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Decentralization of wage determination

Evidence from a national teacher reform

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IFAU INSTITUTE FOR EVALUATION OF LABOUR MARKET AND EDUCATION POLICY

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Decentralization of wage determination[a]

Evidence from a national teacher reform

by

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Abstract

Despite a global trend of wage decentralization over the past 30 years, we know very little about the labor market implications of decentralized wage determination. A main reason is the lack of exogenous variation in wage regulation linked to detailed outcome data. Using Swedish registry data and exploiting a reform that replaced the fixed national pay scale for teachers with individual wage bargaining, I overcome these issues and provide new evidence on the labor market effects of wage decentralization. The paper presents three sets of empirical results. First, I show that the reform significantly changed the wage structure of teachers. Second, I demonstrate that these wage changes did not affect teacher composition or student outcomes. Finally, I find support for a wage spillover effect to substitute occupations, providing evidence on the dynamics of wage determination across occupations. I argue that the wage spillover effect coupled with the compressed Swedish wage structure likely explains the lack of effects on teacher and student outcomes.

Keywords: Wage Regulation, Decentralization, Teacher Labor Market

JEL-codes:I20, I28, J31, J45

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1 Introduction

Even though centralized wage-setting remains a prevalent feature of public sector labor markets, most OECD countries have undergone a process of wage decentralization over the past 30 years.[1] This may have important implications for wage formation, occupational decisions and labor productivity. However, a lack of exogenous variation in wage regulation linked to detailed outcome data has made it difficult to examine the labor market implications of decentralized wage determination, and our knowledge on this topic is very limited.

In this paper, I use Swedish registry data to evaluate a reform that replaced the fixed national pay scale for teachers with individual wage bargaining. This allows me to empirically examine the labor market effects of decentralized wage determination. Exploring this question in the teacher labor market is particularly interesting, because relative teacher pay has declined monotonically since 1940 (Hanushek and Rivkin 2007), and it has become increasingly difficult to recruit and retain a sufficient stock of qualified teachers (OECD 2015; Corcoran et al. 2004; Hoxby and Leigh 2004).[2] Wage decentralization has been proposed as a potential solution to these negative trends, and this reform can therefore have large implications for improving school quality (Björklund, Clark, Edin, Fredriksson and Krueger 2005).

The key empirical challenge associated with isolating the effects of the reform is that it was implemented in the whole country at the same time. I overcome this issue by exploiting the fact that the pre-reform wage scale was fixed across the entire country and did not account for local labor market conditions, such that the regulated pay was relatively worse in areas with strong local labor markets. The wage response to the policy should therefore be proportional to the outside wage. My main empirical strategy consists of using pre-reform variation in college-educated non-teacher (CENT) employment income across local labor markets (LLMs) as a measure of treatment intensity in a difference-in-difference framework.[3] While this strategy enables me to evaluate the reform and provide

[1] Between 1970 and 1990, the majority of OECD countries moved towards greater wage decentralization (OECD 2004). One exception to this was Norway, which witnessed a re-centralization of certain industries in the 80s (Kahn 1998).

[2] In Sweden, there was a substantial fall in relative teacher pay from 1950 to 1977. Since 1977, relative teacher pay has been held fairly constant, though there is some evidence of a slight decrease in the 80s and early 90s. See Persson and Skult (2014).

[3] This method is similar to Card (1992), where cross-state variation in fraction of individuals that earn less than the 1990 Federal minimum wage is used as a measure of treatment intensity in a difference-in-difference model to examine the effect of wage floors. More directly related studies that rely on similar research designs are Britton and Propper (2016) and Propper and Van Reenen (2010). See Section 3.

new evidence on how wage decentralization affects outcomes, it should be noted that it does not allow me to identify the overall national effect.[4]

My main identifying assumption is that there are no secular trends, policies or shocks that affect outcomes differently depending on the area's pre-reform CENT employment income. I show extensive evidence that the data are consistent with this assumption. First, I show that the results are robust to the inclusion of an extensive set of fixed effects and controls for other factors that may be correlated both with the pre-reform CENT employment income and the outcomes. Second, I provide event studies showing that pre-reform trends in outcomes did not differ across municipalities with different 1995 CENT incomes. Third, I provide evidence that other reforms that took place in the years surrounding the decentralization reform do not drive the results. Fourth, I show results from rerunning my main specification for occupational groups that can be assumed to have been isolated from, and unaffected by, the reform. Fifth, I show that the 1995 CENT income level is uncorrelated with the change in CENT income over the analysis period. Finally, I show that the results are robust to the way in which the treatment measure is constructed, and that alternative measures produce similar estimates.[5] Results from all these tests are inconsistent with plausible sources of bias from secular shocks or trends and support a causal interpretation of my estimates.

My first empirical result is that the reform induced significant changes in wage structure in regions that had a higher pre-reform non-teacher employment income relative to regions with a lower pre-reform non-teacher employment income. In terms of effect size, I find a long-run teacher wage response elasticity of 0.2 with respect to pre-reform CENT income.[6] This implies that a region with a 10 percent higher 1995 CENT income than another region experienced a 2 percent higher teacher pay increase due to the reform. The magnitude of this effect is negatively related to teacher age, such that the reform led to a disproportionate increase in entry wage and a flattening of the age-wage relationship. The analysis further identifies modest increases in wage dispersion among young and mid-career teachers.

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[4] Specifically, any nation-wide level shifts are subsumed by the year fixed effects that I include in my main specification. Thus, the results should be interpreted as the effect of the reform across regions that had higher pre-reform non-teacher employment income relative to regions with lower pre-reform non-teacher employment income.

[5] One of these measures involve first estimating Mincer earnings functions (in which wage is modeled as a function of years of schooling, potential experience and potential experience squared) for non-teachers (separately by gender) in the year prior to the reform (controlling for municipality fixed effects) and then using the estimated values from these regressions to predict what the wages of teachers would be had they not been teachers.

[6] Using data on school spending, I show that the wage effects are entirely driven by a reallocation of existing education resources.

The second empirical result is that the identified wage effects do not impact teacher retention, recruitment, or composition, and do not affect student outcomes – as measured both by short-term educational achievement results and long-term labor market outcomes.[7] These results may partly be explained by the relatively compressed Swedish wage structure: the 0.2 response elasticity only translates into modest absolute pay differences across municipalities post the reform, and these may not be sufficiently large to overcome existing search and matching frictions and mobility costs.

The third set of results provides support for a wage spillover effect to substitute occupations, and presents another explanation to why the reform did not impact teacher composition and student outcomes. Specifically, the results show that wages in occupations that can be considered close substitutes to teaching (measured by the occupational mobility of teachers in and out of these occupations prior to the reform) also were affected by the reform. The reform has no impact on wages in non-substitute occupations. The spillover effect occurs almost contemporaneously with the teacher wage effect, and this result provides new evidence on the dynamics of wage determination across substitute occupations.[8]

Taken together, my results provide little evidence to suggest that decentralization of wage determination is associated with any substantial labor market effects, despite its impact on wages. However, the results do suggest that wage regulation changes in one occupation may have important spillover effects in other occupations. I underscore that my estimates do not capture any overall effects that impact all municipalities in the same way. I also note that the effects may be different in a country with less wage compression.

The rest of this paper is organized as follows: Section 2 provides a brief overview of the Swedish education system, describes the reform, and offers a theoretical discussion on the potential effects of the policy; Section 3 reviews the literature; Section 4 describes the data and empirical strategy; Section 5 presents baseline results; Section 6 examines mechanisms; Section 7 discusses results from diagnostic tests and robustness checks; and Section 8 concludes.

[7] For example, I can rule out both positive and negative GPA effects larger than 0.09 national percentile ranks from a 10 percent increase in pre-reform CENT income.

[8] This result is particularly interesting from a policy perspective as one argument for centralized wage-setting has been that it can curb inflationary pressure by restricting wage competition within and across industries (Iversen 1996).

2 Institutional background

2.1 The Swedish education system

The Swedish education system consists of nine years of tuition-free comprehensive compulsory education starting at age 7, with the curriculum set by the central government. Following the completion of compulsory school, each child has the right to three years of tuition-free upper secondary school. In 2013, 98 percent of students that finished compulsory school continued to upper-secondary school (Skolverket 2014). Primary and secondary education is funded primarily through a local municipality tax (70 percent) as well as through earmarked (5 percent) and non-earmarked (15 percent) grants from the national government.

The majority of students attend public institutions; during my analysis period less than 1 percent of students attended private tuition-charging schools (Böhlmark and Lindahl 2015) and no more than 8 percent were enrolled at charter schools (Statistics Sweden 2006).[9] Children can choose to enroll at any school provided that space is available. However, individuals residing closest to the school are given priority at the grade school level, and proximity remains the main principle for allocating students to compulsory schools (Böhlmark and Lindahl 2015).

Since 1990, municipalities hold full financial responsibility for 1-12 education, though substantial cross-municipality cooperation exists at upper-secondary school. To teach, an individual must hold a teaching certificate. Prior to 2011, this certificate was obtained through a common university examination for all teachers. In 2011, this exam was replaced with four specialized programs based on which grades and subjects the teacher desires to teach. A school is not allowed to hire an unqualified teacher if there is a certified teacher available for the position. In the event that no certified teacher is available, and a non-certified teacher is hired, such a person can only be hired on a temporary one-year contract.[10] Teacher pay negotiations remained centralized until the elimination of the pay scales in 1996. Decisions related to non-pecuniary benefits and work conditions remained at the national level throughout the analysis period.

[9] Private sector teachers are not included in this analysis.
[10] After the termination of the temporary contract, the school must open the position for new applicants. If there still is no certified teacher available, the school may re-hire the non-certified teacher on a temporary basis.

2.2 Decentralization of teacher wages

A main feature of the Swedish labor market in the post-war era was its “solidarity wage policy” – equal pay for equal work (Edin and Holmlund 1995). This policy was pursued through wage bargaining between peak associations (associations of industries or groups with shared interests or goals) of workers and employers (Iversen 1996). For employers, centralized bargaining ensured overall wage restraint and reduced competition for certain workers (Karlson and Lindberg 2011). For employees, it provided an egalitarian wage distribution and guaranteed stable wage increases over time (Karlson and Lindberg 2008). This feature of the labor market induced substantial pay compression across and within occupations: a 30 percent pay increase would move a worker from the bottom to the top decile of the distribution (Hibbs and Locking 1995). For an industrial worker in the US, the same move required a 400 percent pay increase (Hibbs and Locking 1995).

Concurrently with other OECD countries, Sweden moved away from its egalitarian and centralized wage-setting system in the late 70s (Granqvist and Regnér 2008). The transition toward individualized wage-setting began in the private sector as employers started questioning the centralized system: it gave little room for local and individual wage variation and made it difficult to recruit and retain high-quality workers (Granqvist and Regnér 2008). This idea soon gained support within the public sector as well, and Sweden is currently considered to have one of the most decentralized public sector wage-setting system in Europe (Ibsen et al. 2011).

The teacher pay decentralization reform was implemented in 1996, much later than similar reforms in other public sector occupations in Sweden.[11] Prior to the reform, wages among primary and secondary teachers were determined by national pay scales based on type of teaching: (1) primary level, (2) lower-secondary level, (3) music/art/sport, (4) general subjects at the upper-secondary level, (5) vocational subjects at the upper-secondary level, and (6) lectureship. These scales determined entry wage, increases in wages with experience, and the maximum wage that could be received.[12] Figure 1 depicts the pay scale for teachers in 1990. On average, teachers enjoyed wage increases every 18 months for 15 years and then yearly for 5-8 years. After 20-23 years, a common wage ceiling was reached. Through negotiations between the government and the

[11] For example, both nurses and doctors were introduced to individual wage-setting in 1989 (Calmfors and Richardson 2004).
[12] For non-certified teachers, the monthly wage was traditionally $200 less than the wage dictated by the pay scale (Söderström 2006).

central teacher unions (*Lärarnas Riksförbund* and *Lärarförbundet*), these scales were subject to upward shifts every 6 to 12 months (Söderström 2006).

There were two exceptions to the wage scales: teachers could receive small premiums for non-teaching duties (e.g. being responsible for gym equipment) and teachers in subjects that suffered from teacher shortages could receive small bonuses (Söderström 2006). Deviations from the pay scales based on these exceptions were very uncommon at the primary and lower secondary level – the focus of this analysis – and were generally considered restricted to teachers at the high school level. However, even at this level deviations were uncommon, and the single salary schedule should be considered deterministic prior to the reform (Söderström 2006).[13]

The decentralization reform was the result of careful negotiations between the two national teacher unions and the employer organization of teachers. Even though the employer organization was the main proponent of the reform, both unions were dissatisfied with the salary schedule because they felt it was unfair and led to compensation levels that were too low in general (Söderström 2006). Following the reform, wages were determined through annual negotiations between individual teachers and principals. The transition to individual wage-setting was gradual, and the new labor contract contained certain limited wage guarantees: a \$50 general increase for 1996 (an additional \$39 was given to teachers that had reached the ceiling) and a minimum wage to teachers that had worked 1 and 5 years respectively beginning in 1997 (Söderström 2006). This contract should therefore be seen as a transition contract.[14] The teacher labor contract was replaced again in 2000, at which point the transition was complete and the limited guarantees removed.

2.3 Theoretical predictions

Following the existing literature on the labor market effects of centralized wage-setting (Britton and Propper 2016; Cardullo 2015; Propper and Van Reenen 2010; Cappelli and Chauvin 1991), consider a dual-region model with region = $\{L, H\}$. Assume that the only difference between these regions concerns labor productivity, which is held constant across industries. Let L represent the low-productivity region and let H depict the high-productivity region. The

[13] My analysis focuses on compulsory school. The results for high school teachers are similar but noisier. This is discussed in Section 4.

[14] In addition to the limited guarantees, the transition contract had very few restrictions on the advisory role that local unions could play with respect to individual wage negotiations, and the pace of adoption of individual wage-setting is likely to differ depending on the intensity of the advisory role that local municipalities assumed with respect to the individual wage negotiations. Further, it may have taken time for both teachers and principals to develop bargaining skills and become comfortable with negotiating over wages.

unregulated non-teacher wage will be lower in *L* due to the lower productivity of the region.[15]

At any given teacher wage, the difference in non-teacher pay across regions will cause teacher supply to be higher in *L* than in *H*. To equalize local supply and demand, the teacher wage would need to differ across the regions. This is not possible under centralized wage-setting, and the regulated pay creates a wedge between it and the local equilibrium wage that would prevail in the absence of the wage control. Figure 2 provides a visual depiction of this scenario.[16]

The resultant local labor market disequilibria may have negative effects on labor supply in *H*. First, teaching will be more appealing in *L* since the relative wage of teachers (compared to non-teachers) is higher in *L*, and this may lead more, and more productive, workers to sort into teaching in *L*. Second, worker productivity may be higher in *L* because a higher relative wage induces greater worker effort (Shapiro and Stiglitz 1984) and improves worker morale (Akerlof 1982).[17] Third, the unemployment rate of teachers may be higher in *L* since teacher supply is higher in that region.

In the event of wage decentralization, it is likely that local wage-setters start paying wages that better align with local competitive equilibrium pay. Such wage changes may impact both the supply and composition of teachers across regions that had higher pre-reform non-teacher wages relative to regions with lower pre-reform non-teacher wages. In turn, this could affect student outcomes.

A limitation of the above discussion is that it only considers how pay regulation affects wage levels. An equally important aspect of centralized wage-setting is that it restricts the return to skill in the profession. Under wage decentralization, observed and unobserved individual characteristics that are more representative of productivity may become more likely to enter the wage equation. This could raise the return to skill in the profession and lead to an increase in the quality of individuals sorting into the profession (Dahl et al. 2013). This follows from the Roy model, which predicts occupational choice to

[15] This discussion assumes that there is not perfect geographic mobility across municipalities in Sweden. This is a fairly innocent assumption to make, especially in a country like Sweden where less than 4 percent of the population moves across municipalities in any given year (SCB 2017). Teachers are predominantly female and secondary household earners, making it even less likely that this assumption is violated.

[16] In Figure 2, the centralized wage falls right between the local equilibrium wages in *H* and *L*. This is not a necessary for the validity of the identification strategy used in this paper.

[17] The centralized wage may not only have undesired consequences in *H*-type districts. In *L*-type districts the centralized wage acts as a price floor, and both teacher supply as well as teacher quality may exceed the optimal amount in these regions. The effect of the reform in these districts should be a relatively slower average growth rate of teacher wages over time (as sticky wages coupled with institutional features make it unlikely that these districts can cut teacher wages) and thus a relative reduction in teacher supply and quality.

be a function of not only the relative wage, but also of the return to skill in the profession and the complementarity of skills across professions.[18]

Three caveats accompany the above predictions. First, local governments face budget constraints, and their ability to raise teacher wages will depend on their capacity to raise funds and reallocate resources. Thus, the magnitude of the reform effect on teacher pay will be a function of the ability of municipalities to reallocate resources toward teacher wages.

Second, even if local governments can obtain the necessary funds, the way in which they obtain these funds may directly impact how the reform affects teacher and student outcomes. In theory, there are four ways through which municipalities can finance an increase in teacher pay: (1) lowering the teacher-student ratio, (2) reallocating resources across educational inputs, (3) raising the local income tax, or (4) redirecting resources from other parts of the public sector. If municipalities rely on (1), (2) or (3), the predicted effects of wage decentralization on teacher composition and student outcomes could be muted (and differentially so depending on which of these alternatives the municipalities choose).[19]

Finally, an increase in teacher wage makes it harder for firms outside the education sector to recruit and retain individuals with teaching qualifications. Further, it may induce individuals with non-teaching careers to consider teaching. This puts upward pressure on wages in these other industries. Given the size of the teacher work force, the wage spillover effects are likely non-negligible. Such spillovers would reduce any relative teacher wage effect and the impact of the reform on teacher and student outcomes. This part of the analysis is therefore imperative for understanding the net effect of the reform.

Taken together, the above discussion predicts that the reform will encourage local wage-setters to pay wages that better align with local market conditions. Such wage effects may have an impact on both teacher composition and student outcomes in areas with high CENT incomes relative to areas with low CENT incomes. However, any wage response to the reform represents an increase in the cost of teachers, and there may be spending and resource allocation effects that obscure these predictions. Potential general equilibrium and wage spillover

[18] An unconditional increase in wage level – not accompanied by an increase in pay dispersion or return to skill – should raise the supply of workers across the whole skill distribution. If employers are unable to differentiate between these types when making hiring decisions, there may not be a change in teacher composition following such an unconditional wage level increase. However, even in such cases, there are theoretical reasons for why teacher productivity – and thus indirectly student outcomes - may increase. See de Ree et al. (2018).

[19] If they rely on (4), the decentralization reform may negatively impact other public sector occupations. However, the results presented in Section 6 provide no evidence of municipalities resorting to this option.

effects add to the difficulty of anticipating the likely consequences of the reform on teacher composition and student outcomes.

3 Prior literature

The central challenge facing the existing wage decentralization literature is a lack of exogenous variation in pay-setting regulation. The majority of research on this topic is therefore cross-sectional, leveraging variation in individual wage-setting power across and within industries at a given point in time (Daouli et al. 2013; Fitzenberger et al. 2013; Granqvist and Regnér 2008; Fitzenberger et al. 2008; Plasman et al. 2007; Dell'Aringa and Pagani 2007; Card and de la Rica 2006; Gerlach and Stephan 2005; Cardoso and Portugal 2005; Rycx 2003; Dell'Aringa and Lucifora 1994a). This literature suggests that decentralization is associated with a large wage premium. Card and de la Rica (2006), for example, estimate the premium to be about 10 percent. However, the literature fails to reach a consensus on how wage dispersion is affected.[20]

A major limitation with the cross-sectional studies is the likely existence of unobserved heterogeneity in worker productivity. If pay decentralization affects wage structure, then there may be sorting across occupations with and without local wage bargaining that bias the results. For example, if local wage bargaining generates increased wage dispersion and raises the wage level, then high ability workers may self-select into occupations with local wage bargaining. This would lead to an upward bias of the effect of wage decentralization on pay level. More recent studies have accounted for this heterogeneity by using longitudinal employer-employee matched data and find that the pay premium becomes smaller (2-4 percent) – but remains statistically significant – once this heterogeneity is accounted for (Andréasson 2014; Dahl et al. 2013; Gurtzen 2007). These papers focus on the wage effect of private sector pay decentralization caused by a general shift in employer attitude. It is not clear that the wage effect of a government-mandated national decentralization reform in the public sector should be the same.

The papers most closely related to the current analysis are Britton and Propper (2016), Biasi (2017), and Söderström (2006). Britton and Propper (2016) study the effect of centralized wage-setting on education quality. To examine this question, the authors exploit the wage gap between the regulated teacher wage and the non-regulated outside wage across regions in the UK to analyze the effect

[20] Dell'Aringa and Lucifora (1994b) find that wage dispersion goes down, Card and de la Rica (2006) and Rycx (2003) find that wage dispersion goes up, and Plasman et al. (2007) and Dell'Aringa and Pagani (2007) find mixed results.

of centralized wage-setting through a difference-in-difference design.[21] They find that a 10 percent increase in the wage gap reduce school performance in key compulsory school exams by 2 percent.

While it is important to identify the labor market effects of centralized wage-setting, it is equally important to understand how the elimination of such pay schedules impact teacher composition and student outcomes. This cannot be inferred from Britton and Propper, because decentralization may affect not only relative wage, but also wage dispersion, spending, resource allocation, teacher-student ratios and local taxes. Further, it may have wage spillover effects to other occupations. Thus, it is unlikely that the effect of decentralization is symmetric to the effect of centralized wage-setting.

Biasi (2017) studies the effects of Wisconsin's 2011 Budget Repair Bill. This bill aimed to resolve the $3.6 billion deficit that the state was facing by changing several public sector employee regulations within the areas of collective bargaining, compensation, health insurance and sick leave. One change was that districts now could choose to negotiate individual wages with teachers. By comparing teacher in districts that chose individual bargaining to teachers in districts that decided against it, Biasi finds that pay dispersion increased in districts that chose individual bargaining and that this increase is positively correlated with teacher value-added.

An important distinction between Biasi (2017) and this study is that I investigate the effects of a government-mandated national reform, while the analysis in Biasi is based on voluntary adoption of individual wage-setting by districts made possible through a Bill that affected much more than the salary schedule of teachers. While both settings are interesting, my analysis is more likely to isolate the effect of wage decentralization. Further, the Swedish registry data permit a more comprehensive analysis of the labor market effects of wage decentralization. For example, these data provide information on which occupations teachers come from and to which occupations they leave, allowing me to examine both sorting and wage spillover effects. In addition, I can examine how the reform affects the long-run outcomes of students.

The third paper closely related to the current analysis is Söderström (2006), which uses a difference-in-difference framework to compare the wages of Swedish teachers to that of other public sector employees before and after the teacher wage decentralization reform. The results suggest that the reform led to an increase in entry-wage, that the age-earnings profile became flatter and that

[21] Rather than exploiting changes in bargaining regime, they use wage shocks in the rest of the economy. Propper and Van Reenen (2010) rely on the same identification strategy to study the effect of centralized wage-setting on the quality of nurses. They find that the number of hospital deaths for acute myocardial infarction is 6 percent higher in regions where the outside wage is 10 percent higher.

dispersion increased for old teachers. A limitation with Söderström (2006) is that the paper only examines the wage effect of the reform. I contribute to this literature by examining how the reform affects the composition of teachers and the outcomes of students, and by investigating the mechanisms through which these effects operate. It is also important to note that I find significant wage spillover effects associated with the reform, which suggests that the results in Söderström (2006) may be attenuated since his control group also was affected by the reform.

My paper is also related to several strands of the education economics literature. One of these strands focuses on the effect of wages on teacher supply. Results from these studies are in line with conventional labor theory (Guarino et al. 2006): wage is positively associated with retention and inversely related to attrition (Clotfelter et al. 2008; Imazeki 2005; Podgursky et al. 2004; Hanushek et al. 2004; Stockhard and Lehman 2004; Lankford et al. 2002; Kirby et al. 1999; Weiss 1999; Brewer 1996), improving teacher pay raises a district's ability to recruit quality teachers (Leigh 2012; Figlio 2002), and higher wages lead to an increase in teacher supply (Falch 2010). If the reform leads to higher wages, these studies suggest it will improve teacher quality. However, since the reform also may affect other components of the education system (e.g. through a reallocation of resources) and broader labor market, this prediction may not come true.

Another body of research investigates the effect of teacher wages on student outcomes. The earlier studies within this field fail to identify statistically significant effects, suggesting that teacher pay does not affect student outcomes (Hanushek 1997; Grogger 1996; Betts 1995; Altonji 1988). In an influential paper, Loeb and Page (2000) offer another explanation: the earlier papers are unable to isolate the effect of interest because they fail to account for alternative labor market opportunities and non-pecuniary school characteristics. Accounting for these factors, Loeb and Page (2000) finds that a 10 percent increase in teacher pay reduces high school dropout rates by 4 percent and increases college enrollment by 1.6 percent. More recent papers have found similar effects (e.g. Hendricks 2014; Dolton and Marcenaro-Gutierrez 2011). If decentralization leads to an increase in teacher pay, this strand of research would predict the reform to improve student educational attainment.[22] However, this assumes that the reform does not affect other components of the education sector and broader labor market in ways that offset the effects of higher wages.[23]

[22] One exception to this is de Ree et al. (2018), which finds that a doubling of teacher wage in Indonesia has no effect on student outcomes.

[23] There is also a literature on how teacher supply affects student outcomes. The results from these studies are in line with theoretical predictions: teacher turnover has a disruptive impact on student performance,

To summarize, existing research largely agrees on the effect of individual wage-setting on the level of pay, but fails to reach a consensus on how it affects wage dispersion. There is no research on the effects of decentralization on spending and resource allocation, or on whether decentralization in one occupation has spillover effects to other occupations. Thus, even though existing research produces relatively clear predictions on the supply response to wage changes, this is not sufficient for identifying the labor market effects of wage decentralization.

4 Data and estimation strategy

4.1 Data

The main analysis relies on registry data from 1991 to 2006 drawn from the Institute for Evaluation of Labor Market and Education Policy database, originally collected by Statistics Sweden. The first registry is the *Teacher Registry*, which contains annual information on all teachers in Sweden (workplace, contract, subject taught and if they are on leave).[24] I complement these data with the *Wage Registry for Public Sector Employees*, which contains data on wage and occupation, as well as work hours, for every public sector employee. I further use the *Teacher Education Registry*, which provides data on the education level of each teacher.

Although the reform affected all teachers in grades 1 through 12, I focus on teachers in grades 1 through 9. I impose this restriction due to certain institutional features. First, several municipalities are members of cross-municipality groups that share the responsibility of providing upper-secondary education. These groups vary in size, across time and with respect to the specific agreements. This means that wage decisions are not made at the municipality level, but at the group level through negotiations between the involved municipalities, and those municipalities change over time.[25] Second, as discussed in Section 2, the two exceptions to the wage scales were more common at the high school level.

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experienced teachers have a large positive effect on student achievement, and high quality teachers have a positive impact on student outcomes (Chetty et al. 2014; Ronfeldt et al. 2011; Staiger and Rockoff 2010; Clotfelter et al. 2007; Boyd et al. 2006; Rivkin et al. 2005; Rockoff 2004). If the reform positively impact teacher supply, these studies would anticipate positive effects on student outcomes. However, it is unlikely that the reform only affects teacher supply, making it inappropriate to hypothesize the effects of the reform based only on this strand of research.

[24] The data do not include individuals over age 65. However, less than 2.5 percent of teachers were 65 in any of the analysis years (Hansson 2015).

[25] It is also the case that municipalities with very few students do not have high schools and pay for their students to attend high schools in larger municipalities nearby in which they have no control over wage decisions.

However, the results for high school teachers are similar to those for elementary school teachers, though the standard errors are larger.[26]

Excluding teachers in leadership roles, there is an average of 85,000 public elementary school teachers per year between 1991 and 2006. The first two columns of Table 1 provide summary statistics of key characteristics for teachers and non-teachers. Table 1 shows that teaching in elementary schools is female-dominated - only 26 percent of teachers are male. This is consistent with the gender composition of teachers across the industrialized world. The table also shows that the teacher workforce is relatively old. The mean age is 45.4, compared to 41.3 in other sectors. Another feature of the teacher labor market is that teachers are highly educated. The average years of schooling among elementary school teachers is 14.9. This is higher than that of non-teachers (11.6). Finally, almost 64 percent of teachers are married, and 59 percent have at least one child under the age of 18. These numbers are noticeably higher than the averages in other sectors, which suggests that geographic labor mobility may be lower among teachers than among workers in general.

Table 1 also provides information on these characteristics for specific time-periods: the pre-reform period (91-95), the immediate post-reform period (96- 00) and the more distant post-reform period (01-06). Some trends are worth noting. First, there is a reduction in the fraction of teachers on permanent contracts, fewer teachers hold teaching certificates and they work fewer hours. Second, the family composition of teachers appears to change, with fewer teachers getting married and having children. In terms of wage, there is a large increase in the wage level.

In Section 2, I note that centralized wage-setting prevents local wage-setters from paying the local competitive equilibrium wage and that this may negatively impact teacher composition in municipalities with high non-teacher pay. To examine if my data support this hypothesis, Table 2 compares the teacher composition in municipalities in the bottom decile of the CENT income distribution with that of teachers in municipalities in the top decile in the year prior to the reform.

Table 2 suggests that the composition of teachers in municipalities in the top decile of the CENT income distribution is significantly different from that in municipalities in the bottom decile. Specifically, teachers in these municipalities are 3 percent less likely to remain in the profession, are 0.8 years older, are 7 percent less likely to hold a teacher certificate, have 0.25 fewer years of schooling and are 4 percent more likely to be female. Although simple

[26] Results available upon request.

differences in means cannot be used for causal inference, the results are consistent with both theory and prior literature.

The rich teacher data permit a detailed analysis of how the reform affected teacher composition. The outcome characteristics I look at include age, years of schooling, master's degree, immigrant status, gender, the probability of being on leave, the probability of switching from private to public school, the probability of being on a temporary contract, hours worked (as a percent of a 40 hour work week), certification status, the probability of moving to a different municipality, the probability of moving to a municipality with a higher pre-reform CENT income, retention and recruitment. Unfortunately, it is not possible to construct value-added measures with Swedish registry data since teachers cannot be linked to specific students or classes.[27] Panel A of Appendix Table A1 shows summary statistics of the teacher composition variables.[28] Despite the high education level of teachers, the monthly mean wage of teachers is low ($2,806), and Swedish teachers are in the left-tail of the OECD teacher pay distribution (OECD 2016).[29]

I also use the *Longitudinal Database for Education, Income and Labor Market Participation* (*LOUISE*) registry. These data contain annual socioeconomic and demographic information on all Swedish residents between the ages of 16 and 65. The data include education, labor market, income and welfare program participation information. I use these data for three purposes. First, to obtain municipality covariates that, if omitted from the model, could confound the identified effects. Second, to obtain labor market outcomes of the students that were exposed to the reform while in compulsory school. Finally, to obtain the treatment variable – pre-reform employment income of college-educated non-teacher individuals across LLMs. This variable differs from the teacher wage variable as it includes compensation for sick leave and for commuting to and from work.[30] This choice is unlikely to affect the results as

[27] The only teacher skill measure in the Swedish registry data is the GPA teachers use to apply to high school with. I do not have access to this data for most of the sample. Further, prior literature suggests that there is no correlation between student test scores and their teachers' college GPA (D'Agostino and Powers 2009); it is unlikely that an analysis using teacher elementary GPA would benefit this paper.

[28] The wages are expressed in real 2005 values.

[29] Raw wage comparisons are misleading as there are important cross-country differences in working conditions. For example, Swedish teachers had less than two-thirds as many teaching hours as teachers in Switzerland, and were responsible for half as many children as teachers in Ireland, where wages were much higher during the analysis period (OECD 1996).

[30] I rely on this measure because the *Wage Registry for Private Sector Employees* is a survey based on a random sample that covers less than 50 percent of private employees, suffers from nonrandom nonresponses and is subject to a stratification method that leads to a disproportionate loss of observations in small municipalities. The measure of CENT employment income is based on information reported to the national tax authority, and excludes self-employment income. This is important, as including self-employment income would complicate the analysis.

the correlation between employment income and wage is greater than 0.9 among college-educated non-teachers that are present in both registries.[31]

I use Statistic Sweden's classification of local labor markets. A municipality is considered the center of a LLM if less than 20 percent of its working-age population commute to a job outside the municipality and no more than 7.5 percent of the working-age population commute to one specific outside municipality for work. Municipalities that do not meet these restrictions are allocated to the LLM to which the majority of its working-age population commutes. There is at least one municipality in each local labor market, and no municipality is associated with more than one local labor market. In 1995, there were 106 LLMs and 288 municipalities in Sweden.

The mean employment incomes of CENT males and females in 1995 were $39,954 and $25,205, respectively.[32] The average difference between the CENT employment income and the teacher wage in a municipality was $828 among males and -$178 among females. These values represent 2.1 and 0.7 percent of the employment income of CENT males and females prior to the reform. In no municipality was the male teacher wage higher than the associated LLM CENT employment income. Female teachers enjoyed higher wages than their CENT counterparts in almost 90 percent of the municipalities. The large cross-gender difference in the wedge between the outside non-regulated employment income and the teacher wage is driven entirely by differences in CENT employment incomes; the average difference between monthly male and female teacher wage across municipalities was only $65 in the year prior to the reform.

Figure 3 provides a visual depiction of the cross-LLM variation in pre-reform non-teacher employment income with respect to males and females respectively. In the Figure, LLMs have been color-coded based on which decile of the gender-specific non-teacher employment income distribution they belong to, with LLMs in yellow belonging to the bottom decile and LLMs in brown belonging to the top decile. Black solid lines indicate 1995 LLM borders, while gray solid lines indicate municipality boarders. Looking across Figure 3, there is substantial geographic variation in the treatment variable. There are also some gender

[31] To bias the results, it would further have to be the case that there are systematic differences between employment income and wage across individuals in municipalities that have different CENT wages in the year prior to the reform, and this is unlikely. Note that the estimates in this paper are robust to using teacher employment income rather than teacher wage. For example, if I use teacher employment income, the π_{1999}, π_{2001} and π_{2006} estimates (standard errors) in Table 3 with respect to the mean become 0.090 (0.029), 0.116 (0.034) and 0.196 (0.054).

[32] While the female mean is very similar to that in the US ($24,555), the male mean is noticeably smaller ($49,928). Information on US employment income has been taken from Census (1996). These numbers have been deflated to represent real 2005 values.

differences in the geographic variation of pre-reform CENT income (the cross-gender correlation coefficient is 0.524).

Appendix Figure A1 plots the correlation between the 1995 CENT income and the growth in CENT income between 1995 and 2006. The figure suggests that the pre-reform CENT income level cannot predict post-reform CENT income changes, providing some suggestive evidence that the treatment measure is not correlated with area-specific market developments over time.

To examine the impact of the reform on short-term student educational outcomes, I use the *Grade 9 Registry*, which provides information on the academic performance of individuals in 9th grade including GPA and individual grades in the core subjects of math, Swedish and English.[33] I track these students through high school (via the *High School Registry*) and into the labor market ten years after graduating from 9th grade (via *LOUISE*) to examine long-term education and labor market effects of the reform. The education outcomes I look at in this part of the analysis are high school GPA, whether the student attended a university-preparatory high school track, graduated from a high school science program, and was ever enrolled at university. The labor market outcomes I study are the probability of being in the earnings sample, the probability of being a social security recipient and employment income. Summary statistics are provided in Panel B of Appendix Table A1. All grades have been converted to yearly national percentile rankings.

In addition to the registry data described above, I rely on three public-use data sets released by the Swedish National Agency for Education (SNAE) and Statistics Sweden (SCB). First, SNAE releases municipality-specific information on education spending stratified by input (teaching, food, facilities, health, supplies and other items). I use this information to examine if the reform affected education spending and resource allocation.[34] Appendix Table A2 shows statistics on per student spending by educational input, and Appendix Table A3 provides detailed information on what each of the expense categories include. Second, SNAE also releases municipality-specific information on the number of students and the fraction of students enrolled at charter schools. I use this data to control for potential variation in public school cohort size that could

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[33] During my analysis period, Statistics Sweden did not collect statistics on grades until the students reached 9th grade.

[34] The inputs for which spending is reported vary by year, and I use only the categories that are consistently measured throughout the analysis period. Per-student spending on uncategorized items has been constructed to equal the difference between total per-student spending and per-student spending on teaching, food, health, supplies and facilities. This measure accounts for any spending that did not fall into any of the categories that municipalities were asked to report spending on, and any spending that falls into categories that municipalities were asked to report spending on for only a subset of the years under examination. These categories include, but are not restricted to, cost of school library, career services, administration, student transportation, home language instruction and Swedish for immigrants.

confound my estimates. This information further enables me to investigate if the teacher-student ratio was affected by the decentralization reform. Third, SCB publishes information on local tax rates. I use this information to examine if the potential wage effect was funded, at least in part, through an increase in the municipality income tax. The SNAE data are only available beginning in 1992.

4.2 Estimation strategy

I exploit cross-LLM variation in 1995 CENT employment income as a measure of treatment intensity in a dose-response difference-in-difference framework. The potential wage effect of the reform is likely to vary over time as local wage-setters cannot change the pay of workers overnight, especially given the limited guarantees that were in place during the transition period of 1996-2000. As a consequence, any potential effects on teacher composition and student outcomes are also likely to vary over time. Thus, my main empirical approach is to non-parametrically trace out the full adjustment path of the potential treatment effect through event study models. This allows me to examine the dynamic response to the reform and if there are any time-varying impacts of wage decentralization. I estimate models of the following form:

$$Y_{gmlt} = \beta_0 + \sum_{t=1991}^{2006}[\pi_t(lnNTW_{gl,1995})] + \gamma X_{gmlt} + \delta_{gm} + \theta_{gt} + \varepsilon_{gmlt}, \quad (1)$$

where Y_{gmlt} is an outcome of public elementary school teachers of gender g in municipality m and LLM l at time t. $lnNTW_{gl,1995}$ is log 1995 CENT employment income of gender g in LLM l. π_t therefore represents the effect of 1995 CENT income on Y in year t. Thus, the π_t coefficients non-parametrically trace out pre-treatment relative trends (π_{1991} - π_{1995}) as well as time-varying treatment effects (π_{1996} - π_{2006}). In practice, I omit π_{1995} such that all π estimates are relative to the pre-reform year. I cluster the standard errors on the treatment level.

The unit of observation is a municipality-gender-year. Aggregation to this level is sensible because municipalities hold full responsibility for education at the elementary school level, and males and females face disparate labor market opportunities. The identifying variation stems from differences in 1995 CENT employment income across LLMs.[35] The parameters of interest are π_{1996}-π_{2006},

[35] An alternative treatment measure can be obtained by first estimating Mincer earnings functions (where wage is modeled as a function of years of schooling, potential experience and potential experience squared) for non-teachers in the year prior to the reform (separately by gender), controlling for municipality fixed effects, and then using the estimated values from these regressions to predict what the wages of teachers would be had they not been teachers (this measure might provide a better depiction of the wage that teachers could have had, had they not been teachers). Results using this measure yield larger, but not statistically

which show the time-varying effect of the reform on Y. These coefficients capture the part of the effect of the reform that can be explained by variation in pre-reform CENT employment income across LLMs.

Equation (1) also includes a set of gender-municipality (δ_{gm}) and gender-time (θ_{gt}) fixed effects. The former controls for variation in teacher pay common to all teachers of a specific gender within a municipality over time. The latter controls for variation in teacher pay common to all teachers of a particular gender across municipalities in a given year. Equation (1) further includes a set of municipality socioeconomic and demographic characteristics (X_{gmlt}).

My main identifying assumption is that there are no secular trends, policies or shocks that affect outcomes differently depending on the area's pre-reform CENT income. I show extensive evidence that the data are consistent with this assumption. First, I show that the results are robust to the inclusion of an extensive set of fixed effects and controls for other factors that may be correlated both with the pre-reform CENT income and the outcomes. Second, I provide event studies showing that pre-reform trends in outcomes did not differ across municipalities with different 1995 CENT incomes. Third, I provide evidence that other reforms that took place in the years surrounding the decentralization reform do not drive the results. Fourth, I show results from rerunning my main specification for occupational groups that can be assumed to have been isolate from, and unaffected by, the reform. Finally, I show that the results are robust to the way in which the treatment measure is constructed, and that alternative measures produce similar estimates. Results from all these tests are inconsistent with plausible sources of bias from secular shocks or trends and support a causal interpretation of my estimates.

5 Results

5.1 Effect on wage structure

The effect on teacher pay is shown in Figure 4 (a), obtained from estimation of equation (1) using all public elementary school teachers. Each dot is an estimate of relative time parameter π_t for the given year. The bars extending from each point show the bounds of the 95 percent confidence interval. Both the treatment and dependent variable are measured in logarithmic form; π_t represents the response elasticity of teacher wage with respect to pre-reform CENT employment income.

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significantly different, estimates. These results are shown in the Appendix, and are discussed at length in the robustness section.

Figure 4 (a) shows a clear wage effect associated with the reform, and several observations are worth highlighting. First, wages are trending similarly across municipalities in the pre-period as a function of the 1995 CENT employment income; there is no evidence of differential trends in wages across pre-treatment cohorts.[36] Second, it takes three years for the wage to react to the reform. This lag is expected: local wage-setters cannot change the pay of its workers overnight, especially given the guarantees that were in place during the 96-00 transition period. Third, the treatment effect grows over time until it levels out seven years after the reform. Interpreting this as the stable long-run treatment effect, the figure suggests a long-run response elasticity of teacher wage with respect to pre-reform non-teacher employment income of about 0.2.[37] The reform thus induces local wage-setters to pay wages that better align with local competitive equilibrium pay.

Assuming that teacher quality is constant across municipalities, equalization of relative teacher pay conditional on quality would require a response elasticity of 1. Even if the results in Table 2 suggest that teacher quality is not constant across municipalities, the differences in teacher characteristics across the municipalities are relatively small. Thus, the response elasticity would likely need to be greater than 0.2 in order to equalize teacher pay – conditional on quality - across municipalities. This incomplete response is suggestive of wage-setters being unable, or unwilling, to fully eliminate the difference in relative teacher pay across local markets. One potential reason for this relates to budget constraints, something I explore in Section 6.

When interpreting the effect in Figure 4 (a), it is useful to note that the teacher wage did not decline in any municipality during the analysis period. Rather, it increased differentially across municipalities as a function of the 95 CENT income. This is shown in Appendix Figure A4**,** which provides binned

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[36] This is expected as teacher pay was held constant across municipalities prior to the reform. An alternative way through which one can examine if teacher wage was held constant across municipalities prior to the reform is to note that municipality fixed effects should not be able to explain much of the variation in teacher wages before 1996 (when appropriately weighted for variation in the determinants of the wage schedule). Appendix Figure A2 shows the F statistic of joint significance for all municipality fixed effects obtained from earnings regressions estimated at the individual-level for each year of the analysis period, controlling for those factors that enter the wage calculation prior to the reform. The F statistic exhibit a pattern similar to the main wage pattern in Figure 4(a); it is flat prior to the reform, gradually increases after the reform until it levels out 7 years after the reform. The F statistic in 2003 is five times as high as before the reform. These results lowers the concern that there were unobserved municipality characteristics correlated with teacher wages prior to the reform.

[37] Given the gender imbalance in the teaching profession, it is possible that there are asymmetric treatment effects with respect to gender. This is examined in Appendix Figure A3; no such treatment heterogeneity is observed.

scatterplots of the correlation between the pre-reform wage gap (between CENT income and teacher wage) and the change in teacher wage between 95 and 06.[38]

The mean wage effect shown in Figure 4 (a) could mask substantial treatment heterogeneity across the teacher wage distribution. In other panels of Figure 4, I therefore show how the reform affected (b) the median wage, (c) the 10th percentile wage, (d) the 90th percentile wage, (e) the interquartile range, and (f) the standard deviation.[39] The median wage effect mirrors the mean wage effect, but the magnitude of the effect is marginally larger. This is indicative of the reform causing a slight tightening of the overall wage schedule. Figures 4 (c) and (d) show that this wage compression is due to the reform having a greater wage effect at the left-tail of the distribution, with a point elasticity twice as large as that in the top decile.[40] Figure 4 (e) shows a statistically and economically significant negative effect of the reform on the interquartile range, providing direct evidence of a tightening of the wage distribution. Similar to the wage level effect, the effect on the interquartile range is time-varying and reaches a new long-run equilibrium seven years after the reform. However, the magnitude of this effect is small: a 10 percent larger pre-reform CENT income is associated with a reduction in the interquartile range of $25 ten years after the reform. The standard deviation is unaffected. The wage effects are summarized in Table 3, showing effects at 3 (π_{1999}), 5 (π_{2001}) and 10 (π_{2006}) years. Effects at 10 years are my preferred estimates because Figure 4 suggests that this represents the stable long-run effect of the reform.[41]

The effects shown in Table 3 are consistent with Söderström (2006) who finds evidence of the reform causing a disproportionate increase in teacher entry wage and a flattening of the age-wage relationship. This result is also consistent with the idea that the pre-reform schedule compressed entry wage and provided an above-market return to experience. Before the reform, a 50-year-old teacher earned 50 percent more than a 26-year-old teacher, while a 50-year-old non-teacher earned only about 20 percent more than a 26-year-old (Söderström 2006). Thus, a flattening of the age-wage relationship is consistent with local

[38] Since teacher wages were fixed across LLMs prior to the reform, using 1995 CENT employment income instead of the 1995 wage gap yields identical results. However, using the wage gap in Figure A4 is informative for understanding whether areas with positive and negative wage gaps behave differentially. As seen in the figure, there is no evidence that would support such an interpretation.

[39] The wage dispersion measures have not been subject to log transformations because the pre-reform wage distribution is very compressed, in particular for old teachers that have reached the wage ceiling. Regressions stratified by age can therefore not be performed with log transformed dispersion measures as several of the municipalities have values of zero. For consistency, none of the dispersion results are based on log transformed dispersion measures. However, all non age-specific wage dispersion estimates are robust to this adjustment.

[41] To ensure that these effects are not driven by the three major cities of Sweden, I have also estimated the model excluding Stockholm, Gothenburg and Malmo. The point estimates (standard errors) for the mean wage effect at 3, 5 and 10 years become 0.064 (0.016), 0.113 (0.032) and 0.167 (0.041). These effects are not statistically significantly different from the main results in Table 3.

wage-setters adjusting the teacher pay structure to better align with the non-regulated market rate of return to experience. Appendix Figure A5 shows how the age-wage profile for teachers change between 1995 and 2006 in municipalities in the LLM with (a) the highest 1995 CENT income and (b) the lowest 1995 CENT income. The figure shows that wages went up, and that the age-wage relationship became flatter, in both types of municipalities over the analysis period. Consistent with the results in Table 3, municipalities in the LLM with the highest 1995 CENT income experienced a greater change.

The pooled results in Table 3 may hide substantial treatment heterogeneity across different teacher cohorts, and I therefore estimate the wage effects separately for young (20-34 years old), mid-career (35-49 years old) and old (50-64 years old) teachers. The full set of π estimates are shown in Appendix Figure 6 with respect to log wage and standard deviation. The effects are summarized in a more parsimonious way in Table 4, showing the effect after 10 years (π_{2006}).

The results in Table 4 suggest that the wage effect is negatively related to age. This supports the notion that the reform led to a disproportionate increase in entry wage and a flattening of the age-wage relationship. The table also shows that the wage dispersion effects in Table 3 hides substantial heterogeneity across age: A 10 percent higher 1995 CENT income leads to a \$10.7 increase in the standard deviation of the monthly wage among young teachers and a \$10.5 increase in the standard deviation of the wage among mid-career teachers.[42] Though the latter is not statistically significant at conventional levels, the standard error is smaller than the coefficient estimate and the event study result is suggestive of an effect. The standard deviation among old teachers is unaffected. Thus, despite a flattening of the age-wage relationship and an overall reduction in wage dispersion, the reform did generate within-age cohort increases in wage variation.

A few policies occurred during the analysis period that could potentially bias the results shown in Tables 3 and 4. First, *Skolvalsreformen* of 1992, which allowed for-profit charter schools to enter the education market. If 1995 CENT income is correlated with the growth of charter schools, that may bias me towards finding positive wage effects. The reason is that these schools may cause increased competition over labor and potentially higher wages. However, my results are robust to controlling for both the fraction of students in charter schools

[42] The identified wage effects may differ depending on the size of the teacher work force in the municipality as well. Larger municipalities face more intense competition over labor, and it is thus possible that local wage-setters in larger municipalities respond more strongly to the reform. In results not shown, I examine this hypothesis by estimating equation (1) for the municipalities in the bottom 50 percent of the teacher work force size distribution and for the municipalities in the top 50 percent of the teacher work force size distribution. Although the point estimates for the municipalities with larger teacher work forces are larger, the differences are not statistically significant.

over time and its interaction with the treatment variable. Second, *Kunskapslyftet*, a government program that ran from 1997 to 2002 with the purpose of providing individuals with less than a high school degree complementary education. This may have increased the demand for teachers, and if this is correlated with 1995 CENT income, it could bias my results. However, my results are robust to controlling for both the number of people with less than a high school degree, and its interaction with the treatment variable, between 1997 and 2002. Third, *the Balkan war* led to a large increase in immigration to Sweden between 1993 and 1996. If these immigrants disproportionally migrated to labor markets with high CENT income that could bias the result, since it may also increase the demand for teachers and put upward pressure on their wages. However, my results are robust to controlling for the number of immigrants and its interaction with the treatment variable between 1993 and 1996. Finally, there may be differential trends in the demand for teachers due to demographic shifts over time. To investigate this, I control for the total number of students in the municipality, and its interaction with the treatment variable. Adding these controls do not affect the estimates. The full sets of π estimates obtained from each of these regressions are shown in Appendix Figure A7. The effect at 10 years is shown in Table 5.

5.2 Effect on Teacher Composition

Table 6 shows the π_{2006} estimates from equation (1) for each of the teacher composition outcomes listed in Section 4: age, years of schooling, master's degree, immigrant status, fraction on leave, fraction that switched from private school, fraction on temporary contract, work hours (percent of a 40 hour work week), certification, fraction that move to a different district, fraction that move to a district that had higher 1995 CENT income, fraction that remain as teachers and fraction of new teachers. The full set of π estimates are shown in Appendix Figures A8 and A9.

Table 6 suggests that the reform had a negligible effect on teacher composition in regions that had higher pre-reform non-teacher employment income relative to regions with lower pre-reform non-teacher employment income, only showing statistically significant results for age (negative) and the probability of switching from a private to public school (positive). These effects are very small from an economic perspective: a 10 percent higher 1995 CENT income is associated with a reduction in teacher age of 0.3 years and an increase in the probability of switching from a private to public employer by 0.001. I find no effects on the other outcomes, including retention, recruitment, certification, mobility and education.

In Appendix Table A4, I explore if this pattern of results generalizes to each of the three age cohorts that I examined separately in Section 5.1. Looking across the table, the estimates are slightly noisier, and each age cohort has one statistically significant effect associated with it. However, these effects are economically modest, and there are no statistically significant differences in the point estimates across the age groups. This suggests that the results in Table 6 provide an accurate depiction of the teacher composition effects associated with the reform.

It is important to highlight that the substantial size of the teacher workforce means that one would need a relatively large change in teacher composition to identify an effect. One way to overcome this issue is by repeating the analysis using only teachers that enter and exit the profession in each year. Any change in the composition of the workforce must be driven by these individuals, and since they constitute only a small fraction of the workforce, this analysis permits identification of much smaller effects. However, the results obtained from this auxiliary analysis (not shown) are not statistically or economically significantly different from the baseline results.

5.3 Effect on student outcomes

The lack of effects on observable teacher characteristics does not mean that there is no productivity effect associated with the reform, and it is therefore still possible that the reform had an effect on student outcomes. First, the reform may impact teacher incentive and motivation in ways that cannot be identified with the outcomes in Section 5.2. Second, if the wage response to the reform was financed through a reallocation of educational resources, this could also have an effect on student outcomes. It is important to note that one may not expect any potential effect on student outcomes to show up until much later than 1996 due to treatment intensification across cohorts: those that graduated from 9th grade in 1996 were exposed to the new system for only one year (10 percent of their time in elementary school) while those that graduated in 2004 were exposed to the new system for nine years (100 percent of their time in elementary school).

Table 7 displays the π estimates obtained from estimating equation (1) for each of the student outcomes listed in Section 4: educational performance in grade 9, educational performance of the same students at the high school and university level, and labor market outcomes of these students ten years after graduating from grade 9. Event studies are shown in Appendix Figures A10-A12.

The results in Panel A of Table 7 suggest that the reform did not have a statistically or economically significant effect on educational performance in elementary school as measured by GPA, English, Swedish or math percentile ranking. Regarding the precision of these estimates, I can rule out GPA percentile ranking effects ten years after the reform larger than 0.054 and smaller than -0.093 from a 10 percent increase in pre-reform non-teacher employment income.[43]

With respect to high school and university attainment, the results in Table 7 show that the reform had no statistically significant effect on the probability of enrolling in a natural science high school track, on high school GPA or on the probability of attending university. There is some indication of an adverse effect on the probability of enrolling in a university-preparatory program, though this effect is very small: a 10 percent higher pre-treatment CENT income is associated with a reduction in the probability of enrolling in a university-preparatory program by 0.01.

The results in Panel C of Table 7 show that the reform did not have an impact on the labor market outcomes of these individuals ten years after graduating from 9th grade either; there is no statistically significant effect with respect to employment income, the probability of being in the employment sample or the probability of being a social security recipient. It should be noted that the labor market outcomes are measured when the students are between 25 and 26 years old (ten years after graduation from grade 9). This is below the age at which individuals are usually considered to be on a part of their labor market profile where earnings are representative of lifetime earnings. Even so, more than 70 percent of 26 year olds are part of the labor force in Sweden, and it is therefore still worthwhile examining these outcomes (Statistics Sweden 2014).

6 Mechanisms

6.1 Spending and resource allocation

The identified wage effects suggest that the reform caused an increase in spending on teachers. If this is financed through a reduction in spending on other inputs that are important for attracting teachers and raising student achievement, this could help corroborate why the reform had an impact on wages but not on teacher composition and student outcomes. This is examined in Figure 5, showing results from estimation of equation (1) with respect to (a) per student

[43] The coefficient estimate on GPA is -1.957 with a standard error of 3.729. One can therefore rule out effects larger than $\frac{-1.957+1.96(3.729)}{100} = 0.054$, and smaller than $\frac{-1.957-1.96(3.729)}{100} = -0.093$, from a 1 percent increase in pre-reform non-teacher employment income.

spending, (b) per student spending on teachers and (c) per student spending on non-teachers.

Figure 5 shows that the reform did not affect total spending (a), but that it did lead to a large increase in spending on teachers (b) and to a large decrease in non-teacher spending (c). The long-run response elasticity of teacher spending with respect to 1995 CENT income is 0.22, and the magnitude of this effect is very similar to the wage level effect in Section 5. The reform thus led to a reallocation of existing education resources, but not to an overall increase in spending.

To further examine the reform's effect on spending and resource allocation, Figure 6 shows the effect of the reform on spending on mutually exclusive and collectively exhaustive non-teacher education inputs: (a) food, (b) health, (c) supplies, (d) facilities and (e) other items. Figure A13 shows the effect of the reform on these same outcomes measured as a percent of total spending.

Despite the relatively large standard errors, subfigures (a) and (e) in Figure 6 shows reductions in spending on food and uncategorized items. While there is a short-term reduction in spending on facilities (d), this effect disappears in the long-run. With respect to health (b) and supplies (c), the event studies fail to identify significant effects.

All spending and resource allocation event studies are parsimoniously summarized in Table 8, which shows the effect ten years after the reform. Taken together, these results show that the reform did not have an effect on total education spending, but that it did lead to a reallocation of resources away from non-teacher inputs toward teachers. The majority of the reallocation effect is operating though a reduction in spending on uncategorized item, and the results provide little evidence to suggest that spending shifted in ways that make teachers and students worse off. These results also suggest that there are no effects on student-teacher ratio or local tax rates.[44]

6.2 Wage spillovers

Another factor that may help explain why teacher composition and student outcomes were unaffected by the reform, is that the reform may have had spillover effects to closely related occupations. Specifically, an increase in teacher wage will make it harder for firms outside the education sector to recruit

[44] Since total education spending was unaffected by the reform it is unlikely that we would observe an effect on the local income tax rate. Since the coefficient on teacher spending (Figure 5) closely mirrors the teacher wage effect (Figure 4) it is very unlikely that the reform affected the teacher-student ratio. Appendix Figure A14 shows event studies of the reform effect on (a) the local income tax rate and (b) the number of teacher per 100 students, and Appendix Table A5 summarizes this Figure by showing the effects after ten years. Figure A14 and Table A5 show that the reform did not have a statistically or economically significant effect on local tax rate and teacher-student ratio.

and retain individuals with teaching degrees. Further, it may induce individuals with non-teaching careers to consider teaching. This could put upward pressure on wages in these other industries, and mute the relative effect on teacher wages.

To examine the presence of wage spillovers, I use the teacher registries to identify individuals employed as teachers during the two years prior to the reform. I merge these data with the public sector wage registry, which provides wage and occupation information for every public sector worker in the country. This allows me to identify which public sector occupations teachers came from, and left for, in the year prior to the reform.[45] As shown in Appendix Table A6, more than 80 percent came from, or left for, jobs within public administration, social services and health care services. Using all workers in the occupations listed in Appendix Table A6, I estimate equation (1) to examine if the reform affected the wages in these occupations, weighting the regressions by the fraction of teachers that came from, and left for, each of the professions.

The result from the wage spillover exercise is shown in Figure 7 (a). The figure reveals a clear wage spillover effect that starts three to four years after the reform and stabilizes two years later. The estimated long-run response elasticity is half that of the teacher wage effect. Performing the same analysis on wages in occupation that no teacher came from, or left for, in the years prior to the reform, yields neither statistically nor economically significant results (Figure 7 (b)). That the wage spillover effect occurs almost contemporaneous with the main teacher wage effect suggests that employers are able to adjust wages quickly to avoid potential negative recruitment and retention effects caused by wage changes in closely related occupations, and provides valuable evidence on the dynamics of interplay in wage determination across occupations. The results from Figures 7 are summarized in Table 9, showing effects 3, 5 and 10 years after the reform.

As an additional robustness check, Appendix Figure A15 shows potential spillover effects to occupations within the healthcare sector that require advanced and specialized professional healthcare degrees (e.g. nurse, pharmacist, dental nurse, doctor). Since teachers cannot switch into these professions without going back to school and acquiring additional education, they do not represent plausible substitute occupations, and there should be no

[45] I focus on the pre-reform period to minimize the likelihood that occupational mobility patterns are confounded by the reform. However, a recent report from Statistics Sweden suggests that these patterns are the same in 2016 (Hellsing 2016). I constrain this part of the analysis to the public sector due to the limitations of the *Wage Registry of Private Sector Employees* elaborated on in the data section. However, the majority of teachers in non-teaching occupations work in the public sector, such that this is not a major limitation (Hellsing 2016).

spillover effects to these professions. As shown in Appendix Figure A15, there is no indication of spillover effects to these occupations.

The wage spillover effect coupled with the compressed wage distribution in Sweden provides a likely explanation to why the reform did not affect teacher composition and student outcomes (OECD 2001). As shown in Appendix Table A7, the difference in pre-reform CENT income between municipalities one standard deviation below the mean and one standard deviation above the mean is 7 and 10 percent, for females and males respectively. With a teacher wage response elasticity of 0.2 with respect to 1995 CENT income, the long-run post-reform difference in absolute teacher wages across 68 percent of the municipalities will be less than 2.8 percent for females and 4 percent for males. These differences are reduced to 1.4 and 2 percent after accounting for the wage spillover effect. It is unlikely that these modest post-reform differences in absolute teacher wages can overcome standard search and matching frictions and mobility costs.

6.3 Treatment heterogeneity

Another factor that may help corroborate the findings in Section 5 relates to treatment heterogeneity. To this end, I follow the existing literature and note that the supply response to wage changes – in particular with respect to mobility - may differ across gender, marital status and parenthood (Falch 2010). In results not shown, I have estimated equation (1) separately for (a) males, (b) females, (c) married teachers, (d) non-married teachers, (e) teachers with at least one child under 18 that lives at home and (f) teachers with no child under 18 that lives at home. The results from this exercise do not rule out homogeneous effects along these dimensions. Consistent with these findings, I find no differential effect of the reform on the wages of these subgroups (Appendix Figure A16). As the decentralization reform made it easier for local wage-setters to engage in wage discrimination across groups of workers, this is an important finding.[46]

7 Robustness checks and sensitivity analysis

In this section, I perform a series of additional sensitivity checks to study the robustness of my results to alterations of the empirical model. For each of these modifications, I report how π_t on teacher wage is affected.

[46] Another potential source of heterogeneity comes from the fact that teachers already earn more than non-teachers in some municipalities, while they earn less than non-teachers in other municipalities. Thus, it is possible that reform effects on teacher composition only are visible in municipalities where the non-teacher employment income was higher than the teacher wage prior to the reform. However, imposing this sample restriction does not change the coefficient estimates in a statistically significant way (results available upon request).

The first concern is that there may be persistent transitory fluctuations in earnings in any one year that introduce noise in the treatment measure and attenuate my results (Bhashkar 2005). To this end, I estimate equation (1) using CENT income averaged over the five years preceding the reform as the measure of treatment intensity. As illustrated in Appendix Figure A17 (a), this adjustment has no impact on the economic and statistical significance of the coefficient estimates.

Another concern is that the CENT employment income may not provide an accurate measure of the wage that teachers can command had they not been teachers.[47] An alternative measure can be obtained by first estimating Mincer earnings functions (in which wage is modeled as a function of years of schooling, potential experience and potential experience squared) for non-teachers (separately by gender) in the year prior to the reform (controlling for municipality fixed effects) and then using the estimated values from these regressions to predict what the wages of teachers would be had they not been teachers. The result from estimating equation (1) using this alternative measure is depicted in Appendix Figure A17 (b). The figure shows that this alternative measure yields a teacher wage elasticity that is 0.1 higher than the baseline result. However, the relative time parameter estimates remain within the 95 percent confidence interval of the baseline result.

Relatedly, basing the treatment measure on the full distribution of CENT incomes may introduce unnecessary noise, as observations in the tails of the distribution likely are not useful predictors of the wage that teachers could command had they not been teachers. I have therefore estimated equation (1) excluding gender-specific employment incomes in the top and bottom 5 percent of the distribution. As shown in Appendix Figure A17 (c), this exercise yields a wage effect that is larger than, but not statistically significantly different from, the baseline estimate.

An issue specific to municipality-level analyses in Sweden during the 90s and 00s is that some areas broke away from their municipalities and created their own municipalities during these years.[48] Though the newer municipalities are not included in the analysis as only partial time-series information is available,

[47] As an additional validity check of the treatment measure, I first use the method outlines in Section 6.2 to identify individuals that switched from teaching to non-teaching occupations in 1994. I then examine the correlation between their new non-teaching wage and the CENT pay in 1995. This allows me to examine if teachers who switched to occupations in areas with high CENT pay prior to the reform actually obtains a higher wage compared to switchers in low CENT pay areas. I obtain a correlation close to 0.9, providing suggestive evidence that the CENT employment income is a good measure of the wage that teachers can command had they not been teachers.

[48] Trosa from Nyköping (92), Gnesta from Nyköping (92), Bollebygd from Borås (95), Lekeberg from Örebro (95), Nykvarn from Södertälje (99) and Knivsta from Uppsala (03).

the municipalities that they originally belonged to are. To ensure that the results are robust to excluding these municipalities, as there could be compositional shifts that bias the results, Appendix Figure A17 (d) shows the result when these municipalities have been omitted. The results are not different from the baseline results.

An implicit assumption underlying my estimation strategy is that the CENT income is a more accurate reflection of the alternative job market opportunities of teachers than the employment income of non-college-educated non-teachers. To examine this assumption, Appendix Figure A17 (e) shows how the wage effect changes when using the 1995 employment income of non-college-educated non-teachers. The figure shows that the baseline result is robust to this adjustment. This is an interesting finding likely driven by the relatively low internal rate of return to education in Sweden (OECD 2002), a compressed labor market (Kahn 2015), and a strong correlation between college-educated and non-college-educated non-teacher income within each municipality.[49]

Another assumption behind my estimation strategy is that LLMs, not municipalities, matter when predicting the alternative job opportunities of teachers. The idea underlying this assumption is that municipalities do not represent unified labor markets and likely fail to fully capture the alternative wage that teachers can command. To examine this assumption, Appendix Figure A17 (f) demonstrates how the wage effect of the reform changes when the treatment measure is based on pre-reform variation in non-teacher employment income at the municipality level. Although the dynamics of the wage response is unaffected, the magnitude of the effect is reduced by approximately 50 percent. This is consistent with the idea that municipalities do not represent unified labor markets and fail to fully capture the alternative wage that teachers can command.

Lastly, a worry specific to the results that examine how the reform affects teacher wages at different deciles of the teacher wage distribution is that it is not clear why the treatment should be based on the mean 1995 CENT income. To this end, Appendix Table A8 shows how the wage effect after 10 years (π_{2006}) changes depending on which decile of the pre-reform non-teacher employment income distribution that is used to construct the treatment measure. Looking across the rows in Table A8, however, it becomes apparent that the wage effect is driven primarily by the general wage level in the municipality, and not by the wage at any one part of the distribution.

[49] During my analysis period, the correlation between CENT and non-CENT income within each municipality exceeds 0.9.

8 Conclusion

Despite a global trend of wage decentralization over the past 30 years, we know very little about the labor market implications of decentralized wage determination. A main reason is the lack of exogenous variation in wage regulation linked to detailed outcome data. I address this gap in the literature by evaluating a unique reform in Sweden that replaced the fixed national pay scale for teachers with individual wage bargaining.

I find that the reform induced significant changes in teacher pay in regions that had higher pre-reform non-teacher employment income relative to regions with lower pre-reform non-teacher employment income, and that these changes are financed entirely through a reallocation of existing education resources. I do not find these wage changes to impact teacher composition and student outcomes. While these results may partly be explained by the relatively compressed Swedish wage structure, I show that another potential explanation has to do with a wage spillover effect to substitute occupations. This effect is half that of the teacher wage effect, reducing the relative teacher wage response to the reform by 50 percent.

Taken together, my results provide little evidence to suggest that decentralization of wage determination is associated with any substantial labor market effects, despite its impact on wages. However, the results do suggest that wage regulation changes in one occupation may have important spillover effects in other occupations. I underscore that my estimates do not capture any level effects that impact all municipalities of Sweden in the same way, so caution should be exercised in extrapolating the results to the overall effects of decentralization reforms.

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Appendix

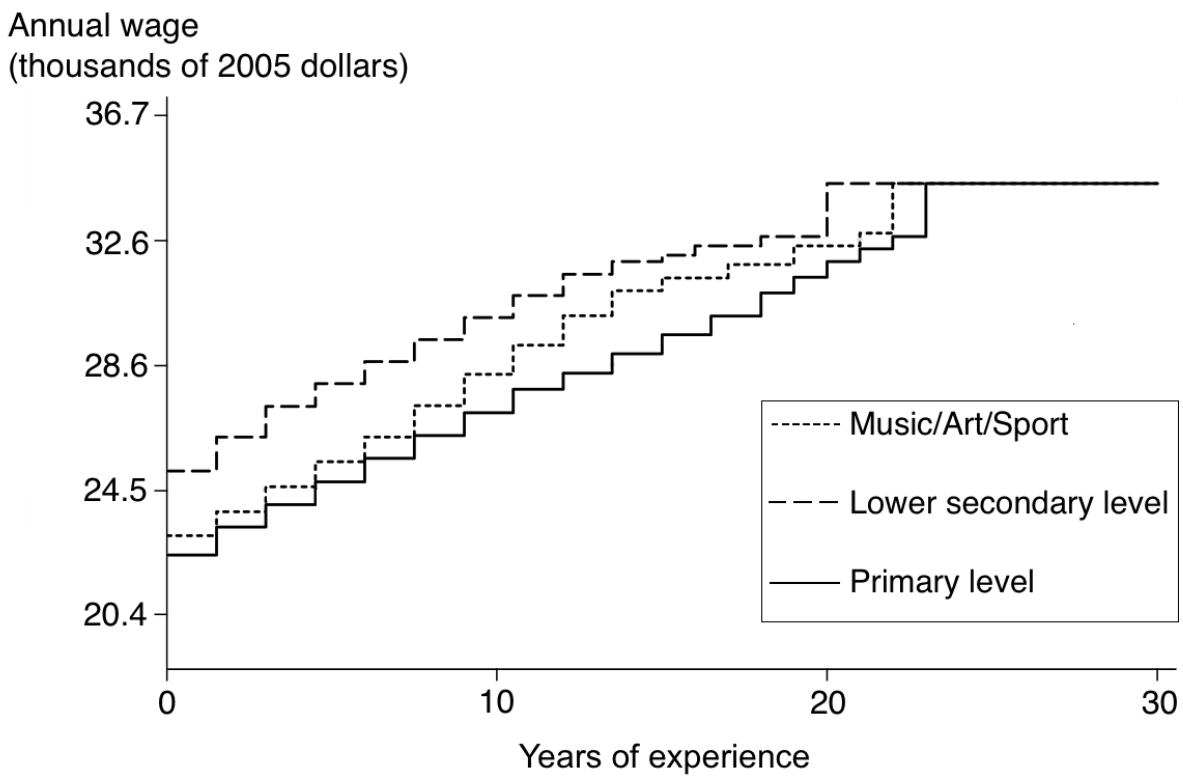


Figure 1: Pre-reform teacher steps-and-lanes salary schedule at the elementary school level (in 000's)
Notes: This figure is based on information from Söderström (2006), and shows 1990 the teacher salary schedule. See Section 2.2 for a detailed description of the centralized wage schedule that was used prior to the 1996 reform.

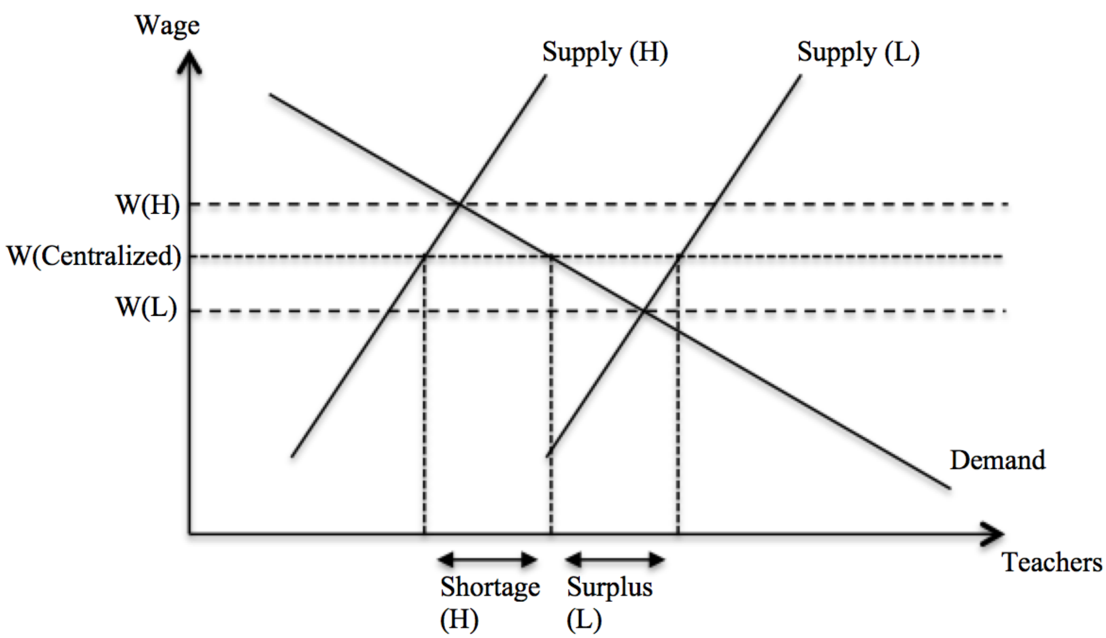
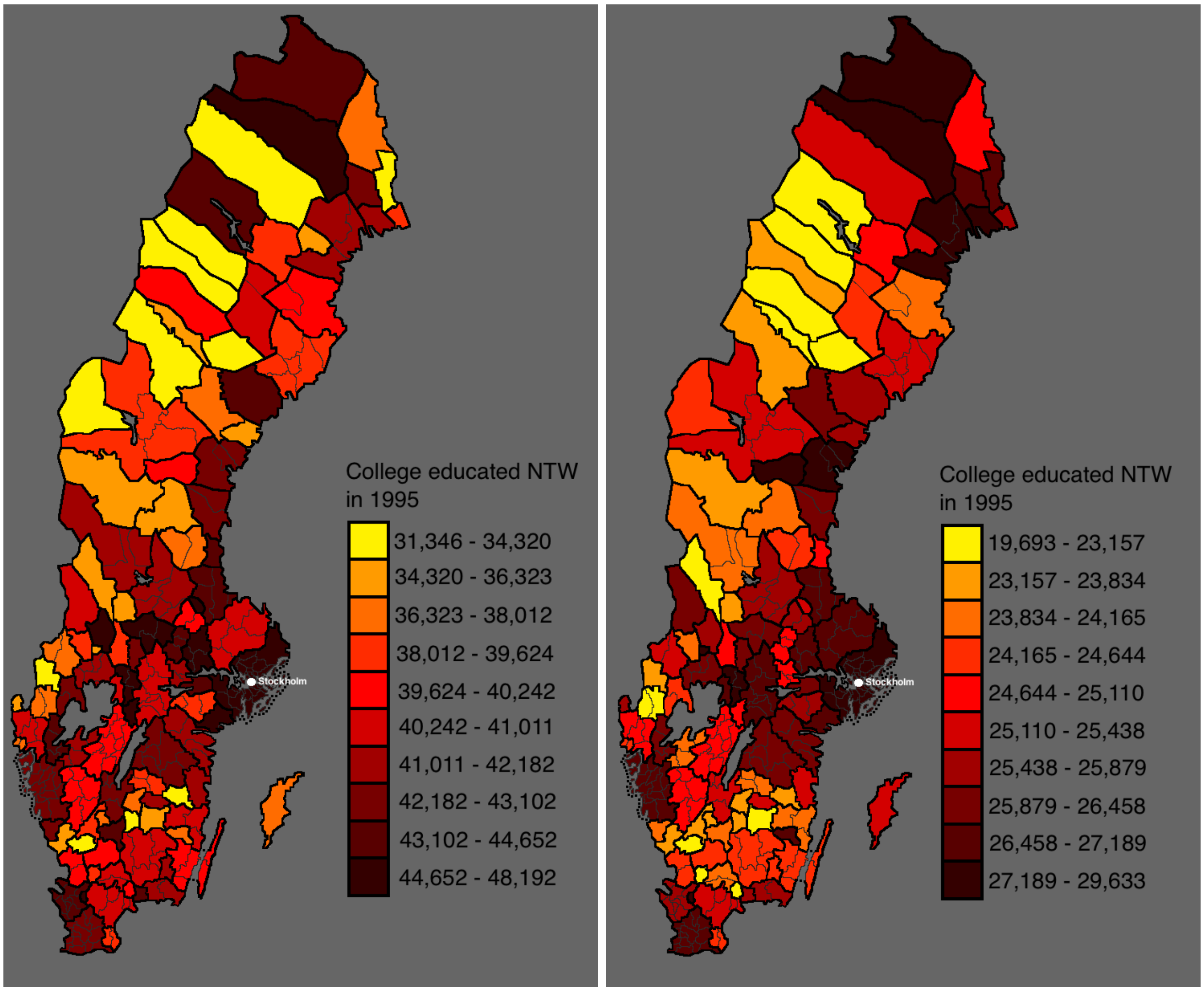


Figure 2: Centralized wage-setting

Notes: Figure based on Britton and Propper (2016). L represents the low-productivity region and H depicts the high-productivity region. See Section 2.3 for a detailed description of the anticipated labor market implications associated with centralized wage-setting.

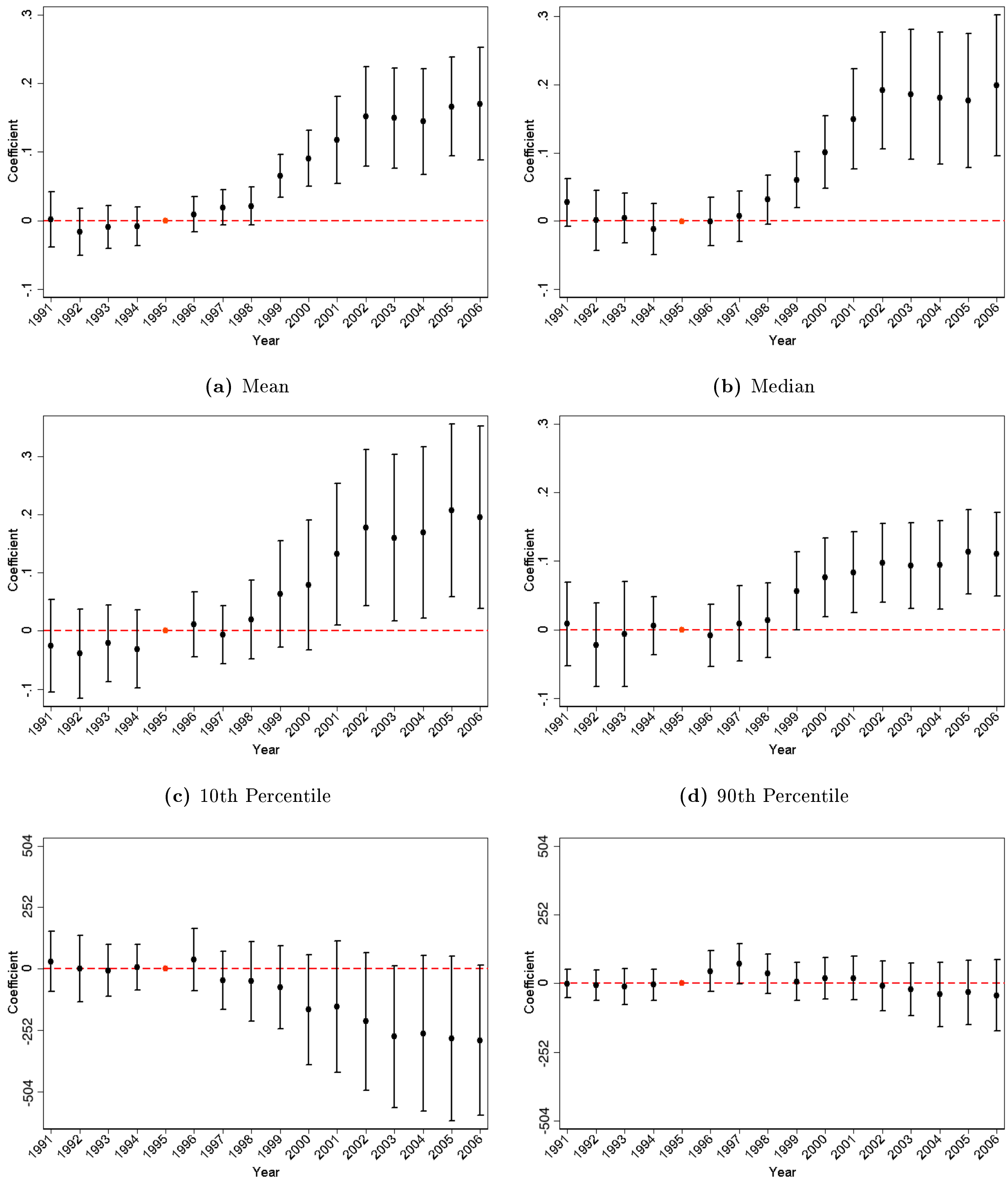


(a) Males

(b) Females

Figure 3: Variation in CENT employment income across local labor markets in 1995

Notes: The heat maps show the geographic variation in CENT employment income (thousands of 2005 dollars) across local labor markets in 1995 for (a) males and (b) females. The gender-specific CENT employment income distributions have been divided into deciles, with local labor markets in yellow belonging to the bottom decile and local labor markets in brown belonging to the top decile. Black solid lines indicate 1995 local labor market borders and gray solid lines indicate 1995 municipality borders. Each local labor market border is also a municipality border. The dotted black lines are used to signal that all islands inside those lines also belong to the local labor market.



(a) Mean

(b) Median

(c) 10th Percentile

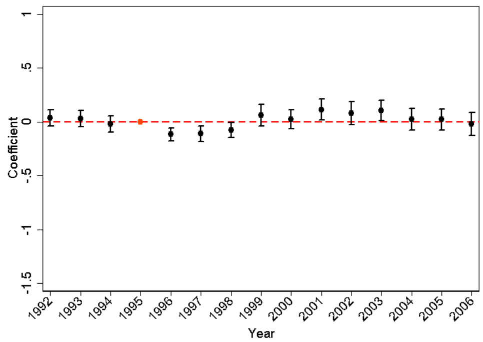
(d) 90th Percentile

(e) Interquartile range

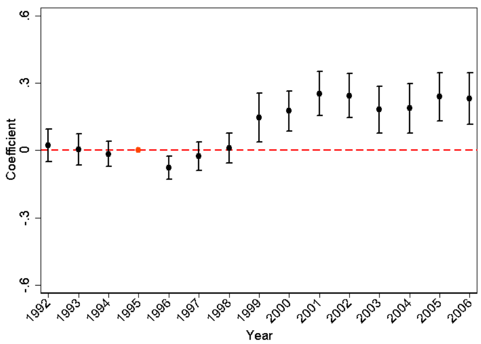
(f) Standard deviation

Figure 4: Event study estimates - wage structure

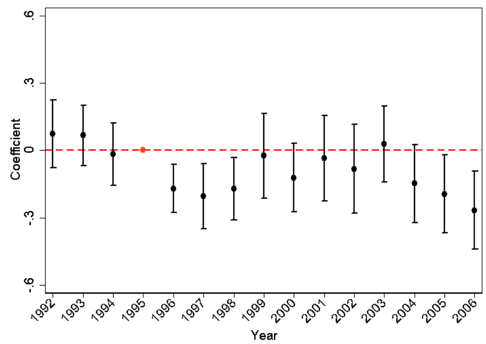
Notes: Author's estimation of equation (1) as described in the text using 1991-2006 teacher registry data on all public elementary school teachers in Sweden. Year 1995 is omitted, so all estimates are in relationship to this year. All estimates include gender-by-year and gender-by-municipality fixed effects as well as municipality controls for fraction males, fraction immigrants, average income, fraction unemployed, average years of schooling, fraction on social security benefits and average age. Each point is a relative time parameter estimate, while the bars extending from each point show the bounds of the 95% confidence interval calculated from standard errors that are clustered at the treatment level.



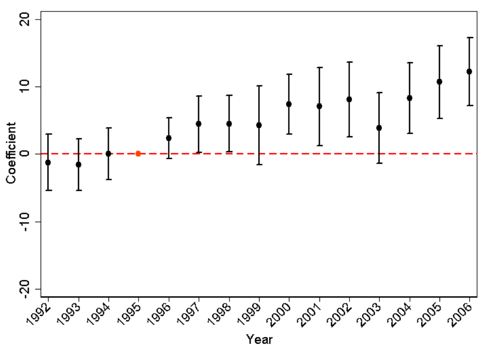
(a) Total



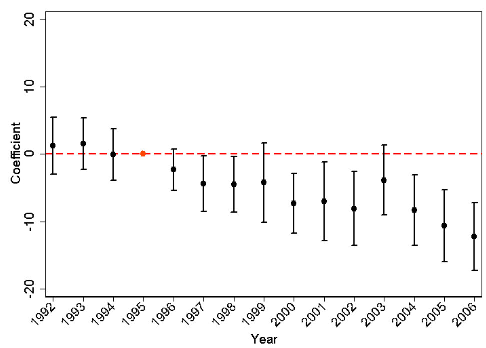
(b) Teaching



(c) Non-teaching items



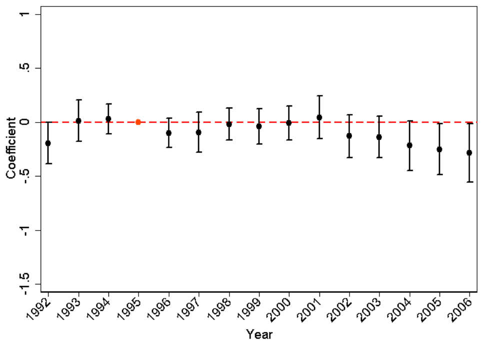
(d) Fraction Teaching



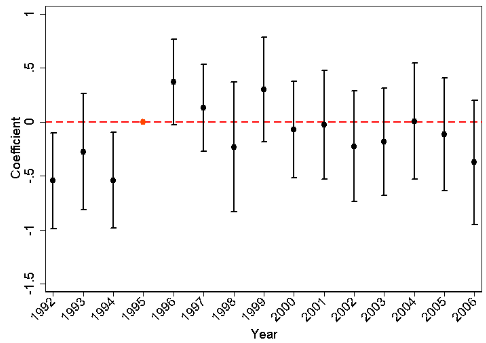
(e) Fraction Non-Teaching

Figure 5: Event study estimates - education spending

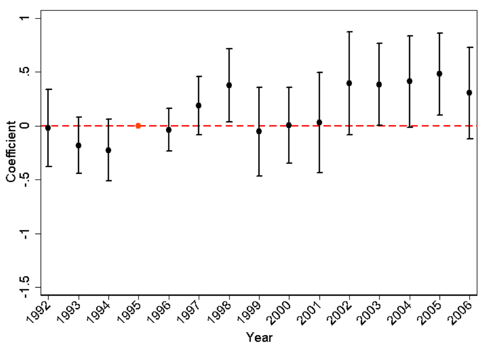
Notes: Author's estimation of equation (1) as described in the text using 1992-2006 registry data supplemented with public-use data from Statistics Sweden and SNAE. Year 1995 is omitted, so all estimates are in relationship to this year. All estimates include gender-by-year and gender-by-municipality fixed effects as well as municipality controls for fraction males, fraction immigrants, average income, fraction unemployed, average years of schooling, fraction on social security benefits and average age. Each point is a relative time parameter estimate, while the bars extending from each point show the bounds of the 95% confidence interval calculated from standard errors that are clustered at the treatment level.



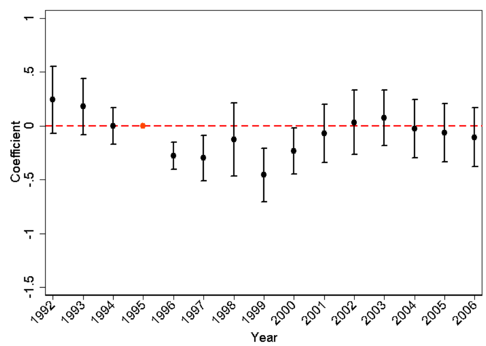
(a) Food



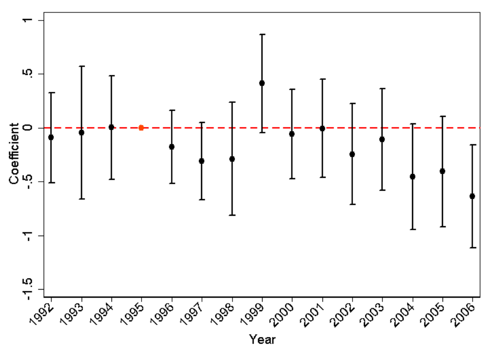
(b) Health



(c) Supplies



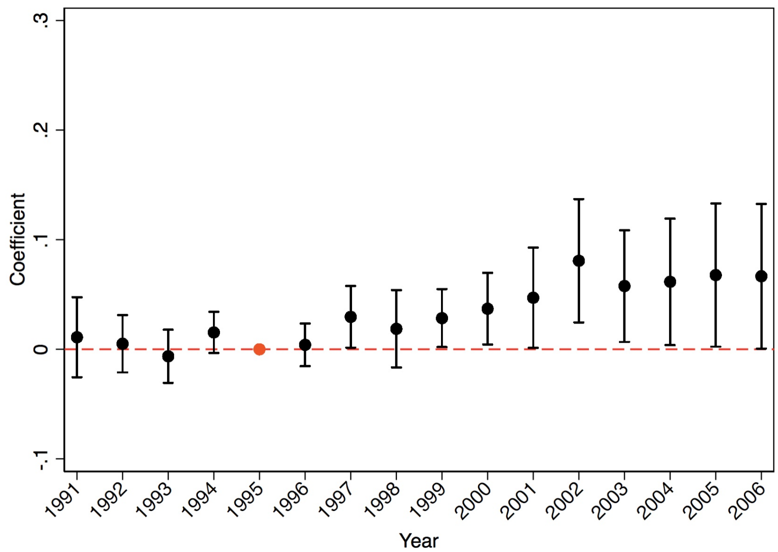
(d) Facilities



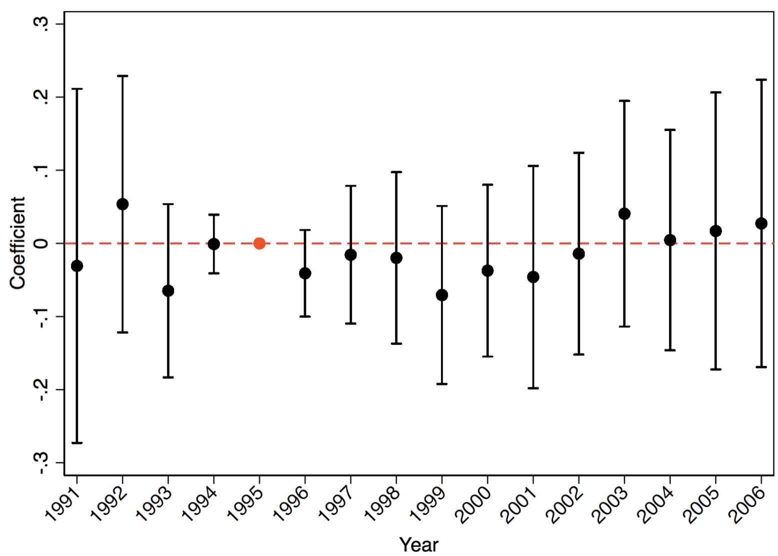
(e) Uncategorized items

Figure 6: Event study estimates - education spending by input

Notes: Author's estimation of equation (1) as described in the text using 1992-2006 registry data supplemented with public-use data from Statistics Sweden and SNAE. Year 1995 is omitted, so all estimates are in relationship to this year. All estimates include gender-by-year and gender-by-municipality fixed effects as well as municipality controls for fraction males, fraction immigrants, average income, fraction unemployed, average years of schooling, fraction on social security benefits and average age. Outcome variables are measured as cost per student (in logarithmic form). Each point is a relative time parameter estimate, while the bars extending from each point show the bounds of the 95% confidence interval calculated from standard errors that are clustered at the treatment level.



(a) Substitute occupations



(b) Non-substitute occupations

Figure 7: Event study estimates - wage spillover

Notes: Author's estimation of equation (1) as described in the text using 1992-2006 registry data. The unit of observation is a municipality-gender-occupation. The substitutes occupations sample (subfigure A) includes all workers in the three-digit public occupation groups that any teacher switched to/from in the year prior to the reform. The non-substitute occupations sample (subfigure B) includes all workers in the three-digit public occupation groups that no teacher switched to/from in the year prior to the reform. The estimates include municipality-gender, year-gender, occupation-gender and occupation-year fixed effects, as well as controls for gender composition, average years of schooling, fraction of social security benefit recepients, mean income, average age and fraction immigrants. The regression underlying estimation of subfigure (a) is weighted by the fraction of teachers that switched to each of the occupation groups, while the regression underlying estimation of subfigure (b) is weighted by the number of people in each municipality-gender-occupation. Each point is a relative time parameter estimate, while the bars extending from each point show the bounds of the 95% confidence interval calculated from standard errors that are clustered at the treatment level.

Table 1: Summary statistics of public elementary school teachers and non-teachers

	Full Period		1991-1995		1996-2000		2001-2006	
	Teachers	Non-teachers	Teachers	Non-teachers	Teachers	Non-teachers	Teachers	Non-teachers
Male	0.269	0.512	0.285	0.511	0.266	0.512	0.259	0.512
Age	45.433	41.299	45.578	40.436	45.277	41.221	45.440	42.044
Immigrant	0.090	0.148	0.077	0.126	0.086	0.146	0.104	0.168
Married	0.636	0.495	0.701	0.537	0.632	0.495	0.586	0.462
Partners	0.070	0.096	0.046	0.081	0.063	0.095	0.094	0.108
Child	0.593	0.510	0.646	0.533	0.579	0.508	0.561	0.495
Single parent	0.077	0.089	0.076	0.084	0.076	0.088	0.079	0.093
Years of schooling	14.941	11.612	14.986	11.297	14.916	11.547	14.923	11.913
Masters degree	0.194	0.045	0.180	0.038	0.183	0.043	0.216	0.051
Social security recipient	0.013	0.059	0.017	0.076	0.017	0.077	0.005	0.031
Observations	1,318,287	82,194,429	407,236	24,898,410	398,706	25,705,147	512,345	31,590,872

Notes: Author's calculation using 1991-2006 teacher registry data on all public elementary school teachers in Sweden and 1991-2006 LOUISE registry data on all non-teachers in Sweden that are between 18 and 65 years old.

Table 2: Differences in teacher composition between high and low CENT employment income regions prior to the reform

	Bottom 10 Percent	Top 10 Percent	Difference in Means	
	Mean	Mean	Difference	T statistic
Years of Schooling	14.923	14.672	0.251	6.105***
Age	45.324	46.157	-0.833	-2.489**
Temporary Contract	0.075	0.065	-0.010	1.119
Percent Work	91.356	92.616	-1.260	-2.388**
Immigrant	0.045	0.105	-0.060	-5.505***
Female	0.695	0.737	-0.042	-4.427***
Mover	0.011	0.008	0.003	1.039
On Leave	0.015	0.024	-0.009	-1.707*
Private Switch	0.001	0.003	-0.002	-1.916*
Certificate	0.922	0.852	0.070	6.744***
Stayers	0.849	0.819	0.030	2.022**
Hires	0.082	0.103	-0.021	-2.320**

Notes: Author's calculation using 1995 teacher registry data on all public elementary school teachers in Sweden. "Bottom 10 Percent" refers to the municipalities at the bottom decile of the CENT employment income distribution, while "Top 10 Percent" refers to the municipalities at the top decile of the CENT employment income distribution. Column 5 depicts the differences in means between the two groups, and is equal to the value in Column 2 minus the value in Column 3. The T-statistic shows the Student t-statistic associated with the null hypothesis that the differences in means between the two groups are zero, allowing for differences in variances across the two groups. *** indicates significance at the 1% level, ** indicates significance at the 5% level and * indicates significance at the 10% level (based on two-tailed p-values).

Table 3: Effect on teacher wage structure

	Mean	Median	10th percentile	90th percentile	IQR	SD
Effect after 3 years	0.065***	0.061***	0.063	0.056*	-75.576	6.668
	(0.016)	(0.021)	(0.047)	(0.029)	(87.279)	(35.845)
Effect after 5 years	0.117***	0.150***	0.132**	0.083***	-155.886	19.230
	(0.032)	(0.038)	(0.062)	(0.030)	(137.330)	(40.756)
Effect after 10 years	0.170***	0.199***	0.196**	0.109***	-293.251*	44.600
	(0.042)	(0.053)	(0.080)	(0.031)	(156.484)	(66.564)

Notes: Author's estimation of equation (1) as described in the text using 1991-2006 teacher registry data on all public elementary school teachers in Sweden. Regressions are based on 9120 municipality-gender-year observations. All estimates include municipality-gender and year-gender fixed effects, as well as controls for gender composition, average years of schooling, fraction of social security benefit recepients, mean income, average age and fraction immigrants. Standard errors clustered at the treatment level are in parentheses. The wage level outcomes are measured in logarithmic form, such that the wage level coefficients (beta) represent the response elasticity of teacher wage with respect to the 1995 CENT employment income. That is, a 1% change in 1995 CENT employment income is associated with a beta% change in wage level 3, 5 and 10 years after the reform. The wage dispersion outcomes are not measured in logarithmic form, such that a 1% change in 1995 CENT employment income is associated with a beta/100 unit change in wage dispersion 3, 5 and 10 years after the reform. *** indicates significance at the 1% level, ** indicates significance at the 5% level and * indicates significance at the 10% level.

Table 4: Effect on teacher wage structure, by age cohort

	Mean	Median	10th percentile	90th percentile	IQR	SD
Effect after 10 years, 20-34 year olds	0.251***	0.231***	0.208***	0.299***	206.379*	107.121*
	(0.068)	(0.076)	(0.076)	(0.089)	(131.405)	(66.521)
Effect after 10 years, 35-49 year olds	0.197***	0.200***	0.127	0.204***	0.761	105.556
	(0.057)	(0.064)	(0.102)	(0.050)	(107.325)	(74.378)
Effect after 10 years, 50-64 year olds	0.144***	0.115***	0.307***	0.155***	-53.236	-51.905
	(0.033)	(0.032)	(0.077)	(0.037)	(111.656)	(58.158)

Notes: Author's estimation of equation (1) as described in the text using 1991-2006 teacher registry data on all public elementary school teachers in Sweden. Regressions are based on 9120 municipality-gender-year observations. All estimates include municipality-gender and year-gender fixed effects, as well as controls for gender composition, average years of schooling, fraction of social security benefit recepients, mean income, average age and fraction immigrants. Standard errors clustered at the treatment level are in parentheses. The wage level outcomes are measured in logarithmic form, such that the wage level coefficients (beta) represent the response elasticity of teacher wage with respect to the 1995 CENT employment income. That is, a 1% change in 1995 CENT employment income is associated with a beta% change in wage level ten years after the reform. The wage dispersion outcomes are not measured in logarithmic form, such that a 1% change in 1995 CENT employment income is associated with a beta/100 unit change in wage dispersion ten years after the reform. *** indicates significance at the 1% level, ** indicates significance at the 5% level and * indicates significance at the 10% level.

Table 5: Robustness of wage results to potential confounders

	Control for friskolereformen	Control for the Balkan war	Control for demographic changes	Control for kunskapslyftet
Effect after 10 years	0.148***	0.151***	0.167***	0.156***
	(0.037)	(0.037)	(0.041)	(0.036)

Notes: Author's estimation of equations (1) as described in the text using 1992-2006 registry data supplemented with public-use data from Statistics Sweden and SNAE. The estimates include municipality-gender, year-gender, occupation-gender and occupation-year fixed effects, as well as controls for gender composition, average years of schooling, fraction of social security benefit recepients, mean income, average age and fraction immigrants. The regression underlying the estimation of the results in Column 2 further controls for the fraction of elementary school students that attend friskolor and its interaction with the treatment variable. The regression underlying the estimation of the results in Column 3 includes an interaction between the fraction of immigrants and the treatment variable for the years in which the Balkan war generated a large inflow of immigrants to Sweden (1993 through 1996). The regression underlying the estimation of the results in Column 4 includes an interaction between the number of students in elementary school and the treatment variable. The regression underlying the estimation of the results in Column 5 includes a control for the fraction of individuals with less than a high school degree, and its interaction with the treatment variable (for the years in which kunskapslyftet was in effect, 1997 through 2002). Standard errors clustered at the treatment level are in parentheses. The wage level is measured in logarithmic form, such that the wage level coefficient (beta) represent the response elasticity of teacher wage with respect to the 1995 CENT employment income. That is, a 1% change in 1995 CENT employment income is associated with a beta% change in wage level ten years after the reform. *** indicates significance at the 1% level, ** indicates significance at the 5% level and * indicates significance at the 10% level.

Table 6: Effect on teacher composition

	Age	Master's Degree	Immigrants	On leave	Switched from private school
Effect after 10 years	-3.445**	0.019	0.007	0.012	0.010**
	(1.711)	(0.064)	(0.033)	(0.021)	(0.005)

	Temporary contract	Percent worked	Certificate	Movers	High Movers
Effect after 10 years	-0.023	1.401	-0.031	-0.015	-0.140
	(0.044)	(2.754)	(0.083)	(0.020)	(0.395)

	Stayers	Hires	Years of Schooling
Effect after 10 years	-0.075	-0.047	-0.190
	(0.062)	(0.041)	(0.205)

Notes: Author's estimation of equations (1) as described in the text using 1992-2006 teacher registry data on all public elementary school teachers in Sweden supplemented with 1992-2006 public-use data from Statistics Sweden and SNAE. Regressions are based on 8550 municipality-gender observations. All estimates include municipality-gender and year-gender fixed effects, as well as controls for gender composition, average years of schooling, fraction of social security benefit recepients, mean income, average age, fraction immigrants, fraction of student that attend private schools and total number of elementary school students. With the exception of age, percent work and years of schooling, all outcome variables range from 0 to 1 and represent the fraction of the municipality-gender-year cell for which the outcome was true. In terms of interpretation, a 1% change in 1995 CENT employment income is associated with a beta/100 unit change in the outcome. Standard errors clustered at the treatment level are in parentheses. *** indicates significance at the 1% level, ** indicates significance at the 5% level and * indicates significance at the 10% level.

Table 7: Effect on student education-and labor market outcomes

Panel A: 9th Grade				
	GPA	Swedish	English	Math
Effect after 10 years	-1.957	-2.184	-1.558	-2.019
	(3.729)	(3.405)	(3.053)	(4.144)
Panel B: High School				
	University-Prep. HS Program	Natural Science HS Track	GPA	
Effect after 7 years	0.155**	0.001	-2.201	
	(0.065)	(0.036)	(3.651)	
Panel C: Labor Market & Higher Education				
	Employment Income	Employment Sample	Social Security Recipient	University Enrollment
Effect after 8 years	0.102	0.029	-0.001	0.020
	(0.079)	(0.025)	(0.016)	(0.042)

Notes: Author's estimation of equations (1) as described in the text using Swedish registry data supplemented with public-use data from Statistics Sweden and SNAE. Regressions in Panel A are based on 8550 municipality-gender-year observations. High shool information for these individuals are available three years after completion of 9th grade, such that I have three less years of data for estimating the regressions underlying the results in Panel B. The results in Panel B are therefore based on 6852 municipality-gender-year observations. Students that graduated from 9th grade between 2004 and 2006 are thus excluded from Panel B. Regressions in Panel C are based on 7410 municipality-gender-year observations as these outcomes are measured 10 years after students graduate from 9th grade, and the most recent labor market data I have access to is from 2014 (these outcomes are pulled from the LOUISE registry discussed in the data section, and I have access to the LOUISE registry up until 2014, while I only have access to the High School registry used to obtain the outcomes in Panel B until 2006). Students that graduated 9th grade between 2005 and 2006 are therefore excluded from Panel C. All estimates include municipality-gender and year-gender fixed effects, as well as controls for gender composition, average years of schooling, fraction of social security benefit recepients, mean income, average age and fraction immigrants. Standard errors clustered at the treatment level are in parentheses. GPA, Swedish, English, Math and high school GPA are measured in yearly national percentile rankings. Employment income is measured in logarithmic form. The remaining variables range from 0 to 1 and represent the fraction of individuals in each municipality-gender-year cell for which the outcome was true. In terms of interpretation, a 1% change in 1995 CENT employment income is associated with a beta/100 unit change in the relevant outcome ten years after the reform, with the exception of the coefficient on employment income (for which a 1% change in 1995 CENT employment income is associated with a 1% change in employment income ten years after the reform). *** indicates significance at the 1% level, ** indicates significance at the 5% level and * indicates significance at the 10% level.

Table 8: Effect on spending and resource allocation

	Total	Non-teaching	Facilities	Teaching	Supplies	Health	Food	Other
Panel A: Per student spending								
Effect after 10 years	-0.022	-0.266***	-0.107	0.229***	0.305	-0.377	-0.287**	-0.637***
	(0.055)	(0.089)	(0.139)	(0.059)	(0.216)	(0.293)	(0.138)	(0.244)
Panel B: Resource allocation								
Effect after 10 years		-12.198***	-2.240	12.198***	0.995	-0.451	-1.905**	-8.033**
		(2.562)	(2.370)	(2.562)	(0.773)	(0.736)	(0.905)	(2.830)

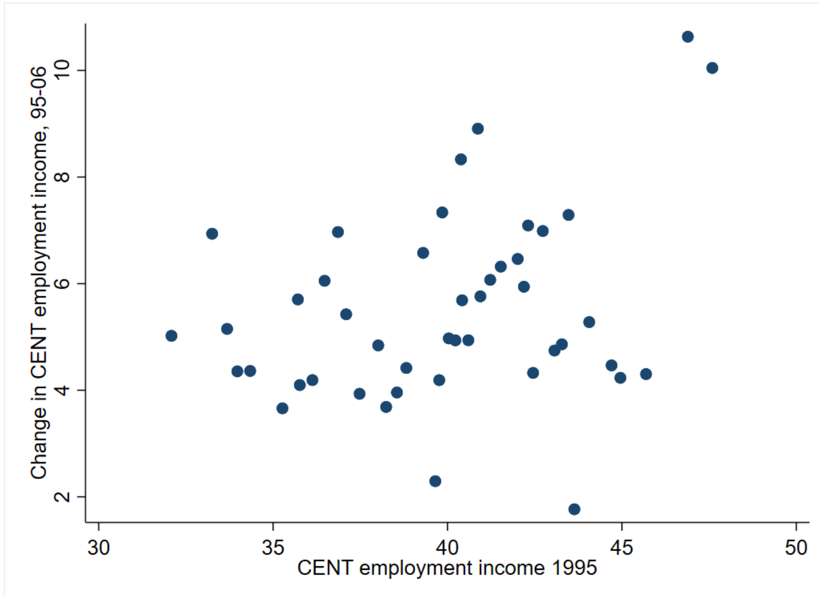
Notes: Author's estimation of equations (1) as described in the text using 1992-2006 registry data supplemented with public-use data from Statistics Sweden and SNAE. In Panel A, the outcome variables are measured as costs per student (in logarithmic form). In Panel B, the outcome variables are measured as cost per student divided by total education spending multiplied by 100, and therefore represent the percent of total education spending dedicated to that input. Regressions are based on 8550 municipality-gender observations. All estimates include municipality-gender and year-gender fixed effects, as well as controls for gender composition, average years of schooling, fraction of social security benefit recepients, mean income, average age, fraction immigrants, fraction of student that attend private schools and total number of elementary school students. Standard errors clustered at the treatment level are in parentheses. All outcomes in Panel A are measured in logarithmic form, such that the coefficients (beta) represent the response elasticity of the given outcome with respect to the 1995 CENT employment income. That is, a 1% change in 1995 CENT employment income is associated with a beta% change in the outcome ten years after the reform. All outcomes in Panel B are measured as percent of total education spending. In terms of interpretation of the coefficient estimates in Panel B, a 1% change in 1995 CENT employment income is associated with a beta/100 change in the outcome ten years after the reform. *** indicates significance at the 1% level, ** indicates significance at the 5% level and * indicates significance at the 10% level.

Table 9: Effect on wages in non-teaching occupations

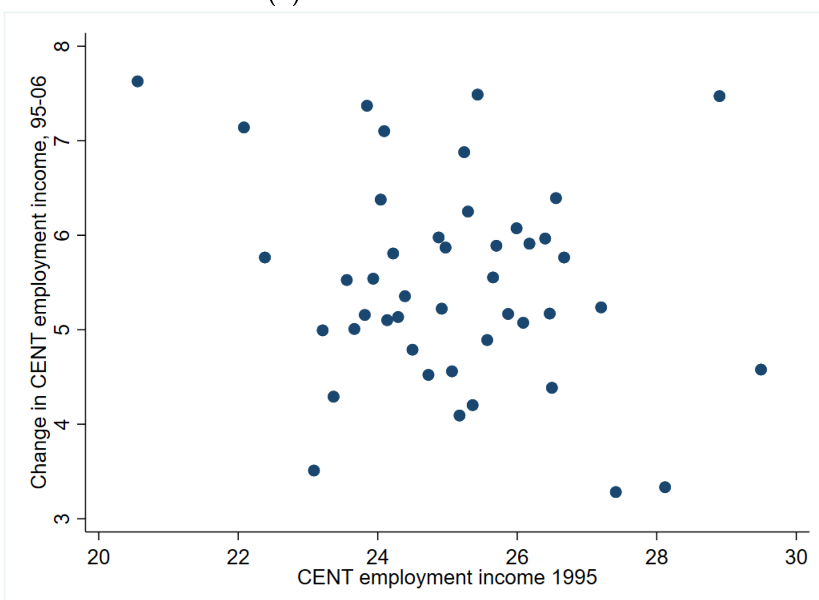
	Substitute occupations	Non-substitute occupations
Effect after 3 years	0.028**	-0.071
	(0.013)	(0.062)
Effect after 5 years	0.047**	-0.046
	(0.023)	(0.078)
Effect after 10 years	0.067**	0.027
	(0.034)	(0.100)

Notes: Author's estimation of equations (1) as described in the text using 1991-2006 registry data. The unit of observation is a municipality-gender-occupation. The substitutes occupations sample (subfigure A) includes all workers in the three-digit public occupation groups that any teacher switched to/from in the year prior to the reform. The non-substitute occupations sample (subfigure B) includes all workers in the three-digit public occupation groups that no teacher switched to/from in the year prior to the reform. The estimates include municipality-gender, year-gender, occupation-gender and occupation-year fixed effects, as well as controls for gender composition, average years of schooling, fraction of social security benefit recepients, mean income, average age and fraction immigrants. The regression underlying estimation of subfigure (a) is weighted by the fraction of teachers that switched to each of the occupation groups, while the regression underlying estimation of subfigure (b) is weighted by the number of people in each municipality-gender-occupation. Standard errors clustered at the treatment level are in parentheses. All outcomes in are measured in logarithmic form, such that the coefficients (beta) represent the response elasticity of the given outcome with respect to the 1995 CENT employment income. That is, a 1% change in 1995 CENT employment income is associated with a beta% change in the outcome ten years after the reform. *** indicates significance at the 1% level, ** indicates significance at the 5% level and * indicates significance at the 10% level.

Online Appendix: Not For Publication



(a) Male



(b) Female

Figure A1: Bin scatterplots: 1995 CENT income and CENT income development between 1995 and 2006

Notes: Author's estimation based on 1995-2006 registry data. The bin scatterplots show the correlation between 1995 CENT income and the change in CENT income between 1995 and 2006 for (a) males and (b) females (in 000's).

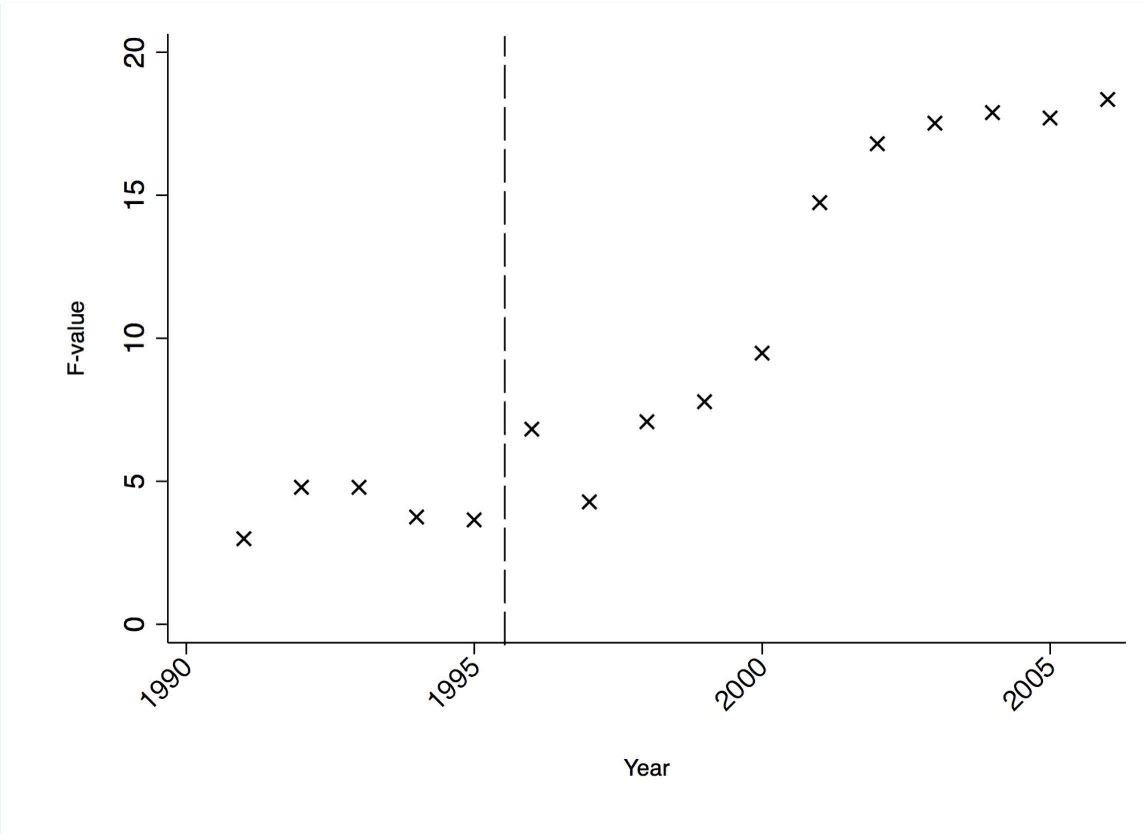
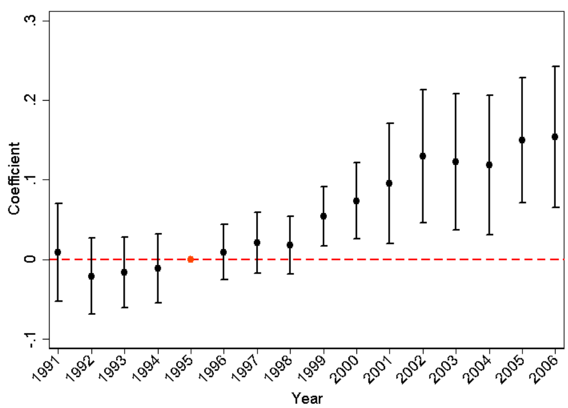
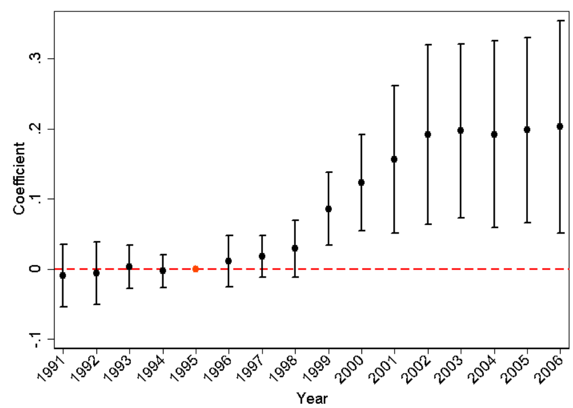


Figure A2: Joint significant of municipality fixed effects by year

Notes: Author's calculation using 1991-2006 teacher registry data on all full-time public elementary school teachers in Sweden. The figure shows the F statistic of joint significance of the municipality fixed effects for each year of the analysis period. These values are obtained from individual-level year-specific mincer earnings regressions that control only for the characteristics that enter the pre-reform wage scale calculation (age, age squared, potential experience, potential experience squared, type of teacher and teacher certification). By fully interacting these variables with a gender dummy, all variables are allowed to have gender-specific effects.



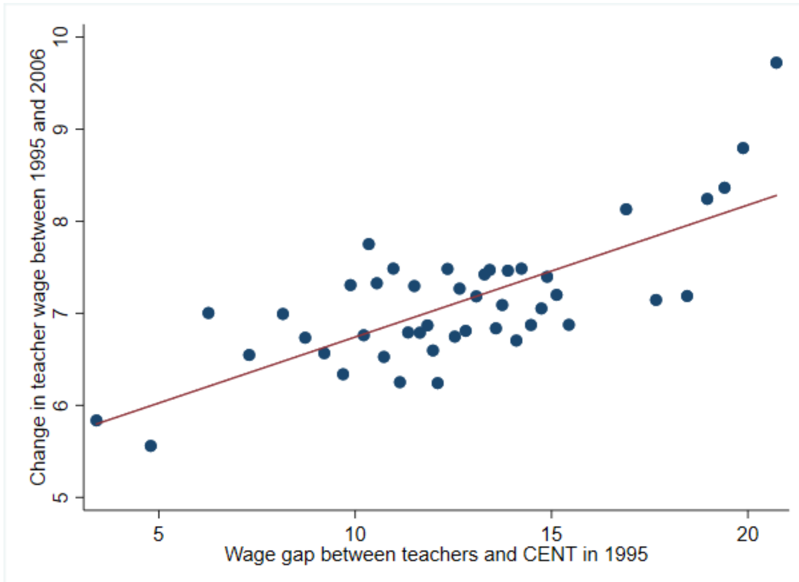
(a) Male



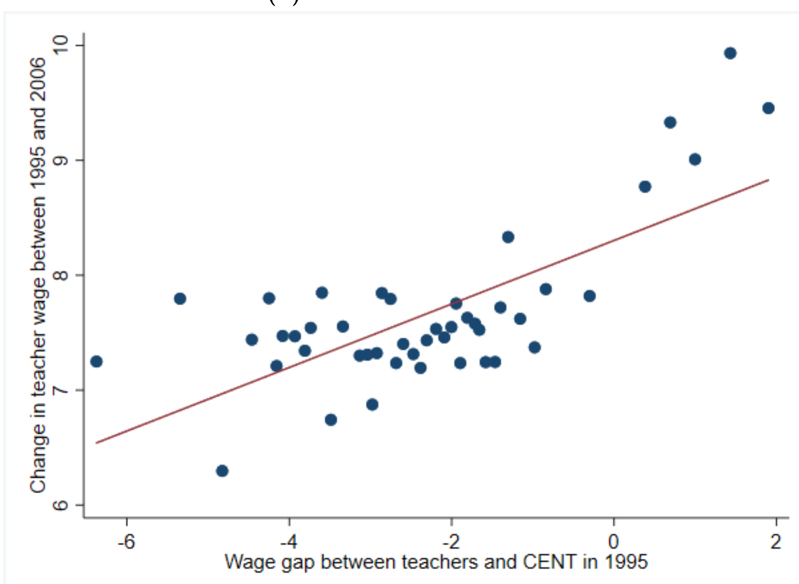
(b) Female

Figure A3: Event study estimates by gender - mean wage

Notes: Author's estimation of equation (1) as described in the text using 1991-2006 teacher registry data on the indicated subsample of public elementary school teachers in Sweden. Year 1995 is omitted, so all estimates are in relationship to this year. All estimates include year and municipality fixed effects as well as municipality controls for fraction males, fraction immigrants, average income, fraction unemployed, average years of schooling, fraction on social security benefits and average age. Each point is a relative time parameter estimate, while the bars extending from each point show the bounds of the 95% confidence interval calculated from standard errors that are clustered at the treatment level.



(a) Male



(b) Female

Figure A4: Bin scatterplots

Notes: Author's estimation based on 1995-2006 teacher registry data. The bin scatterplots show the correlation between the pre-reform wage gap (the difference between CENT employment income and teacher wage in 1995) and the change in teacher wages between 1995 and 2006 (controlling for municipality fixed effects).

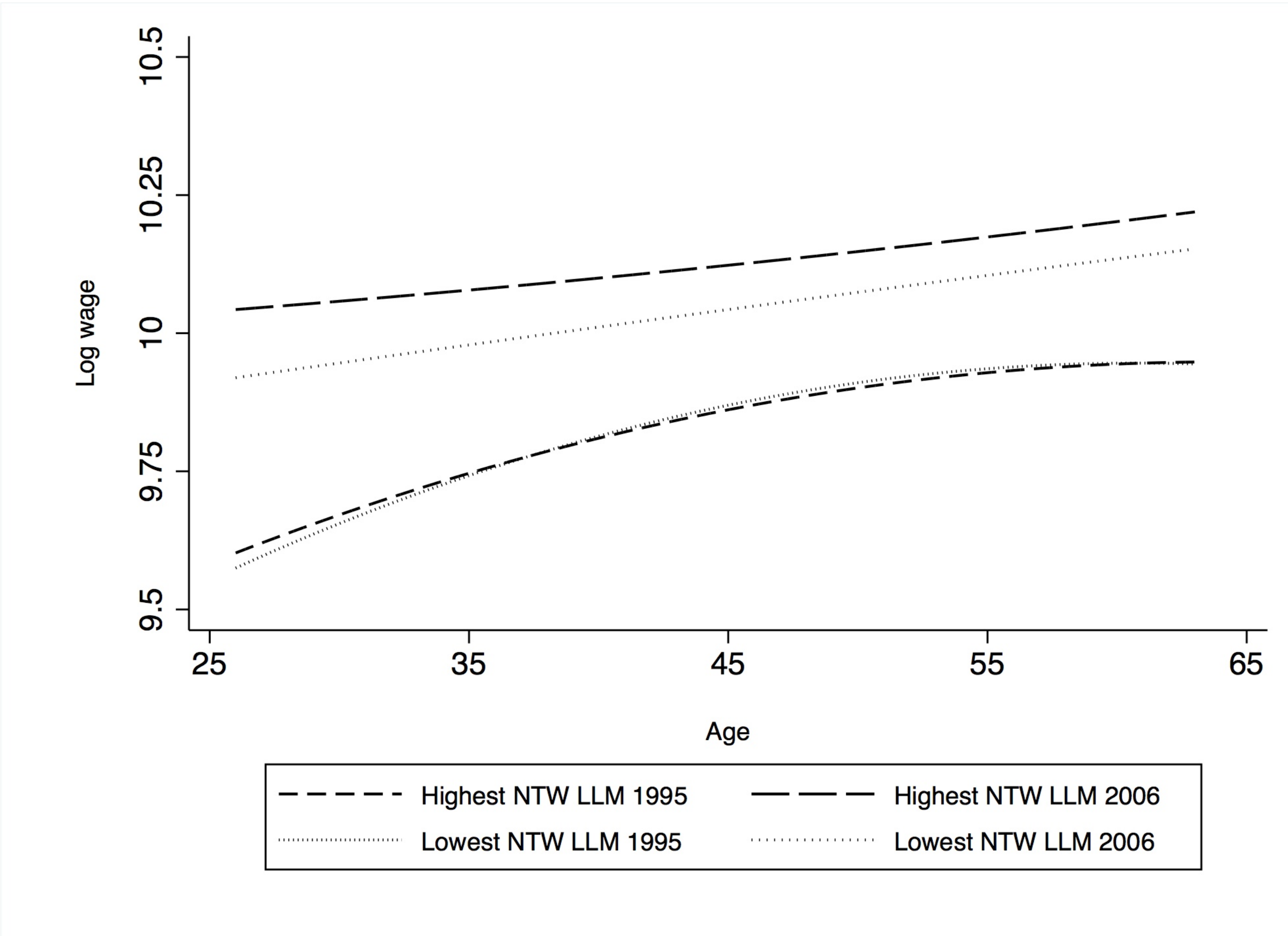
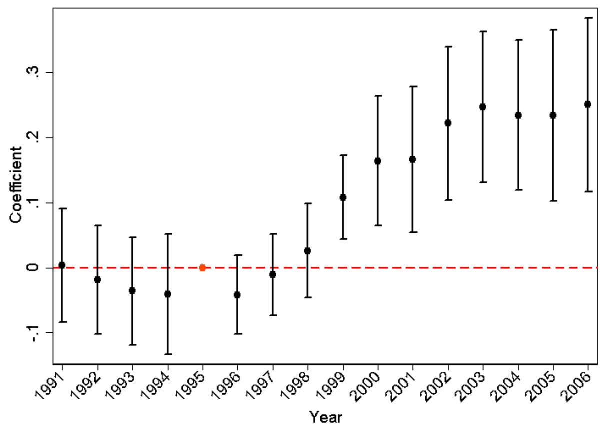
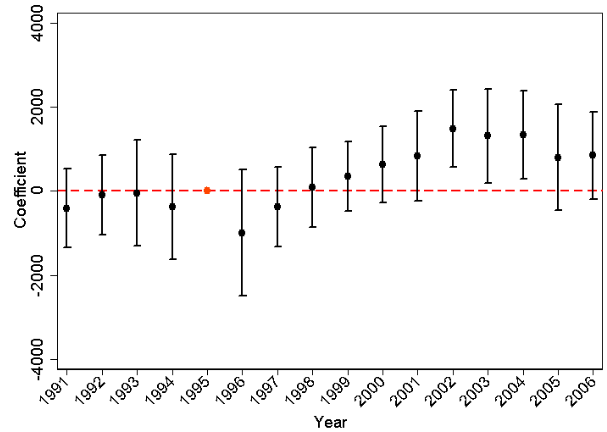


Figure A5: Age-wage profile pre-and post-reform

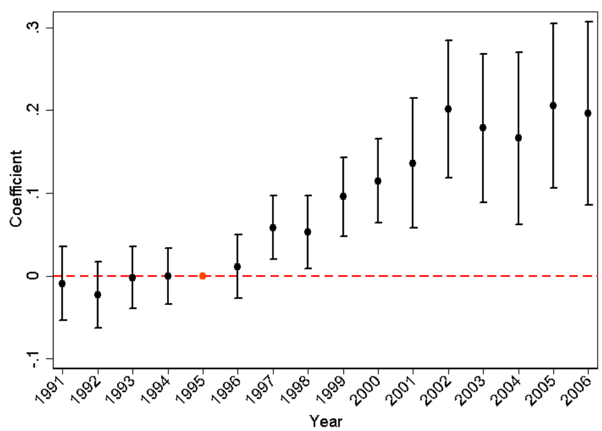
Notes: Author's calculation using 1995-2006 teacher registry data on public elementary school teachers in Sweden. The figure depicts the teacher age-wage relationship in 1995 and in 2006 for (a) all municipalicites in the local labor market with the highest 1995 CENT employment income and (b) all the municipalities in the local labor market with the lowest CENT employment income. The curves show the prediction for log teacher wage obtained from linear regressions of log teacher wage on age and age squared. Teachers aged 25 to 64 have been used for this depiction, as this was the age range for which there were teachers in both local labor markets.



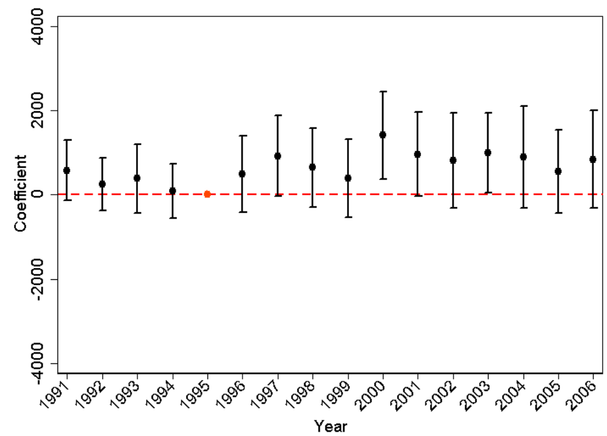
(a) Log wage, young (20-34 years old) teachers



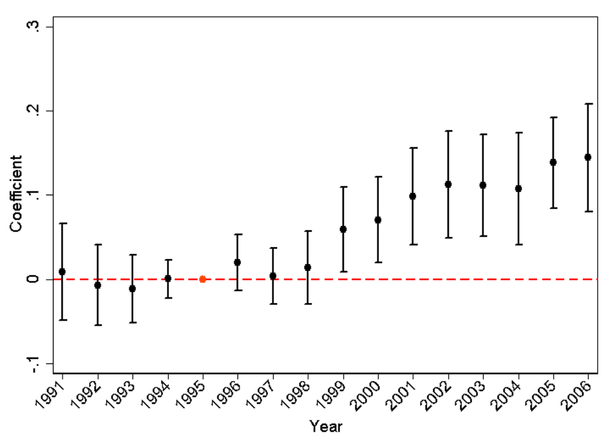
(b) Standard deviation, , young (20-34 years old) teachers



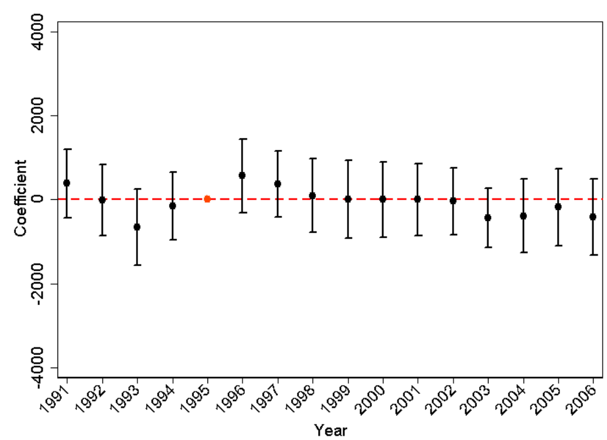
(c) Log wage, mid-career (35-49 years old) teachers



(d) Standard deviation, mid-career (35-49 years old) teachers



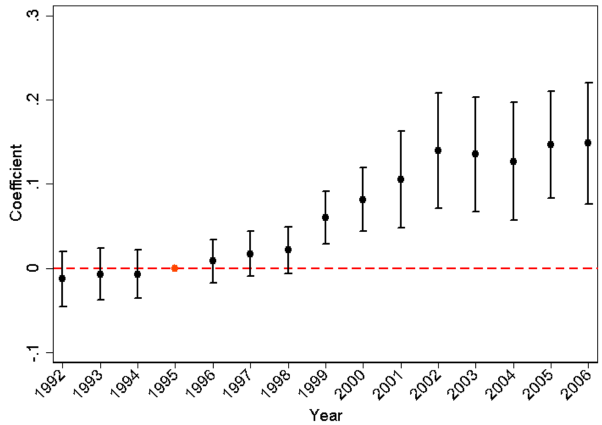
(e) Log wage, old (50-64 years old) teachers



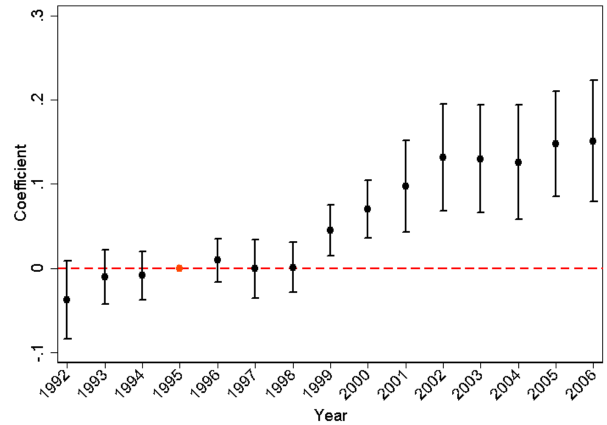
(f) Standard deviation, old (50-64 years old) teachers

Figure A6: Event study estimates - wage structure by age cohort

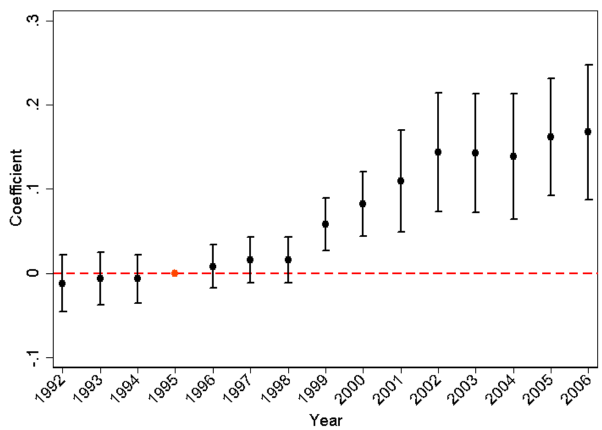
Notes: Author's estimation of equation (1) as described in the text using 1991-2006 teacher registry data on all public elementary school teachers in Sweden. Year 1995 is omitted, so all estimates are in relationship to this year. All estimates include gender-by-year and gender-by-municipality fixed effects as well as municipality controls for fraction males, fraction immigrants, average income, fraction unemployed, average years of schooling, fraction on social security benefits and average age. Each point is a relative time parameter estimate, while the bars extending from each point show the bounds of the 95% confidence interval calculated from standard errors that are clustered at the treatment level.



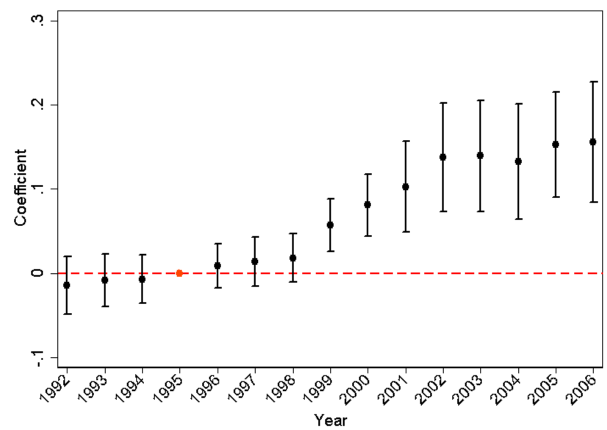
(a) Accounting for Friskolereformen



(b) Accounting for the Balkan war



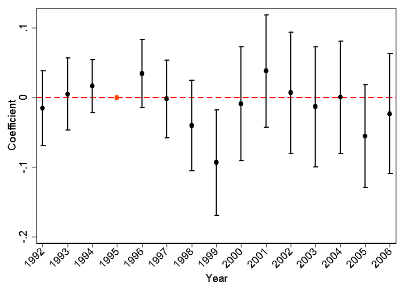
(c) Accounting for demographic changes



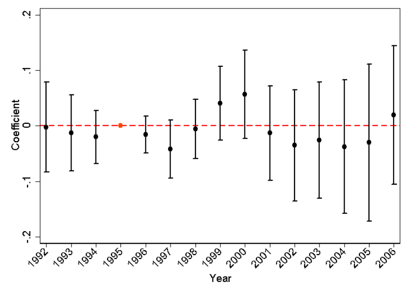
(d) Accounting for Kunskapslyftet

Figure A7: Event study estimates - accounting for potential confounders

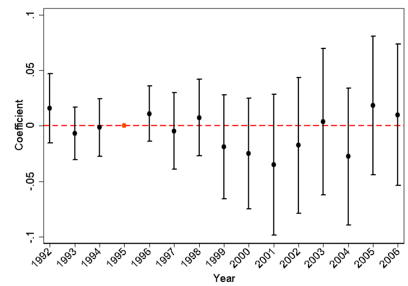
Notes: Author's estimation of equation (1) as described in the text using 1992-2006 teacher registry data on all public elementary school teachers in Sweden. Year 1995 is omitted, so all estimates are in relationship to this year. All estimates include gender-by-year and gender-by-municipality fixed effects as well as municipality controls for fraction males, fraction immigrants, average income, fraction unemployed, average years of schooling, fraction on social security benefits and average age. The regression underlying the estimation of the result in (a) further controls for the fraction of elementary school students that attend friskolor and its interaction with the treatment variable. The regression underlying the estimation of the result in (b) includes an interaction between the fraction of immigrants and the treatment variable for the years in which the Balkan war generated a large inflow of immigrants to Sweden (1993 through 1996). The regression underlying the estimation of the result in (c) includes an interaction between the number of students in elementary school and the treatment variable. The regression underlying the estimation of the result in (d) includes a control for the fraction of individuals with less than a high school degree. The regression underlying the result in (d) also include the interaction between the fraction of individuals with less than a high school degree and the treatment variable for the years in which kunskapslyftet was in effect (1997 through 2002). Each point is a relative time parameter estimate, while the bars extending from each point show the bounds of the 95% confidence interval calculated from standard errors that are clustered at the treatment level.



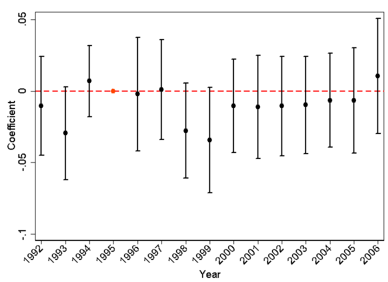
(a) Temporary contract



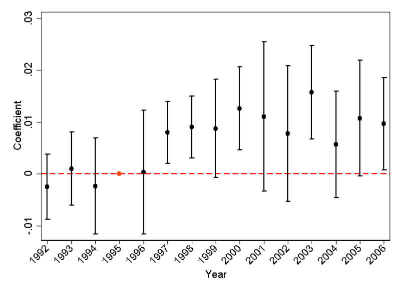
(b) Master's degree



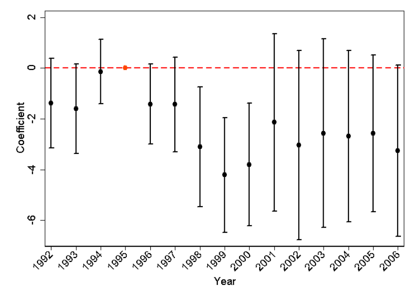
(c) Immigrants



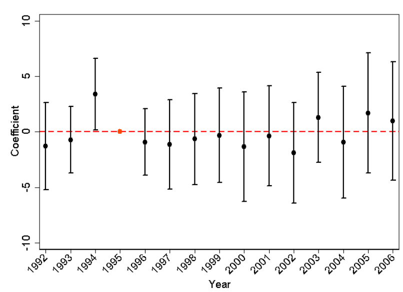
(d) On leave



(e) Switched from private school



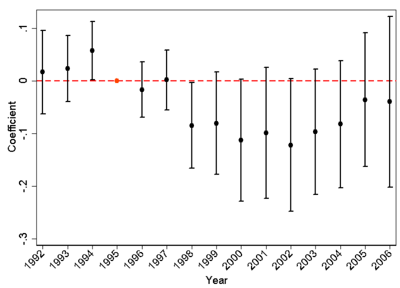
(f) Age



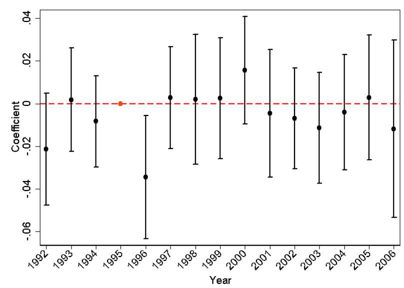
(g) Percent worked

Figure A8: Event study estimates - teacher composition

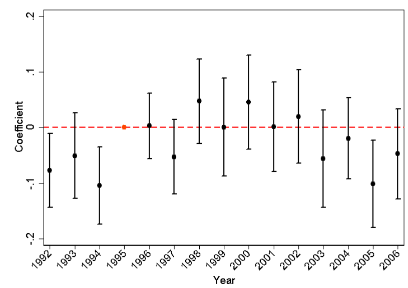
Notes: Author's estimation of equation (1) as described in the text using 1992-2006 teacher registry data on all public elementary school teachers in Sweden supplemented with 1992-2006 public-use data from Statistics Sweden and SNAE. Year 1995 is omitted, so all estimates are in relationship to this year. All estimates include gender-by-year and gender-by-municipality fixed effects as well as municipality controls for fraction males, fraction immigrants, average income, fraction unemployed, average years of schooling, fraction on social security benefits and average age. Each point is a relative time parameter estimate, while the bars extending from each point show the bounds of the 95% confidence interval calculated from standard errors that are clustered at the treatment level.



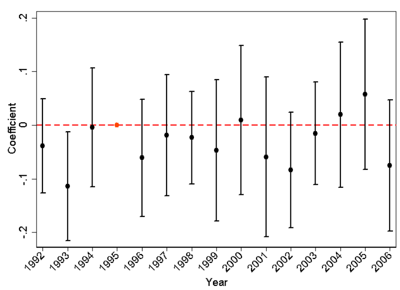
(a) Certificate



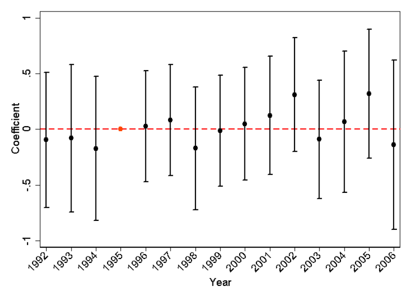
(b) Movers



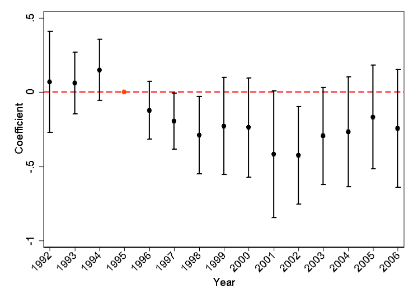
(c) Hires



(d) Stayers



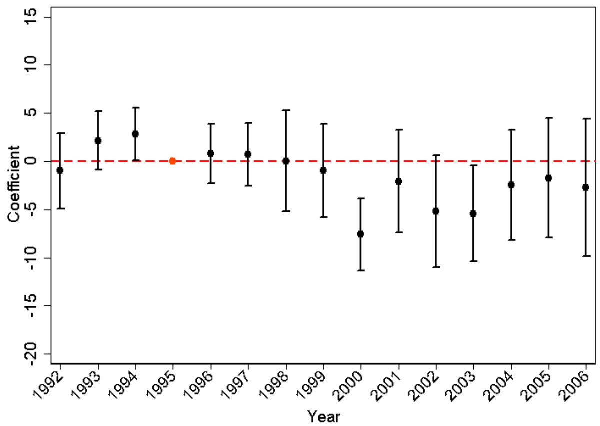
(e) High Movers



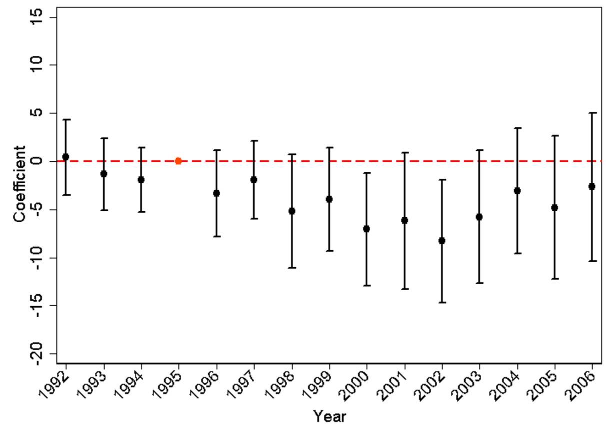
(f) Years of schooling

Figure A9: Event study estimates - teacher composition

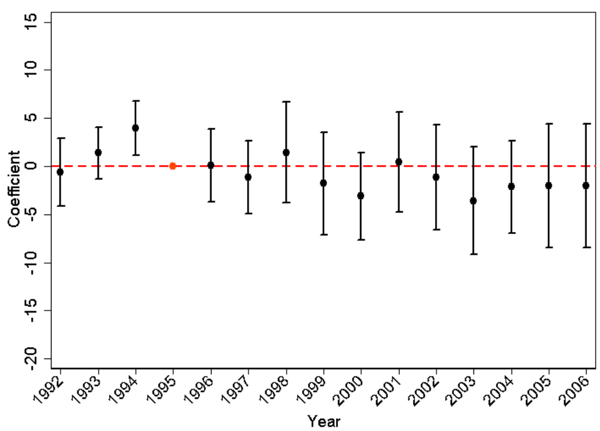
Notes: Author's estimation of equation (1) as described in the text using 1992-2006 teacher registry data on all public elementary school teachers in Sweden supplemented with 1992-2006 public-use data from Statistics Sweden and SNAE. Year 1995 is omitted, so all estimates are in relationship to this year. All estimates include gender-by-year and gender-by-municipality fixed effects as well as municipality controls for fraction males, fraction immigrants, average income, fraction unemployed, average years of schooling, fraction on social security benefits and average age. Each point is a relative time parameter estimate, while the bars extending from each point show the bounds of the 95% confidence interval calculated from standard errors that are clustered at the treatment level.



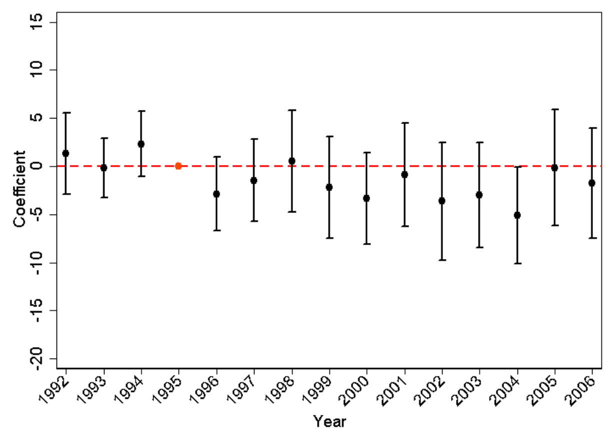
(a) GPA



(b) Math



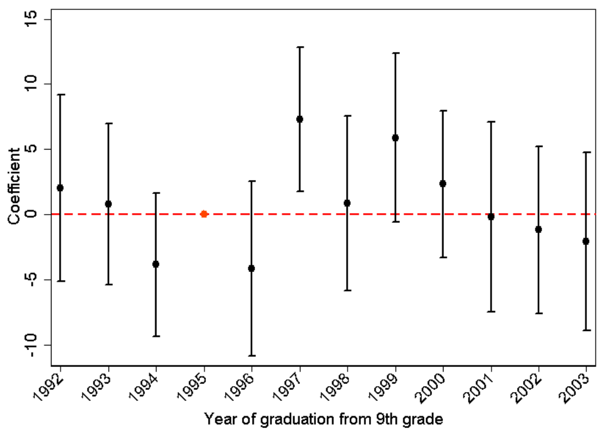
(c) Swedish



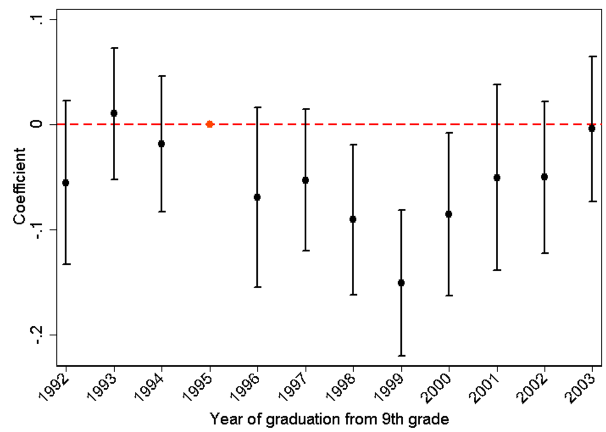
(d) English

Figure A10: Event study estimates - 9th grade outcomes

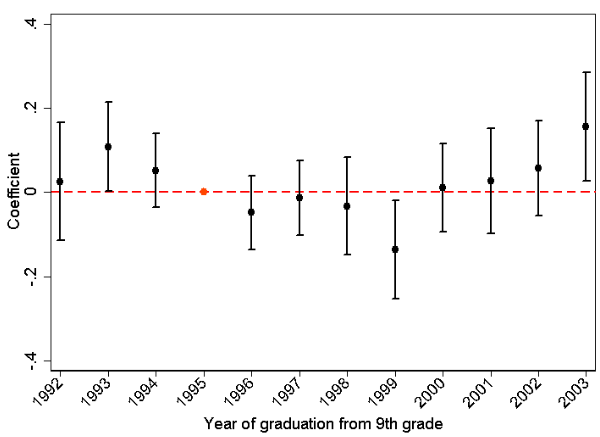
Notes: Author's estimation of equation (1) as described in the text using 1992-2006 registry data supplemented with public-use data from Statistics Sweden and SNAE. Year 1995 is omitted, so all estimates are in relationship to this year. All estimates include gender-by-year and gender-by-municipality fixed effects as well as municipality controls for fraction males, fraction immigrants, average income, fraction unemployed, average years of schooling, fraction on social security benefits and average age. Each point is a relative time parameter estimate, while the bars extending from each point show the bounds of the 95% confidence interval calculated from standard errors that are clustered at the treatment level.



(a) GPA



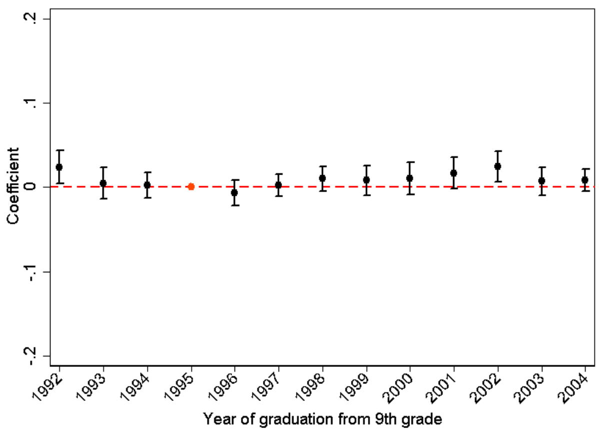
(b) Natural Science Track



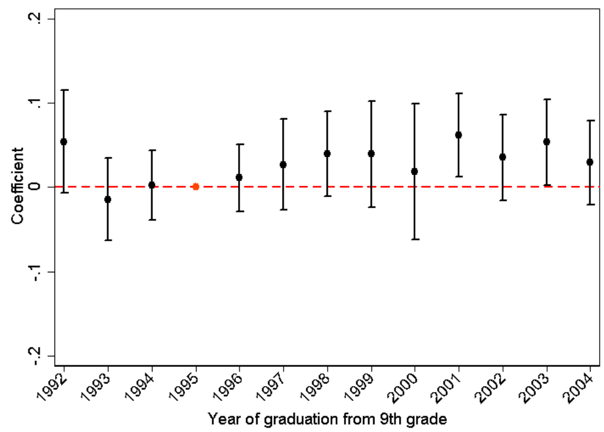
(c) University-Prep. Program

Figure A11: Event study estimates - high school outcomes

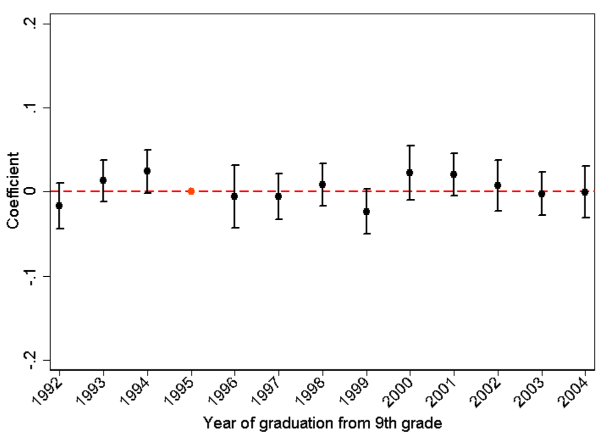
Notes: Author's estimation of equation (2) as described in the text using 1992-2006 registry data supplemented with public-use data from Statistics Sweden and SNAE. High school outcomes are available first three years after students have finished 9th grade, and these outcomes are therefore measured between 1995 and 2006. Students that graduated from 9th grade between 2004 and 2006, while included in the analysis of 9th grade education attainment effects, are excluded from the sample underlying the results in this figure since the most recent year for which I have information on high school outcomes is 2006. All estimates include gender-by-year and gender-by-municipality fixed effects as well as municipality controls for fraction males, fraction immigrants, average income, fraction unemployed, average years of schooling, fraction on social security benefits and average age. Each point is a relative time parameter estimate, while the bars extending from each point show the bounds of the 95% confidence interval calculated from standard errors that are clustered at the treatment level.



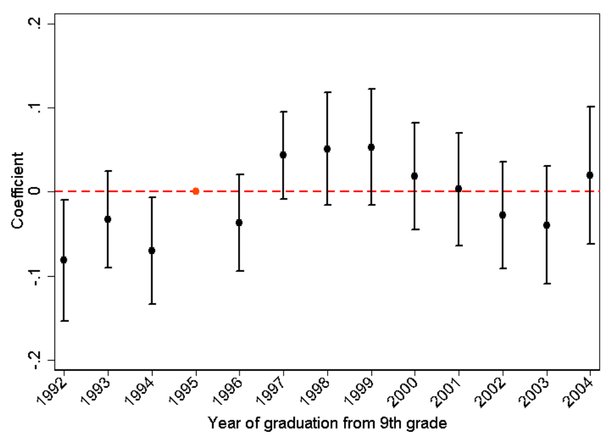
(a) Employment Income



(b) Employment Sample



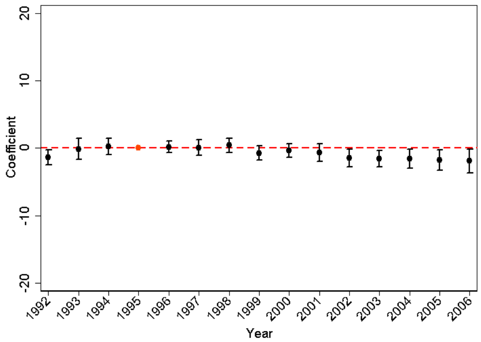
(c) Social Security



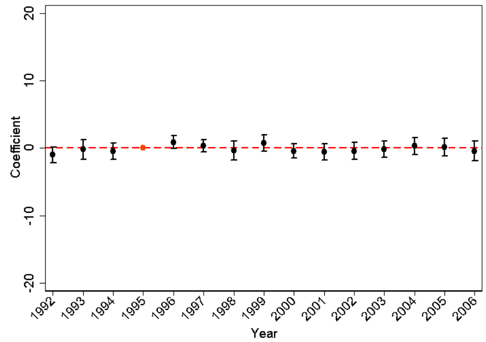
(d) University Enrollment

Figure A12: Event study estimates - higher education and labor market outcomes

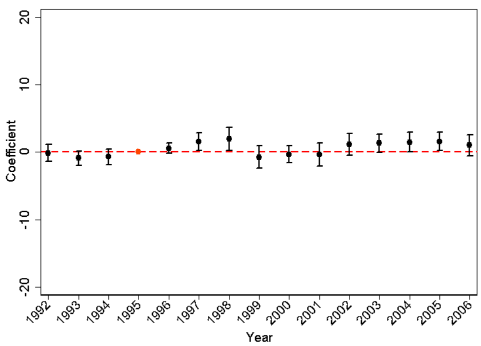
Notes: Author's estimation of equation (1) as described in the text using registry data supplemented with public-use data from Statistics Sweden and SNAE. These outcomes are measured 10 years after the students have graduated from 9th grade, and are obtained from a data registry for which the most recent year I have access to is 2014. Students that graduated from 9th grade between 2005 and 2006, while included in the analysis of 9th grade education attainment effects, are therefore excluded from the sample underlying the results in this figure (since I do not have access to information on these outcomes in 2015 and 2016). Relative year -1 is omitted, so all estimates are in relationship to this year. All estimates include gender-by-year and gender-by-municipality fixed effects as well as municipality controls for fraction males, fraction immigrants, average income, fraction unemployed, average years of schooling, fraction on social security benefits and average age. Each point is a relative time parameter estimate, while the bars extending from each point show the bounds of the 95% confidence interval calculated from standard errors that are clustered at the treatment level.



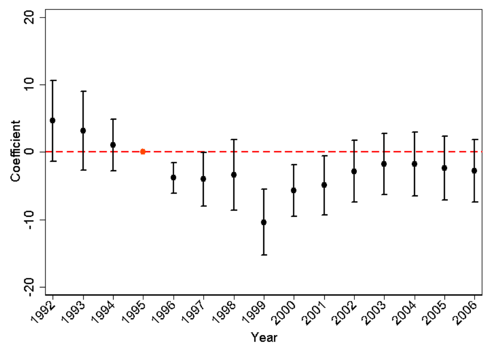
(a) Food



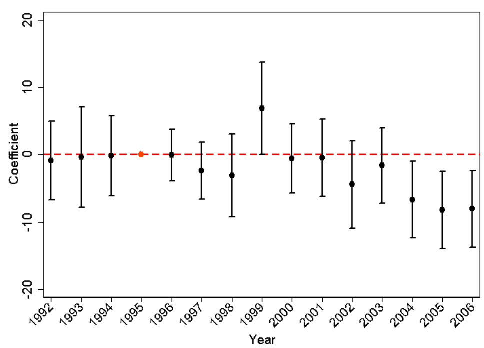
(b) Health



(c) Supplies



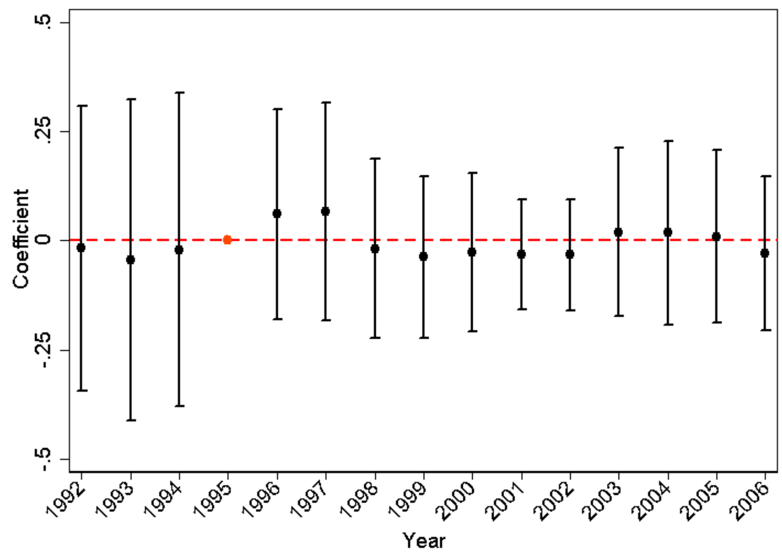
(d) Facilities



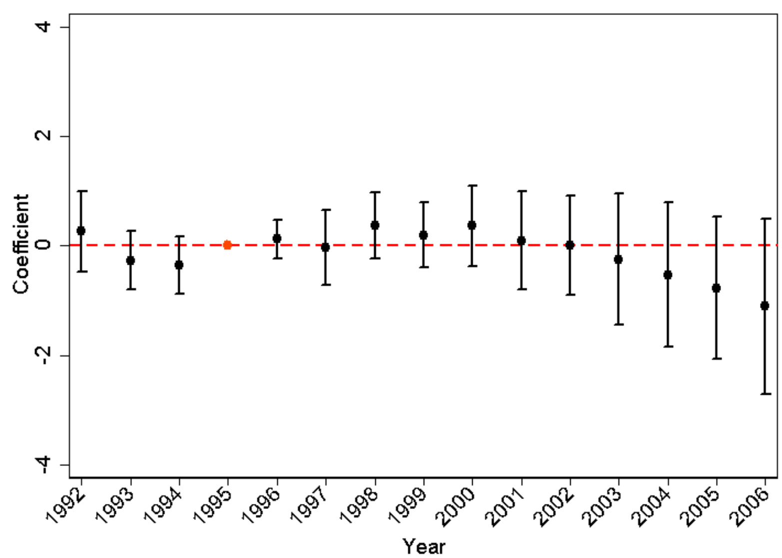
(e) Uncategorized items

Figure A13: Event study estimates - resource allocation

Notes: Author's estimation of equation (1) as described in the text using 1992-2006 registry data supplemented with public-use data from Statistics Sweden and SNAE. Year 1995 is omitted, so all estimates are in relationship to this year. All estimates include gender-by-year and gender-by-municipality fixed effects as well as municipality controls for fraction males, fraction immigrants, average income, fraction unemployed, average years of schooling, fraction on social security benefits and average age. Outcome variables are measured as cost per student divided by total education spending multiplied by 100, and therefore represent the percent of total education spending dedicated to that input. Each point is a relative time parameter estimate, while the bars extending from each point show the bounds of the 95% confidence interval calculated from standard errors that are clustered at the treatment level.



(a) Local tax rate



(b) Teachers per 100 students

Figure A14: Event study estimates - local tax rate and teacher-student ratio

Notes: Author's estimation of equation (1) as described in the text using 1992-2006 registry data supplemented with public-use data from Statistics Sweden and SNAE. Year 1995 is omitted, so all estimates are in relationship to this year. All estimates include gender-by-year and gender-by-municipality fixed effects as well as municipality controls for fraction males, fraction immigrants, average income, fraction unemployed, average years of schooling, fraction on social security benefits and average age. Each point is a relative time parameter estimate, while the bars extending from each point show the bounds of the 95% confidence interval calculated from standard errors that are clustered at the treatment level.

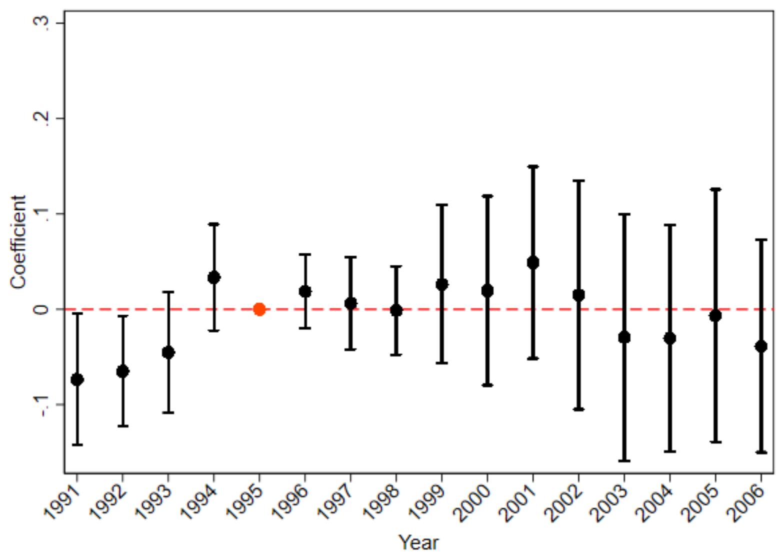
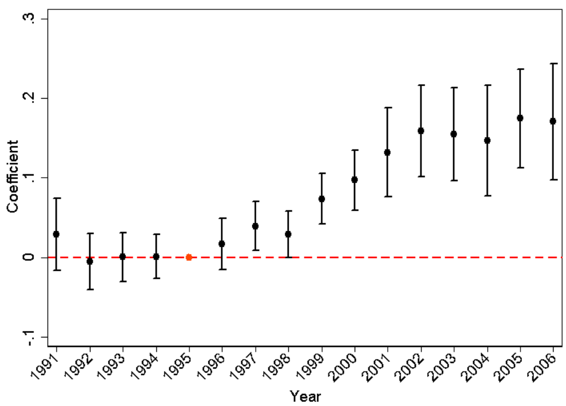
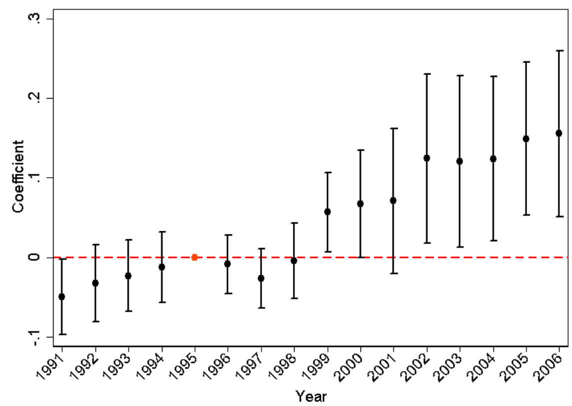


Figure A15: Event study estimates - spillover placebo

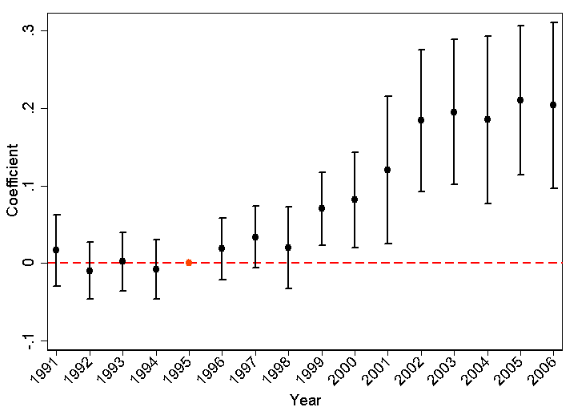
Notes: Author's estimation of equation (1) as described in the text using 1991-2006 registry data. The unit of observation is a municipality-gender-year.The sample includes all workers in the three-digit public occupation group corresponding to the healthcare sector. The sample is further restricted to individuals with an advanced education within the healthcare field (Health; Medicine; Nursing and midwifery; Dental studies; Medical diagnostic and treatment technology; Therapy and rehabilitation; Pharmacy) to ensure that only healthcare professionals in healthcare occupations that require advanced certifications are included. The estimates include municipality-gender, year-gender, field-gender and field-year fixed effects, as well as controls for gender composition, average years of schooling, fraction of social security benefit recepients, mean income, average age and fraction immigrants. Each point is a relative time parameter estimate, while the bars extending from each point show the bounds of the 95% confidence interval calculated from standard errors that are clustered at the treatment level.



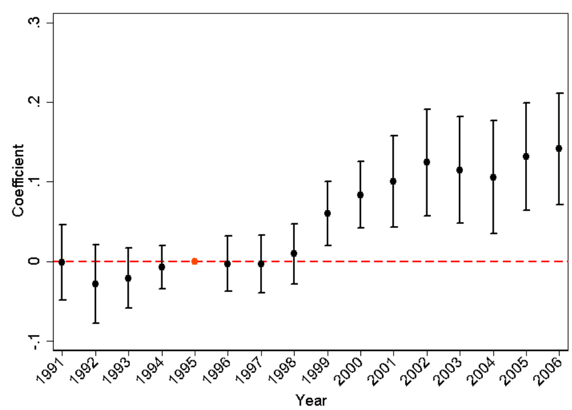
(a) Married



(b) Not Married



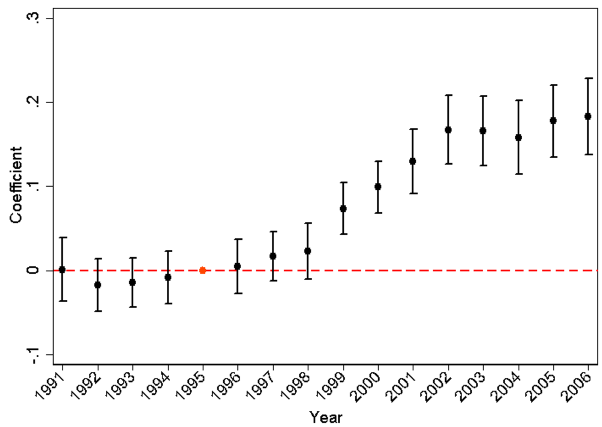
(c) Child present



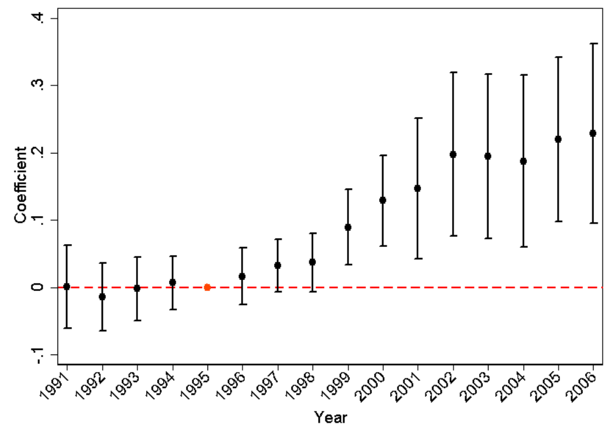
(d) No child present

Figure A16: Event study estimates - mean wage by teacher group

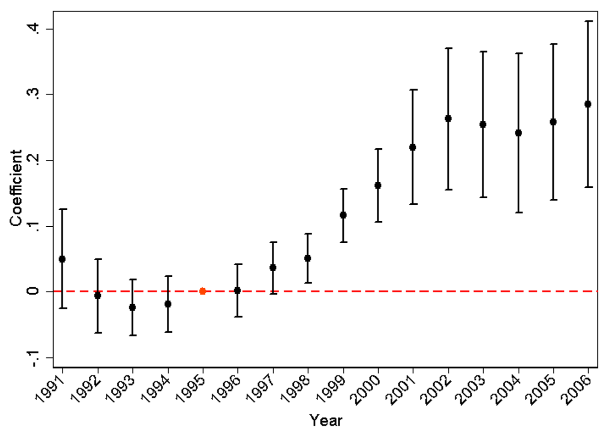
Notes: Author's estimation of equation (1) as described in the text using 1991-2006 teacher registry data on the indicated subsample of public elementary school teachers in Sweden. Year 1995 is omitted, so all estimates are in relationship to this year. All estimates include gender-by-year and gender-by-municipality fixed effects as well as municipality controls for fraction males, fraction immigrants, average income, fraction unemployed, average years of schooling, fraction on social security benefits and average age. Each point is a relative time parameter estimate, while the bars extending from each point show the bounds of the 95% confidence interval calculated from standard errors that are clustered at the treatment level.



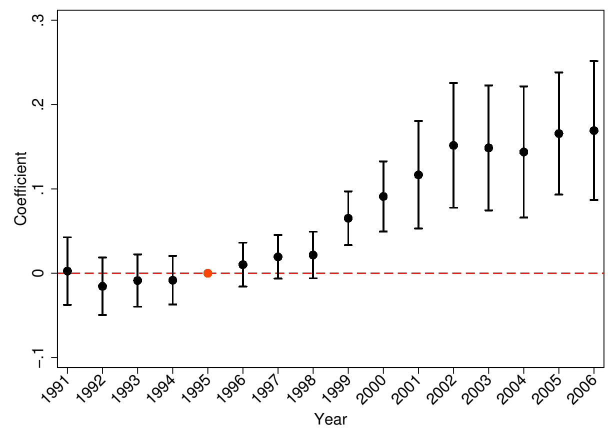
(a) Treatment based on five year pre-reform average CENT employment income



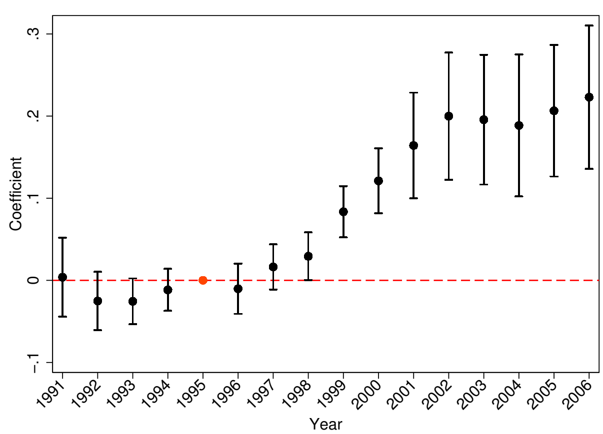
(b) Treatment based on middle 90 percent of CENT employment income distribution



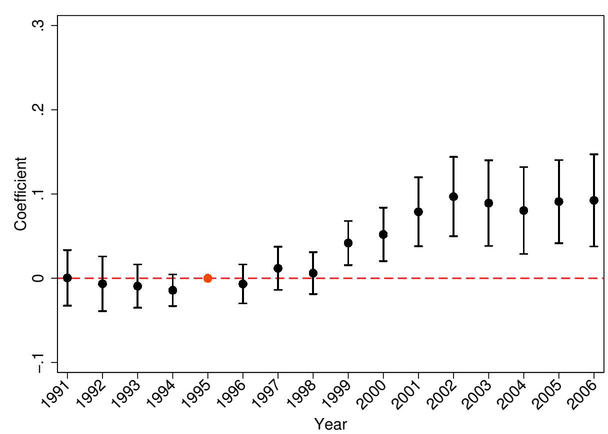
(c) Treatment based on predicted pre-refom teacher employment income



(d) Excluding municipalities that experienced border changes



(e) Treatment based on workers without college degrees



(f) Treatment defined on the municipality level

Figure A17: Event study estimates - sensitivity and robustness analyses

Notes: Author's estimation of equation (1) as described in the text using 1991-2006 teacher registry data on all public elementary school teachers in Sweden. Year 1995 is omitted, so all estimates are in relationship to this year. All estimates include gender-by-year and gender-by-municipality fixed effects as well as municipality controls for fraction males, fraction immigrants, average income, fraction unemployed, average years of schooling, fraction on social security benefits and average age. Each point is a relative time parameter estimate, while the bars extending from each point show the bounds of the 95% confidence interval calculated from standard errors that are clustered at the tretment level.

Table A1: Dependent variable sample means

	Mean
Panel A: Public elementary school teachers	
Switch from private school	0.002
On leave	0.022
Stayer	0.843
Hire	0.095
Master	0.196
Age	46.128
Certificate	0.860
Temporary contract	0.074
Percent worked	91.995
Immigrant	0.074
Female	0.726
Mover	0.011
High mover	0.466
Log mean wage	9.946
Mean monthly wage (000 dollars)	2.640
Standard deviation (000 dollars)	0.321
Interquartile range (000 dollars)	0.477
Panel B: Student education and labor market outcomes	
Percentile math ranking, 9th grade	49.117
Percentile swedish ranking, 9th grade	48.903
Percentile english ranking, 9th grade	48.519
Percentile GPA ranking, 9th grade	48.985
University-preparatory program, high school	0.510
Science track, high school	0.188
GPA, high school	49.864
Ever enrolled, university	0.268
Log employment income, labor market	11.762
Employment sample, labor market	0.899
Social security recepient, labor market	0.043
Panel C: College-educated non-teachers	
Mean monthly employment income (000 dollars)	2.806

Notes: Author's calculation using 1992-2006 teacher registry data on all public elementary school teachers in Sweden, 1992-2006 educational attainment data from the grade 9 registry, 1995-2006 educational attainment data from the high school registry and 2002-2014 labor market data from LOUISE. Each observation is a municipality-gender-year. Note that the summary statistics for the high shool outcomes are based on 6852 municipality-gender-year observations since for these outcomes are available three years after individuals have completion of 9th grade, such that I have three less years of data for these outcomes. Summary statistics for the higher education and student labor market outcomes are based on 7410 municipality-gender-year observations as these outcomes are measured 10 years after students graduate from 9th grade, and the most recent labor market data I have access to is from 2014.

Table A2: Education spending by input

Input	Per Student Spending (000 dollars)	Fraction of Total Per Student Spending
Teachers	3.847	0.510
Supplies	0.309	0.041
Health	0.171	0.023
Food	0.485	0.064
Facilities	1.578	0.210
Other	1.171	0.152

Notes: Author's own calculation based on 1992-2006 public data released by the Swedish National Agency of Education. Costs represent real 2005 values and are shown in 000 dollars.

Table A3: Definition of school cost variables

Teaching	Total cost, primarily wage costs, for all teaching activities, such as classes and mentoring. Wage costs to teachers during training and skills development, as well as any wage costs for substitute teachers, are included in this category as well.
Facilities	Total cost for all school facilities (owned and rented), including inventory and cost of capital as well as operating costs for heating and maintenance.
Food	Total cost of school canteen and cafeteria. Facility costs not included.
Supplies	Total cost for teacher aids and educational material, including textbook costs, library costs and light and sound costs for the classrooms.
Health	Total cost for school doctors, school nurses, school counselors and school psychologists (and any other health related services and initiatives paid for by the school). Special education costs are accounted for under the teaching category.
Other	All costs not accounted for by the above categories, such as cost for school buses, school administration and leadership.
Total	Total cost for the school (sum of all costs accounted for by the above categories).

Notes: Information obtained through SNEA. A full description of these cost categories, in Swedish, can be found at https://www.skolverket.se/statistik-och-utvardering/statistik-i-tabeller/grundskola/kostnader/kostnader-for-grundskolan-ar-2006-1.42723

Table A4: Effect on teacher composition stratified by age cohort

	Age	Master's Degree	Immigrants	On leave	Switched from private school
Effect after 10 years, 20-34 year olds	0.937	-0.070	-0.043	0.015	0.034
	(1.274)	(0.180)	(0.091)	(0.036)	(0.021)
Effect after 10 years, 35-49 year olds	-1.103	0.119	-0.099	-0.011	0.021***
	(1.131)	(0.115)	(0.074)	(0.027)	(0.007)
Effect after 10 years, 50-64 year olds	0.165	-0.145	0.081*	0.061	-0.007
	(1.200)	(0.134)	(0.046)	(0.041)	(0.008)

	Temporary contract	Percent worked	Certificate	Movers	High Movers
Effect after 10 years, 20-34 year olds	-0.465**	5.395	0.003	-0.007	-0.159
	(0.187)	(12.483)	(0.168)	(0.097)	(0.437)
Effect after 10 years, 35-49 year olds	0.025	0.342	-0.162	-0.008	-0.404
	(0.085)	(4.178)	(0.110)	(0.023)	(0.818)
Effect after 10 years, 50-64 year olds	-0.006	5.132	0.045	-0.007	0.507
	(0.036)	(5.044)	(0.084)	(0.007)	(0.901)

	Stayers	Hires	Years of Schooling
Effect after 10 years, 20-34 year olds	-0.064	-0.101	0.385
	(0.176)	(0.155)	(0.529)
Effect after 10 years, 35-49 year olds	0.104	-0.066	-0.404
	(0.088)	(0.072)	(0.305)
Effect after 10 years, 50-64 year olds	-0.218**	-0.038	-0.471
	(0.091)	(0.058)	(0.282)

Notes: Author's estimation of equations (1) as described in the text using 1992-2006 teacher registry data on all public elementary school teachers in Sweden supplemented with 1992-2006 public-use data from Statistics Sweden and SNAE. Regressions are based on 8550 municipality-gender observations. All estimates include municipality-gender and year-gender fixed effects, as well as controls for gender composition, average years of schooling, fraction of social security benefit recepients, mean income, average age, fraction immigrants, fraction of student that attend private schools and total number of elementary school students. Standard errors clustered at the treatment level are in parentheses: *** indicates significance at the 1% level, ** indicates significance at the 5% level and * indicates significance at the 10% level.

Table A5: Effect on teacher-student ratio and the local tax rate

	Teacher-student ratio	Local income tax rate
Effect after 10 years	-1.109	-0.030
	(0.820)	(-0.332)

Notes: Author's estimation of equations (1) as described in the text using 1992-2006 registry data supplemented with public-use data from Statistics Sweden and SNAE. All estimates include municipality-gender and year-gender fixed effects, as well as controls for gender composition, average years of schooling, fraction of social security benefit recepients, mean income, average age and fraction immigrants. Standard errors clustered at the treatment level are in parentheses: *** indicates significance at the 1% level, ** indicates significance at the 5% level and * indicates significance at the 10% level.

Table A6: Public sector occupation groups that teachers came from, and left for, in the year prior to the reform

Occupation	Fraction of Teachers
Travel and Tourist Services	0.001
Research and Development within the Social Sciences and Humanities	0.001
Cleaning and Chimney Sweeping	0.001
Electric Power Supply Services	0.002
Transportation Support Services	0.002
Railroad Transportation	0.003
Cultural and Entertainment Services	0.003
Renting out Properties	0.004
Construction and Building Operations	0.006
Research and Development within the Natural Sciences and Technology	0.007
Employment and Recruitment Services	0.008
Foreign Affaris, Defense, Law Enforcement and Fire Protection	0.012
Religious and Interest Group Services	0.017
Sport Services	0.020
Library, Archive and Museum Services e.tc.	0.021
Other Recreational Services	0.044
Health Care Services	0.083
Public Administration	0.134
Social Services	0.631

Notes: Author's own calculation based on information from the teacher registry and the wage registry for public cector employees between 1994 and 1995. Occupation groups are based on the Swedish three-digit SNI 92 classification.

Table A7: Cross-LLM variation in 1995 CENT employment income (000 dollars)

	Mean	Standard deviation
Panel A: Males		
College-educated non-teacher employment income, 1995	39.954	3.746
Panel B: Females		
College-educated non-teacher employment income, 1995	25.205	1.728

Notes: Author's calculation using information on employment income for all employed college-educated individuals in Sweden from the 1995 Longitudinal Database for Education, Income and Labor Market Participation (LOUISE). Values have been converted to represent real 2005 dollars. See data section for details on sample construction.

Table A8: Effect on wage structure, decile treatment

	Mean Wage	10th Percentile Wage	90th Percentile Wage
Effect after 10 years (Decile 1)	0.011	-0.002	0.024
	(0.012)	(0.017)	(0.017)
Effect after 10 years (Decile 2)	0.055*	0.050	0.051*
	(0.030)	(0.050)	(0.031)
Effect after 10 years (Decile 3)	0.128**	0.114	0.100***
	(0.053)	(0.093)	(0.039)
Effect after 10 years (Decile 4)	0.150***	0.153	0.108***
	(0.053)	(0.096)	(0.035)
Effect after 10 years (Decile 5)	0.165***	0.182*	0.107***
	(0.053)	(0.097)	(0.038)
Effect after 10 years (Decile 6)	0.166***	0.185**	0.109***
	(0.048)	(0.090)	(0.034)
Effect after 10 years (Decile 7)	0.168***	0.183**	0.103***
	(0.046)	(0.088)	(0.033)
Effect after 10 years (Decile 8)	0.165***	0.178**	0.111***
	(0.038)	(0.075)	(0.026)
Effect after 10 years (Decile 9)	0.128***	0.152**	0.072***
	(0.035)	(0.060)	(0.024)

Notes: Author's estimation of equations (1) as described in the text using 1991-2006 teacher registry data on all public elementary school teachers in Sweden. All estimates include municipality-gender and year-gender fixed effects, as well as controls for gender composition, average years of schooling, fraction of social security benefit recepients, mean income, average age and fraction immigrants. Standard errors clustered at the treatment level are in parentheses: *** indicates significance at the 1% level, ** indicates significance at the 5% level and * indicates significance at the 10% level.