is hired is higher if the manager (immigrant or native) has worked with more immigrant workers in the past. Moreover, the probability for an immigrant to be hired by a native manager is the same as for native workers when the hiring manager and the worker have worked together in the past. These results point to the importance of networks acquired through previous work-life interactions.

The remainder of the paper is structured as follows. Section 2 briefly discusses the theoretical arguments on the importance of manager origin for hiring patterns and the institutional background. Section 3 presents the data. Section 4 provides some descriptive patterns and sample statistics. Sections 5–7 present the empirical results along with the respective methodological approaches. Section 5 presents the results on the impact of manager origin on the origin of hires. Section 6 analyzes the impact of origin similarity on separations and wages. Section 7 analyzes how previous interactions in the labor market affect the probability of hiring people with similar/dissimilar origin. Section 8 concludes.

2 Background

2.1 Why ethnic similarity may matter

A hiring constitutes a match between an individual and a workplace. The behavior of both parts, as well as the total surplus from a realized match, may therefore be important for who gets hired. Below we briefly discuss different explanations for why ethnic similarity between workers and managers may be important for recruitment patterns. In the presentation of the empirical results and in the conclusions we try to link the findings to the respective hypotheses.

First, workers who have a similar background as their manager may become more *productive*. A common language or business culture can e.g. lower transaction and communication costs (Lazear 1999; den Butter et al., 2004). A case where this mechanism should be particularly relevant is enterprises providing specific "ethnic" goods and services (e.g. restaurants).[3]

Second, systematic sorting can arise due to *preferences* among the agents. In Becker's (1957) discrimination model, some—but not all—employers are unwilling to hire minority workers at the majority wage simply because they derive disutility from doing so.[4] Group-biased preferences among majority and/or minority managers would lead to a relationship between manager origin and workforce composition. It should be noted that preferences can be important on both sides of the recruitment decision, i.e. not only among managers but also among (potential) applicants. In fact, Giuliano et al. (2011) argue that worker preferences are the key factor for why black managers recruit less white applicants in the retail firm they study.

A third explanation is *informational asymmetries*. Theories of statistical discrimination often assume that managers find it more difficult to value merits and qualities among applicants with a different background than themselves. Managers may therefore prefer to hire workers who are similar to themselves if acquiring information is costly. Conversely, it is conceivable that workers have difficulties valuing managers with a background that differs from their own.

Fourth*, networks* could be important if they provide information on the availability and quality of workers and/or vacancies (see e.g. Montgomery, 1991; Calvo-Armengol and Jackson, 2004 and Granovetter 1973). There is a large and growing empirical literature suggesting that social networks are very important when workers get hired; Ioannides and Loury (2004) provide a survey. Individuals who live in the same residential area are more likely to work together (Bayer et al., 2008), parents help their children to find their first job (Kramarz and Skans, 2007), former co-workers share information about new jobs (Cingano and Rosalia, 2008), and immigrants with larger exogenous networks are more successful in the labor market (Munshi, 2003). A series of recent papers provide indirect evidence that ethnic labor market networks are important for black and Hispanic workers in the US (Heller-

[3] In terms of our empirical model, preferences that affect productivity (such as customer preferences or preferences among the existing stock of employees) could also be included in this category.

[4] Laboratory experiments suggest that people tend to favor/trust others with a similar ethnic background (e.g. Fershtman and Gneezy, 2001 and Ahmed, 2007). Field experiments point at substantial ethnic discrimination in the hiring procedure against African-Americans in the US (Bertrand and Mullainathan, 2004) and workers of Middle-Eastern descent in Sweden (Carlsson and Rooth, 2007). For quasi-experimental evidence of discrimination in actual recruitments, see Åslund and Skans (2007).

stein and Neumark, 2008; Hellerstein et al., 2008a, 2008b and 2009) and for immigrants in Germany (Dustmann et al., 2010).

It is noteworthy that most of this previous literature focuses on the effects of social contacts among employees, and not on the manager specifically. Managers are expected to use social contacts if they have higher utility of employing workers whom they know, or if informal hiring methods imply lower recruitment costs. In the hiring process, networks formed at professional arenas can be of particular importance. Managers may hire individuals of their own origin simply because they have met more people sharing their own background, e.g. at previous workplaces; i.e. what is sometimes labeled *baseline homophily*. This differs from *inbreeding bias*, which is the excess effect of similarity arising because individuals associate more with similar people given their available network (see e.g. Currarini et al., 2009 and McPherson et al., 2001).

2.2 Immigrants at the Swedish labor market

Since 1960, the number of first-generation immigrants living in Sweden has grown from 300,000 to more than one million. Today, the foreign-born constitute about 13 percent of Sweden's nine million residents and define most of the country's diversity in terms of origin or ethnicity.

As in many other western countries the labor market position of the immigrant population has deteriorated during the last thirty years. In the 1950s and 1960s, labor migration from the Nordic countries (especially Finland) and continental Europe dominated the inflow. Immigration then gradually shifted toward refugees and family reunification migrants, many times from developing and geographically distant countries (e.g. Chile in the 1970s, Iran from the 1980s, Somalia and former Yugoslavia in the early 1990s, and Iraq in the 1990s and 2000s.)

Even though natives on average perform better in the labor market than almost all groups of migrants, the great divider seems to be between those of western and non-western origin. In 2002 (in the midst of our observation period, see below), the employment rate among natives was 76.8 percent. The corresponding figure for EU/EES migrants was 69.3 percent, compared to 53.5 percent among those born outside Europe. Wage differences are smaller, but follow the same pattern: the average monthly (full-time) wage among natives was SEK 22,250 in 2002; for immigrants from non-European countries it was SEK 19,050, while EU migrants had an average wage almost identical to the one received by natives.[5] For a further discussion of these differences and their possible causes, see e.g. Eriksson (2010).

[5] Figures for employment and unemployment come from the Swedish labor force surveys. Wages are calculated from the LINDA database (see Edin and Fredriksson, 2000), which contains a three-percent representative sample of Sweden's population.

3 Data

Our primary source of data is a Swedish linked employer-employee database (RAMS) covering the period 1985 to 2005. By combining this data with additional administrative data sources we are able to track managers, employees and establishments over time and link each of these subjects to detailed information on individual demographic characteristics (gender, age, region of birth, education and place of residence) as well as to basic information about each establishment (location, industry and sector). Our main working dataset includes all newly recruited workers in establishments with less than 50 employees during the period 1997 to 2005 together with information on the immigration status of each worker and manager. The rationale for restricting the analysis to small and medium sized establishments is that it is more likely that the managers we identify are directly involved in the hiring and firing decisions in such establishments.[6]

Immigration

The main analysis aggregates the individuals by their country of birth into two categories: (i) workers of Western origin i.e. natives and immigrants from Western countries; (ii) immigrant workers of Non-Western origin. For convenience, we label the groups "Natives" and "Immigrants". This division is motivated by the main divider in terms of differences in labor market outcomes, and also with the public perception of "being foreign" (see e.g. Mella and Palm, 2009). We also present robustness checks using four groups ("Native", "Western", "Eastern Europe", and "Non-Europe").[7] In some cases, where the specification allows for it, we use more detailed information on the individual's country of origin and investigate the differential impact of immigrant managers on the hiring probabilities for own-country versus other-country workers (see Table A1 in Appendix A for a list of countries).

Managers and wages

To identify managers, and for data on wages, we use a register (Strukturlönestatistiken) containing occupation and wages for all employees in the public sector and for a large sample (drawn at the firm level) of employees in the private sector. The data cover 1997–2005. Occupational data are structured according to the Swedish Standard for Classification of Occupations (SSYK), which is based on international standards (ISCO-88). The first digit in the occupational code divides the data into ten major occupational levels

[6] Since previous research shows that segregation is most prevalent among small to medium sized establishments (see e.g. Åslund and Skans, 2010) our results are not necessarily representative for larger establishments. However, given that the data show that the median worker is employed in an establishment with 52 employees (2001), we cover a substantial part of the workforce.

[7] Using a finer grouping yields very few managers of some origin types.

based on the skill requirements and with a specific number for managerial positions. Using additional digits, we can also distinguish between top and middle managers. In addition to occupational information, we use information on ownership, which is available for all establishments run by self-employed managers in the data.

The manager definition is based on the following hierarchical criteria: (1) Owner of the firm; (2) Top manager; (3) Middle manager; (4) Highest wage.[8] In case there are multiple observations fulfilling the same criterion we use lower ranked criteria to identify the manager (e.g. the middle manager with the highest wage).[9] Although this strategy is likely to introduce some measurement error in the manager code it is reassuring that 83 (78) percent of the managers identified by their occupational status as top (middle) managers are also the highest wage earners in their establishment. We also report estimates separately by manager classification.

Because the sampling probabilities for the information on occupation and wages depend on firm size, we will have an under-representation of establishments belonging to smaller private firms in our full sample. We therefore present results separately for each firm size bracket (i.e. each stratum). But it should also be noted that although the sampling probabilities are small for small firms, many large firms have small establishments, and thus our final dataset covers approximately 30 percent of all small and medium sized private establishments (Table A2, Appendix).

Data on recruited workers

We define workers who received remuneration from the establishment in a given year, but not during any of the preceding five years as a newly hired worker. We disregard individuals earning below the 10th percentile of the overall annual earnings distribution in order to avoid classifying very loosely connected (i.e. working a few hours within the year) workers as new hires. We are primarily interested in recruitments within continuing plants and therefore require that the establishment existed in the preceding year. For the same reason we also classify an establishment as new (and remove it) if

[8] Cardoso and Winter Ebmer (2010) use this strategy when identifying managers from a similar dataset from Portugal.

[9] To increase sample size (particularly in the establishment fixed effect estimations), we use also information from population-wide data on estimated monthly wages (see for example Skans et al., 2009 for procedures). If an establishment is sampled at two separate points in time with the same manager, the same person is assumed to be manager also in the years in between (provided he/she is at the establishment). If the sample data identifies a manager in one year, the same person is assumed to be manager in all continuously preceding and following years in which he/she has the highest estimated wage (and where the establishment is not sampled).

more than 2/3 of the workforce changed (in either direction) from one year to the next.[10]

Data on recruitments of former colleagues

The long panel of individuals and establishments allows us identify each managers set of co-workers in previous jobs dating back to 1985. We use this to: (i) measure manager exposure to immigrant co-workers in the past by calculating the fraction of immigrants among all other workers at every (past) establishment the manager was employed by since the start of the data base in 1985; (ii) create a dataset containing all cases where newly hired worked with their hiring manager sometime in the past. In the latter case we also keep other employees in the last year the hire and the manager worked together. We require that the manager worked at the new establishment at least one year prior to the hire. We also restrict the analysis to individuals that worked together in an establishment with less than 100 employees. The reason is that we want it to be likely that the two agents interacted at the old workplace so that they were able to eliminate uncertainty about each other's productivity. We do not however put any restriction on the manager's occupational level at the past workplace. Thus he or she did not have to be a manager at the previous job. The only restriction is that the manager and the hire received compensation from the same establishment during the same year at some point from 1985 and up to the new hiring. [11]

4 Descriptive patterns: migrant status among managers and the newly-hired

Figure 1 presents the share of immigrant hires and managers, overall and by industry. Two important facts emerge. First, immigrants are underrepresented among managers in relation to their share of hires. Whereas 7 percent of the hired workers are immigrants the corresponding number for managers is less than 4 percent.[12] Second, there is systematic sorting across industries both among hired workers and managers; the representation of immigrants is for example much higher in "hotels and restaurants" and "transportation"

[10] When checking that new hires did not receive earnings from the same establishment in the past we use the original workplace identification number in order to make sure that the hires were really externally recruited.

[11] Because every workplace where a future manager is observed with a future hire will be blown up by its' size times the number of future managers, the size-restriction is also motivated by practical reasons.

[12] Official statistics from the 2007 labor force surveys confirm this picture for the overall population of employees: 6 percent of all native employees are managers whereas the figure is less than 2 percent for immigrants from non-EU/EES countries.

than in other industries. Industries with fewer immigrant managers also recruit a lower share of immigrant workers.

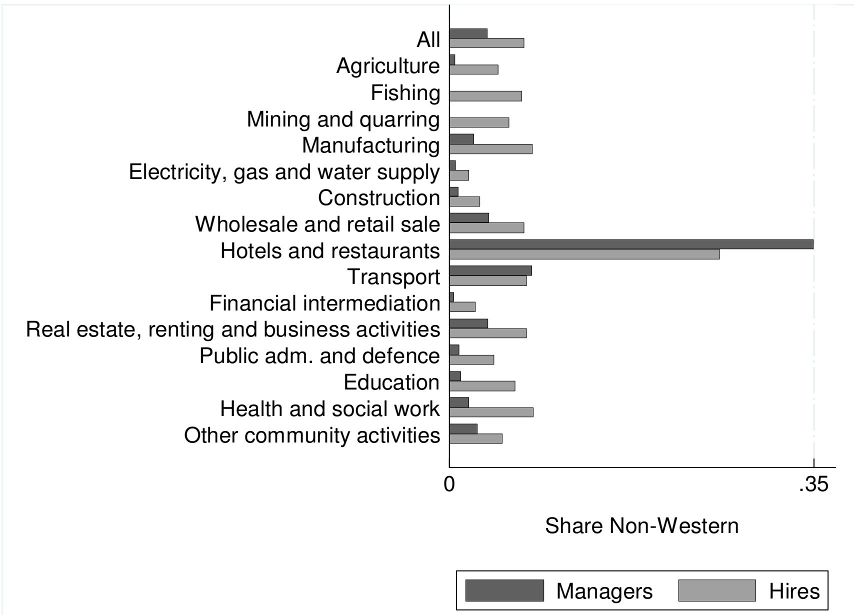

Figure 1 Share of immigrant managers and hired immigrants in different industries.

Table 1 shows that the sorting at the industry level also carries down to the establishment level. Establishments with immigrant managers (column 3) employ a substantially larger share of immigrants than establishments with native managers (column 2). This holds also for the newly hired and the magnitudes are striking: the share of immigrants hired under immigrant management is 43 percent, compared to 6 percent under native management. Thus, immigrant managers hire immigrant workers with a 7 times higher probability than native managers. The table also shows that immigrants manage smaller establishments, hire fewer individuals and operate in more immigrant dense municipalities than native managers. About half of the immigrant workers that are hired by immigrant managers are from other countries than the manager (not in table).

Table 1 Sample Statistics.

	(1)	(2)	(3)
Manager origin:	All	Native	Immigrant
Establishment size	24.3	24.5	17.9
Immigrant share in 5-digit industry by municipality	0.06	0.05	0.20
New hires/Year	4.95	4.99	4.02
[Sd]	[3.74]	[3.74]	[3.34]
Immigrant share in establishment	0.05	0.04	0.43
Immigrant share among new hires	0.07	0.06	0.43
Immigrant share among manager's former co–workers [A]	0.03	0.03	0.21
Manager type:			
Owner	0.09	0.07	0.51
Top Manager	0.14	0.14	0.08
Middle Manager	0.24	0.24	0.07
Highest wage	0.53	0.54	0.33
Manager origin			
Native (treated as "natives")	0.94	0.96	-
Western countries (treated as "natives")	0.04	0.04	-
Eastern Europe (treated as "immigrants")	0.01	-	0.40
Other (treated as "immigrants")	0.02	-	0.60
Number of observations	843,085	818,752	24,333

Notes: The sample consists of all establishments that hired at least one individual during the period 1997–2005. [A] The share of immigrants among former co-workers is calculated from all previous workplaces (from 1985) of each manager.

Turning to the manager characteristics we see that a much larger fraction of the immigrant managers are self-employed owners (51 percent vs. 7 percent of native managers), which is in line with the fact that self-employment is comparatively common among immigrants. A large share of both the native and the immigrant managers are identified by their wage. As previously discussed this is a potential concern since measurement error is likely to be prevalent in this group of managers. We therefore present the key results separately by type of manager in the analysis below.

5 Manager origin and hiring patterns

5.1 Empirical framework

Our aim is to identify the causal impact of manager origin on the probability that new hires are immigrants. To this end we estimate linear probability models of the following type:

$$H_{ijt}^{im} = \gamma M_{jt}^{im} + X_{jt}\beta + \varepsilon_{ijt} \qquad (1)$$

where H is an indicator for whether the hired individual i in establishment j in year t was immigrant (*im*); M is a dummy variable for immigrant manager; X is a vector of control variables and ε is the error term. In order to remove potential confounding factors we use several identification strategies, all implemented as variation of controls in the X-vector of equation (1). These are described below together with the results.

5.2 Baseline results

Table 2 presents results from estimation of equation (1).[13] The dependent variable is the probability that a hired worker is of immigrant origin and the covariate of interest is a dummy for whether the manager is immigrant. All specifications include year dummies to account for national trends in recruitment patterns and workplace size dummies in intervals of 10 employees. Other controls vary between columns.

Columns (1) to (3) of Table 2, add controls stepwise and the results confirm a very strong and robust correlation between the manager's and the recruited worker's immigration status. The specification presented in column 3 is quite rich: we compare similar firms in the same locations by including dummies (fixed effects) for each combination of year, municipality and industry. It is noteworthy that the 5-digit industry codes are quite detailed, which implies that we, for example, compare hiring patterns between different pharmacies (code 52310) or taxi businesses (60220) located in the same area (the average municipality has 30,000 residents). The estimate of 0.244 implies that an immigrant manager is 4–5 times as likely to hire an immigrant compared to a native manager. Thus, manager origin is highly corre-

[13] A logit specification instead of the linear probability model yields very similar results in the cases where we can test it.

lated with the origin of new hires even when establishments are both similar and located nearby.

In order to account for remaining unobservable confounders, column 4 includes the share of immigrants among the other employees (excluding the manager and new hires) at the establishment as a covariate. Workforce composition serves as a proxy for omitted establishment specific factors, e.g. customer preferences. Including this variable substantially reduces the coefficient, but the estimate is still large (0.123) and highly significant.[14] Hence, also when we compare two firms in the same industry, year and geographic area, and also take into account the demographic composition of the current workers, the probability that the newly hired is an immigrant is nearly three times as high if the manager is also of immigrant origin.[15] Replacing municipalities with neighborhood indicators ("SAMS") which on average contain a population 1000 inhabitants (of any age) does not alter any of the results.[16]

When interpreting these results it is important to note that they do not imply perfect, or even increasing, segregation over time. The reason is that the job durations are finite, and the sorting less than perfect. Thus, even if firms with an immigrant manager (and a high share of immigrant workers) tend to hire more immigrants, they will not necessarily end up having a homogenous workforce since some workers will leave, and some of the workers who replace those who leave will be natives.

Finally, in column 5 we allow the estimates to vary depending on the type of manager. The effect is substantially larger in owner-managed establishments than in establishments with other types of management, an issue we will return to below. But we do find large and significant effects for all types of managers; 5 percentage points implies that the probability of hiring an immigrant is twice as high in establishments that are managed by immigrants than in very similar establishments that are managed by natives and located in the same area.

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[14] We have also verified that the origin of the manager is significantly *more* important than the origin of other workers by re-estimating the model with the manager included in the share of incumbent immigrant workers.

[15] Interestingly, our estimated effect from the share of immigrant co-workers is not far off from what Dustmann et al. (2010) found for Germany in a similar specification.

[16] The estimates corresponding to column 2 (3) [4] of Table 2 are 0.321 (0.177) [0.102], all with standard errors around 0.01.

Table 2 OLS estimates of manager origin on origin of new hires.

	Dependent variable: Pr(Hired worker is immigrant)				
	(1)	(2)	(3)	(4)	(5)
Immigrant manager	0.369*** (0.007)	0.332*** (0.006)	0.244*** (0.007)	0.123*** (0.005)	
Owner					0.260*** (0.009)
Top manager					0.059*** (0.015)
Middle manager					0.036** (0.016)
Highest wage					0.052*** (0.007)
Establishment immigrant share				0.466*** (0.008)	0.415*** (0.008)
Fixed effects:					
Year	yes	yes	yes	yes	yes
Municipality × Year	-	yes	yes	yes	yes
Industry×Municipality×Year	-	-	yes	yes	yes
Mean dep. variable	0.07	0.07	0.07	0.07	0.07
Observations	843,085	843,085	843,085	843,085	843,085
R^2	0.059	0.087	0.251	0.272	0.274

Notes: Each column represents a separate regression. *,** and *** denote statistical significance at 10,5 and 1 percent level respectively. Standard errors robust for clustering at the establishment level are shown in parentheses. The share of immigrants is the share when the manager is excluded. This implies that the sample is restricted to establishments with 2-50 employees. Industry is defined at the 5-digit level. All regressions control for establishment size dummies of ten employee intervals and year fixed effects.

5.2.1 Comparisons within firms and establishments

Even though the specifications presented above are quite rich, one could still worry about remaining unobserved characteristics that are correlated with the origin of the manager. To test the validity of our results we perform a series of robustness tests addressing concerns about endogenous workplace selection of managers, shocks, and trends in hiring patterns. Results are presented in Table 3.

First, in order to remove (potentially year-specific) unobserved heterogeneity at the firm level we use data from firms with multiple establishments in the same location. Second, to handle unobserved factors at the establishment level, we also estimate specifications relying on establishments where management changed from native to immigrant (or vice versa) during our sample period. As shown in columns (3) and (4) of Table A3 both of these criteria basically exclude all owner-managed establishments.[17] To evaluate the importance of unobserved heterogeneity, we should therefore compare the estimates from the firm/establishment fixed effects models to a baseline specification excluding owners; this is done below. For comparison, columns (1) and (2) of Table 3 present the baseline estimates both with and without owners. As already indicated in Table 2, the average effect is smaller without owners (but still large and significant).

Firm fixed effects

We first analyze within-firm variations. We include dummies for the combination of year, firm, municipality, and industry of the establishment. The idea is to compare establishments with the same firm (and year) specific culture, involved in a similar production process and located in the same local labor market. Given the year interaction, this specification also handles unobserved time effects or trends at the firm level (e.g. changes in a firm's human resource policies). The specification also controls for differences in the composition in the workforce parametrically as in Table 2, column (3). We only include firms in the private sector since the distinction between establishments and firms is less clear in the public sector. As shown by Table A3 in appendix the establishments which fulfill these criteria are often found in consumer services, e.g. retailers and banks. The estimate of (0.044) presented in column (3) of Table 3 is very similar and not statistically different from that of the baseline model for the corresponding sample (i.e. without owners: 0.051).

Establishment fixed effects

Including establishment dummies in equation (1) means that we remove all fixed workplace characteristics, but in contrast to the firm fixed effects model above assume that confounding characteristics are fixed over time. Since the immigrant establishment share is a lagged dependent variable in this specification, we instead include the fraction of immigrant employees in other establishments in the same municipality, industry (5-digit) and year to capture trends in unobserved factors at the industry-location level.

The identifying sample only includes establishments where a native manager is replaced by an immigrant manager or vice versa. This reduces sample size substantially. To reduce the risk of false manager changes (particularly in the "highest wage" category) we restrict the sample to establishments that change manager origin only once during the observation period and we also

[17] Due to the organization of multi-establishment firms we are not particularly surprised finding no owner defined workplaces in this category. Furthermore there are very few changes in origin ownership (they account for 2 percent of the cases, see column 3) which may be partly explained by more persistence in this category and more informal ownership takeovers e.g. within families.

require that each manager is observed more than one year. This restriction removes about half of the changes in manager origin.[18]

The estimate reported in column (4) is close to those of previous columns. We have also re-estimated the model by management type (in column 5) and the results show that the effects are robustly similar over different types of (non-owner) managers and in all cases very similar to the estimates presented in Table 2, column 5.

Establishment level trends

An additional concern is workplace-specific trends in hiring patterns, which may generate a spurious relationship between the origin of the manager and the origin of newly hired workers. Establishments with an increasing share of immigrant hires may more often end up having immigrant managers, and increases in immigrant hires may lead to a change in manager origin. Both of these mechanisms would introduce an upward bias to our estimates.

Note though that the firm fixed effects model discussed above handles all trends (and other time varying shocks) that are shared within a firm and location. As a further robustness check we have, however, estimated models that include linear trends (centered on the year of the manager change for the interval [–6, 6]) for establishments that change manager. This sample differs from the establishment fixed effects sample in that it does not require hires both before and after the manager change in the same establishment. In column (6) the trend is allowed to differ depending on the direction of the manager change and in column (7) we also let the slope differ before-after the change (by direction, thus 4 slopes). The estimates in these columns are, again, in the vicinity of the baseline specification (0.051 and 0.056).

Overall, our robustness checks suggest that the impact of manager origin as captured by the industry fixed effects model gives a reasonable account of the underlying process since the estimates change very little when firm-location-year specific factors, establishment specific factors, or establishment level trends are accounted for.

[18] In the original sample there are 2,747 changes in manager origin. After imposing the restriction, 1,148 manager changes remains of which 145 are changes in “top manager” origin, 178 in middle manager origin and 651 changes in the origin of the individual with the highest wage. False manager changes can for instance arise in establishments with multiple managers. Imposing the restriction reduces this risk since we require the same manager to be observed for at least two subsequent years.

Table 3 Firm and establishment fixed effects.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Baseline ALL	Baseline No owners	Firm/Year FE No owners	Est. FE No owners	Est. FE No owners	Est. Trends No owners	Est. Trends No owners
Immigrant manager	0.123***	0.051***	0.044***	0.045***		0.051***	0.056***
	(0.005)	(0.006)	(0.014)	(0.013)		(0.019)	(0.020)
Top manager					0.062**		
					(0.028)		
Middle manager					0.044		
					(0.028)		
Highest wage					0.035**		
					(0.016)		
Establishment immigrant share	yes	yes	yes	yes[A]	yes[A]	yes	yes
Fixed effects:							
Year	yes	yes	yes	yes	yes	yes	yes
Industry×Municipality×Year	yes	Yes	yes	-	-	-	-
Firm×Industry×Municipality×Year	-	-	yes	-	-	-	-
Establishment	-	-	-	yes	yes	-	-
Trends							
2 trends	-	-	-	-	-	yes	yes
4 trends	-	-	-	-	-	-	yes
Observations	843,085	766,983	155,085	5,504	5,504	7,706	7,706
R^2	0.272	0.195	0.111	0.241	0.241	0.075	0.076

Notes : Each column represents a separate regression. *,** and *** denote statistical significance at 10,5 and 1 percent level respectively. Standard errors robust for clustering at the establishment level are shown in parentheses. Column (3) is restricted to private firms with multiple establishments. Column (4)–(7) is restricted to establishments that changed manager origin once during the period and where the same manager is observed for at least two years. All regressions include dummies for establishment size of ten employee intervals. The trends control for the *distance to change* (-6 – 6) and we allow the trends to vary with respect to the direction of the change in the 2-trend case (column 6), and also before and after the manager change in the (pooled) 4-trend models (column 7). [A] In these models the immigrant establishment share is a lagged dependent variable and is therefore not included. Instead we control for the fraction of immigrant employees in other establishments in the same municipality, industry (5-digit) and year.

6 Heterogeneity, wages, and separations

6.1 Heterogeneous effects

This section investigates whether the impact of worker-manager similarity varies by establishment, manager and worker characteristics. In order to increase the power we use the industry-fixed effects specification since the results in Table 3 above show that the main results are robust between models and specifications.

Table B1 presents estimates by establishment/firm size, sector, and industry. Given the differences between owners and other managers discussed above, we report the estimates separately for the full sample where owners are included (column 1) and for the more restricted sample where owners are excluded (column 2). Overall we find large, positive and significant effects of manager origin on the origin of new hires in establishments of all sizes, in almost all industries and in the private as well as in the public sector. Hence, similarity bias appear to be a general phenomenon, and not driven by a particular set of establishments.

There is however substantial heterogeneity in the magnitude of the impact. Manager origin matters more in small firms and small establishments and the effects are larger in the private than in the public sector. Here it is important to note that recruitments in the Swedish public sector are as decentralized and informal as in the private sector. Hence, there are no institutional barriers preventing public sector managers from hiring workers that are similar to themselves, and previous studies have also found evidence of ethnic discrimination in recruitments to public sector jobs in Sweden (see Åslund and Skans, 2007). In terms of industry, we find that the effect is strongest in the service sector such as in "Construction" and "Hotels and Restaurants", but also in "Education" and "Health and Social Work" which are predominantly in the public sector. An additional estimate which stands out is the manufacturing industry where origin does not seem to matter at all. By and large, the conclusions remain the same irrespectively if we include the owners or not. One exception is the strong effect in the transport industry, which is entirely driven by owner-managed firms (mostly taxi businesses).

The upper panel of Table B2 shows that the impact is somewhat stronger for less educated workers. The lower panel of the table moreover shows that in-group bias is more prevalent for male workers. Both of these results are in line with estimates from the network literature, where low educated and males often are found to rely more on networks and informal contacts than females and more highly educated workers (see e.g. Pellizzari, 2004). We have replicated the analysis for the establishment fixed effects model and found a similar pattern in terms of point estimates although the differences are statistically insignificant.

In Table B3 we split the immigrant sample into three groups: "Western Immigrants", Eastern-European Immigrants and Non-European immigrants. We estimate four linear probability models, one for the probability of hiring a worker of each of the four origin groups. In all cases we let the reference category be managers of the same origin as the category defining the dependent variable. All regressions control for the workplace composition of employees in the four groups. The results show that managers of all origins are significantly more likely to hire workers from their own group than from any other group, relative to other managers. Cross-group bias between natives and Western immigrants is relatively small, suggesting that our main division of the data provides a reasonable baseline. Immigrants from Western countries and from Eastern Europe face rather small differences between same-group managers and managers of other origin. In contrast, native workers as well as Non-Western workers have substantially lower hiring probabilities when the manager does not belong to the same group as themselves. Including the owners in the sample does not alter this pattern.

6.2 Starting wages

As discussed in Section 2 origin in-group bias may arise if there are productivity gains from employing individuals that share the same language or business culture. As long as there is some rent sharing, for example due to search frictions, productivity gains should affect starting wages even if the gains are purely match specific.[19]

Table 4 summarizes the main results from traditional wage equations with the log of starting wages as the dependent variable. The specifications include controls for age, age^2, education and gender as explanatory variables alongside time effects, establishment size, own origin and manager origin. The richest specification in the last column controls for 2-digit occupation codes, establishment fixed effects and previous wages.

Columns 1–3 suggest a modest but statistically significant wage premium for immigrants hired by an immigrant.[20] But column 4 shows that when we include previous wage among the control variables (to capture for unobserved skill differences), this association disappears.[21] Neither is there any

[19] Previous work suggests that productivity gains do affect starting wages, see e.g. Haefke et al., (2008).

[20] As expected, wages at entry are higher for males and are increasing in age, the level of education and in establishment size. Consistent with previous research we also find that immigrant workers receive lower starting wages, even when controlling for detailed level of occupation.

[21] Note that the final column of the table is estimated on a more restrictive sample. This is because wage information from the previous year is only available for a sample of employees

significant additional premium for sharing *country* of origin (and thereby typically culture and language) with the manager. As long as we believe that match-specific productivity should affect starting wages, these findings suggest that the similarity bias in recruitments is at least not fully driven by productivity considerations. It is also worth pointing out that the results indicate that immigrant managers on average hire immigrant workers with higher productivity (as indicated by their previous wages), given their other characteristics.

Table 4 Starting wages.

	Dependent variable: Log monthly starting wage			
	(1)	(2)	(3)	(4)
Both immigrant	0.021***	0.011*	0.011*	-0.006
	(0.007)	(0.005)	(0.006)	(0.011)
Both immigrants, same country [A]	--	--	0.001	0.026
			(0.014)	(0.023)
Immigrant manager	-0.005	-0.002	-0.002	-0.0004
	(0.004)	(0.004)	(0.004)	(0.004)
Immigrant hire	-0.063***	-0.049***	-0.049***	-0.021***
	(0.001)	(0.001)	(0.001)	(0.002)
Year fixed effects	yes	yes	yes	yes
Establishment fixed effects	yes	yes	yes	yes
Individual controls	yes	yes	yes	yes
Occupational dummies (2-digit)	-	yes	yes	yes
Wage in prev. job	-	-	-	yes
Observations	445,234	445,234	445,234	166,963
R^2	0.687	0.746	0.746	0.880

Notes: Each column represents a separate regression. *,** and *** denote statistical significance at 10,5 and 1 percent level respectively. Standard errors robust for clustering at the establishment level are shown in parentheses. All models control for age, age^2, education, gender, establishment size in ten employee intervals, worker origin, and manager origin. The sample includes both workplaces in the public sector and private sector. [A] See Table A1 for a list of countries.

6.3 Separations

Next we explore whether workers with a similar background as the manager is more or less likely to leave the establishment. Here, we use data on all establishments with less than 50 employees in the year 2000 and estimate the probability of job separations in a linear probability framework with year-establishment fixed effects to account for all (even time varying) establishment specific factors. In addition to establishment-year fixed effects (and

(since the wage data are sampled at the firm level, see the data section). Further analysis shows that the fall in the effect of both being immigrants comes from controlling for previous wage, whereas the (insignificant) rise in the estimate of being hired by a migrant from a similar country is driven by the change of sample. Estimating the effect separately by sector does not alter the results.

hence implicitly manager origin), the model accounts for the effects of worker origin through a dummy and (parametrically) for worker human capital and tenure.[22]

The main variable of interest is the interaction term between the origin of the manager and the worker, which measures the effect of similar origin on the probability of job separations. As shown by the first column in Table 5 workers are less likely to leave when they have a similar immigration background as the manager.[23] This is true also when the establishment fixed effects are interacted with tenure or co-worker skill group (college/no college). Evaluated at the sample mean, the estimate implies 26 percent higher probability of separation under dissimilar management.[24]

Unfortunately, we cannot distinguish between voluntary quits and firings. Results presented in the second panel of Table 5 however, show that the impact is unlikely to be driven by downsizing firms: restricting the sample to establishments which hired at least one new worker in the following year yields very similar results.

We have also estimated how the impact of similarity on the probability of a separation changes over the duration of an employment spell. Interestingly, these estimates reveal that the effect on separations is concentrated to the first year of employment. If the decreased job separation rate is due to imperfect information in the hiring stage, which is more of a problem when managers and workers are of dissimilar origin, this could explain the observed pattern. The fact that the separations happen relatively short after the time of the hiring is potentially reinforced by the Swedish employment protection legislation which allows for flexibility use of temporary workers, but offer strict protection of tenured workers (see e.g. OECD, 2004).[25]

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[22] All control variables show the expected signs. The probability to quit decreases with age and tenure, male workers are more likely to quit than females.

[23] To harmonize the analysis with the analysis of hiring patterns we focus on establishments where someone actually leaves the establishment. Using the full sample attenuates the results but signs are identical.

[24] Splitting the sample according to manager origin shows that this same origin bias is primarily driven by immigrant workers being more likely to quit under western management.

[25] All employees with less three years of tenure are being employed on an open-ended contract.

Table 5 Estimated effects on the probability of separation.

	Dependent variable: Pr(Quit)		
	(1)	(2)	(3)
	All	Same hiring year	Same skill group
A: All establishments			
Worker and manager immigrants	-0.037*	-0.054*	-0.037*
	(0.020)	(0.033)	(0.023)
Worker immigrant	0.039***	0.037***	0.039***
	(0.004)	(0.006)	(0.004)
Manager immigrant	- Captured by establishment fixed effects -		
Mean dep. variable	0.145	0.145	0.145
Observation	435,142	435,142	435,142
R^2	0.159	0.457	0.226
B: Establishments that hired next year:			
Worker and manager immigrants	-0.043**	-0.053	-0.043**
	(0.021)	(0.031)	(0.023)
Worker immigrant	0.038***	0.036***	0.036***
	(0.004)	(0.006)	(0.004)
Manager immigrant	- Captured by establishment fixed effects -		
Mean dep. variable	0.142	0.142	0.142
Observations	380,002	380,002	380,002
R^2	0.143	0.443	0.205
Tenure dummies	yes	yes	yes
Individual controls	yes	yes	yes
Year specific fixed effects:			
Establishment	yes	yes	yes
Establishment×Hiring Year	-	yes	-
Establishment×Skill group	-	-	yes

Notes: *,** and *** denote statistical significance at 10,5 and 1 percent level respectively. Standard errors robust for clustering at the establishment level are shown in parentheses. The dependent variable indicates whether the individual quit from the establishment within 2000. The sample is restricted to establishments where at least one individual quit in the specific year. Panel B is restricted to establishments that in addition hired at least one worker the following year. All regressions include establishment fixed effects, individual human capital controls (gender, education dummies, age and age^2) and tenure dummies (0, 1-2, 3-4, 5-10, >10 years). In column (2) we only compare individuals hired in the same year (all workers hired before 1986 belong to the same group) and in column (3) we compare individuals belonging to the same skill group (college/no college).

7 The importance of past interactions

In this section we explore the role of interactions at previous jobs. Since the labor market is segregated (see e.g. Åslund and Skans, 2010), it is not surprising that immigrant managers on average have a much higher fraction of immigrants among their previous co-workers than native managers (see Table 1 above). Thus, immigrant managers' professional networks are likely to include a higher fraction of immigrant workers on average. Previous research (e.g. Granovetter, 1995) does suggest that former colleagues account for a large portion of jobs found through personal contacts.[26] If these (or other similarly segregated) networks are important sources of information for future recruitments, then segregation may self-propagate. Our data include a long panel of individual employment histories, which allows us to analyse this question in more detail.

We construct a variable capturing the share of each manager's former co-workers (from 1985) that were immigrants and re-estimate the models including this variable. For simplicity, we only show the results from the establishment fixed effects model of Table 3 since this is the most conservative specification. The estimates reported in Table 6, column (2) indicate that the probability of hiring an immigrant is higher the higher the share of immigrants among the former co-workers. An increase in the share of immigrants among the manager's former co-workers by 30 percentage points has the same impact on hiring patterns as a change of manager origin from native to immigrant. Letting the impact of former co-workers vary by manager origin suggests that the association between hiring patterns and past co-workers is the same for native and immigrant managers although the precision is poor.[27] Hence, the estimates suggest that managers (irrespective of origin) hire more immigrants if they have worked with more immigrants in the past.

[26] See also Cingano and Rosalia (2008) who show that the employment rate of former co-workers significantly shortens unemployment duration for recently displaced workers.

[27] The industry fixed effects models lead to the same conclusions and suggest that there is no differential impact of former co-workers depending on the background of the manager. These results are available upon request.

Table 6 Fixed effect estimates of the effect of past share of immigrant co-workers.

	Dependent variable: Pr(Hire is immigrant)		
	(1)	(2)	(3)
Immigrant manager	0.042* (0.022)	0.039* (0.022)	0.039* (0.022)
Immigrant share among manager's former co-workers		0.114* (0.060)	0.115 (0.075)
Immigrant manager × immigrant co-workers			-0.004 (0.134)
Establishment size dummies	Yes	Yes	Yes
Immigrant share in industry by mun.	Yes	Yes	Yes
Establishment fixed effects	Yes	Yes	Yes
Observations	68,813	68,813	68,813
R^2	0.187	0.194	0.194

Notes: Each column represents a separate regression. *,** and *** denote statistical significance at 10,5 and 1 percent level respectively. Standard errors robust for clustering at the establishment level are shown in parentheses. The specification is the same establishment fixed effects specification as in column (4) of Table 3. We apply the same sample restrictions to the data, hence, we include establishments that changed manager once during the period and where the same manager is observed for at least two years. Columns (2) and (3) include the share of each manager's former co-workers calculated using the working history from 1985.

We further analyse how managers behave when they actually do recruit former co-workers. This setting isolates within-network differences at the same time as information asymmetries should be less of a concern (assuming that work-related information is revealed when working together). We use the sample of past co-workers and estimate a model predicting the probability that each colleague (at the old workplace) is recruited.

The model includes a fixed effect for each establishment where a manager has worked using data up to 20 years back in time. An important feature of the model is that we, by the inclusion of the fixed effect at the old establishment, capture all unobserved differences that are shared between workers at the past workplace (*k*) at the time of past interaction (*s*). Our sample restrictions require variation within each fixed effect group (*k*,*s*), which in practice means that someone (but not everyone) has to be hired from each past establishment, and also that there are workers of different origin present. The data therefore include all workers at past establishments meeting these criteria. Formally, we estimate the following model:

$$E_{i,ks,jt} = \gamma M^{im}_{ks,jt} + \lambda W^{im}_{i,ks,jt} + \delta M^{im}_{ks,jt} \times W^{im}_{i,ks,jt} + X_{i,ks,jt}\beta + \eta_{ks} + \varepsilon_{jt} \qquad (2)$$

where E is a dummy taking the value one if worker i in establishment k in year s is hired to establishment j in year t. W is an indicator for the origin of the worker. M is an indicator for an immigrant manager in establishment j at time t (who worked in establishment k at time s) and η is the fixed effect for each set of previous coworkers (i.e. establishment×year).

The coefficient of interest δ measures whether similarity with the (future) manager increases the probability to be hired relative to other workers in the (past) establishment.[28] A positive estimate of δ would indicate that managers are more likely to recruit those of the former co-workers who are of similar origin. By contrast, if the similarity bias is driven by informal hiring and origin segregation in the networks then we expect there to be little remaining impact of origin similarity when accounting for the differential composition of these networks (former co-workers). Hence, we are able to infer whether the impact of similarity remains when information asymmetries are substantially reduced. Among all new hires in our sample during the period 1997–2005, 4 percent (around 39,000) had worked at the same establishment as the recruiting manager for at least one year in the period from 1985 and up to the hire.

The results, presented in Table 7, suggest that workers are more likely to be hired by former colleagues, now managers, if they share the same broad origin. The effect is strongest when immigrant workers and managers are from the same source country (as indicated by the coefficient on "Same immigrant source country") but there are also significant cross-group effects. Note, however, the zero coefficients on "Worker immigrant" suggesting that there is no difference under native management.

It is reassuring to find that the results hold if we limit the comparison to former colleagues of similar skill as the individual(s) actually recruited (column (3)). Correlations between manager ethnicity and the skill composition of workers of different origin in the past workplace do thus not drive the results.

[28] The strategy is similar to the network studies of Bayer et al (2008) who look at whether individuals are more likely to work together if they live in the same block than individuals who live in the same census tract but not in the same block. The analogue is that we treat the previous workplace as the census tract and estimate whether a worker who belongs to the same ethnicity (block) is more likely to follow the manager than a worker belonging to the same previous establishment (tract) but not to the same ethnicity. Kramarz and Skans (2007) use a similar strategy to study whether parents are more likely to hire their own children than other individuals who belong to the same cohort and graduate from the same school, class, and field of study.

Table 7 Fixed effects estimates of the effect of origin among former colleagues

	Dependent variable: Pr(Follow from old workplace)		
	(1)	(2)	(3)
	All	All	Same skill group
Worker and manager immigrants	0.048***	0.020***	0.021**
	(0.007)	(0.007)	(0.010)
Same immigrant source country	-	0.098***	0.111***
	-	(0.015)	(0.020)
Worker immigrant	-0.002	-0.002	-0.0007
	(0.003)	(0.003)	(0.004)
Manager immigrant	– Captured by establishment fixed effects –		
Establishment fixed effects	yes	yes	yes
Establishment×education fixed effects	no	no	yes
Observations	646,036	646,036	406,784
R^2	0.209	0.209	0.134

Notes: Each column represents a separate regression. *,** and *** denote statistical significance at 10,5 and 1 percent level respectively. The sample includes all workers at establishments where at least one individual followed the manager to a new workplace and the outcome variable is taking the value of one if the worker is the "follower" and zero, otherwise. The last column includes establishment×education fixed effects where "education" is a dummy taking a value of one if the individual has at least some college education, and zero otherwise. A worker was hired by an immigrant (native) former co–worker (now-managers) in 751 (37,527) cases.

8 Discussion

Our analysis provides strong evidence that manager origin does matter for who gets hired. Establishments disproportionately often hire workers who share background with the manager. This pattern holds in a large set of specifications, utilizing variation in several dimensions to control for observed and unobserved characteristics and trends, suggesting that we actually capture a causal effect of manager origin. These results are consistent with racial and ethnic hiring biases documented in single firm studies by Giuliano et al. (2009) and Bandiera et al. (2009). Overall, these results indicate that lack of access to "key players" at the labor market can explain some of the difficulties faced by workers of Non-Western descent. Increasing the representation of immigrants in managerial positions could therefore improve other immigrants' employment prospects. Hence, promoting the careers of already employed immigrants may be an important complement to current integration policies which nearly exclusively focus on getting the non-employed into work.

Although it is hard to pin down exactly *why* managers are more likely to hire their origin peers, our results do leave some suggestive evidence of the mechanisms at work. Two observations speak against the possibility that managers hire similar workers for efficiency reasons. The first is that immigrant managers are also more likely to hire immigrants of other descent than their own, whereas efficiency gains are likely to come with e.g. a common language. Furthermore, we find no evidence that similarity affects entry wages. Thus, a pure productivity story can only explain the results if the employer extracts all match-specific productivity gains.

Instead we interpret our findings as favoring an explanation based on networks or information asymmetries. We find that immigrant workers are more likely to be hired by managers with a higher proportion of immigrants in their network of former co-workers. There is moreover no impact of ethnic similarity when native managers, who make up the vast majority of managers, recruit among previous co-workers. This suggests that native managers are unbiased in a setting where information asymmetries are reduced through previous interactions. An alternative interpretation is that they are still biased, but their behavior is counteracted by immigrant workers being more inclined to follow former co-workers. Since this latter explanation requires that the two effects cancel out exactly, we lean towards the former.

Our results regarding separation rates indicate that at least parts of the effects are driven by actions taken by the managers rather than the workers. It seems unlikely that Non-Western immigrants (who have the lowest job finding rates) would be willing to leave voluntarily due to the origin of their manager, especially since the separations only occur as long as workers are unsheltered by employment protection legislation. Another finding implicating that manager behavior matters is that the impact on recruitment patterns is larger when the manager has a higher financial stake in the outcomes, e.g. at firms in the for-profit sector and in owner-managed establishments. The latter finding is in some contrast to Bandiera et al. (2009) who conclude that stronger financial incentives for managers reduce the ethnic bias in their decisions.

This difference may partly stem from the fact that our data are drawn from a much more general labor market. Bad hiring decisions may carry a lower cost at the high turnover jobs studied previously. This difference can also be reinforced by a higher union coverage and more stringent employment protection legislation in Sweden than in the US or the UK. It is also conceivable that the uncertainty about worker productivity is greater at the overall labor market than e.g. in the case of fruit pickers studied in Bandiera et al. (2009). A conjecture consistent with the results is that in-group bias may be a pure consumption good under perfect information, but primarily a tool for reducing information asymmetries when uncertainty about worker productivity is high and when recruitments are difficult to reverse.

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Appendix A Descriptives

Table A1 Countries and regions.

Region	Countries included
"Natives"	
Native	*0- Sweden*
Western	*1-Finland*
	2-Denmark
	3-Norway+ Iceland
	4-GB + Ireland
	5-Germany
	6-Mediterr. Europe (Greece + Italy + Spain + Portugal + the Vatican + Monaco + Malta + San Marino)
	7-Other Europe (Andorra + Belgium + France + Liechtenstein + Luxemburg + the Netherlands + Switzerland + Austria)
	8-US + Canada
"Immigrants"	
Eastern Europe	*9-Bosnia-Herzegovina*
	10-Former Yugoslavia (Yugoslavia + Croatia + Macedonia + Slovenia)
	11-Poland
	12-The Baltic states (Estonia + Latvia + Lithuania)
	13-Eastern Europe 1 (Rumania + The former USSR + Bulgaria + Albania)
	14-Eastern Europe 2 (Hungary + The former Czechoslovakia)
Non-Western, Non-Europe	*15-Mexico and Central America*
	16-Chile
	17-Other South America (Argentina + Bolivia + Peru + Colombia + Uruguay + Ecuador + Guyana + Paraguay + Surinam + Venezuela)
	18-African Horn (Ethiopia + Somalia +Sudan + Djibouti),
	19- North Africa + Middle East (Lebanon + Syria + Morocco + Tunisia + Egypt + Algeria + Israel + Palestine + Jordan + South Yemen + Yemen + the United Arab Emirates + Kuwait + Bahrain + Qatar + Saudi Arabia + Cyprus)
	20- Other African (all African countries not included elsewhere)
	21-Iran
	22-Iraq
	23-Turkey
	24-East Asia (Japan + China + Korea + Hong Kong + Taiwan)
	25-Southeast Asia (Vietnam + Thailand + the Philippines + Malaysia + Laos + Burma + Indonesia + Singapore)
	26-Other Asia (Sri Lanka + Bangladesh + India + Afghanistan + Pakistan + Brunei + Bhutan + Kampuchea + the Maldives + Mongolia + Nepal + Oman + Sikkim)
	27-Oceania (Australia + New Zealand etc…)

Table A2 Share of total number of establishments in the economy by establishment size.

Sample:	All	Public	Private	Private multiple
1-9	0.37	0.93	0.35	0.48
10-49	0.50	0.98	0.27	0.60
50-199	0.75	0.98	0.53	0.69
200-499	0.86	0.97	0.81	0.88
500-	0.96	0.98	0.95	0.98

Notes: The table shows the number of establishments in our sample as a share of the total number of establishments in the economy. The sampling is stratified by firm size with the sampling probabilities 3%, 12%, 41%, 70% and 100% for the firm size intervals reported in the table respectively. The share of establishments with more than 500 employees and the share of public establishments should be 100%. However since the figures are obtained from the data by dividing the number of establishments where we have information on wages with the total number of establishments in the nation wide data some of the establishments are missing for other reasons than sampling.

Table A3 Sample statistics for robustness specifications.

	(1)	(2)	(3)	(4)
Sample	All	All No owners	Firm FE sample	Est.FE sample
Establishment size	24.3	25.6	24.6	25.5
Immigrant share in 5-digit industry by municipality	0.06	0.05	0.05	0.10
New Hires/Year	5.0	5.2	4.8	6.5
[Sd]	[3.74]	[3.77]	[3.59]	[4.61]
Manager Type				
Owner	0.09	-	0.00	0.02
Top Manager	0.14	0.15	0.20	0.16
Middle Manager	0.24	0.26	0.38	0.21
Highest wage	0.53	0.58	0.41	0.61
Industry				
Agriculture, hunting and forestry	2.1	0.9	1.1	0.3
Fishing	0.0	0.0	0.0	0.0
Mining and quarrying	0.0	0.0	0.0	0.0
Manufacturing	3.1	2.9	5.1	1.3
Electricity, gas and water supply	0.6	0.7	0.4	0.0
Construction	3.9	3.0	4.5	0.6
Wholesale and retail sale	14.2	13.9	39.7	18.0
Hotels and restaurants	3.0	1.6	3.0	2.9
Transport, storage and communication	5.3	4.5	5.1	1.7
Financial intermediation	3.5	3.8	16.4	2.0
Real Estate, renting and business activities	6.7	6.4	12.2	4.3
Public administration and defense	4.8	5.3	-	4.5
Education	17.1	18.6	3.0	13.3
Health and Social work	28.0	31.6	5.8	46.1
Other community, social and personal service activities	6.7	6.9	3.8	5.1
Observations	843,085	766,983	155,085	5,504

Notes: Column (1) reports sample characteristics for the overall sample of hires whereas column (2), (3) and (4) shows the characteristics for the samples used in the robustness specifications reported in Table 3 in the main text. The level of observation is the individual and hence the table shows the fraction of new hires in each industry. The four samples correspond to 95,910 (1), 68,307 (2), 17,706 (3) and 372 (4) establishments respectively.

Appendix B Additional results

Table B1 Heterogeneity: Size, industry and sector.

	Dependent variable: Pr(Hired worker is immigrant)			
	All		No owners	
	estimate	se	estimate	se
Main effect	**0.123*** **	**0.005**	**0.051*** **	**0.005**
Establishment size				
2-9	0.244***	0.008	0.078***	0.015
10-19	0.121***	0.008	0.048***	0.010
20-29	0.068***	0.010	0.056***	0.012
30-39	0.049***	0.013	0.032***	0.012
40-49	0.053***	0.013	0.047***	0.013
Sector				
Public	0.038***	0.007	0.038***	0.007
Private – single establishment firm	0.218***	0.013	0.038	0.034
Private – multiple establishment firm	0.056***	0.013	0.056***	0.013
Firm size				
Private – firm with less than 10 workers	0.308***	0.012	0.183***	0.050
Private – firm with 10-49 workers	0.194***	0.013	0.121***	0.032
Private – firm with 50-99 workers	0.071	0.047	0.080	0.048
Private – firm with 100+ workers	0.046***	0.012	0.050***	0.013
Industry[(1)]				
Agriculture, hunting and forestry	0.212***	0.059	0.101	0.066
Manufacturing	0.038	0.046	-0.008	0.051
Construction	0.279***	0.042	0.161***	0.055
Wholesale and retail sale	0.157***	0.013	0.048***	0.012
Hotels and restaurants	0.209***	0.012	0.113***	0.028
Transport, storage and communication	0.240***	0.017	0.026	0.023
Financial intermediation	0.018	0.018	0.020	0.017
Real Estate, renting and business activities	0.163***	0.020	0.045*	0.026
Compulsory social security	0.073***	0.027	0.075**	0.027
Education	0.072***	0.016	0.075***	0.016
Health and Social work	0.042***	0.008	0.037***	0.008
Other community, social and personal services	0.085***	0.019	0.042**	0.020
Establishment size dummies	yes	yes	yes	yes
Establishment immigrant share	yes	yes	yes	yes
Industry×Municipality×Year fixed effects	yes	yes	yes	yes

Notes: *,** and *** denote statistical significance at 10,5 and 1 percent level respectively. Standard errors robust for clustering at the establishment level are shown in parentheses. The table reports results from six integrated regressions (per outcome). The specification is the same industry fixed effects specification as in the last two columns of Table 2. All regressions control for the share of immigrants at the establishment, establishment size dummies of ten employee intervals and industry (5-digit)×municipality×year dummies. [(1)]Estimates for industries with less than 1 percent of all hires are not shown in the table; see the first column of Table A4.

Table B2 Heterogeneity: Skill group and gender.

	Dependent variable: Pr(Hired worker is immigrant)			
	All		No Owners	
	estimate	se	estimate	se
Education:				
College	0.080***	0.006	0.036***	0.007
No college	0.121***	0.006	0.056***	0.008
Gender:				
Male	0.237***	0.007	0.092***	0.010
Female	0.061***	0.006	0.038***	0.006
Establishment size dummies	yes	yes	yes	yes
Establishment immigrant share	yes	yes	yes	yes
Industry×Municipality×Year fixed effects	yes	yes	yes	yes

Notes: see Table B1

Table B3 Heterogeneity: Origin groups (no owners).

	Hired worker's origin			
	(1)	(2)	(3)	(4)
	Native	Non-native Western	Eastern European	Immigrant, Non-European
Manager's origin				
Native	-	-0.013***	-0.023***	-0.056***
	-	(0.002)	(0.004)	(0.008)
Western countries	-0.020***	-	-0.021***	-0.050***
	(0.004)	-	(0.005)	(0.008)
Eastern Europe	-0.044***	-0.008*	-	-0.039***
	(0.008)	(0.004)	-	(0.010)
Non-W., Non-Europe	-0.068***	-0.008	-0.016**	-
	(0.009)	(0.003)	(0.006)	-
Establishment size dummies	yes	yes	yes	yes
Establishment immigrant share	yes	yes	yes	yes
Industry×Municipality×Year fixed effects	yes	yes	yes	yes
Observations	766,983	766,983	766,983	766,983
R^2	0.195	0.157	0.158	0.171

Notes: Each column represents a separate regression. *,** and *** denote statistical significance at 10,5 and 1 percent level respectively. Standard errors robust for clustering at the establishment level are shown in parentheses. The dependent variable indicates whether the new hire belongs to each of the groups in column (1)-(4) and the specification is the same industry-fixed-effects specification as in the last two columns of Table 2. All regressions control for establishment size dummies of ten employee intervals as well as for industry (5-digit)×municipality×year dummies. Each regression also controls for the share of immigrants that origin from the group corresponding to the dependent variable.

Essay 3: Manager Impartiality? Worker-Firm Matching and the Gender Wage Gap♦

1 Introduction

The underrepresentation of women in certain occupations and manager positions is often cited as one reason for the persistent gender labor market disparities observed in many countries. To improve women's outcomes in the labor market, several countries have recently taken action to encourage gender parity in top positions, for example, by imposing mandated gender quotas at the highest levels of public and private organizations.[1]

The aim of this paper is to examine whether and why workplaces with female managers have narrower gender wage gaps. Despite a substantial amount of theoretical work on why female representation among managers could be important, there is very little empirical evidence quantifying the effects of the manager's gender on the outcomes of their female employees.

The gender differences in the labor market are still substantial throughout the industrial world (Polachek and Xiang, 2009). Findings suggest that occupational sorting accounts for around half of the estimated male-female wage gap and that there is a "glass ceiling" preventing women from moving up the career ladder (Albrecht et al., 2003, Arulampalam et al., 2005, Bayard et al., 2003). An increased share of women in decision-making positions may both directly help close the gender wage gap and narrow the wage differences between male and female sub-ordinates through multiple channels. Female managers may, for example, have better information about women's productivity and/or stronger social ties to other women. They may, moreover, reduce discriminatory behavior, increase women's human capital through mentoring, or serve as important role models (Becker, 1971, Athey et al., 2000, Akerlof and Kranton, 2000).

Previous empirical research on the importance of the manager's gender on employee wages has mainly used cross-sectional data. Hence, they generally

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[1] Norway was the first country to adopt mandated gender quotas in corporate boards in 2003. The law required at least 40 percent of the directors of public firms to be women in 2005. A similar law came in effect in France in 2010.

suffer from the difficulty in separating the influence of female managers from other factors that could motivate gender differences in pay, such as differences in unobserved worker productivity or firm practices.[2] A few more recent studies have attempted to address this potential identification problem using longitudinal data. In particular, Bell (2005) and Cardoso and Winter-Ebmer (2010) use establishment fixed effects models to reduce bias from static unobserved establishment characteristics. However, their approach is limited by the fact that they are unable to control for productivity-related changes among the employed.

Because the employment relationship is influenced by the choice of workers and employers, such compositional effects are potentially important. As decision-makers of the firms, managers may not only directly affect the wages of the current workers but also affect the identity of the workers selected into employment (Bandiera et. al., 2009). Female managers may, for example recruit higher quality, non-managerial female workers because they have better information about other women's productivity. The most qualified female workers may also enter female-led firms due to self-selection, for example, if career-oriented women anticipate better opportunities in female-led firms. In any case, manager changes could be correlated with simultaneous changes in workforce composition, which in turn may lead to a positive correlation between women-led firms and female wages.[3]

The key contribution of this paper is that I am able to control for invariant sources of heterogeneity across workers, such as their ability, when investigating the role of female managers for employee wages. In addition, the paper sheds further light on the potential mechanisms at work, by examining how female managers affect the gender- and skill composition of new hires and how the wage effects of having female managers vary over the employment spell.

The empirical approach uses administrative longitudinal matched employee-employee data from Sweden. The data contain high-quality wage measures and detailed occupational data for a large sample of workers over more than 20 years, which enables me to calculate and account for an individual's experience and job tenure. Importantly, I can also account for time-varying proxies for establishment heterogeneity as well as establishment fixed effects to capture unobserved workplace attributes that could influence

[2] Laboratory experiments, for example, suggest that women are more risk-averse and less willingness to compete and seek challenges compared to men (see Bertrand, 2011, for a summary of these findings). Bertrand et al (2010) empirically demonstrate that high-skilled women sort into family-friendly work environment with shorter and more flexible hours. Due to compensating differentials, these environments may also pay less on average.

[3] The only paper that I am aware of that accounts for worker heterogeneity when analyzing the role of managers is Bandiera et al. (2009) study how social connections between managers and employees affect productivity in a British fruit-picking company. However, they focus on similarity with respect to nationality and not gender per se.

both the gender composition of managers and the establishment gender wage gap, even in the absence of a causal influence of female managers.

Descriptive analysis suggests that the gender wage gap within non-managerial occupations decreased by one percentage point between 1996 and 2008. During the same period, the proportion of female managers increased by more than 10 percentage points. Despite this increase, women remained under-represented among managers, in particular among executives, and on high-paying positions in the private sector.

A more formal analysis of the association between a higher share of female managers and the gender wage gap shows that female workers receive 1.4 percent higher wages in women-led compared to male-led establishments. Male workers, in contrast, receive 3.6 percent lower wages, giving a 5 percent narrowing of the total gender wage gap in women-led firms. This result is robust to the inclusion of establishment fixed effects, hence unobserved establishment characteristics that are fixed over time are not driving the relationship.

However, most of the association goes away when accounting for worker fixed effects, suggesting that sorting on unobserved skills, such as unobserved human capital or effort, is an important determinant of the establishment gender wage gap. These findings indicate that the importance of female managers primarily goes through the selection of employees that join or leave the establishment's workforce. In order to shed further insights to this conjecture, I examine how the manager's gender affects the composition of new hires. More specifically, I use pre-determined measures of worker skills derived using the estimation methods developed by Abowd et al. (1999) and relate the skill composition of hires to the gender composition of managers. The analysis suggests that female managers do not hire more women per se, but they do hire women with higher (unobserved) portable earnings capacity.

Finally, there is also a small entry-wage premium in female-led establishments that diminishes with tenure. In addition wage growth is higher for women who themselves are in minority but who are supervised by female middle managers. Nevertheless, the overall results do suggest that skill sorting is more important than differential treatment of equally productive workers.

The rest of the paper is outlined as follows. Section 2 provides an overview on the literature on gender management and wages; Section 3 describes the data. Section 4 first provides a descriptive analysis of gender wage differences and women-led establishments in Sweden, followed by a more formal analysis of the association between female managers and the gender wage gap. Section 5 investigates the impact of female managers on the composition of hires and Section 6 concludes.

2 Background and related literature

There are several potential mechanisms that explain why female-led firms may have systematically narrower gender wage gaps than firms led by men. First, theoretical models suggest that taste-based or statistical discrimination can give rise to lower female wages in male-led firms (Becker, 1971; Lazear and Rosen, 1990). If male managers have less information about women's productivity or systematically assign women to less favorable positions, the gender wage gap is expected to narrow when the share of female managers increase.[4]

Second, women may obtain more (or better) mentoring by female managers, either because they find it easier to establish mentoring relationships with other women or because they receive better mentoring from more similar supervisors (Athey et al., 2000). Empirical studies have to a large extent focused on mentoring relationships in academia. Neumark and Gardecki (1998) and Hilmer and Hilmer (2007) find no evidence of positive effects from gender similarity between economics PhD students and their advisors. However, in a recent evaluation of a randomized trial of a mentoring program for female economists, Blau et al. (2010) demonstrate an increased number of publications and successful grants among women who received mentoring relative to those who did not.[5]

More recently, a growing literature has emphasized the role of social networks for gender-driven labor market outcomes. Social connections with the manager could affect employment and wages because networks may disseminate information about jobs and job candidates and may also affect how workers are assigned to jobs and ranks within the workplace. When the representation of female managers increase partially segregated networks could thus help women by pulling them into better jobs than they otherwise would have obtained in a comparable male-led firm (Bell, 2005).[6]

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[4] Men and women who do the same job for the same employer receive similar wages in Sweden, in other Scandinavian countries as well as in the US. Differential treatment of men and women doing equal work for the same employer is thus likely to be of second order (Meyersson Milgrom et al., 2001). Still, gender-related differences in job assignment and promotions could be important factors for the observed gender wage gap. The empirical literature offers mixed evidence on gender differences in promotions. Some studies find that women have lower promotion rates than observably identical males (c.f. Olson and Becker, 1983; Blau and DeVaro, 2007, Cobb-Clark, 2001, Ransom and Oaxaca, 2005), while others find no relationship or a reversed relationship between gender and promotions (Barnett et al., 2000).

[5] Bettinger et al., (2005) look at college faculty composition in Ohio and find that female faculty enhanced the outcomes for female students, which supports a possible role model effect.

[6] Several studies have found that a large percentage of jobs are found through social contacts. In summary, these surveys find that between one-third and two-thirds of workers find their jobs through friends, relatives, and other social contacts. See Ioannides and Datcher Loury (2004) for an overview of this literature. Bandiera et al., (2009) show that workers that have social connections to the manager are assigned to better jobs.

Laboratory experiments and observational studies also point toward the importance of gender differences in preferences and attitudes related to, for example, competitiveness and the willingness to negotiate (Bertrand, 2011, provides an overview).[7] Consistent with this literature Säve-Söderberg (2009) finds that women submit systematically lower wage bids than men and are also offered lower wages in Sweden. Even if this and similar studies do not explicitly consider how negotiations vary with the gender of the negotiating parties, a higher share of female managers could potentially help neutralize the gender differences in negotiation, if negotiations between similar parties are more efficient (Kolb and McGinn, 2008).

The literature also discusses the role of gender-related norms and social status (Akerlof and Kranton, 2000, Goldin, 2002). An increasing number of women who break traditional gender roles on the modern labor market can encourage other women to invest in similar career paths. If women therefore have higher motivation under same-sex managers, this may explain why employers hire and promote same-sex employees.

Others have at the same time highlighted that persistent gender-related norms may cause traditionally "male" managerial behaviors to persist even in the event of a management change (Ely, 1995, Graves and Powell, 1995). A similar argument is that female managers may be appointed as "gatekeepers" with the intention to maintain the majority's dominance.[8]

Despite the many plausible reasons for why women could benefit from having female managers, only a few studies have looked at the empirical relevance of these arguments. Most of this evidence is based on cross-sectional studies relating the gender wage gap to the representation of managers (Hultin and Szulkin, 2003, Cohen and Huffman, 2007). While these provide suggestive evidence of the importance of managers, it should be noted that they suffer from potential problems generated by omitted variables.

Two recent studies use more reliable identification strategies to establish a relationship between manager characteristics and individual outcomes. Cardoso and Winter-Ebmer (2010) study a large representative sample from Portugal and find that women receive higher wages in female-led firms. In addition, Bell (2005) shows that female executives in women-led firms have higher compensation and are more likely to be among the top-five paid executives than comparable women in male-led firms.

[7] Sweden moved to a more decentralized wage bargaining system in the 1980s with greater scope for individual wage variation. Thus, mechanisms of this kind may have become more important (Nordström Skans et al., 2009).

[8] Bagues and Esteve-Volart (2010) exploit random assignment of candidates to evaluation committees in public examinations in Spain and show that female candidates are *less* likely to be hired when the committee consists of a greater share of women. They attribute this finding to that female evaluators are either overestimating the true quality of male candidates or that the presence of women strengthens the male committee members' bias towards male candidates.

These studies are compelling because they deal with some of the potential differences between male-led and female-led firms that could bias the coefficients of interest. However, they are also limited in the sense that they do not account for the potentially endogenous selection of workers into male- and female-led firms.[9]

3 Data

The data used for this analysis come from administrative registers collected by Statistics Sweden. The main register contains annual information on detailed occupational characteristics and monthly full-time wages for all establishments in the public sector and for a large sample of establishments in the private sector. Individuals are included in the annual dataset conditional on being employed in the month of November. Altogether, the data cover around 50 percent of those employed in the private sector with sampling weights to make the results representative for the population. The sample contains about 1.7 million workers per year for 13 years and 60,000 unique establishments, each observed for an average of 5 years.

Managers are identified according to the Swedish Standard for Classification of Occupations (SSYK), which is based on international standards (ISCO-88). The first digit in the occupational code divides the data into ten broad occupational levels with a specific number for managerial positions. It is also possible to identify more detailed manager types using the 3-digit code, such as top managers (directors and chief executives as well as managers for small enterprises) and middle managers (production and operation managers and other specialist managers in marketing, sales, human resources, and so on).

The main analysis focuses on the impact of women among the establishment's highest ranked managers on the wages of non-managerial workers. This means that I consider the share of women in top management for establishments that have top managers (72 percent of the sampled establishments) and the share of female middle managers, otherwise. For firms with multiple managerial levels, I also examine the effects of the proportion of women in top *and* middle management, as managers at lower tiers may provide stronger mentoring relationships or role models.

The main analysis focuses on the period 1996-2008 because 1996 is the first year that wages and occupations are observed. I do, however, also ex-

[9] A related literature documents substantial amounts of occupational and establishment gender segregation and that female-dense firms pay lower wages and have higher gender wage gaps (Carrington and Troske, 1995, Bayard et al., 2003, Datta Gupta and Rothstein, 2005). A few recent studies also looks at racial (Giuliano et al., 2009 and Giuliano and Ransom, 2010) and immigrant (Aslund et al., 2009) bias in hirings and quits.

ploit the data from the earlier period of 1985-1995 to construct individual skill measures with the objective of analyzing worker sorting. The data and estimation procedure for this analysis are described in more detail in Section 4.4.

The data is linked to information on individual characteristics such as age, gender and education as well as annual earnings. This information is available for the entire working-age population, and it stretches back to 1985. I use this data to derive measures of individual work experience and job tenure (truncated at 1985). In addition, I also calculate the annual proportion of female entry level co-workers (excluding managers) for each establishment in the sample.

4 Female managers and the gender wage gap

4 1 Descriptive evidence from Sweden

Columns 1 and 2 of Table 1 report statistics for the weighted sample, broken down by gender. These display well-known patterns documented in earlier studies in several countries; despite small gender differences in age, education and experience, women have more female co-workers, work in larger establishments and more often in the public sector. The occupational distribution also varies between men and women; more women work as clerks and service workers, whereas males are more likely to be craft workers and machine operators.

One number that stands out is the low representation of women in management occupations; three percent of female employees have managerial jobs compared to nine percent of males. Table 2 describes the characteristics of male and female managers. On average, female managers are younger and have higher education levels compared to male managers, which may reflect that the smaller group of female managers is more selected relative to the males. Yet, the wages received by female managers are lower on average and display less variation. Women are in addition found at lower managerial levels on average.[10]

[10] Other Scandinavian countries display similar wage gaps and high levels of occupational segregation (Datta Gupta et al., 2006). Meyersson-Milgrom and Petersen (2006) provide evidence on trends in female management from Sweden and the US.

Table 1 Summary statistics 1996-2008

	All Workers		Non-Manager Workers in:	
	Females	Males	Female-led	Male-led
	(1)	(2)	(3)	(4)
Monthly wage	9.88	10.03	9.87	9.90
(standard deviation)	(0.28)	(0.36)	(0.25)	(0.28)
Share of female co-workers	0.67	0.31	0.77	0.53
Age	42.1	40.1	42.6	41.7
Experience	14.8	14.7	15.0	14.6
Tenure	6.8	7.2	6.6	7.4
Education level:				
Less or equal than primary school	0.13	0.17	0.10	0.15
2 years of high school	0.31	0.30	0.31	0.28
3 years of high school	0.18	0.21	0.16	0.20
Some college	0.17	0.14	0.17	0.15
At least 3 years of college	0.21	0.16	0.26	0.20
Graduate	0.01	0.02	0.01	0.01
Unknown	0.002	0.003	0.002	0.002
Occupation:				
Legislators, senior officials and managers	0.03	0.09	-	-
Professionals	0.19	0.17	0.24	0.21
Technicians and associate professionals	0.21	0.19	0.19	0.23
Clerks	0.15	0.06	0.10	0.23
Service workers and shop sales workers	0.30	0.07	0.38	0.14
Craft and related trade workers	0.01	0.18	0.01	0.02
Plant and machine operators and assemblers	0.04	0.19	0.01	0.09
Elementary occupations	0.07	0.05	0.07	0.07
Establishment characteristics:				
Age of establishment	14.6	14.2	14.3	15.2
Private Sector	0.35	0.62	0.28	0.51
Establishment size	441	364	299	693
Observations (unweighted)	13,496,367	10,149,707	4,991,116	4,800,542
Observations (weighted)	17,977,446	18,809,877	5,621,508	6,562,965
Years	13	13	13	13

Notes: Establishments are counted as female-led if more than 50 percent of the highest ranked managers are women. The variables for experience, tenure and age of establishment are calculated from the data and truncated in 1985. The observations are weighted according to their sampling probabilities.

Table 2 Manager characteristics by gender.

	Managers:	
	Females (1)	Males (2)
Monthly wage	10.29	10.42
(standard deviation)	(0.36)	(0.42)
Age	45.4	46.3
Experience	17.3	17.2
Education level:		
Less or equal than primary	0.06	0.11
2 years of high school	0.15	0.20
3 years of high school	0.14	0.20
Some college	0.28	0.20
At least 3 years of college	0.36	0.27
Graduate	0.01	0.02
Unknown	-	-
Manager type:		
Share of managers in top management	0.31	0.38
Share of managers in middle management	0.69	0.62
Observations (unweighted)	347,907	606,578
Observations (weighted)	511,296	1,223,305
Years	13	13

Notes: The sample consists of all of the highest ranked managers within each establishment. The observations are weighted according to their sampling probabilities.

The analysis examines the impact of manager sex composition on wages received by non-managerial workers. Figure 1 displays the male-female wage gap for these workers in the period 1996-2008, based on yearly wage regressions that account for standard human capital variables (age, age^2, education level and experience) and 3-digit occupation. Figure 2 shows the share of female managers over the same period.

In line with the relevant literature, differences in the occupational distribution explain about half of the gender wage gap adjusted for standard human capital controls. We can also see that the within-occupation gender wage gap narrowed by 1.5 percentage point during this period at the same time as the proportion of female managers experienced a substantial increase from 26 percent in the beginning of the study period to 36 percent in 2008.[11][12]

[11] The gender wage gap narrowed dramatically from the 1960's to the early 1980's. An increased wage compression was partly responsible for this, although changes in other factors, such as unobserved skills and discrimination, seem to have been more important (Edin and Richardsson, 2002). The closing of the gender gap slowed down and even increased somewhat during the 1980s.

[12] The number of female managers is steadily increasing throughout the study period. Hence the "jump" in the female management share in the beginning and end of the period is attributable to changes in the number of male-led firms, which display more variation over this period. The variation is consistent with the growth of the Swedish economy, which continued to increase since the recession in the early 1990s but slowed down between 2007 and 2008.

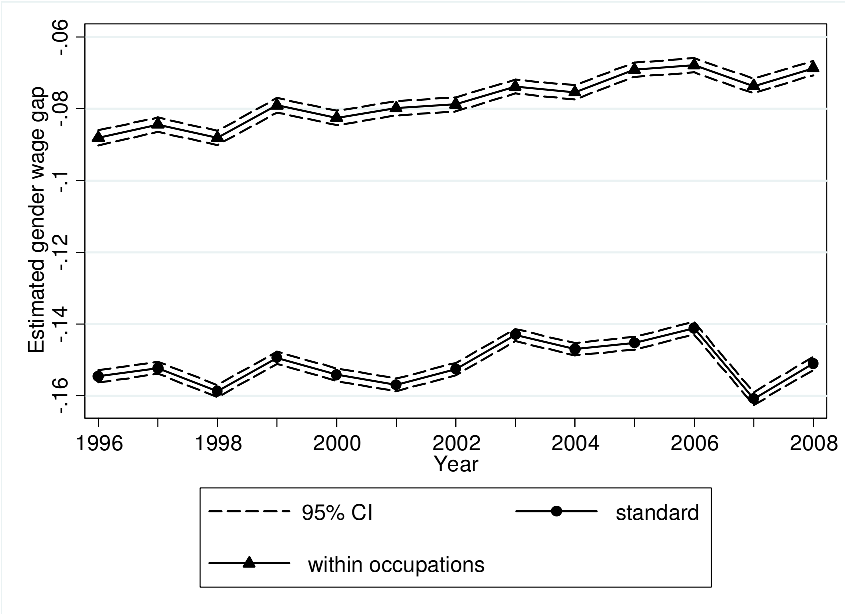

Figure 1 Estimated male-female wage gap for non-managers 1996-2008.

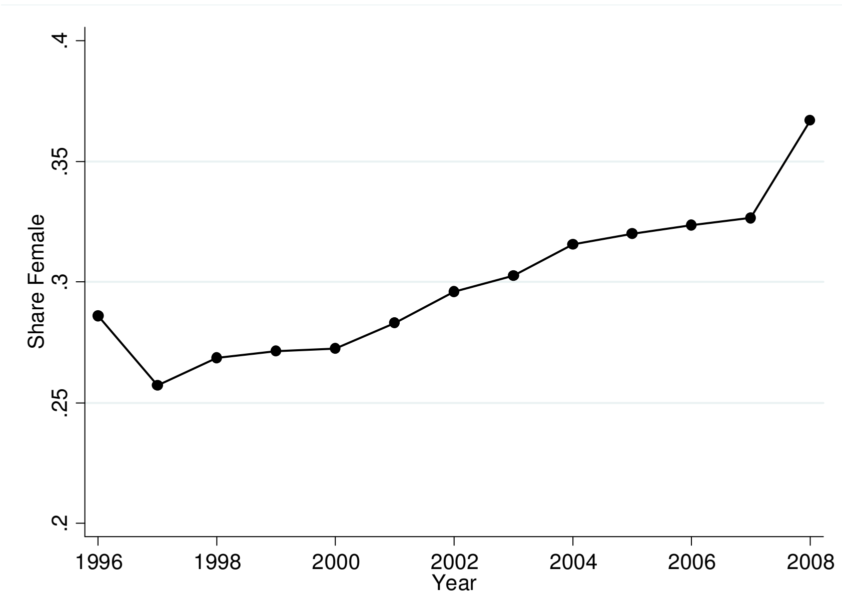

Figure 2 Share of female (highest ranked) managers 1996-2008.

Columns 3 and 4 in Table 1 provide descriptive evidence on female non-managerial workers in female-led versus male-led establishments, which are defined as female-led if more than half of the highest ranked managers are women.[13] Compared to male managers, women disproportionally manage other women in lower-paying occupations and in smaller establishments in the public sector; only 28 percent of women in female-led firms are found in the private sector compared to 51 percent in male-led firms. Furthermore, female-led establishments employ a substantially higher share of women.

In sum, this descriptive analysis suggests that the share of female managers has increased substantially during the study period and that the gender wage gap has narrowed. However, women are still in lower-ranked managerial positions, and they manage a substantially higher share of women relative to male managers. There is moreover a clear negative correlation between the proportion of female managers and women's wages, which seems partly due to the high concentration of female managers in the public sector and in lower-paying jobs. The next step is to try to assess whether the increased share of women in management is associated with reductions in the male-female wage gap.

4.1 Empirical strategy

To examine whether the gender composition of managers affects the gender wage gap I estimate models of the following form:

$$\log(w)_{ijt} = \beta_1 F_{ijt} + \gamma_1 F_{ijt}^M \times S_{ijt}^M + \gamma_2 S_{ijt}^M + X_{ijt} + W_{ijt} + \delta_i + \delta_t + \varepsilon_{ijt} \quad (1)$$

where $\log(w)_{ijt}$ is the log monthly wage of worker i in establishment j in year t; F_{ijt} is a dummy that takes the value of one if the worker is female; S_{ijt}^{M} is the proportion of female managers; X_{ijt} and W_{jt} are vectors of individual and establishment characteristics.[14] Finally, δ_t and δ_i denote year and worker fixed effects, respectively, and ε_{ijt} is the error term. The main coefficient of interest is γ_1, which measures the impact of female managers on the male-female wage gap.

The model accounts for many of the possible factors that could confound the association between the share of female managers and the gender wage

[13] Figure A1 displays the distribution of female managers and co-workers for men and women in the sample. It confirms that women have higher exposure to both female co-workers and female managers. We also see that most workers have either zero or all female managers, which reflect that most establishments have one manager at the highest rank.

[14] The individual characteristics are age, age^2, educational attainment (6 levels), experience and tenure divided into five categories (0, 1-2, 3-4, 5-10, and >10 years). The workplace characteristics are ln(workplace size), industry, sector and the proportion of female co-workers.

gap. Establishment size accounts for the fact that women tend to manage smaller establishments and the industry and sector dummies capture systematic differences in the proportion of female managers across these domains, which in turn may be correlated with the size of the gender wage gap. Finally, the year dummy variables account for secular changes in the male-female wage gap and the proportion of female managers.

To control for unobserved time-varying differences between establishments, such as wage practices and organizational structure, I also include the proportion of female co-workers. This could be a concern if female managers have a direct impact on the female composition of the establishment, in which case the inclusion of the female establishment share would produce an inconsistent estimate of γ_1. To assess the importance of this concern, I examine the relationship between female management and female hires in Section 4.4. I also test whether the estimates are sensitive to time-invariant establishment heterogeneity by including establishment fixed effect in (1).

The worker fixed effects account for the fact that manager characteristics may affect the type of workers who want to join and leave the firm. The variation in exposure to female managers comes from workers switching jobs, as well as from changes in manager composition within a given job spell. This model thus addresses the main concern that the variation in the gender composition of managers may be correlated with unobserved differences in worker quality. For example, if the most talented women enter female-led establishments, either because they anticipate better career opportunities in female-led firms or because female managers have better knowledge about other women's productivity, this could bias the results towards finding a negative effect of female management on the gender-wage gap.[15]

4.2 Results

Table 3 reports the baseline results; the last column is the worker fixed effects model obtained from estimation of equation (1). The variable of interest is the interaction term between the gender dummy and the proportion of female managers reported in the first row. To make the interpretation of the results meaningful, the proportion of female managers and co-workers are centered around their means throughout the table, and the individual observations are weighted according to their sampling probabilities.

Column (1) shows the estimated gender wage gap and the association between the wage and the proportion of female managers when including standard Mincerian human capital controls (education, the age-earnings profile,

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[15] Female managers may also affect the composition of those who *leave* the firm. For example, using matched employer-employee data from Sweden for the period 1970-1990, Kwon and Meyersson Milgrom (2010) show that males are more likely to quit under female management in male-dominated occupations. In female-dominated occupations, men are indifferent. These effects are particularly strong among workers with a college education.

experience and job tenure) and year effects. The estimates of the controls are not shown in the table, but these have the expected signs. The estimated coefficients suggest that the adjusted gender wage gap is 14 percent and that workers receive 8 percent lower wages in female-led establishments compared to male-led establishments.

Column (2) allows the impact of having female managers to differ between male and female workers. The wage penalty of being employed in a women-led establishment is lower for female than for male employees, suggesting a negative correlation between the proportion of female managers and the gender wage gap. Column (3) further adds industry and establishment controls including the share of female co-workers to the focal worker, column (4) includes establishment fixed effects, column (5) *establishment×(3-digit) occupation* fixed effects and column (6) includes worker fixed effects.[16]

Both the gender wage gap and the wage penalty from being employed in a women-led establishment diminish as more establishment attributes are controlled for, suggesting that part of these associations reflect that female employees and managers work in lower-paying industries and establishments. The estimates in column (3) suggest that men receive 4 percent lower and women receive 0.7 percent higher wages (4.7 versus 4.0) under female compared to male management. The relationship remains when including establishment fixed effects in column (5), suggesting that time-invariant establishment heterogeneity is not driving the relationship between female-led establishments and the gender wage gap.

[16] I allow the impact of female co-workers to be different for women and men.

Table 3 Female managers and the gender wage gap.

	(1)	(2)	(3)	(4)	(5)	(6)
	OLS Human Capital Controls	OLS Human Capital Controls	OLS Industry and Establishment Controls	Establishment Fixed Effects	Establishment × Occupation Fixed Effects	Worker Fixed Effects
Impact on gender wage gap:						
Female managers × Female		0.023***	0.047***	0.050***	0.035***	0.003**
		(0.004)	(0.004)	(0.002)	(0.001)	(0.001)
Baseline estimates:						
Female	-0.141***	-0.139***	-0.111***	-0.107***	-0.064***	-
	(0.001)	(0.002)	(0.001)	(0.002)	(0.001)	
Female Managers	-0.084***	-0.100***	-0.040***	-0.036***	-0.025**	-0.004**
	(0.003)	(0.005)	(0.004)	(0.002)	(0.001)	(0.002)
Year dummies	yes	yes	yes	yes	yes	yes
Share female co-workers	no	no	yes	yes	yes	yes
Industry dummies	no	no	yes	yes	yes	yes
Establishment fixed effects	no	no	no	yes	yes	no
Establishment × Occ. fixed effects	no	no	no	no	yes	no
Worker fixed effects	no	no	no	no	no	yes
R^2	0.46	0.46	0.52	0.65	0.76	0.92
Establishments	61,684	61,684	58,150	58,150	59,486	58,150
Observations	23,232,506	23,232,506	22,275,484	22,275,484	22,329,637	22,275,484

Notes: *, ** and *** denote statistical significance at the 10, 5 and 1 percent levels, respectively. Standard errors robust for clustering at the establishment level are shown in parentheses. Apart from the controls reported in the table, all regressions control for age, age^2, education dummies (6 categories), experience and tenure dummies (0, 1-2, 3-4, 5-10, and >10 years). Columns (3)-(6) include log(workplace size) and a variable indicating whether the individual works in the public sector. Column (3) includes industry dummies at the 5-digit level, while columns (4)-(6) replace the 5-digit industry dummies with dummies defined at the 2-digit level. The occupation dummies included in column (5) are defined at the 3-digit level. The observations are weighted according to their sampling probabilities.

One potential reason for the lower wage-gap in female-led firms is that women work in relatively better jobs when working for female employers. I test this by including *workplace*×*occupation* fixed effects in column (5), which explains about half of the wage premium from having female managers.[17] However, even when comparing the wages of men and women who hold similar jobs for the same employer, there is still a three percent lower wage gap in female-led versus male-led establishments.[18]

The last column includes worker fixed effects. These estimates suggest that individual sorting is very important; accounting for individual heterogeneity reduces the estimates substantially; male workers receive 0.4 percent lower wages in female-led firms, whereas women receive 0.1 percent lower wages. Thus, the impact of female managers on the gender wage gap is economically small (but still precisely estimated). The results from separate regressions for men and women lead to the same conclusions (Table A1).

Table A2 in the Appendix examines how sensitive the main results from Table 3, column (6) are to changes in the choice of the continuous measure of female managers and the use of sampling weights. First, I test whether a dichotomous measure of female leadership produces different results. I constructed a female-led dummy that takes the value of one if more than 50 percent of the highest-ranked managers are women and zero, otherwise. As we can see in Panel B, these results are very similar to those in Table 3, although somewhat less precisely estimated. This is not particularly surprising since most establishments are either fully managed by males or by females (Figure A1, Appendix).[19] Unweighted regressions also produce very similar results (Panel C), suggesting that the underrepresentation of small establishments in the private sector is unlikely to affect the estimates to any large extent.

[17] It should be noted that the inclusion of occupational controls implies that I may be overcontrolling in the sense that job allocation is one of the mechanisms through which women could benefit from female managers.

[18] The 3-digit occupational code covers 113 different occupations that distinguish between, for example, college and primary teaching professionals or business and legal professionals. Meyersson et al. (2001) document an occupation-establishment gender wage gap of one and five percent for blue-collar and white-collar workers, respectively, using a large sample of private sector employees in 1990. They use a more detailed occupational code, which is a possible explanation for the smaller wage gap in their study compared to that documented in column (5) of Table 3.

[19] The median number of managers is one, and thus, in most cases, the female share is either zero or one.

5 Additional results

5.1 Female managers and worker skill sorting

The results in Table 3 indicate that most of the association between female managers and wages is driven by compositional effects. To further substantiate the importance of worker selection, this section examines whether the skill composition of hires varies with the gender composition of managers.

The data for the analysis contain all newly hired workers during the period 1996-2008; defined as those who did not receive compensation from their current employer in any of the five preceding years.[20] Inspired by Carlsson et al. (2011), the data are further linked to pre-determined measures of worker skills obtained using the regression framework developed by Abowd et al. (1999), which decomposes wages into individual and firm heterogeneity. In practice, I use data for the pre-sample period 1985-1995 and estimate models of the following form:[21]

$$\log(w)_{ijt} = \delta_1 Age_{ijt} + \delta_2 Age_{ijt}^2 + \theta_i + \psi_{J(i,t)} + \varphi_t + \varepsilon_{ijt} \qquad (2)$$

where θ_i is a vector of individual specific indicators; $\psi_{J(i,t)}$ comprises the establishment indicators; φ_t captures the time effects; and ε_{ijt} is the error term. The model also accounts for the age-earnings profile of the worker.

The estimated person effect $\hat{\theta}_i$ measures the part of the wage that does not vary as the employee moves from one establishment to another, thus reflecting the portable earnings capacity of workers. The $\hat{\theta}_i$'s may include both observable characteristics such as education and experience as well as unobservable traits, such as innate ability or motivation. For simplicity I refer to these estimates as worker "skills".[22] The main advantage of using pre-period data is that this reassures that the skill measures are exogenous to the gender composition of managers at the time of hire.[23] Moreover, as skills

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[20] Some additional restrictions are applied to the data. First, to focus attention on actual hires, I disregard workers earning below the 10^{th} percentile of the overall earnings distribution in order to avoid classifying loosely connected workers as new hires. Second, I also require that the establishment existed the year before the hire, and I remove establishments that changed more than two-thirds of the workforce from one year to the next.

[21] I estimate the person-effects using the a2reg.do code written by Amine Quazad. This program follows Abowd et al. (2002).

[22] Note that worker's observed human capital will be included in the second stage hiring equation (eq. 3).

[23] A drawback is that the early period lacks information on actual wages. Thus, instead of having wages as the dependent variable in eq. (2), I use monthly full-time earnings. These are calculated as worker's annual earnings divided by months of employment, including only employment spells that cover November each year. In order to focus on full-time or close to full-time earnings, I use a minimum wage cut-off of 75 percent of the mean wage of janitors. In addition, I retain worker's main source of income. Other studies have used this approximated wage measure and shown that the earnings distribution resembles the true wage distri-

are measured in terms of wages, it is easy to relate the effects of female management on the skill composition of hires to the portion of the gender wage gap explained by the worker fixed effects in the main analysis.[24]

Figure A2 in the Appendix shows the distribution of the $\hat{\theta}$ values among newly hired workers during 1996-2008, broken down by gender. There is a wide variation in the person effects, suggesting that workers differ substantially in their permanent skills. The skill distributions are, moreover, different in the sample of hired men and women; the estimated person effects are higher on average and display less variation for male workers compared to female workers.[25]

Table 4 examines the association between the gender composition of managers and the skill composition of new hires. For comparability, the empirical approach is identical to that used when analyzing the wages. Thus, the model is:

$$\hat{\theta}^{H}_{ijt} = \beta_1 F_{ijt} + \gamma_1 F_{ijt} \times S^{M}_{ijt} + \gamma_2 S^{M}_{ijt} + X_{ijt} + \delta_t + \varepsilon_{ijt} \tag{3}$$

where $\hat{\theta}^{H}_{ijt}$ is the estimated skill component obtained from eq. (2) for individual i hired by establishment j in year t; F_{ijt} is a dummy that takes the value of one if the individual is female; S^{M}_{ijt} is the proportion of female managers; and X_{ijt} includes worker i:s observable human capital (age and education). The variable of interest is γ_1, which measures whether female-led establishments recruit female workers with higher permanent skills compared to male-led establishments.

The estimates in Table 4 suggest that this is indeed the case; there is a positive and statistically significant impact of female managers on the skill level of newly hired women.[26] Importantly, this estimate does not change much throughout the table, suggesting that unobserved heterogeneity at the

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bution (Nordström Skans et al., 2009 and Carlsson et al., 2011). To be sure, I also checked the correlation between the person effects derived from monthly earnings and wages in the later period (see Table A3 in Appendix). The correlation is high (88 percent), suggesting that using monthly full time earnings instead of true wages is not likely to be an issue of large concern for this analysis.

[24] This is also highlighted by Carlsson et al., (2011), who use a similar strategy to examine the importance of worker selection in explaining the relationship between firm-level productivity and individual wages.

[25] Both of these may (partly) reflect differences and variations in hours worked.

[26] We learn from looking at the female dummy that there is a substantial overall difference in the skill level between hired men and women. Although I disregard monthly earnings below a minimum wage in order to resemble the true wage distribution when estimating the person effects in the pre-period, the distribution of the approximated wage measure displays higher variation than true wages, which probably reflects differences in hours. The magnitude of the gender difference should thus be interpreted with caution, as it may indicate systematic gender differences in hours worked in the pre-period.

industry-, establishment- or job level does not explain these gender-related differences in recruitments. Interestingly, the magnitudes are also similar to the portion of the association between female management and the gender wage gap explained by the worker fixed effects in the main analysis presented in Table 3. This confirms that most of this relationship is explained by compositional affects, which moreover seem to arise through differential hiring rather than employee turnover.[27]

In the lower panel of Table 4, I also look at the relationship between the gender composition of managers and the proportion of female hires. This relationship is interesting in itself and it is also informative regarding the validity of the main empirical strategy, which uses co-worker composition to proxy for unobserved differences at the establishment level. The results are obtained from estimating linear probability models, where the dependent variable takes the value of one if the hire is female and zero, otherwise. The explanatory variable of interest is the proportion of female managers. The rest of the controls included are those indicated by the table.

As we can see, there is a substantial raw correlation between the female composition of managers and hires, but only a small part of this effect remains when industry and establishment characteristics are taken into account. Evaluated at the average female share of hires (54 percent), the estimates suggest that gender-biased recruitments is of minor importance in this context.[28]

[27] Using the unweighted sample also yields similar results.

[28] Using similar models, Essay 2 documents that immigrant managers are three times as likely to hire immigrants compared to native managers. In comparison to those findings, the impact of female managers is small. This also implies that the potential bias in the main results is likely to be small, since workplace gender composition is only mildly affected by the share of female managers.

Table 4 Female managers and the composition of hires.

	(1)	(2)	(3)	(4)
Specification:	OLS Human capital controls	OLS Industry and establishment controls	Est. Fixed Effects	Est. × Occ. Fixed Effects
A: Dep. var.: Person effect				
Female managers× Female	0.024***	0.028***	0.035***	0.029***
	(0.003)	(0.004)	(0.003)	(0.003)
Baseline estimates:				
Female	-0.163***	-0.149***	-0.150***	-0.122***
	(0.001)	(0.001)	(0.001)	(0.001)
Female managers	-0.060***	-0.024***	-0.025***	-0.019***
	(0.003)	(0.003)	(0.002)	(0.002)
R^2	0.802	0.809	0.824	0.852
Observations	1,500,109	1,483,766	1,453,064	1,453,064
B: Dep. var.: Hire is female				
Female managers	0.386***	0.071***	0.010***	0.008***
	(0.005)	(0.004)	(0.003)	(0.003)
R^2	0.108	0.238	0.301	0.404
Observations	2,902,637	2,893,096	2,815,111	2,815,111
Year dummies	yes	yes	yes	yes
Education dummies (observable general human capital)	yes	yes	yes	yes
Industry dummies	no	yes	yes	yes
Establishment fixed effects	no	no	yes	yes
Establishment×occupation (3-digit) fixed effects	no	no	no	yes

Notes: . *, ** and *** denote statistical significance at the 10, 5 and 1 percent levels, respectively. Standard errors robust for clustering at the establishment level are shown in parentheses. The sample consists of newly hired workers during 1996-2008. The dependent variable in the first panel is the estimated worker fixed effects obtained from the estimation of eq. (2). The dependent variable in the second panel is a dummy that takes the value of one if the hire is female and zero, otherwise. The proportion of female managers is mean-centered. Apart from the controls reported in the table, all regressions control for age and age^2. Columns (2)-(4) include log(establishment size) and a variable indicating whether the individual works in the public sector. Column (2) includes industry dummies at the 5-digit level, while columns (3) and (4) replace the 5-digit industry dummies with dummies defined at the 2-digit level. The observations are weighted according to the sampling probabilities.

5.2 Heterogeneity analysis

Despite the small effects found in Table 3, it is not possible to rule out that some women may still benefit from having female managers. To examine this in further detail, I look at the impact of female managers in various subsamples. This section provides a summary of these results, which are presented in Table A4-A6 in the Appendix. For simplicity, I estimate separate regressions for women and men, but I also discuss the implications of the estimates for the gender wage gap.

I start out with the differential impact of female managers with respect to sector and establishment size. One potential explanation for the small effects found earlier is that female managers are more concentrated to the public sector, where government objectives and policies against discrimination may limit the scope for discrimination in the first place.[29] Columns 1 and 2 in Table A4 display the results from separate estimations of model (1) for public and private employees. I find no evidence that women-led firms are more important in the private sector compared to the public sector. If anything, women receive lower wages in female-led private establishments (Panel A), but this is also true for male workers (Panel B). Hence, there is no association between the proportion of female managers and the gender wage gap in the private sector. I also test whether the impact differs with establishment size, but I reject that managers have a significantly differential impact in small (column 3) and large (column 4) establishments.

Finally, I examine whether there is a difference in the impact of female managers in women-dominated versus male dominated establishments by allowing the effect of female managers to vary with the sex composition of the co-workers. The results, as reported in columns (5)-(7) suggest that there is a small wage premium from being employed in a female-led establishment with many women compared to being employed in a female-dense male-led firm (column 5). There are at least two possible mechanisms that could motivate this finding. First, a higher proportion of women may reflect that the establishment is female-friendly, with female managers, a more women dense workforce and higher female wages. Second, women managers may have better information about women's productivity in the hiring stage, for example, if they interact more closely with their female employees or use them as referrals.[30]

Columns (6) and (7) show results from separate regressions for workers entering an establishment and for those with at least one year of job tenure. The female wage premium of being hired by a woman manager is concentrated at the beginning of the employment spell. This lends suggestive sup-

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[29] Although the public sector in Sweden is decentralized and wages to a large extent are determined through individual wage bargaining, wages are less dispersed in the public sector than in the private sector.

[30] Hires mediated by referrals reduce employer uncertainty about worker productivity both by transmitting information to the prospective employee about the employer and by informing the employer about potential employees (Montgomery, 1991). Dustmann et al. (2010) argue that since the network improves the ability of employers to recognize workers with the highest match-specific productivity, hires found through referrals receive an entry wage premium that diminishes with tenure. If female managers are more likely to use other women as referrals, this could lead to an entry wage premium that increases with the number of potential referrals (i.e., women) and that diminishes as employers learn about the true productivity of workers.

port to the notion that female managers in women-dense establishments may have an information advantage in the hiring stage, which diminishes as employers learn about the true productivity of workers.

So far, I have only concentrated on the highest ranked managers in the establishments under study. This could be a misleading approach if workers are influenced by managers with whom they closely interact. For example, if managers are as important as mentors or role models, then being exposed to women at lower managerial levels may be equally – or even more – important than being employed in a firm with a female executives. To examine this, I use a sample of establishments with multiple manager levels and allow for a separate impact from middle and top managers.[31] The importance of middle managers is particularly interesting because this group contains a larger fraction of women than the group of top managers.

The results, displayed in Table A5, show that there is no significant impact of either top or middle managers on female wages. However, there is a negative and significant interaction effect between the impact of female middle managers and wages (column 2), though only for tenured workers (column 4). This indicates that being exposed to middle managers on the job seems to increase women's wages when women are in the minority in their establishment. The fact that managers only matter when the woman-to-female-manager ratio is low may suggest that there is a cost attached to helping other women advance.[32]

To illustrate this further, I also estimated models similar to model (1), but I allowed both the baseline effect of managers as well as the interaction between managers and entry-level co-workers to vary with job tenure according to five categories (0, 1-2, 3-4, 5-10, and >10 years). The results for women are plotted in Figure 3. For estimates and standard errors, see Table A6 in the Appendix.

Although the precision of the estimates is poor, they clearly support that women with a high share of female middle managers have higher wage growth if they are in the minority in their establishment. However, while women seem to benefit from a low female-to-manager ratio, male workers lose out and receive a relatively lower wage growth compared to male workers in male-led firms (Figure A3 and Figure A4).

[31] Twenty-six percent of the workers are exposed both to top and middle managers.

[32] This finding is consistent with Hultin and Szulkin (2003), who show that the proportion of males at lower decision-making hierarchical levels (i.e., supervisors) has a larger impact on the gender-wage gap than male representation at higher levels (i.e., managers), and it is also consistent with Cardoso and Winter Ebmer (2010), who show that the wage premium from having lower-level female managers decreases with the share of females in the firm.

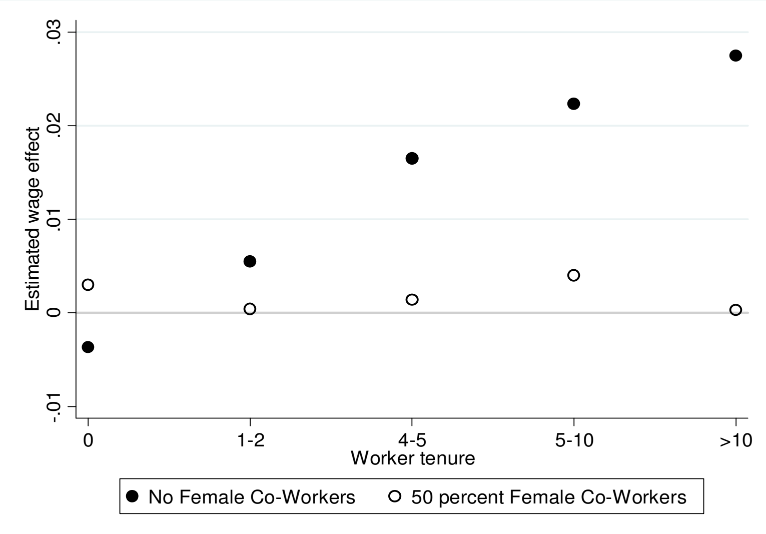

Figure 3 Female wage premium of female *middle* management by tenure and co–worker composition.

Notes: The black circles show the estimated effect of having female middle managers for women without any female co-workers. The hollow circles show the estimated effects for women with 50 percent female co-workers. For full results and standard errors, see Table A4 in the Appendix.

6 Conclusions

The underrepresentation of women in management positions is often highlighted as one explanation for the observed gender inequality in the labor market. A theoretical literature has also argued that female managers may break the glass ceiling for female employees, e.g., by serving as mentors and role models for lower-level employees or by eliminating discriminatory behavior. However, the existing empirical evidence provides scarce evidence on the relevance of gender biased wage setting and promotion practices and has not been able to separate such effects from alternative explanations.

This paper has examined whether gender bias in the worker-manager relationship is an important determinant of wages using Swedish longitudinal matched employer-employee data covering 13 years. I document that the gender wage gap among non-managerial workers decreased by one percentage point during this period, while the share of female managers increased by more than ten percentage points. In addition, I find a negative correlation between the proportion of female managers and the within-establishment gender wage gap, which is both economically and statistically relevant. The magnitude of this effect is in line with previous work; Carduso and Winter-Ebmer (2010) document a similar association in Portugal.

In addition, I am also able to account for time-invariant productivity differences across workers. This is a contribution to previous studies that rely on the assumption that variation in the sex composition of managers is uncorrelated with productivity-related changes in the workforce of establishments. I find that worker heterogeneity explains most of the association between female managers and the gender wage gap, which suggests that worker sorting is a non-trivial determinant of the gender wage gap associated with manager composition. The remaining effects imply a 0.3 percent narrowing of the gender wage gap in female-led versus male-led establishments. The effects do not seem to vary with establishment size or sector.

Looking at a large sample of hired workers I find that the compositional effects captured by the worker fixed effects arise primarily because female managers hire female workers with higher unobserved portable earnings capacity compared to male managers. I cannot separate whether this is mainly due to behavior on the demand or supply side of the labor market. In other words, female talent may either enter female-led firms because they anticipate better career opportunities or because female managers have better information about other women's productivity. I do find a small wage premium for women hired by female managers in female-dense firms, concentrated to the beginning of the employment spell. This lends some suggestive support to the latter explanation, as women may interact more closely with their female subordinates or use them as referrals.

In light of the underrepresentation of female managers, and their concentration to certain industries, my findings draw attention to the role of female managers for the (gender-specific) skill-allocation of workers across firms. While this is consistent with models of imperfect information, a better knowledge about the potential role of more specific explanations, such as statistical discrimination and the use of segregated networks is an important and interesting area for future research.

Finally, women also seem to benefit from having more female managers in middle management positions, but this positive impact diminishes as the share of female co-workers increase. Together, these results suggest that female supervisors might protect or mentor other women in male-dominated work environments, but the role of female managers can be overstated if we do not account for the selection of workers into male- versus female-led establishments.

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Appendix A

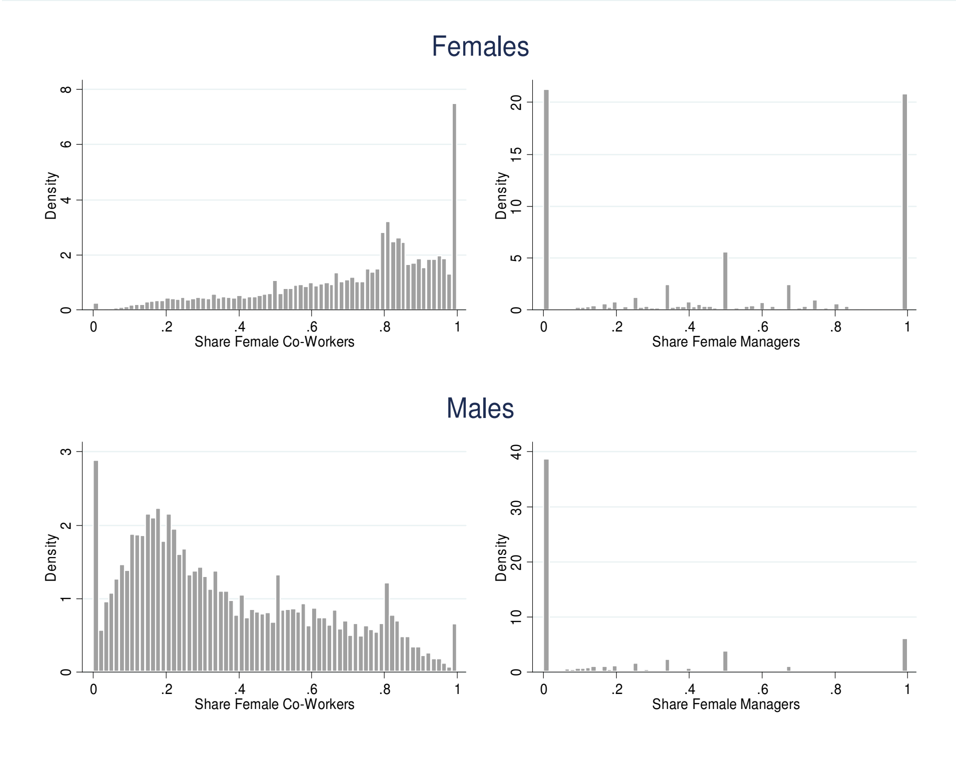

Figure A1 Distribution of female co-workers and highest ranked female managers for female (upper) and male (lower) employees.

Table A1Female managers and wages, separate regressions.

	(1)	(2)
Specification:	Worker FE Females	Worker FE Males
Female Managers	-0.001**	-0.003**
	(0.001)	(0.002)
R^2	0.91	0.92
Establishments	55,288	52,385
Observations	11,263,781	11,011,703
Year dummies	yes	yes
Industry dummies	yes	yes
Worker fixed effects	yes	yes

Notes: Each column represents a separate regression. *, ** and *** denote statistical significance at the 10, 5 and 1 percent levels, respectively. Standard errors robust for clustering at the establishment level are shown in parentheses. The table provides results from separate regressions of model (1) on samples of non-manager male and female employees. All regressions control for age, age^2, education dummies (6 categories), experience and tenure dummies (0, 1-2, 3-4, 5-10 and >10 years), log(workplace size) and a public sector dummy.

Table A2 Sensitivity of the main results.

Variable of interest:	Female managers × Female	Female Managers
A: Baseline (column 5, Table 2)	0.003**	-0.004**
	(0.002)	(0.002)
B: Female majority (binary variable)	0.003	-0.003**
	(0.002)	(0.002)
C: Unweighted regressions	0.003**	-0.004**
	(0.001)	(0.002)

Notes: Each panel (row) represents a separate regression. *, ** and *** denote statistical significance at the 10, 5 and 1 percent levels, respectively. Standard errors robust for clustering at the establishment level are shown in parentheses. All regressions control for age, age^2, education dummies (6 categories), experience and tenure dummies (0, 1-2, 3-4, 5-10 and >10 years), log(workplace size), a public sector dummy and year effects. They also include industry (2-digit) and worker fixed effects.

Table A3 Correlation between estimated person effects from monthly wages and monthly earnings.

	Person effects monthly wages	Person effects monthly full time earnings
Person effects monthly wages	1	
Person effects monthly full time earnings	0.88	1

Notes: The table shows the correlation between the estimated person effects using monthly full-time earnings and true monthly wages obtained from equation (2) as described in Section 5.1 using data for the period 1996-2008.

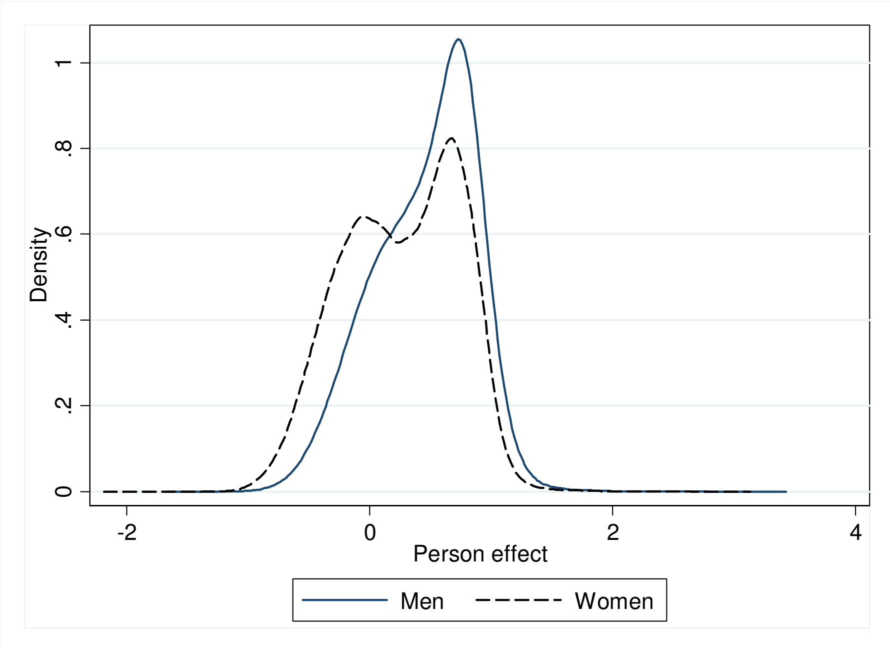

Figure A2 Distribution of hires permanent skills.

Notes: This figure shows a kernel density estimate of the person effects obtained from equation (2) in Section 5.1 I use an Epanechnikov kernel and “optimal” band-width. The sample consists of new hires during 1996-2008.

Table A4 Effect of female managers on wages: subsamples.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Public	Private	Workplace size <100	Workplace size >99	Female density	Worker tenure <1 (entry wage)	Worker tenure >1
Panel A: *Females*							
Female managers	0.0003	-0.007**	-0.001*	-0.002*	-0.001**	-0.002	-0.001*
	(0.0007)	(0.003)	(0.001)	(0.002)	(0.0007)	(0.001)	(0.008)
Female managers × female co-workers					0.010**	0.027***	0.006
					(0.003)	(0.006)	(0.004)
R^2	0.93	0.93	0.93	0.92	0.91	0.90	0.92
Establishments	34,977	25,526	52,295	6,120	55,288	46,569	53,183
Observations	7,815,406	3,448,375	5,715,810	5,517,327	11,263,781	1,815,254	9,448,527
Panel B: *Males*							
Female managers	-0.002	-0.007*	-0.003**	-0.004	-0.004**	-0.008**	-0.003
	(0.001)	(0.004)	(0.001)	(0.003)	(0.002)	(0.003)	(0.002)
Female managers × female co-workers					0.005	0.010	0.006
					(0.003)	(0.007)	(0.006)
R^2	0.94	0.93	0.95	0.91	0.92	0.94	0.93
Establishments	32,551	24,977	49,380	6,120	52,385	43,790	49,304
Observations	5,036,324	5,975,379	5,160,882	5,826,623	11,011,703	1,543,324	9,468,379
Year dummies	yes	yes	yes	yes	yes	yes	yes
Industry dummies	yes	yes	yes	yes	yes	yes	yes
Worker fixed effects	yes	yes	yes	yes	yes	yes	yes

Notes: *, ** and *** denote statistical significance at the 10, 5 and 1 percent levels, respectively. Standard errors robust for clustering at the establishment level are shown in parentheses. Apart from the controls reported in the table, all regressions control for age, age^2, education dummies (6 categories), experience and tenure dummies (0, 1-2, 3-4, 5-10 and >10 years), log(workplace size) and a dummy indicating whether the individual works in an establishment in the public sector. The industry dummies are defined at the 2-digit level. All variables shown in the table are mean-centered.

Table A5 Impact of female managers on female wages (top and middle managers).

	(1)	(2)	(3)	(4)
	Baseline	Co-worker interactions	Worker job tenure <1 (entry wage)	Worker job tenure >1
Female middle managers	0.002	0.001	0.005	0.002
	(0.002)	(0.002)	(0.005)	(0.002)
Female top managers	-0.004	-0.004*	0.002	-0.003
	(0.003)	(0.002)	(0.004)	(0.002)
Manager–co-worker interactions:				
Female middle × female		-0.015	0.018	-0.020**
co-workers		(0.008)	(0.020)	(0.010)
Female top × female		0.003	0.006	0.004
co-workers		(0.010)	(0.018)	(0.010)
Year dummies	yes	yes	yes	yes
Industry dummies (2-digit)	yes	yes	yes	yes
Worker fixed effects	yes	yes	yes	yes
R^2	0.93	0.93	0.95	0.94§
Establishments	13,404	13,404	11,441	13,302
Observations	3,529,925	3,529,925	533,321	2,996,604

Notes: *, ** and *** denote statistical significance at the 10, 5 and 1 percent levels, respectively. Standard errors robust for clustering at the establishment level are shown in parentheses. Apart from the controls reported in the table, all regressions control for age, age^2, education dummies (6 categories), experience and tenure dummies (0, 1-2, 3-4, 5-10, and >10 years), log(workplace size) and a dummy indicating whether the individual works in an establishment in the public sector. All variables shares shown in the table are mean-centered.

Table A6 Effect of female managers, manager type and tenure interactions.

	(1)		(2)	
Sample:	Females		Males	
	Estimate	se	Estimate	se
Female middle managers × tenure				
0	-0.004	0.009	-0.001	0.011
1-2	0.006	0.009	-0.014	0.009
3-4	0.017	0.012	-0.020*	0.012
5-10	0.022*	0.013	-0.008	0.016
>10	0.027*	0.016	-0.008	0.017
Manager-co-worker interactions:				
Female middle × female co-workers × tenure				
0	0.014	0.013	0.025	0.018
1-2	-0.010*	0.011	0.036**	0.014
3-4	-0.030**	0.016	0.043**	0.014
5	-0.037**	0.018	0.015	0.026
>10	-0.054	0.21	-0.010	0.028
Female top managers × tenure				
0	-0.011	0.011	-0.010	0.011
1-2	-0.002	0.007	-0.001	0.007
3-4	-0.007	0.009	0.001	0.010
5	0.001	0.010	-0.008	0.013
>10	0.020	0.013	-0.007	0.019
Manager-co-worker interactions:				
Female top × female co-workers × tenure				
0	0.016	0.014	0.022	0.014
1-2	-0.0004	0.009	-0.013	0.012
3-4	0.001	0.013	-0.030*	0.016
5	-0.010	0.014	-0.038*	0.020
>10	-0.037**	0.017	-0.005	0.030
R^2	0.93	-	0.94	-
Observations	3,529,925	-	4,693,533	-
Establishments	13,404	-	13,424	-
Year dummies	yes	yes	yes	yes
Industry dummies (2-digit)	yes	yes	yes	yes
Worker dummies	yes	yes	yes	yes

Notes: *, ** and *** denote statistical significance at the 10, 5 and 1 percent levels, respectively. Standard errors robust for clustering at the establishment level are shown in parentheses. Apart from the controls reported in the table, all regressions control for age, age^2, education dummies (6 categories), experience and tenure dummies (0, 1-2, 3-4, 5-10, and >10 years), log(workplace size) and a dummy indicating whether the individual works in an establishment in the public sector. All variable shares shown in the table are mean-centered.

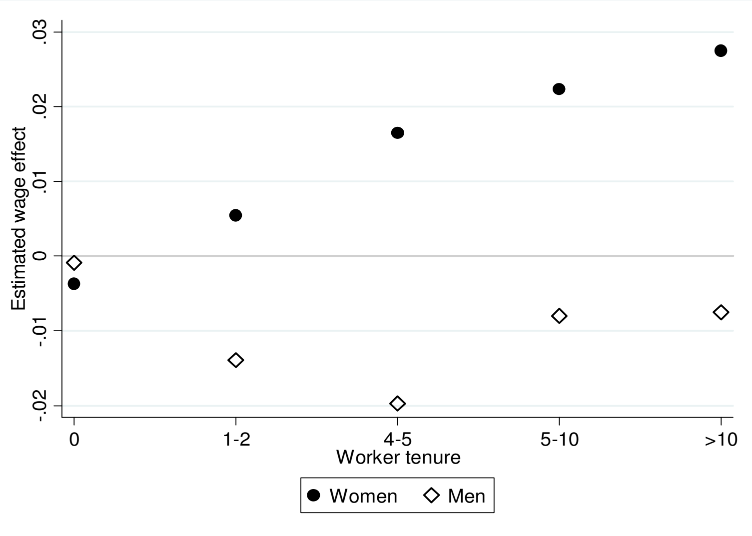

Figure A3 Impact of female middle managers on male and female wages for employees without female co-workers.

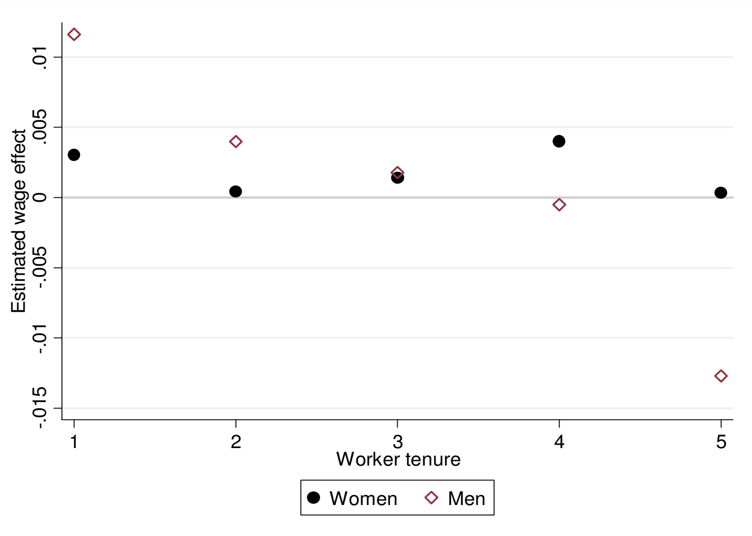

Figure A4 Impact of female middle managers on male and female wages for employees with 50 percent female co-workers.

Notes: The estimates are obtained from separate regressions of female managers on wages for males and females. For full results and standard errors, see Table A6..

Essay 4: Businesses, Buddies and Babies: Fertility and Social Interactions at Work[♦]

Co-authored with Peter Nilsson

1 Introduction

When the payoffs from different actions are uncertain, decisions and experiences of peers may provide valuable guidance. Learning and mimicking may be especially important when actions are irreversible and erroneous choices are costly; in particular when own experiences are limited.

The timing of childbearing is an example of an action that fulfils these conditions. When deciding about the timing of childbearing women face a trade-off. Delayed motherhood is associated with higher risks of childlessness and adverse health outcomes for mothers and children (Mincer and Ofek, 1982; Royer, 2004; Miller, *forthcoming*). At the same time, childbearing constitutes one of the most costly types of career interruptions for women and postponing childbearing may have a large effect on lifetime earnings (Mincer and Polacheck, 1974; Albrecht et al., 1999; Bertrand, Goldin and Katz, 2010). In addition, uncertainty about the net benefits of childbearing at a particular point in time may generate an option value of waiting with childbearing (Iyer and Velu, 2006). Peers childbearing experiences can potentially dispel such uncertainty and lead to an increase in the contemporaneous birth-rate.

In this paper we study peer influences in reproductive behavior within the workplace using extraordinary rich panel data on monthly fertility decisions among 150,000 Swedish women and all of their co-workers over an eight-

[♦] We are grateful to Gerard van den Berg, Janet Currie, Gordon Dahl, Giacomo DeGiorgi, Liran Einav, Feliz Garip, Claudia Goldin, Matthew Jackson, Per Johansson, Lawrence Katz, Magne Krogstad Asphjell, Eva Meyerson-Milgrom, Enrico Moretti, Oskar Nordström Skans, Luigi Pistaferri, Olof Åslund and seminar participants at the ELE meeting in Uppsala IFAU, ESPE 2009 in Seville, EEA 2009 in Barcelona and the workshop in Demographic Economics in Mölle, SOFI, Århus, the All-California Labor Conference 2010 in Santa Barbara and the Stanford Labor Development Public Reading group for helpful discussions and comments. Part of this project was completed while the first author was visiting the Department of Economics at Harvard University. Both authors acknowledge financial support from the Tom Hedelius foundation and from FAS (dnr 2005-2007). All errors are our own.

year period. Co-workers may constitute a particularly relevant peer group when it concerns fertility-timing decisions.[1] First, information about the job specific consequences of childbearing may be difficult to obtain from other social networks or sources. Second, the similarity between co-workers and the day-to-day interactions also suggest that social influences could be important within this peer group. Finally, unlike many other types of actions, childbearing decisions are easily observable enabling workers to learn from the experiences of their co-workers through observational learning about choices and subsequent outcomes.

This is the first study assessing the influence of co-workers on fertility decisions, and few previous studies have used micro data to examine the role of social influences in fertility decisions for any peer group.[2] Unlike most previous studies focusing on social interactions and fertility decisions, we focus on timing of births.[3] This is partly because the nature of timing of childbearing facilitates identification, but also because we believe that the timing decision is the key margin where peer influences are likely to matter most in our context.

Two main econometric issues arise when attempting to identify the influence of peers' behavior on individual behavior (c.f. Manski, 1993; Moffitt, 2001). First, as peers may simultaneously influence each other, it is notoriously difficult to distinguish whether it is the individual that affects the group or the group that affects the individual. Second, because the workplace is a choice variable, women may sort into workplaces based on unobserved characteristics related to their fertility decisions. For example, family friendliness of jobs is a potentially significant determinant of many women's employment decisions (Herr and Wolfram, 2009), and friends and relatives are important channels for job search (Granovetter, 1995, Montgomery, 1991; Ioannides and Loury, 2004). Similarly, unobserved shocks that independently affect the timing of co-workers' fertility decisions could also lead to a spurious correlation. For example, correlations in co-workers' childbearing could simply proxy for changes in firm policy, or an increased risk of mass lay-offs etc., rather than true peer effects.

[1] In Keim, Klärner and Bernadi, (2009) subjects where asked to rank the importance of differing peer groups in terms of their influence on the subjects childbearing and family formation decisions. 35% percent stated that co-workers had an *important* or *very important* influence on their fertility intentions and family formation plans (compared to e.g. 39% for cousins and 12% for neighbors). The order of stated importance is partners, children, three closest friends, parents, siblings, parents-in-law, other relatives, cousins, colleagues, neighbors, and acquaintances. Note that these figures only reflect the part of the influence that the respondents are aware of themselves and not subtler influences that may influence behavior.

[2] Those studies that have used micro data either looks at interactions within developing countries (Bloom et. al., 2008, Manski and Mayshar, 2003; Munshi och Myaux, 2006), among very young women (Crane 1991; Case and Katz, 1991) or within families (Kuziemko, 2006).

[3] Although our focus is on timing of childbearing, we also provide suggestive results for impacts on completed fertility.

It is therefore important to make sure that the estimated peer effect is not simply reflecting a spurious correlation in co-workers behavior induced by endogenous sorting of workers sharing similar preferences or other unobserved determinants of childbearing across firms.

The detailed and high frequency longitudinal data and the focus on the timing of childbearing help us address these issues. First, the simultaneity problem is mitigated by focusing on the influence of co-workers past childbearing. While using lagged behavior of a peer group to identify the effects of social influences breaks the simultaneity in outcomes, it is in general not a fail-proof plan since it requires that the agents are not forward looking, or that the transmission of the social effect follows the assumed temporal pattern (Manski, 1993). In this context, the inherent random nature of the *exact* timing of conception (together with the monthly data on childbirths) allows us to relax the assumption of non-forward looking agents. It is arguably very difficult, both for the individual and the co-workers, to exactly predict when conception takes place. This key notion together with the possibility to consider a detailed lag-structure also allows us to form empirical predictions about the dynamic pattern that the estimated peer effects would follow if these were driven by correlated shocks and/or endogenous sorting.

The estimated effect of a co-worker's recent childbearing on own childbearing follows a distinct dynamic pattern. During the first 12 months following the birth of a co-workers child the probability of having a child is unaffected only to sharply increase after 13–24 months (9% increase) and then slowly decline. This dynamic pattern, which speaks against the generic sorting and correlated shocks hypotheses, is remarkably robust across specifications and subgroups and controls for non-parametric monthly duration dependence, time-effects, workplace size, regional unemployment rate, industry, and several important individual and co-worker characteristics.

It is still, however, possible that the correlations in fertility decisions simply reflect changes in unobserved circumstances affecting childbearing choices of all workers in a workplace. We cannot completely rule out this possibility, but we provide several additional important pieces of evidence that strengthen a causal interpretation of the results and our conclusions.

We first test whether the documented peer effects are related to the degree of similarity between the co-worker and the focal worker. In line with the literature on the formation of social ties we document stronger peer effects between "same-type" co-workers than "different-type" co-workers. Much more weight is put on the fertility decisions made by other female co-workers and co-workers who are close-in-age. However, we also find important asymmetries in this same-type pattern. For example, consistent with models giving weight to social status, employees are only affected by co-workers who have the same or higher, but not lower, educational attainments. We also find that while the number of previous children of the childbearing co-worker does not matter for first-time mothers, mothers with previous childbearing experiences are only influenced by co-workers having the same number of previous children. This finding is not only interesting in its own right but it also speaks against the alternative hypothesis of common workplace specific shocks since these must be parity specific in order to explain the observed effect.

We also consider three falsification exercises where we test if the worker is affected by *(i)* the contemporaneous childbearing of future co-workers, *(ii)* the childbearing of the true co-workers' siblings, and finally *(iii)* the childbearing of the co-workers employed in the same firm but in a different workplace. The individuals in these three "placebo peer groups" are likely to share many of the unmeasured attributes of the true co-workers and the focal worker, and are also likely to experience similar types of unobserved shocks. However, since they are not employed in the same workplace we do not expect them to influence the childbearing decisions of the focal worker unless our baseline effect is spurious. We find no evidence of any similar influences from these placebo peers.

Clustering in childbearing may arise because co-workers actions convey information about the cost/benefits of having children at a particular point in time (c.f. Bikhchandani, Hirshleifer and Welch, 1992, 1998; and Banerjee, 1992). Since most women experience significant career interruptions after childbearing, they may learn from co-workers about how to combine job and family, about manager reactions, or about institutional arrangements regarding parental leave.[4] Alternatively, clustering may arise because of network externalities, i.e. when the payoffs of childbearing directly depend on the childbearing of others (Schelling, 1960; Katz and Shapiro 1985, Arthur 1989). For example the utility of having children may increase because individuals want to conform to norms in the workplace[5], because they value joint parental leave[6] (Hamermesh, 2002), because of economies of scale (e.g. from coordinated childcare and the sharing of material expenses) or simply because people don't want to be left out from conversation among peers centered around children.[7]

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[4] A frequently suggested example of the importance of social learning concerns the role of dissemination of information about the use of modern contraceptives (c.f. Behrman et al 2001; Munshi and Myaux, 2006). In our case information about contraceptives is likely of limited relevance, but individuals may still benefit from social or observational learning for example about the pros and cons of childbearing at a particular time (Montgomery and Casterline, 1996).

[5] In the only study we know about where subjects were directly asked about the influence of peers in fertility decisions the authors concludes that with regards to e.g. co-workers "[…] one is either somewhat on the line and conforming, or one is deviant. Considerations about the timing of childbirth and the perception of […] own readiness often include this kind of evaluation" (Keim, Klärner and Bernadi, 2009; p.12).

[6] In Sweden mothers take 329 days of parental leave on average (which are fully financed through the social insurance system) during the first year of a child's life (RFV 2004:14)

[7] Note that the network externality effect needs not be positive. If employees compete for e.g. promotion opportunities within the workplace they may take strategic considerations into

While many studies provide evidence on the existence of peer effects in various contexts, few have provided insights about the underlying mechanisms.[8] In an extended analysis we provide an attempt to distinguish between the two broad mechanisms outlined above. More specifically, we argue that (*i*) the dynamic pattern of the fertility peer effect should differ under the two models. Under the social learning model the strength of the peer effect should increase with time as more information accumulates about the experiences of childbearing peers and the uncertainty about potential job-related costs diminishes. In addition (*ii*) the gains of learning from the experience of co-workers should be particularly high when uncertainty about the potential job-specific costs of childbearing is high. On the contrary, under the network externalities model the peer effects should be stronger the lower the uncertainty about job-related costs and benefits.

Based on these predictions we provide a simple test designed to shed light on the relative importance of social learning and network externalities in this context. We link information on manager tenure in each workplace, and assume that tenure of the manager is proxy for uncertainty about job-specific costs of childbearing.[9] Our main prediction is that if social learning (network externalities) is the dominant mechanism underlying the baseline effect the strength of the peer effect should be negatively (positively) correlated with manager tenure.

We find that peer influences on individual childbearing are much lower when uncertainty is high (new manager) then when it is low (tenured manager), but also that the pattern of the estimated peer effect differs with respect to uncertainty. When the uncertainty is high the impact of peers childbearing *increases* with time, when uncertainty is low the impact of peers' childbearing *decreases* with time (i.e. after the initial increase in fertility). These findings are robust to controlling for (3-digit) industry×time×region dummies and worker tenure and they suggest that both social learning and network externalities may influence childbearing decisions, but that network externalities seem to dominate in our setting.

Understanding the magnitude and the underlying mechanisms of fertility peer effects may have important policy implications. Economists and demographers have long investigated the sources and consequences of the strong fluctuations in fertility rates observed in many countries, finding that

account when deciding about whether and when to have children. This argument can be motivated by a human capital model where time out of work leads to loss of human capital, as well as by a signalling model where there is a penalty for being the "first-mover" in the workplace. Hence if individuals take the relative timing of childbearing into account it is easy to imagine how one worker's fertility can be very contagious within the workplace.

8 Exceptions include Mas and Moretti (2009); Bandiera, Barankay and Rasul (2005), Hesselius, Johansson and Nilsson (2009).

9 This could occur due to that manager tenure is correlated with e.g. re-organizations, risk of lay-off, change in policy, change in manager attitudes, change in manager knowledge about workers true productivity etc.

cohort size is related to labor market prospects, inequality and productivity.[10] In addition, prospects of accurately predicting the needs for daycare, schooling, and housing may be hampered by strong fluctuations in cohort sizes.

It has been suggested that strong enough social multiplier effects (Glaeser, Sacerdote and Scheinkman, 2003) can generate or at least exacerbate fluctuations in aggregate fertility rates (Kohler, 2000).[11] If attempting to reduce (or at least predict) such fluctuations it seems important to understand the underlying mechanisms. For example, if individuals only care about the decisions of others because they have something to learn about the cost/benefits of childbearing increased information may reduce fluctuations in fertility rates. On the contrary, if network externality effects instead dominate increased information about cost/benefits may result in as strong or even stronger social multipliers.

The rest of the paper is structured as follows. Section 2 lays out the empirical strategy, section 3 describes the data, Section 4 presents the results and Section 5 summarizes and concludes.

2 Empirical strategy

The aim of this paper is to assess whether co-workers timing of childbearing influences the individual childbearing decision. The two fundamental problems of empirically establishing the existence of such effects are the simultaneity problem (reflection problem) and the presence of unobservable factors directly influencing the fertility timing decisions of all co-workers. We begin this section by describing the baseline econometric specification we use and then discuss under which conditions fertility peer effects are identified empirically.

2.1 Empirical specification

Timing of fertility is an intrinsically dynamic decision. We model the individual fertility decision as a function of co-workers past childbearing. The empirical strategy follows the spirit of Kuziemko (2006) with some important modifications. To capture the dynamic pattern co-workers' fertility have on individual childbearing we estimate conditional linear probability models

[10] See e.g. Freeman (1979); Welch (1979); Easterlin (1975); Katz and Murphy (1992); Murphy and Welch (1992); Kohler (1997, 2001); Durlauf and Walker (1998); Higgins and Williamson (2002); Feyrer (*forthcoming*).

[11] Sweden displays large variation in fertility rates during the 20th century (see figure A1 in Appendix A, and Andersson (1996) and Hoem (1990) for further evidence). The total fertility rate is positively correlated with the business cycle, which has been suggested to be due to the tight link between the parental leave benefits and permanent employment (c.f. Björklund, 2006). Our results suggest that a substantial part of the strong fluctuations may be due to the interaction of these incentives and contagion effects.

which can be thought of as a linear approximation of a hazard model allowing for time-varying covariates, non-parametric duration dependence and time period effects (c.f. Allison, 1982).[12] Our baseline specification is:

$$
\begin{aligned}
Y_{ijtc} = \alpha_t &+ \beta_1(\text{Any co-worker had a child within 12 months})_{ijtc} \\
&+ \beta_2(\text{Any co-worker had a child within 13-24 months})_{ijtc} \\
&+ \beta_3(\text{Any co-worker had a child within 25-36 months})_{ijtc} \\
&+ X_{ijtc}\lambda + C_{ijtc}\delta + \eta_c + \varepsilon_{ijtc}
\end{aligned} \tag{1}
$$

where the dependent variable Y_{ijtc} indicates whether employee *i* in workplace *j* had a child in calendar month *c* at duration month *t*. α_t is month of duration dummies that non-parametrically control for the fact that the baseline hazard of childbearing varies dramatically over the fertility cycle (as clearly illustrated in Figure A2 and A3). The variables "Any co-worker had a child within 12, 13–24 or 25–36 months" are indicators for whether a co-worker had a child within 12, 13-24 and finally 25-36 months prior to month *c*.[13] X_{ijtc} is a vector of individual background characteristics (marriage status and education), C_{ijtc} is a vector of co-worker and workplace background characteristics such as the previous number of children to all co-workers, age distribution, gender and educational attainment composition, and dummies controlling for establishment size in 10 worker intervals. In some specifications we will also control for own tenure, sector (public/private), industry affiliation, regional location and the age of the establishment. Furthermore η_c is calendar time (year$\times$month) dummies that capture common macro shocks that influence fertility decisions and finally ε_{ijtc} is the error term. The reported standard errors are heteroscedasticity robust and adjusted for clustering at the workplace level.

The main parameters of interest in equation (1) is β_1, β_2, and β_3. The estimates of these parameters intend to capture the dynamic impact of co-workers' recent fertility decisions on the likelihood of childbearing in a

[12] We have also re-estimated the model using a Maximum Likelihood estimator. This provided qualitatively similar results.

[13] The variable "Any colleague had a child within 12 months" counts from *t-1* to *t-12*. Hence by construction the dummy takes on the value zero if the colleague delivered in the *same* month as the individual. This implies that we avoid the possibility that two colleagues having a child together show up as one of them responding to the other. It is important to note that peer effects may arise not only from if any co-worker recently had a child, but also from the share of co-workers who had a child. Empirically, since we focus on small and medium workplaces, this is not going to make much of a difference. In the robustness checks we do however provide evidence on this from regressions where we interact the baseline exposure variables with a dummy indicating if more than one co-worker gave birth to a child within the same time period.

specific month. Our main analysis focus on how co-workers' childbearing affects the timing of first births since the variation in timing is largest for these births, but we also report estimates for higher order births. We estimate equation (1) for individual at risk of having the first, second and third child separately using OLS.[14] For first births the duration dependence is accounted for by "months since age 20"– specific indicator variables up until the first birth (or until censoring) and for higher order births the number of months from the previous birth. Note that the combination of the duration dummies (months since age 20) and calendar time effects also accounts for general cohort effects.

2.2 Threats to identification

The parameters of interest are identified under the assumption that the timing of co-workers' childbearing is uncorrelated with omitted variables affecting individual childbearing, after controlling for month of the fertility cycle specific effects, calendar time effects and time-varying individual and co-worker characteristics.

When could this assumption be violated? Changes in labor market conditions could change the individuals' and the co-workers' fertility decisions simultaneously. Much of this variation in labor market conditions will be controlled for by the year×month dummies and the yearly regional unemployment rate. In some specifications we also control for year×month×region×industry dummies. However, common shocks at the firm and/or workplace level, such as increased risk of lay-offs, policy changes etc., that change the probability of childbearing for all co-workers could also violate our key identifying assumption. Additionally, if workers sort into workplaces based on unobserved characteristics e.g. childbearing preferences, we may find a spurious correlation between childbearing of co-workers' and the focal worker. Even though we are controlling for many important co-worker characteristics related to timing of childbearing (average number of children, share in fertile ages, share close-in-age (±4 years), share of co-workers with college education, share females, share married), individuals may still end up in the same workplace and have children at approximately the same time for unobserved reasons, despite that they are not influenced by each other directly.[15]

[14] During our observation period higher order births are uncommon.

[15] A simple but unfeasible path to follow in order to try to control for workers sorting would be to add workplace fixed effects to equation (1). However, considering that we have a panel stretching only over 8 years and that we include lagged dependent variables for up to 36 months (which would be what the "co-worker had a child" dummies would be characterized as in a within-workplace analysis) the within-workplace estimates would, as is well known, be severely downward biased using an OLS estimator (Nickell, 1981). An alternative way to solve this problem would be to aggregate the data to the workplace level and then run regres-

To get a first sense of the potential severity of these basic and generic concerns we exploit the difficulty of foreseeing exactly when conception takes place and the longitudinal data to form predictions about how the estimates of β_1, β_2, and β_3 should behave if omitted factors are important. To see this clearly, suppose that two co-workers start trying to conceive at the same time (e.g. due to a change in firm policy). Due to the partly random nature of timing of conception some will conceive sooner than others. However, calculations in Kuziemko (2006) suggest the probability that individuals who start trying to conceive at the same time will end up having children more than 6 months apart is only around 14%. This implies that if unobserved common shocks are causing a spurious correlation between co-workers' fertility decisions then we expect the strongest effect to show up during the first 12 months period after the birth of a co-worker's child and then decline (i.e. $\beta_1 > \beta_2 > \beta_3$).

Furthermore if the estimates simply reflect endogenous sorting of workers across workplaces rather than peer influences then we expect the timing of co-workers' childbearing to be irrelevant. To make this clear, suppose that workers conceive independently of each other (i.e. no social influence) with some given probability each month. Then since there is an equal chance to have a co-worker who gave birth within 12, 13–24, and 25–36 months we would expect that $\beta_1 = \beta_2 = \beta_3$.

In the following sections we will see that our estimates do not match either of these predictions.[16] We also provide additional important pieces of evidence that help assessing the validity of our assumptions.

sions using a GMM estimator. But since an important focus of our analysis is to study in which way peer effects operate in relation to individual characteristics we feel reluctant to take this measure, and instead focus on other ways to make sure that the peer effects are not driven by endogenous sorting across workplaces.

[16] An example of a case when these baseline predictions would fail to fully rule out sorting is the case of staggered hiring and promotions. Assume that hiring take place in a staggered manner generating a uniform distribution of tenure in the workplace. Now combine this situation with workers having preference to have children just after some specific point in their career, for example after promotions. If promotions occur with regular intervals then it is possible to imagine a dynamic pattern different from the ones suggested above. In some specification we do control for tenure at the plant and 3-digit industry dummies, which should soak up much of this potential spurious variation in childbearing clustering. In addition, most of our results on the heterogeneous influences of peers, and the placebo peer group tests speak against this alternative hypothesis.

3 Data

The data we use come from the IFAU-database that contains various administrative registers covering the entire Swedish population aged 16–65. In addition to detailed individual background characteristics (LOUISE) the data contain firm and workplace identifiers (RAMS). From the "multi-generation" register we add data on the full history of births as well as the month of birth of each child. This allows us to construct our measure of co-worker fertility and our binary outcome variable; whether the focal worker gave birth to a child in a given month or not.

We restrict the analysis to female workers between age 20 and 44 employed in a workplace with less than 50 employees.[17] We focus on women first of all because their fertility cycle is well-defined, but also because childbearing among women is associated with significant career interruptions. This restriction does not apply to the co-workers. That is, the analysis looks at the impact of both male and female co-workers' fertility on female workers fertility. The size restriction is important since it allows us to focus on a well-defined peer group where interactions occur on a day-to-day basis.

We select a 50 percent random sample of women employed in 2004 and follow these eight years back in time (1997–2004). Hence, women are defined to be under risk of childbearing from 1997 through the end of 2004 as long as they are observed in a workplace, until the month when they give birth or until the month they turn 45.[18] To avoid including individuals who are only loosely connected to the workplace we retain only workers with yearly labor earnings above the 10th percentile.[19] For simplicity, for workers employed in multiple workplaces, we assume that the workplace giving the primary source of earnings also is the main arena for social interaction.

Because time until pregnancy as well as the social influence of peers may be different for women having their first, second and third child we consider up to three fertility spells. For women without previous children we define duration as the number of months from age 20 and up to their first birth (or

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[17] The medical literature defines the childbearing age as years falling between 15 and 44 years old. However for simplicity we restrict our sample to individuals who were above 20 years old. Our choice is motivated by the fact that due to compulsory schooling in Sweden it is very rare that individuals start working and having children before this age. In 2004 only 3.4 percent of Swedish women had their first child before their 20th birthday and the average age at first birth were 29 and 31 for women and men respectively in 2004 (National Board of Health and Welfare).

[18] Since we require that the individuals should be working we include them in our sample only those years that we observe them in a workplace. This restriction implies that we will over sample individuals with stable employment. However, note that almost all women in Sweden remain in employment after birth and hence attrition is therefore a minor concern.

[19] The threshold is based on all employees at the labor market, both males and females.

censoring), and for mothers with one child (two children) duration is defined as the number of months from their previous child birth up to the second (third) or until they are censored. Individuals are followed from when they became fertile (had their previous child) and as long as they are of fertile age between 1997 and 2004.

We combine this data with time varying information on the co-workers in the particular year, month and workplace and create indicators for whether any co-worker had a child in a specific month. We also add information on the age structure, sex composition, the share of co-workers with college education, workplace size, number of children of the co-workers, region of work and the sector (public/private) and 3-digit industry of employment.

Descriptive statistics for first, second and third order spells are reported in Appendix A, Table A1. In our sample, mothers to first-born children are, on average, 27.6 years old and employed by workplaces with 18 employees. The mean probability of having a child in a specific month is 0.005. The mean probability of having a second child is more than twice as high (0.011) reflecting that those who already have a child are much more likely to give birth to another child. The monthly probability of third child is only 0.002. These patterns reflect the two-child norm in Sweden.

As shown in Figure A2 in Appendix A the likelihood of childbearing for first-time parents in our sample peaks around age 30. This is somewhat higher than the average age (29 years), which is expected since our sample is restricted to women with a relatively strong connection to the labor market. Figure A3 suggests that the probability of delivering the second child peaks after 28 months (2.3 years) and that most parents (70 percent) had their second child within 6 years from their first child. [20]

[20] The main modification to the empirical strategy in Kuziemko (2006) is that we do not include individual fixed effects when estimating equation (1). In a duration model framework the closest equivalent of controlling for individual fixed effects is to exploit variation in timing of treatment across multiple spells and allow for individual specific baseline hazards. This approach may be sound when we can expect that the baseline hazard follows a reasonably similar pattern across spells, in which case controlling for the common baseline hazard across spells captures important unobserved determinants of the timing of exit. While this approach may be reasonable when it concerns e.g. unemployment or sickness absence spells, as clearly displayed in Figures A2 and A3, the baseline hazards of having the first and the second child are very different. Hence exploiting variation in timing of co-workers' childbearing across first and second birth spells is unlikely to provide a venue for identifying the impact of peer's childbearing decisions.

4 Do co-workers influence the timing of childbearing?

4.1 Main results

Column 1 of Table 1 shows the baseline estimates of the three β's from equation (1) capturing the impact of co-workers' childbearing on own fertility for first-birth women after controlling for duration dependence and calendar month fixed effects. The first, second and third row report the estimates of β_1, β_2 and β_3, i.e. the estimated impact of being exposed to a co-worker who had a child 1–12, 13–24 and 25–36 months ago respectively.

The estimates of the coefficients of co-worker fertility are robust across specifications. The estimates of β_1 are small and not significantly different from zero but still precisely estimated. In contrast the estimates of β_2 and β_3 indicate a positive (and declining) association between the focal workers childbearing and the past childbearing of her co-workers. The pattern of the parameters does not change when controlling for individual marital status and college education in column (2) and co-worker and workplace controls in column (3) (see Table A2 in Appendix A for all controls). The robustness of the estimates to the inclusion of these important covariates is reassuring since it suggests that bias due to omitted variables probably also are less of a concern.

Together the estimates suggest that the co-workers' fertility decisions primarily increase fertility with a lag of about one year after the birth of a co-worker's child. Evaluated at the mean probability of childbearing the full specified model suggests that individuals are on average 9 percent (0.00047/0.00523) more likely to have their first child 13–24 months after the birth of a co-worker's child. To put the estimates into perspective consider first that for example Del Bono, Weber and Winter-Ebmer (*forthcoming*) find that women are about 10% less likely to have a child in the first couple of years after losing their job. The 12-24 month effect is also comparable to increasing the focal workers age by one (1) year in the age interval 20 through 30 or equivalently decreasing ones age between ages 30 through 40. Kuziemko (2006) find that the probability of having a child within the first 24 months after the birth of a sibling's child increases by 17% on average.[21]

[21] We also replicated the baseline results in Kuziemko (2006) (not reported) for Swedish siblings and found almost identical sized effects. However, the methods used for estimating the peer effects among siblings by Kuziemko differs from ours in the current setting, complicating a direct comparison of the size of the effects for siblings and co-workers also see footnote 21. Interestingly the magnitude of the social effect is furthermore very similar to those found in recent studies also focusing on co-worker peer effects in general. For example, Mas and Moretti (2009); Falk and Ichino, (2006); Ichino and Maggi (2000) and Hesselius, Johansson and Nilsson (2009) all find co-worker peer effects which are in the vicinity of our estimates, but for very different outcomes.

Table 1 Baseline estimates of co-worker's fertility on timing of first birth.

	(1)	(2)	(3)
Any co-worker had a child within:			
1–12 months	0.00002	0.00003	0.00005
	(0.00007)	(0.00007)	(0.00007)
13–24 months	0.00057***	0.00056***	0.00047***
	(0.00007)	(0.00007)	(0.00007)
25–36 months	0.00029***	0.00028***	0.00013*
	(0.00007)	(0.00007)	(0.00007)
Duration dummies	Yes	Yes	Yes
Calendar time dummies	Yes	Yes	Yes
Individual characteristics	No	Yes	Yes
Workplace characteristics	No	No	Yes
Mean of dependent variable	0.00523	0.00523	0.00523
Observations	5,575,497	5,575,497	5,573,397

Notes: *,** and *** denote statistical significance at 10,5 and 1 percent level respectively. Standard errors robust for clustering at the workplace level are shown in parentheses. Calendar time is defined at the Year×Month level. Individual characteristics include civil status and a dummy for college education. Workplace characteristics include establishment size dummies in intervals of ten employees, the regional (county/year) unemployment rate where the workplace is located, the number of previous children in the workplace and the share of fertile, close-in-age, female, married and college educated co-workers.

The dynamic and consistent pattern across specifications and (as we show below) sub-samples suggests that common unobserved shocks is not driving these results. As discussed above if unobserved common shocks would induce individuals to start trying to conceive simultaneously we would expect to find the largest effect within the first 6 months. We do not find a significant increase in childbearing until 12–month after a birth of a co-workers' child. Similarly, as motivated above the pattern does not seem to be consistent with a situation where endogenous sorting of workers is causing a spurious correlation in the timing of pregnancy. Nor does it seem likely that co-workers plan their births so to be able to enjoy joint maternity leave.

We also explore the heterogeneity of the results with respect to individual characteristics (education, stage of the fertility cycle, and marriage status) and also report results for second and third order births. The estimates for these groups are strikingly similarity to the main results. Perhaps the two most interesting results from this analysis is that the peers seems to affect fertility decisions throughout the fertility cycle for first time-mothers, and that women under risk of having her third child also are influenced to some extent.[22] These results are important because they provide suggestive evidence that peers may not only influence the timing of childbearing but po-

[22] However, as will become clear below, second and third time mothers are only affected by births among women with the same number of children, and not at all by lower order births.

tentially also completed fertility rates.[23] For brevity the full results are reported in Appendix B.

4.2 Robustness checks

As a first specification check in column (1) of Table 2 we have re-specified the baseline model by replacing the three 12-month indicators of interest with six 6-months interval dummies. These estimates confirm that the baseline specification indeed seems to do a god job in modeling the dynamic impact of co-workers' childbearing on timing of fertility. The main impact shows up after 13–18 months and then declines until it turns insignificant after 31–36 months. Again, the absence of effects within the first 6 months strengthens the conclusion that omitted factors are not driving the estimated social effect. To further control for transitory unobserved shocks across regions (21 regions), 3-digit industries, and calendar time we add year×month×region×industry specific effects to in column (2). That is we now compare fertility decisions among employees in workplaces in the same 3-digit industry/region/calendar month with and without co-workers who recently had a child.[24] The estimates remain virtually the same.

Next we assess if increasing the dose of exposure matters; that is if the number of children born within each period matters. We do this by interacting the baseline variables of interest with dummy variables indicating whether more than one co-worker had a child 1–12, 13–24 and 25–36 months ago. The estimates in column (3) provide a clear dose-response pattern of being exposed to the childbearing of several co-workers; the interaction terms are positive and of significant size. Controlling for additional births does, however, leave the baseline estimates essentially unchanged suggesting that the main effect is not simply driven by exposure to many births. We therefore stick to the more parsimonious specification for the remainder of the analysis. We also assess the relationship between workplace size and the peer effect. The largest effects are found in the smallest workplaces and then decreases (although not necessarily monotonically with workplace size). The results from this exercise can be found in Appendix B.

[23] This interpretation hinges on the assumption that women in the later stages of the fertility cycle (aged 37-44) and mothers with two previous children (the average in Sweden) are "nudged" to have a (additional) child they would not otherwise have.

[24] The 3-digit industry classification is fairly detailed. As an example for the education sector this splits the sample into primary school, secondary school, higher education, and vocational school/adult education. In the manufacturing industry it distinguishes between workplace related to the production of rubber or plastic goods. In the hotel and restaurant business it distinguishes between workplace in the hotel, restaurant, camping, bar, and canteens/catering businesses. See http://www.foretagsregistret.scb.se/sni/040115snisorteradeng.pdf for full details.

As common shocks do not seem to explain the estimated peer effect we now investigate whether sorting of workers based on e.g. child-friendliness of the workplace is a valid concern. It is important to remember that even in the baseline model we control for number of previous children in the workplace, which to a large degree should capture selective sorting. Still it is possible that workers planning to have children systematically move to workplaces where childbearing is more frequent. As a first test of the validity of this concern we split the sample with respect to tenure and report the results separately in columns (3) and (4) of Table 2. Comparing the estimates we see that there are no major differences in the impact of peers on women with more and less than five years of tenure. If anything the effect seems to be somewhat stronger for women with longer tenure, suggesting that sorting into establishments just before planning a pregnancy is not driving our results.

Table 2 Robustness checks.

	(1)	(2)	(3)	(4)	(5)
Sample:	Baseline	Baseline	Baseline	< 5 years of tenure	≥ 5 years of tenure
Any co-worker had a child within:					
1–6 months	0.00010	0.00007			
	(0.00008)	(0.00008)			
7–12 months	0.00012	0.00017**			
	(0.00008)	(0.00008)			
13–18 months	0.00048***	0.00050***			
	(0.00008)	(0.00008)			
19–24 months	0.00028***	0.00028***			
	(0.00008)	(0.00008)			
25–30 months	0.00016**	0.00017**			
	(0.00008)	(0.00008)			
31–36 months	0.00005	0.00005			
	(0.00008)	(0.00008)			
12 months			0.00002	-0.00001	0.00029
			(0.00008)	(0.00007)	(0.00021)
13–24 months			0.00043***	0.00044***	0.00059***
			(0.00008)	(0.00007)	(0.00021)
25–36 months			0.00013	0.00011	0.00040*
			(0.00008)	(0.00007)	(0.00021)
Multiple births:					
12 months × 1(>1 birth)			0.00024**		
			(0.00012)		
13–24 months ×1(>1 birth)			0.00030***		
			(0.0001)		
25–36 months ×1(>1 birth)			0.00001		
			(0.00011)		
Duration dummies	Yes	Yes	Yes	Yes	Yes
Calendar time dummies	Yes	Yes	Yes	Yes	Yes
Calendar time × Industry ×Region	No	Yes	No	No	No
Individual char.	Yes	Yes	Yes	Yes	Yes
Workplace char.	Yes	Yes	Yes	Yes	Yes
Mean Y	0.00523	0.00523	0.00523	0.00523	0.00523
Observations	5,573,397	5,573,397	5,573,397	4,559,220	1,014,177

Notes: *,** and *** denote statistical significance at 10,5 and 1 percent level respectively. Standard errors robust for clustering at the workplace level are shown in parentheses. The level of analysis is the individual-month. Calendar time is defined at the Year×Month level. Individual characteristics include civil status and a dummy for college education. Workplace characteristics include establishment size dummies in intervals of ten employees, the regional (county/year) unemployment rate where the workplace is located, the number of previous children in the workplace and the share of fertile, close-in-age, female, married and college educated co-workers. The specification in column (2) additionally controls for Year×Month×Industry (3-digit)×County fixed effects and an indicator for public sector. 1(>1 birth) is a dummy variable equal to 1 if at least two co-workers gave birth to a child during the previous ≤12, 13-24 and 25-36 months.

4.3 Placebo co-workers

Next we re-estimate the specification in equation (1), but instead of focusing on the impact of the true co-workers, we now investigate whether the child-bearing behavior in three "placebo peer groups" also matter for individual childbearing. The placebo co-workers groups we consider are:

I FIRM-LEVEL CO-WORKERS: These workers are employed in the same firm, region (21 regions), and 2-digit industry, but not in the same workplace as the focal worker.

II FUTURE CO-WORKERS: This placebo-peer group consists of the future co-workers to the female employees in our sample that switch workplace during the eight-year observation window.[25]

III SIBLINGS OF CO-WORKERS: This placebo-peer group is likely to share many of the co-workers observed and unobserved characteristics. They have experienced similar upbringing and might therefore have formed similar preferences for the timing of childbearing.

These three placebo peer groups are likely to share many of the unobserved characteristics and experience the same type of unobserved shocks as the focal worker and the true co-workers. However, *a priori* we do not expect to find a correlation between childbearing in either of these placebo peer groups and the focal worker *unless* i) the baseline peer effect simply reflects a spurious correlation induced by correlation in unobserved factors that affect the timing of childbearing, or ii) they are directly influencing the focal worker. Table A3 presents descriptive statistics for the main sample and the three placebo peer groups.[26]

Table 3 presents the estimates from these falsification tests. Column (2) report the estimates for the first placebo peer group, "the firm co-workers", column (4) presents the results for second placebo peer group "the future co-workers", and column (5) shows the estimates for the third placebo peer

[25] To make sure that we capture actual job switchers we restrict the sample to women who switch jobs only once during the observation period and we require that the individual is observed for at least 2 years before and after the change in jobs.

[26] The observed characteristics of the true co-workers are all highly similar to the placebo peer groups. There are essentially two exceptions; the average number of co-workers in the average firm is naturally much higher than in the average workplace, and since the labor market is segregated with respect to gender the average share of females among the true co-workers is higher than that among the co-workers' sibling since this placebo group to a higher extent consist of brothers. In the empirical specification we address these differences by controlling for co-workers' siblings' characteristics and we also include nine dummies for firm size where relevant. Note that since the three placebo-peer groups are fairly balanced on observed characteristics it is reasonable to expect that they are similar in terms of unobserved characteristics too.

group “co-workers’ siblings”. In addition since the placebo-tests restrict the samples to women who work in private firms with more than one workplace in column (1) and to those who switch jobs in column (3) for comparison we also report the impact of the true co–workers childbearing in each of these samples. While the estimates for these true co-workers are highly similar to the baseline estimates in Table 1 neither one of the three placebo co-worker regressions provides results that are even close to the main results.[27]

If childbearing really is contagious then it is possible that the childbearing of siblings could influence the focal worker via the fertility decisions of the actual co-worker. In this case we would expect the effect to show up after the additional lag it takes for first the co-worker and then the focal worker to react. Alternatively, if the sibling, co-worker and the focal worker do not affect each other at all but just share unobserved determinants of timing of childbearing or if the sibling and the focal worker influence each other directly, we may find a spurious placebo co-worker effect that follows the same pattern as the baseline effect.

Consistent with this, the only estimate that is significantly different from zero in any of the placebo peer group regressions is the 25–36 month lagged effect in the co-workers’ sibling sample. To further assess this pattern we estimated a regression where we allowed co-workers’ siblings to affect childbearing decisions of the focal worker in 6-months intervals for up to 48 months. The results are presented in the lower graph of Figure 1 and for comparison we also show the 6-month interval estimates for the true co-workers in the upper graph.

The parameter estimates are small and insignificant for the first 30 months after a birth to a co-worker’s siblings but there is an effect showing up with a lag of 31–36 months, which then fades out slowly. This suggests that the fertility decision spills over *from* the sibling of the co-worker *via* the co-worker *to* the focal worker.

[27] One concern is that since the number of co-workers in the same firm can be much larger than the number of co-workers within the same workplace we have also estimated the “same firm different workplace” regression using only firm that have less than 50 employees in total. These estimates were very similar to the full placebo group sample estimates.

Table 3 Placebo co-workers.

	(1)	(2)	(3)	(4)	(5)
Sample:	Private firms with multiple work-places	Private firms with multiple work-places	Job switchers	Job switchers	All
Peer group	***True**: Same firm, same workplace*	***Placebo**: Same firm, different workplace*	***True**: Contemporary co-workers*	***Placebo**: Future co-workers*	***Placebo**: The true co-workers siblings*
Any co-worker had a child within:					
12 months	0.00012	0.00015	0.00026	-0.00003	0.00005
	(0.00016)	(0.00025)	(0.00021)	(0.00020)	(0.00007)
13-24 months	0.00067***	-0.00015	0.00072***	0.00015	0.00011
	(0.00015)	(0.00025)	(0.00021)	(0.00020)	(0.00007)
25-36 months	0.00019	0.00010	0.00032	0.00000	0.00031***
	(0.00016)	(0.00025)	(0.00022)	(0.00020)	(0.00007)
Duration dummies	Yes	Yes	Yes	Yes	Yes
Calendar time dummies	Yes	Yes	Yes	Yes	Yes
Individual char.	Yes	Yes	Yes	Yes	Yes
True co-work. char.	Yes	Yes	Yes	Yes	Yes
Placebo co-work. char.	No	Yes	No	Yes	Yes
Mean dependent variable	0.00503	0.00503	0.0058	0.0058	0.00523
Observations	1,066,052	1,066,052	729,767	729,767	5,403,084

Notes: *,** and *** denote statistical significance at 10,5 and 1 percent level respectively. Standard errors robust for clustering at the workplace level are shown in parentheses. The level of analysis is the individual-month. Calendar time is defined at the Year×Month level. Individual characteristics include civil status and a dummy for college education. Workplace characteristics include establishment size dummies in intervals of ten employees, the regional (county/year) unemployment rate where the workplace is located, the number of previous children in the workplace and the share of fertile, close-in-age, female, married and college educated co-workers. The specification in column (2) additionally controls for firm size using nine dummies (2–9, 10–19, 20–29, 30–39, 40–49, 50–99, 100–199, 200–499, >499 employees).

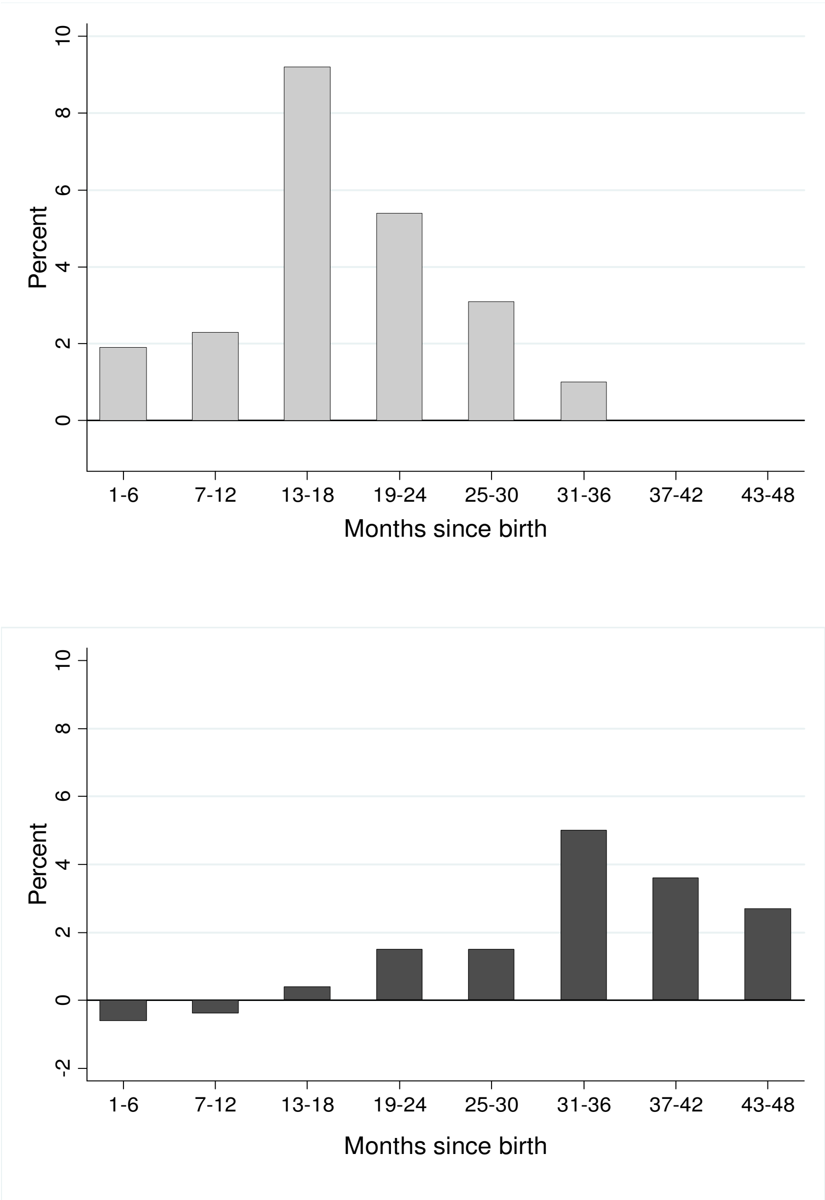

Figure 1 Impact of co-workers' childbearing (upper) and of co-workers' siblings' childbearing (lower).

5 Additional results

5.1 Who is influencing whom?

A large literature document that individuals are much more likely to form social ties with "same type" peers than "other-type" peers within social networks.[28] In this section we present results from specifications where we allow the response to co-workers' childbearing to vary with respect to the similarity between the childbearing co-worker and the focal worker. Specifically we estimate

$$\begin{aligned} Y_{ijtc} = \Omega + \gamma_1 & (\text{Any co-worker had a child within 12 months} \times \text{TYPE})_{ijtc} \\ + \ \gamma_2 & (\text{Any co-worker had a child within 13-24 months} \times \text{TYPE})_{ijtc} \qquad (2) \\ + \ \gamma_3 & (\text{Any co-worker had a child within 25-36 months} \times \text{TYPE})_{ijtc} \end{aligned}$$

Where Ω corresponds to the right hand side of equation (1) and TYPE is an indicator variable equal to 1 if any of the co-worker who had a child in the previous periods are male/female, close-in-age (±4 years), have similar educational attainment (college/no college), or have the same number of previous children as the focal worker, and zero otherwise.

The full set of estimates from this specification follows the familiar pattern of the baseline results and is reported in Table B2 in Appendix B. Figure 2 summarizes the key findings by reporting only the estimated impact 13-24 months after the co-worker child is born is reported (i.e. γ_2). The estimates in Figure 2 provide evidence that similarity, social status, and prior experiences all play distinct roles in the social transmission of fertility decisions in social networks.

[28] For a evidence of the relevance of homophily in social networks c.f. Currarini, Jackson and Pin (2009) and McPherson, Smith-Lovin and Cook (2001)

Figure 2 Increase in probability that an employee give birth 13-24 months after a same-type/different-type co-worker did. Point estimates of γ_2 are evaluated at the mean monthly childbirth probability along with 95% confidence intervals reported. For full results, see Appendix B Table B2.

First, for comparison, the main effect (9% increase) for the full sample is repeated in the first row. However, as row 2 and 3 reveals the entire baseline peer effect seem to be driven by the influence of female co-workers. If a female co-worker recently gave birth the chance of giving birth to a child 13-24 months later increase by 13.5%, while childbearing among male co-workers' partners does not influence childbearing of the focal worker at all. Closer connections among female co-workers and/or gender-specific learning are both possible explanations for this result. We always control for the share of same type co-workers in the workplace and hence female co-workers' stronger influence is not simply explained by gender-segregated workplaces. Hence, our estimates reflect the additional impact women have on each other given the potential number of female-female ties.

The influences of co-workers who are close-in-age (±4 years) are substantially stronger (22%, row 5) than the impact of those of other ages (3%, row 4), suggesting that the experiences of co-workers in a similar stage of the life-cycle are more important.[29]

[29] Remember that we always control for the stage of the fertility cycle using monthly duration dummies.

College educated women seem to be affected by other college educated co-workers (12%, row 9) but not by those with lower education (row 8). On the contrary women without college education is similarly affected by both college educated co-workers and co-workers without college education (12% and 10%, rows 6 and 7). These asymmetric patterns suggest that social status matters (Akerlof and Kranton, 2000) and they are in line with studies showing that behavior among higher but not lower ranking peers influences decisions in laboratory experiments (Ball et al. 2001; Kumru and Vesterlund, 2010).[30]

Similarly, mothers with previous childbearing experiences are 16% more likely to give birth 13-24 months after a co-worker *with* previous children (row 12), whereas the influence from co-workers without previous children is negligible (row 13). Women without previous children are on the other hand similarly affected by both same order *and* higher order births (10%) (row 10 & 11). One explanation consistent with this asymmetric pattern is that since higher parity women already have personal experiences of childbearing, the decisions and experiences of first time mothers do not generate any new information of value to the focal worker.[31] Alternatively co-workers with children may have had the chance to form stronger ties because of a similar family situation, and hence have stronger effects on each others decisions.

In addition, these patterns cannot easily be explained by alternative hypothesis, such for example workers "taking turns" of childbearing, in order to ensure an uninterrupted conduct of business. Assuming that leave-related costs are the same irrespectively of whether women are on leave with their first or second child, it is difficult to see why influences from first to second time mothers are completely absent. Furtermore, while workers with the same education level are likely to perform the same type of jobs these could potentially substitute for one another if one is having a child (and one of them wait with childbearing until the other worker is back), low education workers would have to substitute for high education workers, in order to generate the observed pattern.[32]

[30] This asymmetric pattern is unlikely to occur simply because individuals interact mainly with co-workers who have the same educational level. If so we would have expected both high and low educated women to primarily be influenced by their same type peers.

[31] For instance, mothers with one child might look at the behavior of their two-children peers to draw inferences of about the labor market consequences of having a second child, the organization of work and family with two kids, or the optimal timing of the second child.

[32] The parity and education specific fertility peer effects also provide important support for the validity of our main identifying assumption. The only unobserved shocks that could explain these asymmetric patterns are workplace specific shocks that only affect childbearing decisions among women with previous children (college education) but not women without children (without college education). On the contrary unobserved shocks that affect childbearing decisions among women without previous children (without college education) must always also affect women with previous children (college education). The standard omitted

5.2 Potential mechanisms: social learning or network externalities?

The potential mechanisms underlying the observed peer effect can be grouped into two broad mechanisms. The first is social learning, i.e. when co-worker's childbearing decisions reduce uncertainty about the costs and benefits of motherhood, for example by dispelling information about wage-growth and career prospects after childbearing.[33] Second, co-workers' childbearing could also have a direct impact on women's utility of having children. We refer to this mechanism as network externalities. An example of why this mechanism could be important is the sharp changes in time spent socializing with friends before and after childbearing.[34] If joint leisure time is valued, childbearing of co-workers could reduce the value of leisure and potentially lead to an increased desire to have children. Other examples include that individuals derive utility from conforming, joint parental leave or economies of scale (e.g. from coordinated childcare and the sharing of material expenses).

We attempt to provide some insight to the relative merits of these two very different explanations behind the fertility peer effect. A priori it is difficult with any certainty to know which one of these two distinctly differing explanation that is most relevant. In fact it seems likely that both mechanisms may play a role. We argue that (*i*) under the social learning model the strength of the peer effect should increase with time, as more information accumulates about the experience of childbearing peers and the uncertainty about job-related costs diminishes. As we have already seen this is not consistent with the results of the main analysis.

Second, (*ii*) the benefits of learning from the experience of co-workers should be particularly strong when uncertainty about the potential job-specific costs of childbearing is high. Under the network externalities model the influence of peer's decisions should increase when uncertainty about the potential job-specific costs is low. Hence, if we would have direct measures of uncertainty about job-related costs of childbearing we could examine how the estimated baseline peer effect varies with uncertainty. The prediction is that the strength of the peer effect should be negatively (posi-

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variables that we worry could lead to spurious correlations in fertility decisions within the workplace are not likely to generate these asymmetric patterns.

[33] See Wilde, Batchelder and Ellwood (2010) for a comprehensive study on the impact of timing of childbearing on wage-profiles in different groups.

[34] Cohabiting/married women with small children spend on an average day 30 percent less of their leisure time socializing with non-household members on an average day and over 50 percent less time on weekends compared to cohabiting/married women without young children. Statistics based on authors own calculations of the data from the Swedish time-use study 2000.

tively) correlated with uncertainty if social learning (network externalities) was driving the main results.[35]

We do not have a clean measure of uncertainty but we can link information on the tenure of the manager for about a third of the women in our sample. Manager tenure is likely correlated with e.g. the risk of re-organizations and worker turnover, changes in firm policies, changes in manager attitudes, and knowledge about workers true productivity. In the following exercise we therefore use manager tenure as a proxy for uncertainty about job-specific costs of childbearing. We also assume that as manager tenure increases, the uncertainty about the job-related costs of childbearing decreases.

To test the outlined predictions we estimate the baseline model separately for the women employed in workplaces with a new manager (less than four years of tenure) versus a tenured manager (four years or longer tenure). Table 4 reports the estimates from this exercise. In column (1), we report the baseline estimates for the sample where we can identify the manager's tenure.[36] These are very similar to the baseline results. Columns (2) and (3) show that the peer effects are much lower when uncertainty is high (new manager) compared to when it is low (tenured manager), but also that the dynamic pattern of the estimated peer effects differs with respect to uncertainty. When the uncertainty is high the impact of peers childbearing *increases* with time, when uncertainty is low the impact of peers' childbearing *decreases* with time (i.e. after the initial increase in fertility).

A potential concern is that high turnover firms will be overrepresented in the "new manager" sample. If women sort into more or less stable workplace environments depending on their childbearing preferences this could bias our results. In column (4) and (5) we therefore added information in individual

[35] In a future version of this paper we will provide a formal model of fertility timing decisions where uncertainty about the net benefits of childbearing are incorporated based on real options theory. The main implication of this model is that greater uncertainty about the net benefits of an investment opportunity generates an increased value of waiting to invest that a traditional net present value analysis does not take into account (Arrow, 1968, Dixit and Pindyck, 1994). The option approach will therefore delay the investment compared to what an NPV optimizing framework would predict. Inspired by Iyer and Velu (2006) we apply this model to the childbearing timing decision. In particular we assume that peers may, apart from directly affecting the net benefit of childbearing also reduce uncertainty about factors influencing the NPV. By reducing uncertainty, the option value of waiting decreases, which in turn increases the likelihood of childbearing at a particular point in time. Using this framework, we expect that the strength of the peer effects should be stronger when uncertainty is high, if the primary mechanism is social learning.

[36] To identify the manager we use occupational codes and information on ownership. The data contains information on detailed occupational status for all establishments in the public sector and for a sample of private establishments (see Essay 2 and 3 for more detailed information). Information on ownership is available for all establishments in the economy. We identify the manager using the following hierarchical criteria: (1) Owner, (2) Top manager and (3) Middle manager. In case that there are multiple managers at the same level, we assume that the manager is the individual with the highest wage. For sampling reasons manager tenure is defined as years at the workplace (truncated in 1985). The mangers have an average tenure of 5.9 years (sd 5.07).

and average co-worker tenure as well as year×month×region×industry dummies to mitigate this concern. Reassuringly, this does not change our conclusions.

From the results in Table 4 we conclude that both social learning and network externalities seem to influence childbearing decisions. However, since the peer effect is much stronger when uncertainty about job specific consequences of childbearing is low network externalities seem to be the dominant mechanism behind the clustering in fertility timing decision in our setting.

Table 4: Social learning or network externalities?

	(1)	(2)	(3)	(4)	(5)
Sample:	Baseline	Manager tenure ≤3 yrs.	Manager tenure >3 yrs.	Manager tenure ≤ 3 yrs.	Manager tenure >3 yrs.
Any co-worker had a child within:					
12 months	0.00013	0.00019	0.00010	0.00008	0.00010
	(0.00017)	(0.00026)	(0.00023)	(0.00030)	(0.00027)
13-24 months	0.00061***	0.00034	0.00083***	0.00017	0.00072***
	(0.00017)	(0.00025)	(0.00023)	(0.00029)	(0.00027)
25-36 months	0.00042**	0.00043*	0.00042*	0.00049	0.00052*
	(0.00017)	(0.00026)	(0.00023)	(0.00030)	(0.00027)
Duration dummies	Yes	Yes	Yes	Yes	Yes
Calendar time dummies	Yes	Yes	Yes	Yes	Yes
Individual char.	Yes	Yes	Yes	Yes	Yes
Workplace char.	Yes	Yes	Yes	Yes	Yes
Time×Industry ×Region	No	No	No	Yes	Yes
Own tenure	No	No	No	Yes	Yes
Average co-worker tenure	No	No	No	Yes	Yes
Mean Y	0.00544	0.00516	0.00564	0.00516	0.00564
Observations	921,811	377,110	544,701	377,110	544,701

Notes: *,** and *** denote statistical significance at 10, 5 and 1 percent level respectively. Robust standard errors clustered at the workplace level are shown in parentheses. The level of analysis is the individual-month. Calendar time is defined at the Year×Month level. Individual characteristics include civil status and a dummy for college education. Workplace characteristics include establishment 5 workplace size categories (<10, 10-19, 20-29, 30-39, 40-49), 5 workplace age categories (0, 1-2, 3-4, 5-10, >10), the regional (county/year) unemployment rate where the workplace is located, the number of previous children in the workplace and the share of fertile, close-in-age, female, married and college educated co-workers. Columns (4) and (5) include Year×Month×Industry (3-digit)×County fixed effects, own tenure (0, 1-2, 3-4, 5-10, >10 years) and average tenure among the co-workers.

6 Conclusions

Understanding individual fertility decisions have important policy implications. Economists and demographers have long investigated the sources and consequences of the strong fluctuations in fertility rates observed in many countries, finding that cohort size is related to labor market prospects, inequality and productivity. Previous studies have suggested that social interactions contribute to these fluctuations. Yet, only a limited number of studies have used micro-data to assess the actual magnitudes of fertility peer effects.

In this study we investigate how fertility decisions are transmitted in social networks. Specifically, we use unique matched employer-employee data and examine how recent births among co-workers affect the subsequent childbearing decisions among 150,000 Swedish women. We find that co-workers have a significant impact on the timing of childbearing; the average effect is comparable to increasing a woman's age by one year in the age interval 20 through 30. Consistent with the literature on the formation of social ties, same type peers are much more influential than other type peers.

Even though we non-parametrically control for monthly duration dependence, time-effects, workplace size, regional unemployment rate, industry, and several important individual and co-worker characteristics, we cannot fully rule out the possibility that unobserved workplace specific factors explain the effects. But we provide a number of robustness checks and specification checks to assess the validity of the identifying assumptions.

First, the estimated effects follow a distinct dynamic pattern which speaks against both sorting and correlated shocks and that is remarkably robust across specifications and subgroups. Second, we also consider three falsification exercises, which show that workers are neither affected by the contemporaneous childbearing of future co-workers, by the childbearing of the true co-workers' siblings, nor by the childbearing of co-workers employed in the same firm but in a different workplace.

Having established the existence of fertility spill-over effects among co-workers, we then investigate the mechanisms through which these effects arise. We show that the peer effects are much less pronounced when uncertainty about job related costs of childbearing is higher, but also that the dynamic pattern of the estimated peer effects differs with respect to uncertainty. When uncertainty is high the impact of peers childbearing *increases* with time, when uncertainty is low the impact of peers' childbearing *decreases* with time (i.e. after the initial increase in fertility). We argue that together these patterns indicate that network externalities are the most likely underlying explanation behind the fertility peer effect.

The existence of peer effects in such an important decision as the timing of childbearing clearly suggest that social influences may be relevant also for other types of career related decisions. If family choices have the tendency to spread within networks then such peer effects may be very important for understanding observed differences between men's and women's individual career choices and the organization of work and family. To uncover to what extent gender specific peer effects at work affect other labor supply related decisions such as exits from the labor force, moves to part-time work or the take-up of managerial positions are important and interesting questions for future research.

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Appendix A

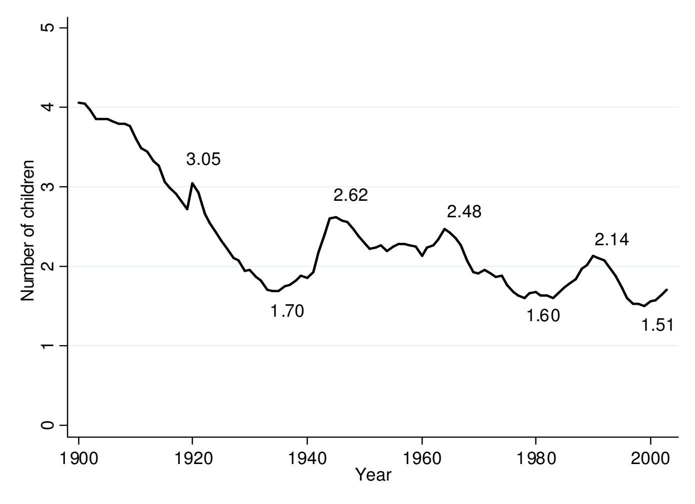

Figure A1 Total fertility rate, 1900-2003, *Source*: Socialstyrelsen (2005).

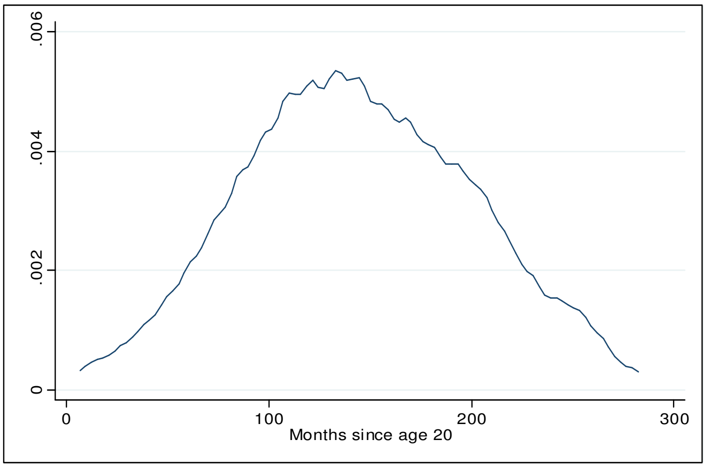

Figure A2 Smoothed baseline hazard of first births.

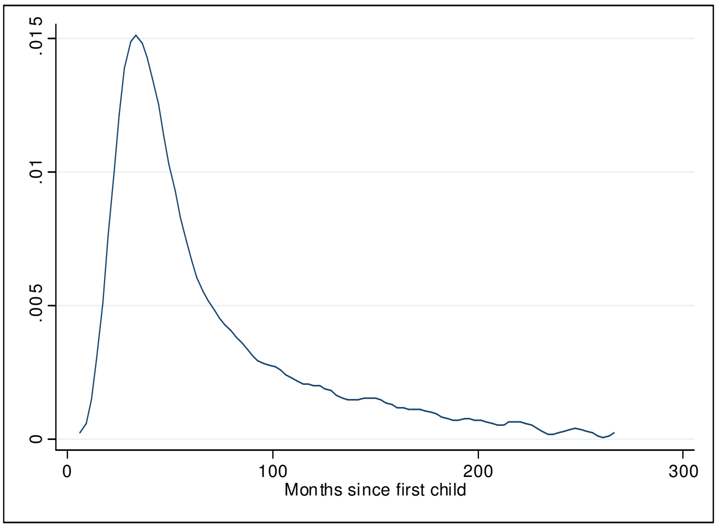

Figure A3 Smoothed baseline hazard of second births.

Table A1 Descriptive statistics.

Sample:	First birth		Second birth		Third birth	
	Mean	Sd	Mean	Sd	Mean	Sd
Had a child in current month	0.005	0.072	0.011	0.105	0.002	0.045
Age	27.6	5.4	32.5	5.1	35.3	4.3
College education	0.38	0.49	0.31	0.46	0.31	0.46
Number of children to co-workers	20.5	18.9	23.6	20.2	25.6	20.8
Share fertile co-workers	0.67	0.25	0.62	0.25	0.59	0.25
Share close in age co-workers	0.23	0.20	0.20	0.18	0.20	0.17
Share female co-workers	0.65	0.29	0.66	0.30	0.67	0.31
Establishment size	18.2	12.5	18.1	12.6	18.2	12.4
Public sector	0.27	0.45	0.34	0.47	0.40	0.49
Private sector	0.73	0.45	0.66	0.47	0.60	0.49
Observations	5,575,497		2,015,434		3,730,264	
Individuals	139,020		60,534		73,518	

Table A2 Baseline estimates of co-workers' fertility on the probability of first birth.

	(1)	(2)	(3)
Any co-worker had a child within:			
12 months	0.00001	0.00001	0.00004
	(0.00007)	(0.00007)	(0.00007)
13–24 months	0.00057***	0.00056***	0.00048***
	(0.00007)	(0.00007)	(0.00007)
24–36 months	0.00033***	0.00033***	0.00018**
	(0.00007)	(0.00007)	(0.00007)
Married		0.01184***	0.01177***
		(0.00016)	(0.00016)
College education		0.00034***	0.00030***
		(0.00008)	(0.00008)
No children to all co-workers			0.00005***
			(0.00000)
Share fertile co-workers			0.00017
			(0.00015)
Share close-in-age co-workers			0.00051***
			(0.00017)
Share female co-workers			0.00087***
			(0.00011)
Share married co-workers			0.00026
			(0.00016)
Share co-workers with college edu.			0.00034***
			(0.00012)
Duration dummies	Yes	Yes	Yes
Calendar time dummies	Yes	Yes	Yes
Own characteristics	-	Yes	Yes
Establishment characteristics	-	-	Yes
Mean Y	0.00523	0.00523	0.00523
Observations	5,575,497	5,575,497	5,573,397

Notes: *,** and *** denote statistical significance at 10,5 and 1 percent level respectively. Standard errors robust for clustering at the workplace level are shown in parentheses. The level of analysis is the individual-month. Calendar time is defined at the Year×Month level. Individual characteristics include civil status and a dummy for college education. Workplace characteristics include establishment size dummies in intervals of ten employees, the regional (county/year) unemployment rate where the workplace is located, the number of previous children in the workplace and the share of fertile, close-in-age, female, married and college educated co-workers.

Table A3 Descriptive statistics for true and placebo peer groups.

Sample:	Private firms with multiple workplaces		Job switchers		All	
	True: *Same firm same workplace*	***Placebo:*** *Same firm different workplace*	***True:*** *Current co-work.*	***Placebo:*** *Future co-work.*	***True:*** *All co-work.*	***Placebo:*** *Co-work. siblings*
Age	35.3	36.2	37.6	36.1	36.7	38.2
	(7.3)	(6.4)	(7.1)	(7.0)	(7.6)	(8.0)
Total # of	18.5	1,178	20.3	19.9	20.5	19.05
children	(16.4)	(2196)	(18.6)	(18.5)	(18.9)	(17.93)
Female	0.64	0.64	0.66	0.65	0.65	0.49
	(0.27)	(0.26)	(0.29)	(0.29)	(0.29)	(0.211)
Fertile	0.69	0.66	0.64	0.63	0.65	0.57
	(0.22)	(0.18)	(0.24)	(0.23)	(0.24)	(0.242)
High edu.	0.58	0.57	0.30	0.32	0.31	0.27
	(0.25)	(0.20)	(0.28)	(0.28)	(0.28)	(0.215)
Married	0.35	0.36	0.41	0.39	0.38	0.36
	(0.22)	(0.18)	(0.24)	(0.24)	(0.24)	(0.224)
This peer had a child within:						
12 months	0.39	0.81	0.34	0.39	0.36	0.36
	(0.49)	(0.40)	(0.47)	(0.49)	(0.479)	(0.480)
13-24	0.42	0.82	0.38	0.40	0.39	0.36
months	(0.49)	(0.39)	(0.49)	(0.49)	(0.488)	(0.479)
25-36	0.42	0.82	0.37	0.38	0.37	0.34
months	(0.49)	(0.38)	(0.48)	(0.49)	(0.484)	(0.472)
Obs.	1,066,052	1,066,052	730,356	730,356	5,575,497	5,385,787

Notes: High education is defined as having at least some college education. The co-worker characteristics are calculated at the individual-year level.

Appendix B: Additional results

This section provides a more detailed discussion and the full regression results summarized in Figure 2 in Section 5.1 in the main text. It also provides additional results with respect to the heterogeneity of the fertility peer effect depending on own characteristics, the degree of similarity between the focal individual and the co-workers and workplace size.

B1 The peer effect at different stages of the fertility cycle

We begin in Table B1 by investigating whether the peer effect differs depending on where the individual is in the fertility cycle. We divide the fertility cycle into an early (age 20–27), primary (age 28–36) and late (age 37–44) stage.[37] Columns (1)–(3) in Table B1 show that women are influenced in all stages of the fertility cycle and in fact most strongly towards the later stages.[38]

Since we do not have data on completed fertility for all workers in our sample, the distinction between pure timing effects and effects on completed family size is difficult. The fact that peers childbearing also influence women without previous children who are above their primary childbearing age does however indicate that social interactions may not only affect the timing of childbearing but also the decision of whether to have a child or not.[39]

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[37] Since we focus on women without any previous children the number of months under risk corresponds to their age.

[38] Evaluated at the mean, the estimates correspond to an increase in own childbearing of 7.3 percent in the early stage, 9.4 percent in the primary stage and 14.5 percent in the late stage of the fertility cycle.

[39] In terms of individual characteristics, we have also investigates whether the response to peers childbearing choices differs w.r.t civil status and education level. The former effect is ex ante ambiguous since on the one hand unmarried women may on average have less stable relationships making them unable to react as fast as married women can. On the other hand, married women may be less prone to be affected by outside influences if they already have made plans about the timing of childbearing. However, it is important to remember that more than 2/3 of the first time mothers are unmarried at the birth of the first child in Sweden, suggesting that marriage status perhaps is not such an important factor with respect to peer influences on childbearing. Evaluated at the mean probability of having a child we find no remarkable difference in the reaction to peers based on own marriage. We also found that the peer influence for women with college education is stronger than for those without college education. This results squares poorly with that the peer influence should be due to economies of scale associated with coordinated childbearing.

Table B1 Heterogeneous peer effects: fertility cycle.

	Dependent variable: Individual had her first child in month *t*		
	(1)	(2)	(3)
Sample:	Early (age 20-27)	Primary (age 28-36)	Late (age 37-44)
Any co-worker had a child within:			
12 months	-0.00004	-0.00009	-0.00013
	(0.00008)	(0.00025)	(0.00020)
13-24 months	0.00030***	0.00087***	0.00043**
	(0.00008)	(0.00019)	(0.00020)
24-36 months	0.00007	0.00032*	0.00033
	(0.00008)	(0.00019)	(0.00020)
Duration dummies	Yes	Yes	Yes
Calendar time dummies	Yes	Yes	Yes
Own characteristics	Yes	Yes	Yes
Workplace characteristics	Yes	Yes	Yes
Mean Y	0.00409	0.00921	0.00297
Observations	3,838,904	1,324,836	409,657

Notes: *,** and *** denote statistical significance at 10,5 and 1 percent level respectively. Standard errors robust for clustering at the workplace level are shown in parentheses. The level of analysis is the individual-month. Calendar time is defined at the Year×Month level. Individual characteristics include civil status and a dummy for college education. Workplace characteristics include establishment size dummies in intervals of ten employees, the regional (county/year) unemployment rate where the workplace is located, the number of previous children in the workplace and the share of fertile, close-in-age, female, married and college educated co-workers.

B2 Who is influencing whom? Gender, age and education

Table B2 presents the full results from estimation of model (2) described in Section 5.1. The estimates of the three β's are presented (which as before corresponds to the impact of any co-workers' childbearing), and in the bottom panel the estimates of the three γ's (which reflects the additional effect the childbearing of similar co-workers have). The total effect of a same-type co-worker is obtained by adding the main effect and the interaction effect.

First and foremost we find that the entire baseline peer effect seems to be driven by the influence of female co-workers (i.e. same sex). More frequent interaction among female co-workers and/or gender-specific learning are both possible explanations for this result. In our model we always control for the fraction of same type co-workers in the workplace so the stronger influence that female co-workers exhibit cannot be explained by tighter friendships with other women due to workplace gender segregation but rather that they associate more given the fraction of female co-workers in the establishment. The estimates reported in column (2) suggest that the influence of co-workers who are close-in-age is substantially stronger than from other co-workers; individual fertility increases with 10 percent within the first 12 months and 18 percent after 13-25 months.

We also look at the impact of co-workers with the same versus different educational level as the focal worker. Interestingly these estimates suggest that whereas highly educated women are affected only by other highly educated peers (column 3), low educated women are influenced by all co-workers regardless of educational level (remember that the total effect of same type co-workers in column (4) is the sum of the main effect and the interaction effect). If individuals interact mainly with co-workers who have the same educational level then we expect both high and low educated women to be primarily influenced by their same type peers. However, the asymmetric pattern we find w.r.t. the worker/co-worker education are in line with the literature on the importance of social status (Akerlof and Kranton, 2000), and with recent laboratory experiments suggesting that behavior by higher, but not lower, social ranking individuals are influential (Kumru and Vesterlund, 2010).

Birth order

The baseline results reported the peer effect for women at risk of having their first child. Here, we also examine whether co-workers also influence the timing of the second and third child. Since these women already had previous children they should have little use of further information from peers about the nature of childbearing. However, looking at second time mothers in column (6) of Table B2 see that the peer influence is almost as strong as for first time mothers. Moreover, for this group of women peers childbearing increases the propensity of giving birth even within 12 months after they had a child. This is not surprising since couples who already have had a previous child are likely to be able to react sooner than couples who are about to have their first child.[40]

Even for women with two previous children we find some weak evidence (a 5% increase within 13–24 months) of a peer effect as suggested by column (7). Besides the astounding homogeneity of the timing of the effect across the birth orders, the fact that also third-order births may be influenced again indicates that peers may potentially also shift the preferences for optimal family size. Women having their third child are reacting somewhat slower to peer influences than second order births which consistent with that Swedish couples generally decide to stop trying to have more children after the second child is born. Hence, the time it takes women to re-negotiate the views of the optimal family size with partners may perhaps delay and mute any response to the influences of peers. This notion is also supported by the fact that the estimate for the 25-36 month interval for the third order births is only slightly lower than the 13-24 months estimates, while the differences between the same two coefficients for the first and second order births are considerably larger.

In the last three columns of Table B2 we look at whether individuals are differentially affected by co-workers who have the same number of previous children. This could be the case if there is some type of information that is unrelated to the childbearing experience in general but specific to the birth order of the child. For instance, mothers with one child might look at the behavior of their two-children peers to draw inferences of about the labor market consequences of having a second child, the organization of work and family with two kids, or the optimal timing of the second child. Another plausible alternative is that co-workers who already have a child have formed tighter bonds with the co-workers who already have a child.

The estimates in columns (8)–(10) are estimated using the model in equation (2), where TYPE now is equal to 1 if the co-worker who just gave birth previously had the same number of children. We find that first-time mothers are influenced by all childbearing co-workers' irrespectively of the birth order of the co-worker's child (column 8). In contrast, second and third time mothers (Columns 9 and 10), are only influenced by co-workers with the same number of previous children.

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[40] We have also estimated this model using 6-months intervals. The estimates from this more flexible specification show that the entire within 12 month effect is driven by women giving birth between 7 and 12 after the birth of a co-worker's child [est.: 0.00068 (std.err.: 0.0002)]. These estimates are retain for expositional purposes but are available upon request from the authors.

Table B2 Heterogeneous peer effects: Gender, age, education and birth order.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
SAMPLE:	Same sex (female) co-workers:	Close in age (± 4 years) co-worker:	Same education co-workers: College	Same education co-workers: No College	1st birth	2nd birth	3rd birth	1st birth	2nd birth	3rd birth
12 months	0.00007	-0.00031***	0.00011	-0.00035**	0.00004	0.00044**	-0.00005	0.00001	0.00020	-0.00007
	(0.00010)	(0.00008)	(0.00015)	(0.00016)	(0.00007)	(0.00017)	(0.00005)	(0.00012)	(0.00019)	(0.00006)
13–24 months	0.00016	0.00009	0.00011	0.00063***	0.00048***	0.00083***	0.00010*	0.00047***	0.00023	0.00009
	(0.00011)	(0.00008)	(0.00014)	(0.00017)	(0.00007)	(0.00017)	(0.00005)	(0.00011)	(0.00019)	(0.00005)
24–36 months	0.00000	-0.00014*	0.00005	-0.00021	0.00018**	0.00033**	0.00008	0.00024**	-0.00009	0.00007
	(0.00011)	(0.00008)	(0.00014)	(0.00017)	(0.00007)	(0.00017)	(0.00005)	(0.00011)	(0.00019)	(0.00005)
This type of co-worker had a child within:										
12 months	-0.00000	0.00088***	-0.00005	0.00052***				0.00003	0.00029	0.00022
	(0.00012)	(0.00012)	(0.00019)	(0.00017)				(0.00013)	(0.00028)	(0.00028)
13–-24months	0.00047***	0.00107***	0.00058***	-0.00011				0.00000	0.00151***	0.00040*
	(0.00012)	(0.00012)	(0.00018)	(0.00018)				(0.00012)	(0.00025)	(0.00022)
24–36 months	0.00026**	0.00096***	0.00042**	0.00034**				-0.00009	0.00104***	0.00040**
	(0.00012)	(0.00012)	(0.00018)	(0.00017)				(0.00011)	(0.00024)	(0.00019)
Dur. dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Calendar time	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Own char.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Workpl. char.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean Y	0.00523	0.00523	0.00562	0.00498	0.00523	0.01105	0.00202	0.00523	0.01105	0.00202
Observations	5,575,497	5,575,497	2,140,535	3,432,418	5,573,397	2,015,434	3,729,137	5,573,397	2,015,434	3,729,137

Notes: *,** and *** denote statistical significance at 10,5 and 1 percent level respectively. Standard errors robust for clustering at the workplace level are shown in parentheses. The level of analysis is the individual-month. Calendar time is defined at the Year×Month level. Individual characteristics include civil status and a dummy for college education. Workplace characteristics include establishment size dummies in intervals of ten employees, the regional (county/year) unemployment rate where the workplace is located, the number of previous children in the workplace and the share of fertile, close-in-age, female, married and college educated co-workers.

Network Size

In this section we examine if the observed fertility peer effect varies with respect to workplace size. The peer effect may differ by workplace (network) size either because the true fertility peer effect differs between workplaces with different size, or because co-workers interact differently within different sized workplaces.[41]

To explore the relevance of network size effects in this case we divided the sample into three groups based on the number of employees and estimated a separate regression for each sample. These estimates are reported in Table B3. As seen in columns (1)–(3) the largest estimated peer effect is found in the smallest workplaces (2–10 employees, 15%) and in the largest workplaces considered (30–49 employees, 9%). The smallest peer effect is found in medium sized workplaces with 10–29 employees (7%). This u-shaped pattern with respect to workplace size is further reinforced when dividing the sample into smaller size brackets (2–9, 10–19, 20–29, 30–39, 40–49); the marginal peer effect remains strongest in the smallest and largest workplaces and lowest for the medium sized workplaces with 20-29 employees (not reported).

One explanation consistent with the seemingly u-shaped workplace size pattern is that while the precision of our network measure *decreases* with workplace size, the frequency of exposure to co-worker childbearing *increases* with workplace size. Hence, as the network size becomes larger the cumulative influence of multiple births among co-workers potentially dominates the decreasing "network precision" effect. This is further consistent with the dose-response pattern we found in Table 2; more exposure implies stronger peer effects.

Alternatively, individuals may interact differentially within different sized networks. For example, on average the number of social ties and the tendency to associated disproportionally with "same-type" peers increases with network size (c.f. Currarini, Jackson and Pin, 2009; Weinberg 2007). Hence, when network size increases the possibility to form more ties with individuals of the same type also increase and the strong within-type specific peer effects (reported in Figure 2) could potentially dominate the negative "network precision" effect.[42]

[41] Note, however, that it is a priori not possible to determine the direction of the bias if for example the true peer group consists of a smaller subset of workers within each workplace (c.f. Manski, 1993).

[42] We also investigated if the marginal peer effect differs with respect to workplace sector. If employees take into account the costs of maternity leave imposed upon the establishment when deciding about own childbearing we would potentially see a weaker peer influence in the for-profit sector. The effects are not significantly different from each other (not reported). It should be noted that the direct costs for employers associated with maternity leave in Sweden is zero and thus the only costs upon the establishment is indirect costs related to e.g. temporary human capital loss and labor substitution.

To explore whether more exposure or more homophily can explain the observed u-shaped peer effect pattern with respect to workplace size we re-estimated the model and included an indicator for if more than one co-worker gave birth 1–12, 13–24 and 25–36 months ago to control for differences in exposure between the different sized workplaces. As shown in the three last columns in Table B3, including dummies for more than one birth, if anything, reinforces the u-shaped pattern. Thus at least it seems as if higher exposure to births cannot explain why the peer effect is stronger in larger workplaces than in middle-sized, instead suggesting that workers in large workplaces have more ties and/or more same-type ties.

Table B3 Workplace size.

	(1)	(2)	(3)	(4)	(5)	(6)
Workplace size:	2-9	10-29	30-49	2-9	10-29	30-49
1-12 months	-0.0002	0.0001	0.00002	-0.0003*	0.0001	-0.0001
	(0.0002)	(0.0001)	(0.0001)	(0.0002)	(0.0001)	(0.0002)
13–24 months	0.0008***	0.0004***	0.0005***	0.0009***	0.0002**	0.0004**
	(0.0002)	(0.0001)	(0.0002)	(0.0002)	(0.0001)	(0.0002)
24–36 months	-0.0001	0.0001	0.0002	-0.00005	0.0002	0.0001
	(0.0002)	(0.0001)	(0.0002)	(0.0002)	(0.0001)	(0.0002)
Duration dummies	Yes	Yes	Yes	Yes	Yes	Yes
Calendar time	Yes	Yes	Yes	Yes	Yes	Yes
Individual.char.	Yes	Yes	Yes	Yes	Yes	Yes
Workplace characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Dummy for more than one child	-	-	-	Yes	Yes	Yes
Mean of Y	0.00512	0.00524	0.00535	0.00512	0.00524	0.00535
Observations	1,760,442	2,664,386	1,148,125	1,760,442	2,664,386	1,148,125

Notes: see Table B2

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