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Wages and their impact on individuals, households and firms

Anna Thoresson

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Abstract
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Essay I: This paper studies how wages respond to employer concentration. It exploits a reform that deregulated the Swedish pharmacy market, which until 2009 was a monopoly. The reform involved a substantial increase in the number of employers on the pharmacy labor market. However, the change in employer concentration was not geographically uniform: certain areas experienced large changes while others were largely unaffected. Exploiting this geographical variation, elasticities of wages with respect to labor market concentration are estimated to be between -0.02 and -0.05. The empirical approach relies only on the variation in concentration controlled by the policymaker to remedy the concern that actual labor market concentration is endogenous. The positive wage effects from reduced labor market concentration are found to be most prevalent for stayers, rather than new hires, as well as those with more industry experience and longer tenure. Overall, the paper adds to a growing literature that finds that market concentration matters for workers' wages, in a context where labor is highly industry-specific.

Essay II (with Olof Åslund, Cristina Bratu and Stefano Lombardi): This paper studies the role of firm productivity in explaining earnings differences between immigrants and natives in Sweden. We first show that firms with higher value added per worker pay higher earnings and document that immigrant workers are under-represented in high-productive firms relative to natives. Next, we estimate substantial positive earnings returns to working in more productive firms, with significantly larger returns for immigrants from non-Western countries. We also find that immigrants are less likely to move up the firm productivity distribution. Sorting into less productive firms thus decreases earnings in poor-performing immigrant groups that would gain the most from firm productivity. The results are consistent with firms having differential wage-setting power over immigrants and natives.

Essay III (with Erik Grönqvist and Lena Hensvik): We study the effects of introducing a performance-based promotion program for teachers in Sweden. The program intended to make the teaching profession more attractive by raising wages for skilled teachers and taking advantage of teachers' professional competence. Our results show that: (i) high-wage teachers are more likely to be promoted; (ii) the stipulated wage increase has full pass-through onto wages for promoted teachers; (iii) schools with promotions have lower teacher separations and an improved pool of teachers; (iv) the promotion program improved student performance. These results suggest that performance-based promotions could be an important tool for raising school quality.

Essay IV (with Erik Grönqvist and Lena Hensvik): This paper studies the impact of a substantial change in household relative wage on the reallocation of childcare time across parents. Our empirical strategy takes advantage of a promotion program for teachers, which led to a sudden and persistent 20 percent wage increase for the promoted spouse and a 32 percent decrease in the couple's gender wage gap (reflecting the higher promotion rate of the female spouses). Our findings suggest that female promotions lead to a more even division of childcare and that the change in bargaining power of the promoted spouse is a contributing mechanism behind the effect. Overall, these findings suggest that improved career opportunities for women can improve gender equality in the household.

Keywords: Wages, wage inequality, firms, competition, immigrant-native earnings gaps, career opportunities, teacher labor markets, household behavior, gender gaps

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Sofie's place, lobster rolls in Boston, and game nights at Daniel and Sofia's. The friendships we have made are indeed what I cherish the most. A special thank you to Lillit, Sofia and Sofie for the dinners and the chats. Lillit, it's been wonderful sharing an office with you. Whenever I've had doubts, you've always been there to give me an encouraging word.

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Contents

Introduction

Wages are an important determinant of individuals' well-being. They are an essential component of household income, and are a prerequisite for obtaining a decent standard of living. Still, wages differ considerably between groups: CEOs earn more than production workers, men earn more than women, and natives earn more than immigrants. Even workers who share the same characteristics earn substantially different wages. Moreover, in recent decades wages of low-income workers have been stagnating while wages of high-income workers have increased rapidly, widening inequality in many countries around the world.

To be able to explain these patterns requires an understanding of what determines wages and why wages differ across workers. Economists have a long history of trying to understand exactly this. The classic interpretation is that market forces determine wages. Wages are set such that labor supply equates labor demand for similar types of workers in similar types of jobs. In this interpretation, wages differ across workers for two main reasons. First, not all workers are equally productive. Workers that have higher so-called human capital are rewarded on the labor market for this in terms of higher wages. Second, jobs come with different amenities. Being a fishermen in winter is more dangerous than in summer, and wages for fishing move similarly. Already in the 18th century Adam Smith (1776) wrote about the idea that these unpleasant aspects of jobs can influence the wages that they pay.

While there is much merit to the classic interpretation – for example, more highly educated workers indeed tend to earn more – it does not capture all the complexities of reality. In the past decades there has been a shift among many economists to viewing labor markets as imperfectly competitive. They are types of markets that are riddled with frictions and information asymmetries. It is difficult to gather information about jobs, it takes time to search for a new job, and workers are normally restricted to working close to their home. This has repercussions for the wages that workers earn.[1]

In this thesis I dig deeper into some of the key determinants and consequences of wages. I study different aspects of wage-setting such as the role of competition between firms in chapter 1, the role of firm productivity in chapter

[1] There are many economic theories of wage determination that differ from the standard competitive model. These include efficiency wage theories (Shapiro and Stiglitz 1984), insider-outsider models (Lindbeck and Snower 2001), matching models (Mortensen and Pissarides 1999), and monopsony models (Manning 2003). Workers may also earn different wages due to discrimination, though this is normally modelled in a competitive model (see Becker 1957).

2, the role of institutions in chapter 3, and the impact that wages can have on household behavior in chapter 4. Workers can earn different wages depending on how many firms compete for their services, which I explore in chapter 1. The types of firms that individuals have access to can also drive differences in wages across groups, which I address in chapter 2. Moreover, wages matter for more than the consumption possibilities of workers. They can affect where people choose to work, something which I analyze particularly in chapter 3. They can also matter for how we choose to live our lives outside the labor market. As an illustration of this, chapter 4 studies how parents choose to allocate household chores – particularly, taking care of an ill child – when one person in the couple receives a wage increase. I proceed by outlining each chapter in more detail.

In the thesis' first chapter, titled **Employer concentration and wages for specialized workers**, I study a major reform to provide evidence on how increasing the number of employers affects workers' wages. The reform deregulated the Swedish pharmacy market. Prior to 2009, pharmacies were operated by a state-run monopoly. This monopoly was dismantled in 2009, after which entry was allowed into the market. At first sight, this involves a change on the product market: following the deregulation, both private firms and the former monopoly operate pharmacies. If, for example, profits fall as more firms enter and firms tend to share these rents with their workers, then we expect wages to fall following the deregulation. However, this type of market – and many others, like teaching or healthcare – is interesting because there is a tight link between the product market and the labor market. Put simply, the majority of pharmacists and pharmacy technicians work in pharmacies. This means that competition between firms for labor increases at the same time, which can have a counteracting effect on wages and push them up.

When firms have the power to set wages, we deviate from a setting where competition disciplines wage-setting. A topic that takes a central role in the first chapter is monopsony. In the strictest and classic sense, monopsony can be contrasted to monopoly – instead of one seller, there is one buyer. Pioneered by Robinson (1933), classic monopsony power is therefore closely linked to the number of employers on a market.[2] A lack of competition between firms means that workers are not as sensitive to wage changes as competitive models would suggest. This allows employers to set lower wages than they otherwise would be able to.

In the chapter, I isolate the effect of increasing labor market competition and indeed find that wages increase in response to the reduction in employer concentration. Empirical economists spend a lot of time and energy trying to establish causality: how can we know that the increase in the number of

[2] In more modern versions of the theory, that it takes time to search for new jobs (Burdett and Mortensen 1998; Manning 2003) or that workers' have heterogeneous tastes for jobs in ways which are not perfectly observed by employers (Card et al. 2018) are the key mechanisms explaining why firms have market power.

employers *caused* wages to increase, rather than something else, which may affect both the number of employers and wages? The "gold standard" in economics to obtain causal estimates, similar to medicine, are randomized experiments.[3] In absence of these, economists often look for natural experiments to estimate causal effects. In natural experiments we can compare one group who experienced a change to another group that did not. In the first chapter, the deregulation serves as such an experiment. I study pharmacy workers who all experienced the abolition of the national monopoly, but whose experiences of changes in their local labor market conditions differ. The change in the number of employers that the workers face will depend on where in Sweden they work: in some parts of the country, the increase in the number of employers was large, while in others, the increase was modest to non-existent. I am therefore able to compare workers that experienced different changes in the number of employers over time to obtain causal estimates of the effect of competition between employers on wages.

In a world where firms have wage-setting power, not only worker characteristics but also the place of work can matter for workers' wages. Previous research points to two empirical facts. First, wage differences between firms have increased over time (Card et al. 2013; Barth et al. 2016). Put differently, it has become increasingly important where you work for your take-home pay. Second, there is substantial ethnic workplace segregation (Hellerstein and Neumark 2008). However, we know relatively little about the repercussions of this for immigrant workers, and particularly for immigrant-native differences in earnings.

In the second chapter, titled **Firms, productivity, and the immigrant-native earnings gap**, co-authored with Olof Åslund, Cristina Bratu and Stefano Lombardi, we bridge this gap in the literature. Like in the other chapters in this thesis, we take Sweden as a case study, where we have access to rich matched employee-employer data. It is interesting to study for a number of reasons. First, Sweden is a heterogeneous country, thanks largely to the considerable and diverse immigration it has experienced in recent history. In 2020, the share of foreign born was 20%, nearly double what is was in 2000 (SCB 2020). Second, the data we use show that the immigrant-native earnings gap has increased by six percentage points between 1998 and 2017, the period we study. This change is driven by changes between rather than within firms. Understanding in which types of firms immigrants and natives work, and how earnings differ for immigrants and natives in these firms, are an important piece of the puzzle to understanding immigrant–native earnings differences.

In the chapter, we focus on a particular aspect of firms – namely firm productivity – and study how this translates into differences in earnings between immigrants and natives. In line with authors before us (see Card et al. 2018), we find that more productive firms pay more. We add to the literature by fo-

[3] For a historical review of empirical economics, see Angrist and Pischke (2010).

cusing on the relation between immigrant-native earnings differences and firm productivity. We find that immigrants are over-represented in low-productivity firms and are less likely to move to higher productivity firms than natives are. However, immigrants that manage to climb the productivity ladder experience larger gains in earnings than natives do. This is particularly true for groups of immigrants that tend to do relatively worse on the labor market: immigrants from non-Western countries as well as those that have arrived to Sweden more recently. Overall, our work shows that firm policies are a key contributor toward understanding why earnings differ between immigrants and natives.

The third chapter in the thesis shifts its focus to teacher labor markets. Teaching has historically been a female-dominated profession, especially at lower levels of education, characterized by low wages compared to other workers with comparable skills. While good teachers are key to students doing well in school and beyond (see Chetty et al. 2014), the relatively low and compressed wages can hamper the ability to attract and retain qualified teachers. Making sure that there are sufficient numbers of good teachers is also of high importance at a time when many developed countries are facing large retirement waves among teachers.

One way to make wages more aligned to the skills that teachers have are through promotions. In the third chapter, titled **Teacher career opportunities and school quality**, co-authored with Erik Grönqvist and Lena Hensvik, we study the effects of a performance-based promotion program. In 2013, the Swedish government introduced a new career step: the career teacher. If promoted, the teacher receives a substantial wage increase of SEK 5,000 (USD 520), representing a 15% to 20% increase compared to what the teacher earned prior to being promoted. Career teachers primarily continue to teach but also undertake additional tasks, such as mentoring their teacher peers and working with the school's pedagogical development.

By making use of an institutional feature – that the reform was rolled out across schools over time – we are able to study the effects of career teachers on teachers and students. Overall, introducing promotion opportunities has a positive impact on several important school outcomes. We find that providing additional career opportunities reduces the likelihood that teachers quit, both to other schools and out of the profession, and lead schools to retain a larger share of experienced and certified teachers. We also find that student performance improves in schools that introduced the career opportunity, though the changes are modest in size. More generally, the chapter shows how wages can influence where individuals choose to work. In the case of teachers, this can also have long-term repercussions for the students that they teach.

The final chapter of this thesis builds on the same reform studied in the third chapter but shifts its focus to the home. One of the most significant labor market changes in the past century is the rapid increase in female labor force participation. As Goldin (2006) puts forward, there is much more to this than an increase in hours worked. Particularly from the late 1970s onward, there has been a shift from women holding intermittent jobs to having careers, and from women being secondary earners in the household to actively making career decisions. Despite this significant shift, there is a persistent gender gap in earnings and wages that remains to this day. The gap is intertwined with the fact that women spend considerably more time doing household chores and caring for children than men do. This is true in many countries around the world, including in Sweden, which we study in this chapter.

In the fourth chapter, titled **Spousal earnings and household dynamics: Evidence from a promotion reform**, also co-authored with Erik Grönqvist and Lena Hensvik, we study how household decision-making with regards to taking care of children is affected when one parent – primarily the mother – receives a promotion. The wage increase that the promotion entails has the ability to influence time spent at home because the cost of not working is now higher. Moreover, as mothers are promoted, it may influence their bargaining position in the household. At the same time, gender norms may hinder a more even division of household chores. In the chapter we analyze the take-up of temporary parental leave (TPL, commonly referred to as *VAB* in Swedish) around the time of promotion to a career teacher. TPL is a benefit available in Sweden for working parents to temporarily care for ill children during work hours. While interesting to study in its own right, it also proxies well for how parents divide household chores more generally (Eriksson and Nermo 2010). To obtain causal estimates, we use the fact that promotions happened at different points in time for different parents, and compare how couples divide their time spent taking care of their ill children before and after promotion.

In line with our results in chapter 3, we find that promoted persons, relative to their partners, earn SEK 5,000 more following promotion. Because around 80% of the promoted teachers are women, this reduces the within-couple gender wage gap by a third among all promoted persons. Regarding take-up of temporary parental leave, we find that the couples respond to the new economic incentives. The promoted person reduces their take-up of leave relative to their partner once promoted. For women, promotions reduce the gender gap in temporary parental leave days taken by over half. Overall, the findings in the chapter suggest that, by improving career opportunities for women, gender inequality in the household can be reduced.

As a whole, the chapters in this thesis support the conclusion that wages are influenced by many factors beyond only human capital differences across workers. The place of work matters for wages, including labor market competition between firms. At the same time, access to good firms is unevenly distributed across workers. Moreover, institutional interventions on the labor market have the ability to impact the wages that individuals earn and the workplaces they choose to work in. They may also have repercussions beyond what was initially intended, such as affecting equality in the home.

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1. Employer Concentration and Wages for Specialized Workers

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1.1 Introduction

A recent and growing literature has revived interest in the idea that employers have wage-setting power. This research has sought to answer whether market power matters for workers' wages;[1] whether antitrust policy should focus more on judging labor market effects when reviewing mergers;[2] and to explain important labor market trends such as the falling labor share, stagnant wage growth and rising wage inequality by changes in product market power[3] or labor market power.[4] While there can be many sources of market power, a canonical source is market concentration. Market concentration relates to the existence of only a small number of sellers or buyers on a particular market. In the case of labor markets, having access to a limited number of employers can give employers the ability to depress wages below competitive levels.

In this paper, I focus on a particular market – pharmacies – and use a major policy reform to deduce quasi-experimental evidence on how increasing the number of employers affects workers' wages. Prior to 2009, only the state-run monopoly *Apoteket* could retail pharmaceuticals in Sweden. In 2009, entry barriers were removed and private firms could enter. *Apoteket* had to privatize two thirds of its pharmacies as part of the deregulation. Since pharmacists have highly industry-specific skills, the deregulation causes changes to the number of employers on the labor market. I exploit this setting to study how wages respond when labor market concentration falls. I rely only on the variation in concentration induced by the privatization of pre-existing pharmacies in 2009 to remedy the concern that actual labor market concentration is endogenous. This variation in concentration is controlled by the policymaker. I find that wages respond negatively to local labor market concentration, with estimated elasticities ranging from -0.02 to -0.05.

This paper studies a classic question in novel way. It uses a rare natural experiment that provides geographic variation in employer concentration coupled with rich matched employee-employer data. The setting is unique and interesting for a number of reasons. First, the deregulation resulted in substantial variation in employer concentration caused by a policy-decision. Second, product prices are mainly regulated and set by the state both before and after the reform, which make them independent of product and labor market concentration. The setting allows me to isolate the effects of changes to labor market concentration without confounding it with those related to product market concentration. Third, wage-setting in the industry is decentralized and wages are set in individual negotiations both before and after the deregulation.

[1] See Berger et al. (2019); Jarosch et al. (2019); Lamadon et al. (2019); Card et al. (2018); Benmelech et al. (2018); and Azar et al. (2020a).

[2] See Naidu et al. (2018) and Marinescu and Hovenkamp (2018). Naidu and Posner (2019) argue that, due to the frictional nature of labor markets, there are limitations to what antitrust policy can achieve.

[3] See Autor et al. (2020); De Loecker et al. (2020); and Barkai (2020).

[4] See Berger et al. (2019); Rinz (2020); and Lipsius (2018).

Fourth, the study uses high quality matched employee-employer data. I observe all workers inside and outside the pharmacy industry, including detailed information about the workers such as their educational level and educational specialization, as well as transitions they make on the labor market.

The paper consists of four main parts. In the first part of the paper, I present a simple model that relates wages to labor market concentration. The overall effect on wages of the deregulation is ambiguous *a priori*. When isolating the effects related to reduced labor market concentration, however, that model predicts that wages increase. The framework highlights that the overall effect depends on two channels: changes to labor market power and changes to product market power. A decrease in product market power is likely to decrease wages. This can, for example, arise if profits fall when there is rent-sharing. However, the increase in the number of employers can have a counteracting, and positive, effect on wages as monopsony power is reduced. Originally coined by Robinson (1933), monopsony in the strictest sense is a situation with only one buyer. In labor markets, it has more generally come to refer to a situation where individual firms face upward-sloping labor supply (Boal and Ransom 1997).[5] In contrast to competitive labor markets, firms are able to extract rents by setting wages below the marginal revenue product of labor.

The second part of the paper uses rich employee-employer data between 2004 and 2016 to characterize aggregate changes in the pharmacy industry, which is female dominated and highly skilled. The reform led to a large aggregate reduction in labor market concentration, measured by the Herfindahl-Hirschman Index (HHI), from 1 to 0.25. Wages for pharmacy employees increased on aggregate by 2 to 4 percentage points upon deregulation, relative to comparable workers in other industries. The number of pharmacies increased by around 50% and employment by 10%. Overall the market became more fragmented as employees split into more pharmacies operated by more firms.

The third and fourth parts of the paper include the main results. The third part estimates how labor market concentration affects wages. The change in labor market concentration induced by the deregulation differs across local markets. Making use of this geographical variation, elasticities of wages with respect to labor market concentration (or precisely, HHI) are estimated to be between -0.02 and -0.05. These results are consistent with previous studies (see for example Rinz 2020 or Hershbein et al. 2019). The effects materialize within two years of the deregulation and are relatively stable until the last period of observation in 2016.

[5]For a recent review of monopsony, see Manning (2020). Given the theoretical link between labor supply elasticities to the firm and labor market power, several papers have estimated the elasticity of labor supply to the firm in particular markets and found these to be far from perfectly elastic (see Sullivan (1989), Staiger et al. (2010) and Matsudaira (2014) for nurses; Falch (2010) and Ransom and Sims (2010) for teachers; and Dube et al. (2018) for the gig economy). This has been taken as support of that at least specific labor markets are imperfectly competitive, while being agnostic as to the actual source of labor market power.

The causal interpretation is supported by a battery of checks. Log wages evolve in parallel in local markets prior to deregulation and effects are not found in a related but unaffected industry. Only variation in labor market concentration from the sale of pre-existing pharmacies in 2009, the year the market was deregulated, is used to remedy the concern that actual labor market concentration is endogenous. This depends on the privatization of pre-existing pharmacies, a process controlled by the policymaker, and is neither affected by firms' decisions to open up new pharmacies nor to mobility decisions that workers make post-deregulation.

The fourth part of the paper studies which employees in the pharmacy industry benefit most from reduced labor market concentration. Stayers rather than new hires benefit with higher wages. That is, conditional on joining a new employer, the results suggest that there is no additional return to making the transition in a labor market with relatively low labor market concentration. This result should be interpreted with caution, however, as the likelihood of moving is itself affected by the deregulation. The positive wage effects are also estimated to be prevalent primarily for those with more industry experience and longer tenure. Individual characteristics do not appear to matter for the wage returns. Instead, similar positive wage returns from reduced labor market concentration are estimated for employees of different age, country of birth, gender, educational level and educational specialization.

The paper is related to three main strands of literature. First, it contributes to the literature that studies the effect of labor market concentration on wages. Whether there exists such a relationship is an old question, reviewed in Boal and Ransom (1997). More generally, this concerns imperfect competition in labor markets under the assumption that labor market concentration captures labor market power (see Manning (2011) for an overview). The question of whether labor market concentration affects wages, which according to Manning (2020) was originally studied by Bunting (1962), was revived recently by Azar et al. (2020a) and Benmelech et al. (2018), who estimate a negative relationship between these variables in a U.S. context.[6] Empirically, these papers tend to exploit broad changes in concentration at the region by industry or occupation level. A key concern is that many factors affect both market concentration and wages (Berry et al. 2019), and it is hard in data to identify exogenous shifts in concentration. This paper contributes to the literature by focusing on a particular market where there is a policy change that has market structure implications.

[6]This negative relationship has been confirmed in many other studies including Hershbein et al. (2019), Qiu and Sojourner (2019), Lipsius (2018), Rinz (2020) and Schubert et al. (2020) using U.S. data; in Martins (2018) using data from Portugal; and in Marinescu et al. (2020) using data for France.

A second yet scarce strand of literature uses variation from mergers to study how market concentration affects workers.[7] Arnold (2020) exploits merger-induced changes in concentration in the U.S. to find negative effects of increased labor market concentration on earnings when the change in concentration is large. A related approach is taken by Prager and Schmitt (2019) who focus on hospital consolidation only. They find a negative effect on wages when the change in concentration is large and worker skills are industry-specific.[8] While compelling, these papers suffer from the drawback that firms choose whether or not to merge. If firms merge for labor cost-saving reasons, then that could result in a spurious correlation between labor market concentration and wages. I take an alternative approach by focusing on changes in concentration induced by a policy change rather than a firm decision.

Third, the paper contributes to the literature on the wage effects of privatization and deregulation. In contrast to these papers, I isolate the wage effect of the regulatory change to that associated with changes in labor market concentration. The literature on how workers are affected by privatization is scarce. In two recent papers, Olsson and Tåg (2018) find increased unemployment incidence and duration while annual labor income and labor force participation remain unchanged for privatization events in Sweden, while Arnold (2019) finds that privatization lowered incumbents' wages substantially in Brazil. Regarding deregulation, this is often analyzed as a shock to profitability in a specific industry, which under rent-sharing would put downward pressure on wages. Peoples (1998) provides on overview of the wage effects of reducing entry barriers to specific industries. The literature predominantly finds negative industry wage effects of deregulation, including in the U.S. airline (Card 1998, Hirsch and Macpherson 2000), trucking (Rose 1987, Hirsch 1988) and banking industries (Black and Strahan 2001).[9] This literature is often based on cross-sectional data and is, unlike this paper, unable to control for unobserved worker characteristics. An exception is Lergetporer et al. (2018), who find negative wage effects for incumbent workers after lifting entry barriers in the German crafts sector.

Overall, the results in this paper underpin that an increase in the number of employers in an industry with specialized labor increases wages. The paper lends support to the literature that finds that labor market concentration can matter for workers' wages. The rest of the paper proceeds as follows. A theoretical framework is included in Section 1.2. Key definitions and data

[7] In a related paper Hensvik (2012) studies the relation between school competition and teacher wages in Sweden. Hensvik (ibid.) focuses on how public-school hiring and wages are affected by private entry, finding that wages respond positively to the increased competition.

[8] Currie et al. (2005) consider labor market effects of mergers for hospitals without linking this explicitly to changes in market concentration. They find increases in nurse effort but no wage effects when studying hospital consolidation.

[9] The deregulation literature also finds that reducing entry barriers leads to employment growth. Bertrand and Kramarz (2002) study entry deterrence in the French retail industry.

are described in Section 1.3. Institutional details and descriptive patterns are provided in Section 1.4. Section 1.5 outlines the empirical strategy, Section 1.6 provides estimates of the wage effects of reduced labor market concentration and Section 1.7 considers which employees benefit most from this reduction. Finally, Section 1.8 concludes.

1.2 Theoretical framework

1.2.1 Sources of labor market power

In imperfectly competitive labor markets, monopsony powers can stem from many sources. The quasi-experiment studied in this paper naturally lends itself to studying the effect of labor market concentration on wages. Market concentration relates to the existence of only a small number of potential employers; that is, labor markets are thin. Due to regulatory barriers, only one firm was allowed to operate in the pharmacy industry prior to the reform. Building on a tradition in industrial organization and in antitrust policy, the intuition is that firms are interdependent and take actions of other firms operating in the same labor market into account when making employment decisions. In such a world, it may be profitable for firms to hire fewer workers and thereby set lower wages than in a perfectly competitive world. This type of argument is emphasized by classic models such as Cournot oligopsony and in empirical work by Azar et al. (2020a) and Benmelech et al. (2018), among others. Recent theoretical work by Berger et al. (2019) provides a micro-foundation to the relationship between market power and market structure. Their model allows for a large but finite number of employers, and market power arises from the ability of firms to exploit the market-share dependent upward-sloping labor supply curves to the firm.[10]

While I focus on a particular source of labor market power – labor market concentration – there are sources of monopsony power that arise even with a continuum of firms. These sources include differentiation and preference heterogeneity (see Bhaskar et al. 2002 and Card et al. 2018), as well as search frictions (Burdett and Mortensen 1998). Search frictions are key to the seminal dynamic monopsony models by Manning (2003).

1.2.2 A Cournot oligopsony model

In this section, I outline a simple static Cournot oligopsony model that relates wages and labor market concentration. The framework follows Arnold (2020),

[10] Jarosch et al. (2019) develop a model where a different mechanism gives rise to a relation between market concentration and wages. Market power stems from employer size where each employer recognizes it can exploit its power by eliminating its own vacancies from the worker's outside option and thereby not compete with itself. In the model, employment is not directly affected by market structure. It therefore departs from the Robinson-style models.

who exploits mergers and acquisitions to study how labor market concentration affects workers.[11] There are F firms in a market m, indexed $f = 1, \ldots, F$ and total employment on the market is $L_m = \sum_{f=1}^{F} l_f$, where l_f is firm f's level of employment. For simplicity, labor is assumed to be the only input into production. Each firm maximizes its objective function by choosing its employment, l_f, taking the labor demand of other firms on the same market as given. The market wage $w_m(L_m)$ depends on total employment in the market. This is a posted wage; there is no wage bargaining in this model.

$$\max_{l_f} R_f(l_f) - w_m(L_m)l_f \tag{1.1}$$

$R_f(l_f)$ is the firm's revenue function. This will depend on product market factors, like price and quantity of goods sold, as well as productivity parameters. More generally, $R_f(l_f)$ can be interpreted as any concave function that is increasing in l_f. This covers the case of the public monopsonist that may have additional objectives beyond maximizing revenue minus cost. This leads to the following first-order condition:

$$\underbrace{\frac{\partial R_f(l_f)}{\partial l_f}}_{\Omega_f \equiv MRPL_f} - \underbrace{\left[w_m(L_m) + \frac{\partial w_m(L_m)}{\partial l_f} l_f\right]}_{MLC_f} = 0 \tag{1.2}$$

Notice that, in the absence of labor market power, the firm's labor decision would not affect wages and wages would be set to equal the marginal revenue product of labor (*MRPL*). The first order conditions can be re-written as follows:

$$\Omega_f - w_m(L_m)\left[1 + \frac{s_f}{\varepsilon_m}\right] = 0 \tag{1.3}$$

where $s_f = \frac{l_f}{L_m}$ is firm f's employment share in market m and ε_m is the market-level labor supply elasticity, $\varepsilon_m = \frac{\partial L_m}{\partial w_m(L_m)} \frac{w_m(L_m)}{L_m}$. Multiplying each side by s_f and then summing the first order conditions across all firms, we can rearrange to find an expression for market wages:

$$w_m = \underbrace{\left[\frac{\varepsilon_m}{HHI_m + \varepsilon_m}\right]}_{\sigma_m} \Omega_m \tag{1.4}$$

where $HHI_m = \sum_f (s_f)^2$ is the Herfindahl-Hirschman Index and $\Omega_m = \sum_f s_f \Omega_f$ is the employment-weighted average of the marginal revenue product of labor. HHI_m can take values in the interval $(0, 1]$ where values approaching 0 represent perfect competition and 1 represents only one employer in the market.

[11] Arnold (2020) decomposes the effects of mergers into three components: monopsony effects, market power effects and productivity effects. It draws on classic Cournot oligopsony results, among others outlined in Boal and Ransom (1997) and Naidu and Posner (2019).

Unlike in the competitive model, workers will only get a fraction (denoted σ_m) of the average marginal revenue product. The model implies that higher concentration is negatively associated with wages, holding all else constant. Moreover, if market power only stems from market concentration, then the wage approaches the competitive wage as the number of firms increases.

Channels through which deregulation affects wages

In the simple model outlined above, the deregulation may impact wages through changes in labor market power (operating through σ_m) and through changes in productivity, product market power or objectives as ownership shifts from public to private (operating through Ω_m). Taking logs, the average treatment effect of the deregulation on log wages $\widetilde{w}_m$ can be written as follows. *post* refers to post-deregulation and *pre* to pre-deregulation:

$$E[\widetilde{w}_{m,post} - \widetilde{w}_{m,pre}] = E[\widetilde{\sigma}_{m,post} - \widetilde{\sigma}_{m,pre}] + E[\widetilde{\Omega}_{m,post} - \widetilde{\Omega}_{m,pre}] \quad (1.5)$$

While decreases to labor market concentration will increase wages, decreases to Ω_m are, on the other hand, likely to put downward pressure on wages. To the extent that these two channels are correlated, I am likely to estimate lower bound effects of reduced labor market concentration on wages.

Focusing on the Ω_m component, increased product market competition will in general put downward pressure on prices, and under rent-sharing, also on wages.[12] While the price of prescription drugs, the dominant product category, is regulated, this is not the case for non-prescription drugs and retail items which represents around 25% of revenue. Indeed, earlier deregulation studies (Black and Strahan 2001) have used deregulation as a shock to profitability and find support for the rent-sharing channel as wages fall post-deregulation. Peoples (1998) highlights how labor earnings may fall after deregulation as the bargaining power of workers falls. Recalling that the reform considered in this paper also involves privatization and that the revenue function can be interpreted as any function that is increasing in l_f, Arnold (2019) finds that state-owned enterprises pay significant wage premiums over private firms, also suggesting that changes to Ω_m could put downward pressure on wages. This is consistent with state-owned enterprises having wider objectives than only maximizing profits (Haskel and Szymanski 1993). Indeed, trade unions often fear privatization will lower wages and much academic literature has assumed privatization has negative effects on wages.[13]

[12] The model in Section 1.2.2 implies that, if decreases to product market power increase employment, then wages also increase. This is not the case with wage bargaining, where lower revenue per worker results in lower wages.

[13] Even so, there is no general theory of how deregulation and privatization will affect wages, and certain product market channels could push wages up. Many papers find that efficiency and profits increase once state owned enterprises are privatized (see Megginson and Netter (2001) for a review), which could put upward pressure on wages if private firms share rents at least as

To isolate labor market power effects (captured by σ_m) from productivity or product market aspects (captured by Ω_m), a sufficient condition is that changes to Ω_m are independent of changes to σ_m, conditional on included controls. For example, if changes to labor market power are correlated with changes to product market power, and if higher product market power has a positive effect on wages, then I am likely to underestimate the effect of labor market concentration on wages. Similarly, if public monopsonists overpay compared to private monopsonists, then the state-to-market quasi-experiment that this paper relies on will also underestimate the effects of reduced labor market concentration. In support of this assumption, firstly note that decisions about product pricing and campaigns are normally taken nationally and the product ranges at pharmacies are relatively homogeneous (KKV 2013). The empirical strategy only exploits within-industry changes. Moreover, while pharmacies do sell products with unregulated prices, around three quarters of pharmacies' revenues are from products with nationally regulated prices. That product prices are regulated both before and after the reform make them independent of product and labor market concentration.

1.3 Definitions, data and sample

1.3.1 Definitions

In order to calculate concentration measures, it is necessary to define what a market is. The definition of the labor market should capture the set of potential employers for a worker. Because workers are tied to their workplace, labor markets tend to be local. In this paper, a local labor market (LLM) m is defined by the interaction of the industry for dispensing chemists and commuting zones (CZ). The industry for dispensing chemists is identified by workplace industry codes included in the data.[14] Commuting zones encompass all industries in a geographic area and are taken from Statistics Sweden who define CZs using commuting patterns.

In line with the theoretical framework, labor market concentration is measured using the Herfindahl–Hirschman Index (HHI) in employment shares. This measure captures concentration among pharmacies. For all workplaces that are operating as dispensing chemists, a unique workplace identifier is assumed to be a pharmacy. A firm is defined using firm identifiers provided in the data as a collection of pharmacies. HHI is calculated separately by year t and LLM m as the sum of squared employment shares s_f across firms in each

much as public firms do. Earle and Shpak (2019) summarize why wages may rise or fall as a result of privatization.

[14] Industry (SNI) code 52.310 is used until 2007 and 47.730 from 2008 onward. There is a one-to-one mapping between these codes. To remedy potential miss-classifications of workplaces that may occur especially around the time of the deregulation, I (iteratively) assume that a workplace is a dispensing chemist if it was classified as a dispensing chemist in the previous year.

local pharmacy market:

$$HHI_{mt} = \sum_{f=1}^{F} s_{fmt}^2 \quad (1.6)$$

An HHI approaching 0 corresponds to perfect competition while an HHI equal to 1 corresponds to a single employer.[15] A higher value means higher concentration and thus lower competition. HHI is a canonical measure of labor market concentration, used among others in Benmelech et al. (2018), Lipsius (2018) and Rinz (2020), who calculate HHI using employment shares, and in Azar et al. (2020a), who calculate HHI using vacancy shares. In addition, HHI is widely used in merger control (see the U.S. DoJ and FTC's as well as the European Commission's horizontal merger guidelines) as a measure of market power.

A relevant question is whether industries capture reasonable employment opportunities for workers, and whether workers travel across commuting zone borders to work. In support of that CZs are a reasonable geographical denomination, 92.5% of employees in the pharmacy market work and reside in the same CZ. A commonly used alternative to industry is occupation.[16] In this paper industry is used instead of occupation both because the deregulation took place at the industry level, and because the data lacks complete and consistent occupation information. Out of the employees that worked at a pharmacy in 2004, only one third have worked in a different five-digit industry after 2004. To the extent that industry is too narrow to represent the employment opportunities for workers, HHI in both the pre-deregulation and post-deregulation period will be too high; the identification relies on differences in HHI over time.

1.3.2 Data

The data is drawn from a panel of annual register data collected by Statistics Sweden. I have access to full-population data, meaning that I can identify educated pharmacists and non-pharmacists working both inside and outside the pharmacy industry. The main dataset is a matched employer–employee register (*RAMS*) that includes firm, workplace and person identifiers as well as gross labor earnings and the months worked at each workplace for all gainfully

[15] The measure assumes that pharmacies operated by the same firm in the same LLM do not compete for workers. In support of this, the estimated returns to being a new hire in a pharmacy in the same LLM between 2004 and 2008 are marginal. For the full sample, the wage returns to joining are not statistically different from 0. Restricting to those who join from another pharmacy, the returns are estimated to be 1.46%. Restricting further to those who move from another pharmacy but are not managers, the return is 1.01%.

[16] Previous work has defined markets both using geography–industry (Berger et al. 2019, Lipsius 2018 Benmelech et al. 2018 and Rinz 2020) and geography–occupation (Azar et al. 2020a, Azar et al. 2020b and Qiu and Sojourner 2019).

employed individuals. For those employees with more than one workplace, only one workplace per employee and year is kept, defined as the workplace in November where the individual has the highest annual earnings.[17] Using year, firm and workplace identifiers, firm and workplace characteristics such as workplace industry and ownership structure are matched in. Wages and occupations are taken from structural earnings statistics. Using person and year identifiers, the employer-employee data is linked to demographic registers (*Louise* and *Födelseland*) that include variables such as year and country of birth, gender, education level and field of educational specialization. Commuting zones (*lokala arbetsmarknader*) are taken from Statistics Sweden and are matched in based on the municipality of the workplace.[18] Financial data, only available until 2015, is also matched in at the firm and year level.

As outlined in Section 1.3.1, the pharmacy market is delineated from the full-population data using the workplace industry code for dispensing chemists. Pharmacists are identified using information on education. The demographic registers not only has information on the level of education that individuals hold but also detailed information on which field the education is in, based on the Swedish educational nomenclature (*SUN*). Consequently pharmacists can be identified using the individual's educational specialization in pharmacy together with the level of education the individual has.[19]

The main wage measure is monthly full-time adjusted wages in Swedish crowns (SEK), measured between September and November each year. Wages are available for all individuals in the public (non-market) sector and for an annual random stratified sample in the private (market) sector.[20] The main analysis uses log full-time equivalent wages as the outcome. These data are accurate but, due to the sampling design, incomplete (see Table 1.1 for summary statistics). To ensure the results are not driven by extreme outliers, the data is trimmed so that wages 50% below the 1st percentile or 50% above the 99th percentile of the monthly full-time adjusted national wage distribution every year are excluded from the regressions. Sensitivity analyses, included in the appendix, instead use gross monthly earnings (defined as gross annual labor earnings divided by the number of months worked at the primary em-

[17]Using November is in line with Sweden's official statistics, in turn based on ILO's methodology. Before identifying the main workplace, I drop employees that cannot be linked to a physical workplace and therefore obtain a false workplace code. I also exclude self-employed, who have a different employment relationship to employees.

[18]The boundaries of commuting zones are revised periodically. To maintain a consistent measure of commuting zones throughout the time period considered, commuting zones from 2013 are used.

[19]Pharmacists are defined using education rather than occupation to have complete coverage in the data. Occupation in the registry data is only available for around 50% of workers.

[20]Approximately 50% of private sector employees are included in the sample. The sample is stratified by industry and firm size, with an oversampling of larger firms.

ployer) from the matched employer-employee data. The earnings measure has complete coverage in the data.[21]

1.3.3 Main sample

The main sample consists of all employees (pharmacists, pharmacy technicians and non-pharmacists) who are employed at a pharmacy as their main place of work. The sample period is restricted to the years 2004 to 2016, which means pre-trends can be analyzed alongside post-deregulation effects. As the employment relationship is different for self-employed, self-employed are excluded from the sample.[22]

Table 1.1 includes summary statistics for the full sample period (2004 to 2016) as well as pre-deregulation and post-deregulation. The pharmacy industry is highly skilled and female dominated: 88% are women and 67% have at least post-secondary education. Half the share of employees are educated pharmacists. The statistics also highlight that there have been compositional changes over time that will be important to control for in the empirical analysis. Workers are on average younger and have slightly less tenure and experience in the post-period. The share with post-secondary education has increased over time and the share of foreign born, defined as being born outside of Sweden, has increased sharply from 12% pre-deregulation to 23% post-deregulation.

1.4 Institutional setting and descriptive patterns

1.4.1 Introducing competition on the pharmacy market

Between 1971 and 2009, *Apoteket* (the National Corporation of Swedish Pharmacies) had the exclusive right to retail prescription and non-prescription pharmaceuticals in Sweden. *Apoteket* was a state-owned enterprise whose primary purpose was to ensure a nationwide pharmaceutical supply system. In 2009, a regulatory reform ended the monopoly. The main implication was that *Apoteket* lost its monopoly rights and private firms could enter.

[21] There are two main differences between wages and earnings: (i) due to the sampling design of the official statistics, wages are predominantly available for larger firms, and (ii) earnings are not full-time equivalent. Like for wages, the earnings measure is restricted to drop earnings more than 50% below the 1st percentile or 50% above the 99th percentile of the monthly national wage distribution every year.

[22] Self-employment among pharmacists today is low. Goldin and Katz (2016) find that the fraction of pharmacists in the U.S. who are self-employed has decreased from 40% in 1966 to just under 5% in 2011. Figure A.1 shows the share of self-employment among all educated pharmacists in Sweden between 2004 and 2016. Under 4% of pharmacists are self-employed. There is an increase in self-employment in conjunction with the deregulation. This is not explored further in this paper.

Table 1.1. *Summary statistics, pharmacy industry*

	2004–2016		2004–2008		2009–2012		2013–2016	
	Mean	Std. dev.	Mean	Std. dev.	Mean	Std. dev.	Mean	Std. dev.
Female	0.88	(0.32)	0.91	(0.29)	0.88	(0.32)	0.86	(0.35)
Age (years)	43.79	(13.96)	45.60	(13.20)	43.32	(14.32)	42.13	(14.23)
Age < 30	0.20	(0.40)	0.16	(0.37)	0.22	(0.41)	0.23	(0.42)
Age ≥ 50	0.40	(0.49)	0.47	(0.50)	0.38	(0.49)	0.33	(0.47)
Foreign born	0.17	(0.38)	0.12	(0.32)	0.17	(0.37)	0.23	(0.42)
Post-secondary	0.67	(0.47)	0.64	(0.48)	0.66	(0.47)	0.71	(0.45)
Pharmacist	0.49	(0.50)	0.51	(0.50)	0.48	(0.50)	0.47	(0.50)
Tenured	0.43	(0.50)	0.50	(0.50)	0.41	(0.49)	0.37	(0.48)
Industry experienced	0.73	(0.44)	0.77	(0.42)	0.73	(0.44)	0.69	(0.46)
Non-missing wage	0.69		0.84		0.54		0.67	
Monthly wage (2004 SEK)	26,193	(8,166)	24,246	(7,404)	26,151	(8,096)	29,083	(8,429)
Monthly earnings (2004 SEK)	23,806	(10,353)	22,097	(9,198)	23,707	(9,955)	26,059	(11,614)
Number of employee-year obs.	159,863		59,392		49,277		51,194	

Note: The table shows summary statistics for all employees in the pharmacy industry for the full period (2004–2016), the pre-period (2004–2008) and the post-period, split into two parts (2009–2012 and 2013–2016). Foreign born are those born in a country other than Sweden. Tenured hold at least five years of tenure at a pharmacy. Industry experienced hold at least five years of experience from the pharmacy industry.

The timeline of the deregulation is included in Figure 1.1. A special inquiry was commissioned in December 2006 to submit proposals regarding deregulating the market. *Apoteket*'s exclusive rights were abolished through the implementation of three bills between September 2008 and November 2009. Only the second bill (Regeringen 2009b Prop 2008/09:145) is pivotal to the experiment that this paper relies on. The bill, which passed in parliament in April 2009 and into law in July 2009, made it possible for private firms to operate pharmacies. Following this law change, *Apoteket* sold the majority of its pharmacies to private owners but remained in the market. The first private pharmacies opened to customers in January 2010.[23]

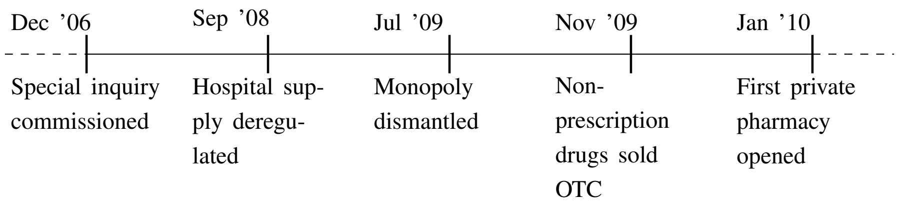


Figure 1.1. Timeline of regulatory changes

Entry into the market took place through two channels. First, two thirds of *Apoteket*'s pharmacies were privatized. Second, firms could open new pharmacies, subject to obtaining a permit from the Swedish Medical Products Agency. Only variation from the first channel is used in the empirical strategy. Based on public records, 466 out of *Apoteket*'s 946 pharmacies were sold to private firms and a further 150 pharmacies were transferred to a separate state-run company, *Apoteksgruppen*, where entrepreneurs could enter as majority owners (Nya apoteksmarknadsutredningen 2017). The privatization involved sales of pharmacies in eight clusters during 2009.[24] The number of clusters and composition of pharmacies within each cluster was formed by the policymaker. The stated aim was to promote competition in and after the bidding process and to achieve competitive neutrality between public and private owners (Riksrevisionen 2012).

The share of employment at a public owner in the pharmacy industry fell from 100% to 30% and the number of pharmacies increased by over 400 fol-

[23]The other two bills are not directly relevant to retail pharmacies. The first bill (Regeringen 2008 Prop 2007/08:142) involves a separate product market – the supply of drugs to hospitals. Prior to 2008, only *Apoteket* or the caregiver could supply inpatient pharmaceuticals. From September 2008, inpatient drugs can be publicly procured from other suppliers. The third bill, implemented in November 2009, implied that certain non-prescription drugs can retail in new locations, such as supermarkets (Regeringen 2009a Prop 2008/09:190). Price competition for non-prescription drugs therefore intensified by breaking up the monopoly and by retailing non-prescription drugs in new locations.

[24]There were also bids for individual pharmacies in *Apoteksgruppen*. They were first received in 2010 (see Riksrevisionen 2012).

lowing the deregulation. Likewise, the number of firms increased substantially, reaching over 150 within two years of the deregulation. Labor market concentration also fell substantially. Average HHI in the pharmacy market, weighted by employment in each local labor market, fell from 1 in prederegulation to just over 0.25 in 2016.

The variation in HHI that I use in the empirical strategy is based on observed ownership changes in the data in 2009.[25] In this year, most of the privatization occurred but no additional firms had yet entered and no new pharmacies had been opened. Figure 1.2 maps HHI by local labor market in 2009. Recall that the industry was a monopoly pre-2009. Thus light-colored areas with relatively low levels of HHI in 2009 have experienced the largest reductions in employer concentration, or similarly the largest increases in competition. In line with earlier literature (see Rinz 2020), urban areas are least concentrated post-deregulation and rural areas are most concentrated. Nevertheless, there is substantial variation in HHI across local labor markets.[26]

[25] I observe firm and workplace identifiers over time, as well as which employees work at each workplace and firm. I neither have access to data on the pharmacies included in each cluster nor the winners of the bids.

[26] The reform variation is also illustrated in Figure A.3, which shows the distribution of HHI in 2009 across LLMs and employees. Table A.1 includes pre-reform summary statistics separately for local markets where the change in HHI is high, medium or low. The groups are similar along many dimensions, like the gender and age composition of the workers, but they also differ along some dimensions, like share of foreign born. LLM fixed effects control for permanent differences between local markets in the empirical estimation.

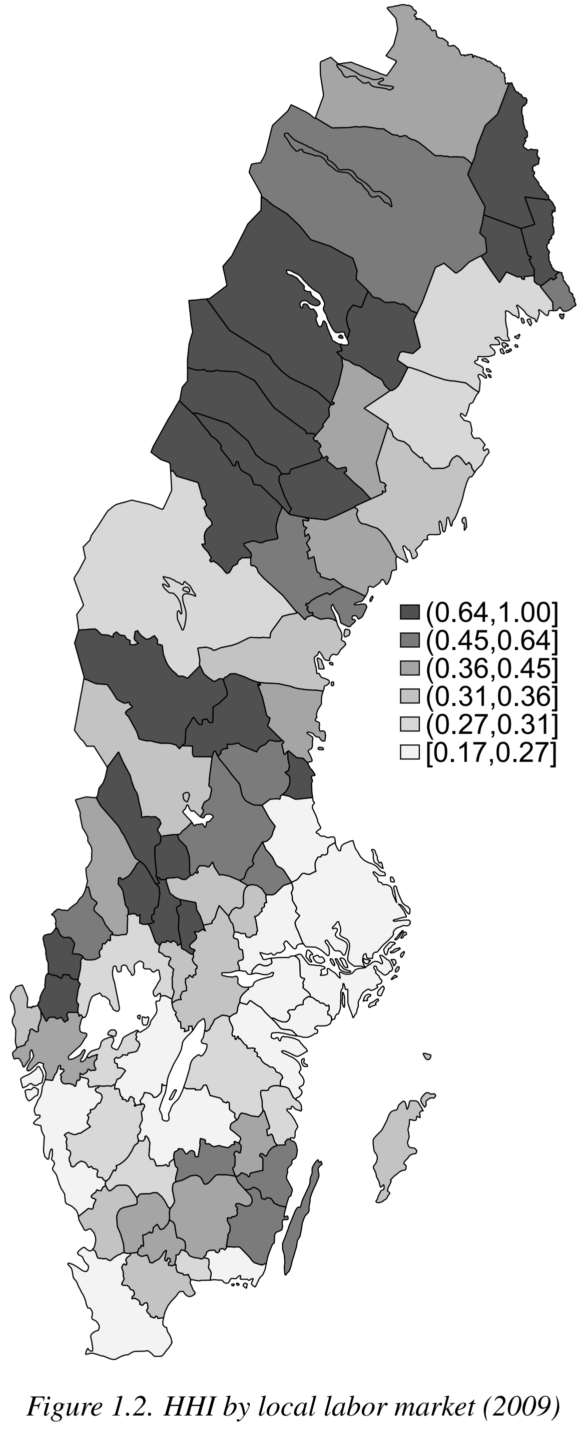


Figure 1.2. HHI by local labor market (2009)

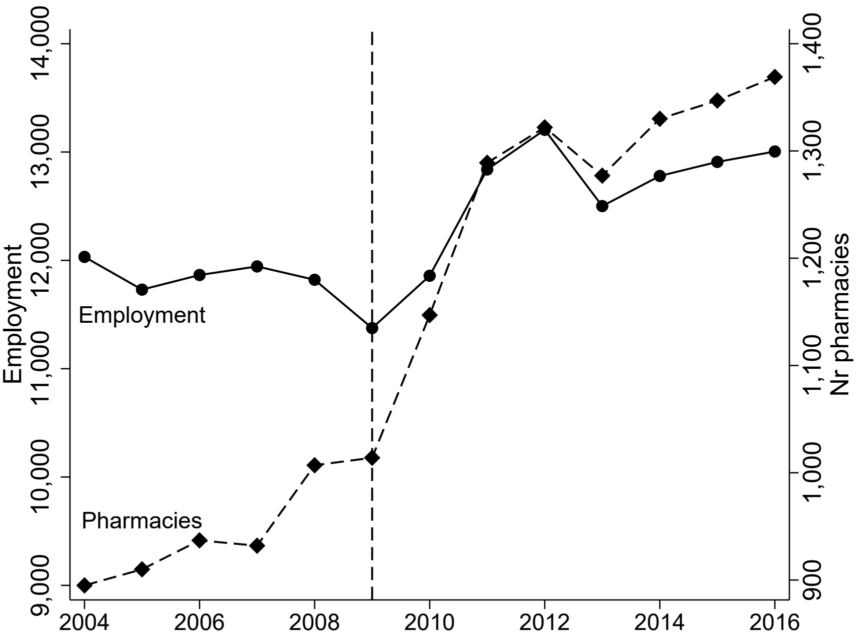


Figure 1.3. Employment and number of pharmacies in pharmacy industry

1.4.2 Employees

Figure 1.3 shows the number of employees and the number of pharmacies in the pharmacy industry annually between 2004 and 2016, based on data in the main sample. Prior to the deregulation, there were 12,000 individuals working in the industry. This has increased by just under 10%, to 13,000, post-deregulation. The number of pharmacies has increased by around 50%, from 900 to nearly 1,400.[27] It follows that the number of employees per pharmacy has decreased and that the market has become fragmented: there are many more pharmacies operated by many more firms, but not equally more employees.

A simple monopsony model predicts that employment should respond positively to reduced monopsony power. This is in line with what we see in the data. Similarly, if the market experienced a market-wide labor demand shock once deregulated, a competitive model would also predict that employment

[27] The number of pharmacies in the data is similar but generally slightly higher than that reported by the Dental and Pharmaceutical Benefits Agency (*TLV*), see TLV (2018) Figure 2. This could arise if the industry code for dispensing chemists is wider than that used by *TLV*, and/or if some pharmacies organize the pharmacy under more than one workplace identifier. Similarly, the number of employees is higher than that reported by the industry organization *Sveriges Apoteksförening*. In addition to the above, this could for example arise if fewer employee categories are included in the industry organization's reporting.

increases. The latter, however, is unlikely. Labor demand is derived from product demand, and the demand for pharmaceuticals is unlikely to jump discontinuously in 2009. I return to this in Section 1.6.3 below.

Type of pharmacy employees

Pharmacists make up 50% to 60% of the workforce in pharmacies, pharmacy technicians around 25% and non-pharmacists, including managers, the remainder. Pharmacies must have a pharmacist on duty during opening hours. They have an occupational license, issued by the National Board of Health and Welfare.[28] In 2016, 64% of pharmacists worked in a pharmacy. The second largest industry for pharmacists at the five-digit level was manufacture of pharmaceutical preparations (6%) and the third largest wholesale of pharmaceutical goods (5%).[29] Pharmacy technicians primarily work with sales and advice on non-prescription drugs and retail items, and can assist with dispensing medicines. They are tied to pharmacies – over 95% of pharmacy technicians work there – but are harder to identify in data as they lack unifying educational backgrounds or occupational codes.[30] Prior to the deregulation, *Apoteket* internally trained pharmacy technicians. Post-deregulation, there are vocational degrees. In 2016, 62% had upper secondary schooling or less.

1.4.3 Wages

Despite being a state-owned enterprise, employees negotiate wages individually both pre-deregulation and post-deregulation. The pharmacy market is fully covered by collective agreements. Wages are not specified in the collective agreements but are set flexibly in annual wage negotiations between the employee and their manager.

Aggregate changes in wages

The deregulation may on aggregate result in either increases or decreases in wages, as outlined in Section 1.2. To descriptively gauge aggregate changes in wages for pharmacy employees over time, workers in the pharmacy industry are compared either to the school sector (preschools and compulsory schools) or to the labor market as a whole. The school sector is used because spillovers

[28] Formally, there are two types of pharmacists: those who hold at least a Master's degree in Pharmacy (*apotekare*) and those who hold a Bachelor's degree in Pharmacy (*receptarie*). The legal requirements apply to either type of pharmacist, and there are only small differences in tasks performed. Consequently no distinction is made between the two categories in this paper.

[29] Table A.2 includes the top 5 industries by employment in 2016 for educated pharmacists.

[30] From 2014, there is an occupational code for pharmacy technicians. The occupational code prior to 2014 is broader and does not only encompass pharmacy technicians.

to this sector are unlikely, and because it shares important features with the pharmacy industry:[31]

$$\ln(w_{it}) = \alpha_1 Pharmacy_{it} + \alpha_2 Pharmacy_{it} \times Post_t + \lambda_t + \beta X_{it} + \varepsilon_{it} \quad (1.7)$$

The coefficient of interest is α_2. This summarizes the aggregate change in wages in pharmacies in the post- compared to pre-period, relative to changes in wages on the whole labor market and in schools. Individual-level controls, X_{it}, for age, gender, foreign-born and education are included in the specification to make the comparison between similar workers. In certain specifications, CZ fixed effects are included to control for permanent regional wage differences, or individual by CZ fixed effects to control for individual heterogeneity in wages. Estimates of α_2 in Table 1.2 indicate that wages on aggregate increased by 2 to 4 percentage points more for workers in pharmacies in the post-period (from 2009 onward) than they did in the rest of the labor market or for individuals working in schools.[32]

Table 1.2. *Aggregate changes in* $\ln(wage)$

	(1)	(2)	(3)
Panel A: Full labor market			
Pharmacy × Post	0.030***	0.034***	0.023***
	(0.008)	(0.009)	(0.002)
R^2	0.394	0.419	0.936
N	26,587,246	26,587,246	25,613,134
Panel B: Pharmacies and schools			
Pharmacy × Post	0.040***	0.042***	0.020***
	(0.006)	(0.006)	(0.002)
R^2	0.646	0.663	0.958
N	3,256,605	3,256,605	3,113,725
Year FE	Yes	Yes	Yes
Controls	Yes	Yes	Yes
CZ FE		Yes	
Person × CZ FE			Yes

Note: This table provides the estimates of α_2 from estimating equation (1.7). Controls are age (in five categories), gender, foreign born and level of education (in five categories). Standard errors are clustered by commuting zone and reported in parentheses.

[31] The school sector is defined using workplace industry codes 85.321 and 80.100 (SNI92), 80.101 and 80.102 (SNI02) and 85.100 and 85.201 (SNI07). Both public and private organizations run schools. Similar to pharmacies, it is highly skilled and female dominated (see Figure A.2, which compares workers in the pharmacy industry to workers in schools and the full labor market). Teachers are certified and, from 2011, there is an occupational license.

[32] Comparing changes in mean (deflated) wages over time gives a similar picture. Wages have increased 2 (3.7) percentage points more in pharmacies than in the full labor market (schools).

1.5 Empirical strategy

The deregulation provides variation in market concentration that is oftentimes hard to observe. While the national pharmacy market was deregulated, the change in local labor market concentration varied over the country. The empirical strategy exploits the fact that the deregulation gave rise to differences in the size of changes in labor market concentration across local labor markets within the pharmacy industry. Recognizing that actual HHI is potentially endogenous, it only makes use of the variation in concentration from the privatization of pre-existing pharmacies. The empirical strategy is outlined in detail below.[33]

I would like to estimate regressions of the following form to understand the effect that labor market concentration has on wages:

$$\ln(w_{imt}) = \alpha \ln(HHI_{mt}) + \lambda_m + \lambda_t + \beta X_{imt} + \varepsilon_{imt} \tag{1.8}$$

λ_m are local labor market (LLM) fixed effects, λ_t are year fixed effects and X_{imt} are additional controls. The concern is that actual HHI_{mt} is potentially endogenous, leading to biased estimates of the coefficient of interest, α. For example, market concentration depends on firms' location choices. If unobserved factors affect both the choice of where to locate pharmacies as well as wages in those locations in ways which are not controlled for, $\hat{\alpha}$ will be biased. Another potential concern is that HHI is based on employment shares, which relate to individuals' decisions of where to work. The reform coincides with increased aggregate mobility on the labor market (see Figure 1.7) and wages are a key component in the labor supply decision.[34]

To address these concerns, I adopt an approach where, instead of using actual changes to concentration, only the change that arises from the privatization of pharmacies that exist when the market is deregulated is used. This is the part of the variation that was within the policymakers' control. Reduced form regressions of the following form are estimated for everyone who is employed at a pharmacy:

$$\ln(w_{imt}) = \gamma[\ln(HHI_{m,2009}) \times Post_t] + \lambda_m + \lambda_t + \beta X_{imt} + \varepsilon_{imt} \tag{1.9}$$

As above, λ_m are LLM fixed effects and λ_t are year fixed effects. X_{imt} are additional controls for age, gender, foreign-born, level of education, pharmacist, tenure and industry experience. $\ln(HHI_{m,2009})$ is log HHI in 2009 and $Post_t$ is a dummy variable equal to one from 2009 onward. Each local labor market

[33] The empirical models are estimated in Stata using the Multi-Way Fixed Effects estimator (Correia 2017).

[34] Schubert et al. (2020) stress that wage–HHI regressions are likely to be biased if workers' outside options are ignored. This is likely to be less of a concern in this setting for at least two reasons. First, pharmacists and pharmacy technicians are closely tied to pharmacies, so outside options that require their skills are limited. Second, permanent differences in outside options across local labor markets are controlled for in the specifications.

receives a constant value of $\ln(HHI_{m,2009})$, such that $\ln(HHI_{m,2009}) \times Post_t$ captures treatment intensity at the local market level from the year of deregulation onward. The specification exploits the full variation in market concentration based on the sale of pre-existing pharmacies as a predictor of the actual change in concentration. $HHI_{m,2009}$ is neither affected by firms' decisions to open up new pharmacies nor to any mobility decisions that workers make post-deregulation, but is instead a function of the deregulatory design (see Section 1.4 for details).[35]

The local labor market fixed effects control for permanent differences across local markets and year fixed effects for general time trends in industry wages, including those due to inflation. Importantly, national effects of deregulating the market will be absorbed in the time fixed effects. The empirical strategy allows me to capture the effects of changes to labor market concentration without confounding it with effects related to product market concentration, which are likely to be national.[36] The local labor market fixed effects are important both because wages are likely to differ throughout the country for reasons unrelated to the reform, and because firms generally want to locate pharmacies in highly populated areas. Finally, as wages are particularly driven by individual characteristics, certain specifications use person by local labor market fixed effects instead of local labor market fixed effects. This keeps composition constant. The identifying variation then comes from workers who have stayed in their local labor market over time and consequently experienced different levels of concentration pre- and post deregulation.

Equation (1.9) is a type of difference-in-difference specification with different treatment intensities that, in addition to correct functional form, relies on parallel trends in log wages across local markets in the absence of the deregulation. To support this assumption, I also estimate event versions of equation (1.9):

$$\ln(w_{imt}) = \sum_{t \neq 2008} \gamma_t \ln(HHI_{m,2009}) 1[year = t] + \lambda_m + \lambda_t + \beta X_{imt} + \varepsilon_{imt} \quad (1.10)$$

Estimating responses by year serve a dual purpose. This both shows whether there are pre-treatment trends in log wages relative to 2008 (the omitted year) and the time trajectories of post-treatment effects. In Section 1.6.3 I include extensive additional robustness checks.

[35] We may worry that there is some measurement error in employment in 2009 as employment seems to dip slightly in that year, see Figure 1.3. The results are robust to instead calculating HHI based on pharmacies and employment from 2004 but ownership structures from 2009, see Figure B.2.

[36] Qiu and Sojourner (2019) highlight the importance of distinguishing between concentration in product and in labor markets. Kroft et al. (2020) study the construction industry to find that firms in this industry enjoy rents both due to markups of prices and markdowns of wages.

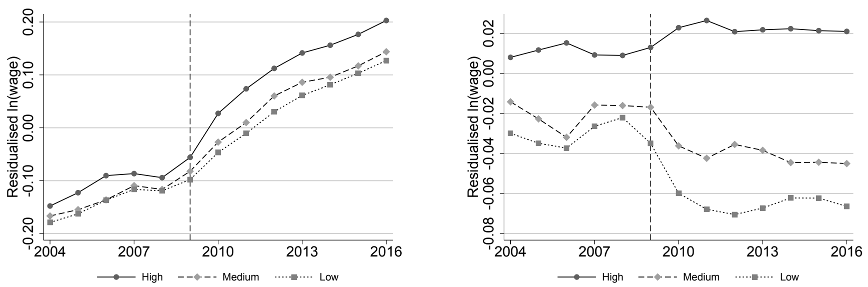
1.6 Results

1.6.1 Wage effects

Descriptive patterns

Before formally estimating the effect of labor market concentration on wages, I show descriptively how wages have evolved over time in local markets that experienced different changes in employer concentration. Figure 1.4 plots residualized log wages separately by change in HHI. Pharmacy workers are split into three categories: those that work in local markets where the change in HHI is high, medium or low. Wages are residualized by age, gender, foreign born and level of education in Panel A. To control for general time trends in wages, year fixed effects are also included in Panel B.

The figure suggests that wages evolve in parallel in markets that ended up experiencing different changes in concentration prior to deregulation. Notice that mean wages are not the same even though HHI is the same everywhere in the pre-period. Wages are set flexibly, and differences in wages are driven by factors other than employer concentration. Upon deregulation, wages grow faster in markets that experience larger reductions in labor market concentration.



(a) Without year fixed effects   *(b)* With year fixed effects

Figure 1.4. Residualized log wages by change in labor market concentration

Note: This plots mean residualized log wages separately by three groups of changes in labor market concentration: "High" (where $HHI_{m,2009}$ is less than or equal to the 25th percentile of the distribution of $HHI_{m,2009}$ by LLM), "Medium" (where $HHI_{m,2009}$ is above the 25th but less than or equal to the 50th percentile), and "Low" (where $HHI_{m,2009}$ above the 50th percentile). In Panel A, log wages are residualized by age, gender, foreign born and level of education. In Panel B, year fixed effects are additionally included.

Estimations

Columns (1) to (3) in Table 1.3 report the results of estimating equation (1.8) in Panel A, and equation (1.9) in Panel C. Columns (4) and (5) include the first stage and IV results of instead using $\ln(HHI_{m,2009}) \times Post_t$ as an instrument for $\ln(HHI_{mt})$. Focusing first on the OLS results, the point estimates are negative and statistically significant at the 1% level. Taken at face value, they imply elasticities of wages with respect to labor market concentration between -0.016

(see 0.003) and -0.048 (se 0.007). The reduced form estimates in Panel C are between -0.022 (se 0.006) and -0.048 (se 0.011), and the elasticities implied by the IV estimation are between -0.020 (se 0.006) and -0.046 (se 0.008).[37] The similarity in estimates across estimation techniques arises because the change in market concentration in the year of deregulation is highly predictive of actual changes in market concentration on the pharmacy market. The first stage coefficient is between 1.023 and 1.065, and the F-statistic between 80.45 and 107.21, depending on whether person by LLM fixed effects are used or not. Figure A.4 in the appendix plots actual HHI against $\ln(HHI_{m,2009}) \times Post_t$. The two measures are highly correlated: the R^2 of regressing $\ln(HHI_{m,t})$ on $\ln(HHI_{m,2009}) \times Post_t$ is 0.91.

Table 1.3. *Effect of labor market concentration on* $\ln(wage)$ *– OLS, RF and IV results*

	(1)	(2)	(3)	(4)	(5)
	OLS			IV	
Panel A: OLS & IV					
$ln(HHI_{mt})$	-0.045***	-0.048***	-0.016***	-0.046***	-0.020***
	(0.009)	(0.007)	(0.003)	(0.008)	(0.006)
R^2	0.217	0.552	0.924		
Panel B: First stage					
$ln(HHI_{m,2009}) \times Post_t$				1.023***	1.065***
				(0.114)	(0.103)
F-statistic				80.45	107.21
Panel C: Reduced form					
$ln(HHI_{m,2009}) \times Post_t$	-0.033**	-0.048***	-0.022***		
	(0.013)	(0.011)	(0.006)		
R^2	0.216	0.551	0.924		
N	110,825	110,722	104,968	110,722	104,968
Year FE	Yes	Yes	Yes	Yes	Yes
LLM FE	Yes	Yes		Yes	
Person × LLM FE			Yes		Yes
Controls		Yes		Yes	

Note: This provides the results of estimating equations (1.8) and (1.9) for log wages. OLS and IV results are presented in Panel A. The first stage in Panel B show the results of regressing $\ln(HHI_{mt})$ on the instrument $\ln(HHI_{m,2009}) \times Post_t$ and exogenous regressors. Controls are age (in five categories), gender, foreign born, pharmacist, level of education (in five categories), tenure (in three categories) and industry experience (in three categories). Standard errors are clustered by LLM and reported in parentheses. The F-statistic is the Kleibergen-Paap Wald rk F-statistic.

To put the estimated effects into perspective, a local market that ended up at the 25th percentile of market concentration instead of the 75th percentile following the deregulation (an HHI of 0.28 instead of an HHI of 1 in the LLM-year distribution) would have wages that are 2.5 to 6 percent higher.

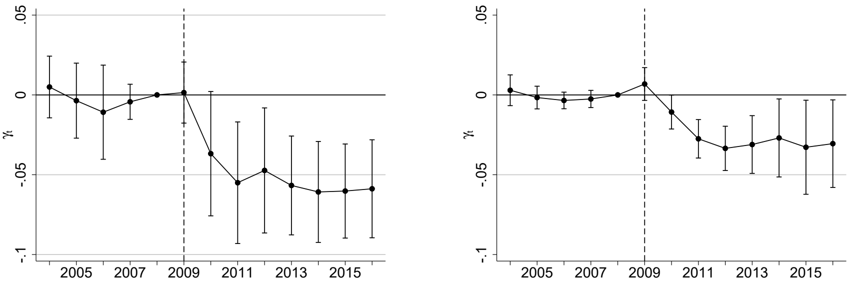
[37] Similar but somewhat more imprecise results are found when instead using log earnings as the outcome, see Appendix Table A.3.

The negative effect that market concentration has on wages echoes a growing literature in labor economics.[38] The results are very similar to those found by Hershbein et al. (2019) and Rinz (2020), who report elasticities between -0.01 and -0.05, and somewhat smaller than those found by Azar et al. (2020a) who report elasticities between -0.03 (OLS) and -0.14 (IV). The somewhat smaller magnitude found in this paper could reflect that the public monopsonist did not depress wages as much as its market power would allow it to. It could also be that labor market institutions, such as the high degree of collective bargaining and the fact that this is a highly specialized group of labor, limit the firm's bargaining power pre-deregulation. In line with this, Jarosch et al. (2019)'s model implies that the elasticity of wages with respect to concentration becomes smaller when worker bargaining power increases, i.e. wages are less sensitive to concentration when workers have more bargaining power. Moreover, the estimates in this paper can be regarded as lower-bound estimates of the effect of labor market concentration on wages, as discussed in Section 1.2.2.

In the remainder of the discussion, I focus on the reduced form results from estimating equation (1.9). To check pre-reform parallel trends in log wages as well as post-reform differences in effects, Figure 1.5 plots estimates of γ_t from equation (1.10). The results support that wages evolve in parallel in local labor markets prior to deregulation, responding positively to the lower concentration post-deregulation.[39] As expected, there is no response to the deregulation in 2009: while the deregulation came into force in 2009, the new firms formally began trading in 2010. The positive wage effects rise between 2009 and 2010, and again between 2010 and 2011, and at least at the 5% level they are not equal between 2009 and 2011. From 2011 onward, the estimated effects are relatively stable. This stability over time is also supported by regressions of equation (1.9) when the pre-2009 period is grouped and the post-2009 period is split into two (2010–2012 and 2013–2016). The coefficients on the two sub-periods post-deregulation are not found to be statistically significantly different from each other.

[38] See Arnold (2020); Azar et al. (2020a); Benmelech et al. (2018); Hershbein et al. (2019); Jarosch et al. (2019); Lipsius (2018); Martins (2018); Prager and Schmitt (2019); Qiu and Sojourner (2019); Rinz (2020); Schubert et al. (2020).

[39] Figure 1.4 suggests that the evolution of mean wages slows down in 2008. The parallel trends are robust to instead using 2004 as the base year.



(a) LLM fixed effects

(b) Person by LLM fixed effects

Figure 1.5. The effect of labor market concentration on ln(*wage*) *over time*

Note: This plots estimates of γ_t from equation (1.10) with 95% confidence intervals. Panel A also controls for age (in five categories), gender, foreign born, pharmacist, level of education (in five categories), workplace tenure (in three categories) and industry experience (in three categories).

1.6.2 Composition effects

The results in Table 1.3 show that there are differences in point estimates across models. The estimates of γ that are only identified from differences in concentration across LLMs over time, without any controls, are -0.033. When adding controls, the point estimates increase to nearly -0.048. This suggests that the composition of workers bears at least partial importance for the estimated wage effects.[40] The estimates that use within-individual variation over time and therefore fully control for composition are around -0.02. That these estimates are slightly smaller suggests that high-wage workers become employed in markets where concentration changed more. Put differently, the lower concentration that materializes post-deregulation both results in higher wages for workers conditional on worker quality (seen by the regressions that include individual by local labor market fixed effects), and changes the composition of workers toward higher-paid workers.[41]

To gauge how composition changes following the deregulation, Figure 1.6 plots the estimated γ-coefficients from estimating equation (1.9) without controls for five indicator outcomes: female, foreign born, post-secondary education, pharmacist and young workers (defined as being below age 40). The results indicate that there is no statistically significant effect at the 5% level on the share with post-secondary education. Local markets where where concen-

[40] That labor market concentration can affect the types of workers hired is highlighted by Hershbein et al. (2019), who find that local labor market concentration is negatively correlated with wages but positively correlated with skill demand.

[41] This result is also supported by the fact that when the sample is restricted to incumbents, the estimated wage effects from reduced labor market concentration are -0.035 (compared to -0.048 for the full sample), in a model that includes year fixed effects, local labor market fixed effects and controls.

tration was reduced the most have experienced faster growth in the share of foreign born and the share of young employees. On the contrary, the share of women and pharmacists has declined more in these markets, though the effect is only statistically significant at the 5% level for women, not pharmacists.

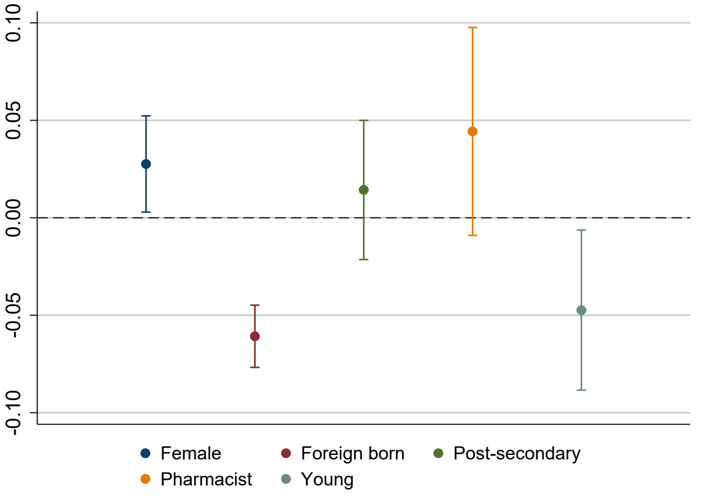


Figure 1.6. Compositional changes

Note: The figure plots estimated γ-coefficients with 95% confidence intervals from the following model for five indicator outcomes: $Y_{imt} = \gamma[\ln(HHI_{m,2009}) \times Post_t] + \lambda_m + \lambda_t + \varepsilon_{imt}$. Young is defined as being below age 40.

1.6.3 Robustness checks

Robustness checks are included in Appendix B. To assess the stability of the estimates, a set of robustness checks estimates equation (1.9) using different samples or controls. The γ-estimates are included in Appendix Table B.1. In column (1) local labor market fixed effects and controls are included and in column (2) individual by local labor market fixed effects are included. Baseline results are shown in Panel A.

We may be worried that the relationship between labor market concentration and wages is biased if labor demand changes in conjunction with concentration. Boal and Ransom (1997) highlight that a negative correlation between labor market concentration and wages cannot be taken as evidence against competitive labor markets unless total labor demand is fixed. If the deregulation coincides with a market-level increase in labor demand, we may see increases in wages as well as the number of firms who enter, leading to lower levels of market concentration. This would bias $\hat{\gamma}$ away from 0.[42] To alleviate this concern, I include controls for log number of pharmacy employees at the LLM-year level.[43] The results of this estimation are included in Panel B of Table B.1. The point estimates at between -0.020 and -0.046 are very similar to the baseline results of -0.022 to -0.048, indicating that the results are not driven by labor demand effects. We also expect demand effects, to the extent that they exist, to be moderated over time. The event analysis presented in Figure 1.5 shows that the wage effects from reduced labor market concentration persist also over the longer term, which supports that the estimated effects do not reflect a demand shock.

In Panel C of Table B.1, controls for log value added per employee at firm-level are included to remedy the concern that productivity changes coupled with rent-sharing may drive observed wage effects. In Panel D, urban areas (precisely, local markets that encompass the three largest cities in Sweden) are excluded to ensure that the effects are not driven by these markets alone. The share of managers employed in the pharmacy industry increases from 2% to between 6% and 8% post-deregulation. In Panel E, managers are excluded from the sample to ensure that the results are not driven by the increase in the number of managers. In Panel F only the public sector is included. This is to remedy the concern that the effects are driven by the entry of private firms only, that may have different objectives to public firms. Finally, in Panel G controls are included for mean log wages at the CZ and year level, to remedy the concern that wages may in general be growing faster in certain commuting zones. The estimates are remarkably stable across the empirical models, and the result that the reduction in market concentration positively influences wages is robust to these checks. The most conservative estimates that hold composition constant find estimates between -0.013 and -0.023.

While the analysis above displays reassuring pre-trends (see Figure 1.5) and is robust to controlling for log wages in the CZ (see Panel G of Table B.1), we may nevertheless worry that the regressions are picking up spurious changes within these local labor markets rather than effects related to the competitive changes. To alleviate this concern, a placebo analysis is performed where event specification (1.10) is estimated for the school sector. Figure B.1 plots γ_t separately for the school sector and the pharmacy market. $HHI_{m,2009}$ is defined as previously. Reassuringly, the analysis shows that employees in schools have not experienced corresponding wage growth as in the pharmacy industry, supporting that the estimated wage effects are real rather than spuri-

[42] Given that labor demand is derived from product demand, and that there is little reason to expect demand for pharmaceuticals to be affected by the deregulation, this alleviates the concern that there is a concurrent labor demand shock.

[43] An alternative is to control for log number of pharmacies per LLM. The correlation between log number pharmacies and log employment is 0.98, thus I do not include both at the same time. Instead controlling for the number of pharmacies also yields statistically significant and negative estimates of employer concentration on wages at the 5% level.

ous. The analysis for the school sector suggests that wages have grown slightly faster over time in markets where concentration has changed more, but these effects are much smaller than those estimated in the pharmacy industry. Taken together, the results support that reducing market concentration leads to a positive wage effect not observed in general in local labor markets.

1.7 Heterogeneity in estimated effects

In this section I explore who benefits most from reduced labor market concentration. First, I study new hires and stayers in the pharmacy industry. I consider how mobility is affected in Section 1.7.1, and how wage effects of reduced labor market concentration differ for new hires and stayers in Section 1.7.1. Second, I consider whether the estimated wage effects differ by individual characteristics such as being female or foreign born in Section 1.7.2.

1.7.1 New hires and stayers

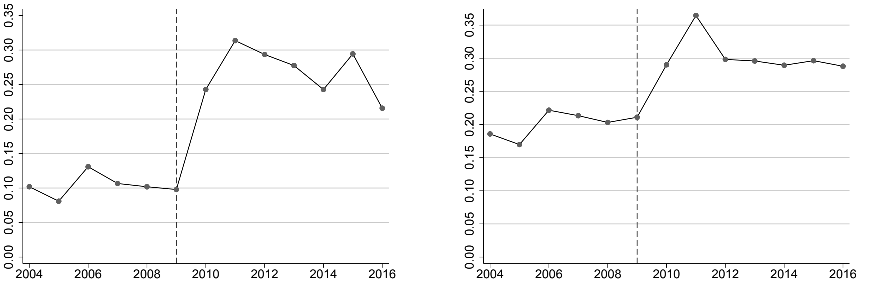
Mobility effects

To understand the types of career options that individuals face, it is instructive to consider transitions on the labor market. The rate at which workers change jobs is informative of the extent of competition between employers, as the ability of workers to leave for another employer limits the wage-setting power that employers hold. Mobility can be measured in different ways. Mobility to firms (where one firm can have many workplaces, or in this case, pharmacies) captures the fraction of employees who change firms or transition from non-employment, while mobility to workplaces captures the fraction of employees that change workplace (which can either be within the same firm, across firms or from non-employment). For wages, mobility between firms is likely important: changing firms involves changing employers, and is an opportunity to renegotiate wages. Mobility between workplaces can also be important for wages and can represent a career transition.

To gauge whether the opportunity to move has been affected by the deregulation, Figure 1.7 plots the fraction of new hires to a firm (Panel A) or pharmacy (Panel B) across all employees working at a pharmacy.[44] This shows that mobility in the pharmacy industry as a whole increased following the deregulation. A new hire is defined to be an employee who works at the firm (pharmacy) in year t but did not work there the year before, in $t-1$. To capture genuine recruits rather than moves that happen due to restructuring, the restriction that it is not a move if over 50% of the employees at the firm (pharmacy) in year t come from the same firm (pharmacy) in $t-1$ is applied. Prior to the deregulation, the fraction of new firm hires was 10%. Because there was only

[44] New hire is used interchangeably with mover, joiner or recruit in the text.

one firm in the pharmacy industry, this represents transitions from firms in other industries and from non-employment. Firm mobility fluctuates between 25% and 30% post-deregulation, spiking at 31% in 2011. Recruits to pharmacies is higher than to firms, at 20% pre-deregulation. Post-deregulation, recruits to pharmacies increases by around ten percentage points, to 30%. Just like firm mobility, pharmacy mobility peaks in 2011 at 36%, supporting that there is a reshuffling of employees in close conjunction with the deregulation. The increase in mobility between pharmacies is not only driven by the opening of new pharmacies but is also present at pre-existing pharmacies (though the changes are less stark; see Appendix Figure A.5).



(a) Share new hires to firm *(b)* Share new hires to pharmacy

Figure 1.7. Mobility in pharmacy industry

Note: Panel A and B show the annual share of employees that join a new firm and join a new pharmacy respectively.

To assess how the fraction of new hires relates to changes in labor market concentration, Table 1.4 reports estimates from equation (1.9) where the outcome is joining a firm (Panel A) or joining a pharmacy (Panel B). In column (1) the full sample is included. The results suggest that the likelihood of joining a new firm is positively affected by reduced labor market concentration: the point estimate of -0.046 implies that the likelihood of moving increases by 0.44 percentage points when HHI decreases by 10%. The effects for joining a new pharmacy are not statistically significantly different from zero. This implies that the share who move pharmacy does not increase more in areas where the change in concentration is high compared to low.

New hires can either come from another employer or from non-employment. Manning (2003) explains that a simple measure of monopsony power is the share of recruits from non-employment. If workers who quit are easily replaced by new hires from non-employment, the threat of quitting is limited and we expect employers' wage setting powers to be high. In column (2), attention is limited to the sample of new hires and the outcome is a dummy that indicates whether the new hire came from non-employment. The results suggest that the likelihood of joining a firm from non-employment is negatively related to the reduction in labor market concentration, but the effects are only statistically significant at the 10% level. A 10% decrease in HHI is related to a 0.78 percentage point decrease in the likelihood of joining a firm from non-employment, or similarly a 0.78 percentage point increase in the likelihood of making a firm-to-firm transition.

Table 1.4. *Effect of labor market concentration on hiring*

	(1) Join	(2) Non-employment
Panel A: Firm hires		
$ln(HHI_{m,2009}) \times Post_t$	-0.046***	0.082*
	(0.008)	(0.042)
R^2	0.367	0.166
N	159,633	31,140
Panel B: Pharmacy hires		
$ln(HHI_{m,2009}) \times Post_t$	0.006	-0.015
	(0.007)	(0.019)
R^2	0.454	0.204
N	159,633	41,083
Sample	Full	New hires
Year FE	Yes	Yes
LLM FE	Yes	Yes
Controls	Yes	Yes

Note: This provides estimates of γ from equation (1.9) for the likelihood of joining a new firm (Panel A) or a new pharmacy (Panel B). In column (1) the outcome is an indicator variable for joining the pharmacy or firm from anywhere, and in column (2) it is an indicator for joining from non-employment, conditional on being a new hire to the firm or pharmacy. Column (1) uses the full sample and column (2) only new hires. Controls are included for age, gender, foreign born, pharmacist, level of education, tenure and industry experience. Standard errors are clustered by LLM and reported in parentheses.

Wage effects

Next, I consider wage effects in the pharmacy industry for new hires and stayers. The analysis should be interpreted with caution: it is based on comparisons of a selected sample, and we saw above that the likelihood of moving firm is itself related to labor market concentration. Table 1.5 shows results of estimating equation (1.9) separately for four sub-groups: stayers, new hires, new hires from another firm (which can be inside or outside the pharmacy industry), and new hires from non-employment. As before, a new hire is defined to be an employee who works at the firm in year t but does not work there in $t-1$.[45] A stayer is defined to be someone who is neither a new hire in year t nor in year $t+1$.

[45] As previously, the restriction that it is not a move if over 50% of the employees at the firm (pharmacy) in year t come from the same firm (pharmacy) in $t-1$ is applied.

The results show that stayers (compared to other stayers) experience positive wage gains from the reduced labor market concentration while no statistically significant is found for joiners (compared to other joiners). Put differently, conditional on moving to a new firm, there is no additional return to moving in a local market with relatively low levels of concentration.[46] The effect for new hires is not distinguishable from zero irrespective of whether they are coming directly from another firm or from non-employment (see columns 3 and 4).[47]

Table 1.5. *Effect of labor market concentration on* $\ln(wage)$ *– firm stayers and new hires*

	(1)	(2)	(3)	(4)
	Stay	*Join*	*Firm-to-firm*	*Non-employment*
$ln(HHI_{m,2009}) \times Post_t$	-0.048***	-0.023	0.011	-0.026
	(0.013)	(0.027)	(0.042)	(0.034)
R^2	0.560	0.555	0.539	0.516
N	90,554	14,123	9,388	4,732
Year FE	Yes	Yes	Yes	Yes
LLM FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes

Note: This provides estimates of γ from equation (1.9) for different sub-samples, specified in the column headings. Columns 3 and 4 break out the new hires into two mutually exclusive types: those coming from another firm or those coming from non-employment. Controls are included for age, gender, foreign born, pharmacist, level of education, tenure and industry experience. Standard errors are clustered by LLM and reported in parentheses.

Employees' tenure and industry experience is related to the career moves that they make. Figure 1.8 instead considers the effects of labor market concentration separately for employees with different levels of tenure and industry experience. Tenure is defined as the number of years that the employee has spent at the same pharmacy since 2000. Industry experience is analogously defined as the number of years that the employee has spent working in the pharmacy industry since 2000. The results indicate that positive wage effects of lower labor market concentration materialize predominantly for those with longer tenure and more industry experience. This echoes the results for stayers

[46] Similar estimates are found for pharmacy hires and stayers, see Appendix Table A.4.

[47] The results should not be interpreted as saying that there are no wage gains to moving. Table A.5 compares wages for joiners and non-joiners in the same firm or pharmacy before and after deregulation. This abstracts from the role of labor market concentration. Focusing on firm hires in Panel A, the results indicate that new hires to the firm in the pre-period have lower wages than those already working in the firm. In the post-period, the pattern is reversed. Similar results are found when making comparisons in the same pharmacy.

found above. These workers are also likely to be the hardest to replace, given that labor supply in this market is likely to be relatively inelastic.

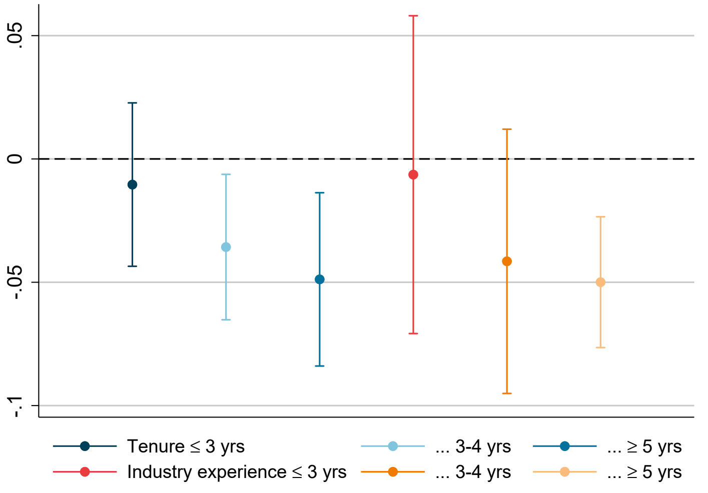


Figure 1.8. Effect of labor market concentration on ln(*wage*) *– tenure and industry experience*

Note: This plots estimated γ-coefficients with 95% CI from estimating equation (1.9) for sub-samples, as specified by the labels. Full results are included in Table A.6.

1.7.2 Individual characteristics

Next, I turn to consider how the wage effects from reduced labor market concentration differ by individual characteristics. Just like the analysis for movers, the results should be interpreted with caution since the deregulation has involved compositional changes (see Section 1.6.2). Figure 1.9 shows the results of estimating equation (1.9) by five sub-groups based on gender, country of birth, level of education, being an educated pharmacist and age. The results are remarkably stable across groups. Overall they suggest that all groups benefit from reduced labor market concentration.

Gender and country of birth: If the monopsonist engages in monopsonistic discrimination (Robinson 1933), which arises from the fact that firms can set lower wages to groups of individuals with more inelastic labor supply to the firm, we expect wages for women and foreign born to respond more strongly to the change in concentration.[48] The estimates in Figure 1.9 do not, however, suggest that this is the case. There are several plausible explanations for this. A public monopsonist may be less likely to discriminate than

[48] There are several papers on firm-level monopsony and the gender pay gap (see Hirsch 2009, Barth and Dale-Olsen 2009 and Webber 2016) and the immigrant-native pay gap (see Hirsch and Jahn 2015).

a private monopsonist and the collective bargaining institutions may dampen potential discrimination across groups. Moreover, the majority of employees are women both before and after the deregulation.[49]

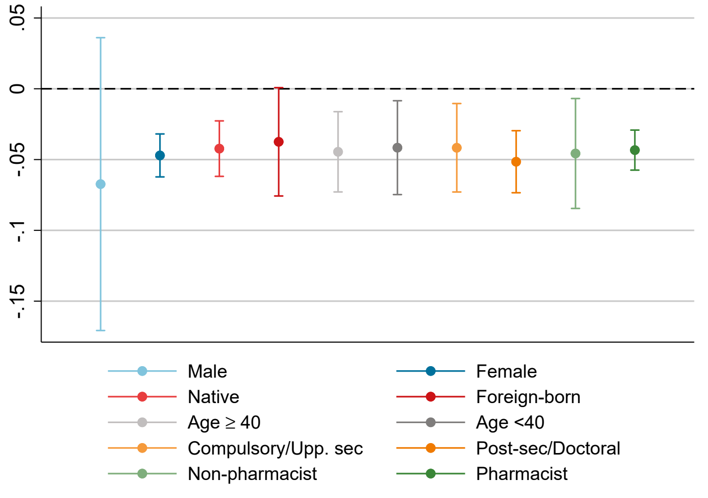


Figure 1.9. Effect of labor market concentration on $\ln(wage)$ *– individual characteristics*

Note: This plots estimated γ-coefficients with 95% CI from estimating equation (1.9) for subsamples, as specified by the labels. Full results are included in Table A.7.

Age: Another dimension along which employers potentially discriminate is age. In the gray-colored bars in Figure 1.9, the sample is split into employees in the pharmacy industry that are below 40 or 40 and above. The point estimates suggest that both younger and older workers experience positive wage effects of similar magnitudes from the reduction in market concentration.

Pharmacists and level of education: We expect the deregulation to change competition for specialized labor but not necessarily non-specialized labor. As Boal and Ransom (1997) point out, concentration of hospitals is not likely to monopsonize the market for hospital housekeepers. Turning to the analysis based on education, the results suggest that both those with at least some post-secondary education and those educated as pharmacists have gained similarly from the reduction in concentration. At the same time, the results also support that those with less education and non-educated pharmacists have gained from the deregulation. While initially surprising, this can arise for several reasons. First, as explained in Section 1.4, the group of non-pharmacists consists to a large extent of pharmacy technicians. This group is tied to the pharmacy industry and is consequently likely to experience positive wage gains from re-

[49]This is also in line with Goldin and Katz (2016) who find that the pharmacy profession in the U.S. is among the most egalitarian profession of all.

duced market concentration. The majority of pharmacy technicians have less than post-secondary education. Second, the similarity in point estimates for pharmacists and non-pharmacists could relate to the bargaining power of pharmacists before the deregulation. If their bargaining power is high – pharmacies are not allowed to operate without a pharmacist on site – this can limit the firm's labor market power in the pre-period and attenuate the estimated gain from reduced labor market concentration for pharmacists. In the absence of this bargaining power, we might expect larger returns to reduced labor market concentration for pharmacists than for less specialized labor.

1.8 Conclusion

This paper studies a classic question in novel way. It uses a rare natural experiment that provides geographic variation in employer concentration coupled with rich matched employee-employer data to estimate the effect of reduced labor market concentration on wages. In 2009, the Swedish state-run pharmacy monopoly was deregulated. Entry barriers were lifted and private firms entered the market. Because pharmacists and pharmacy technicians primarily work in pharmacies, the reform provides a stark change in employer concentration for employees in the industry. The change in labor market concentration differs across local markets. Making use of this geographic variation in a difference-in-difference design, elasticities of wages with respect to labor market concentration are estimated to be between -0.02 and -0.05. The empirical strategy relies only on the variation in concentration induced by the sale of pre-existing pharmacies in 2009 to remedy the concern that actual labor market concentration is endogenous. This variation in concentration arises directly from the privatization of pharmacies, a process controlled by the policymaker, but does not relate to the opening of new pharmacies post-reform. The positive wage effects from reduced labor market concentration are robust to extensive checks and are not observed in a similar, but unaffected, industry.

Regarding who benefits from reduced labor market concentration, similar positive wage returns are found for employees of different age, country of birth, gender, educational level and educational specialization. Effects differ for individuals who make different career moves: the positive wage effects primarily arise for stayers, but not new hires, and those with longer tenure and more experience. This does not mean that wages do not respond positively to changing employer. Rather, conditional on changing employer, the results suggest that it does not matter for wages whether the move is made in a local market where the change in labor market concentration is high or low. The result should be interpreted with caution, however, as the likelihood of moving is itself affected by the deregulation.

Overall, the paper lends support to the growing literature that finds that labor market concentration can matter for workers' wages. It finds that wages respond positively to reduced employer concentration in a context with high industry-specificity in skills. This is likely to be true in many other settings where labor is similarly tied, such as teachers or health professionals.

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Appendix A: Additional description and results

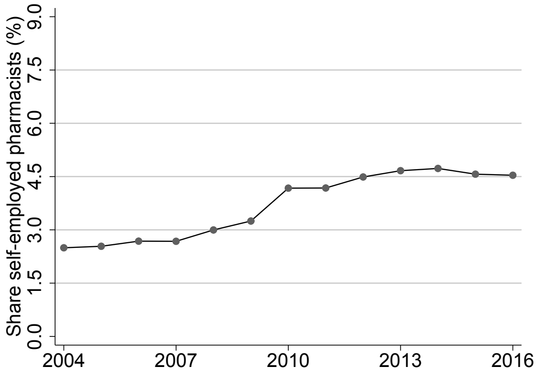
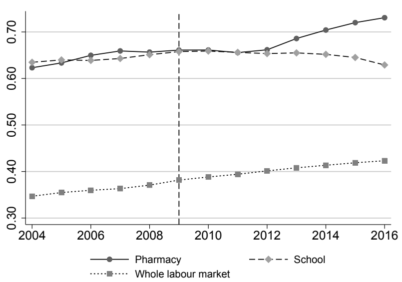
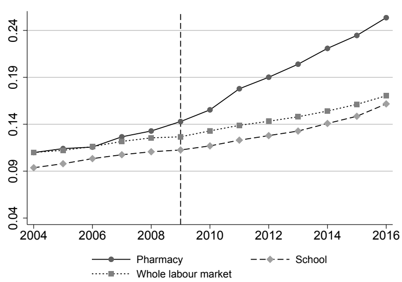


Figure A.1. Self-employment among pharmacists

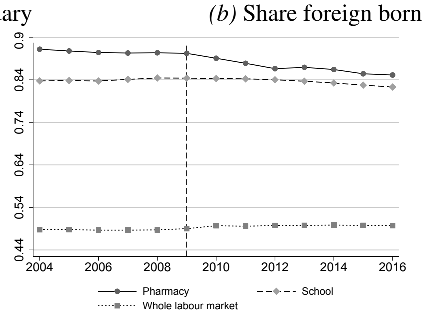
Note: The figure shows the share of all educated pharmacists that are self-employed.



(a) Share post-secondary

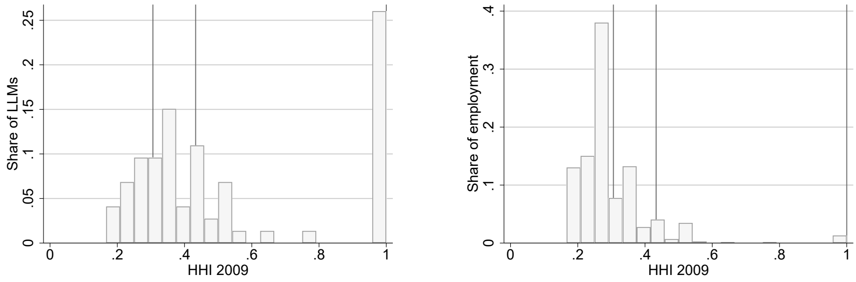


(b) Share foreign born



(c) Share female

Figure A.2. Worker composition in pharmacy, school sector and whole labor market



(a) LLMs *(b)* Employment

Figure A.3. Distribution of $HHI_{m,2009}$

Note: The figure shows the distribution of $HHI_{m,2009}$ across LLMs (Panel A) and employees (Panel B). The three lines mark the 25th, 50th and 75th percentiles of the distribution of $HHI_{m,2009}$ across LLMs. They are 0.306, 0.433 and 1 respectively.

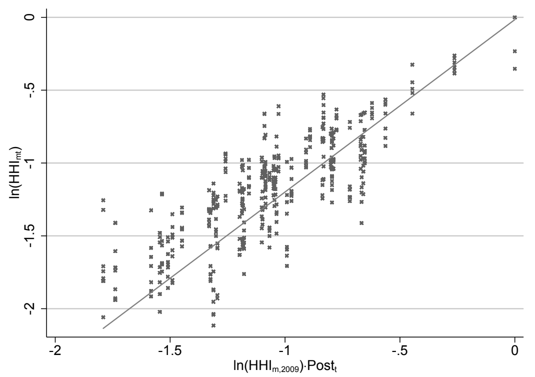


Figure A.4. Scatter of $\ln(HHI_{mt})$ vs. $\ln(HHI_{m,2009}) \times Post_t$

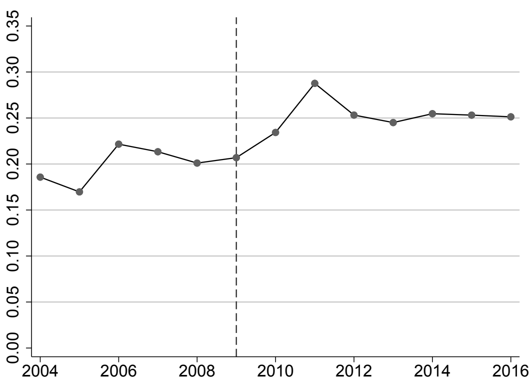


Figure A.5. Share new hires to pharmacy (pre-reform pharmacies)

Table A.1. *Means by change in labor market concentration (2004–2008)*

	$HHI_{m,2009}$ interval		
	≤25th (*high*)	>25th & ≤50th (*medium*)	>50th (*low*)
Panel A: Characteristics of LLMs			
Employment	433.44	138.68	34.64
Pharmacies	31.16	12.38	3.59
Population ('000)	341.19	100.70	25.08
$HHI_{m,2009}$	0.25	0.35	0.76
$HHI_{m,2009}$ (empl. weighted)	0.25	0.35	0.55
Number of LLM-year obs.	95	85	185
Number of LLMs	19	17	37
Panel B: Characteristics of employees			
Female	0.91	0.91	0.89
Age (years)	45.64	45.36	45.80
Age < 30	0.16	0.17	0.17
Age ≥ 50	0.47	0.46	0.49
Foreign born	0.14	0.09	0.04
Post-secondary	0.66	0.61	0.58
Pharmacist	0.52	0.50	0.48
Tenured	0.50	0.47	0.52
Industry experienced	0.79	0.76	0.71
Monthly wage (2004 SEK)	24,650	23,492	23,102
Monthly earnings (2004 SEK)	22,459	21,449	20,975
Number of employee-year obs.	41,177	11,788	6,408
Number of employees (2008)	8,182	2,366	1,273

Note: The table shows means across LLMs (panel A) or employees (panel B) for the pre-deregulation period (2004 to 2008), separately by three groups of changes in labor market concentration: "High" (where $HHI_{m,2009}$ is less than or equal to the 25th percentile of the distribution of $HHI_{m,2009}$ by LLM), "Medium" (where $HHI_{m,2009}$ is above the 25th but less than or equal to the 50th percentile), and "Low" (where $HHI_{m,2009}$ above the 50th percentile). The 25th (50th) percentile of $HHI_{m,2009}$ is 0.306 (0.433). Foreign born are born in a country other than Sweden. Tenured hold at least five years of tenure at a pharmacy. Industry experienced hold at least five years of experience from the pharmacy market. $HHI_{m,2009}$ (empl. weighted) is the mean value of HHI 2009, weighting by employment in 2008.

Table A.2. *Top 5 industries for pharmacists (2016)*

	%
Dispensing chemist	63.90
Manufacture of pharmaceutical preparations	6.16
Wholesale of pharmaceutical goods	4.61
Specialized hospital somatic activities	3.60
Inspection, control, permit & licensing activities of central & local gov't	3.16

Note: The table shows the top 5 industries for pharmacists in Sweden in 2016 by share of employment. Pharmacists are identified by their educational level and specialization. Industries are defined by five-digit SNI codes.

Table A.3. *The effect of market concentration on* $\ln(earnings)$ *– OLS, RF and IV results*

	(1)	(2)	(3)	(4)	(5)
	OLS			IV	
Panel A: OLS & IV					
$ln(HHI_{mt})$	-0.035**	-0.051***	-0.034***	-0.029**	-0.019*
	(0.014)	(0.012)	(0.012)	(0.013)	(0.010)
R^2	0.082	0.306	0.726		
Panel B: First stage					
$ln(HHI_{m,2009}) \times Post_t$				1.035***	1.063***
				(0.108)	(0.091)
F-statistic				91.66	136.80
Panel C: Reduced form					
$ln(HHI_{m,2009}) \times Post_t$	-0.003	-0.030**	-0.020*		
	(0.012)	(0.015)	(0.012)		
R^2	0.081	0.305	0.726		
N	137,664	137,511	131,149	137,511	131,149
Year FE	Yes	Yes	Yes	Yes	Yes
LLM FE	Yes	Yes		Yes	
Person × LLM FE			Yes		Yes
Controls		Yes		Yes	

Note: This table provides the results of estimating equations (1.8) and (1.9) for the outcome log earnings. OLS and IV results are presented in Panel A. The first stage in Panel B show the results of regressing $\ln(HHI_{mt})$ on the instrument $\ln(HHI_{m,2009}) \times Post_t$ and exogenous regressors. Controls are age (in five categories), gender, foreign born, pharmacist, level of education (in five categories), tenure (in three categories) and industry experience (in three categories). Standard errors are clustered by LLM and reported in parentheses. The F-statistic is the Kleibergen-Paap Wald rk F-statistic.

Table A.4. *Effect of labor market concentration on* $\ln(wage)$ *– pharmacy stayers and new hires*

	(1)	(2)	(3)	(4)
	Stay	*Join*	*Plant-to-plant*	*Non-employment*
$ln(HHI_{m,2009}) \times Post_t$	-0.049***	-0.023	-0.022	-0.041
	(0.014)	(0.015)	(0.013)	(0.028)
R^2	0.569	0.540	0.532	0.513
N	79,208	21,139	16,679	4,458
Year FE	Yes	Yes	Yes	Yes
LLM FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes

Note: This provides estimates of γ from equation (1.9) for the outcome $\ln(wage)$ for different sub-samples, specified in the column headings. A joiner is someone who is working at the pharmacy in year *t* but not in year *t-1*, while a stayer is someone who is neither a joiner in year *t* or *t+1* (see text for full details). Columns 3 and 4 break out the new hires into two mutually exclusive types: those coming from another pharmacy or those coming from non-employment. Controls are included for age, gender, foreign born, pharmacist, level of education, tenure and industry experience. Standard errors are clustered by LLM and reported in parentheses.

Table A.5. *Wages – new hires vs. non-new hires*

	(1)	(2)
Panel A: Firm hires		
Join firm	-0.064***	-0.047***
	(0.004)	(0.006)
Join firm × Post	0.070***	0.058***
	(0.008)	(0.004)
R^2	0.545	0.573
N	110,722	93,872
Year × firm FE	Yes	Yes
Panel B: Pharmacy hires		
Join pharmacy	-0.014***	-0.007**
	(0.003)	(0.003)
Join pharmacy × Post	0.026***	0.022***
	(0.008)	(0.007)
R^2	0.674	0.691
N	109,778	92,555
Year × pharmacy FE	Yes	Yes
Sample	Full	Incumbents
Controls	Yes	Yes

Note: The table compares wages for new hires and non-new hires in the same firm or pharmacy in the pharmacy industry. It provides estimates of θ_1 and θ_2 from estimating $\ln(w_{it}) = \theta_1 Join_{ipt} + \theta_2 Join_{ipt} \times Post_t + \lambda_{pt} + \beta X_{it} + \varepsilon_{it}$, where p is either the firm or the pharmacy. A joiner to a firm (pharmacy) is someone who is working at the firm (pharmacy) in year t but not in year *t-1* (see text for full details). In column (1) the full sample is included, while in column (2) only incumbents are included, defined to be employees who are working in the pharmacy industry in the pre-period. Controls are included for age (in five categories), gender, foreign born, pharmacist, level of education (in five categories), tenure (in three categories) and industry experience (in three categories). Standard errors are clustered by LLM and reported in parentheses.

Table A.6. *Heterogeneous effects of labor market concentration on* $\ln(wage)$ *– tenure and experience*

	(1)	(2)	(3)
Tenure (years):	*<3*	*3–4*	*≥5*
$ln(HHI_{m,2009}) \times Post_t$	-0.010	-0.036**	-0.049***
	(0.017)	(0.015)	(0.018)
Mean log wage (pre)	10.05	10.08	10.1
R^2	0.544	0.566	0.566
N	34,007	22,190	54,525
Industry experience (years):	*<3*	*3–4*	*≥5*
$ln(HHI_{m,2009}) \times Post_t$	-0.006	-0.042	-0.050***
	(0.032)	(0.027)	(0.013)
Mean log wage (pre)	9.88	9.93	10.12
R^2	0.502	0.542	0.554
N	9,375	12,200	89,145
Year FE	Yes	Yes	Yes
LLM FE	Yes	Yes	Yes
Controls	Yes	Yes	Yes

Note: This provides estimates of γ from equation (1.9) for different sub-samples, specified in the column headings. Controls are age (in five categories), gender, foreign born, pharmacist, level of education (in five categories), tenure (in three categories) (only in the bottom panel) and industry experience (in three categories) (only in the top panel). Standard errors are clustered by LLM and reported in parentheses.

Table A.7. *Heterogeneous effects of labor market concentration on* ln(*wage*) *– individual characteristics*

	(1)	(2)	(3)	(4)	(5)	(6)
	Female		Foreign born		Young	
	no	*yes*	*no*	*yes*	*no*	*yes*
$ln(HHI_{m,2009}) \times Post_t$	-0.067	-0.047***	-0.042***	-0.037*	-0.045***	-0.042**
	(0.052)	(0.008)	(0.010)	(0.019)	(0.014)	(0.017)
Mean log wage (pre)	10.18	10.08	10.09	10.04	10.14	9.96
R^2	0.528	0.571	0.553	0.593	0.538	0.569
N	11,094	99,626	92,654	18,067	71,109	39,612
	Post secondary		Pharmacist			
	no	*yes*	*no*	*yes*		
$ln(HHI_{m,2009}) \times Post_t$	-0.042***	-0.052***	-0.046**	-0.043***		
	(0.016)	(0.011)	(0.019)	(0.007)		
Mean log wage (pre)	9.88	10.19	9.93	10.22		
R^2	0.480	0.391	0.493	0.400		
N	35,443	75,279	52,975	57,747		
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
LLM FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes

Note: This provides estimates of γ from equation (1.9) for different sub-samples, specified in the column headings. Young is defined to be someone below age 40. Controls are age (in five categories), gender, foreign born, pharmacist, level of education (in five categories), tenure (in three categories) and industry experience (in three categories). Standard errors are clustered by LLM and reported in parentheses.

Appendix B: Robustness checks

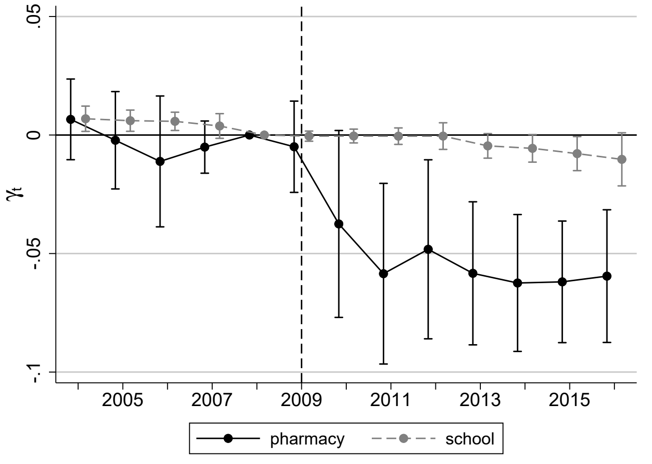


Figure B.1. Placebo analysis of the effect of market concentration on wages

Note: The figure plots γ_t from Equation (1.10) with 95% confidence intervals for regressions that are run separately for the pharmacy industry and for the school sector (preschools and compulsory schools). The outcome is log wages. Included in X_{imt} are age (in five categories), gender, foreign born and level of education (in five categories). Standard errors are clustered by LLM.

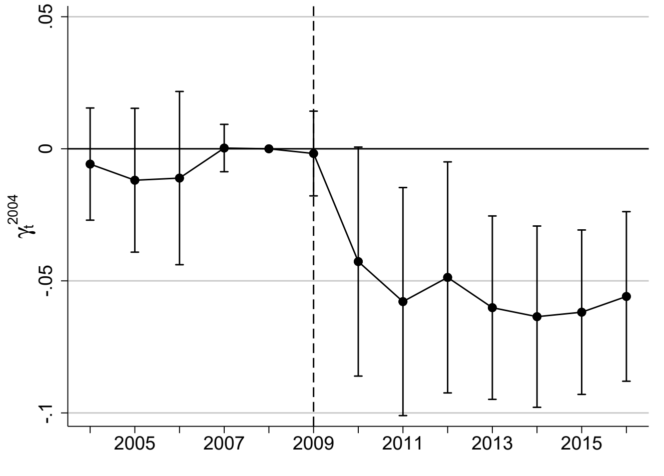


Figure B.2. The effect of $\ln(HHI_{m,2009})$ *on* $\ln(wage)$ *– 2004 pharmacies*

Note: The figure plots γ_t from Equation (1.10) with 95% confidence intervals. *HHI* is based on employment and pharmacies from 2004 and ownership structures from 2009. Included in X_{imt} are age (in five categories), gender, foreign born, pharmacist, level of education (in five categories), tenure (in three categories) and industry experience (in three categories). Standard errors are clustered by LLM.

Table B.1. *Robustness – Effect of labor market concentration on* $\ln(wage)$

	(1)	(2)
Panel A: Baseline		
$ln(HHI_{m,2009}) \times Post_t$	-0.048***	-0.022***
	(0.011)	(0.006)
R^2	0.551	0.924
N	110,722	104,968
Panel B: Control for nr employees		
$ln(HHI_{m,2009}) \times Post_t$	-0.046***	-0.020***
	(0.010)	(0.006)
R^2	0.551	0.924
N	110,722	104,968
Panel C: Control for value added		
$ln(HHI_{m,2009}) \times Post_t$	-0.040***	-0.017***
	(0.011)	(0.006)
R^2	0.544	0.926
N	99,052	93,588
Panel D: Omit urban areas		
$ln(HHI_{m,2009}) \times Post_t$	-0.039***	-0.014*
	(0.011)	(0.008)
R^2	0.607	0.921
N	58,254	55,326
Panel E: No managers		
$ln(HHI_{m,2009}) \times Post_t$	-0.051***	-0.023***
	(0.010)	(0.006)
R^2	0.566	0.913
N	104,041	98,272
Panel F: Public sector only		
$ln(HHI_{m,2009}) \times Post_t$	-0.051***	-0.013***
	(0.019)	(0.004)
R^2	0.541	0.945
N	87,237	82,697
Panel G: Control for mean wage		
$ln(HHI_{m,2009}) \times Post_t$	-0.047***	-0.021***
	(0.011)	(0.006)
R^2	0.551	0.924
N	110,722	104,968
Year FE	Yes	Yes
LLM FE	Yes	
Person $\times$ LLM FE		Yes
Controls	Yes	

Note: This provides robustness checks for estimating equation (1.9). Column (1) controls for age, gender, foreign born, pharmacist, education, tenure and industry experience. Both columns control for log number of employees per LLM (Panel B), log value added per employee (Panel C), and mean log wages on the whole labor market by CZ and year (Panel G). Value added is available until 2015. Urban areas are LLMs that encompass Stockholm, Gothenburg and Malmö (the three largest cities in Sweden). There is some measurement error which reports a too high share in the public sector particularly between 2009 and 2011. Omitting these years leads to somewhat larger point estimates in absolute terms. Standard errors are clustered by LLM and reported in parentheses.

2. Firms, Productivity, and the Immigrant-Native Earnings Gap

Co-authored with Olof Åslund, Cristina Bratu and Stefano Lombardi

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2.1 Introduction

The extent, sources, and remedies of immigrant labor market disadvantages seen in many Western economies are topics of intense scholarly and political debates. A vast literature documents wage and employment gaps relative to native workers, greater among recent migrants but present for decades.[1] Lack of country-specific human capital, such as language skills, is a commonly proposed explanation to why outcomes are particularly poor among the recently arrived. Other sources of inequalities are less immigrant-specific, but highly relevant. Substantial bodies of work investigate factors like employer discrimination, residential segregation, and the importance of networks and contacts.[2]

A much smaller but growing literature considers the role of firms and their hiring and pay-setting practices. These studies are partly motivated by two empirical observations: (i) there are significant and growing differences in wages and earnings across firms and establishments (Nordström Skans et al. 2007; Card et al. 2013; Barth et al. 2016), and firm practices increase inequality across groups in general (Card et al. 2016; Card et al. 2018; Gerard et al. 2018); (ii) there is substantial origin-based workplace segregation, which is also correlated with economic outcomes of individuals and groups (Hellerstein and Neumark 2008; Åslund and Nordström Skans 2010).

This study focuses on a particular aspect of firms, namely firm productivity. Consistent with models of rent-sharing that imply a positive relationship between wages and firm productivity (Manning 2011), our population-wide linked employer-employee data for Sweden show that persistent measures of firm value added per worker are strongly related to firm-level earnings. We study how this measure of productivity relates to the allocation and workplace mobility of immigrant and native workers, the group-specific earnings gains from working at more productive firms, and how these factors relate to overall immigrant-native earnings gaps.

Sweden provides an interesting case for several reasons. First, Sweden is a diverse country, thanks largely to its immigration. Over the past decades the country has experienced substantial and varied economic and humanitarian immigration, bringing the fraction of foreign-born close to 20 percent in 2020 (SCB 2020). Second, the overall immigrant-native employment differentials are among the greatest in the OECD, and our data show that the raw earnings ratio decreased from about 0.92 to 0.86 between 1998 and 2017. However, in line with existing evidence on within- and across-firm wage dispersion, the within-firm earnings gap is smaller and has not increased over time. Third, the country's low wage dispersion and high degree of unionization and collective bargaining may imply different roles for firm policies than in less regulated labor markets.

[1] See Kerr and Kerr (2011), Borjas (2014), and Duleep (2015) for overviews.

[2] See e.g. Neumark (2018) on discrimination, Gobillon et al. (2007) and Chetty et al. (2020) on segregation, and Dustmann et al. (2016) on networks.

Previous studies show that between-workplace variation explains significant shares of immigrant-native earnings gaps (Barth et al. 2012; Damas de Matos 2017; Dostie et al. 2020). There is also evidence that workplaces are related to the assimilation process. Eliasson (2013) finds that most earnings convergence occurs within establishments rather than through transitions between workplaces. Ansala et al. (2020) find that workplace conditions are strongly related to entry job earnings and subsequent performance among immigrants. Arellano-Bover and San (2020) find that recent migrants tend to work in low-paying firms, while access to higher-paying firms over time explains a significant fraction of immigrant-native pay differences.[3]

In addition to an impact of sorting over "good" and "bad" firms, the immigrant native earnings gap can also be influenced by systematic across-group differences in the premium from working in a specific type of firm. The latter phenomenon can for example arise if firms have more market power over immigrants than natives.[4] Manning (2020) highlights that when labor markets are less competitive, wages will be more closely linked to reservation wages than to worker productivity. Hirsch and Jahn (2015) find that immigrants supply labor to the firm less elastically than natives. Bassier et al. (2020) show that the degree of monopsony power is higher in low-wage labor markets.

To capture persistent differences in firm productivity, we rank firms based on average productivity over our sample period, 1998 to 2017. Average productivity comes from a regression of log value added per worker on firm and year fixed effects. The procedure allows us to classify firms into a tractable number of groups, and is similar in spirit to Bonhomme et al. (2019) who bin firms into classes via *k*–means clustering. We use productivity percentiles instead of clusters, and argue that there is value in basing the analysis on an easily observable measure of productivity. We find no indication that this measure is influenced by the composition of workers in the firm, including the share of immigrants the firm employs.

We begin by documenting the association between productivity and earnings at the firm level. Based on the firm productivity ranking, we find a positive and gradually increasing relationship between firm productivity and earnings starting from the 20th percentile of the firm productivity distribution. Using an AKM model (Abowd et al. 1999) to control for worker heterogeneity, we show an almost linear association between estimated firm earnings premiums and the productivity rank across the entire distribution of firms. AKM results also suggest that firm effects explain more of the earnings variation among immigrants than natives,[5] and that there is little sorting of high-earning migrants

[3]Orefice and Peri (2020) find that immigration can also drive positive assortative matching between workers and firms, with consequences for the overall wage structure.

[4]Closely linked are mechanisms related to employer discrimination and immigrants having worse bargaining positions.

[5]When we estimate AKM models separately for immigrants and natives, firm effects account for 11% (22%) of native (immigrant) earnings variation.

into high-earning firms. In other words, where you work appears to be even more important for immigrant workers than for native workers.

When looking at the distribution of workers across firm types, we find that foreign-born workers are over-represented in low-productive firms and under-represented in the upper part of the productivity distribution relative to natives. Comparing the 1998–2009 period to the 2010–2017 period, we find that this sorting has become stronger over time. This pattern holds also when controlling for compositional changes in the immigrant population, even though the increased under-representation in high-productive firms is related to changes in source countries. In general, people born outside Western countries (and thus less likely to be economic migrants) are relatively more concentrated in low-productive firms.

Average earnings are higher among natives than immigrants in all firm productivity deciles. The raw difference is greater where the immigrant share of the workforce is higher (about 15 log points in decile 2 to 4, 16% foreign-born) compared to the highest deciles (5 log points, 8%). Estimates controlling for worker fixed effects suggest that the earnings returns to working in a firm of high persistent productivity are substantial and positive for both groups, but greater for immigrants. For example, for natives there is a 7 log point difference between working in the fifth compared to the first decile. For immigrants, the difference is over 10 log points. Relating within-employment spell (and thus within-firm) variation in value added per worker to individual earnings gives a similar picture. A doubling of firm value added per worker is estimated to raise native earnings by around 2% and immigrant earnings by around 3%. The differential returns are primarily found at lower levels of firm productivity and are primarily driven by immigrants from non-Western countries. Western migrants have returns very close to those of natives.

Given that firm productivity matters for worker outcomes, it is relevant to study transitions between firm types and to ask who gets access to high-productivity firms. For all starting productivity deciles, natives are more likely to have moved upwards five years later. The immigrant-native difference in mobility is about 5 percentage points up to and including the 7th productivity decile, which is substantial relative to a baseline mobility of 10–15 percent. Differences across subgroups of the foreign-born also suggest that upward mobility is linked to overall socioeconomic positions. First, upward mobility is more common among immigrants with 10 or more years in the host country than among those who arrived more recently. Second, Western migrants transition to more productive firms more often than non-Western migrants. Third, upward mobility is less prevalent when working in peer-dense firms than when one has more native colleagues.

Finally, we present a decomposition analysis to evaluate the contribution of firm productivity pay premiums to the immigrant-native earnings gap. The average premium is the sum of sorting across deciles, and a pay-setting component for working in a given decile (relative to working in the lowest productivity decile). This average is 1 percentage point higher for immigrants, amounting to 8% of the earnings gap in the overall sample. Importantly, however, sorting and pay-setting work in opposite directions. Assuming migrants had the same returns to firm productivity as natives, their over-representation in less productive firms increases the earnings gap by 10%. The sorting component is particularly striking for women (20% of the gap) and those up to the age of 30 (about 30%), but for both of these groups the overall earnings gap is also much smaller (around 4%). On the other hand, if the allocation across firm types would have been the same among immigrant and native workers, the higher returns among immigrants would have reduced the gap by around 18%.

In sum, we find that: (i) more productive firms pay higher wages; (ii) earnings gains from firm productivity are greater for immigrant groups with poor average labor market positions, especially in the lower part of the productivity distribution; (iii) immigrants are more concentrated in low-productivity firms, and have lower rates of upward mobility. Higher returns are consistent with greater monopsony power over immigrants in the lower part of the firm productivity distribution. A greater earnings gap in the lower end means a steeper gradient to the pay offered by more productive firms. Our findings are also consistent with greater mismatch between firms and workers for immigrants. The relative concentration in low-productive firms and lower upward mobility rates fit predictions of ethnically segregated and segmented labor markets (Reich et al. 1973; Massey and Denton 1993).[6]

Since immigrants gain more from entering better firms but do so less frequently, it seems likely that that there are thresholds for immigrants to climb the productivity ladder. There are of course many potential candidates for such thresholds; for example, language barriers or manager hiring practices following ethnic delineations (Åslund et al. 2014). From a policy perspective, it is particularly striking that immigrant groups with poor labor market positions deviate the most from natives in sorting, mobility, and returns. This speaks against voluntary sorting due to worker preferences, and signal the individual and societal gains from more equal employer access.

The rest of the paper proceeds as follows. In Section 2.2 we outline the data and main sample. In Section 2.3 we present descriptive evidence on the native-immigrant earnings gap, focusing on the role of firms in explaining this gap. Section 2.4 explains how we measure firm productivity. Our main results are included in Section 2.5, where we analyze the sorting of immigrants and natives across the firm productivity distribution, as well as the earnings returns associated with working in more productive firms. This also considers mobility up the productivity distribution and presents a decomposition of the earnings premium. Finally, Section 2.6 concludes.

[6] The broader literature on transitions of workers up the job ladder and on worker sorting across firms also finds differences across groups of workers (see Haltiwanger et al. 2018).

2.2 Data and main sample

We use data for the entire Swedish population over the period 1998 to 2017, combining information from several administrative registers collected by Statistics Sweden. The RAMS matched employer–employee database is used to construct an employer-employee panel with yearly information on firm size in November, industry and total earnings paid by the firm. Statistics Sweden's business register on firm-level accounts provides information on value added (VA) for private firms. VA is defined as total value added at each production stage net of costs for intermediate goods and services, and is equal to total revenues minus intermediate consumption of goods and services.[7] We complement this information with a rich set of socioeconomic characteristics from the Louise/Lisa database.

We restrict our sample to people aged between 18 and 65 who are employed at firms in the private sector that have at least two employees in November. We drop the self-employed. For each employee we compute total annual earnings, job tenure, and total number of months worked at the primary employer.[8] All monetary values are deflated to 2010 Swedish Kronor (SEK) and winsorized at the 99th percentile of their respective yearly distribution. We drop individual histories if log-earnings in any year are three standard deviations or more above the sample mean. In addition, we drop observations where earnings from the primary employer are lower than the Price Base Amount (PBA) for 2010.[9] Our main outcome of interest is monthly earnings from the primary employer, obtained by dividing annual earnings by the number of months worked.

Immigrants are defined as foreign-born with two foreign-born parents. People born abroad to at least one Swedish-born parent are excluded from the sample. We also present results where immigrants are divided into "West" (i.e. Western Europe, USA and Australia) and "Rest of the world" based on country of birth.[10]

[7]Firm accounts are available until 2015. We eliminate from the starting sample firm observations for which VA information is missing. This results in about 12% employee-year observations being omitted. Table A.2 in the Appendix shows that the sample statistics are virtually unaffected.

[8]The primary employer is defined as the firm paying the highest yearly earnings. To compute job tenure we use data from 1985 onward.

[9]PBA is used to calculate benefits and fees in Sweden. Ruist (2018) argues that an earnings level equal to three times the PBA is a threshold for being self-supporting. Therefore, one PBA is a rather low threshold.

[10]"West" consists of the Nordics (Denmark, Finland, Norway and Iceland but not Sweden), Western Europe (Ireland, the UK, Germany, Greece, Italy, Malta, Monaco, Portugal, San Marino, Spain, the Vatican Sate, Andorra, Belgium, France, Liechtenstein, Luxembourg, the Netherlands, Switzerland and Austria), Canada, USA, Australia and New Zealand. "Rest of World" are countries that are not in the West.

2.2.1 Summary statistics

Table 2.1 shows summary statistics for our main analysis sample. Panel A shows worker-level characteristics. At the start of the sample period, 9% are immigrants. Out of these, 51% were born in the West while 49% were born in the Rest of the World. The share of immigrants has doubled over the sample period. This change is driven primarily by an increase in non-Western migrants. 18% of employees work at firms that are either completely native-segregated or completely immigrant-segregated. Considering how this varies by immigrants and natives, 6% of immigrants work at all-immigrant firms, and 20% of natives work at all-native firms.

The sample is more male than the population overall – 64% are men – and the average age is 40 years old.[11] Over time, the educational level has increased on average, reflected by a decline in the share with only compulsory education and an increase in the share with upper secondary education. Moreover, earnings (in 2010 prices) have increased from 21,000 SEK to 28,000 SEK between 1998 and 2017.

Turning to firm characteristics in Panel B, there are approximately 145,000 firms per year in the sample. The mean share of immigrants per firm has evolved roughly in line with the mean share of immigrants in the population. The mean masks the fact that there is a large portion of completely segregated firms: 62% of firms are native-segregated and 5% of firms are immigrant-segregated. The share of firms that are native-segregated has declined over time while the share that are immigrant-segregated has increased. Regarding firm size, there are on average 22 employees per firm (the median firm size is 6). Firm size has increased somewhat over time, where mean firm size was just under 21 in 1998 and 29 in 2017.

2.3 Immigrant-native earnings ratio

We start by analyzing the immigrant-native earnings gap and how it has evolved over time. Figure 2.1 reports the yearly immigrant–native earnings ratio between 1998 and 2017. It is given by the exponential of the coefficient θ_t from the following yearly regressions:

$$ln(e_{it}) = c + \theta_t imm_i + \beta_t X_{it} + \lambda_{jt} + \varepsilon_{it} \quad (2.1)$$

$ln(e_{it})$ are log monthly earnings for worker i in year t at the primary employer. Included in X_{it} are controls for age, age squared, gender, level of education (dashed line), as well as industry dummies $\lambda_{ind,t}$ (dotted line) or firm fixed effects $\lambda_{f,t}$ (triangles). The figure shows that there is a persistent earnings gap between natives and immigrants in all years. The raw immigrant-native earnings ratio has been declining over time, meaning that the gap in earnings

[11] The male concentration follows from the fact that we are focusing on the private sector only.

Table 2.1. *Summary statistics, analysis sample*

	1998–2017 (1)	1998 (2)	2017 (3)
Panel A: Employees			
Immigrant	0.13	0.09	0.18
Immigrant from West	0.04	0.05	0.03
Immigrant from Rest of World	0.09	0.05	0.15
Native-segregated firms	0.17	0.22	0.11
Immigrant-segregated firms	0.01	0.00	0.01
Male	0.64	0.66	0.62
Age	40.29	39.93	40.22
Age $\leq$ 30	0.27	0.27	0.29
Age $\geq$ 50	0.27	0.26	0.28
Education, compulsory	0.16	0.25	0.11
Education, secondary	0.55	0.54	0.53
Education, upper secondary	0.29	0.21	0.35
Education, missing	0.01	0.01	0.01
Monthly earnings (2010 SEK)	24,624	20,838	28,004
No. of employees $\times$ year	46,870,657	1,883,222	2,504,695
Panel B: Firms			
Fraction immigrants at employer	0.13	0.09	0.19
Yearly employer size	22.38	20.55	29.00
Share native-segregated firms	0.62	0.71	0.49
Share immigrant-segregated firms	0.05	0.03	0.07
No. of firms $\times$ year	2,883,741	129,322	115,210

has widened by approximately six percentage points between 1998 and 2017. Adjusting for age, gender, and education, the gap has widened even more, at approximately seven percentage points. The results with industry dummies indicate that the widening gap is present also within industries. However, adding firm fixed effects, the earnings ratio is higher in all years and even rises slightly over the period. It follows that earnings differences are substantially lower within than between firms. This suggests that the increase in the earnings gap is driven by differences between rather than within firms (see e.g. Tomaskovic-Devey et al. 2020), which motivates focusing on firms as one key element to understanding differences in earnings between immigrants and natives.[12]

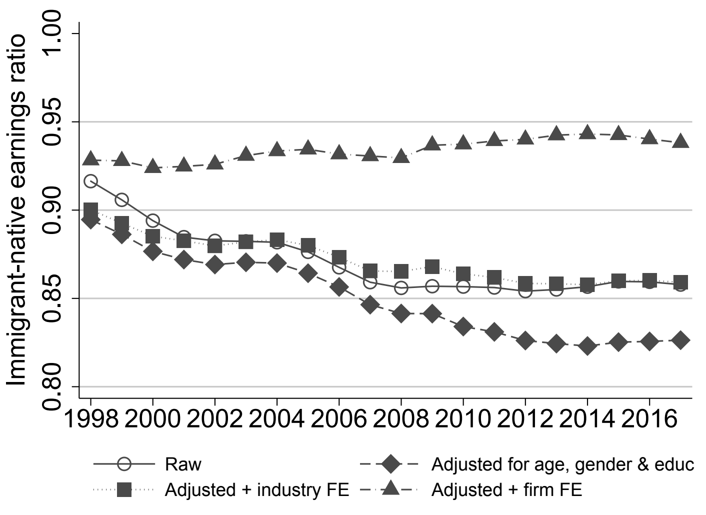


Figure 2.1. Immigrant–native earnings ratio

That the place of work is important for earnings is also confirmed when we estimate AKM models separately for immigrants and natives. Full results, including an outline of the AKM model, are included in Appendix Table A.1. The standard deviation of the estimated person effects are nearly twice as big as the standard deviation of the estimated firm effects for both natives and immigrants, indicating that a large share of earnings differences across workers depends on worker characteristics that are rewarded equally across firms. This

[12]In Appendix Figure A.1, we plot the earnings ratio of immigrants compared to natives separately for immigrants from Western countries and from Rest of World countries. The West/native earnings ratio is flat and equal to approximately 0.97 throughout the period, once we include controls. The Rest of World/native earnings ratio is much lower, approximately between 0.8 and 0.85 throughout the period. The overall earnings gap within firms has been closing over time when we compare the Rest of World immigrants to natives. Thus, at least some of the overall trends are driven by differences in the composition of migrants, bearing in mind that the share of Rest of World migrants has increased over time (see Table 2.1).

is in line with the large body of work that finds that skill differences across workers are important in explaining earnings. Even so, the results suggest that firms are also a key component in explaining earnings differences across workers.

Decomposing the overall variance of log earnings, firm effects are found to account for 11% of the overall variance in earnings for natives, and 22% for immigrants (see the bottom of Table A.1). The covariance of person and firm effects accounts for a further 6% for natives and only just over 1% for immigrants, suggesting that at least for immigrants, there is little sorting of high-wage workers to high-wage firms.

For the remainder of the paper, we focus on a measure of firm productivity based on value added per worker, as opposed to the AKM firm fixed effects. This choice allows us to give an explicit interpretation to the relationship between firms and the immigrant-native pay gap, as well as include segregated firms in all results, a feature which is relatively common among immigrants and natives (see Table 2.1).[13] Nonetheless, as we show in the next section, the firm fixed effects from an AKM model are positively correlated with firm productivity based on value added per worker.

2.4 Firm productivity ranking

2.4.1 Firm productivity and earnings

In the previous section, we showed that there are substantial immigrant-native earnings gaps, particularly between firms. Differences in firm productivity could potentially explain this finding.[14] This section presents our procedure for capturing firm value added, describes how this measure of persistent firm productivity is related to firm size and industry, and analyzes its association with earnings at the firm level.

In order to obtain a measure of persistent firm productivity that abstracts from fluctuations due to the business cycle and productivity shocks, we start by estimating the following model:

$$\ln(VA/N)_{ft} = \lambda_f + \lambda_t + \varepsilon_{ft} \tag{2.2}$$

where the fixed effects λ_f capture the permanent component in firm-level productivity and λ_t account for yearly effects common across all firms.

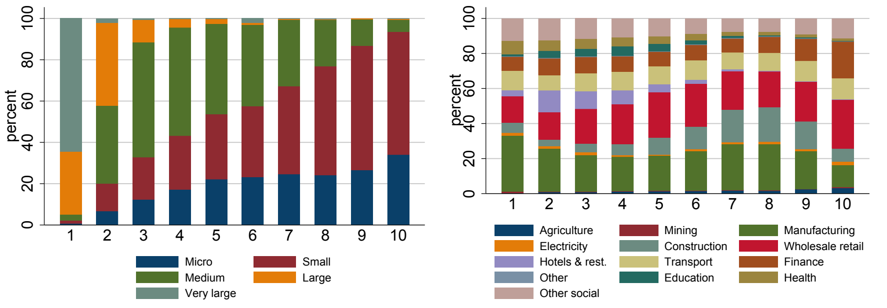
We estimate equation (2.2) using all firms for which information on both value added and firm size is available in at least two years in the matched

[13]We also avoid using firm fixed effects due to known possible incidental parameter problems when estimating the AKM (Andrews et al. 2008). Recent papers have proposed ways to tackle these problems (Kline et al. 2019; Bonhomme et al. 2020), but in our context, due to the smaller size of the group of immigrants, the bias may be particularly severe, further complicating how to interpret the results.

[14]For an extensive overview of why productivity differs between firms, see Syverson (2011).

employer-employee panel during 1998–2015, after restricting firm size to be larger than one.[15] Next, we use the empirical distribution of the $\widehat{\lambda}_f$ firm effects to rank firms into deciles or percentiles. Similar to the clustering procedure used by Bonhomme et al. (2019), grouping firms in this way aims to improve the tractability of our analyses. By construction, each firm's position in the productivity distribution is fixed over time. Even though value added per worker is only available until 2015, we thus obtain a measure of persistent productivity until 2017 (as long as the firm exists in earlier years).

To understand what types of firms are found in each productivity decile, Figure 2.2 shows the distribution of employees by firm size (Panel a) and by industry (Panel b) within each productivity decile of the $\widehat{\lambda}_f$ distribution.



(a) Firm size

(b) Industry

Figure 2.2. Employment distribution within decile rank, by firm size or industry (1998–2017)

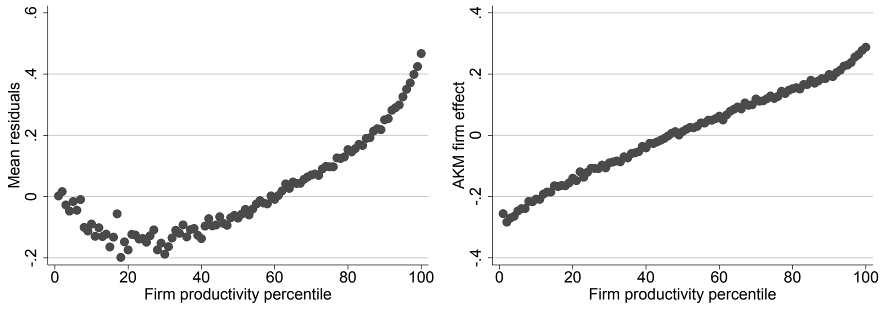
We group firms into five size bands: up to 9 employees (micro), up to 50 employees (small), up to 250 employees (medium), below 1000 employees (large) and 1000 employees and above (very large). Panel (a) shows that small firms tend to be the most productive ones, whereas the first productivity decile primarily consists of the largest firms.[16] This confirms the importance of controlling for firm size in the analysis. Panel (b) additionally shows that virtually all industries are found in each productivity decile. Thus, working in more productive firms does not only reflect working in specific industries. Instead, the whole range of firm productivity types tends to be represented in the different industries.

Figure 2.3, Panel (a), shows log earnings (residualized by firm size band) averaged by each percentile of the firm productivity distribution. To understand how the common component of firm earnings relates to firm productivity above and beyond individual worker heterogeneity, in Panel (b) we plot the firm fixed effects obtained from a pooled AKM regression against productiv-

[15] Dropping the firm size restriction leaves the results qualitatively unaffected.

[16] This stands in contrast to the commonly proposed theoretical prediction that firm size and productivity are positively related, see for example Card et al. (2018).

ity percentiles. The results in Panel (a) indicate that, from the 20th percentile onward, there is a positive relationship between (residualized) earnings and productivity ranking.[17] The common firm-specific pay premium captured by firm fixed effects in the AKM model relates positively to the firm productivity ranking over the distribution of firm productivity percentiles.



(a) Log earnings

(b) AKM firm fixed effects

Figure 2.3. More productive firms pay higher earnings

Panel (b) of Figure 2.3 does not show a “hockey-stick” pattern as found by Card et al. (2016); that is, we do not find that firms in the bottom percentiles have similar earnings premiums. This arises because a large number of employees are clustered in a few unproductive firms at the bottom of the distribution. Consistently, when we instead rank firms by weighting by the number of employees, the relationship between firm FE and productivity ranking is flat in the bottom part of the distribution and positively sloped afterwards (Figure B.2 in the Appendix). Moreover, when we plot AKM firm fixed effects directly against mean log valued added per worker, we also find the characteristic “hockey-stick” patterns (Figure B.1). In the Appendix, we show that our main results in Section 2.5 below are robust to using the alternative employee-weighted ranking.

2.4.2 Robustness of the firm ranking

Table 2.2 compares the firm productivity ranking used in the main analysis with alternative ranking procedures. Panel A shows rank correlation coefficients for alternative specifications. Panel B reports the share of firms classi-

[17] Higher mean earnings in firms in the very lowest deciles could arise for several reasons. First, controlling for five firm size bands might insufficiently control for the firm-size wage premium. Second, the linear relationship in Panel (b) of Figure 2.3 indicates the presence of "high-wage workers" in low-productive firms. A third possibility is that large firms (often found in lower deciles) may in general offer more full-time contracts, implying higher earnings due to more hours worked. The latter, however, is not supported by the data: average or median percentage of full-time contracts doesn’t vary by firm size or value added per worker (graphs not included).

fied higher or lower in the ranking by at least 10 percentiles as compared to the baseline. Reassuringly, we find that the baseline ranking is robust to including additional controls or using different methods to generate the ranking.

Columns (1) to (3) of Table 2.2 show results when either producing the ranking by industry or controlling for the share of immigrants, or both. In general, this leaves the ranking qualitatively unaffected. For the ranking done by industry, despite some 18.5% firms being classified as belonging to a lower decile of the firm productivity distribution, the correlation with the baseline ranking appears to be substantial (0.93).

Log value added per worker may to some extent mechanically reflect that high-skilled workers are concentrated in certain firms; i.e. firm productivity may be a function of worker productivity. Column (4) reports results when we re-estimate equation (2.2) by including staff-composition characteristics averaged at the firm-year level (share of men, share of workers in each education category, average tenure at the firm, share of immigrants).[18] Also in this case the productivity ranking is virtually unaffected.

In Column (5) we rank firms using AKM fixed effects from an earnings regression. Consistent with the correlation between the AKM firm fixed effects and the firm productivity ranking shown in Figure 2.3, the two alternative ranking measures are positively correlated (correlation of 0.44). Recall that the AKM firm fixed effects capture firm-level premiums that might be attributable to several other time-fixed components on top of persistent productivity. In this sense, the observed degree of correlation between the AKM- and the productivity-based rankings appears to be sizable.[19]

The main analysis uses a firm-weighted ranking. An alternative is to use an employee-weighted measure of firm productivity. Since many large firms have low per-worker value added, the first few deciles of the firm-weighted ranking are extended into several deciles of the employee-weighted ranking. Our main results included in the next section are consistent with this pattern.

Finally, we have additionally tested the robustness of the ranking in various ways (detailed results are available upon request). Recall that in the baseline results, firm size is computed using the matched employer-employee panel. If we instead use a measure of firm size from the business statistics register, results are similar. This is also the case if we remove employees whose earnings are below the yearly price base amount from the analysis.

[18] Note that in order to alleviate some of these concerns, throughout our main analysis on the returns to working in more productive firms, we condition on individual-level fixed effects.

[19] Results are unaffected if we correlate the AKM ranking with an employee-weighted ranking.

Table 2.2. *Robustness of firm ranking*

	Industry	Share of immigrants	Industry and share of immigrants	Staff composition	AKM FE ranking
	(1)	(2)	(3)	(4)	(5)
Panel A: Correlation with baseline ranking					
	0.9294	0.9981	0.9276	0.9825	0.4430
Panel B: Share of firms moving in the ranking					
moving down	0.1856	0.0001	0.1844	0.0229	0.3369
moving up	0.0730	0.0062	0.0851	0.0345	0.2980
No. of firms	370,692	370,692	370,692	355,676	339,010

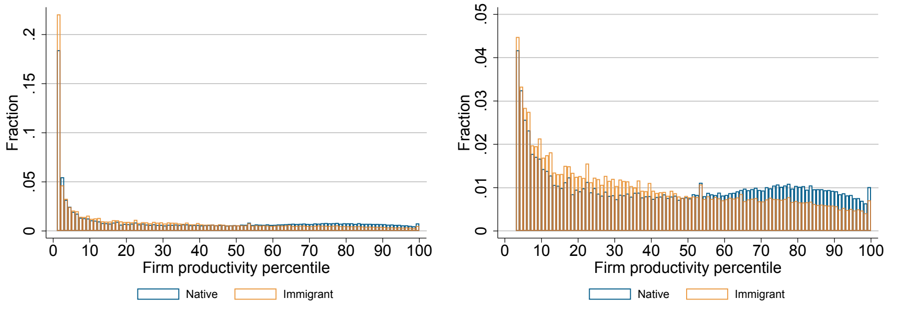
Notes: Panel A reports Spearman's rank correlations between the baseline productivity ranking percentiles and the following alternative measures: Column (1): ranking firms by industry; Column (2): controlling for the yearly share of immigrants at the firm; Column (3): ranking firms by industry and controlling for the share of immigrants at the firm; Column (4): controlling for education categories, gender, age, tenure, share of immigrants averaged at the firm-year level; Column (5): ranking by using AKM firm fixed effects from an earnings regression. Panel B reports the share of firms moving at least 10 percentiles in the ranking as compared to the baseline.

2.5 Sorting and returns to working in productive firms

The relationship between productivity and earnings for immigrants compared to natives can operate through several channels. First, it could be related to sorting: immigrants and natives may work in different types of firms. Second, it could be related to pay setting: in firms of a given productivity, immigrants and natives may be offered or negotiate different earnings (Dostie et al. 2020; Card et al. 2016). This can for example arise because firms have market power over workers and consequently firms are able to set lower wages to groups of individuals with more inelastic labor supply to the firm. Motivated by this, we consider sorting and earnings returns for immigrants and natives in this section.

2.5.1 Distribution of workers in the firm productivity distribution

Figure 2.4a presents the distribution of immigrants and natives across the firm productivity distribution. It shows that a large part of workers are employed in relatively low productivity firms. Figure 2.4b, which omits the first two percentiles, additionally shows that the distribution of immigrants is skewed to the left: immigrants are relatively more concentrated in low productivity firms, while natives are more concentrated in high productivity firms.[20]



(a) Full *(b)* Omit 1st and 2nd percentile

Figure 2.4. Distribution of immigrants and natives across productivity percentiles

Table 2.1 showed that, over the period we are studying, there has been an increase in the fraction of immigrants. This may have affected sorting over time. To investigate this, we break the data into the sub-periods 1998–2009 and 2010–2017. We then estimate the following linear probability model separately by sub-period p:

$$imm_i = \alpha_p + \sum_{d=2}^{10} \beta_{dp} decile_d + \varepsilon_{ip} \quad (2.3)$$

[20]We find very similar patterns when we restrict the sample of migrants to Rest of World immigrants with at least 10 years spent in Sweden who hold upper secondary education.

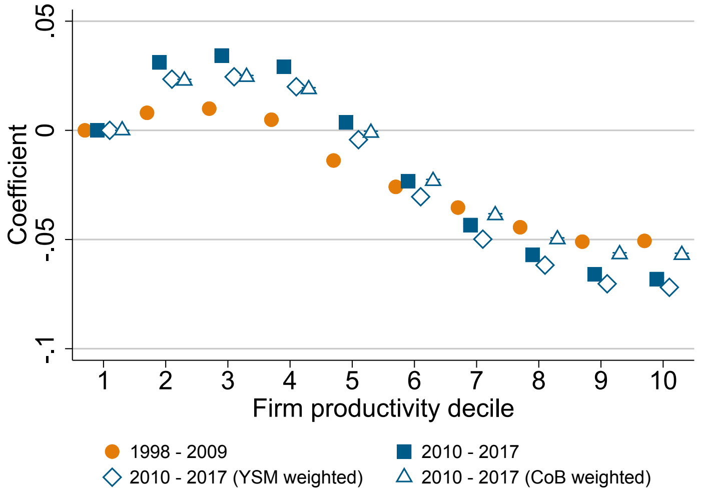


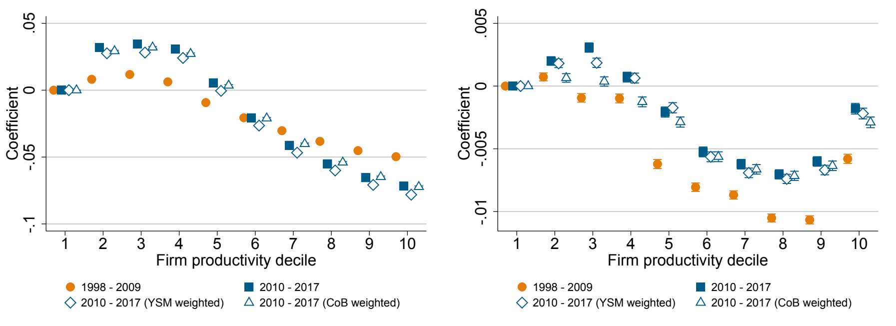
Figure 2.5. Sorting of immigrants across productivity deciles

where $decile_d$ refers to productivity decile. The first decile is omitted from the regressions such that the immigrant shares in a particular decile are estimated relative to the bottom decile.

Figure 2.5 plots $\hat{\beta}_{dp}$ from the regressions. It shows that immigrants have become relatively more concentrated in firms with lower productivity over time, compared to the share in the first decile. In particular, the second period estimates (shown by the dark blue squares) are above the first period (orange dots) at the bottom deciles and vice versa at the top.

One reason for the change in sorting could be due to compositional changes over the long time period considered. To account for this, we weight the second sub-period to match the first in terms of either country of birth (CoB) or years since migration (YSM) to Sweden. Weights are constructed as the ratio of the share in each respective country of birth or year since migration cell. The results (shown by the diamonds) suggest that the change in sorting is observed even when we re-weight by years since migration, though the change at the bottom deciles is somewhat attenuated. Re-weighting by country of birth (shown by the triangles), on the other hand, diminishes the decline at the top but the concentration at the bottom remains. Changes to the composition of workers with regards to their country of birth can thus explain the relative decline at the top but not the relative increase at the bottom.

In order to assess whether sorting differs for the two main groups of immigrants (West and Rest of the World), we estimate equation (2.3) separately for these groups. The first message of Figures 2.6a and 2.6b is that sorting is much stronger among non-Western immigrants (note that the scales are different). Regarding changes over time, Figure 2.6a suggests that the pattern for the Rest of the World group is similar to what we saw above: there has been a relative increase of immigrants in lower productivity firms and a relative decrease in higher productivity firms. When interpreting the results, it is important to bear in mind that the share of this group has increased in the first decile, from 8.6% to 13.8%. For Western immigrants in Figure 2.6b, the share of migrants has decreased in the first decile, from 4.9% to 4.1%. The levels suggest that there has been an aggregate increase in the share of migrants within each decile over time, both at the bottom and at the top of the productivity distribution, in line with the fact that immigration has increased over the period in general.



(a) Rest of World *(b)* West

Figure 2.6. Sorting of immigrants across productivity deciles – by country of birth

2.5.2 Earnings returns to working in more productive firms

Immigrants are relatively more concentrated in low-productivity firms than natives, and thus make up a larger share of employees in low-productive firms than in high-productive firms. Figure 2.7 shows that in the first five deciles, immigrants account for 14 to 16% of employment within each decile. At the top deciles, immigrants account for just over 8%.[21] Not only is there a higher share of immigrants in low-productivity firms, but the persistent raw earnings gap is particularly prevalent in lower productivity firms. Moreover, earnings of both natives and immigrants are higher in high-productivity firms, but there are fewer immigrants than natives as we go up the firm productivity distribution.

To more formally understand how earnings returns differ for immigrants and natives across the firm productivity distribution, we regress log monthly earnings at the primary employer $\ln(e_{it})$ on the firm productivity decile $decile_d$

[21] Figure A.2 shows that this pattern is predominantly driven by immigrants from the Rest of the world. Immigrants from Western countries account for 4% of employees within each decile over the sample period.

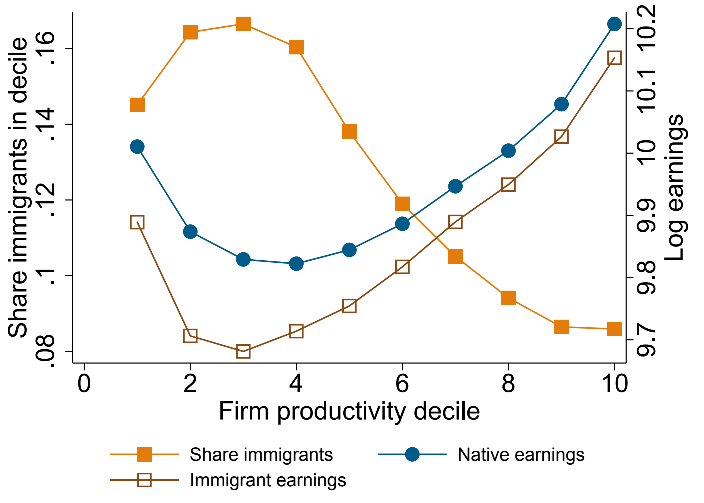


Figure 2.7. Share immigrants and raw log earnings by productivity decile

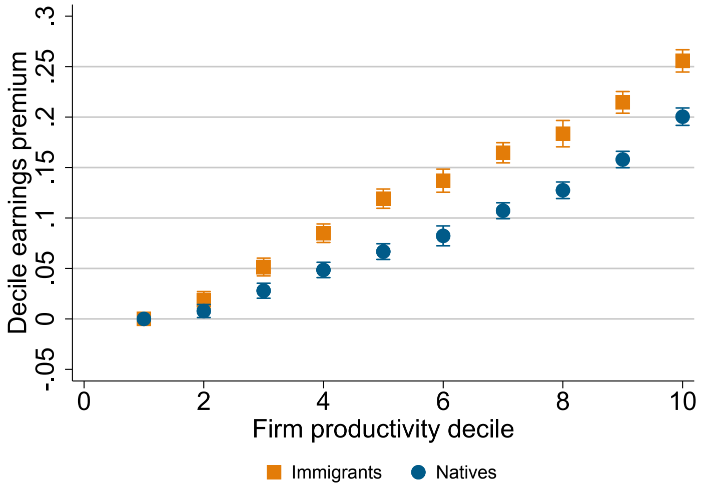
and the interaction of the decile and an immigrant dummy imm_i:

$$\ln(e_{it}) = c + \sum_{d=2}^{10} \theta_d decile_d + \sum_{d=2}^{10} \gamma_d decile_d \times imm_i + \alpha_i + \lambda_t + \beta X_{it} + \varepsilon_{it} \quad (2.4)$$

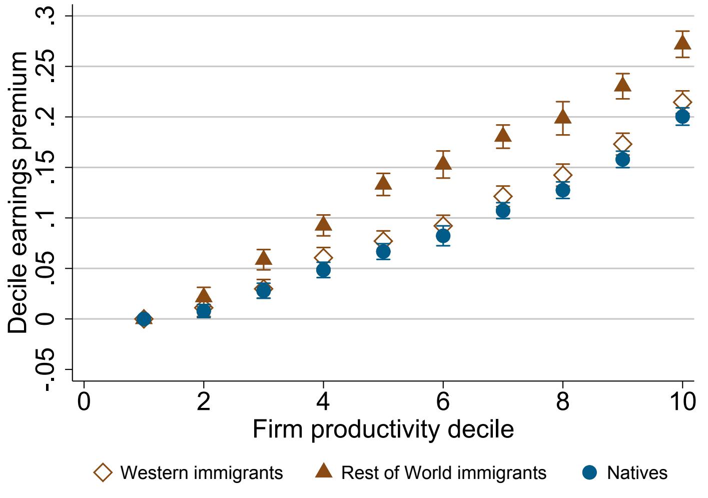
The estimand $\hat{\theta}_d$ ($\hat{\theta}_d + \hat{\gamma}_d$) are the earnings returns to natives (immigrants) of working in relatively more productive firms, compared to natives (immigrants) working in the first productivity decile. Thus, $\hat{\gamma}_d$ is the differential return to immigrants of working in more productive firms. We include individual fixed effects α_i to control for individual heterogeneity in earnings. The identification of the return by productivity is consequently only based on individuals that have transitioned across productivity deciles.[22] We include controls X_{it} for age polynomials (age squared and age cubed) and firm size band (micro, small, medium, large, very large), as well as the same controls interacted with the immigrant dummy to allow differential effects for immigrants and natives. We cluster standard errors at the firm level.

The results are included in Figure 2.8 below. Panel (a) is for the full sample of immigrants, while Panel (b) splits the comparison to natives into the Rest of the World and the West birth region groups. The blue dots show returns to natives ($\hat{\theta}_d$) while the orange dots show returns to immigrants ($\hat{\theta}_d + \hat{\gamma}_d$).

[22] Specifications excluding worker fixed effects give similar results, but the estimated coefficients $\hat{\theta}_d$ and $\hat{\theta}_d + \hat{\gamma}_d$ are much larger in size.



(a) All immigrants



(b) By immigrant group

Figure 2.8. Earnings returns to working in more productive firms

Note: Panel (a) plots $\hat{\theta}_d$ and $\hat{\theta}_d + \hat{\gamma}_d$ from equation (2.4) for the full sample of natives and immigrants. Panel (b) plots $\hat{\theta}_d$ and $\hat{\theta}_d + \hat{\gamma}_d$ from equation (2.4) for: (i) the full sample (circles); (ii) the full sample of natives and Western immigrants (diamonds); and (iii) the full sample of natives and Rest of World immigrants (triangles). All specifications include individual fixed effects, year fixed effects and controls as specified in Section 2.5.2.

For both immigrants and natives, there is a clear positive earnings return to working in more productive firms. For example, for the full sample in Panel (a), the estimated return to natives of working in the fifth decile compared to the first is 6.7 log points, and to immigrants nearly 12 log points. The return in the tenth decile is 20 log points for natives and 25 log points for immigrants. The returns to immigrants of working in more productive firms is generally higher than for natives. This differential return increases gradually in the bottom half of the productivity distribution, after which it remains flat at just over 5 log points. This pattern is clearest for immigrants from the Rest of the World. Immigrants from the West have earnings returns more similar to natives.[23] Recall that there is a persistent raw earnings gap between immigrants and natives across the whole productivity distribution (Figure 2.7). The results thus indicate that for immigrants who climb to more productive firms, each within-individual rung on the ladder is taller, not that immigrants have higher earnings than natives in more productive firms.

The returns to firm productivity may differ across groups of migrants. Above, we already considered how the returns differ by country of birth. Another important component is years since migration. As discussed in the introduction, a large literature suggests that immigrants become more similar to natives with years spent in the host country. Motivated by this, we re-run specification (2.4) above separately for immigrants that have spent less than 10 years in Sweden and for those who have spent at least 10 years in Sweden. The estimates from this exercise are similar to those presented above in the sense that both immigrants that have spent less than 10 years and at least 10 years in Sweden have higher returns than natives, see Figure B.4 in the appendix. However, immigrants with less than 10 years in Sweden experience even higher returns than those who have been in the country longer. This pattern is consistent with that immigrants become more similar to natives with time spent in the country.

Within-firm variation in productivity and earnings

Our definition of productivity is based on log value added per worker, where one firm is assigned a constant productivity rank throughout the sample period. The estimates above are based on comparisons across firms. Next, we consider how within-firm variation in productivity is related to earnings.

We estimate a model allowing for different rent-sharing between immigrants and natives. Motivated by the finding that returns differ for immigrants from different countries, we let the association between earnings and firm value added vary between three groups: Rest of the World, Western, and native (reference). The analysis allows us to gauge how changes to value added at the firm over time translate into changes in earnings, and whether this differs for immigrants and natives. This keeps the sorting of workers to firms constant and controls for time-invariant worker and firm heterogeneity.

[23] Results from estimating equation (2.4) using the employee-weighted ranking are included in Appendix Figure B.3.

The identifying variation comes from workers who do not switch firms (i.e. within an employment spell):

$$\begin{aligned}\ln(e_{ift}) = c + \delta_1 \ln(VA/N)_{ft} + \delta_2 \ln(VA/N)_{ft} \times RoW_i \\ + \delta_3 \ln(VA/N)_{ft} \times Western_i + \lambda_{if} + \lambda_t + \varepsilon_{ift}\end{aligned} \tag{2.5}$$

where $\ln(e_{ift})$ are log earnings for worker i in year t at main firm f, $\ln(VA/N)_{ft}$ is a time-varying measure of log value added per employee at the firm-level and λ_{if} are match fixed effects.

Table 2.3 reports $\widehat{\delta}_1$, $\widehat{\delta}_2$ and $\widehat{\delta}_3$. The column (1) specification does not include any additional controls, while column (2) includes individual time-varying controls (age squared, age cubed, and tenure)[24] and in column (3) we additionally add time-varying firm controls (firm size and share of immigrants at the firm). As before, individual and firm controls are also interacted with the immigrant group to allow the coefficients to vary for natives and immigrants.

The results indicate that firms indeed share profits with their employees: a 1% increase in value added per worker is associated with approximately a 0.03% increase in earnings for natives. In line with the results above, the estimation indicates that there are positive differential returns for immigrants: immigrants from non-Western countries earn an additional 0.01% for a 1% increase in value added, while Western immigrants earn approximately half the additional return. The standard deviation in firm log value added per worker is about 1 and the within-firm standard deviation is 0.5.[25] In other words, if a firm moves from low to high productivity (i.e. increases its log value added by two within-firm standard deviations), earnings are expected to increase by over 4 percent among non-Western migrants. If one is willing to extrapolate to across-firm variations, the earnings increase would be twice as large.

We also estimate equation (2.5) for firms in different productivity deciles of the persistent productivity ranking outlined in Section 2.4. In Table 2.4, column (1) includes workers at firms in the bottom half of the deciles while column (2) includes the top half. Column (3) only includes the bottom decile and column (4) only includes the top decile. The results reveal an interesting pattern. First, the differential rent-sharing for immigrants compared to natives is present in the bottom half of the firm decile distribution, but not in the top. This is consistent with Figure 2.8, which suggests that the difference in gains mainly materializes in the bottom half of the distribution, while the slopes are relatively similar in the upper half. Second, the results suggest that low-productivity firms do not share rents, consistent with the fact that low value-added firms have limited rents to share.

[24]To compute tenure we use data back to 1985. Because we have observed workers in 1998 for fewer years than workers in 2015, the tenure variable is left-truncated. We include tenure in six bands: 1 year (omitted category), 2-3 years, 4-6 years, 7-9 years, 10-13 years and 14+ years.

[25]The overall and between standard deviations in the firm-year sample is 0.94. The within-firm standard deviation is 0.51.

Table 2.3. *Rent-sharing among immigrants and natives*

	(1)	(2)	(3)
Log VA per worker	0.032***	0.026***	0.036***
	(0.003)	(0.003)	(0.002)
Rest of World × Log VA per worker	0.012***	0.008*	0.010***
	(0.004)	(0.004)	(0.003)
Western × Log VA per worker	0.006***	0.004**	0.004**
	(0.002)	(0.002)	(0.002)
R^2	0.765	0.776	0.776
N	37,198,227	37,198,227	37,198,227
Year FE	Yes	Yes	Yes
Spell FE	Yes	Yes	Yes
Individual controls		Yes	Yes
Firm controls			Yes

Note: This table provides the results of estimating equation (2.5). Individual controls are age squared, age cubed, and tenure. Firm controls are firm size band (micro, small, medium, large, very large) and share of immigrants at the firm. Controls are also interacted with the immigrant group. Standard errors are clustered by firm and reported in parentheses.

Table 2.4. *Rent-sharing among immigrants and natives in different productivity deciles*

	(1)	(2)	(3)	(4)
Firm productivity decile	*1-5*	*6-10*	*1*	*10*
Log VA per worker	0.025***	0.055***	-0.000	0.042***
	(0.003)	(0.001)	(0.005)	(0.002)
Rest of World × Log VA per worker	0.018***	0.001	0.005	-0.000
	(0.004)	(0.002)	(0.006)	(0.005)
Western × Log VA per worker	0.006***	0.003	0.004	-0.001
	(0.002)	(0.002)	(0.003)	(0.004)
R^2	0.781	0.763	0.774	0.791
N	24,542,199	12,656,028	14,453,566	1,965,021
Year FE	Yes	Yes	Yes	Yes
Spell FE	Yes	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes	Yes
Firm controls	Yes	Yes	Yes	Yes

Note: This presents the results of estimating equation (2.5), separately for firms in different firm productivity deciles. Individual controls are age squared, age cubed, and tenure. Firm controls are firm size band (micro, small, medium, large, very large) and share of immigrants at the firm. Controls are also interacted with the immigrant group. Standard errors are clustered by firm and reported in parentheses.

2.5.3 Upward mobility in the productivity distribution

The returns to working in more productive firms are higher for immigrants than natives, but immigrants are less likely to work in more productive firms. These two findings raise the question as to which immigrants actually climb up the productivity ladder.

Figure 2.9 plots the share of immigrants in various groups and natives who move up the productivity ranking, conditional on where they start when they enter our sample.[26] We define upward mobility as working in a higher productivity decile five years later compared to when the individual is first observed.[27] We see that, for all groups, the likelihood of moving up decreases with initial productivity decile. However, immigrants are less likely than natives to move at all, and this holds across the whole productivity distribution and for all groups of immigrants.[28]

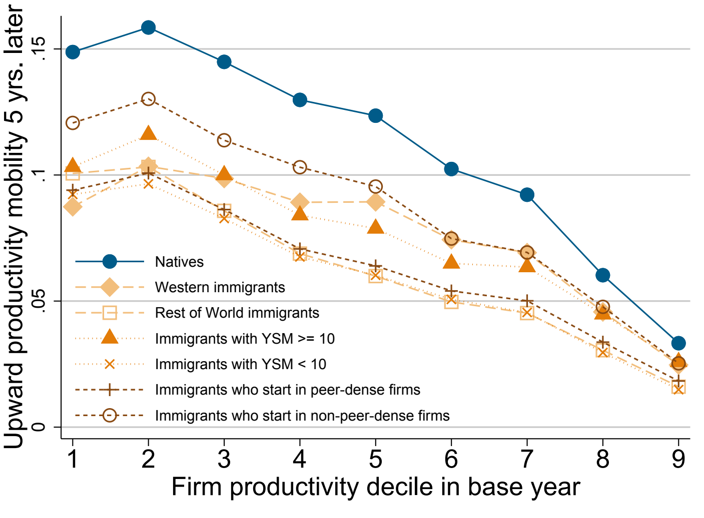


Figure 2.9. Upward mobility, natives vs. various groups of immigrants

[26]We look at the following groups: Western immigrants, Rest of World immigrants, recently-arrived immigrants (YSM less than 10 years), immigrants with YSM more than 10 years, immigrants who start in peer-dense firms, defined as firms with above-median share of immigrants and immigrants who start in non-peer dense firms, defined as firms with below-median share of immigrants. Note that the immigrant groups are not mutually exclusive. For example, there is likely overlap between the group of Rest of World immigrants and the group of recent arrivals, or between the group of recent arrivals and those in peer-dense firms.

[27]The outcome variable takes the value 1 if the productivity decile five years later is *strictly* higher than in the initial year, and 0 otherwise. Since by construction the outcome does not vary for those that start off in the highest decile, we disregard these individuals.

[28]Figure A.3 in the appendix shows these patterns for natives and the pooled sample of immigrants.

The analysis shows that upward mobility is more common among those that start in non-peer dense firms, whereas migrants starting their career in immigrant and peer dense firms largely remain in low-productive workplaces. This is consistent with the influence of ethnic segregation and segmentation. Mobility into high-productivity firms is also strongly correlated with region of birth. It is higher for Western than Rest of World immigrants, at least when transitioning between higher productivity deciles. It is also more common for immigrants with 10 or more years in the host country than among those who arrived more recently.

Could it be that mobility differences are related to differential selection of natives and immigrants? In order to test this hypothesis, we assess the native-immigrant difference in average person fixed effects across the productivity distribution.[29] Figure 2.10 plots this difference for four different groups, by productivity decile: i) all workers; ii) never-movers, defined as workers that never move across productivity distribution deciles throughout their histories; iii) movers, defined as those that move at least once (either up or down) during their history; and iv) upward movers, defined as above as those that move up with respect to the baseline year five years later.

Among the upward movers, the native-immigrant difference in average person fixed effects is the smallest and close to zero from the seventh decile onward. For all the other groups, natives have higher average person fixed effects across the productivity distribution, although generally diminishing as we move up the distribution. The difference is particularly striking for the never-movers. The results suggest that, in terms of their individual-specific earnings capacity, immigrant movers are similar to native movers, while immigrant stayers differ from native stayers. Somewhat speculative, the large gap among never-movers could be seen as an indication that natives stay because they are in a good position, whereas migrants have fewer options and therefore have persistently lower earnings.

[29]Gerard et al. (2018) call this difference the “average skill gap". The person fixed effects are taken from estimating equation (2.4).

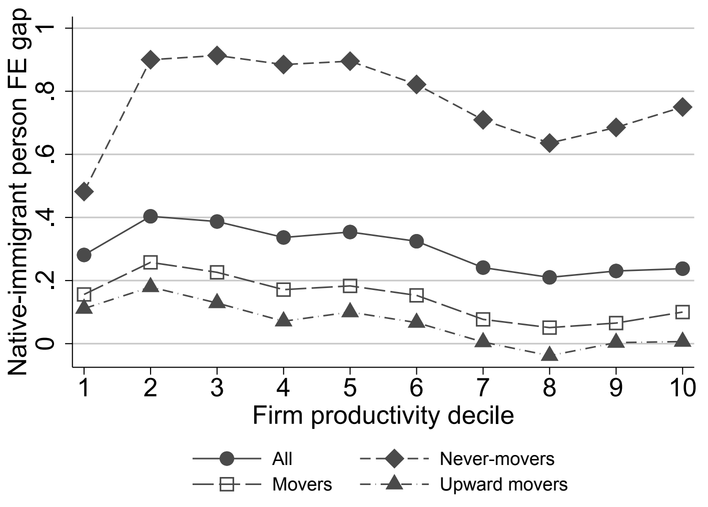


Figure 2.10. Native-immigrant average difference in person fixed effects

2.5.4 Decomposition of the immigrant-native earnings gap

Finally, we tie together the analysis by decomposing the earnings premium associated with working in more productive firms into a sorting and a pay-setting component.[30] Building on our framework from section 2.5.2, assume that the earnings of worker i in group g in time t are given by:

$$\ln e_{git} = \alpha_{gi} + \theta_d^g + X_{git}\beta^g + \varepsilon_{git} \tag{2.6}$$

where α_{gi} is a person effect, θ_d^g is a group-specific earnings premium in productivity decile d, X_{git} is a vector of time-varying controls (age squared, age cubed, firm size and year effects) and β_g a vector of coefficients. ε_{git} captures all remaining determinants of earnings. Let D_{git} indicate whether an individual i in group g is employed in time t. Let $\bar{X}_{It}$ and $\bar{X}_{Nt}$ constitute the means of the observed covariates for employed immigrants (I) and natives (N) in year t, and let π_{It} and π_{Nt} denote the fractions of the two groups employed in decile d in year t. Assuming $E[\varepsilon_{git}|D_{git} = 1] = 0$, we can express mean immigrant and native earnings in the following way:

$$E[\ln e_{Iit}|D_{Iit} = 1] = E[\alpha_{Ii}|D_{Iit} = 1] + E[X_{It}|D_{Iit} = 1]\beta_I + E[\theta_d^I|D_{Iit} = 1]$$
$$E[\ln e_{Nit}|D_{Nit} = 1] = E[\alpha_{Ni}|D_{Nit} = 1] + E[X_{Nt}|D_{Nit} = 1]\beta_N + E[\theta_d^N|D_{Nit} = 1]$$

[30] The decomposition closely follows Dostie et al. (2020), who instead decompose a firm-specific earnings premium that differs for immigrants and natives.

and the mean immigrant-native gap in year t is then:

$$\begin{aligned} E[\ln e_{Nit}|D_{Nit}=1]-E[\ln e_{Iit}|D_{Iit}=1] &= E[\alpha_{Ni}|D_{Nit}=1]-E[\alpha_{Ii}|D_{Iit}=1] \\ &+E[X_{Nt}|D_{Nit}=1]\beta_N - E[X_{It}|D_{Iit}=1]\beta_I \\ &+E[\theta_d^N|D_{Nit}=1]-E[\theta_d^I|D_{Iit}=1] \end{aligned}$$

Since we are interested in the part of the earnings gap explained by the productivity decile premiums, we focus on the third term. A simple decomposition (Oaxaca 1973; Blinder 1973) of the third term gives:

$$\begin{aligned} E[\theta_d^N|D_{Nit}=1]-E[\theta_d^I|D_{Iit}=1] &= E[\theta_d^N|D_{Nit}=1]-E[\theta_d^N|D_{Iit}=1] \\ &+E[\theta_d^N-\theta_d^I|D_{Iit}=1] \\ &= E[\theta_d^I|D_{Nit}=1]-E[\theta_d^I|D_{Iit}=1] \\ &+E[\theta_d^N-\theta_d^I|D_{Nit}=1] \end{aligned}$$

The sample counterpart of the expression above is given by:

$$\begin{aligned} \sum_d \widehat{\theta_d^N}\pi_{Ndt} - \sum_d \widehat{\theta_d^I}\pi_{Idt} &= \underbrace{\sum_d \widehat{\theta_d^N}(\pi_{Ndt}-\pi_{Idt})}_{sorting} + \underbrace{\sum_d (\widehat{\theta_d^N}-\widehat{\theta_d^I})\pi_{Idt}}_{pay-setting} \\ &= \underbrace{\sum_d \widehat{\theta_d^I}(\pi_{Ndt}-\pi_{Idt})}_{sorting} + \underbrace{\sum_d (\widehat{\theta_d^N}-\widehat{\theta_d^I})\pi_{Ndt}}_{pay-setting} \end{aligned} \tag{2.7}$$

where the $\widehat{\theta_d^g}$ terms, $g \in \{N,I\}$ are estimated from equation (2.6).

We find the first expression to be the most intuitive. The contribution of the productivity decile premiums to the immigrant-native earnings gap is given by a weighted average of the differences in employment shares of immigrants and natives (weighted by the earnings premium of natives per decile) and a weighted average of the differences in decile earnings premiums (weighted by the share of immigrants per decile). The first component in the expression in equation (2.7) (sorting) shows the effect of differences in sorting across the productivity distribution, assuming immigrants were paid the same premiums as natives. It will be positive if natives are more likely to work in more productive firms which pay higher premiums. The second component (pay-setting) shows the differential pay-setting across the productivity distribution, given the distribution of immigrants across productivity deciles. Our results from the previous section consistently show higher earnings premiums for immigrants across the decile distribution, hence we expect this term to be negative.

In the estimations we omit the first decile such that decile premiums are estimated relative to this decile. A sufficient condition to perform the decomposition is that firms in the first decile pay zero earnings premiums to both immigrants and natives. Column (3) of Table 2.4 suggests that firms in the bottom decile of the productivity distribution do not share rents with any of their workers, which strengthens the validity of this assumption. The pay-setting component is sensitive to this assumption, while the sorting component is invariant to it. If it does not hold – and in particular if natives earn a higher premium relative to immigrants in the first decile – then the estimated negative pay-setting effect will move toward zero or even become positive.

Table 2.5 shows the decomposition results for the overall group of natives and immigrants and for sub-samples categorized by gender, age, region of origin and education level. Column (1) shows the mean log earnings gap between immigrants and natives in different groups. Columns (2) and (3) show the mean decile premium received by natives and immigrants, respectively. Column (4) gives the difference between column (2) and column (3), while columns (5) and (6) show the sorting and within-decile pay-setting effects, respectively.[31] Starting with the first row, we see that, on average, the decile premium immigrants get is slightly higher than the decile premium natives get (6.5 vs. 5.5 percent), which is in line with our results from Figure 2.8. The difference of 1 percentage point reduces the overall gap by 8%. As expected, the sorting effect in column (5) is positive and accounts for around 10% of the earnings gap. The pay-setting term, instead, reduces the gap by around 18%.[32]

In the next rows, we show how the decomposition results vary for different subgroups. Similar patterns emerge: the pay-setting effect is always negative and larger than the sorting effect. We highlight a few noteworthy patterns. The earnings gap is much larger for men than for women, with sorting explaining a higher proportion of the overall gap for women than for men (20% vs. 9%). In terms of age groups, the biggest earnings gap is observed for the group aged between 31 and 50, and it is slightly lower for the group aged 50 and above, in line with earnings assimilation with time spent in the country. Western immigrants in our sample have an earnings advantage over natives, primarily explained by the pay-setting effect. The mean decile premium for Rest of World immigrants is lower than for Western immigrants due to their concentration in the bottom deciles of the firm productivity distribution. Finally, we note that since we do not fit separate models for these different subgroups, these results do not account for the fact that firms may set different earnings premiums for the different subgroups. As we saw above, premiums may differ for immigrants depending on their country of birth. This aspect is also particularly important in the case of men and women, as previous research finds that firm pay differentials explains an important part of the gender gap (Card et al. 2016; Bruns 2019).

[31] Table A.3 repeats the decomposition exercise when doing the employee-weighted ranking of firms. The results are consistent with the fact that when using the employee-weighted ranking the bottom half of the productivity distribution is now characterized by low decile premiums.

[32] The signs on these effects are in line with those in Dostie et al. (2020), who decompose *firm-specific* as opposed to decile-specific premiums using a similar method; the magnitudes are not directly comparable.

Table 2.5. *Decomposition of immigrant-native earnings gap*

	Earnings gap	Mean decile premium natives	Mean decile premium immigrants	Premium gap	Sorting	Pay-setting
	(1)	(2)	(3)	(4)	(5)	(6)
All	0.124	0.055	0.065	-0.010	0.013	-0.023
By gender						
Male	0.158	0.061	0.071	-0.010	0.014	-0.025
Female	0.044	0.045	0.055	-0.010	0.009	-0.020
By age group						
Up to age 30	0.041	0.051	0.061	-0.010	0.012	-0.022
Between 31 and 50	0.198	0.058	0.067	-0.009	0.014	-0.023
50 and above	0.134	0.056	0.064	-0.009	0.013	-0.022
By region of origin						
West	-0.040	0.055	0.075	-0.020	0.005	-0.025
Rest of World	0.192	0.055	0.061	-0.006	0.016	-0.022
By education						
Compulsory	0.132	0.058	0.060	-0.002	0.020	-0.022
Upper secondary	0.177	0.052	0.065	-0.013	0.009	-0.022
Post Secondary	0.099	0.056	0.066	-0.009	0.014	-0.023

Notes: Column (1) shows the mean log earnings gap between immigrants and natives in different groups. Columns (2) and (3) show the mean decile premium received by natives and immigrants, respectively. Column (4) gives the difference between column (2) and column (3). We decompose the gap in column (4) into a between-decile sorting effect (column (5)) and a differential within-decile pay-setting effect (column (6)). The sorting effect is weighted by the native premium, and the pay-setting effect by the immigrant shares.

2.6 Conclusion

The role of firms in determining immigrant-native earnings differentials is potentially important but under-explored. When firms differ in their wage-setting practices, the average earnings of immigrants relative to that of natives may depend both on the firms in which immigrants and natives work, and on how immigrants fare in a given firm.

This paper focuses on firm productivity as a potential key to understanding labor market differences across groups of workers. We use population-wide linked employer-employee data for Sweden between 1998 and 2017 to study the sorting and earnings of immigrants and natives across the firm productivity distribution. Firm productivity is measured using persistent value added per worker, which we rank into bins. We find no indication that this measure is influenced by the composition of workers in the firm, including the share of immigrants that the firm employs.

We reach four main findings. First, immigrants are more concentrated in low-productivity firms relative to natives and have lower rates of upward mobility in the firm productivity distribution. This is in line with previous research that finds that immigrants do not have access to the same workplaces as natives. Second, more productive firms pay higher wages to both immigrants and natives. Third, the earnings gains from working in more productive firms are greater for immigrants than natives. That is, the within-individual increase in earnings for a worker who climbs the productivity ladder is steeper for immigrants than for natives, especially in the lower half of the productivity distribution. This result is particularly striking for immigrants with poor average labor market outcomes: immigrants who are born in non-Western countries and also immigrants who arrived in Sweden more recently. Fourth, decomposing the contribution of firm productivity pay premiums on the immigrant-native earnings gap, we find that the premiums reduce the earnings gap by 8%. Sorting and pay-setting work in opposite directions: the over-representation of immigrants in less productive firms widens the gap by 10%, while the relatively higher premiums that immigrants earn reduces the gap by 18%.

Immigrant labor market assimilation is a major policy concern in many countries, and the academic literature is voluminous. However, so far, the number of studies analyzing the contribution of firms' hiring patterns and pay-setting policies to immigrant-native differentials is small. Our results clearly suggest that a better understanding of firm-level factors is needed. Productivity, technology, and competition, as well as their interactions with structural change and institutions, appear to be relevant areas for further research.

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Appendix A: Additional description and results

We estimate AKM models (Abowd et al. 1999) of the following form, separately for two groups g: immigrants and natives (see e.g. Dostie et al. 2020).

$$\ln(e_{git}) = \alpha_{gi} + \psi^{g}_{f(g,i,t)} + X_{git}\beta^{g} + \varepsilon_{git} \tag{2.8}$$

α_{gi} captures individual time-invariant skills and other factors that are rewarded equally across all firms; $\psi^{g}_{f(g,i,t)}$ captures a group-specific firm pay premium that is rewarded equally across individuals in a group within the same firm; X_{git} are time-varying individual controls; and the error term ε_{git} captures random match effects, human capital shocks, and other unobservables.[33] A summary of the estimated parameters and model fit are included in Table A.1 below.

[33]The firm- and worker-specific fixed effects are separately identified by job-to-job transitions of workers across firms. Cross-firm mobility is therefore crucial for identification (see e.g. Card et al. (2013)). Under exogenous mobility, both job-to-job transitions and the job assignment process depend solely on time-invariant unobservable characteristics of workers and firms, along with time-varying observables in X_{git}. Provided that the exogenous mobility assumption holds, the model requires a sufficiently high number of transitions in order to consistently estimate its variance components. Failing to have information on enough worker transitions can lead to substantial bias (see e.g. Andrews et al. (2008)), which in empirical applications motivates the pooling of multiple years of data for the model estimation under the assumption that firm-fixed effects are time-constant.

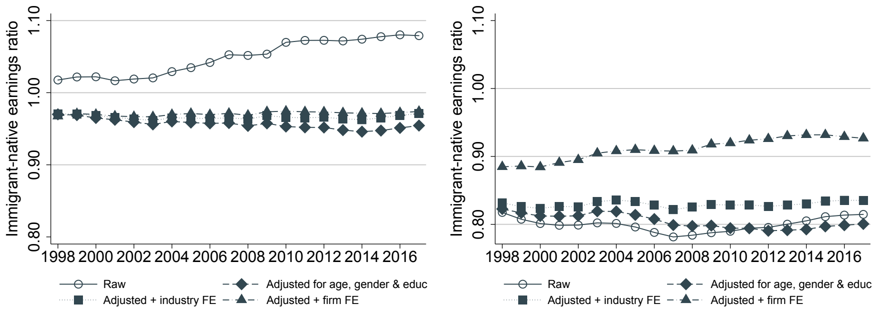
Table A.1. *Summary of estimated AKM models for natives and immigrants*

	Natives (1)		Immigrants (2)
Standard deviation of log earnings	0.593		0.592
Number of person-year observations	45,873,468		6,784,236
Summary of parameter estimates			
Number of person effects	4,564,683		991,165
Number of firm effects	431,937		206,291
Std. dev. person effects (across pers.-yr. obs.)	0.390		0.404
Std. dev. firm effects (across pers.-yr. obs.)	0.200		0.277
Std. dev. Xb (across pers.-yr. obs.)	0.211		0.201
Correlation of person/firm effects	0.130		0.020
RMSE of model	0.325		0.321
Adjusted R-squared of model	0.663		0.643
Correlation native/immigrant firm effects		0.626	
Share of variance of log earnings due to:			
Person effects	0.433		0.466
Firm effects	0.113		0.219
Covariance of person and firm effects	0.058		0.013
Xb and associated covariances	0.096		0.009
Residual	0.300		0.294

Notes: Results from two-way fixed effects models estimated separately for natives (column 1) and immigrants (column 2). Models include year dummies interacted with education dummies, and quadratic and cubic terms in age interacted with education dummies. The correlations of native and immigrant firm effects are calculated for the subset of dual-connected firms.

Table A.2. *Summary statistics, full sample compared to analysis sample*

	Full sample			Analysis sample		
	1998–2017	1998	2017	1998–2017	1998	2017
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Employees						
Immigrant	0.13	0.09	0.19	0.13	0.09	0.18
Immigrant from West	0.04	0.05	0.03	0.04	0.05	0.03
Immigrant from Rest of World	0.09	0.04	0.15	0.09	0.05	0.15
Native-segregated firms	0.17	0.21	0.12	0.17	0.22	0.11
Immigrant-segregated firms	0.01	0.00	0.02	0.01	0.00	0.01
Male	0.63	0.65	0.61	0.64	0.66	0.62
Age	40.53	40.24	40.26	40.29	39.93	40.22
Age $\leq$ 30	0.26	0.26	0.29	0.27	0.27	0.29
Age $\geq$ 50	0.28	0.27	0.28	0.27	0.26	0.28
Education, compulsory	0.15	0.23	0.11	0.16	0.25	0.11
Education, secondary	0.54	0.54	0.52	0.55	0.54	0.53
Education, upper secondary	0.30	0.22	0.36	0.29	0.21	0.35
Education, missing	0.01	0.01	0.01	0.01	0.01	0.01
Monthly earnings (2010 SEK)	24,706	20,828	27,966	24,624	20,838	28,004
No. of employees $\times$ year	52,984,851	2,279,920	2,948,946	46,870,657	1,883,222	2,504,695
Panel B: Firms						
Fraction immigrants at employer	0.14	0.09	0.22	0.13	0.09	0.19
Yearly employer size	23.32	22.30	25.46	22.38	20.55	29.00
Share native-segregated firms	0.62	0.71	0.50	0.62	0.71	0.49
Share immigrant-segregated firms	0.06	0.03	0.10	0.05	0.03	0.07
No. of firms $\times$ year	3,319,820	156,840	163,138	2,883,741	129,322	115,210



(a) West *(b)* Rest of World

Figure A.1. Immigrant–native earnings ratio – by region of birth

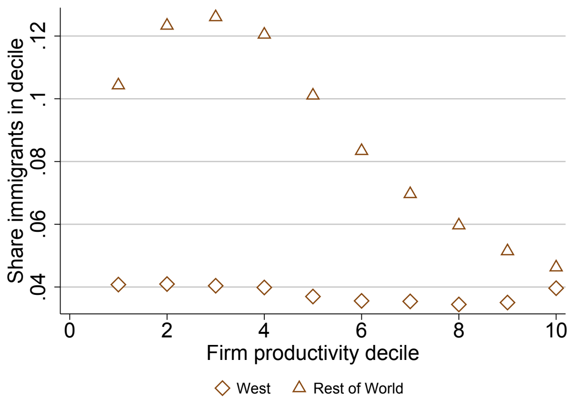


Figure A.2. Share immigrants in productivity decile – by country of birth

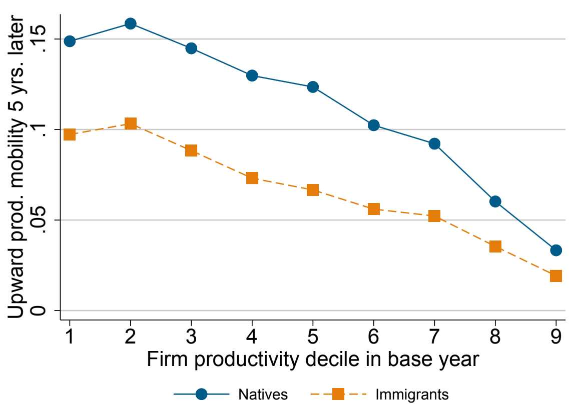


Figure A.3. Upward mobility, natives vs. immigrants

Table A.3. *Decomposition of immigrant-native earnings gap – Employee-weighted ranking*

	Earnings gap	Mean decile premium natives	Mean decile premium immigrants	Premium gap	Sorting	Pay-setting
	(1)	(2)	(3)	(4)	(5)	(6)
All	0.124	0.076	0.078	-0.002	0.012	-0.014
By gender						
Male	0.158	0.082	0.084	-0.003	0.014	-0.016
Female	0.044	0.065	0.067	-0.002	0.009	-0.011
By age group						
Up to age 30	0.041	0.072	0.075	-0.002	0.011	-0.013
Between 31 and 50	0.198	0.078	0.080	-0.002	0.013	-0.015
50 and above	0.134	0.075	0.076	-0.001	0.013	-0.014
By region of origin						
West	-0.040	0.076	0.086	-0.011	0.006	-0.016
Rest of World	0.192	0.076	0.074	0.002	0.015	-0.013
By education						
Compulsory	0.132	0.079	0.073	0.006	0.020	-0.013
Upper secondary	0.177	0.071	0.077	-0.007	0.007	-0.014
Post secondary	0.099	0.077	0.078	-0.001	0.014	-0.015

Notes: Column (1) shows the mean log earnings gap between immigrants and natives in different groups. Columns (2) and (3) show the mean decile premium received by natives and immigrants, respectively. Column (4) gives the difference between column (2) and column (3). We decompose the gap in column (4) into a between-decile sorting effect (column (5)) and a differential within-decile pay-setting effect (column (6)). The sorting effect is weighted by the native premium, and the pay-setting effect by the immigrant shares.

Appendix B: Additional robustness checks

Relationship between AKM firm FE and firm productivity

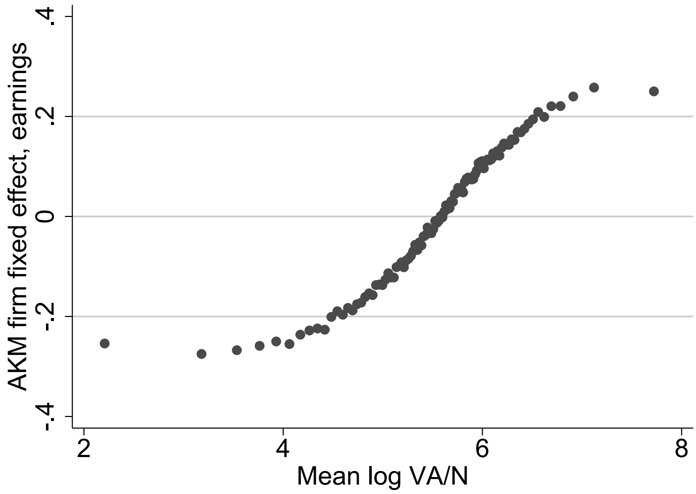
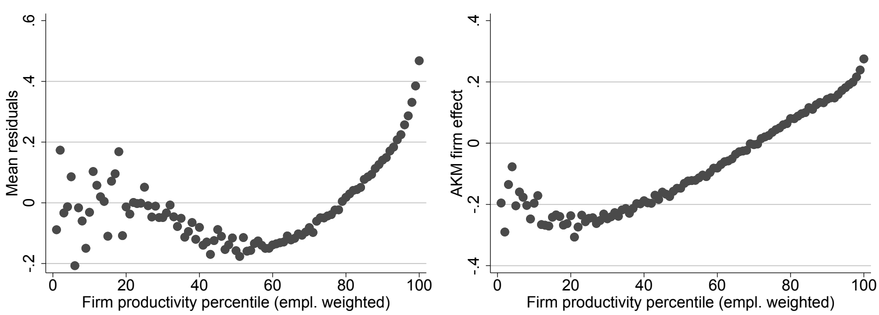


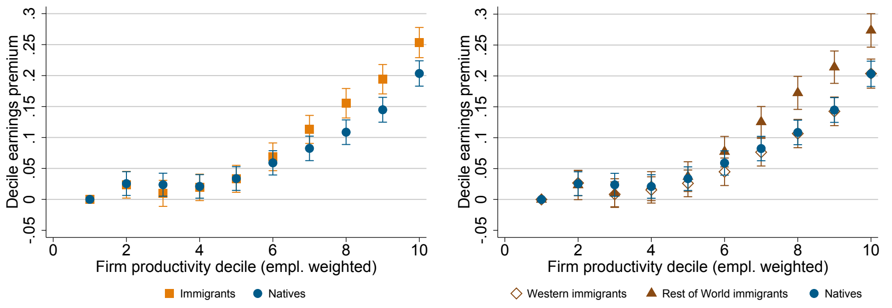
Figure B.1. AKM firm FE by log value added per worker (100 bins)



(a) Log earnings against productivity ranking *(b)* AKM firm FE against productivity ranking

Figure B.2. More productive firms pay higher earnings (employee-weighted)

Earnings returns by productivity decile – weighted ranking



(a) All immigrants *(b)* By immigrant group

Figure B.3. Earnings returns to working in more productive firms (weighted ranking)

Note: The figure plots $\hat{\theta}_d$ and $\hat{\theta}_d + \hat{\gamma}_d$ from equation (2.4) using the employee-weighted ranking of firms.

Earnings returns by productivity decile – by years since migration

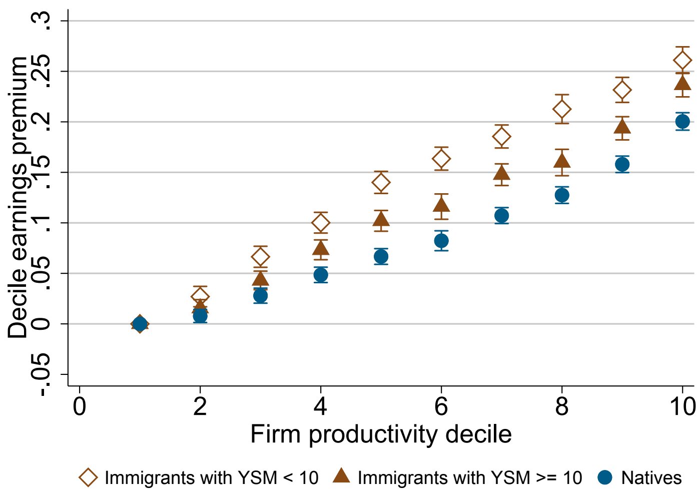


Figure B.4. Earnings returns to working in more productive firms – YSM

Note: The figure plots $\hat{\theta}_d$ and $\hat{\theta}_d + \hat{\gamma}_d$ from equation (2.4), where the immigrant group is split by their years since migration (YSM).

3. Teacher Career Opportunities and School Quality

Co-authored with Erik Grönqvist and Lena Hensvik

Acknowledgments: We are grateful for comments from Peter Fredriksson, David Figlio, Claudia Goldin, Nils Gottfries, Lisa Höckel, Jesse Rothstein, Rune Vejlin and seminar participants at IFAU, UCLS, Uppsala University, Harvard University, IZA Summer School in Labor Economics as well as for helpful research assistance by Kristina Sibbmark.
This chapter is an updated version of IFAU working paper Grönqvist et al. (2020).

3.1 Introduction

Good teachers are an important input to schooling.[1] Still, most teacher labor markets are characterized by problems to recruit and retain talented teachers (Corcoran et al. 2004; Bacolod 2007; Fredriksson and Öckert 2007; Grönqvist and Vlachos 2016; Leigh and Ryan 2008). The compressed wage distribution – relative to other occupations – is often put forth as a primary driving force behind these problems, pointing to a greater need for policy-makers to find ways to tie the pay structure more closely to teacher quality.

There are several ways to make teacher compensation more responsive to teacher skills (see Jackson 2012 for a discussion). In this paper we study how the teacher labor market is affected by improved career opportunities, taking advantage of a unique promotion program for Swedish teachers. In response to deteriorating results in international comparisons like PISA and TIMMS, the Swedish government introduced a career step for experienced and skilled teachers by providing separate funding for a new position called 'career teachers' (Regeringen 2013b). Career teachers receive a substantial monthly wage increase (5,000 SEK (520 USD), which corresponds to nearly 20 percent of mean pre-reform wages) and continue to teach, but are also tasked to work with the school's pedagogical development, like being a mentor or initiating and leading development projects (Statskontoret 2015), to spread their competences to teacher peers.

The intention of the program was to make the teaching profession more attractive by rewarding skilled teachers, thereby increasing the wage dispersion, and to improve student outcomes by motivating, retaining and attracting high quality teachers (Regeringen 2013b). While similar types of career steps also exist in England, New Zealand, Australia, Scotland and Poland (Regeringen 2013c), we know little about their impact on teachers and students.

We address five central questions. First, who is promoted to become a career teacher? Second, what is the pass-through of the stipulated wage increase relative to non-promoted teachers' wages? Third, is there an effect of the career step on teachers' separations from their school, and from the profession overall? Fourth, is there an effect on the composition of teachers? Last, is there an impact on student performance?

The career teacher reform was rolled-out over four years, and the number of career teacher positions increased gradually. The allocation of positions to school districts was in proportion to their student population. School districts had discretion to assign career teacher positions to individual schools within

[1] There is a large and growing literature documenting that teachers matter for both short term student outcomes, like test scores (Rockoff 2004; Rivkin et al. 2005; Leigh 2010; Chetty et al. 2014), and for longer term outcomes like college attendance and earnings (ibid.). It has, however, been difficult to find observable characteristics that are important for student outcomes (Jackson et al. 2014). Factors like education, cognitive ability, and personality, which are found to be important in other parts of the labor market, are only of marginal importance (Hanushek and Rivkin 2006; Rockoff et al. 2011; Grönqvist and Vlachos 2016).

the district, and at the schools it was delegated to the local principals to identify and recruit skilled teachers to the new position.[2]

We start by providing documentation of how the reform was implemented. This analysis suggests that school districts allocated the number of teacher promotions across schools in relation to school size. No other pre-determined observable school characteristics predict this allocation. Thus, even if school districts were free to allocate the promotions across schools, they did not target the promotions to schools with, for example, high teacher turnover or low student performance.

Considering who was promoted, the most salient pattern is that promotions within schools were given to teachers from the higher wage deciles of a compressed wage distribution, conditional on observable teacher characteristics. We find full pass-through of the state-funded stipulated wage increase onto wages. Thus, the reform increased wage dispersion both across and within schools, and there is no indication of compensatory behavior towards non-promoted teachers in regular wage negotiations.[3]

In terms of teacher mobility, promotions can both attract and retain individuals with a higher innate ability and induce individuals to exert more effort (see Lazear and Shaw (2007) and Oyer and Schaefer (2011) for summaries of the personnel economics literature). We expect the career step to make the current job more attractive for promoted teachers, but as promotion signals quality these teachers also become more attractive to other schools. For non-promoted teachers, on the other hand, the likelihood of quitting may go up if individuals care about their relative position (see e.g. Card et al. 2012; Dube et al. 2019). To quit may also be a rational response to a signal that you will not be promoted, but this signal is also observed by other schools. Quit rates may go down for non-promoted teachers if the career step improves school quality and the professional work environment. Based on tournament theory we would also expect the chance of becoming a career teacher to motivate marginal – not yet promoted – teachers to exert more effort (Lazear and Rosen 1981).[4] It is less clear how motivation is affected by being promoted.

[2]The career step reform can be thought of as a performance-based promotion program where talented teachers are awarded a pay rise (Jackson 2012). Given the difficulties to identify good teachers based on observable characteristics, this leaves the principal with a substantial amount of discretion. Still, there is evidence suggesting that principals can identify teacher skills and that teacher assessments can predict high quality teachers (Rockoff and Speroni 2011; Cantrell et al. 2008), and that detailed screening measures used in recruitment (e.g. written assessments, interviews, and sample lessons) strongly predict teacher job performance (Jacob et al. 2018).

[3]In Sweden, teacher wages are set individually by the local principal. The idea is that competence, responsibilities and performance should determine the wage. Still, wages are very compressed and there is a strong equity norm among teachers. There can thus be pressure on principals to compensate non-promoted teachers in the regular wage revision.

[4]However, for promotions perceived as unfair in Chinese schools, Li (2019) finds an erosion of work moral and higher quitting probability among non-promoted teachers.

We study separations, teacher composition and student performance at the school level by exploiting the *timing* of the introduction of promotions in a difference-in-difference design. Our main identifying assumption is that schools' adoption of career teachers is unrelated to trends or school-level shocks that affect the outcomes. In line with this assumption, we find that no factors besides the number of students systematically influences the timing of participation. Our results suggest that schools with career teacher promotions have lower teacher turnover, both in general and in terms of teachers leaving the profession. This result is driven mainly by more senior teachers and applies both to teachers who were promoted and to those who were not promoted. In addition, the teaching pool improves in schools that participate in the reform as they are able to retain a higher share of certified and experienced teachers and teachers who themselves have higher compulsory school grades. Finally, we find non-negligible positive effects on student test scores in Math, English and Swedish in grades 3 and 6.

Our findings contribute to the policy-debate about how to improve school quality by tightening the link between teacher pay and performance. To the best of our knowledge, it is the first assessment of the effectiveness of performance based promotions. Most of the previous literature has focused either on smaller bonus programs (e.g. Clotfelter et al. 2008) or general salary increases (e.g. Figlio 1997; Gilpin 2012; Leigh 2012; Falch 2011; Hendricks 2014). Two recent studies consider the impact of changes from fixed to more flexible pay schemes. Biasi (2018) finds higher effort and teacher quality in school districts in Wisconsin that start to pay high-quality teachers more, compared to districts retaining more rigid pay schemes. In contrast, Willén (2019) finds no support for changes in teacher composition or student outcomes when individualized wage-setting was introduced to teachers in Sweden in the 1990s.

Besides providing mixed results, these studies highlight difficulties for policymakers to achieve a widening of the wage distribution through more local autonomy over teacher pay. In Wisconsin, around half of the school districts chose to maintain the fixed salary-scheme despite their new autonomy (Biasi 2018). Similarly, the wage decentralization reform in Sweden led to a relatively modest increase in wage dispersion among teachers (Willén 2019).[5] The promotion program we study is different from these flexible pay reforms in several aspects: First, the stipulated wage increase is financed by earmarked funding from the central government, putting no strain on the local budget. Second, it restricts the number of teachers that can be promoted, preventing principals to raise wages for everyone. Third, it requires new responsibilities from the promoted teachers, which could make differential pay seem more acceptable. Consistent with this notion, we find that nearly all school districts

[5]Consistent with this, Söderström (2010) finds that the switch from centralized to individualized wage-setting increased entry wages, and wage dispersion late in the career.

chose to participate in the promotion program and that the reform had the intended first-stage impact on the actual wage distribution.

An important contribution of our study is also that we are able to go beyond the impact on teacher quality and also consider the impact on student performance. The direct evidence on the link between teacher pay and student outcomes is mixed with a focus on general pay raises rather than on increased wage dispersion.[6] Our findings suggest that improved career opportunities for teachers in the form of performance-based promotions could be an important tool for policy-makers who aim to improve educational performance.

Finally, drawing on survey data for around a third of the school districts, we are able to shed some light on how the impact of the promotions vary with the local design of the career teacher positions. The data contain information about nine different career teacher responsibilities, which we use to identify two distinct career teacher types: "Coaching and mentoring CTs" and "Teaching development CTs". Our results highlight that teaching development is more important in reducing teacher turnover, but there is no differential impact on student results. Hence, while it is difficult based on the available data to conclude if there is a "best practice" in career teacher behavior our results indicate that increasing pay alone may not be enough to get the desired impact on teacher selection.

The paper proceeds as follows. It begins by describing the career teacher reform and the data in Section 3.2. In Section 3.3 we show how the career teacher promotions were allocated across schools and teachers, and in Section 3.4 we analyze the pass-through of the stipulated wage increase onto wages. Section 3.5 contains analyses of teacher turnover, teacher composition, and student performance. Finally, Section 3.6 concludes.

[6] Several studies from the last two decades also find a positive relationship between general teacher wages and student outcomes (see for example, Loeb and Page 2000; Dolton and Marcenaro-Gutierrez 2011; Britton and Popper 2016; Alva et al. 2017). On the other hand, Ree et al. (2018), who study an unconditional salary increase in Indonesia, and Cabrera and Webbink (2018), who study the impact of a wage policy program in Uruguay, find only modest to no effects of teacher pay on student outcomes. These studies focus on general pay raises rather than on performance-based promotions or increased wage dispersion. For performance-related pay schemes, surveys by Jackson et al. (2014) and Neal (2011) find the earlier literature to be inconclusive. More recently, Dee and Wyckoff (2015) study a program where teachers are rated on a composite measure of teacher performance (value added in test scores and detailed classroom observations), and find that low performing teachers with the risk of dismissal and high performing teachers with the chance of financial rewards improved their performance. The US program most similar to the Swedish reform we study is probably the Minnesota Q Comp program, which encouraged Minnesota school districts to adopt locally designed pay-for-performance schemes and complementary HRM practices. Sojourner et al. (2014) evaluate the impact of the reform using a difference-in-difference strategy, finding positive effects of the program on student performance. As in our case, local districts received additional funding to participate in the program.

3.2 Institutional setting and data

Teachers are formally hired by the school district ("huvudman") which, in the case of the public sector, is the municipality.[7] In practice, principals at schools often make the hiring decision and set wages. Teachers in Sweden are covered by collective agreements, where teacher wage bargaining is decentralized and individualized, primarily set in negotiations between the teacher and the principal. This decentralized pay setting scheme came into place in 1996, replacing a centralized system where pay was set according to nationally-determined pay scales based on type of teaching and years of experience (see Willén (2019) and Söderström (2010) for details).

3.2.1 The career teacher reform

In July 2013 the Swedish government initiated a policy that introduced a new career step: the career teacher.[8] It is financed by ear-marked government funding. The reform, akin to a performance-based promotion (Jackson 2012), targets talented teachers. Its intention is to improve student performance by improving teachers' career opportunities. Broadly speaking, it aims to make the teaching profession more attractive by raising wages for skilled teachers, thereby increasing wage dispersion in the profession, and by taking better advantage of teachers' competencies (Regeringen 2013b). Conceptually, it may entail both a sorting effect, if teachers that otherwise leave the school stay to a larger extent, and an effort effect. In addition, as outlined in more detail below, the reform may also entail spillovers within schools from promoted teachers onto their teacher peers.

The Swedish government also introduced other policy initiatives at about the same time as the career teacher reform. Two further education programs for teachers, the Boost for Mathematics and the Boost for Reading, were initiated in 2013 and 2015 respectively, and a more general program to increase teacher pay, the Teachers' Salary Boost, was initiated in 2016. The Salary

[7] Schooling is provided both by the public and by the private sector. The main public provider is the municipality. During the 2016/2017 academic year, 85 percent of compulsory school students attended public schools run by one of 290 municipal providers while 15 percent of compulsory school students attended voucher schools run by one of the 729 non-public providers (Skolverket 2018a). Children are free to choose which school to attend and incur no tuition fees regardless of provider. If a school is over-subscribed, proximity is the main guiding principle for allocation of places in public schools. We restrict our attention to public compulsory schools in our main analysis.

[8] The reform is regulated in Regulation 2013:70, see Regeringen (2013a). Formally, it introduces two career steps: career teachers ("förstelärare") and lecturers ("lektor"). Career teachers is a position for highly skilled teaching practitioners whereas lecturers is a position for teachers with an academic degree (licentiate or PhD) with a partly different job description. Since only around 1% of the promoted teachers are lecturers, we exclude teachers that are ever lecturers from our sample.

Boost also aimed at increasing wage dispersion, and implied a smaller unconditional wage increase to about half of the teaching pool financed by earmarked money from the state. In sensitivity analyses (see Panel F of Tables C.1 and C.2) we add controls for the three boost reforms. For these reforms to invalidate our results their implementation needs to interact with the career teacher reform. The conclusions regarding the effect of the career teacher reform do not change when we control for the boost reforms.

Design and roll-out of reform

The number of career teaching positions are allocated annually to school districts based on the national share of students across all educational tiers in the school district.[9] For example, if a school district has 5 percent of students, it is allocated 5 percent of the career teaching positions. School districts in turn decide how to allocate the positions across schools within school districts, and to individual teachers. The total number of available positions each year is decided by the size of the total state grant. In 2013 the earmarked funding could finance around 4,000 positions across all educational tiers and types of school districts.[10] This increased to around 14,000 by 2014 (Skolverket 2014). By 2016, the number of available positions was around 16,000 (Skolverket 2016). However, while the allocation of positions available to school districts is rules-based, the school district need not acquire all the funding reserved to it. In 2013, approximately 75 percent of the funding (for approximately 3,000 positions) was acquired (Skolverket 2014). This increased to around 90 percent by 2016 (Skolverket 2016). For municipal compulsory schools, the group that we study, there are 290 school districts, all of which are *potentially* treated from 2013 onward.[11] Out of the 290 school districts, approximately 70 percent in our sample participate in 2013, where participation is defined by having at least one career teacher. By 2014, 97 percent of the school districts participate and in 2016, all but one school district participates. There is thus limited variation in participation across school districts over time. Figure 3.1 plots the school district's share of career teachers against the school district's share of students. The relationship is approximately linear, at least after 2013, which is in support of promotions being allocated in proportion to school district size. The corresponding figure for the variation across schools (see Figure A.2 in the appendix) shows that there is more variation within school districts.

[9]Formally, state grants are allocated according to this rule. A grant of SEK 85 000 (USD 9 600) is given per full-time career teacher. This also includes funding for employer contributions. School districts with fewer than 75 students apply for the grant from a common pool.

[10]The reform covered pre-school, compulsory school, upper-secondary school, Sami schools, schools for children with special needs and adult education from both public providers and voucher schools. In our main sample, we include municipal compulsory schools only.

[11]We restrict our attention to municipal schools both because take-up of the program among voucher schools is substantially lower (Statskontoret 2015) and because we can only observe wages for a sub-sample of the voucher schools.

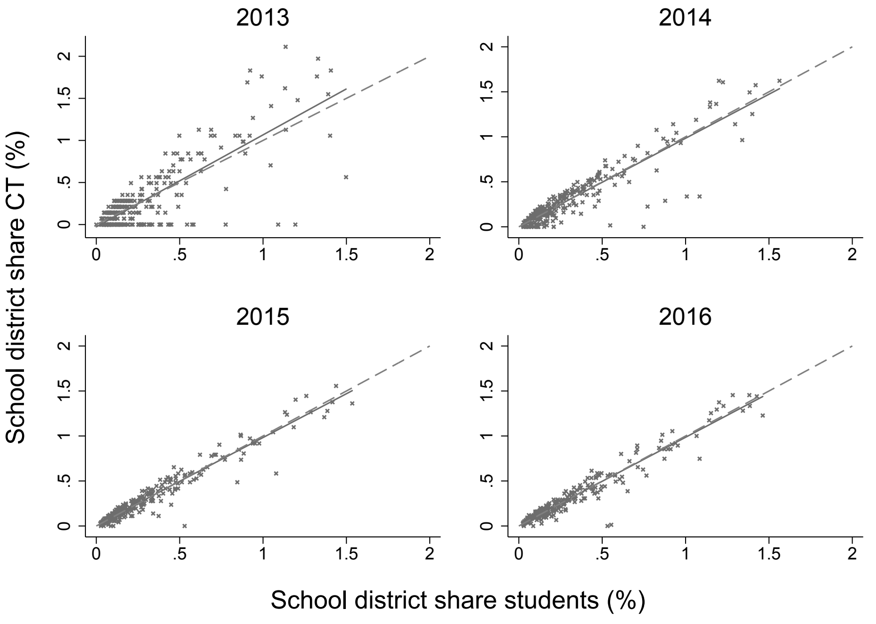


Figure 3.1. Allocation of promotions across school districts

Note: Each cross corresponds to one school district. The solid line is a linear prediction. The dashed line is the 45 degree line. For legibility, the figure excludes the four largest school districts. A figure with all school districts is included in Appendix Figure A.1.

The career teacher position

While the allocation of career teacher positions across schools rests with the school district, the promotion decisions are generally taken by school principals. Four minimum requirements need to be fulfilled to qualify for promotion. The teacher needs to be formally certified; have at least four years of experience with good testimonials from the principal; be able to demonstrate an ability to improve student outcomes and an interest to work with developing teaching; and be deemed particularly qualified by the school district in teaching and teaching-related tasks (Regeringen 2013a).

By 2016, around 14 percent of the compulsory school teachers in our sample have been promoted. The vast majority of promotions in our sample are internal – approximately 85–95 percent are working in the same school the year before they are promoted, depending on year. The career teacher positions are typically time limited but only around 2.5 percent of career teachers have a contract that lasts fewer than 12 months. A reason for having temporary contracts is to induce effort and to maintain flexibility. The proportion of permanent (i.e. not time limited) positions has increased over time and in 2016, over 45 percent were permanent. The same position is not transferable across schools or school districts.

According to the reform's regulation, teachers who become career teachers should receive a monthly wage increase of SEK 5 000 (USD 520). Ear-marked government funding is used to fund the wage increase. A main aim of the reform was to ensure that talented teachers keep teaching, as opposed to, for example, becoming principals or leaving the profession. The reform therefore stipulated that teaching and teaching-related tasks must constitute at least 50 percent of the career teachers' time. As a consequence, most career teachers have not reduced their teaching load in their new position (Statskontoret 2017).

Besides teaching, career teachers are given coaching and mentoring tasks and teaching development tasks aimed to, for example, improve teaching, train other teachers or work toward organizational change at their workplace. There may therefore be spillovers from the promoted teacher onto teacher peers in the same school (for evidence on teacher spillover effects, see Jackson and Bruegmann 2009).

Figure 3.2 describes these career teacher tasks in more detail. We extract this information from a survey to about 30 percent (92/290) of the school districts in our sample. The survey was conducted by the National Board of Education and the response rate is close to 100 percent.[12] The answers to the survey reveal that almost all school districts require that the career teachers are responsible for "Coaching other teachers" and for "Initiating pedagogical discussions". Besides that, there is a fair amount of variation in career teacher tasks across districts. We will return to this information in a heterogeneity analysis in Section 3.5.3 where we look at the differential response of the reform depending on the local design of the career teacher positions.

[12] Appendix Table A.3 compares school district characteristics depending on whether the district was included in the survey or not. Overall, the two groups appear highly similar although urban areas are somewhat underrepresented in the survey.

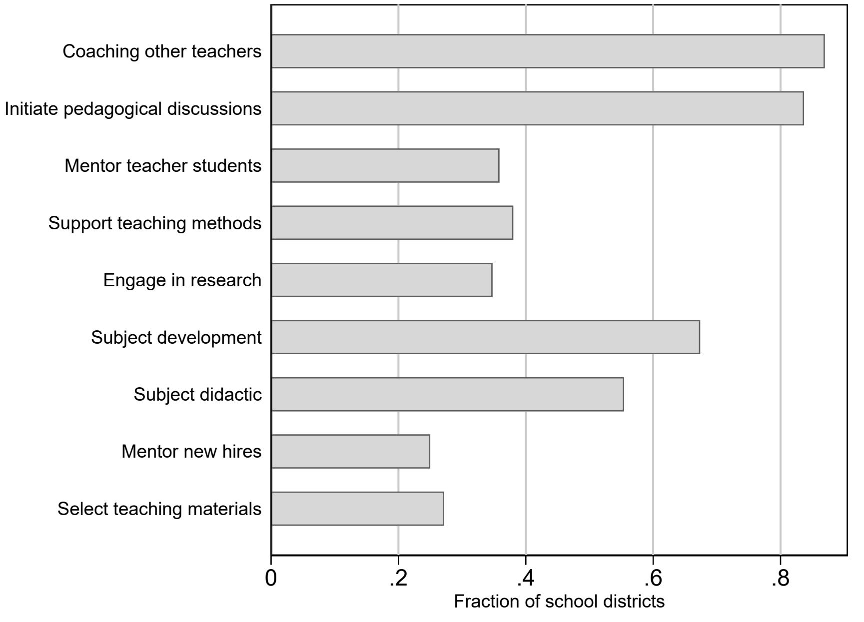


Figure 3.2. Career teacher tasks

Note: The figure shows the allocation of career teacher tasks by school districts. The data come from a survey conducted by the National Board of Education in 2015 covering around 30 percent (92/290) of school districts in our sample. The question was "What are the main tasks for your career teachers in addition/related to teaching?" and the respondents could answer yes or no to the nine tasks shown in the figure.

3.2.2 Data

To analyze the impact of the career teacher reform we combine administrative data from different Swedish registries held at Statistics Sweden. The underlying population for the analysis is the panel of Swedish schools for the years 2010 to 2016 and the teachers working at these schools, and is based on information from the Swedish teacher register (*Lärarregistret*).

The teacher register covers all school staff with educational duties employed at Swedish schools, and is collected as a part of the official statistics in the school area. Data is measured annually, in October each year, and for our purposes it contains information on person identifiers for teachers, information on where the teacher works, the teacher's experience and whether the teacher is certified.[13] The teacher register can be linked to a school register that contains school level characteristics, such as number of students and

[13]To be precise, there is information on whether the teacher is qualified, i.e. has pedagogical higher education. Information on the formal occupational license introduced in 2011 is not available in the teacher register. The main requirement to receive the license is to hold proper credentials.

school district.[14] Schools are defined using a combination of school name and municipality code.

Using person, school and year identifiers, we link the teacher register to a career teacher register that specifies whether the teacher is a career teacher. Using person and year identifiers, we also link the teacher register to demographic registers that include variables such as age, gender, level of education and field of specialization. We have data on teachers' 9th grade GPA from 1988 onward (cohorts born after 1972) which can be linked to our data using person identifiers.[15] GPA is standardized in the full population by year of graduation to have mean 0 and standard derivation 1. In addition, we retrieve information on teachers' wages from the structural earnings statistics, which contains monthly full-time adjusted wages in SEK, measured in November each year. The wage data covers everyone working in the public sector (and about 50 percent of workers in the private sector). As we only include teachers working in municipal schools in our sample (see Section 3.2.2), we have complete wage data.

Our data on student performance are drawn from records of student test scores on standardized central tests in Math, English and Swedish (*Ämnesprovsregistret*). The tests are taken in grade 3 (Math and Swedish only), grade 6 and grade 9.[16] In the first two years of our observation period (2010 and 2011), students took the national test in grade 5 instead of in grade 6. To use as much information as we can from the available data, we let the grade 5 test scores proxy for the performance in grade 6. Using the student-level data, the results of the exams are standardized by year to have mean 0 and standard deviation 1 in the full student population.[17]

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[14]As we only include municipal schools in our sample, school district is proxied by municipality code.

[15]It follows that the share of teachers in our sample for which we observe GPA increases over time. In 2010, we have GPA for 26 percent of teachers. This increases by around three percentage points per year. In 2016, we have GPA for around 45 percent.

[16]The tests are typically graded by the students' own teachers based on centrally provided guidelines, and with a recommendation from the Swedish National Agency for Education that exams are co-evaluated by another teacher (Skolverket 2018b). In 2015, co-evaluation was standard in over 75 percent of public schools (Skolinspektionen 2018). The tests are taken during the spring semester. In our analysis, we associate a spring test score with remaining data the preceding fall semester. For example, we link test scores from spring 2013 to our data from fall 2012. Consequently, regressions in Section 3.5.5 that use student test scores only use data until 2015.

[17]The tests consist of different parts that are graded separately. How many parts a test has can vary by subject and grade, and at times year. Most parts generate a test score, but some parts are pass/fail (P/F) only. An overall test score is provided for the grade 6 tests (from spring 2013) and for the grade 9 tests (all years). Whenever an overall score is provided, we use that score. When an overall score is missing, we calculate a mean test score as the aggregate number of points divided by the number of parts of the test taken, for all parts of the test that are not P/F. The Grade 5 test in all three subjects in spring 2010 only consisted of P/F questions. For this test we calculate an overall score based on the proportion of parts that the student passed. Once each student has one test score per subject and grade, we standardize the test scores by year to

Main sample

We restrict our attention to teachers whose main occupation is teaching. In particular, we include only those individuals who receive their main source of income from teaching and who work at least 50 percent at their main school. If a teacher works at several schools in the same year, only the teacher's main school is included in the sample, defined as the school with the most extensive contract. Moreover, we only include municipal compulsory schools, thereby excluding teachers who work at different tiers of education (notably, in upper secondary education) or at voucher schools. Finally, we only include schools that exist for all seven years and employ at least three teachers per year between 2010 and 2016.

Separation measures

We consider two main separation measures. The first, denoted *separations*, measures the fraction of teachers who are no longer employed next year by the current school. The second, denoted *exits*, applies the additional restriction that the teacher is not working with teaching at any compulsory or upper secondary, public or voucher school in any capacity.

As explained above, we observe teachers at schools in October each year, while career teacher promotions can take place at any time during the school year (Statskontoret 2015). In terms of timing, we therefore relate the fraction who separate between October in year *t–1* and October in year *t* to the presence of career teachers in the fall of year *t*. These career teachers are generally hired at some point during January to December in year *t*. Given data availability, the separation measures can be calculated from 2011 to 2016.

Number of observations

Table 3.1 shows the number of schools and teachers in our main sample. There are annually around 56,000 teachers working at just under 3,000 municipal compulsory schools. Participating schools shows the number of schools that have had at least one career teacher in year *t* or earlier. Promoted teachers considers teachers that have been promoted and is equal to one if the teacher has held a career teaching position in year *t* or earlier. The number of participating schools and promoted teachers are increasing over time from 2013 as the reform is rolled out.

Teacher pay determinants

Before turning to the analysis of the career teacher reform, it is useful to describe how teacher wages were determined prior to the reform. What discretion did school principals have in the wage-setting process? We examine this question in Appendix Table A.1, where we show the results from a standard Mincer regression of teacher characteristics on teacher pay. We focus on

have mean 0 and sd 1. A school is assigned a mean standardized test score by year, subject and grade based on the students that attend the school.

Table 3.1. *Schools and teachers per year*

	2010	2011	2012	2013	2014	2015	2016
Schools	2,950	2,950	2,950	2,950	2,950	2,950	2,950
Teachers	56,150	56,080	55,785	55,864	56,787	56,711	58,583
Participating schools	0	0	0	1,039	2,277	2,532	2,636
Promoted teachers	0	0	0	1,420	6,317	7,457	8,157

the pre-reform period 2010 to 2012, to avoid that the relationships reflect the outcomes of the career teacher reform of interest in this paper. As expected, wages grow with education and experience, and are higher for certified teachers and those on permanent contracts. Perhaps surprisingly, there is a negative wage–tenure relationship, which is likely to reflect that new hires (who are included in the omitted tenure category) are able to bargain their wages in times of teacher shortages. In addition, it is useful to note that the R^2 is only 0.67, even conditioning on a wide set of observable characteristics. This implies that principals to some extent do use their discretion in wage-setting.

3.3 Allocation of career teacher positions across schools and teachers

3.3.1 Allocation across schools

We are interested in studying the effect of improved career opportunities for teachers on school quality. We first explore how school districts allocate career teacher positions across schools and teachers. Panel A of Figure 3.3 shows how many of the 2,950 schools that participate by year – i.e. the timing of treatment for the schools in our sample. Panel B instead considers treatment intensity by plotting the distribution of the share of promoted teachers at a school separately by year. On average, between 1.4 and 2.2 teachers are promoted at a school (see Appendix Table A.2, which includes school summary statistics). The number of teacher promotions increased by a factor of four between 2013 and 2014 followed by a more moderate increase between 2014 and 2016 (see also Table 3.1). As a result, we see that the fraction of schools without career teachers declined from 65 to under 25 percent between 2013 and 2014. In 2016, on average 15 percent of teachers were promoted at a school that employed a career teacher, while 16 percent of schools lacked a career teacher.

To descriptively consider how the career teacher positions were allocated across schools, Table 3.2 relates the presence of career teachers to lagged school characteristics. In particular, it presents the results of the regression:

$$CT_{sdt} = \phi_{dt} + \beta_t X_{sdt-1} + \varepsilon_{sdt} \quad (3.1)$$

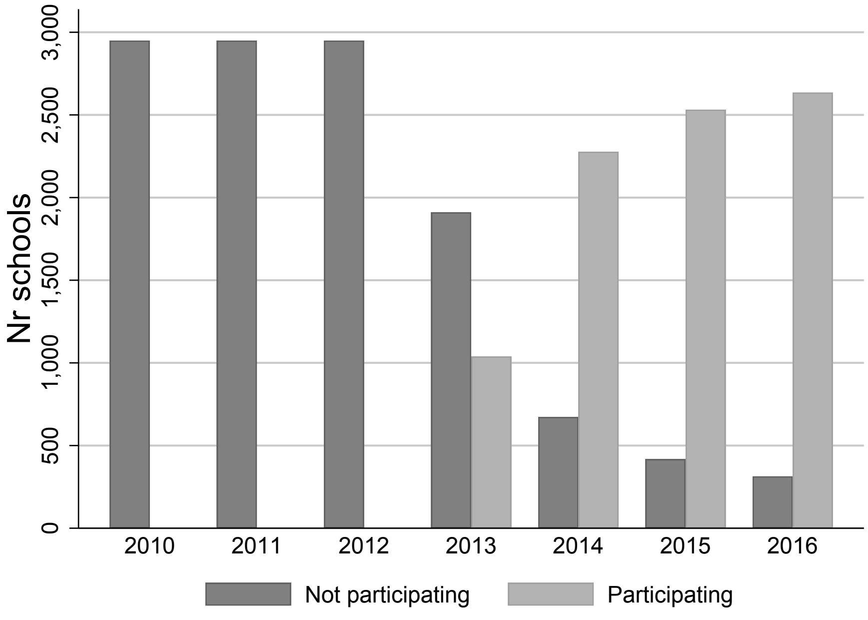
where CT_{sdt} is a dummy equal to 1 if school s in school district d has a career teacher in year t, ϕ_{dt} are school district fixed effects and X_{sdt-1} are lagged school characteristics. In Appendix Table B.1 we estimate equation 3.1 instead using the fraction of of career teachers out of all teachers at the school in year t as the dependent variable. All variables are measured at the school level. We run separate regressions by year between 2013 and 2016. We focus on when the school first participates in the reform and thereby only include schools that have no (never or not yet) career teachers in $t-1$.

The results suggest that the probability of having career teachers is increasing in school size: doubling the number of students increases the likelihood that the school has at least one career teacher (in any year) by between 10 to 20 percentage points. Besides this factor, there appear to be little systematic relation between having career teachers and observable school characteristics. Most surprising is perhaps that there is no systematic relationship between the allocation of career teacher positions across schools and the lagged separation rate. There is thus no indication that school districts allocated the career positions to schools with greater difficulties in retaining their teaching pool.[18]

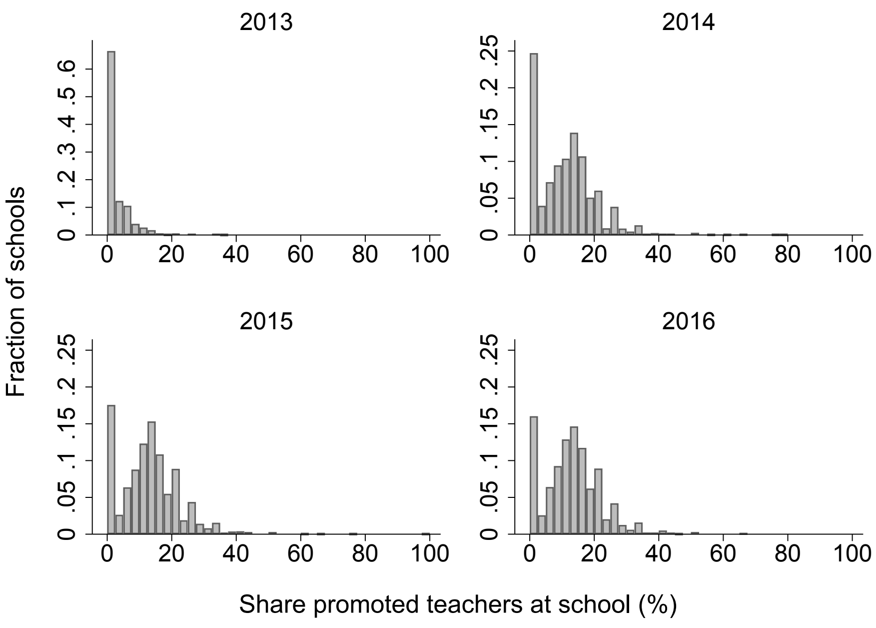
3.3.2 Allocation across teachers

Turning to *who* was promoted, Table 3.3 presents pre-reform (2012) summary statistics for our sample, separately by whether the teacher is ever promoted, never promoted and the full sample. The selection of teachers for promotion officially rested with the school district but was in practice often taken by the school principal. In line with the eligibility requirements, 97 percent of promoted teachers were employed on a permanent contract and were certified in 2012. We also see that promoted teachers are slightly more likely to be female, have slightly less experience and are slightly younger than those who are not promoted. Considering their educational field of specialization, over half of ever career teachers are specialized in either Swedish and social sciences or math and natural sciences, which is higher than those who are not promoted.

[18] In Table B.2, we also include the average test score among third- and sixth-graders, which are available for the subset of schools that have students in those grades (86/68 percent of the schools have students in grade 3/6). Reassuringly, student performance does not predict the selection of schools with promotions.



(a) Participating schools



(b) Share career teachers at schools

Figure 3.3. Variation in reform participation at school level

Note: Panel A shows the number of non-participating and participating schools. Participation is defined as having at least one career teacher. Panel B shows the fraction of schools at different shares of career teachers per year.

Table 3.2. *Factors that predict selection of schools*

	At least one CT at school			
	2013	2014	2015	2016
School characteristics $t-1$:				
Log nr students	0.210***	0.290***	0.211***	0.162***
	(0.019)	(0.015)	(0.045)	(0.048)
Student-to-teacher ratio	-0.010***	-0.011***	-0.007	-0.003
	(0.003)	(0.003)	(0.007)	(0.006)
Separation rate	0.022	0.003	0.081	0.214
	(0.079)	(0.091)	(0.192)	(0.192)
Exit rate	-0.067	0.114	0.050	-0.287
	(0.106)	(0.120)	(0.220)	(0.217)
Certified (share)	0.014	0.252*	0.240	-0.199
	(0.157)	(0.145)	(0.246)	(0.246)
Female (share)	-0.066	0.170*	-0.139	0.098
	(0.076)	(0.091)	(0.153)	(0.170)
Mean age (years)	-0.010**	-0.006	-0.000	0.008
	(0.005)	(0.005)	(0.007)	(0.007)
Mean experience (years)	0.003	-0.001	0.000	-0.004
	(0.004)	(0.005)	(0.008)	(0.007)
Math/natural science (share)	0.042	-0.110	-0.203	-0.036
	(0.079)	(0.110)	(0.146)	(0.171)
Swedish/soc. science (share)	0.073	-0.077	-0.092	0.232
	(0.063)	(0.089)	(0.133)	(0.144)
District FE	Yes	Yes	Yes	Yes
R^2	0.349	0.411	0.471	0.572
N	2,941	1,882	611	346
Mean dep. var.	.35	.65	.38	.25
Nr school districts	290	277	207	169

Note: This presents the results of regressions of $CT_{sdt} = \phi_{dt} + \beta_t X_{sdt-1} + \varepsilon_{sdt}$ where CT_{sdt} is a dummy equal to 1 if the school participates in the reform in year t. Variables are measured at school level. Regressions are estimated separately by year and only include schools that have not (never or yet) participated as well as schools that participate for the first time. Standard errors are clustered at school district level and reported in parenthesis.

Table 3.3. *Teacher summary statistics, 2012*

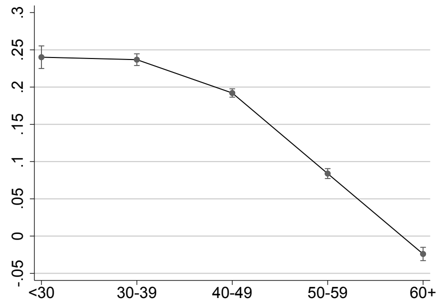
	Ever career		Never career		Full sample	
	Mean	sd	Mean	sd	Mean	sd
Female	0.83	(0.38)	0.78	(0.42)	0.78	(0.41)
Age (years)	43.27	(7.87)	47.02	(10.67)	46.47	(10.39)
Experience (years)	13.75	(7.77)	16.01	(11.27)	15.68	(10.86)
Permanent contract	0.97	(0.16)	0.90	(0.30)	0.91	(0.28)
Certified	0.97	(0.17)	0.90	(0.30)	0.91	(0.28)
Monthly wage (SEK)	27,943	(2,639)	27,426	(2,894)	27,501	(2,864)
Educ. specialization						
Math/natural science	0.25	(0.43)	0.14	(0.35)	0.16	(0.37)
Swedish/social science	0.28	(0.45)	0.19	(0.39)	0.20	(0.40)
Languages	0.08	(0.27)	0.05	(0.22)	0.06	(0.23)
Vocational	0.06	(0.24)	0.11	(0.31)	0.10	(0.30)
Other teaching	0.32	(0.47)	0.44	(0.50)	0.43	(0.49)
Non-teaching	0.01	(0.12)	0.06	(0.23)	0.05	(0.22)
Observations	8,160		47,625		55,785	

To more formally assess who was promoted, we estimate linear models using OLS by regressing a dummy for ever being a career teacher during our sample period, $EverCT_{ist}$, on teacher characteristics X_{ist} (age, gender, teacher GPA, wage decile, educational level and specialization, type of contract, certification, tenure and experience):

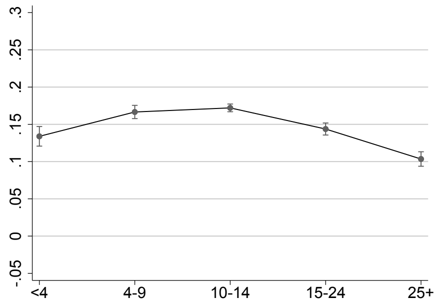
$$EverCT_{ist} = \beta X_{ist} + \delta_s + \varepsilon_{ist} \tag{3.2}$$

School fixed effects are also included. Regressions are estimated using only data from the pre-reform year 2012. Figure 3.4 presents the results of this analysis. It shows linear predictions of promotion with 95% confidence intervals. Full regression results are included in Appendix Table B.3.

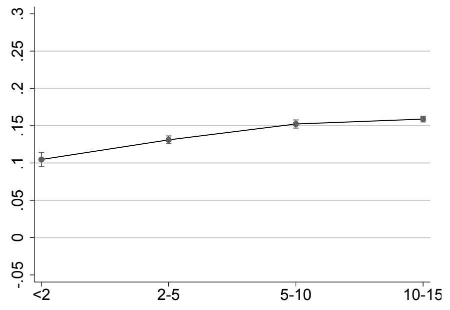
The estimates confirm that the likelihood of being promoted increases slightly with tenure while it decreases with age. Experience shows an inverted U-shape. The likelihood of being promoted also increases marginally with teachers' compulsory school GPA. Wage decile, which measures in which decile in the school wage distribution the teacher is in 2012, predicts promotion most strongly. The results indicate, for example, that someone at the highest wage decile in their school has over a 25% likelihood of being promoted. In addition, we find that women as well as those with a pedagogical educational specialization in math and natural sciences are slightly more likely to be promoted.



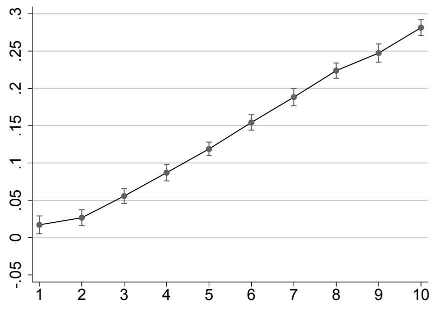
(a) Age band



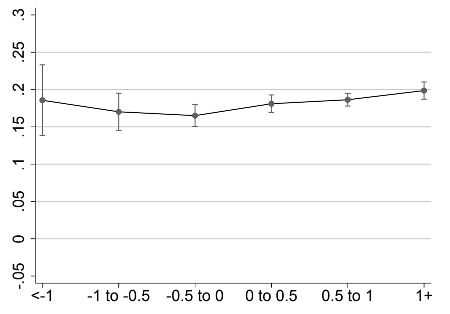
(b) Experience band



(c) Tenure band



(d) Wage decile



(e) Standardized 9th grade GPA

Figure 3.4. Predicted probabilities of ever being promoted, 2012

Note: This presents linear predictions from the regression $EverCT_{ist} = \beta X_{ist} + \delta_s + \varepsilon_{ist}$, using data only from the year before the reform is introduced (2012). Full results are in Appendix Table B.3. Only in Panel (e) do we include the teacher's standardized 9th grade GPA. $EverCT_{ist}$ is an indicator equal to 1 if the teacher is ever a career teacher during our sample period. Standard errors are clustered at school district level.

3.4 Pass-through of stipulated wage increase on promoted teachers' wages

A central component of the reform was to give promoted teachers a wage increase. Considering aggregate wage effects among teachers, Figure 3.5 shows the distribution of teacher wages in the years surrounding the reform. It clearly suggests that the policy had a meaningful effect on the level and distribution of wages: mean full-time wages were around SEK 27 000 (approx. USD 2 800) between 2010 and 2012, and nearly SEK 31 000 (approx. USD 3 200) in 2015, representing a 15 percent increase in mean wages after introducing the promotion program.[19] In addition, the reform seems to have created a much more prominent right hand tail of the wage distribution, as expected to see in professional occupations where pay aligns with performance.

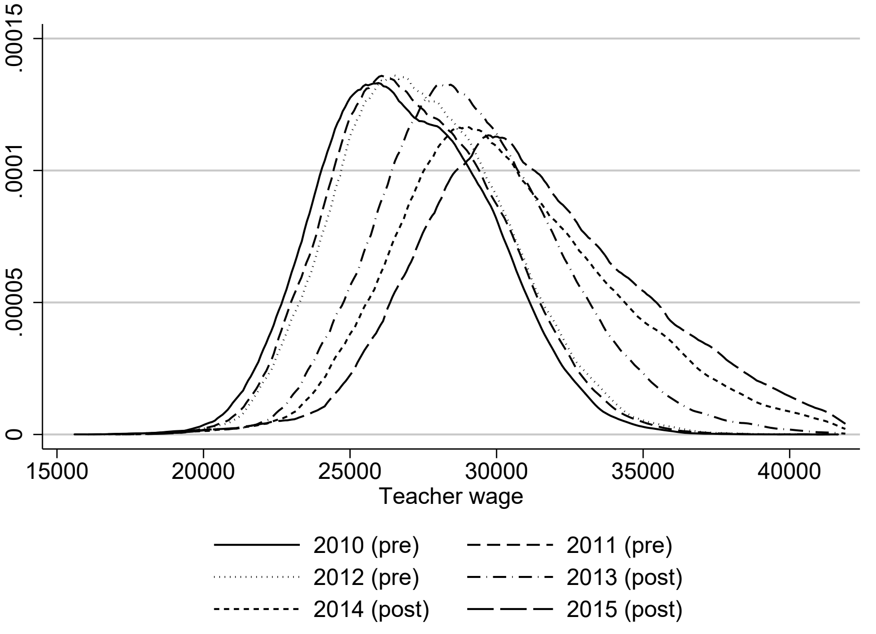


Figure 3.5. Kernel density of teacher wages (2010–2015)

Note: The figure shows the distribution of teacher monthly full-time wages for each year before and after the career teacher reform.

Figure 3.6 shows mean wages for those that never become career teachers during our observation period compared to those that become career teachers, by year of promotion (2013 to 2016). Prior to promotion, mean wages are very similar. While non-career teacher wages trend upwards slightly over time, mean wages for career teachers increase sharply upon promotion.

[19] Wages are expressed in nominal terms since this is a zero inflation period. From 2010 to 2016 CPI increased by 4.3 percent with an average inflation rate of 0.07 percent.

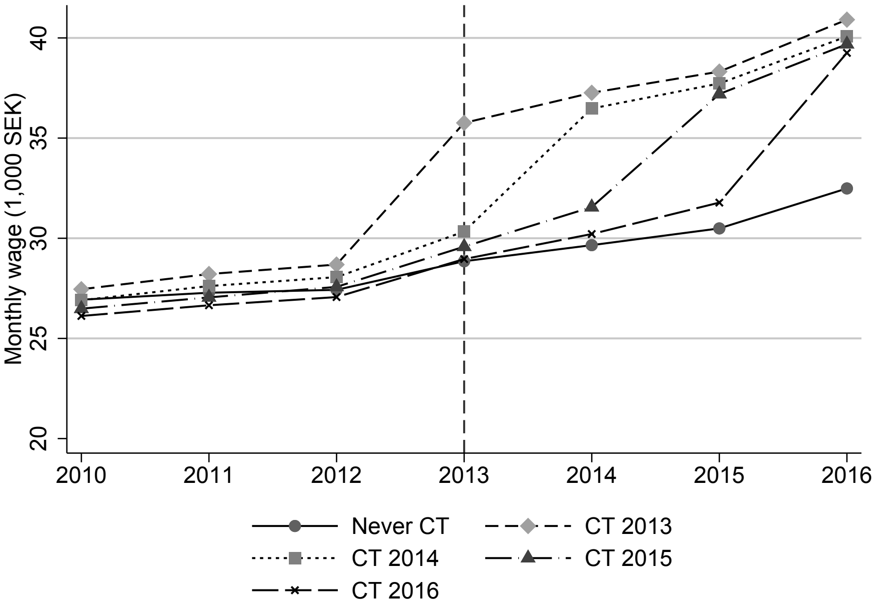


Figure 3.6. Wages and the timing of promotion

Note: The figure shows the mean teacher wage for teachers divided by the year of promotion to career teacher. The solid line shows the wage of never promoted teachers.

The figure supports that wages increased after the implementation of the reform. From a theoretical perspective it is not clear, however, that we should expect full pass-through of the stipulated wage increase onto promoted teachers wages. If, for example, job satisfaction depends on relative pay as shown by Card et al. (2012), school principals may have incentives to, at least partly, compensate non-promoted teachers in local wage negotiations. To more formally assess how wages for promoted teachers differ from those who are not promoted, we estimate regressions of the following form:

$$\ln(w_{ist}) = \alpha_i + \lambda_t + \theta CT_{ist} + \beta X_{ist} + \varepsilon_{ist} \tag{3.3}$$

where $\ln(w_{ist})$ are log monthly full-time-equivalent teacher wages and CT_{ist} is a dummy equal to one if the teacher is promoted.[20] We include year fixed effects λ_t to control for time effects common to all individuals and teacher fixed effects, α_i, to control for individual-specific heterogeneity in wages. We therefore rely on within-individual deviations to identify estimates of θ. To account for correlation in wages between teachers that work in the same school district, standard errors are clustered at the school district level.

[20] As mentioned above, promotions are not necessarily permanent. However, once an individual has held a CT position, we consider the individual to be treated. The dummy is therefore equal to 1 from year t onward, if the teacher is promoted in t.

The results are shown by Panel A of Table 3.4. Column (1) uses the full sample of teachers while column (2) only includes non-promoted teachers as well as career teachers in their first year of promotion. They suggest that the wage increase associated with a promotion is approximately 15%.[21] In Panel B, we use the monthly wage in Swedish crowns (SEK) as the outcome. These results confirm that the wage impact of a promotion is very close to the 5 000 SEK wage increase stipulated by the reform, particularly if we consider the wage increase associated with the first time a teacher is promoted, shown in column 2.[22]

Table 3.4. *Wage effects of promotion*

Sample	(1) Full	(2) First time CT
Panel A: ln*(wage)*		
Promoted	0.149***	0.141***
	(0.002)	(0.002)
R^2	0.958	0.904
N	374,108	260,619
Panel B: Monthly wage (SEK)		
Promoted	5329.2***	4744.3***
	(65.4)	(58.6)
R^2	0.954	0.900
N	374,108	260,619
Year FE	Yes	
Individual FE	Yes	
Controls	Yes	Yes including lag wage

Note: The table provides results of the regressions $y_{ist} = \alpha_i + \lambda_t + \theta CT_{ist} + \beta X_{ist} + \varepsilon_{ist}$. Controls are included for female, age, level of education, teacher certification, permanent contract, educational specialization, experience and tenure. Standard errors are clustered at school district level and reported in parenthesis. In specification (2) the sample is censored to only include the first year of becoming a career teacher.

Given that selection of career teachers is non-random, we may worry that those that are promoted are on a different wage trend than those who are not promoted. Indeed, the analysis above showed that career teachers are more often taken from higher wage deciles in the schools that they work. As a more formal complement to Figure 3.6, we consider whether there are differences in pre-treatment wage trends as well as the dynamics after promotion by esti-

[21] Appendix Table B.4 shows results from alternative specifications including models without teacher fixed effects.

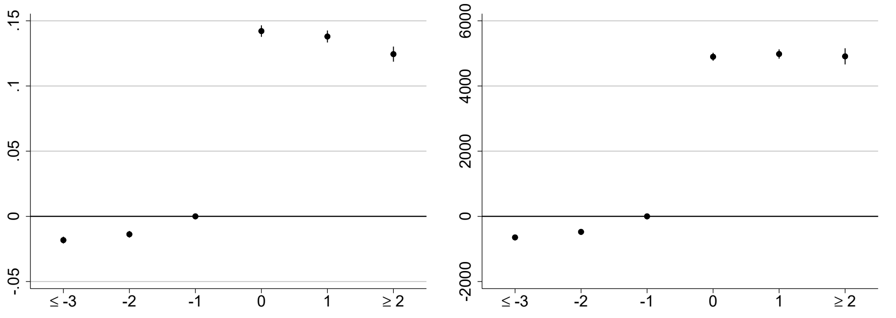
[22] The fact that the estimate in column (1) of Panel B is slightly larger probably reflects the outcome in subsequent wage negotiations, where pay raises are based on the current wage.

mating an event-time specification as follows:

$$\ln(w_{ist}) = \alpha_i + \lambda_t + \sum_{\tau \neq -1} \gamma_\tau D_i 1[\tau] + \beta X_{ist} + \varepsilon_{ist} \tag{3.4}$$

t is calendar time and τ is event-time. τ denotes the time relative to when the teacher is first promoted, which occurs when τ equals 0. Observations three or more event years before treatment ($\tau \leq -3$) or two or more event years after ($\tau \geq 2$) are grouped. D_i is a dummy variable indicating whether the teacher is ever promoted and $1[\tau]$ is an indicator function equal to 1 in τ. The year before treatment is omitted.

From Figure 3.7, which plots the estimates of the parameters γ_τ, we see a clear jump in wages the year the teacher becomes promoted relative to the year prior to promotion. The higher wage is persistent, but does not appear to grow, over time. The non-zero effect prior to promotion suggests that the wage trajectories for promoted and non-promoted individuals are nearly but not exactly parallel; those who are selected for promotion appear to be on a slightly higher wage trend.



(a) ln(wage) *(b)* Monthly wage (SEK)

Figure 3.7. Dynamic effects in teacher wage outcomes

Note: Figure 3.7 displays γ_τ-coefficients with 95%-confidence intervals from estimating equation 3.4. $\tau = -1$ is omitted. Controls included in X_{ist} are dummies for female, certification, permanent contract, age, level of education, educational specialization, experience and tenure. Standard errors are clustered at school district level.

3.5 Impact of promotions on teacher separations, composition and student performance

The results from section 3.4 suggest that the career teacher reform had a substantial impact on the wages of promoted teachers. In this section we examine its impact on teacher separations, teacher composition and student performance. To this end, we use data on outcomes aggregated to the school-level, and rely on variation in the *timing* of participation across schools, which we show below appears to be unrelated to observable school characteristics.

We first discuss the empirical model and the validity of the identifying assumptions in Section 3.5.1. Section 3.5.2 presents results on teacher turnover while Section 3.5.3 provides a heterogeneity analysis. Section 3.5.4 focuses on teacher composition and Section 3.5.5 finally looks at student performance.

3.5.1 Empirical strategy and identification

An empirical challenge we face is the lack of a natural control group: all school districts can potentially participate in the reform, and the extent to which they can potentially participate is determined by their share of students (see Section 3.2.1). Indeed, all but one school district in our sample participates and there is limited variation in the timing of participation.

We therefore estimate the impact of the new career opportunities using school-level variation, rather than school district-level variation, in the appointment of career teachers. More specifically, we employ a difference-in-differences strategy that compares outcomes such as the separation rate in schools that have at least one career teacher to schools that do not (never or yet) have career teachers. We estimate models of the following form:

$$y_{st} = \gamma CT_{st} + \delta_s + \lambda_t + \beta X_{st} + \varepsilon_{st} \tag{3.5}$$

y_{st} is the outcome of interest in school s in year t and CT_{st} is a variable indicating if school s has at least one career teacher in year t (i.e. if it has participated in the reform in year t). We also report results when we use the fraction of career teachers at the school in year t relative to the total number of teachers in $t-1$ as CT_{st} to exploit that treatment intensity may vary across schools, thus gaining more variation.[23] Furthermore, we control for the student to teacher ratio (defined as the number of students in year t as the share of the number of teachers in $t-1$) and the log number of students in t, captured by X_{st}. δ_s and λ_t are school and year fixed effects respectively.

The empirical strategy will identify relative differences in outcomes across schools rather than aggregate effects on school quality. It relies on the assumption that, in absence of appointing a career teacher, the outcome variable would have evolved in parallel in participating and non-participating schools. For our empirical strategy to work, the timing of when the reform is implemented in specific schools must be uncorrelated with changes in other determinants of the outcome that we do not control for.

School districts decide how to allocate the career teaching positions across schools in their district – participation is not random. There is potential selection both with regards to which schools participate and when they partici-

[23] We compute the share using lagged number of teachers as separations may be affected by the reform.

pate. Reassuringly, the results in Table 3.2 suggest that no (lagged) observable school characteristics besides school size systematically predict the probability of participating in the reform in a given year. To further assess the identifying assumption, we consider whether any factors predict the timing of participation, conditional on having at least one promoted teacher between 2013 and 2016. In particular, we are interested in whether pre-reform observed school characteristics are orthogonal to the year of first participation, conditional on sometime participating in the reform. To this end, we estimate the following regression separately by year for schools that do not yet participate:

$$Year_{sd} = \phi_{dt} + \beta_t X_{sdt-1} + \varepsilon_{sdt} \tag{3.6}$$

$Year_{sd}$ is equal to the year that the school first participates (i.e. a year in the interval 2013 to 2016), ϕ_{dt} are school district fixed effects and X_{sdt-1} are lagged school characteristics.[24] The results, included in Appendix Table B.5 and B.6, suggest that schools with more students first participate earlier. No other factors appear to systematically influence the timing of participation. We control for the log number of students and the student-to-teacher ratio in the regressions below.

To further assess the validity of the identifying assumption, we also perform an event-study analysis to rule out pre-participation trend differences in wages and separation rates between teachers in promoting and non-promoting schools using a dynamic version of equation (3.5):

$$y_{st} = \sum_{\tau \neq -1} \gamma_\tau D_s 1[\tau] + \delta_s + \lambda_t + \beta X_{st} + \varepsilon_{st} \tag{3.7}$$

D_s is a dummy variable indicating whether the school ever participates in the reform and τ denotes the time relative to when the school first participates, which occurs when τ equals 0. The year before treatment is omitted.

3.5.2 Results on teacher turnover

Table 3.5 shows the γ-coefficients obtained when estimating the model given by equation 3.5. We focus on four outcomes. First, in column (1) we again confirm that the implementation of the new career step translates into a wage difference between promoting and non-promoting schools; the effect is of similar size as in section 3.4.[25] In line with the intentions of the reform we also see increased wage dispersion in schools with career teachers (see column 2).

[24] This analysis was inspired by Deshpande and Li (2019) who provide a similar analysis to assess the systematic factors predicting the timing of closings of Social Security Administration field offices. It has many parallels to the methodology in Jackson (2010) who uses variation in the time of adoption to analyze a program in Texas that pays students and teachers for passing Advanced Placement exams.

[25] We infer this from column (1) of Panel B.

Wage dispersion is defined as the variance in log wages at the school-level. In columns (3) and (4) we consider separations and exits from teaching. In column (3), we look at school separations in general. Our results suggest that schools with at least one career teacher have a one percentage point lower separation rate, which corresponds to roughly four percent. In column (4), we focus on the fraction of teachers exiting the teaching occupation. This effect is also negative, but smaller in magnitude and does not reach statistical significance at the 10%-level.

In Panel B, we show results when we take the treatment intensity into account by relating the outcomes of interest to the fraction of career teachers at the school-level. While this model gives us more variation and provides estimates that are easier to interpret, it also requires that the "share of promoted teachers" at the school level is exogenous. In Appendix Table B.1 we do not find that any (lagged) observable school characteristics systematically predict the fraction of career teachers in a given year. Bearing this in mind, the estimates suggest that increasing the share of career teachers by 10 percent at a school is associated with a reduction in the separation rate by around two percentage points, or nine percent, and a reduction in exits from the profession with 0.6 percentage points, or five percent. Hence, separations in general and from the occupation is reduced and the effects are significant in size. Moreover, a 10 percent increase in the share of career teachers increases the variance of log wages by 0.0024 units, or 25 percent.

In Figure 3.8 we plot estimates of γ_τ from equation 3.7 to further investigate whether the timing of exposure to the reform is exogenous. Reassuringly, outcomes evolve very similarly in promoting and non-promoting schools prior to the implementation of the new career step. This analysis also shows that the responses grow over time, which is likely to reflect that the number of career teachers increases after the first year of participation in the reform.

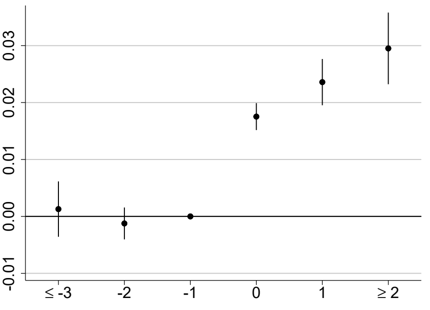
In summary, we find that schools that promote teachers to 'career teachers' have higher average wages, a wider wage distribution, and lower teacher turnover both in terms of general separations and exits from the teacher profession. The impact of the reform tends to grow over time, and there appear to be no pre-reform effects.

Appendix C presents a range of robustness checks to ensure that our results are robust to alterations of the empirical model. Table C.1 shows the results when the treatment is defined as hiring at least one career teacher, whereas Table C.2 considers treatment as the share of teachers at the school that have been promoted. Panel A of Tables C.1 and C.2 first report the baseline results from Table 3.5. Panels B to F include sensitivity analyses. In Panel B we omit the time-varying school controls (i.e. the number of students and the student/teacher ratio); in Panel C we restrict the comparison to outcomes among teachers who were eligible for the career-teacher promotions, i.e. to certified teachers with at least four years of teaching experience; in Panel D we weight the regressions with the number of teachers in $t-1$; in Panel E we restrict the

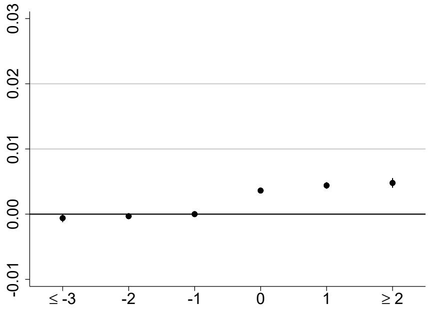
Table 3.5. *Wages and separations in participating vs. non-participating schools*

	(1)	(2)	(3)	(4)
	Log wages	Wage dispersion	Separations	Exits
Panel A:				
At least one CT	0.019***	0.004***	-0.010*	-0.004
	(0.001)	(0.000)	(0.005)	(0.003)
R^2	0.914	0.583	0.339	0.238
N	17,689	17,689	17,689	17,689
Panel B:				
Share CT	0.165***	0.024***	-0.207***	-0.057***
	(0.007)	(0.001)	(0.025)	(0.015)
R^2	0.921	0.596	0.345	0.239
N	17,689	17,689	17,689	17,689
Year FE:s	Yes	Yes	Yes	Yes
School FE:s	Yes	Yes	Yes	Yes
School controls	Yes	Yes	Yes	Yes
Control mean	10.231	0.009	0.222	0.114

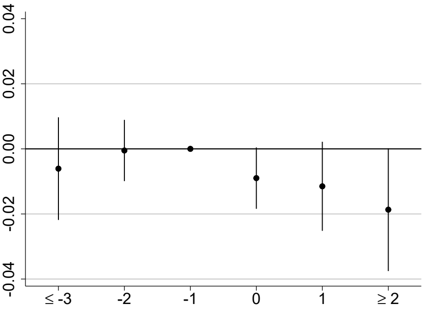
Note: In the table, we relate the change in the presence of career teachers within a school in year t to the change in mean wages (col. 1), wage dispersion (col. 2), school separations (col 3) and exits (col 4) (see Section 3.2.2 for the exact definition of these variables). At least one CT is an indicator variable equal to one from the first year that the school has promoted a career teacher onward. Share CT at school is defined as the number of CT in t divided by the number of teachers in $t-1$. School controls are the student-to-teacher ratio and the log number of students. Standard errors are clustered by school district and reported in parenthesis.



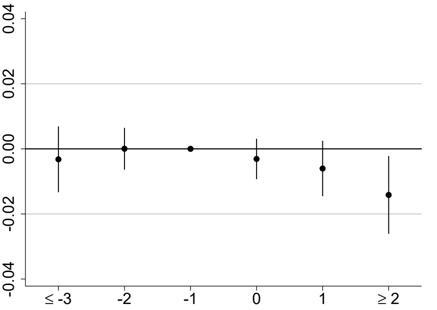
(a) Log wages



(b) Wage dispersion



(c) Separations



(d) Exits

Figure 3.8. Dynamic school responses

Note: The figure plots γ_τ-coefficients with 95%-confidence intervals from estimating eq. 3.7. It shows the evolution of log wages, wage dispersion, school separations and exits from the teaching professions within schools before and after promoting at least one career teacher. $\tau = -1$ is omitted.

comparison to schools that had at least one career teacher during 2013–2016; and in Panel F we control for whether the school participated in the Boost for Mathematics, the Boost for Reading, or the Teachers' Salary Boost, three other policy initiatives implemented in 2013, 2015 and 2016 respectively. We conclude that the results are very stable across different empirical models and samples for both treatment variables.

3.5.3 Heterogeneity analysis

By type of career teacher

To understand how the results differ for different types of career teachers, we perform two types of heterogeneity analysis. First, we assess how the impact of the career teachers varies with the local design of the positions. To this end, we use the school district survey information about career teacher tasks described in Section 3.2.1. To reduce the dimensionality of the data we use principal component analysis focusing on the simplest two-component case in which there are two possible career teacher designs. Based on this exercise, we identify two career teacher designs, which we label "Coaching and mentoring" and "Teaching development".[26] Table A.3 in Appendix shows that districts adopting a "Coaching and mentoring" design are somewhat more common in urban areas and have more students and teachers compared to districts adopting a "Teaching development design". Reassuringly, wages and exit rates appear almost identical in the two groups. Figure 3.9 shows the impact on wages and separations separately in school districts with the two types of career teacher designs. As we would expect, wage responses are the same in the two groups. However, the separation responses, both in terms of school turnover and exits from the teaching profession, appear stronger in districts where the career teachers are instructed to engage more in teaching development (see Panel A in Table B.7 for the estimates obtained when running eq. 3.5 separately for the two groups). This heterogeneous impact suggests that the reduction in teacher turnover is not only due to the pay increase.

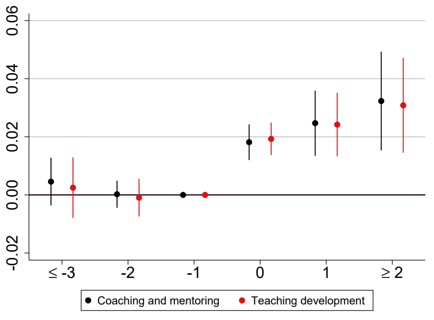
Second, we calculate the four outcomes (log wages, wage dispersion, share separate and share exit) separately for junior and senior teachers.[27] We then estimate equation 3.5 using the outcomes for senior and junior teachers respectively. The results, presented in Panel B of Appendix Table B.7, show that the effects of the reform are driven by the senior teachers exclusively. In particular, it is wages and wage dispersion for senior teachers that respond positively to the reform, and it is senior teachers who quit to a lower extent. This is in line with the reform's design, as one criteria for promotion was to be experienced (see Section 3.2.1).

By district type

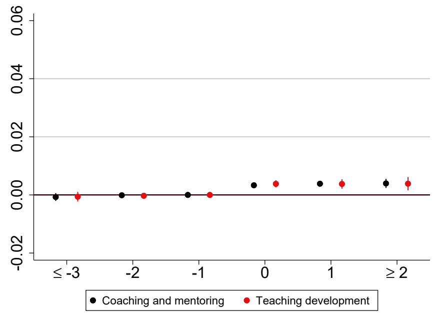
The probability of leaving a school should be related to the outside options for teachers in the local area. Thus, we would expect the reform to have a greater impact in areas with better outside options both in and outside teaching. To assess this, we have estimated equation 3.5 separately for urban and rural school districts. The results of this analysis, presented in Appendix Table B.7, show that the wage effects are very similar across urban and rural school districts. Given that schools have closely followed the rules stipulated by the reform, this is not surprising. The results on separations, on the other hand, suggest that the reduction in separations is largely driven by schools located in urban areas. For schools in urban areas, participating in the reform reduced separations by approximately 1.7 percentage points, or 7 to 8 percent. For

[26]Figure A.3 shows the scatter plot of the loadings on the two components. We note that pedagogical tasks such as "Subject didactics" (CTtask 7) and "Leading pedagogical discussions" (CTtask 2) load on both components, but that mentoring colleagues, interns and students (CTtask 1, 3 and 8) load on component 1 (Coaching and mentoring) only whereas teaching development tasks such as "Research engagement" (CTtask 5) load on component 2 only (Teaching development).

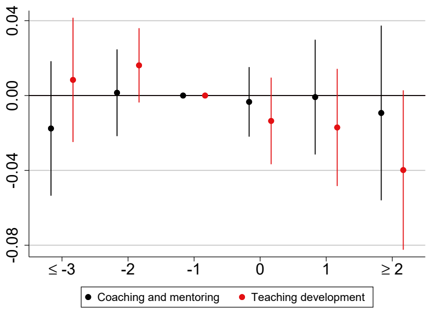
[27]We define senior teachers as those with at least five years of teaching experience.



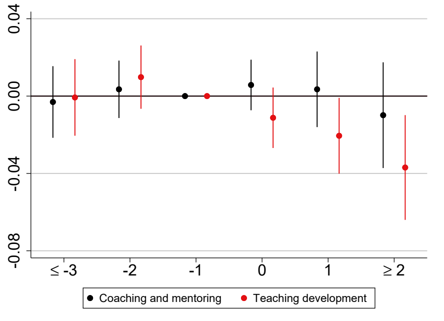
(a) Log wages



(b) Wage dispersion



(c) Separations



(d) Exits

Figure 3.9. Dynamic school responses by local CT design

Note: The figure plots γ-coefficients with 95%-confidence intervals from estimating eq. 3.7 separately for districts using "Coaching and mentoring CTs" and districts using "Teaching development CTs". The survey information is available for 1/3 of the districts used in the main analysis.

schools in rural areas, the result is marginally negative but not statistically significant at the 10%-level.[28]

The second heterogeneity analysis focuses on school districts that appear to follow an explicit decision rule. As explained in Section 3.2.1, career teaching positions were allocated across school districts nationally using the school district's share of students. Figure A.2 shows that there is much more variation within school districts – school districts have not systematically followed this rule to allocate positions across schools. Nevertheless, the extent to which this rule is used differs by school district. To identify school districts that seem to allocate positions across schools according to this rule, we correlate the share of career teachers with the share of students within school districts. The distribution of the correlation coefficient is displayed in Figure B.1. We then estimate equation 3.5 for schools that are above the 50th and 75th percentile of

[28] The results that consider treatment intensity – share of teachers promoted – find negative and statistically significant effects for both urban and rural areas, but the effects in urban areas are of a magnitude 1.5 times higher than those in rural areas. These results are available upon request.

correlation coefficients. The results, presented in Panel D of Appendix Table B.7, show that there is little heterogeneity irrespective of whether the school district allocated positions according to this rule or not.

Responses among non-promoted teachers

Finally, we consider the response among non-promoted teachers. While the career teacher promotions are expected to reduce turnover among promoted teachers it is, as previously discussed, not clear how they will impact turnover in the pool of non-promoted teachers. Since the reform entailed both higher pay and increased responsibilities for teaching development and coaching other teachers, it is possible that part of the overall impact on turnover rates reflects reduced separations among non-promoted teachers, due to, for example, positive effects on work environment and support. At the same time, promotions could also increase teacher turnover among teachers *not* selected for a career teacher promotion via the kind of "discouragement effects" documented by Card et al. (2012).

We explore this aspect by comparing non-promoted teachers in schools that have and have not introduced promotions. A concern is that the pool of non-promoted teachers is likely different in schools with and without promotions (due to the selection of teachers for promotions documented in Section 3.3). To make the comparison as credible as possible, we therefore also account for the teacher's observable lagged characteristics with the purpose of comparing non-promoted teachers with similar chances of being promoted (based on observables). The analysis is based on the following model:

$$y_{ist}^{notCT} = \gamma CT_{st} + \alpha X_{ist-1} + \delta_s + \lambda_t + \beta X_{st} + \varepsilon_{ist} \qquad (3.8)$$

where y_{ist}^{notCT} is a dummy variable equal to 1 if a non-promoted teacher separates or exist, and CT_{st} is either a dummy for having at least one career teacher or the fraction of career teachers at the school in year t. The observable teacher characteristics are included in X_{ist-1}. In addition to the teacher characteristics we, as before, account for the student to teacher ratio and the log number of students, as well as school and year fixed effects.

Results are presented in Table 3.6. Interestingly, these suggest that non-promoted teachers also decrease their separation rate when promotions are introduced at the school. The magnitudes are smaller than the overall effects documented in Table 3.5, but suggest that the lower turnover rates in schools with promotions are partly driven by the pool of non-promoted teachers.[29]

[29] In Table B.8 we use the wage of non-promoted teachers as outcome when we estimate equation 3.8. The results suggest a slight tendency for non-promoted teachers in schools having implemented the reform to be compensated relative to non-promoted teachers in schools without career teachers. However, the magnitudes are small and only statistically significant when we use the share of career teachers as treatment.

Table 3.6. *Separations for non-promoted teachers in participating vs. non-participating schools*

	(1)	(2)	(3)	(4)
	Separations	Separations	Exits	Exits
Panel A:				
At least one CT at school	-0.008**	-0.008*	-0.002	-0.002
	(0.004)	(0.004)	(0.003)	(0.003)
R^2	0.033	0.075	0.016	0.061
N	322,011	321,279	322,011	321,279
Panel B:				
Share CT at school	-0.078***	-0.079***	-0.005	-0.004
	(0.021)	(0.021)	(0.015)	(0.014)
R^2	0.033	0.075	0.016	0.061
N	322,011	321,279	322,011	321,279
Year FE:s	Yes	Yes	Yes	Yes
School FE:s	Yes	Yes	Yes	Yes
School controls	Yes	Yes	Yes	Yes
Teacher controls		Yes		Yes
Control mean	0.223	0.223	0.118	0.118

Note: In the table, we relate the change in the presence of career teachers within a school in year t to an indicator for if the teacher leaves the school or occupation between $t-1$ and t. School controls are the student to teacher ratio and log number of students. Lagged teacher controls are gender, age band, experience band, tenure band, level of education, educational specialization, wage decile and whether the teacher is certified.

3.5.4 Teacher composition

Since our results suggest a reduction in teacher turnover in response to the career teacher promotions, it is interesting to also consider compositional effects. Table 3.7 shows the γ-estimates from equation 3.5 for each of our four teacher composition outcomes: the fraction of certified teachers, the fraction experienced teachers, the median years of experience among teachers per school, and teacher average compulsory school grades.

The results suggest a positive effect on all four outcomes within schools that have career teachers, but the magnitudes are fairly small: a ten percent increase in the fraction of promoted teachers is associated with an increase in the fraction of certified teachers by 0.5 percentage points; an increase in the fraction of experienced teachers by 1 percentage point or about two years of experience; and a 1.5 percent of a standard deviation increase in average teacher grades. The corresponding event study results in Appendix Figure B.2 do not show significant pre-effects.

Table 3.7. *Teacher composition*

	(1)	(2)	(3)	(4)
	Certified	Experienced	Median experience	Teacher grades
Panel A:				
At least one CT at school	0.005***	0.007*	0.361***	0.027***
	(0.002)	(0.004)	(0.135)	(0.010)
R^2	0.627	0.468	0.620	0.608
N	17,689	17,689	17,689	17,057
Panel B:				
Share CT at school	0.060***	0.101***	2.229***	0.149***
	(0.010)	(0.021)	(0.670)	(0.054)
R^2	0.628	0.470	0.621	0.608
N	17,689	17,689	17,689	17,057
Year FE:s	Yes	Yes	Yes	Yes
School FE:s	Yes	Yes	Yes	Yes
School controls	Yes	Yes	Yes	Yes
Control mean	0.930	0.888	13.66	0.587

Note: In the table, we relate the effect of introducing the reform in a school in year t to the change in the share of certified teachers (col. 1), the share of experienced teachers, defined as the share with at least four years of experience (col. 2), the median level of experience at the school (col. 3), and the average standardized grades among teachers (col. 4). Teacher grades are standardized by graduation year to have mean 0 and st.d. 1. School controls are the student to teacher ratio and log number of students.

3.5.5 Student performance

Finally, we look at the impact of promotions on student performance. The fact that the reform had a substantial impact on teacher wages and wage dispersion, a negative effect on teacher separations, and led to a small increase in the share of certified teachers, experienced teachers and teachers who themselves had higher average compulsory school grades implies that the career teacher reform may also have affected student outcomes, either through teacher sorting or effort. As career teachers were tasked with improving teaching practices, to be a mentor, and to lead pedagogical development projects at their schools, the general work environment may also have improved. The reduced separations among non-promoted teachers in participating schools indicates that the reform may have had a positive impact on the motivation of non-promoted teachers. This is in line with the findings by Jackson and Bruegmann (2009) and Papay et al. (2016) that the quality of teacher peers has a positive influence on student outcomes.

As described in Section 3.2.2, we measure student performance using standardized test scores on national exams in Math, English and Swedish at different grade levels.[30] While we can link teachers to students using the school identifier, unfortunately we cannot match teachers to classes. However, to make the analysis more precise, we use information about the teachers' subject and level of teaching in order to associate the standardized result from the subject-specific national exams in grade 3, 6 and 9 to subject and teaching level-specific career teacher reform variables.

First, we pool the data over level l (grade 1–3, grade 4–6, grade 7–9) and subject b (Math, English, Swedish). In Figure 3.10, we plot θ_τ from the following dynamic model:

$$y_{stbl} = \sum_{\tau \neq -1} \theta_\tau D_{sbl} 1[\tau] + \delta_{sbl} + \lambda_{tbl} + \beta X_{st} + \varepsilon_{stbl} \tag{3.9}$$

where y_{stbl} is the standardized national exam result at school s in year t in subject b at level l. D_{sbl} is a dummy variable indicating whether the school has ever promoted a teacher at a particular level in a particular subject. τ denotes the time relative to when a teacher at school s in subject b at level l is first promoted, which occurs when τ equals 0. Observations three or more event years before treatment ($\tau \leq -3$) are grouped.[31] The effect of the career teacher, captured in θ_τ, is pooled over grades and subjects. Controls included in X_{st} are log number of students and student to teacher ratio (which both vary

[30]Note that Chetty et al. (2014) show that teachers who improve test scores improve students' high school completion, college attendance, and earnings, which supports that teachers' impact on students' test scores is a relevant outcome in this case.

[31]Because the reform was introduced in 2013 and we only have student test scores until the school year 2015/2016, τ can at most take value 2.

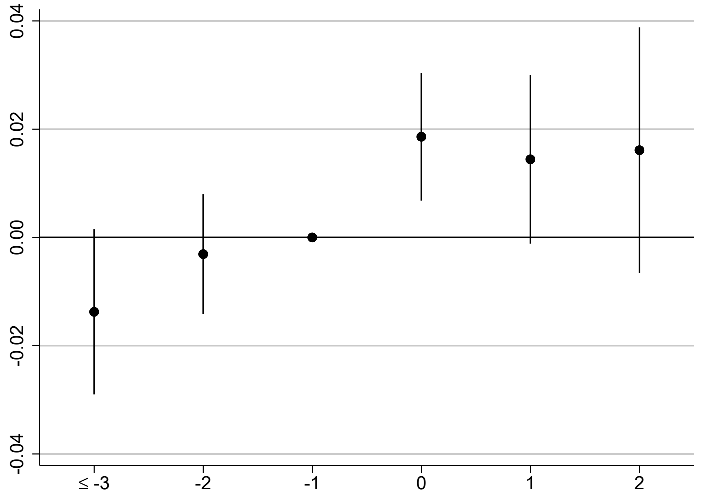


Figure 3.10. Dynamic response: Student performance

Note: The figure plots θ-coefficients with 95%-confidence intervals from estimating eq. 3.9.

by school and year). School by subject by level fixed effects, δ_{sbl}, as well as year by subject by level fixed effects, λ_{tbl}, are also included.[32]

When a school first hires a career teacher in a specific subject and level, the test scores in that subject and level increases with almost 2 percent of a standard deviation in comparison to the reference point the year prior to the career step. The effect is stable over time but only statistically significant at the 5%-level in the first year of participation ($\tau = 0$). We also see that the effect two years prior to participation ($\tau = -2$) is close to zero, which supports the notion that the timing of participation is not related to lagged school characteristics, including student performance. This is also in line with the results in Sections 3.3.1 and 3.5.1. The pre-effects three or more years before a school introduces the career step is negative but not statistically significant at the 5%-level.

Next we break out the results by level or by subject. In Table 3.8 we pool the estimates by either level l or subject b and report θ_q, for $q = b, l$, from the following equation:

$$y_{stbl} = \theta_q CT_{stbl} + \delta_{sbl} + \lambda_{tbl} + \beta X_{st} + \varepsilon_{stbl} \tag{3.10}$$

CT_{stbl} is the treatment variable. This is either an indicator for having at least one career teacher in year t at school s in specific subject b and level l, or the share of career teachers in a specific subject and level in year t among all the teachers in the same subject and level in $t - 1$ at the same school.[33]

[32] See Appendix Figures B.3 and B.4 for the corresponding event graphs by subject and by level.

[33] See Appendix B Table B.9 for the corresponding results for each subject and level separately. In Appendix C Table C.4 we show estimates obtained when we relate student performance to

In Panel A we present the results by teaching level. We see that participating in the reform improves test scores in grades 3 and 6 but not grade 9. In particular, having a career teacher in a specific subject in grade 3 (6) improves test scores with 1.9 (2.5) percent of a standard deviation. When exploiting the treatment intensity, we find that a ten percentage point increase in the share of career teachers in either math or Swedish in the lowest level of education increases test scores with on average 0.97 percent of a standard deviation. The corresponding number for the middle tier is 0.75 percent of a standard deviation. In Panel B, we instead present results by subject. When pooling over levels, we find positive effects for all subjects. The point estimates suggest slightly larger effects in math and Swedish than in English. In Appendix C Table C.3 we find that these results are robust to including controls for the Boosts for Mathematics and Boost for Reading reforms.[34]

A contributing factor to the lack of effect in the highest level of compulsory schooling (grade 7–9) could be that students have subject teachers rather than classroom teachers in the highest level of education, such that exposure to career teachers becomes more fragmented than at the low or middle levels. Pedagogical development projects may also become more specialized at higher tiers. Moreover, larger effects on student outcomes for younger children is consistent with that human capital interventions have higher returns in younger ages (Cunha and Heckman 2007; Heckman 2006).

To appreciate the size of these effects, consider a school with three parallel classes in the lowest level (grade 1–3) where one of the three class teachers (teaching both Swedish and Math) in each parallel class is promoted to career teacher. The results suggest that this would increase the school's grade 3 national exam scores in Swedish and Math with around 3 percent of a standard deviation.[35] As a comparison, it is instructive to note that Rockoff (2004) finds that raising teacher quality by one standard deviation translates into 0.10 standard deviation increase in student test scores; Fryer (2017) finds that 300 hours of principal training, including coaching and feedback to teachers, improves test scores by 0.10 standard deviations; and Fredriksson et al. (2012) find that a reduction in class size with 5 pupils increase test scores with 0.10 standard deviations.[36] In this respect, our results suggest a non-negligible impact of career teachers on student performance. At the same time, these effects are relatively small, for example, compared with that girls in the lowest level had

(i) the share and lead share of promoted teachers, (ii) the number of promoted teachers as a fraction of all teachers, or (iii) the presence of at least one career teacher, irrespective of subject and level.

[34]Since we only have student test scores until the school year 2015/2016, the Teachers' Salary Boost is not relevant as it was only introduced in 2016.

[35]In 2016, the share of career teachers teaching Swedish or Math in the lower tier at participating schools was 24 or 25 percent, respectively.

[36]Our results also resonate with the finding of Jackson and Bruegmann (2009) that about 20 percent of the teacher effectiveness is due to the influence from teachers' peers during the previous three years.

Table 3.8. *Student performance*

	(1)	(2)	(3)
Panel A: Pool across subject	Grade 3	Grade 6	Grade 9
Treat	0.019*	0.025***	0.005
	(0.010)	(0.008)	(0.008)
R^2	0.474	0.590	0.702
Share	0.097***	0.075***	0.027
	(0.028)	(0.018)	(0.020)
R^2	0.474	0.591	0.702
N	25,207	31,702	15,800
Year × subject FE:s	Yes	Yes	Yes
School × subject FE:s	Yes	Yes	Yes
School controls	Yes	Yes	Yes
Panel B: Pool across level	Math	English	Swedish
Treat	0.018**	0.015*	0.021***
	(0.008)	(0.008)	(0.007)
R^2	0.532	0.627	0.561
Share	0.084***	0.039**	0.076***
	(0.017)	(0.018)	(0.017)
R^2	0.533	0.627	0.561
N	28,428	15,831	28,450
Year × level FE:s	Yes	Yes	Yes
School × level FE:s	Yes	Yes	Yes
School controls	Yes	Yes	Yes

Note: The table presents the results of estimating eq. 3.10. In Panel A we pool the results across subjects. In Panel B we pool the results across levels. Treatment variables are "treat" (a dummy equal to one if there is at least one subject & level career teacher at the school) or "share" (the number of subject & level career teachers at the school in year t divided by the number subject & level teachers in the school in $t-1$). School controls are log number of students and student to teacher ratio. The results of the national exams are standardized by year, subject and level to have mean 0 and st.d. 1. Each school obtains a mean standardized score. Standard errors are clustered at school district level and reported in parenthesis.

0.10 standard deviations higher test scores in 2015 than boys did. Appendix Table B.10 shows that, unlike the teacher turnover response, which appeared to be stronger in school districts where the career teachers have more teaching development responsibilities, the impact on student outcomes is about the same irrespective of the type of career teacher.

3.6 Conclusion

Despite the widespread interest in the determinants of student outcomes among policy-makers and researchers, evidence on how policies aimed at improving the teaching pool impacts teachers and students remains scarce. One likely reason is the rigidity of the teacher labor market in many countries, which often prevents large-scale interventions. We contribute to this gap in the literature by analyzing the impact of a Swedish reform which introduced a new career step for teachers starting in 2013. The reform allowed schools to promote talented teachers to a new title ("career teacher") with a substantial associated wage increase financed entirely through ear-marked state funding. The reform intended to reward talented teachers, to increase the attractiveness of the profession through higher wage dispersion, and to take advantage of teachers' professional competence. While the allocation of the number of promotions across school districts was based on the number of students, the school districts had discretion over the allocation of promotions across schools and teachers within each district.

The paper provides evidence on the response to this reform, both in terms of the teachers selected for promotion and the impact it had on the wage structure, teacher separations and student performance. Our estimates capture the overall impact of the promotion program, which entails both higher pay and increased responsibilities for planning the pedagogical work and coaching other teachers. We show that the allocation of teacher promotions across schools is related to school size but unrelated to other pre-determined school characteristics such as teacher turnover rates. Within schools, high-wage teachers were more likely to be promoted. Our interpretation is that principals complied with the intentions of the reform and promoted the most talented teachers.

The reform induced significant changes in teacher pay: the stipulated wage increase had full pass-trough onto promoted teachers' wages and led to an increase in wage dispersion both within and across schools. This wage response is substantially larger than the documented impacts of just introducing more discretion over teacher pay, both in the US (Biasi 2018) and in Sweden (Willén 2019). The ear-marked funding and increased responsibilities for career teachers (making differential pay seem more acceptable) are probably key components behind the minimal crowd-out in our setting, highlighting the importance of policy-design to get the desired impact on the wage dispersion. Compared to schools that did not introduce the career step, we find that the promotion program led to a reduction in teacher separations and small changes in the teaching pool, despite very similar trajectories before the reform. These responses appear driven by school districts using their career teachers for teaching development responsibilities rather than coaching and mentoring tasks, suggesting that the pure salary increase cannot fully explain the turnover patterns.

It is important to highlight that our estimates capture relative differences in teacher turnover (and other outcomes) across schools, not aggregate effects on school quality. Nevertheless, the fact that we find a reduction in the fraction of teachers exiting the teaching profession in schools with career teachers suggests that promotions can incentivize teachers to stay in the profession. We also find non-negligible effects on student test scores in Math, English and Swedish in grades 3 and 6. Our results suggest that promoting one third of grade 1–3 teachers increases test scores at the grade 3 national tests by 3 percent of a standard deviation. As a comparison, previous studies suggest that these effects are about a third of the size of improving teacher quality by one standard deviation. Together, our results lend support to that performance-based promotions could be an important tool for raising school quality.

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Appendix A: Supplementary description

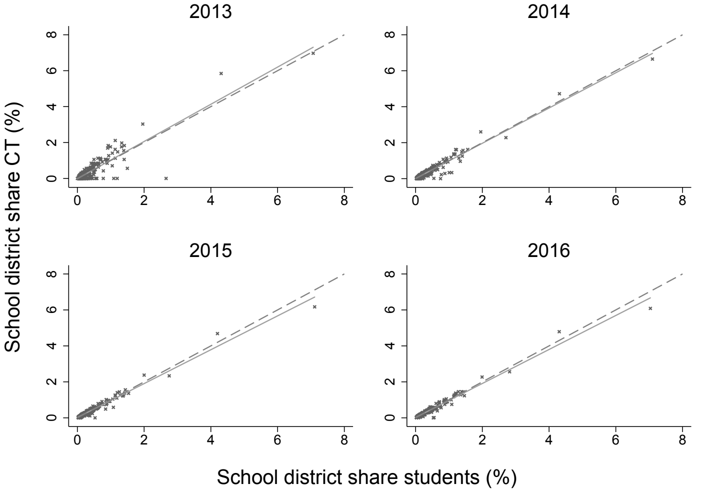


Figure A.1. Allocation of promotions across school districts - all school districts

Note: Each cross corresponds to one school district. The solid line is a linear prediction. The dashed line is the 45 degree line.

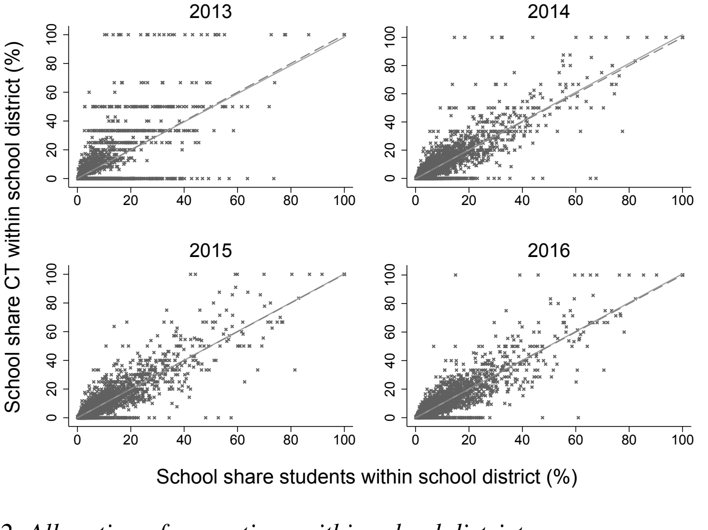


Figure A.2. Allocation of promotions within school districts

Note: Each cross corresponds to one school. The solid line is a linear prediction. The dashed line is the 45 degree line.

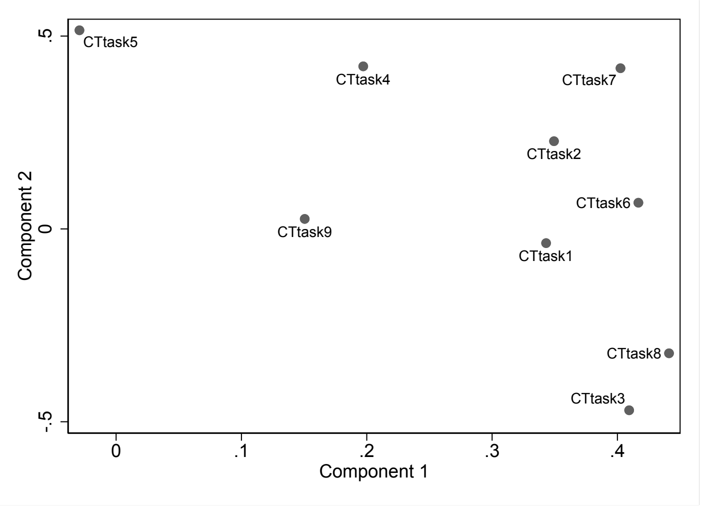


Figure A.3. Career teacher tasks: component loadings

Note: The figure shows the loadings of the different career teacher tasks displayed in Figure 3.2 on the two components. Career teacher tasks are defined as follows: CTtask1="Coaching other teachers colleagues", CTtask2="Initiate pedagogical discussions", CTtask3="Mentor teacher students", CTtask4="Support teaching methods", CTtask5="Engage in research", CTtask6="Subject development", CTtask7="Subject didactic", CTtask8="Mentor new hires", CTtask9="Select teaching materials". Component 1 is "Coaching and mentoring" while component 2 is "Teaching development".

Table A.1. *Determinants of teacher pay*

	(1)
	Log wages
Female	-0.007***
	(0.001)
Age:	
30-39	0.032***
	(0.001)
40-49	0.049***
	(0.002)
50-59	0.064***
	(0.002)
60 and over	0.079***
	(0.003)
Years of experience:	
4-9	0.042***
	(0.001)
10-14	0.081***
	(0.002)
15-24	0.116***
	(0.002)
25 and over	0.167***
	(0.003)
Years of tenure:	
2-5	-0.006***
	(0.001)
5-10	-0.004***
	(0.001)
10-15	0.001
	(0.002)
Level of education:	
Upper secondary	0.025***
	(0.009)
Post-secondary	0.073***
	(0.009)
Doctoral	0.096***
	(0.010)
Certification:	
Certified	0.026***
	(0.002)
Type of contract:	
Permanent	0.033***
	(0.001)

Table A.1. *Determinants of teacher pay – continued from previous page*

	(1)
	Log wages
Educational specialization:	
Swedish & social sciences	-0.009***
	(0.001)
Languages	0.017***
	(0.001)
Vocational	-0.006***
	(0.002)
Other teaching	0.009***
	(0.002)
Non-teaching	-0.005**
	(0.002)
Constant	9.932***
	(0.009)
R^2	0.676
N	167,604
Year FE:s	Yes
School District FE:s	Yes
Control mean	10.22

Note: The table shows the correlation between teacher observable characteristics and the monthly full-time wage. The period is restricted to the pre-reform period, 2010 to 2012. Omitted categories are "Under 30" (Age), "Under 4" (Experience), "Under 2 (Tenure), "Compulsory" (Level of education) and "Math and natural sciences" (Educational specialization). Educational specialization is inferred from the field of education according to the Swedish SUN classification. "Non-teaching" includes everyone without pedagogical specialization.

Table A.2. *School summary statistics*

	No CT		At least one CT	
	Mean	sd	Mean	sd
Year: 2013				
Nr career teachers	0.00	(0.00)	1.37	(0.68)
Nr teachers t-1	15.76	(11.26)	24.70	(13.03)
Nr students t-1	204.92	(140.32)	323.39	(169.24)
Student teacher ratio t-1	13.65	(3.27)	13.56	(3.04)
Share separate t-1	0.22	(0.15)	0.22	(0.12)
Share exit t-1	0.11	(0.10)	0.11	(0.08)
N	1,911		1,039	
Year: 2014				
Nr career teachers	0.00	(0.00)	2.23	(1.49)
Nr teachers t-1	10.64	(8.35)	18.31	(11.45)
Nr students t-1	145.42	(111.07)	243.70	(147.15)
Student teacher ratio t-1	14.36	(4.41)	13.95	(3.80)
Share separate t-1	0.23	(0.17)	0.23	(0.15)
Share exit t-1	0.11	(0.12)	0.11	(0.10)
N	673		1,238	
Year: 2015				
Nr career teachers	0.00	(0.00)	1.56	(0.88)
Nr teachers t-1	8.89	(7.04)	13.48	(9.01)
Nr students t-1	125.29	(104.26)	187.20	(121.69)
Student teacher ratio t-1	14.64	(4.43)	14.53	(3.77)
Share separate t-1	0.24	(0.18)	0.23	(0.15)
Share exit t-1	0.11	(0.13)	0.11	(0.11)
N	418		255	
Year: 2016				
Nr career teachers	0.00	(0.00)	1.40	(0.70)
Nr teachers t-1	7.96	(6.36)	11.41	(7.17)
Nr students t-1	117.74	(109.16)	161.63	(95.47)
Student teacher ratio t-1	14.89	(4.37)	14.81	(4.04)
Share separate t-1	0.26	(0.20)	0.24	(0.17)
Share exit t-1	0.12	(0.14)	0.11	(0.11)
N	314		104	

Note: In each year, we only include schools that have not (never or yet) participated as well as schools that participate for the first time.

Table A.3. *School district summary statistics*

	In survey or not		By career teacher design	
	Not in survey	In survey	Coaching and mentoring	Teaching development
Mean teacher wage	10.28	10.28	10.29	10.28
Separation rate	0.23	0.24	0.24	0.23
Exit rate	0.11	0.12	0.11	0.11
Certified	0.92	0.91	0.91	0.91
Experienced	0.88	0.87	0.87	0.87
Teacher 9th grade GPA	0.56	0.56	0.56	0.57
Urban	0.40	0.34	0.38	0.27
Nr students per school	232	235	252	230
Nr teachers per school	18.24	18.17	19.04	17.99
Student-to-teacher ratio per school	13.55	13.61	14.04	13.62
Nr career teachers per school	1.17	1.17	1.29	1.11
N	198	92	48	49

Note: The table shows district summary statistics. Column 1 and 2 show districts broken down by whether they are included in the survey sample or not. Column 3 and 4 show summary statistics separately for districts with a coaching and mentoring CT design and a teaching development design. The construction of these groups is described in Section 3.5.3. Experienced is the fraction of teachers with at least four years of teaching experience.

Appendix B: Additional results

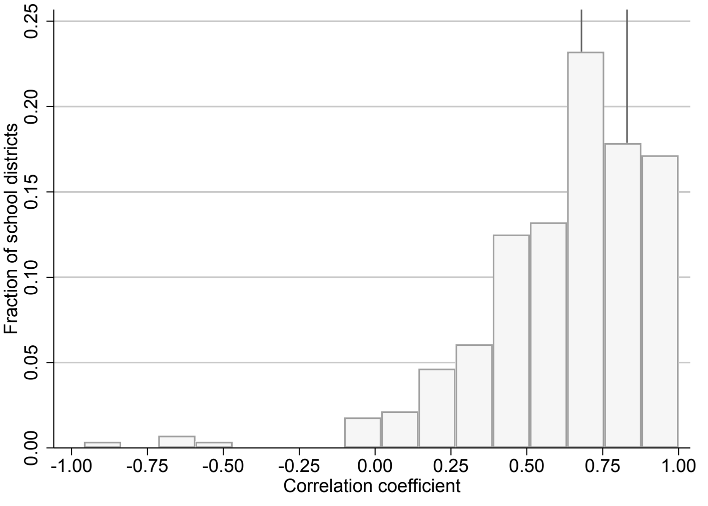
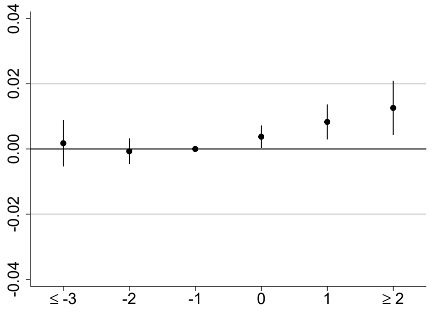
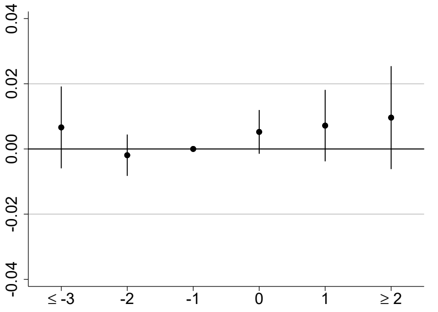


Figure B.1. Distribution of correlation coefficients between share career teachers and share students

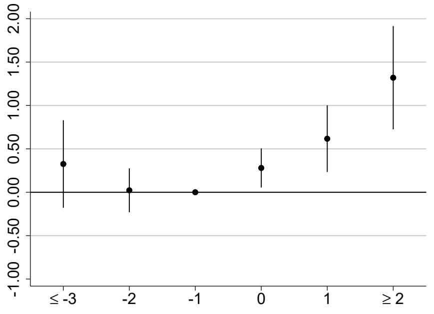
Note: The figure plots the distribution of correlation coefficients between the share of career teachers and share of students within school districts. The two lines mark the 50th and 75th percentiles. A correlation coefficient closer to 1 identifies school districts that appear to allocate career teaching positions according to the same rule used to allocate positions across school districts on the national level. Only school districts that at some point participate in the reform and that have more than one school are included. The figure is based on 280 school districts.



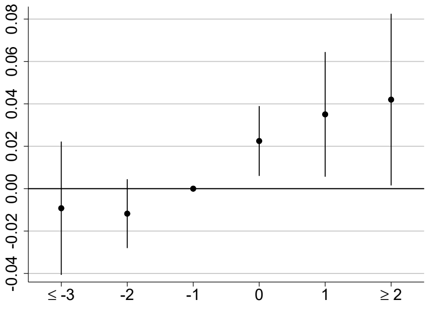
(a) Share certified



(b) Share experienced



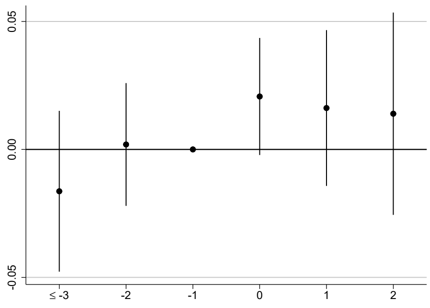
(c) Median experience



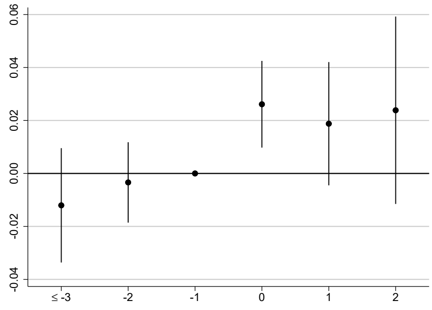
(d) Teacher grades

Figure B.2. Dynamic responses: teacher composition

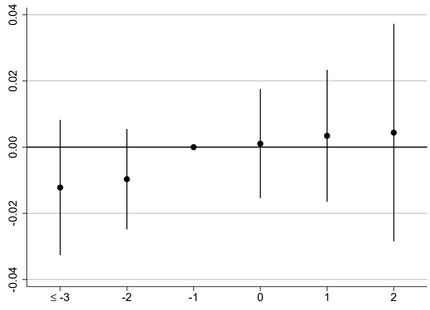
Note: The figure plots γ-coefficients with 95%-confidence intervals from estimating eq. 3.7. It shows the evolution of the share of certified teachers, the share of experienced teachers, the median level of experience at the school, and the average grades among teachers within schools before and after promoting at least one career teacher. $\tau = -1$ is omitted.



(a) Grade 3



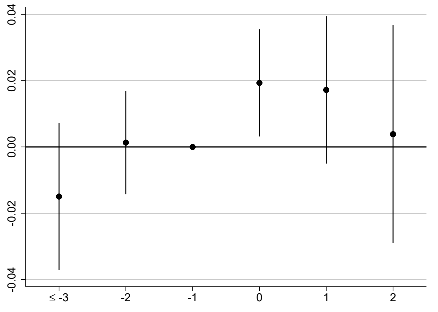
(b) Grade 6



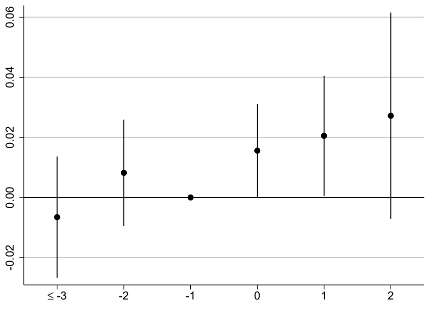
(c) Grade 9

Figure B.3. Dynamic responses: Student performance – pool across subject

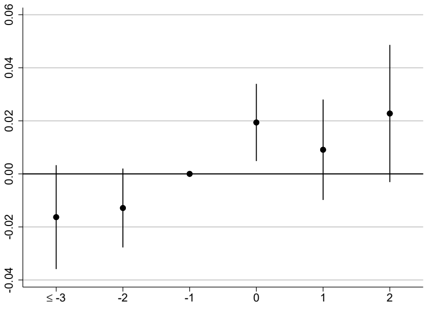
Note: The figure plots the θ-coefficients with 95%-confidence intervals from estimating eq. 3.9, where we only pool across subject.



(a) Mathematics



(b) English



(c) Swedish

Figure B.4. Dynamic responses: Student performance – pool across level

Note: The figure plots the θ-coefficients with 95%-confidence intervals from estimating eq. 3.9, where we only pool across level.

Table B.1. *Factors that predict selection of schools – Share CT*

	Share CT at school			
	2013	2014	2015	2016
School characteristics $t-1$:				
Log nr students	0.001	0.008**	0.000	-0.005
	(0.002)	(0.004)	(0.006)	(0.009)
Student-to-teacher ratio	0.000	-0.000	0.000	0.001
	(0.001)	(0.001)	(0.001)	(0.001)
Separation rate	-0.000	-0.022	0.014	0.009
	(0.010)	(0.022)	(0.037)	(0.048)
Exit rate	-0.003	0.039	0.021	-0.046
	(0.015)	(0.027)	(0.045)	(0.045)
Certified (share)	0.014	0.036	0.078*	-0.029
	(0.015)	(0.034)	(0.044)	(0.041)
Female (share)	0.006	0.022	-0.030	-0.006
	(0.008)	(0.023)	(0.034)	(0.034)
Mean age (years)	-0.001*	-0.001	-0.001	0.001
	(0.001)	(0.001)	(0.001)	(0.002)
Mean experience (years)	0.000	0.000	0.001	-0.001
	(0.001)	(0.001)	(0.002)	(0.001)
Math/natural science (share)	-0.008	-0.003	-0.056*	0.018
	(0.010)	(0.027)	(0.030)	(0.034)
Swedish/social science (share)	0.004	0.024	-0.010	0.068**
	(0.009)	(0.020)	(0.032)	(0.029)
District FE	Yes	Yes	Yes	Yes
R^2	0.183	0.253	0.404	0.458
N	2,941	1,882	611	346
Mean dep. var.	.02	.09	.05	.04

Note: This presents the results of regressions of $CT_{sdt} = \phi_{dt} + \beta_t X_{sdt-1} + \varepsilon_{sdt}$ where CT_{sdt} is a variable equal to the share of career teachers at the school in year t. Variables are measured at school level. Regressions are estimated separately by year and only include schools that have not (never or yet) participated as well as schools that participate for the first time. Standard errors are clustered at school district level and reported in parenthesis.

Table B.2. *Factors that predict selection of schools – with student test scores*

	At least one CT at school				Share CT at school			
	2013	2014	2015	2016	2013	2014	2015	2016
Panel A: 3rd grade test results								
Log nr students	0.228***	0.302***	0.234***	0.144***	0.001	0.008*	0.002	-0.009
	(0.021)	(0.018)	(0.046)	(0.046)	(0.002)	(0.004)	(0.007)	(0.009)
Student-to-teacher ratio	-0.009***	-0.010***	0.000	-0.005	0.000	-0.000	0.001	0.001
	(0.003)	(0.003)	(0.007)	(0.006)	(0.001)	(0.001)	(0.001)	(0.001)
Separation rate	0.009	-0.068	0.003	0.252	0.004	-0.024	0.005	0.022
	(0.088)	(0.095)	(0.217)	(0.222)	(0.010)	(0.023)	(0.044)	(0.054)
Exit rate	-0.084	0.213	0.051	-0.312	-0.008	0.054*	0.026	-0.059
	(0.110)	(0.133)	(0.232)	(0.230)	(0.015)	(0.029)	(0.048)	(0.052)
Certified (share)	-0.109	0.192	0.134	-0.308	0.003	0.019	0.076*	-0.039
	(0.172)	(0.159)	(0.249)	(0.214)	(0.017)	(0.041)	(0.046)	(0.042)
Female (share)	-0.029	0.270**	-0.129	0.047	0.009	0.047*	-0.028	-0.027
	(0.087)	(0.106)	(0.172)	(0.186)	(0.009)	(0.025)	(0.039)	(0.036)
Mean age (years)	-0.007	-0.006	-0.002	0.011	-0.001	-0.001	-0.001	0.001
	(0.005)	(0.005)	(0.007)	(0.008)	(0.001)	(0.001)	(0.001)	(0.002)
Mean experience (years)	0.003	0.001	0.002	-0.010	0.000	0.000	0.001	-0.002
	(0.005)	(0.005)	(0.008)	(0.008)	(0.001)	(0.001)	(0.002)	(0.002)
Math/natural science (share)	0.068	-0.072	-0.244	-0.083	-0.007	0.003	-0.068**	0.003
	(0.081)	(0.115)	(0.153)	(0.170)	(0.011)	(0.029)	(0.033)	(0.034)
Swedish/soc. science (share)	0.133**	-0.009	-0.109	0.205	0.009	0.041*	-0.015	0.063*
	(0.066)	(0.091)	(0.145)	(0.161)	(0.010)	(0.021)	(0.036)	(0.032)

Table B.2. *Factors that predict selection of schools – with student test scores – continued from previous page*

	At least one CT at school				Share CT at school			
	2013	2014	2015	2016	2013	2014	2015	2016
3rd grade math score	-0.008	-0.047	0.020	0.081	-0.000	-0.002	0.012	0.009
	(0.021)	(0.029)	(0.043)	(0.069)	(0.003)	(0.006)	(0.009)	(0.014)
3rd grade Swedish score	0.013	0.045	-0.039	-0.029	0.006	0.012*	-0.017	0.013
	(0.031)	(0.029)	(0.060)	(0.076)	(0.004)	(0.006)	(0.013)	(0.015)
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.360	0.415	0.481	0.586	0.194	0.272	0.417	0.489
N	2,509	1,635	543	307	2,509	1,635	543	307
Mean dep. var.	.34	.63	.37	.24	.02	.09	.05	.04
Panel B: 6th grade test results								
Log nr students	0.210***	0.298***	0.255***	0.103*	-0.001	0.010**	0.007	-0.007
	(0.022)	(0.019)	(0.051)	(0.052)	(0.002)	(0.005)	(0.008)	(0.012)
Student-to-teacher ratio	-0.009**	-0.008***	-0.002	-0.008	0.000	0.000	0.000	0.000
	(0.004)	(0.003)	(0.008)	(0.008)	(0.001)	(0.001)	(0.001)	(0.001)
Separation rate	0.072	-0.197	0.329	0.551**	0.001	-0.044	0.070	0.066
	(0.081)	(0.120)	(0.241)	(0.233)	(0.011)	(0.029)	(0.046)	(0.052)
Exit rate	-0.107	0.366**	-0.484	-0.615**	-0.010	0.074**	-0.091	-0.119***
	(0.125)	(0.156)	(0.298)	(0.254)	(0.016)	(0.034)	(0.056)	(0.044)
Certified (share)	-0.031	0.181	0.020	-0.452*	0.003	0.030	0.034	-0.081
	(0.162)	(0.166)	(0.292)	(0.248)	(0.016)	(0.041)	(0.058)	(0.053)

Table B.2. *Factors that predict selection of schools – with student test scores – continued from previous page*

	At least one CT at school				Share CT at school			
	2013	2014	2015	2016	2013	2014	2015	2016
Female (share)	-0.024	0.127	-0.207	0.093	0.002	0.042	-0.042	-0.007
	(0.087)	(0.120)	(0.189)	(0.202)	(0.010)	(0.028)	(0.040)	(0.037)
Mean age (years)	-0.011**	-0.007	0.005	0.018*	-0.001**	-0.002*	0.001	0.004**
	(0.005)	(0.006)	(0.008)	(0.009)	(0.001)	(0.001)	(0.002)	(0.002)
Mean experience (years)	0.004	0.001	0.009	-0.007	0.000	0.001	0.002	-0.002
	(0.005)	(0.006)	(0.009)	(0.010)	(0.001)	(0.001)	(0.002)	(0.002)
Math/natural science (share)	-0.091	-0.142	-0.247	-0.182	-0.020	-0.023	-0.047	-0.019
	(0.101)	(0.132)	(0.197)	(0.260)	(0.015)	(0.034)	(0.040)	(0.046)
Swedish/soc. science (share)	0.140*	-0.055	0.333*	-0.054	0.006	0.029	0.096**	0.036
	(0.078)	(0.119)	(0.177)	(0.216)	(0.010)	(0.026)	(0.041)	(0.042)
6th grade math score	-0.035	-0.001	0.122	-0.025	-0.005	0.009	0.011	0.002
	(0.033)	(0.047)	(0.074)	(0.068)	(0.004)	(0.010)	(0.013)	(0.015)
6th grade English score	0.007	0.006	-0.072	0.001	-0.002	-0.004	-0.008	-0.003
	(0.036)	(0.046)	(0.079)	(0.074)	(0.005)	(0.010)	(0.015)	(0.017)
6th grade Swedish score	0.015	-0.028	-0.117	0.046	0.006	-0.009	-0.006	0.001
	(0.029)	(0.047)	(0.084)	(0.085)	(0.004)	(0.010)	(0.015)	(0.019)
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.397	0.450	0.585	0.629	0.217	0.287	0.504	0.555
N	2,041	1,280	390	206	2,041	1,280	390	206
Mean dep. var.	.38	.65	.40	.23	.03	.09	.05	.03

Note: This presents results of estimating $CT_{sdt} = \phi_{dt} + \beta_t X_{sdt-1} + \varepsilon_{sdt}$ where CT_{sdt} is equal to 1 if the school participates in the reform in year t (columns 1–4), or the share of career teachers at the school in year t (columns 5–8). Variables are measured at school level. Regressions are estimated separately by year and only include schools that have not (never or yet) participated and schools that participate for the first time. Standard errors are clustered by school district.

Table B.3. *Selection of teachers for promotion*

	(1)	(2)	(3)
Female	0.036***	0.024***	0.010
	(0.004)	(0.004)	(0.008)
Permanent contract		0.024***	0.035***
		(0.005)	(0.008)
Certified		0.041***	0.058***
		(0.006)	(0.011)
Age			
30-39	0.029***	-0.003	-0.017*
	(0.008)	(0.008)	(0.010)
40-49	-0.003	-0.048***	-0.046***
	(0.008)	(0.009)	(0.015)
50-59	-0.109***	-0.156***	
	(0.009)	(0.009)	
60+	-0.208***	-0.264***	
	(0.009)	(0.010)	
Experience			
4-9	0.105***	0.033***	0.011
	(0.006)	(0.006)	(0.009)
10-14	0.174***	0.038***	-0.000
	(0.006)	(0.007)	(0.014)
15-24	0.178***	0.010	-0.036
	(0.007)	(0.010)	(0.024)
25+	0.181***	-0.030***	
	(0.007)	(0.010)	
Tenure			
2-5		0.026***	0.039***
		(0.005)	(0.010)
5-10		0.048***	0.071***
		(0.006)	(0.011)
10-15		0.054***	0.090***
		(0.006)	(0.012)
Level of education			
Upper secondary		-0.051***	-0.065
		(0.017)	(0.045)
Post-secondary		-0.079***	-0.082*
		(0.018)	(0.048)
Doctoral		-0.125***	-0.018
		(0.030)	(0.125)
Educational specialization			
Swedish and social sciences		-0.022***	-0.015
		(0.007)	(0.010)
Languages		-0.039***	-0.042***
		(0.008)	(0.015)
Vocational		-0.103***	-0.106***
		(0.006)	(0.014)
Other teaching		-0.070***	-0.050***
		(0.005)	(0.009)

Table B.3. *Selection of teachers for promotion – continued from previous page*

	(1)	(2)	(3)
Non-teaching		-0.072***	-0.062***
		(0.008)	(0.015)
School wage decile			
2nd wage decile		0.010*	0.014*
		(0.005)	(0.009)
3rd wage decile		0.039***	0.049***
		(0.007)	(0.011)
4th wage decile		0.070***	0.083***
		(0.007)	(0.012)
5th wage decile		0.102***	0.125***
		(0.007)	(0.013)
6th wage decile		0.137***	0.161***
		(0.008)	(0.015)
7th wage decile		0.171***	0.220***
		(0.010)	(0.021)
8th wage decile		0.207***	0.291***
		(0.009)	(0.024)
9th wage decile		0.230***	0.289***
		(0.011)	(0.027)
10th wage decile		0.264***	0.374***
		(0.010)	(0.036)
Teacher 9th grade GPA			
-1 to -0.5			-0.015
			(0.028)
-0.5 to 0			-0.021
			(0.026)
0 to 0.5			-0.005
			(0.026)
0.5 to 1			0.001
			(0.025)
1+			0.013
			(0.024)
Constant	0.026***	0.108***	0.082*
	(0.007)	(0.021)	(0.044)
School FE	Yes	Yes	Yes
R^2	0.101	0.142	0.240
N	55,785	55,658	17,881

Note: This shows the results of estimating $EverCT_{ist} = \beta X_{ist} + \delta_s + \varepsilon_{ist}$, using data only from 2012. Omitted categories are "Under 30" (Age), "Under 4" (Experience), "Under 2 (Tenure), "Compulsory" (Level of education), "Math and natural sciences" (Educational specialization), "1" (Wage decile), and "Less than -1" (Teacher GPA). "Non-teaching" includes everyone without pedagogical specialization. $EverCT_{ist}$ is an indicator equal to 1 if the teacher is ever a career teacher. As we only have data on teacher's 9th grade GPA from 1988 onward, the sample size that includes this variable is smaller than in the other specifications. Standard errors are clustered at school district level and included in parenthesis.

Table B.4. *Wage effects of promotion*

Sample	(1) Full	(2) Full	(3) Full	(4) Full	(5) Full	(6) Full	(7) First time CT	(8) First time CT
Panel A: ln*(wage)*								
Promoted	0.212***	0.210***	0.179***	0.179***	0.179***	0.149***	0.141***	
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	
2012 mean wage	10.22							
R^2	0.404	0.512	0.800	0.796	0.807	0.958	0.904	
Panel B: Monthly wage (SEK)								
Promoted	7176.4***	7104.4***	6253.5***	6231.7***	6243.3***	5329.2***	4744.3***	5315.1***
	(90.5)	(84.9)	(73.2)	(74.4)	(73.9)	(65.4)	(58.6)	(56.6)
2012 mean wage	27501.3							
R^2	0.438	0.542	0.799	0.795	0.808	0.954	0.900	0.949
Year FE	Yes	Yes	Yes		Yes	Yes		Yes
School FE		Yes	Yes	Yes	Yes			
Individual FE						Yes		Yes
Year × district FE					Yes			
Linear & quadratic time trends				Yes				
Controls			Yes	Yes	Yes	Yes	Yes incl. lag wage	Yes
N	395,960	395,960	395,083	395,083	395,083	374,108	260,619	360,260

Note: This provides the results of estimating $y_{ist} = \alpha_i + \delta_s + \lambda_t + \theta CT_{ist} + \beta X_{ist} + \varepsilon_{ist}$. Controls are dummies for female, teacher certification, permanent contract, age (in five age bands), level of education (in four categories), educational specialization (in one of six categories), experience (in five bands) and tenure (in five bands). Standard errors are clustered at school district level and included in parenthesis. In specification (7) and (8) the sample is censored to only include the first year of becoming a career teacher as well as those that are not (yet) promoted.

Table B.5. *Factors that predict timing of participation*

	(1) 2013	(2) 2014	(3) 2015
School characteristics $t-1$:			
Log nr students	-0.358***	-0.216***	-0.078
	(0.032)	(0.025)	(0.051)
Student-to-teacher ratio	0.016***	0.012**	0.003
	(0.006)	(0.005)	(0.009)
Separation rate	0.008	-0.079	0.015
	(0.158)	(0.140)	(0.301)
Exit rate	0.009	-0.107	-0.188
	(0.198)	(0.180)	(0.324)
Certified (share)	-0.020	-0.367	-0.225
	(0.249)	(0.243)	(0.414)
Female (share)	0.036	-0.164	0.045
	(0.139)	(0.142)	(0.216)
Mean age (years)	0.022**	0.011	-0.002
	(0.009)	(0.007)	(0.012)
Mean experience (years)	-0.009	0.000	0.000
	(0.008)	(0.007)	(0.011)
Math/natural science (share)	0.002	0.037	0.166
	(0.168)	(0.163)	(0.216)
Swedish/social science (share)	0.082	0.205*	0.118
	(0.118)	(0.111)	(0.253)
District FE	Yes	Yes	Yes
R^2	0.362	0.366	0.536
N	2,620	1,562	297

Note: This presents results of the regressions $Year_{sd} = \phi_{dt} + \beta_t X_{sdt-1} + \varepsilon_{sdt}$ where $Year_{sd}$ is equal to the year that the school first has promoted a career teacher. Regressions are estimated separately by year, as indicated by the column headings. Regressions only include schools that at some point participate in the reform between 2013 and 2016, and that at the earliest participate in the year indicated by the column heading. Columns (2) and (3) therefore includes schools that had not yet promoted teachers earlier than 2014 (col. 2) or 2015 (col. 3). Standard errors are clustered at school district level.

Table B.6. *Factors that predict timing of participation – with student test scores*

	(1) 2013	(2) 2013	(3) 2014	(4) 2014	(5) 2015	(6) 2015
Log nr students	-0.383***	-0.360***	-0.244***	-0.239***	-0.094	-0.163***
	(0.033)	(0.039)	(0.030)	(0.035)	(0.057)	(0.060)
Student/teacher	0.016**	0.012	0.010**	0.005	0.000	0.009
	(0.006)	(0.008)	(0.005)	(0.005)	(0.009)	(0.013)
Separation rate	0.063	-0.080	-0.013	0.121	-0.072	-0.680*
	(0.172)	(0.172)	(0.151)	(0.197)	(0.327)	(0.373)
Exit rate	-0.017	0.074	-0.231	-0.348	-0.026	0.570
	(0.214)	(0.238)	(0.207)	(0.229)	(0.330)	(0.463)
Certified	0.234	-0.009	-0.309	-0.168	-0.082	-0.249
	(0.262)	(0.276)	(0.249)	(0.263)	(0.506)	(0.568)
Female	-0.101	0.114	-0.225	-0.039	-0.079	0.396
	(0.178)	(0.179)	(0.175)	(0.185)	(0.274)	(0.243)
Mean age	0.021**	0.031***	0.014*	0.018*	0.005	0.019
	(0.009)	(0.009)	(0.008)	(0.010)	(0.012)	(0.013)
Mean exp.	-0.012	-0.017*	-0.003	-0.009	-0.006	-0.025*
	(0.009)	(0.009)	(0.008)	(0.010)	(0.011)	(0.014)
Math/nat. sci.	-0.081	0.213	0.017	0.182	0.229	0.710**
	(0.175)	(0.215)	(0.172)	(0.204)	(0.239)	(0.273)
Swe./soc. sci.	-0.044	0.031	0.093	0.174	0.063	-0.238
	(0.128)	(0.165)	(0.118)	(0.160)	(0.271)	(0.232)
3rd grade math	0.006		0.096**		-0.029	
	(0.043)		(0.040)		(0.067)	
3rd grade Swe.	0.017		-0.062		0.073	
	(0.057)		(0.043)		(0.091)	
6th grade math		0.140**		-0.001		-0.182*
		(0.062)		(0.067)		(0.100)
6th grade Eng.		-0.039		-0.029		0.144*
		(0.066)		(0.067)		(0.083)
6th grade Swe.		-0.080		0.005		0.062
		(0.054)		(0.067)		(0.116)
District FE	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.381	0.420	0.371	0.418	0.567	0.669
N	2,205	1,828	1,332	1,060	254	185

Note: This presents results of the regressions $Year_{sd} = \phi_{dt} + \beta_t X_{sdt-1} + \varepsilon_{sdt}$ where $Year_{sd}$ is equal to the year that the school first has promoted a career teacher. Regressions are estimated separately by year, as indicated by the column headings. Regressions only include schools that at some point participate in the reform between 2013 and 2016, and that at the earliest participate in the year indicated by the column heading. Standard errors are clustered at school district level and included in parenthesis.

Table B.7. *Heterogeneous effects w.r.t teacher and district characteristics*

	(1)	(2)	(3)	(4)
	Log wages	Wage dispersion	Separations	Exits
Panel A: By CT type				
Coaching and mentoring	0.020***	0.003***	-0.004	0.003
	(0.004)	(0.000)	(0.012)	(0.007)
R^2	0.913	0.578	0.369	0.261
N	3,498	3,498	3,498	3,498
Teaching development	0.021***	0.004***	-0.020	-0.018**
	(0.004)	(0.001)	(0.014)	(0.008)
R^2	0.897	0.538	0.357	0.234
N	2,442	2,442	2,442	2,442
Panel B: By teacher seniority				
Senior teachers	0.022***	0.004***	-0.009*	-0.004
	(0.001)	(0.000)	(0.005)	(0.003)
R^2	0.929	0.552	0.301	0.206
N	17,682	17,622	17,677	17,677
Junior teachers	0.000	0.000	-0.001	0.003
	(0.002)	(0.000)	(0.013)	(0.011)
R^2	0.798	0.426	0.265	0.240
N	14,709	10,905	12,970	12,970
Panel C: By district location				
Urban areas	0.019***	0.004***	-0.017**	-0.007
	(0.002)	(0.000)	(0.007)	(0.004)
R^2	0.926	0.589	0.354	0.240
N	10,874	10,874	10,874	10,874
Rural areas	0.020***	0.004***	-0.001	-0.001
	(0.002)	(0.000)	(0.007)	(0.005)
R^2	0.890	0.572	0.318	0.235
N	6,815	6,815	6,815	6,815
Panel D: Districts following decision rule				
50th percentile and above	0.023***	0.004***	-0.014	-0.006
	(0.002)	(0.000)	(0.008)	(0.005)
R^2	0.904	0.572	0.341	0.238
N	6,655	6,655	6,655	6,655

Table B.7. *Heterogeneous effects w.r.t teacher and district characteristics – continued from previous page*

	(1)	(2)	(3)	(4)
	Log wages	Wage dispersion	Separations	Exits
75th percentile and above	0.022***	0.004***	-0.014	-0.012
	(0.003)	(0.001)	(0.012)	(0.009)
R^2	0.880	0.569	0.320	0.239
N	2,237	2,237	2,237	2,237

Note: In the table, we relate the change in the presence of career teachers in a school in year *t* to the change in mean wages (col. 1), wage dispersion (col. 2), school separations (col 3) and exits (col 4). School controls are the student to teacher ratio and log number of students. We include year and school FE in all regressions. In Panel A we divide by the school district design of the career teachers. Career teacher types are derived from the career teacher tasks displayed in Figure 3.2 using principal components analysis resulting in 48 school districts adopting the Coaching and mentoring type and 49 school districts adopting the Teaching development type. Figure A.3 shows the component loadings. Panel B displays separate regressions by teacher seniority status where senior teachers are those with at least five years of teaching experience. In Panel C, urbanization is defined using Eurostat's degree of urbanization (degurba) variable. School districts that are in cities (code 1) or towns and suburbs (code 2) are treated as urban, while school districts that are in rural (code 3) areas are treated as rural. There are 111 urban school districts and 179 rural school districts. In Panel D, we run regressions separately by whether school districts follow a rule to allocate promotions. The rule considered is whether school districts allocate career teaching positions in proportion to the share of students at the school within the school district (i.e. whether they apply the national rule in the school district). The 50th percentile correlation coefficient is 0.683 (140 school districts) and the 75th percentile is 0.829 (70 school districts).

Table B.8. *Log wages among non-promoted teachers in participating vs. non-participating schools*

	(1)	(2)
Panel A:		
At least one CT at school	-0.002*	0.001
	(0.001)	(0.001)
R^2	0.366	0.869
N	316,290	252,083
Panel B:		
Share CT at school	-0.001	0.044***
	(0.008)	(0.007)
R^2	0.366	0.869
N	316,290	252,083
Year FE:s	Yes	Yes
School FE:s	Yes	Yes
School controls	Yes	Yes
Teacher controls		Yes
Control mean	10.231	10.243

Note: The table shows estimates from equation 3.8 where the dependent variable is the log monthly full-time non-CT teaching wage. That is, we relate the change in the presence of career teachers within a school in year t to the log wage in year t in the sample of non-CT teachers. School controls included are the student to teacher ratio and log number of students. Lagged teacher controls are gender, age band, experience band, tenure band, level of education, educational specialization, wage decile, and whether the teacher is certified. Standard errors are clustered at school district level and reported in parenthesis.

Table B.9. *Student performance – detailed*

	Grade 3		Grade 6			Grade 9		
	Math	Swedish	Math	English	Swedish	Math	English	Swedish
Panel A								
Treat	0.015	0.023**	0.035***	0.018*	0.022*	-0.009	0.009	0.015
	(0.014)	(0.010)	(0.012)	(0.010)	(0.012)	(0.013)	(0.011)	(0.012)
R^2	0.465	0.489	0.596	0.585	0.583	0.681	0.727	0.697
N	12,599	12,608	10,566	10,564	10,572	5,263	5,267	5,270
Panel B								
Share	0.097***	0.097***	0.109***	0.044**	0.075***	0.005	0.025	0.051*
	(0.037)	(0.028)	(0.027)	(0.022)	(0.027)	(0.028)	(0.032)	(0.028)
R^2	0.465	0.489	0.597	0.585	0.583	0.681	0.727	0.697
N	12,599	12,608	10,566	10,564	10,572	5,263	5,267	5,270
Year FE:s	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
School FE:s	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
School controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: In the table we regress the standardized national exam result on subject & level-specific reform variables. In Panel A we use the treatment variable "treat" (a dummy equal to one if there is at least one subject & level career teacher at the school). In Panel B we use the treatment variable "share" (the number of subject & level career teachers at the school in year t divided by the number subject & level teachers in the school in $t-1$). School controls are log number of students and student to teacher ratio. The results of the national exams are standardized by year, subject and level to have mean 0 and st.d. 1. Each school obtains a mean standardized score. Standard errors are clustered by school district.

Table B.10. *Student performance by career teacher type*

	(1)	(2)	(3)
Panel A: Pool across subject	Grade 3	Grade 6	Grade 9
Coaching and mentoring CT	0.034	0.028*	0.012
	(0.021)	(0.016)	(0.017)
R^2	0.490	0.632	0.729
N	4,936	6,262	3,029
Development work CT	0.026	0.028	0.009
	(0.025)	(0.025)	(0.023)
R^2	0.424	0.497	0.696
N	3,343	4,282	2,199
Year × subject FE:s	Yes	Yes	Yes
School × subject FE:s	Yes	Yes	Yes
School controls	Yes	Yes	Yes
Panel B: Pool across level	Math	English	Swedish
Coaching and mentoring CT	0.032*	0.009	0.030**
	(0.017)	(0.017)	(0.012)
R^2	0.535	0.687	0.623
N	5,564	3,096	5,567
Development work CT	0.025	0.015	0.025
	(0.022)	(0.021)	(0.016)
R^2	0.472	0.560	0.497
N	3,831	2,160	3,833
Year × level FE:s	Yes	Yes	Yes
School × level FE:s	Yes	Yes	Yes
School controls	Yes	Yes	Yes

Note: The table presents the results of estimating eq. 3.10. In Panel A we pool the results across subjects. In Panel B we pool the results across levels. Treatment variables are "treat" (a dummy equal to one if there is at least one subject & level career teacher at the school), and the results are run separately for Coaching and mentoring CTs and Development work CTs. School controls are log number of students and student to teacher ratio. Standard errors are clustered at school district level and reported in parenthesis.

Appendix C: Robustness checks

Table C.1. *Sensitivity checks – Participating schools*

	(1)	(2)	(3)	(4)
	Log wages	Wage disp.	Separations	Exits
Panel A: Baseline results				
At least one CT	0.019***	0.004***	-0.010*	-0.004
	(0.001)	(0.000)	(0.005)	(0.003)
R^2	0.914	0.583	0.339	0.238
N	17,689	17,689	17,689	17,689
Panel B: No school controls				
At least one CT	0.019***	0.004***	-0.010**	-0.003
	(0.001)	(0.000)	(0.005)	(0.003)
R^2	0.914	0.582	0.311	0.230
N	17,700	17,700	17,700	17,700
Panel C: Eligible teachers				
At least one CT	0.022***	0.004***	-0.011**	-0.004
	(0.001)	(0.000)	(0.005)	(0.003)
R^2	0.930	0.560	0.304	0.212
N	17,684	17,628	17,684	17,684
Panel D: Weighted				
At least one CT	0.013***	0.003***	-0.009**	-0.003
	(0.001)	(0.000)	(0.004)	(0.003)
R^2	0.936	0.641	0.377	0.249
N	17,689	17,689	17,689	17,689
Panel E: Excl. never participating schools				
At least one CT	0.015***	0.003***	-0.009*	-0.002
	(0.001)	(0.000)	(0.005)	(0.004)
R^2	0.921	0.588	0.342	0.241
N	15,808	15,808	15,808	15,808
Panel F: With boost controls				
At least one CT	0.019***	0.004***	-0.009*	-0.004
	(0.001)	(0.000)	(0.005)	(0.003)
R^2	0.915	0.583	0.340	0.239
N	17,689	17,689	17,689	17,689

Note: We relate the change in the presence of career teachers in a school to the change in mean wages, wage dispersion, school separations and exits. The regressions (apart from Panel B) control for student to teacher ratio and log number of students. In Panel F we also include controls for the boost reforms (math, reading and teacher wage boost). Year FE and school FE are included in all regressions. Standard errors are clustered by school district.

Table C.2. *Sensitivity checks – Share of career teachers*

	(1)	(2)	(3)	(4)
	Log wages	Wage disp.	Separations	Exits
Panel A: Baseline results				
Share CT	0.165***	0.024***	-0.207***	-0.057***
	(0.007)	(0.001)	(0.025)	(0.015)
R^2	0.921	0.596	0.345	0.239
N	17,689	17,689	17,689	17,689
Panel B: No school controls				
Share CT	0.161***	0.024***	-0.264***	-0.078***
	(0.006)	(0.001)	(0.024)	(0.015)
R^2	0.921	0.596	0.321	0.231
N	17,700	17,700	17,700	17,700
Panel C: Eligible teachers				
Share CT	0.175***	0.022***	-0.240***	-0.078***
	(0.007)	(0.001)	(0.026)	(0.017)
R^2	0.936	0.577	0.311	0.214
N	17,684	17,628	17,684	17,684
Panel D: Weighted				
Share CT	0.174***	0.022***	-0.230***	-0.067***
	(0.007)	(0.001)	(0.021)	(0.014)
R^2	0.942	0.653	0.383	0.250
N	17,689	17,689	17,689	17,689
Panel E: Excl. never participating schools				
Share CT	0.161***	0.022***	-0.234***	-0.059***
	(0.006)	(0.002)	(0.025)	(0.016)
R^2	0.929	0.603	0.350	0.242
N	15,808	15,808	15,808	15,808
Panel F: With boost controls				
Share CT	0.163***	0.024***	-0.203***	-0.054***
	(0.007)	(0.001)	(0.025)	(0.015)
R^2	0.922	0.596	0.346	0.240
N	17,689	17,689	17,689	17,689

Note: We relate the change in the presence of career teachers in a school to the change in mean wages, wage dispersion, school separations and exits. The regressions (apart from Panel B) control for student to teacher ratio and log number of students. In Panel F we also include controls for the boost reforms (math, reading and teacher wage boost). Year FE and school FE are included in all regressions. Standard errors are clustered by school district.

Table C.3. *Student performance – with boost controls*

	(1)	(2)	(3)
Panel A: Pool across subject	Grade 3	Grade 6	Grade 9
Treat	0.018*	0.025***	0.005
	(0.010)	(0.008)	(0.008)
R^2	0.474	0.590	0.702
Share	0.095***	0.074***	0.025
	(0.028)	(0.018)	(0.020)
R^2	0.474	0.591	0.702
N	25,207	31,702	15,800
Year × subject FE:s	Yes	Yes	Yes
School × subject FE:s	Yes	Yes	Yes
School controls	Yes incl boost	Yes incl boost	Yes incl boost
Panel B: Pool across level	Maths	English	Swedish
Treat	0.018**	0.015*	0.020***
	(0.008)	(0.008)	(0.007)
R^2	0.533	0.627	0.561
Share	0.083***	0.039**	0.075***
	(0.017)	(0.018)	(0.017)
R^2	0.533	0.627	0.561
N	28,428	15,831	28,450
Year × level FE:s	Yes	Yes	Yes
School × level FE:s	Yes	Yes	Yes
School controls	Yes incl boost	Yes incl boost	Yes incl boost

Note: The table presents the results of estimating eq. 3.10, with additional controls for the Math and Reading Boost reforms. In Panel A we pool the results across subjects. In Panel B we pool the results across levels. Treatment variables are "treat" (a dummy equal to one if there is at least one subject & level career teacher at the school) or "share" (the number of subject & level career teachers at the school in year t divided by the number subject & level teachers in the school in $t-1$). School controls are log number of students, student to teacher ratio, and dummies for participating in the boost reforms. The results of the national exams are standardized by year, subject and level to have mean 0 and st.d. 1. Each school obtains a mean standardized score. Standard errors are clustered at school district level.

Table C.4. *Sensitivity checks – Student performance*

	Grade 3		Grade 6			Grade 9		
	Math	Swedish	Math	English	Swedish	Math	English	Swedish
Panel A: Lead subject CT								
Share	0.095**	0.089***	0.108***	0.044*	0.073***	0.006	0.020	0.045
	(0.038)	(0.028)	(0.027)	(0.022)	(0.027)	(0.028)	(0.031)	(0.028)
Lead share	0.019	0.047	0.014	0.009	0.022	-0.012	0.042**	0.051**
	(0.044)	(0.033)	(0.022)	(0.017)	(0.028)	(0.025)	(0.019)	(0.023)
R^2	0.465	0.489	0.597	0.585	0.583	0.681	0.727	0.697
N	12,599	12,608	10,566	10,564	10,572	5,263	5,267	5,270
Panel B: Share all teachers								
Subject/level CT as share of all teachers	0.166*	0.165***	0.422***	0.296**	0.234*	0.040	0.188	0.326*
	(0.092)	(0.061)	(0.115)	(0.122)	(0.120)	(0.204)	(0.186)	(0.195)
R^2	0.465	0.489	0.597	0.585	0.583	0.681	0.727	0.697
N	12,599	12,608	10,566	10,564	10,572	5,263	5,267	5,270
Panel C: Participating schools								
At least one CT at school	0.005	0.002	0.028**	0.010	0.025**	-0.010	-0.007	0.026
	(0.017)	(0.010)	(0.014)	(0.011)	(0.012)	(0.014)	(0.017)	(0.016)
R^2	0.465	0.489	0.596	0.585	0.583	0.681	0.727	0.697
N	12,599	12,608	10,566	10,564	10,572	5,263	5,267	5,270

Note: We regress standardized test scores on subject & teaching level-specific reform variables. In Panel A we use the number of subject & teaching level career teachers in year t over the number of subject and teaching level teachers in $t-1$, and the lead of that share. In Panel B we use the number of subject & teaching-level career teachers in year t over all teachers in $t-1$. In Panel C we use the presence of at least one career teacher at the school. All regressions include year FE, school FE, log number of students and student to teacher ratio. Standard errors are clustered by school district.

4. Spousal Earnings and Household Dynamics: Evidence From a Promotion Reform

Co-authored with Erik Grönqvist and Lena Hensvik

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4.1 Introduction

Compared to men, women spend considerably more time caring for children and the household in all OECD countries (OECD 2016). On an average day, American women spend 0.77 hours more on household activities compared to men and 0.62 fewer hours on paid work.[1] Even in Sweden, known as a progressive country with equal labor force participation of men and women and with generous childcare policies for mothers and fathers, women spend about an hour more on household chores, and work one hour less, than men each day.[2] Swedish mothers also take 70 percent of the paid parental leave to care for a children when they are small (Försäkringskassan 2020).

A recent literature suggests that these differences in time allocation across mothers and fathers are crucial to understanding the remaining gender pay gap (Goldin 2014; Cortés and Pan 2020).[3] Yet, the underlying causes behind the uneven distribution of household and family responsibilities are still not well-understood. Gender pay inequality may contribute to the persistence by enabling division of labor to exploit comparative advantages (Becker et al. 1977) or limit women's bargaining power (Lundberg and Pollak 1993). At the same time, prior literature also highlights the role of preferences or gender-specific norms as obstacles towards a more equal division of household and family responsibilities (Akerlof and Kranton 2000; Bertrand et al. 2015; Kleven et al. 2020b).

In this paper we study how a sudden and substantial improvement in female labor market outcomes affects the spousal division of time to care for children. More specifically, we leverage a Swedish promotion reform in a traditionally female-dominated occupation with relatively low wages, namely teaching. Teaching is the second most common occupation among Swedish mothers and the majority of teachers are secondary earners of the household.[4] In 2013, Sweden introduced a new career step in the teaching profession, aimed at increasing the attractiveness of the occupation. The new career step entailed a substantial monthly wage increase corresponding to nearly 20 percent of mean pre-reform wages. Consequently, the reform led to a sudden and substantial shift in affected couples' relative wages. By disproportionately af-

[1] Based on 2019 American Time Use Survey data.

[2] Based on the 2010/11 Swedish Time Use Survey, which is the latest survey conducted.

[3] For example, Duchini and Van Effenterre (2020) find that reduced time commitments to child care for mothers raised their average wages in France; Cortés and Pan (2019) show that low-skilled immigration inflows to the U.S have decreased the gender pay gap in occupations that disproportionately reward long work hours; and Denning et al. (2019) show that differences in hours worked have slowed women's labor market progress.

[4] 47 percent of all working mothers with children under the age of 11 work in the public sector compared to 17 percent of fathers. 15 percent are found within the three detailed 4-digit occupations nursing, lower secondary teaching and childcare. Prior literature suggest that women's preferences for these public sector jobs is highly linked to motherhood (Hotz et al. 2017; Pertold-Gebicka et al. 2016).

fecting female spouses, the reform also led to an average increase in women's contribution to household earnings.

We focus on dual-earner couples with children, and quantify the impact of this positive labor market shock on the reallocation of time spent to care for children. We present results both for all promoted persons and for promoted women only. Information on childcare is inferred from the relative use of Temporary Parental Leave (TPL), a benefit available to either spouse to care for an ill child during working hours. In Sweden, mothers and fathers of children aged between eight months and twelve years can claim up to 120 days of TPL annually to care for sick children, reimbursed at 80 per cent of current earnings (up to a cap). However, the families do not divide the days equally; the division is skewed towards women (60 percent) and is a good indicator of the gender division of household work more generally (Eriksson and Nermo 2010; Ichino et al. 2019).[5]

Our empirical strategy uses an event study design relying on the *timing of promotion*. The promotion reform was implemented gradually due to budgetary constraints, and we use Swedish register data to estimate within-couple differences in wages and use of TPL around the time of promotion in households where one of the spouses was promoted between 2013 and 2015. We focus on ever promoted teachers as teachers that receive promotions may differ from teachers that do not get promoted. We follow couples over time, even if they separate, and show that within-couple outcomes evolve very similarly in couples with a promoted partner and not-yet promoted partner, supporting the validity of the empirical design.

Reflecting the female nature of the teaching profession, the vast majority (almost 80 percent) of promoted teachers are women. As a consequence, we show that the within-household gender pay gap declines by 6.4 percentage points (32 percent) in couples affected by the reform. The probability that the woman is the primary earner furthermore increases by 9.1 percentage points (30 percent). Promoted spouses respond to these new economic positions by reducing their use of temporary parental leave relative to their partners. The within-couple difference in TPL benefits (as share of monthly wages) declines by three percentage points, or approximately 0.6 days. We find a similar response when restricting the sample to couples where the female spouse is promoted, suggesting a non-trivial narrowing of more than half of the pre-promotion gender imbalance in childcare time. Estimating the impact of TPL use separately for both the promoted woman and her spouse, the results suggest that promotions induce a reallocation within the couple: the promoted woman reduces the number of days taken, while the male spouse

[5]Unequal division of care for sick children may directly increase the gender wage gap. For example, Azmat et al. (2020) show that TPL is negatively related to wages in jobs with few employee substitutes, suggesting that unpredicted absence is problematic for firms.

increases the number of days taken.[6] The impact on partner TPL use is partly driven by a change on the extensive margin, increasing their propensity to use TPL by 2.5 percentage points (or 5 percent).

While the impact on TPL is highly interesting in its own right, prior literature suggests that the division of TPL is also linked to couples' overall allocation of time spent on household chores (Ichino et al. 2019). A back-of-the-envelope calculation based on our estimates for TPL use and the association between TPL days and household work in prior literature suggests that promotion of the mother increases the father's share of total home production by 6 percentage points, or 13 percent.[7]

The response in TPL use is considerably larger in couples with pre-school children, where the overall childcare demand is higher. Furthermore, the promoted person responds 50% stronger if the promotion makes them the primary earner. This suggests that the bargaining positions within the couple have a non-linear relationship to relative wage. Finally, we consider how the effect varies depending on spousal job characteristics. There is no differential effect on the within-couple difference in TPL when the spouse has a high ability to work from home. However, for promoted women the effect is over twice as large if the non-promoted spouse is also a teacher, possibly suggesting a larger shift in comparative advantage or relative bargaining power when the couple is on the same labor market.

Our paper contributes to the literature on how women's relative earnings relate to the household allocation of time between market work and home production by exploiting a policy reform that led to a substantial wage increase for highly educated mothers. While Bertrand et al. (2015) show that higher female relative income is associated with less time spent on home production until the woman becomes the primary earner, at which point she tends to endogenously adopt a more traditional gender role, we find no indication that promoted mothers compensate by increasing their share of family responsibilities once they become the primary earner. Our paper is also closely related to Ichino et al. (2019) who focus on the relationship between taxation and home production, finding that tax cuts generated by the Swedish EITC induced couples to reallocate time from childcare to the market. Whereas the response to tax cuts is symmetric for husbands and wives in the average couple, seemingly traditional couples respond more strongly to husband tax cuts.[8]

[6] As the reform targeted a female-dominated occupation, we do not have enough power to separately estimate the TPL-response for male promoted spouses.

[7] Two years prior to the promotion reform, in 2011, fathers' fraction of the weekly total amount of non-paid work in couples living together with small children was 45 percent (39.55 hours for women and 32.20 hours for men), according to the Swedish Time Use Survey (see Stanfors (2018) for a detailed description of how non-paid work differs between men and women depending on couple characteristics).

[8] Gelber and Mitchell (2012) find that single women in the US respond to stronger economic incentives by substituting housework for market work. However, due to potential adjustment

In contrast to these studies, we focus on an unusually large wage shift in a predominantly female segment of the labor market, enabling us to credibly assert the impact of women's labor market position. An important difference between the aforementioned studies and our paper is that we study the role of promotions in contrast to pure wage effects.[9] While wages and promotions are highly linked, the consequences of a promotion may be broader than simply monetary. For instance, promotions may represent a shift in status and power within the workplace that spills over to the bargaining power in the family. It is important to note that the reform we study stipulated that teachers should continue to teach and that the workload of the promoted teachers appear to have remained fairly constant.[10] Even if there is a shift in job responsibilities, we believe that the main influence of the new career step on the family was the sudden and substantial change in relative wage and changed status.

Our paper also contributes to the literature and discussion on the effectiveness of policy in addressing the unequal division of household and family responsibilities. Sweden and other countries have implemented a series of family policy reforms to facilitate the combination of parenthood and careers. Such reforms, however, appear to have little impact on fathers' involvement in the care of children (Ekberg et al. 2013) or on the overall child penalty (Kleven et al. 2020a).[11]

By showing that couples respond to promotions by reallocating childcare time to the non-promoted spouse, our results highlight that reforms improving the careers of mothers can be effective in promoting gender equality in the domestic sphere. As such, labor market policies directly targeting gender pay inequalities and career opportunities can be a powerful tool in promoting a more equal division of child responsibilities between mothers and fathers. Our results suggest that more traditional economic explanations manifested in the labor market should not be overlooked, thus partly contrasting the recent literature highlighting the role of norms in explaining the persistence of traditional gender roles in the family.

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frictions or norms it is not clear that these substitution effects are transferable to women in couples.

[9]Folke and Rickne (2020) consider the effect of promotions in a different context. They study how promotions to top political jobs affect marriage stability, finding that promotions increase the likelihood of divorce for promoted women but not promoted men.

[10]Statskontoret (2015) concludes that total working time did not increase due to a promotion. See Section 4.2.1 for a more detailed description of the reform.

[11]Ekberg et al. (2013) study how the introduction of mandated paternity leave in Sweden affects fathers' share of TPL. While the daddy-month reform was supposed to promote gender equality in the household, there is no evidence that the reform had an impact on fathers' involvement in household work in addition to a positive effect on their paternity leave. In addition, Avdic and Karimi (2018) show that marriage stability was negatively affected by the reform. Studies from Germany, Canada and Spain do, however, provide more encouraging results regarding the relationship between paternity leave policies and fathers' involvement in childcare (see Schober 2014; Patnaik 2019; and Farré and González 2019).

The paper proceeds as follows. It begins by describing the institutional context in Section 4.2. In Section 4.3 we describe our empirical strategy and the data used. Section 4.4 presents summary statistics for teachers and couples before the reform and describes how the reform affected household relative wage. Section 4.5 presents the main results on the impact of promotions on the household division of TPL. Section 4.6 concludes.

4.2 Institutional setting

The labor market for secondary school teachers is dominated by women. In the year prior to implementing the promotion reform, the fraction of female teachers was 78 percent (Grönqvist et al. 2020). As in many other countries, the teacher labor market is also characterized by low and compressed wages compared to other occupations with similar education requirements (four years of higher education). As a consequence the majority of teachers are secondary wage earners in the household.

4.2.1 The promotion reform

To make the teaching profession more attractive by aligning teacher pay more closely to teacher skills, the Swedish government introduced a new career step for teachers in 2013 called 'career teachers'.[12] This promotion program was launched as a response to deteriorating student achievement and aimed to improve student outcomes by keeping and encouraging high quality teachers (Regeringen 2013b).

Promoted teachers receive a significant monthly wage increase of 5,000 SEK (520 USD), which is fully funded by the state and corresponds to nearly 20 percent of mean pre-reform wages. Career teachers primarily continue to teach but also engage in tasks like coaching their colleagues. Total working time did not increase in general due to a promotion, and it was delegated to the school districts (and principals) to free up time for the new responsibilities while maintaining at least a 50 percent teaching load. Many promoted teachers report that the time set aside was not sufficient, which infringed on their capacity to carry out the new tasks and for principals to make proper use of promoted teachers. As promotions reduced the time in the classroom it also opened up opportunities to more flexibly allocate working time and working location to adapt to domestic needs (Statskontoret 2017).

[12] This section draws on Grönqvist et al. (2020). In the companion paper Grönqvist et al. (ibid.) we study the impact of the career teacher reform on teacher and school outcomes. The reform had full pass-through onto wages and schools that implement career teacher promotions have lower teacher turnover, a more qualified teaching pool and higher student test scores in Math, English and Swedish in grades 3 and 6. In this paper, we instead turn attention to the impact of the reform-induced promotions on household allocation of childcare vs. market work.

The reform was rolled out over four years, and the number of positions increased year-on-year in line with the funding provided by the state. Career teacher positions were allocated to school districts in proportion to their total student population. School districts, in turn, allocated career teacher positions to individual schools at their discretion. In practice, school principals at individual schools often recruited teachers to become career teachers. According to the reform's regulation (Regeringen 2013a), the teacher needed to fulfill certain criteria to qualify for promotion. They needed to be certified; have at least four years of teaching experience; demonstrate an ability to improve student outcomes and a keen interest to develop teaching; and be deemed particularly qualified as a teacher. Because eventually promoted teachers differ in observable ways from teachers that are not promoted (see Grönqvist et al. 2020), we focus on promoted teachers only in our empirical strategy, and utilize the fact that promotions happened at different points in time.

4.2.2 Temporary parental leave (TPL) system

The Swedish parental leave system is very flexible, allowing parents up to 120 days of leave annually to care for sick children during work hours. In 2019, 877,000 parents received TPL to care for 801,000 children (Försäkringskassan 2020). TPL is normally used to care for children with minor illnesses such as common colds, and take-out therefore tends to follow seasonal patterns. It can be taken to care for children aged 8 months until the child turns 12, though parents cannot generally take leave at the same time. It is reimbursed at almost 80 per cent of current earnings (up to a cap), and can be taken out as full days, three quarters of a day, half days, one quarter of a day or one eight of a day.[13]

This generous system, which facilitates parental reconciliation of work and family duties, is not utilized to the same extent by mothers and fathers. While it is more equally shared than the use of standard parental leave, about 60 percent of TPL is taken by mothers (ibid.). While interesting to study in its own right, TPL has also been found to proxy well for the general gender division of household chores, and is, unlike other time-use data, available in registry data (Eriksson and Nermo 2010). In fact, Ichino et al. (2019) provide evidence of a positive relation between fathers' share of TPL and their share of total home production for dual earner couples with children aged 3 and above (conditioning on total spouse home production time and their human capital levels), suggesting that TPL is an informative proxy for the male contribution to household work.

[13] If earnings are above a cap, TPL is instead reimbursed at almost 80% of the cap. The cap is set at 7.5 so-called price basis amounts. In 2020, the cap was 29,563 SEK (3,400 USD) per month. A price basis amount is used in Sweden for the calculation of various benefits and fees, and is adjusted annually for inflation.

4.3 Empirical strategy and data

We are interested in how promotions affect household behavior. To estimate the impact of promotions on the relative take-up of temporary parental leave and relative wage changes within couples, we consider the following specification for households:

$$\tilde{y}_{hct} = \alpha + \beta Promoted_{hct} + \lambda_c + \sum_a \lambda_{ac}^i + \sum_a \lambda_{ac}^j + \theta \tilde{I}_{h,-4} + \varepsilon_{hct} \qquad (4.1)$$

$\tilde{y}_{hct}$ is the within-household difference in the outcome of interest in household h in calendar year c observed t years before or after promotion, and $Promoted_{hct}$ is a dummy variable equal to one for the household from the year of promotion onward. We estimate equation 4.1 using couples where one of the spouses was promoted during the period 2013 to 2015. We include calendar year fixed effects, λ_c, to control for general shocks and trends in the economy. $\lambda_{ac}^g, g \in i, j$ are age dummies for the promoted person i and the partner j to control for that the age trajectories in the outcome may differ by spouse.[14] To avoid data sparseness, we bin the ages by five year bands. $\tilde{I}_{h,-4}$ is the within-household difference in income four years prior to promotion, included to capture how (un)equal the couple is. We test the robustness of the results in the appendix. When we consider TPL outcomes we also control for the number of children aged 0–3, 4–6 and 7–10. Standard errors ε_{hct} are clustered by municipality (equivalent to the school district for public schools).[15]

Couple differences in outcomes are defined as the promoted person minus the non-promoted spouse. Estimates of β therefore show the change in the within-couple difference in wages and temporary parental leave between the promoted teacher and their partner relative to the within-couple difference between not-yet promoted teachers and their partners. We also consider the effect of promotion on the gender wage gap and on the probability that the wife earns more than the husband. For TPL, we also consider the likelihood that the person takes out TPL.

The empirical strategy relies on the assumption that, in absence of promotion, the outcome variable would have evolved in parallel for promoted and not-yet promoted couples. Hence, the *timing* of when the teacher is promoted must be uncorrelated with changes in other determinants of the outcome that we do not control for. To assess whether the identifying assumption is likely to hold and to estimate the dynamics of being promoted, we adopt an event-study approach similar to Angelov et al. (2016), who study the effect of parenthood on the couple gender pay gap. Specifically, we consider the following expansion of equation 4.1:

[14] We also estimate the specification separately by gender of the promoted person. When doing so, the age dummies control for that age trajectories may differ by gender.

[15] In light of our long pre-period (we have data from 2007), we include a dummy in all estimations of equation 4.1 to capture observations more than three event years before promotion. The results are not sensitive to the inclusion of this dummy.

$$\tilde{y}_{hct} = \alpha + \sum_{t \neq -1} \gamma_t 1[t] + \lambda_c + \sum_a \lambda^i_{ac} + \sum_a \lambda^j_{ac} + \theta \tilde{I}_{h,-4} + \varepsilon_{hct} \qquad (4.2)$$

where t denotes time relative to the year of promotion and promotion occurs when t equals 0.[16] Observations three or more event years after promotion ($t \geq 3$) are grouped.[17] The year before promotion is omitted, such that the parameters of interest γ_t show the couples' gap in outcome relative to the year prior to promotion.

4.3.1 Data

The underlying population used for the analysis consists of all ever promoted teachers and their spouses. Using a family identifier that combines spouses, a family panel with annual information is created. Information is linked to the household from different registry data sources collected and compiled by Statistics Sweden. The panel includes detailed information about the couple such as their wages and earnings, place of work, use of social insurance benefits, number of children and demographic variables. We follow couples even if they separate, as long as they are a couple four years prior to the teacher's promotion. Data is drawn from three main data sources: a longitudinal individual-level database that covers all individuals in Sweden aged 16 to 74 (*LOUISE*), matched employee-employer data (*RAMS*), and structural earnings statistics. We have access to *LOUISE* between 2007 and 2017, and *RAMS* and the structural earnings statistics between 2007 and 2018.

Promoted teachers: We identify promoted teachers using occupational codes and full-time equivalent wages, which come from the structural earnings statistics. Structural earnings statistics are available for all individuals in the public sector and a sample of individuals in the private sector.[18] Teachers are defined as individuals who have an occupational code as a teacher at their main place of work.[19] While we do not have identifiers for actual promotions in the data, we exploit the institutional features of the reform: a teacher is

[16] Notice that this is not a standard event-specification, just like equation 4.1 is not a standard difference-in-difference specification, as we include the lagged difference in income instead of couple fixed effects. The results are robust to instead including couple fixed effects, reported in the appendix.

[17] As before, a dummy is also included to capture observations more than three event years before promotion ($t < -3$) in all estimations of 4.2. An alternative to binning endpoints is to fully saturate the model with event indicators. As Borusyak and Jaravel (2018) point out, in a standard event study design such a fully dynamic specification is underidentified and treatment effects can only be identified up to a linear trend. Thus, two pre-treatment indicators need to be omitted. We test this alternative specification and find that our results are robust.

[18] Approximately 50% of private sector employees are included. The sampling is stratified by industry and firm size, with an oversampling of larger firms.

[19] Precisely, we use occupation (*SSYK*) codes 2321, 2322, 2323, 2330, 2340, and 2351 until 2014, and codes 2320, 2330, 2341, 2351 from 2014 onward.

classified as promoted from year s until the last period of observation if the monthly full-time equivalent wage rises by 5,000 SEK or more between $s-1$ and s, where s falls between 2013 and 2015.[20] The year of promotion is the year when the wage increase is observed. We identify 13,500 unique promoted teachers in our data.[21]

We have tested the robustness of this method in several ways. First, we estimate the wage increase associated with being promoted compared to teachers that are not promoted (see Table A.4). The results are very similar to those obtained in our companion paper Grönqvist et al. (2020), where we have access to data that includes promotion identifiers.[22] Second, we have applied the method outlined above to the data used in Grönqvist et al. (ibid.), to compare how many actual promotions we capture using the promotion proxy. We have tested the sensitivity of the results to wage increase thresholds ranging from 3,000 SEK to 7,000 SEK, in 500 SEK intervals. Ideally, we want to capture as many promotions in the correct promotion year as possible, but not capture teachers that have not actually been promoted. We consider four measures to evaluate the proxy: how many actual promotions that the proxy captures, how many promotions the proxy captures in the correct year, how many promotions the proxy captures that it should not capture (termed "excess share"), and finally how many promotions in a particular year the proxy captures that it should not. The denominator in all comparisons is the number of actual promotions based on the career teacher register between 2013 and 2015 for whom the promoted person has an occupational code as a teacher (i.e. the sum of all possible career teacher promotions that we can capture). The results of this exercise are included in Figure A.1. This shows that the proxy does well at capturing promotions, albeit with some measurement error. When the wage increase threshold is set at SEK 5,000, we capture 81% of actual promotions in the right year (87% of actual promotions), and 21% excess promotions-by-year (15% excess promotions). The next-best alternative to using a threshold of SEK 5,000 is to use SEK 5,500. With this threshold we capture 80% of actual promotions in the right year (85% of actual promotions), and 16% excess promotions-by-year (11% excess promotions). We use a wage increase threshold of SEK 5,500 in robustness checks and find similar results.

[20] We stop in 2015 because another teacher salary reform, the Teachers' Salary Boost, was implemented in 2016. This entailed a smaller unconditional wage increase to about half of all teachers.

[21] In Table A.1 in the appendix we include the number of promoted persons and couples per year. The number of promotions is slightly lower, but closely follows, official statistics included in Statskontoret (2017) who identify 3,076 promoted teachers in autumn 2013, 12,114 in autumn 2014 and 14,340 in autumn 2015, compared to 3,064, 10,623 and 13,489 in our data. That we have a slightly lower number could among others be due to data not allowing us to capture all teachers in voucher schools.

[22] The reason why we cannot use promotion identifiers here is that these are not available in the same dataset as the TPL data.

Partners: The analysis data include information on the promoted person and their partner irrespective of how long the couple has been together, as long as they are a couple four years prior to the teacher's promotion. We identify the partner as the oldest non-promoted person with the same family identifier as the promoted person. To ensure that we do not capture single persons with children, we apply the restriction that the age difference between the promoted teacher and the partner cannot exceed 18 years. We exclude couples where both partners have been promoted as well as same-sex couples from our main sample.[23] After these restrictions we identify 9,908 unique partners, or equivalently couples.

Wages: The individual wage measure is monthly full-time equivalent wages at the main workplace. The main workplace is defined as the workplace where the person has the highest positive earnings in a given year, using information from the matched employee-employer data. Wages are taken from the structural earnings statistics and are therefore only available for a subset of individuals. It is available for everyone working in the public sector and a 50 percent stratified sample of workers in the private sector. Wages are measured between September and November each year.

Temporary parental leave (TPL): We observe the annual amount of TPL per person in Swedish crowns (SEK). The benefits are a function of the person's wage, up to a cap. As the temporary parental leave in SEK depends both on the parent's wage and the number of days taken, we divide the temporary parental benefits in SEK by the replacement rate (just under 80%) multiplied by the minimum of the person's wage and the cap each year.[24] We refer to this measure as normalized TPL. It can approximately be interpreted as the fraction of annual TPL out of the person's replaced monthly wage. We can convert the measure to number of TPL days per year by multiplying by the average number of days worked per month (21 days).[25] For ease of interpretation we report the approximated number of TPL days per year in our main results.[26] We also consider TPL on the extensive margin. This is a dummy variable equal to one if the person has received positive TPL benefits.

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[23] There are 164 couples where both partners have been promoted and 32 same-sex couples.

[24] We consequently can only define this measure for the sub-sample of persons for whom we observe monthly wages. The replacement rate is $0.97 \times 0.8 = 77.6\%$.

[25] To see this, note that we observe monthly full-time equivalent wages and annual TPL. Suppose the monthly wages are below the cap. Let D_m be days worked per month and w_d be the daily wage. The wage per month is thus $w_m = w_d \times D_m$. Let D_{TPL} be days with TPL per year and TPL_y be the annual amount of TPL in SEK. Then $TPL_y = D_{TPL} \times 0.776 \times w_d = D_{TPL} \times 0.776 \times \frac{w_m}{D_m}$. Rearranging we see that $D_{TPL} = \frac{TPL_y}{0.776 \times w_m} \times D_m$ where $\frac{TPL_y}{0.776 \times w_m}$ is the normalized measure of TPL. The same calculation follows if the wage is above the cap, but w_m is replaced with the cap.

[26] We have ordered spell data to complement our current data. In future versions of the paper, we plan to update the TPL measure to TPL days based on spell data.

4.4 Descriptive statistics

4.4.1 Summary statistics: teachers and couples before promotion

Table 4.1 describes the couples in the year prior to promotion. Focusing on the promoted teachers, they are predominantly female (79%) and married (82%). The proportion of female promoted teachers is the same as the proportion of women teaching at the lower secondary level in total. Thus, there appears to be no strong gender bias in the selection of teachers for promotion.

Around half of promoted teachers have children under 11 years old (referred to as "young children"), and, conditional on having young children, 75% claim temporary parental leave in the year prior to promotion.[27] In line with the eligibility requirements for promotion, all promoted teachers have post-secondary education. On average, they are more educated than their partners. Despite this, they are more often secondary earners in the household and also the primary childcare takers (75% claim benefits compared to 58% of partners, conditional on having young children). Of these couples, where it is primarily but not only the woman that gets promoted, the share of wage earned by the wife prior to promotion is 46% on average, and 30% are in relationships where the woman has a higher wage already prior to promotion.[28]

The summary statistics mask differences between promoted women and promoted men. In Appendix Table A.2 we split the couples by gender. Promoted men and women are similar in terms of their age (45 on average) and educational level (post-secondary or higher). 59% of promoted men have young children, compared to 49% of promoted women. Conditional on having children under the age of 11, a similar share of promoted women and men claim temporary parental leave (75% compared to 74%). Even so, within-couple differences show that promoted men generally claim less benefits than their partners prior to promotion, while promoted women generally claim more benefits than their partners. Moreover, while promoted men and promoted women have similar wages, the women are typically in relationships with men that earn substantially higher wages (10,500 SEK more) prior to promotion, while the men are typically in relationships with women that earn around the same wage as they do (500 SEK more).

[27] TPL can be claimed until the day the child turns 12. We use 11 rather than 12 because we observe the ages of children in intervals.

[28] In Table A.3 we include couples for whom we can define normalized TPL and who have young children ("TPL sample"). They are similar to the full sample along many dimensions, such as the pre-promotion differences in wages and the share wage earned by the wife. A somewhat lower share of promoted persons are female compared to the overall sample (70% compared to 79%) and they are younger on average (40 compared to 45).

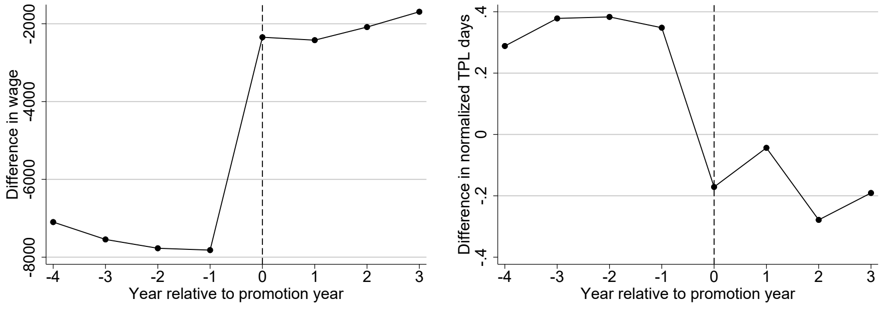
Table 4.1. *Summary statistics in year before promotion*

	Promoted teacher		Partner	
	Mean	sd	Mean	sd
Individual characteristics:				
Female	0.79	(0.41)	0.21	(0.41)
Married	0.82	(0.38)	0.82	(0.38)
Age	44.78	(7.85)	46.27	(8.64)
Compulsory	0.00	(0.02)	0.04	(0.19)
Upper secondary	0.00	(0.06)	0.31	(0.46)
Post-secondary or higher	1.00	(0.06)	0.65	(0.48)
Education missing	0.00	(0.01)	0.00	(0.04)
Young children	0.51	(0.50)	0.51	(0.50)
Claim TPL*	0.75	(0.43)	0.58	(0.49)
TPL (100 SEK)*	39.70	(58.95)	33.34	(57.51)
TPL (normalized)*	0.19	(0.28)	0.17	(0.26)
TPL days*	3.95	(5.87)	3.58	(5.55)
Non-missing wage	1.00	(0.00)	0.53	(0.50)
Monthly wage (SEK)	30,087	(3,165)	38,019	(17,581)
Annual earnings (SEK)	345,542	(71,761)	433,593	(272,555)
Couple characteristics:				
Difference monthly wages	-7,819	(17,539)		
Difference annual earnings	-88,227	(280,801)		
Difference TPL (100 SEK)*	6.63	(59.60)		
Difference TPL (normalized)*	0.02	(0.28)		
Difference TPL days*	0.37	(5.81)		
Share wage earned by wife	0.46	(0.08)		
Wife has higher wage	0.30	(0.46)		
Observations	9,908		9,908	

Note: The table shows summary statistics for promoted teachers with partners (cols. 1 and 2), and partners (cols. 3 and 4) in the year before promotion. Couples where both partners have been promoted, as well as same sex couples, are excluded. Young children identifies couples with children under 11 years old. The difference variables are calculated as the promoted person minus the partner. Wife refers to females, irrespective of whether the couple is legally married. TPL variables (marked with a *) condition on having children below the age of 11. The difference in wages (annual earnings) are only calculated for those couples where wages (annual earnings) are non-missing for both spouses.

4.4.2 Raw within-couple differences by year

Before formally estimating how promotion affects household behavior, we plot the mean within-couple difference in wages and TPL days per year relative to promotion year in Figure 4.1. On average, the promoted person earns SEK 8,000 less than their partner prior to promotion, a difference that is substantially reduced upon promotion. Similarly, the promoted person tends to take more TPL than their partner prior to promotion. The figure suggests that the promotion eliminates or even reverses this gap.



(a) Monthly wage (SEK) *(b)* TPL days per year

Figure 4.1. Raw within-couple differences by year relative to year of promotion

Note: The figure plots the within-couple difference in wages (panel a) and normalized TPL days (panel b). The difference is defined as the promoted person minus the partner. Panel (b) is for the sub-sample of couples that have children below age 11.

4.4.3 Promotion impact on household relative wage

Table 4.2 shows the effect of promotion on household relative wages, i.e. the β-coefficients obtained when estimating equation 4.1. Overall, our results suggest that the reform had a sudden, large and persistent effect on the wage distribution within couples.

Column (1) confirms that the promotion translates into a wage difference between the promoted teacher and his/her non-promoted partner that is very close to the stipulated wage increase of 5,000 SEK. In columns (2) and (3) we consider impacts on the within-couple gender wage distribution. Promotions decrease the gender wage gap by 6.4 percentage points (32 percent).[29] Similarly, the fraction of female primary wage earners increases by 9.1 percentage points (30 percent).

[29]We convert the point estimate to a percentage point change using the formula $100 \times (e^{\hat{\beta}} - 1)$.

Table 4.2. *Effect of promotion on household relative wage*

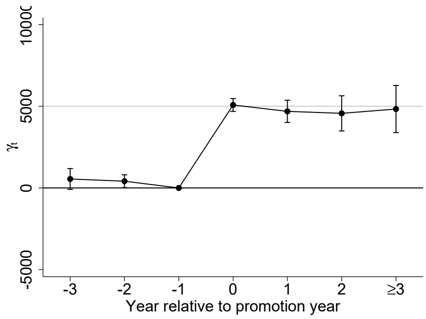
	(1)	(2)	(3)
Outcome:	Wage diff. (SEK)	Couple gender wage gap	Woman earns higher wages
Promoted	4,979***	-0.066***	0.091***
	(229)	(0.007)	(0.012)
R^2	0.693	0.367	0.076
N	52,414	52,414	52,414
Mean outcome$_{-1}$	-7,819	.18	.3
Year FE	Yes	Yes	Yes
Age dummies	Yes	Yes	Yes
Diff earnings$_{-4}$	Yes	Yes	Yes

Note: This includes results on the effect of promotion on the within-couple difference in wage, the couple gender wage gap (defined as $\ln(wage)_m - \ln(wage)_f$), and on the likelihood that the woman has a higher wage. It reports $\hat{\beta}$ from equation 4.1. Standard errors are clustered by municipality and included in parentheses. Mean outcome$_{-1}$ shows the unconditional mean of the outcome in the year prior to promotion.

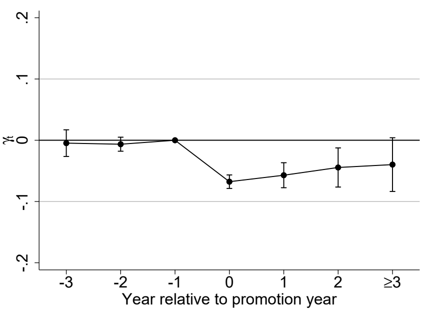
In Figure 4.2 we plot the event dummies from equation 4.2 to further investigate whether the household relative wage outcomes are parallel in promoted and not-yet-promoted households prior to the promotion event. Reassuringly, trends evolve very similar prior to promotion. In addition, the pattern suggests that the promotions generate a within-household wage differential that is persistent over time.[30]

The reform impact on women's wages relative to their partners, documented in Table 4.2, is non-trivial and reflects that the reform had a disproportionate impact on female spouses. In Table 4.3 we consider the household responses separately depending on whether the male or the female spouse was promoted. In line with the intention of the reform, we find that there is a symmetric impact on the within-couple wage difference of around 5,000 SEK (col. (1) and (2)). Consequently, the couple gender wage gap decreases (by 14.4 percentage points) in "female promoted couples" and increases (by 16.3 percentage points) in "male promoted couples". When interpreting these estimates it is useful to note that the pre-promotion wage differential differs quite substantially depending on the gender of the promoted spouse. Promoted women earn substantially less (10,000 SEK/1,100 USD) than their male partner before promotion while wages are much more similar in couples with promoted males.

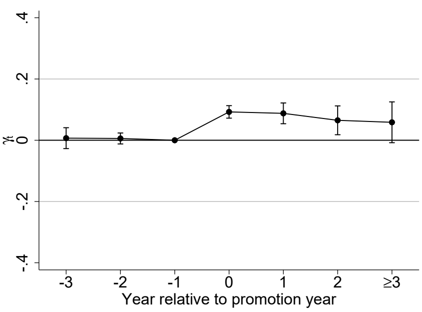
[30] Appendix event figures A.2, A.3 and A.4 show the sensitivity of the results to different empirical models, for the full sample and separately by gender. Reassuringly, the wage effects are very robust to the choice of controls. Appendix table A.5 show that the results are similar if we instead define the sample of promoted teachers using a wage threshold of SEK 5,500.



(a) Difference in wage (SEK)



(b) Couple gender wage gap



(c) Woman earns higher wages

Figure 4.2. Effect of promotion on within-couple difference in wage

Note: The figure plots γ_t with 95% confidence intervals from estimating equation 4.2 for all promoted couples. Calendar year fixed effects, promotion/partner-specific age dummies and the within-couple difference in annual earnings four years prior to promotion are included as controls.

Table 4.3. *Effect of promotion on household relative wage – by gender*

	(1)	(2)	(3)	(4)
Outcome:	Wage difference (SEK)		Couple gender wage gap	
Sample:	Women	Men	Women	Men
Promoted	5,006***	5,219***	-0.155***	0.151***
	(291)	(359)	(0.006)	(0.009)
R^2	0.699	0.493	0.610	0.491
N	38,420	13,994	38,420	13,994
Mean outcome$_{-1}$	-10,542	-534	.24	.01
Year FE	Yes	Yes	Yes	Yes
Age dummies	Yes	Yes	Yes	Yes
Diff earnings$_{-4}$	Yes	Yes	Yes	Yes

Note: This includes results on the effect of promotion on the within-couple difference in wage and couple gender wage gap, separately for promoted women and promoted men. It reports $\hat{\beta}$ from equation 4.1. Standard errors are clustered by municipality and included in parentheses.

So far we have considered the within-couple difference in wages. In Panel A of Table 4.4, we show estimates from individual-level wage regressions. These confirm that the relative wage increase is driven by the wage effect of the promoted person, with no statistically significant effect on the spouse's wage. Another interesting dimension to consider is how hours worked is affected. While the promotion involves a substantial wage increase, the promotion also entails new tasks, even though the teacher continues mainly to teach. Moreover, the spouse may respond to the promotion by reducing the hours worked and shifting attention to the home, in light of their relatively worse bargaining position in the household, or by increasing their hours worked. In Panel B of Table 4.4, we show estimates from individual-level regressions in hours worked per month.[31] The results suggest that promoted persons have increased their hours worked by 2.5 hours per month (around 1.5%) while the partners have not adjusted their hours worked. This also supports our interpretation that the career teacher promotion's main influence on the family was through the wage increase rather than an increase in hours worked.

Table 4.4. *Effect of promotion on allocation of wages and hours within couple*

	(1)	(2)	(3)	(4)	(5)	(6)
	All promotions		Female promotions		Male promotions	
	Promoted	Partner	Promoted	Partner	Promoted	Partner
Panel A: Wages						
Promoted	5244.1***	-83.2	5248.0***	-82.0	5236.2***	-32.5
	(49.1)	(112.9)	(45.3)	(153.4)	(123.8)	(136.2)
R^2	0.947	0.934	0.955	0.932	0.925	0.915
N	52,414	52,414	38,306	38,306	14,108	14,108
Mean outcome$_{-1}$	30,200	38,019	29,946	40,488	30,882	31,416
Panel B: Hours						
Promoted	2.5***	-0.4	2.3**	-0.3	3.1**	-0.8
	(0.9)	(0.7)	(0.9)	(0.9)	(1.4)	(1.4)
R^2	0.406	0.441	0.418	0.433	0.363	0.425
N	45,792	45,792	32,923	32,923	12,869	12,869
Mean outcome$_{-1}$	151.59	148.43	150.58	151.23	154.15	141.34
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Person FE	Yes	Yes	Yes	Yes	Yes	Yes

Note: This includes results on the effect of promotion on the person's monthly wages (Panel A) and total hours worked per month in a month between September and November (Panel B). It is based on estimating the following model: $y_{ict} = \beta Promoted_{hct} + \lambda_i + \lambda_c + \varepsilon_{ict}$, where i refers to the individual. Estimations are done separately for promoted individuals (cols. (1), (3) and (5)), and their partners (cols. (2), (4) and (6)). Standard errors are clustered by municipality and included in parentheses.

[31] Hours are available for the same individuals for whom we observe wages, and are also measured between September and November. Note that the use of TPL tends to be the highest in February.

To sum up, we find that promotions had a sudden and clear impact on relative wages in the household. This response is driven by an increase in the wage for the promoted person, while the partner does not appear to adjust their labor supply. The promotions led to a reduction in couples' gender wage gap on average, reflecting the higher promotion rate of women. In the next section, we turn to our main results, namely the impact of promotions on TPL use. In this analysis, we focus on couples with children below age 11 only.[32]

4.5 Promotion impact on the division of childcare

Table 4.5 shows the promotion effect on the household division of Temporary Parental Leave (TPL). In column (1), we show the impact of the promotion on the within-couple difference in TPL days per year among all promotions.[33] In line with the patterns in the raw data displayed in Figure 4.1, promoted persons decrease their use of TPL relative to their partners. The within-couple difference in TPL benefits as a share of monthly wages decline by three percentage points, which corresponds to approximately 0.6 days (s.e. 0.18). Column (2) shows the estimate separately for female promotions, indicating that the response of female promoted spouses is approximately as strong as the overall response. Since there are relatively few promoted men with young children, we lack statistical power for promoted men. We only present results separately for male promotions in the appendix (see Table A.6).

Table 4.5. *Effect of promotion on within-couple difference in TPL days*

	(1)	(2)
	Total	Female
Promoted	-0.627***	-0.573***
	(0.180)	(0.201)
R^2	0.058	0.050
N	25,680	18,420
Mean outcome$_{-1}$	.35	1.02
Child controls	Yes	Yes
Year FE	Yes	Yes
Age dummies	Yes	Yes
Diff earnings$_{-4}$	Yes	Yes

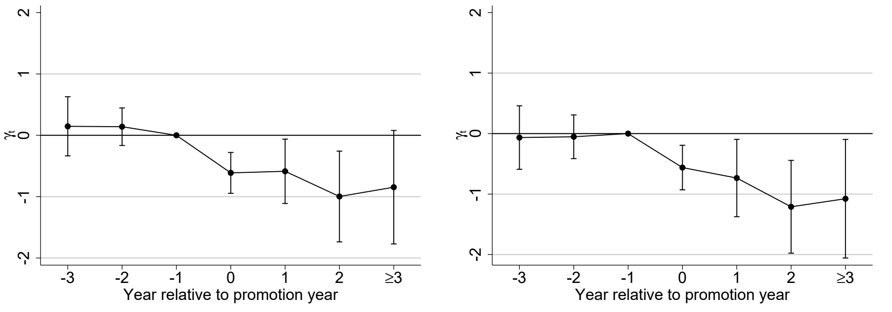
Note: This reports $\hat{\beta}$ from equation 4.1 for the within-couple difference in normalized TPL days, defined as promoted person minus partner. Only couples with children under age 11 are included. Standard errors are clustered by municipality and included in parentheses. Mean outcome$_{-1}$ shows the unconditional mean of the outcome in the year prior to promotion.

[32] Similar wage results are found when we apply this sample restriction, i.e. in the TPL sample.
[33] For details on how we compute this measure, see Section 4.3.1.

To put these estimates into context, promoted women generally take four days of TPL in the year prior to promotion while their partners take three days of TPL in the sample used for the estimation. Over the post-promotion period, the promotion therefore closes over half the gender gap in temporary parental leave for promoted women.

We have tested the robustness of these results in several ways. Appendix table A.6 shows the sensitivity of the results to different empirical models. The estimates are consistently negative and of similar magnitude across empirical specifications, which suggests that differences in observable couple characteristics have little impact on the estimates. We also assess the robustness of the estimates to including couple fixed effects instead of parametrically controlling for the within-couple difference in income four years prior to promotion, also finding negative effects. In our main analysis, we restrict the sample to couples that have children below age 11, but not all parents with young children take TPL. Panel A of appendix table A.7 shows that the point estimates are slightly larger if we further restrict the sample by removing couples where neither parent takes TPL. As above, the effects for promoted women indicate that over half of the pre-promotion gender difference in TPL is closed. We obtain similar estimates to our baseline results if we define the sample of promoted teachers using a wage threshold of SEK 5,500 instead of SEK 5,000, presented in Panel B of the same table. Finally, Panel C of the table includes results that show that the effect of promotion on the within-couple difference in TPL, measured in hundreds of crowns without normalizing, is also negative.

Figure 4.3 shows the corresponding event graphs from estimating equation 4.2. These confirm a clear negative response in total and women's use of TPL after promotion relative to their partners, which appears to grow slightly over time. Two years after the promotion, the promoted woman has reduced her use of normalized TPL just over 1 day compared to her partner. This corresponds to a full closing of the pre-promotion unconditional gap between the promoted woman and her partner. Appendix Figure A.5 shows that the estimated effect on TPL use is robust irrespective of the controls included in equation 4.2.



(a) All promoted couples *(b)* Promoted women

Figure 4.3. Effect of promotion on within-couple difference in TPL days

Note: The figure plots γ_t with 95% confidence intervals from estimating equation 4.2. Only couples with children under age 11 are included. Calendar year fixed effects, promotion/partner-specific age dummies, the number of children aged 0–3, 4–6 and 7–10, and the within-couple difference in annual earnings four years prior to promotion are included as controls.

To better gauge whether the promoted person or the spouse adjusts their use of leave, we turn to individual-level regressions next. We separately estimate the use of TPL for the promoted person and their spouse. In Panel A of Table 4.6, we consider the extensive margin response, i.e. whether the promoted person or their spouse takes any temporary parental leave. The results suggest that the promoted person does not change their extensive margin response, while the promotion coincides with the partner increasing their likelihood of take-up by around 2.5 percentage points. In Panel B we consider normalized TPL days. These results suggest that that the promotion reflects a reallocation within the couple: the promoted person reduces the number of days taken, while the spouse increases the number of days taken. It should be noted that the results are imprecise, though the estimates typically reach statistical significance if we include fewer controls.

While our estimates capture the impact on temporary parental leave use, they can also be seen as a proxy for the allocation of household activities more generally. Using supplementary data, Ichino et al. (2019) show that one additional half-day of father's TPL is associated with a ten percentage point rise in the father's share of total home production. Based on their estimates, the female promotion response in the father's take-up of TPL by 0.3 days would translate into a 6 percentage point increase in the father's share of home production.

Table 4.6. *Effect of promotion on allocation of TPL within couple*

	(1)	(2)	(3)	(4)
	All promotions		Female promotions	
	Promoted	Partner	Promoted	Partner
Panel A: Extensive margin				
Promoted	0.001	0.025**	0.001	0.024
	(0.011)	(0.013)	(0.013)	(0.015)
R^2	0.039	0.079	0.045	0.096
N	53,665	52,882	41,074	40,646
Mean outcome$_{-1}$	.75	.58	.75	.53
Panel B: TPL days				
Promoted	-0.350**	0.245	-0.214	0.292*
	(0.154)	(0.202)	(0.190)	(0.174)
R^2	0.050	0.097	0.045	0.107
N	25,837	25,790	18,485	18,507
Mean outcome$_{-1}$	3.91	3.56	3.99	2.96
Child controls	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Person age dummies	Yes	Yes	Yes	Yes
Person earnings$_{-4}$	Yes	Yes	Yes	Yes

Note: This includes results on the effect of promotion on whether the person takes any leave (Panel A) and TPL days (Panel B). Estimations are done separately for promoted individuals (cols. (1) and (3)), and their partners (cols. (2) and (4)). Only couples with children under age 11 are included. Standard errors are clustered by municipality and included in parentheses.

4.5.1 Heterogeneity analysis

Next, we present results of a heterogeneity analysis. We modify our previous empirical model (4.1) by interacting the effect of promotion with Z as follows, where Z is the characteristic in question that we are interested in:

$$\begin{aligned} \tilde{y}_{hct} = \alpha + \beta Promoted_{hct} + \theta Promoted_{hct} \times Z_h + \psi Z_h \\ + \lambda_c + \sum_a \lambda^i_{ac} + \sum_a \lambda^j_{ac} + \theta \tilde{I}_{h,-4} + \varepsilon_{hct} \end{aligned} \tag{4.3}$$

As previously, $\tilde{y}_{hct}$ is the within-couple difference in TPL days, defined as the promoted person minus the partner. As before we also include controls for calendar year fixed effects, the number of children, the promoted person i and the partner j's age, and the within-couple difference in income four years prior to promotion.

First, we consider whether the promotion response differs by the age of the children. Parents tend to take out the most leave when children are young. For example, in the year prior to promotion the parents on average take a total of eleven days of TPL if they have a child that is between 0 and 3; nine days if the child is between 4 and 6; and a bit less than seven days if the child is between 7 and 10. At the same time, as the child ages the promoted person increasingly tends to take more leave than the partner. For example, for children below age 7 the promoted person tends to take 0.2 days more in the year prior to promotion, while for children aged 7 to 10 the promoted person tends to take 0.4 days more.

In columns (1) and (2) of Table 4.7 we present our coefficients of interest, $\hat{\beta}$ and $\hat{\theta}$. Here Z is an indicator variable equal to one if the couple has at least one child aged 7 to 10.[34] $\hat{\beta}$ shows the estimated effect of promotion in couples where the promoted person only has children aged up to 7, and $\hat{\beta}+\hat{\theta}$ shows the estimated effect in couples where they have at least one child aged 7 to 10. The results suggest that the relative reduction in TPL use is greater among couples with pre-school aged children, with an attenuated effect if the couple has at least one older child. Comparing the estimates to the unconditional means in the outcome in the year prior to promotion, promotions among teachers with children below age 7 reverses the gap in TPL such that the partner takes more leave than the promoted person following promotion, while promotions among couples with children aged 7 and above closes the gap in leave between the promoted person and their spouse. While the same general patterns hold for promoted women, there is no statistically significant differential effect for those with children aged 7 and above at the 10% level. Moreover, since women tend to take more leave than men, women with children below age 7 also take more leave than their spouse on average in the post-period, but the gap in leave is substantially reduced.

The results in Table 4.5 are consistent with traditional specialization models suggesting that when one spouse's contribution to family income increases then his or her time spent on childcare decreases. However, relative income shifts may affect the bargaining positions within couples. To further investigate the underlying mechanisms we consider if the response differs depending on whether the promoted person becomes the primary earner thanks to the promotion or not. In the estimations we compare primary earners and non-primary earners.[35] A positive estimate of $\hat{\theta}$ would suggest that, in couples where the promoted person becomes the primary earner, relative TPL use is affected less than in non-primary earner couples, while a negative estimate of $\hat{\theta}$ would suggest that the same couples respond more strongly to the promotion.

Columns (3) and (4) of Table 4.7 present the results of this exercise. Column (3), which is for both female and male promotions, suggests that the

[34] Because the indicator variable is highly correlated with the number of children in a particular age interval, we do not include child controls in this specification.

[35] We define primary earner couples as couples where the promoted person earned between SEK 0 and $5,000$ less than their spouse in the year prior to promotion. We define non-primary earners as couples where the spouse was earning at least $5,000$ more than the promoted person in the year prior to promotion. We do not include couples where the promoted person was already the primary earner prior to promotion in the analysis.

reduction in childcare time is considerably larger if the promoted teacher becomes the primary earner.[36] In column (4) we focus on promoted women only. We cannot reject that this primary earner effect is equal for women, although we suffer from lack of precision in the gender-specific regression. Even so, the results are consistent with the primary earner effect being particularly strong for promoted men. As a whole the results suggest that, in cases where the promotion leads the teacher to become the primary breadwinner, there is an even stronger effect on the division of household chores, in line with an increase in the bargaining position of the promoted party. We acknowledge that a household bargaining model gives no particular significance to the point where the primary earner shifts *per se*. Even so, the results are at odds with a norms-based interpretation where the woman tries to overcompensate by increasing time spent at home once she is the primary breadwinner (Bertrand et al. 2015).

Next, we consider two different spousal job characteristics. First, we can imagine that couples have different opportunities to reallocate childcare within the household. When the spouse has more flexibility to work from home, it may facilitate their ability to take leave to care for their children when they fall ill. To test for this, we match in information about the spouse's ability to work from home based on their 4-digit occupation. As our primary telework-measure, we use the measure derived in Hensvik et al. (2020), based on the American Time Use Survey. We define an indicator variable that is equal to one if the spouse has a more flexible occupation than the promoted teacher.[37] The results of this analysis are displayed in columns (5) and (6) of Table 4.7, where the variable *Spousal telework* indicates that the spouse's job is more flexible. The results support that there is no differential effect on the within-couple difference in TPL when the spouse is more able to work from home. Thus, the response does not appear to be driven by the types of households with high workplace flexibility.[38]

For the second, and final, spousal characteristic, we consider whether the promoted teacher has a teacher spouse.[39] When the couple consists of two teachers, they will have the same workplace flexibility and their work will place similar demands on them, at least pre-promotion. When on the same labor market, the promotion may involve a larger shift in bargaining power

[36] In our sample, the reform pushes the teacher across the primary earner threshold in around 20 percent of cases.

[37] For data reasions, we can only include couples where we have information on the spouse's occupation (SSYK2012), included in the data from 2014. The telework measure is a constant within the couple, based on the first time we observe the SSYK2012 code for the spouse. The spouse has a more flexible occupation in 30% of cases.

[38] To test the robustness of our results, we have complemented our teleworking-measure with the one derived in Dingle and Neiman (2020) and Mongey et al. (2020), instead based on O*NET. Using these measures, we again find no statistically significant differential effect at the 10% level if the spouse has a higher ability to work from home.

[39] We define the teacher spouse in the year prior to promotion based on their occupation. In our sample, just under 20% of promoted teachers have a teacher spouse.

or comparative advantage when one person in the couple is promoted. At the same time, that the spouse does not get promoted could lead to jealousy and an unwillingness to share household chores that is strengthened by the previous similarity in their jobs. We present estimates of $\hat{\beta}$ and $\hat{\theta}$ in columns (7) and (8) of Table 4.7. The results indicate that promoted teachers who are not married to a teacher reduce their take-out of TPL by around 0.5 days relative to their partner following promotion. The relative reduction is over twice as large when the spouse is a teacher, at least for female promotions.[40] For women with teacher spouses, the woman on average takes 0.3 more days of leave than their spouse in the year prior to promotion, while women without a teacher spouse on average take 1.1 days more. Teacher couples therefore share TPL more evenly pre-promotion. The results support that the promotion reverses this relatively small gap such that the teacher spouse takes more leave than the promoted person post-promotion.

[40] Part of this effect could be driven by the primary earner effect. Around one third of teacher spouses are in couples where the promoted teacher becomes the primary earner.

Table 4.7. *Heterogeneity: Effect of promotion on within-couple difference in TPL days*

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Total	Female	Total	Female	Total	Female	Total	Female
Promoted	-1.143***	-0.841***	-0.698***	-0.555**	-0.563***	-0.523**	-0.509***	-0.441**
	(0.298)	(0.317)	(0.227)	(0.268)	(0.198)	(0.219)	(0.192)	(0.214)
Promoted × Child 7–10	0.702***	0.388						
	(0.268)	(0.306)						
Promoted × Primary earner			-0.380*	-0.272				
			(0.199)	(0.227)				
Promoted × Spousal telework					0.048	0.130		
					(0.202)	(0.225)		
Promoted × Teacher spouse							-0.304	-0.620**
							(0.190)	(0.251)
R^2	0.057	0.045	0.047	0.045	0.070	0.062	0.058	0.050
N	25,680	18,420	14,065	11,562	23,342	16,484	21,630	15,464
Child controls			Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Diff earnings t-4	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: This includes results on how the effect of promotion on the within-couple difference in normalized TPL days differs by different characteristics. It reports $\hat{\beta}$ and $\hat{\theta}$ from eqn. 4.3. Columns (1) and (2) consider whether the couple has a child aged 7–10; columns (3) and (4) show how results differ for primary earners; columns (5) and (6) consider whether the spouse has an occupation with a higher telework ability than the promoted person; and columns (7) and (8) consider whether the promoted person has a teacher spouse. Only couples with children under the age of 11 are included in the regressions. Standard errors are clustered by municipality and included in parentheses.

4.6 Conclusion

Despite women's progress in the labor market and the universal and gender-neutral family policies implemented in the Nordic countries, women continue to have the primary responsibility for child rearing. In this paper, we study the household childcare time allocation response to a policy-generated shift in household relative income. To this end, we exploit matched employer-employee data and a promotion reform in the Swedish labor market for teachers. The reform targeted skilled teachers and its main component was a pay increase of 20 percent of pre-reform mean wages. A particularly interesting feature of the reform is that it primarily affected women and secondary earners of the household.

We focus on dual-earner couples where one of the spouses was promoted due to the reform. Hence, our empirical strategy relies on the assumption that the timing of promotion is exogenous. The assumption is supported by the institutional feature that the number of promotions were rolled-out over time, constraining school districts' ability to promote skilled teachers. We also show that couple outcomes evolve in parallel in promoted vs. not-yet promoted couples prior to promotion, supporting our empirical strategy.

We find that the promotions have a pronounced and persistent impact on the wage distribution within couples: promotions of female spouses reduce a pre-existing couple gender wage gap by over 50 percent while promotions of male spouses generate a wage gap that was non-existent prior to the reform. However, since more women were promoted, the reform give rise to a within-household gender pay gap reduction by 6.4 percentage points (32 percent) and an increase in the fraction of women who are the primary earner by 9.1 percentage points (30 percent).

Our results suggest that couples respond to promotions by reallocating childcare time away from the promoted spouse relative to their partner. The conclusion also holds if we only consider couples where the female spouse is promoted, suggesting that female promotions lead to a more equal division of childcare responsibilities. The impact is stronger when couples have small children and hence when childcare demands are greater. In addition, there is evidence of a "primary earner effect", suggesting a stronger response when the promoted partner becomes the primary wage earner thanks to the reform. A possible explanation for this could be that partners' bargaining power jumps at the point when they out-earn their partner.

In light of a recent literature highlighting the role of norms in explaining the persistence of traditional gender roles in the family, our results suggest that more traditional economic explanations manifested in the labor market should not be overlooked. Labor market policies directly targeting gender pay inequalities can thus be a powerful tool in promoting a more equal division of child responsibilities between mothers and fathers.

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Appendix A: Additional tables and figures

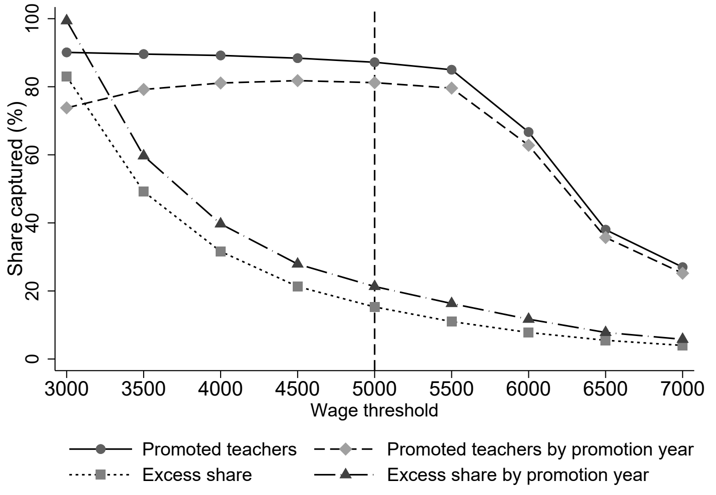
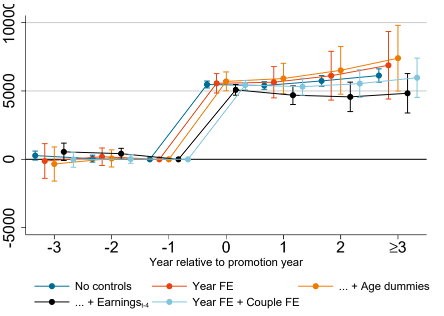
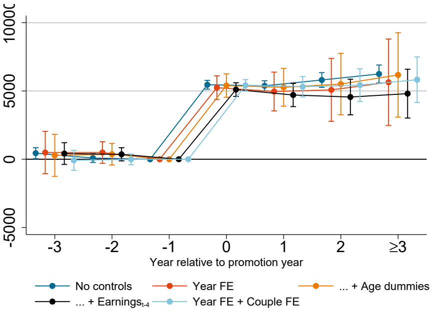


Figure A.1. Robustness: Validating proxy

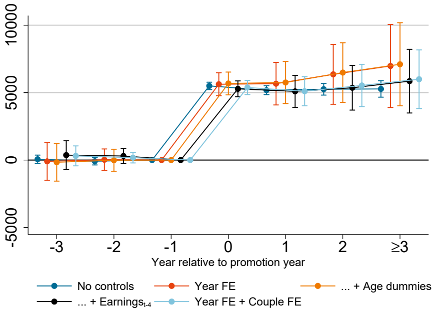
Note: Using data from the career teacher promotion registers in Grönqvist et al. (2020), we investigate the robustness of the method outlined in Section 4.3 to calculate the number of promoted teachers. The figure plots how many actual promotions that the promotion proxy captures (solid line), how many promotions the proxy captures in the correct year (dashed line), how many promotions the proxy captures that it should not capture (termed "excess share") (dotted line), and finally how many promotions in a particular year the proxy captures that it should not (dash-dot line). The denominator in all comparisons is the number of actual promotions based on the career teacher register between 2013 and 2015 for whom the promoted person has an occupational code as a teacher (i.e. the sum of all possible career teacher promotions that we can capture with our method).



(a) All promoted couples



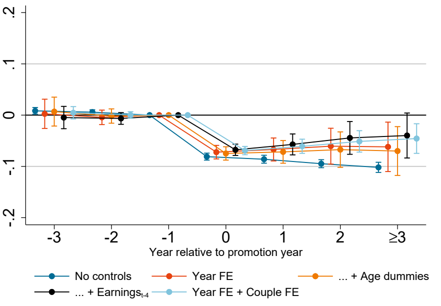
(b) Promoted women



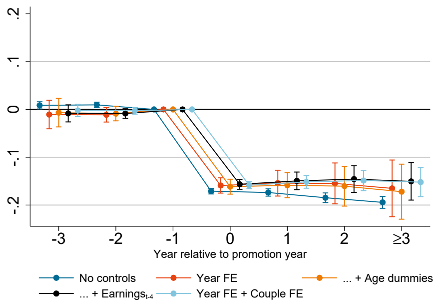
(c) Promoted men

Figure A.2. Effect of promotion on within-couple difference in wage – by gender

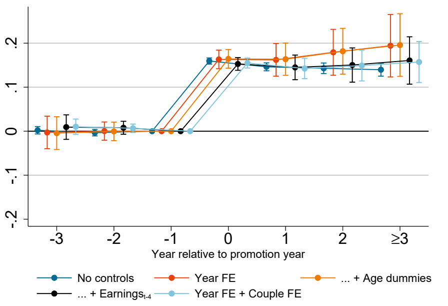
Note: The figure plots γ_t with 95% confidence intervals from estimating equation 4.2 for promoted couples, using various specifications.



(a) All promoted couples



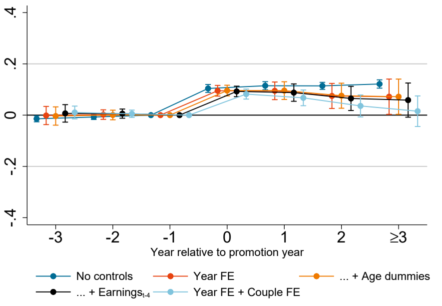
(b) Promoted women



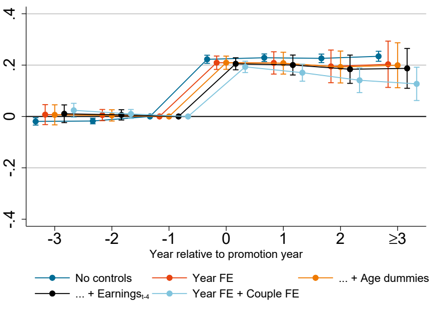
(c) Promoted men

Figure A.3. Effect of promotion on couple gender wage gap – by gender

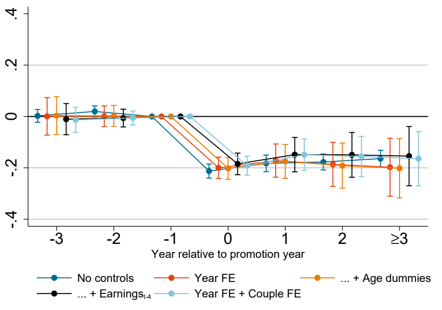
Note: The figure plots γ_t with 95% confidence intervals from estimating equation 4.2 for promoted couples, using various specifications.



(a) All promoted couples



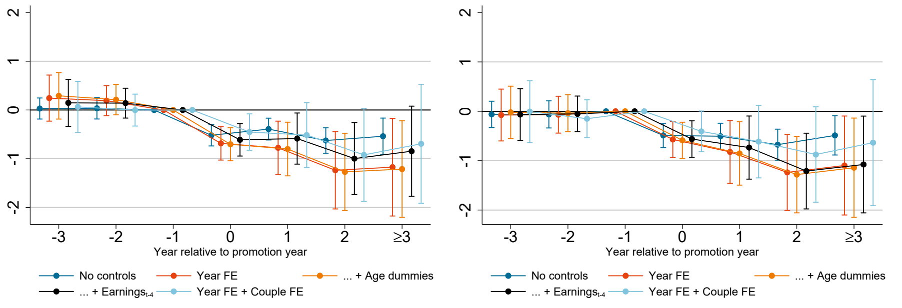
(b) Promoted women



(c) Promoted men

Figure A.4. Effect of promotion on likelihood that woman has higher wage – by gender

Note: The figure plots γ_t with 95% confidence intervals from estimating equation 4.2 for promoted couples, using various specifications.



(a) All promoted couples *(b)* Promoted women

Figure A.5. Robustness: Effect of promotion on within-couple difference in TPL days

Note: The figure plots γ_t with 95% confidence intervals from estimating equation 4.2 on the within-couple difference in normalized TPL days. Only couples with children under the age of 11 are included. The number of children aged 0–3, 4–6 and 7–10 are included as controls in all specifications that have controls.

Table A.1. *Promoted teachers and partners per calendar year*

	2007	2008	2009	2010	2011	2012	2013	2014	2015	2016	2017
Ever promoted	13,368	13,391	13,423	13,444	13,466	13,475	13,488	13,494	13,489	13,473	13,455
Couples ever promoted	9,861	9,869	9,891	9,902	9,907	9,903	9,904	9,905	9,903	9,899	9,885
Singles ever promoted	3,148	3,163	3,173	3,183	3,200	3,213	3,225	3,230	3,227	3,215	3,211
Promoted in c	0	0	0	0	0	0	3,064	10,623	13,489	13,473	13,455
Couples promoted in c	0	0	0	0	0	0	2,201	7,887	9,903	9,899	9,885

Table A.2. *Summary statistics in year before promotion – promoted women and men*

	Woman w. partner		Man w. partner	
	Mean	sd	Mean	sd
Individual characteristics:				
Female	1.00	(0.00)	0.00	(0.00)
Married	0.82	(0.38)	0.81	(0.39)
Age	44.78	(7.73)	44.77	(8.27)
Compulsory	0.00	(0.01)	0.00	(0.04)
Upper secondary	0.00	(0.05)	0.01	(0.09)
Post-secondary or higher	1.00	(0.05)	0.99	(0.10)
Education missing	0.00	(0.00)	0.00	(0.02)
Young children	0.49	(0.50)	0.59	(0.49)
Claim TPL*	0.75	(0.43)	0.74	(0.44)
TPL (100 SEK)*	39.16	(52.18)	41.36	(76.30)
TPL (normalized)*	0.19	(0.25)	0.20	(0.36)
TPL days*	3.90	(5.19)	4.11	(7.61)
Non-missing wage	1.00	(0.00)	1.00	(0.00)
Monthly wage (SEK)	29,937	(3,091)	30,660	(3,371)
Annual earnings (SEK)	340,638	(71,253)	3641,65	(70,637)
Couple characteristics:				
Difference wage	-10,542	(19,148)	-534	(8,766)
Difference annual earnings	-122,761	(295,755)	41,805	(158,127)
Difference TPL (100 SEK)*	10.51	(55.49)	-5.45	(69.54)
Difference TPL (normal.)*	0.05	(0.27)	-0.06	(0.27)
Difference TPL days*	1.05	(5.77)	-1.27	(5.57)
Share wage earned by wife	0.44	(0.08)	0.50	(0.06)
Wife has higher wage	0.26	(0.44)	0.39	(0.49)
Observations	7,843		2,065	

Note: The table shows summary statistics for promoted teachers with partners, separately by whether the promoted person is a woman or a man. Couples where both partners have been promoted, as well as same sex couples, are excluded. Young children identifies couples with children up to 10 years old. The difference variables are calculated as the promoted person minus the partner. Wife refers to females, irrespective of whether the couple is legally married. TPL variables (marked with a *) condition on having children below the age of 11.

Table A.3. *Summary statistics in year before promotion – TPL sample*

	Promoted with partner		Partner	
	Mean	sd	Mean	sd
Individual characteristics:				
Female	0.70	(0.46)	0.30	(0.46)
Married	0.81	(0.39)	0.81	(0.39)
Age	39.65	(4.28)	40.61	(4.90)
Compulsory	0.00	(0.00)	0.01	(0.11)
Upper secondary	0.00	(0.05)	0.20	(0.40)
Post-secondary or higher	1.00	(0.05)	0.78	(0.41)
Education missing	0.00	(0.02)	0.00	(0.04)
Young children	1.00	(0.00)	1.00	(0.00)
Claim TPL	0.76	(0.43)	0.62	(0.49)
TPL (100 SEK)	39.34	(52.08)	35.20	(53.51)
TPL (normalized)	0.19	(0.25)	0.17	(0.26)
TPL days	3.91	(5.16)	3.56	(5.43)
Non-missing wage	1.00	(0.00)	1.00	(0.00)
Monthly wage	29,242	(2,807)	36,725	(16,338)
Annual earnings (SEK)	326,611	(67,251)	422,778	(242,620)
Couple characteristics:				
Difference wages	-7,483	(16,272)		
Difference annual earnings	-96,167	(254,968)		
Difference TPL (100 SEK)	4.13	(57.10)		
Difference TPL (normal.)	0.02	(0.28)		
Difference TPL days	0.35	(5.79)		
Share wage earned by wife	0.46	(0.08)		
Wife has higher wage	0.31	(0.46)		
Observations	2,730		2,730	

Note: The table shows summary statistics for couples that have children below age 11, for whom a normalized measure of temporary parental leave (TPL) can be computed. Couples where both partners have been promoted, as well as same sex couples, are excluded. The difference variables are calculated as the promoted person minus the partner. Wife refers to female, irrespective of whether the couple is legally married.

Table A.4. *Teacher wage effects of promotion*

	(1)	(2)	(3)	(4)
Panel A: ln*(wage)*				
Promoted	0.211***	0.191***	0.184***	0.145***
	(0.002)	(0.002)	(0.002)	(0.001)
R^2	0.382	0.617	0.725	0.940
N	1,281,935	1,281,935	1,279,366	1,225,662
Panel B: Monthly wage (SEK)				
Promoted	7188.7***	6678.4***	6478.8***	5321.1***
	(93.2)	(89.0)	(58.8)	(62.9)
R^2	0.415	0.619	0.723	0.935
N	1,281,935	1,281,935	1,279,366	1,225,662
Year FE	Yes	Yes	Yes	Yes
Workplace FE			Yes	
Teacher FE				Yes
Controls		Yes	Yes	

Note: This table provides the results of estimating $y_{ic} = \theta Promoted_{ic} + \beta X_{ic} + \lambda_c + \varepsilon_{ic}$ among all teachers i in calendar years $c = 2007, 2008, \ldots 2017$. Controls are dummies for female, age (in five age bands) and level of education (in six categories). Standard errors are clustered by municipality and included in parentheses.

Table A.5. *Robustness: Effect of promotion on household relative wage*

	(1)	(2)	(3)
Outcome:	Wage diff. (SEK)	Couple gender wage gap	Woman earns higher wages
Promoted	5,107***	-0.062***	0.086***
	(245)	(0.007)	(0.012)
R^2	0.698	0.365	0.075
N	49,951	49,951	49,951
Mean outcome$_{-1}$	-7,780	.17	.3
Year FE	Yes	Yes	Yes
Age dummies	Yes	Yes	Yes
Diff earnings$_{-4}$	Yes	Yes	Yes

Note: This includes results on the effect of promotion on the within-couple difference in wage, the couple gender wage gap, and on the likelihood that the woman has a higher wage. It reports $\hat{\beta}$ from equation 4.1. Promotions are identified using the method outlined in Section 4.3.1, but a wage increase threshold of SEK 5,500 is used instead of 5,000. Standard errors are clustered by municipality and included in parentheses. Mean outcome$_{-1}$ shows the unconditional mean of the outcome in the year prior to promotion.

Table A.6. *Effect of promotion on within-couple difference in TPL days*

	(1)	(2)	(3)	(4)	(5)
All couples					
Promoted	-0.535***	-0.720***	-0.741***	-0.627***	-0.405**
	(0.082)	(0.187)	(0.185)	(0.180)	(0.161)
R^2	0.002	0.006	0.013	0.058	0.468
N	25,958	25,958	25,958	25,680	25,392
Mean outc.$_{-1}$	.35				
Promoted women					
Promoted	-0.494***	-0.588***	-0.611***	-0.573***	-0.372*
	(0.090)	(0.200)	(0.200)	(0.201)	(0.190)
R^2	0.001	0.010	0.012	0.050	0.463
N	18,581	18,581	18,581	18,420	18,134
Mean outc.$_{-1}$	1.02				
Promoted men					
Promoted	-0.394**	-0.708**	-0.724**	-0.593*	-0.344
	(0.176)	(0.350)	(0.349)	(0.353)	(0.335)
R^2	0.003	0.009	0.014	0.032	0.449
N	7,377	7,377	7,377	7,260	7,258
Mean outc.$_{-1}$	-1.26				
Child controls		Yes	Yes	Yes	Yes
Year FE		Yes	Yes	Yes	Yes
Age dummies			Yes	Yes	
Δ earnings$_{-4}$				Yes	
Couple FE					Yes

Note: The table includes results on the effect of promotion on the within-couple difference in normalized TPL days (defined as promoted person minus partner). It reports $\hat{\beta}$ from equation 4.1. Only couples with children under the age of 11 are included in the regressions. Child controls are number of children aged 0–3, 4–6 and 7–10. Age dummies are promotion/partner-specific age dummies in five-year bins. The difference in earnings shows the within-couple difference in annual earnings four years prior to promotion. A dummy is included to capture data more than three years prior to promotion in all models. Standard errors are clustered by municipality and included in parentheses. Mean outcome$_{-1}$ shows the unconditional mean of the outcome in the year prior to promotion.

Table A.7. *Robustness: Effect of promotion on within-couple difference in TPL*

	(1)	(2)
	Total	Female
Panel A: Exclude no TPL		
Promoted	-0.711***	-0.696***
	(0.207)	(0.239)
R^2	0.075	0.067
N	21,852	15,504
Mean outcome$_{-1}$	.41	1.22
Panel B: SEK 5,500 wage threshold		
Promoted	-0.606***	-0.548***
	(0.186)	(0.210)
R^2	0.058	0.049
N	24,613	17,569
Mean outcome$_{-1}$	.33	.99
Panel C: TPL (100 SEK)		
Promoted	-4.183***	-3.342**
	(1.390)	(1.396)
R^2	0.026	0.065
N	52,521	40,340
Mean outcome$_{-1}$	6.3	10.06
Child controls	Yes	Yes
Year FE	Yes	Yes
Age dummies	Yes	Yes
Diff earnings$_{-4}$	Yes	Yes

Note: This includes results on the effect of promotion on the within-couple difference in TPL, defined as promoted person minus partner. It reports $\hat{\beta}$ from equation 4.1. Only couples with children under age 11 are included. In Panel A and B, the outcome is the difference in normalized TPL days, but the sample differs from the main analysis. In Panel A, we further restrict the sample by excluding couples where neither parent takes TPL. In Panel B, promotions are identified using the method outlined in Section 4.3.1, but a wage increase threshold of SEK 5,500 is used instead of 5,000. In Panel C, couples with children under age 11 are included. The outcome is the difference in TPL, measured in hundreds of SEK. Standard errors are clustered by municipality and included in parentheses.

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