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Does Competition from For-Profit Firms Raise Wages for Preschool Workers?

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Abstract

We study the wage effects of an expansion of for-profit preschools in Sweden, which followed after a reform in 2006 removed municipalities' right to veto private entry. The expansion decreased preschool employers' monopsony power by increasing the number of alternative employers. We use a differences-in-differences event study design to evaluate the impact on preschool workers' wages, and find no evidence that wages were affected. In the context that we study, the absence of an upward wage effect may be explained by the entering for-profit firms' propensity to hire less qualified, lower-paid staff, and workers from other sectors of the economy than the preschool sector.

Keywords: monopsony power, for-profit provision, preschool workers, gender gap

JEL codes: H44, I2, J31, J42,

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1. Introduction

Labour market monopsonies are common in the tax-financed service sector. Local governments are often the sole local employers of professions involved in the production of educational, health and care services. Standard economic theory predicts that employer concentration suppresses wages via lower labour supply elasticities (Azar et al., 2024) and/or weaker worker bargaining power (Jarosch et al., 2024). Moreover, the workforce in the tax-financed education and care sectors is dominated by women. Female workers tend to be less mobile, partly due to more extensive family responsibilities, and may therefore shy away from job opportunities that come with increased commuting time or less flexible work arrangements. This in turn results in lower labour supply elasticities among women, which can be exploited by employers in the form of lower wages.¹ The combination of employer concentration and lower labour supply elasticities in a predominantly female workforce provides a potential explanation for why wages for educators and child carers tend to be low.

During the last decades, however, public sector dominance in the production of welfare services has been challenged by quasi-market reforms that have resulted in growing market shares for private providers (Shleifer, 1998). In Sweden, private provision in publicly financed educational services was very unusual in the early 1990s, but currently, private providers cater to over 20 percent of all preschool children, to 16 percent of children at the primary and lower secondary levels, and to 31 percent of upper secondary students. As alternative providers multiply, market concentration decreases, and the market power of the public employer is weakened. Monopsony theory predicts an upwards pressure on wages as a result of these changes.

We test the prediction that decreased employer concentration increases the wage level in the context of the Swedish preschool sector. In particular, we study the wage impact of for-profit firm entry in municipal preschool markets (municipalities are the lowest level of government in

¹ Joan Robinson (1933), who coined the term “monopsony” to describe the case of a single buyer of labour facing an upward sloping labour supply curve, suggested that differences in labour supply curves could be a source of the gender-wage gap. Ransom and Oaxaca (2010) estimate lower supply elasticity among women and suggests that it explains lower wages among women in retail in the United States. Sharma (2023) furthermore finds that monopsony power explains a substantial share of the gender gap in Brazil. Caldwell and Danieli (2024) find that 20 percent of the gender earnings gap in Germany can be attributed to gender differences in outside options, mostly due to differences in implicit commuting costs.

Sweden). We exploit variation stemming from a policy reform, implemented in 2006, that abolished the municipalities' right to veto privately provided preschools to enter their markets. The reform led to an expansion of preschools organized as for-profit firms. We use an event study differences-in-differences design and compare municipalities where a for-profit expansion took place in direct connection to the reform (treatment) with municipalities where a similar increase occurred but later in time (control). In a complementary analysis, we compare municipalities with a *substantial* expansion (treatment) to municipalities with a *small* expansion (control) in the for-profit preschool share in direct connection to the reform. The causality of the estimates relies on the assumption that wages would have developed along parallel trends in the treated and control municipalities in the absence of treatment, i.e. in the absence of the increase in for-profit preschool provision.

The case of for-profit preschool expansion in Sweden has several interesting features: First, the municipal government was practically the sole preschool provider until reforms implemented in the 90s and 00s gradually allowed free entry of private providers. There is thus substantial variation in employer concentration over time and across local labour markets. Second, the preschool sector has a very high share of females in the workforce – over 90 percent – and its wages are low compared to other sectors of the economy. The theories of female-driven low labour supply elasticities are thus salient, and changes to the local labour market structure is a potential remedy against low wages in female-dominated sectors. Third, we study entry of for-profit preschool providers specifically. Organizational forms (municipal, for-profit, and non-profit) can be expected to differ with respect to wage setting and hiring practises. Therefore, it is relevant to chart different configurations of monopsony and challenger employer types. Our configuration is one where for-profit expansion takes place in local contexts of predominantly municipal, and some non-profit, provision.

Our study relates to a large literature on monopsony power and its impact on wages. Manning (2021) reviews the literature and reports ample evidence from studies of different countries and industries that employers have considerable monopsony power.² When it comes to the extent to which this power is actually exercised in order to depress wages, he concludes that there exists

² For other recent review articles, see Card (2022) and Ashenfelter et al. (2022).

such evidence for some labour markets, but that “the importance of monopsony in the labour market as a whole” is less well-established (Manning, 2021, p9). Solokova and Sorensen (2021) provide a meta study which suggests that there is scope for monopsony power to affect wages, and more so among women than among men. The authors point out, similarly to Manning (2021), that this does not necessarily imply that the power is exploited to suppress wages – institutional or other features of the labour market may dampen this mechanism. A review by Azar and Marinescu (2024) addresses how different types of policies – antitrust regulation, measures that facilitate job switching, and interventions to increase job quality – can reduce the wage suppressing effect of monopsony power. Taken together, the literature provides strong evidence that monopsony power is a feature in many labour markets. The extent to which it suppresses the general wage level is less clear. The literature points to a need for further studies on how institutions and regulations, such as antitrust policy and policies that reduce the barriers to switching jobs, interacts with monopsony power and, ultimately, its effect on the wage level.

Our paper adds another institutional aspect to the study of monopsony – employers’ organizational form – as it studies reduced employer concentration in the specific form of for-profit preschools as alternatives to a predominant public sector employer. We furthermore contribute by using variation induced by a policy reform. Studies that leverage reform variation are scarce in the monopsony literature.³ However, two previous studies using Swedish data are related to our study. Thoresson (2024) estimates that the reduction in employer concentration that followed a deregulation of the Swedish pharmacy industry had a positive impact on wages. Hensvik (2012) finds that a higher voucher school share (including both for- and non-profits) is associated with increasing teacher wages, especially for high-skilled teachers and teachers entering the profession.

Whereas the two studies mentioned above find positive impacts on wages from reduced employer concentration, the results from our present study show no sign of any positive impact on the wage level despite significant decreases in employer concentration, measured by the Herfindahl-Hirschman index (HHI), in treated municipalities after the reform. The estimates we present are close to zero and robust to alternative definitions of treatment and control groups. The zero-result

³ Staiger et al. (2010) utilizes a nurse pay reform and finds evidence of monopsony on the labour market for nurses. See also Thoresson (2024) for other related studies. We have not been able to find other recent studies on changes in market concentration induced by reforms, but suggestions are welcome.

also holds when separately studying workers who were previously employed in municipal preschools, i.e. when isolating the effect of increased bargaining power among incumbent workers. Our analysis of the wage development prior to the reform, as well as the trends in a set of municipality variables, by and large support the parallel trend assumption.

Providers of preschool in Sweden receive all their funding from the public purse and cannot increase revenues by charging higher prices. Instead, profits can roughly be generated in the following ways: i) by quality-enhancing innovations that are attractive to parents and enable the establishment to run at full capacity; ii) by efficiency-enhancing innovations that put downward pressure on costs; iii) by cutting costs with negative effects on service quality (Hart et al. 1997).⁴ If the profit-motivation would induce more quality-enhancing investments – option i) – we might expect to see stronger competition for highly qualified workers when for-profits enter. However, in our heterogeneity analysis we observe, if anything, weak indications of a negative effect on the wages of highly experienced preschool teachers with high upper secondary school grades.

Instead, we propose that the motivation among for-profit providers to reduce costs by hiring less experienced and less qualified workers could be one explanation behind our zero-result. Further analysis indeed shows that for-profit providers tend to hire less qualified and younger – i.e. cheaper – workers than municipal preschools.⁵ Many workers in the for-profit preschools were also hired from other parts of the labour market than the preschool sector. These differences in hiring practices might mitigate the otherwise increased bargaining power of incumbent workers as the employer concentration drops; and will also reduce the general wage level via selection. For-profit providers that are motivated by reducing costs can also be tougher wage negotiators. Indeed, Edmark and Persson (2025) shows that for-profit preschools tend to set lower wages than

⁴ A prerequisite for quality-deteriorating cost cutting to give rise to higher profits is that the possibility to run at full capacity is not affected. When quality is essentially non-contractable from the perspective of the municipality, quality is ultimately upheld by the choices and pressures of parents. Whereas it is reasonable that parents care about quality, it is far from certain that they are able to fully observe their preschool's quality relative to other options. The third profit-making channel thus hinges on one or many market failures – e.g. lack of public information on service quality, weak reputation-building mechanisms, difficulty of merging and valuing information on quality, and switching costs – being present. In addition, the period that we study was characterized by growing preschool age cohorts; the number of 1–5-year-olds was 460 702 in year 2001, and 579 514 in 2013. In other words, the quickly growing demand for childcare services, and the potential difficulties for parents in making information-based preschool choices, in combination, prohibits us from ruling out the third profit-making channel.

⁵ We did however not find any clear evidence that the increased for-profit share affected the number of, or composition of, preschool workers in the municipal aggregate.

municipal-run or non-profit preschools, both conditional and unconditional on workers characteristics.

Since we lack direct measures of preschool quality, we are not able to conclusively separate between options ii) and iii) – i.e. we cannot definitively rule out that for-profit preschools are not able to maintain outcome quality with a different mix of workers. Neither can we rule out that there are dimensions of worker qualifications that are unobservable to us but that are prioritized by for-profit preschools, for instance cognitive-, non-cognitive skills and personality traits (e.g. agreeableness and attentiveness) that are conducive to quality in the context of care services.⁶ For instance, Hensvik (2012) finds that voucher schools are more inclined to hire workers without formal qualifications – and from sectors outside education – but with higher cognitive skills. The for-profit preschools that we study, however, tend to hire workers with both lower formal qualifications *and* lower upper secondary grades. Based on these measures, we thus find no support for the argument that for-profit organizations innovate by hiring workers with equivalent alternative (informal) qualifications (see e.g. Hoxby, 2002), although as mentioned, we cannot rule out that for-profits value other aspects that are unobservable to us.

Furthermore, the institutional barriers against reducing costs through hiring low-qualified personnel are relatively weak in the Swedish preschool setting. Regulation states that pedagogical activities in a preschool must be “led by” a preschool teacher but provides no guidelines on how many workers with a preschool teacher, or childcare assistant, degree are required and how often they must be present. This contrasts to the study of upper secondary school in Hensvik (2012), where qualification requirements state that all teacher positions must be filled with certified teachers if possible, and to the study of the pharmaceutical sector in Thoresson (2024) where regulation states that a pharmacist must always be available during opening times.⁷

⁶ We have not been able to find good quality studies pertaining to the effects of formal qualifications and work experience in preschool, there are some studies looking at the importance of formal qualifications in school; e.g. Dobbie & Fryer (2013) find that formal teacher training and certification is not related to charter school outcomes. Harris & Sass (2011) find a positive relationship between teacher productivity and (early) work experience.

⁷ Certified teachers make up over 70 percent in primary and lower secondary school (Skolverket, 2025b). Pharmacists make up 50–60 percent of the workforce in the pharmaceutical sector, and preschool teachers make up around 40 percent of the workforce in the preschool sector.

To conclude, it is possible that our zero-results reflect a combination of explanations that include the attenuating effect of cost-minimizing entering providers and lower labour supply elasticities in the preschool workforce. We suggest that further studies on the role of organizational form for the wage impact of employer concentration constitute an interesting area for future research.

2. The Swedish preschool sector

2.1 Institutional setting

The 290 local governments, the municipalities, are responsible for supplying and funding preschool in Sweden. Provision can be organized either inhouse – by the municipality – or be outsourced to private organizations, which can be of any organizational form. Most municipalities have a mix of municipal and private providers, with parental choice and availability determining placement. In 2023, around 20 percent of preschool workers (full-time equivalents) were employed in private organizations. These organizations receive funding (“vouchers”) from the municipalities based on the number of children attending their preschools and according to the same guidelines the municipalities rely on to fund their own preschools. The municipalities’ main sources of revenue are the local income tax and central government grants. The municipal funding is complemented with a parental fee which is very low and regulated. The same rate applies irrespective of the chosen preschool provider – municipal or private. Approximately 94 percent of all children in ages 4–5 attend preschool.

2.2 Staff qualifications, requirements and wage setting

The preschool sector is one of the largest sectors of employment for women; according to Statistics Sweden, preschool workers (childcare assistants and preschool teachers) were on the top ten list of female occupations in 2023. Regarding preschool worker qualifications, Swedish law stipulates that the preschool curriculum must be enforced by a certified preschool teacher, but there are no binding rules regarding the share of staff that require a preschool teacher degree. The preschool average share of workers with a preschool teacher degree was around 40 percent in 2023; 44 percent in municipal preschools and 31 percent in privately provided preschools. A preschool teacher degree currently requires 3.5 years of higher education studies. Childcare assistants receive their qualifications at the upper secondary school level or from an adult vocational training

program of 1.5 years of full-time studies. Approximately a third of staff had either completed an upper secondary preschool related degree (16 percent) or had some other type of degree related to working in the education sector as a whole (20 percent). The National Agency for Education furthermore provides guidelines for the child-staff ratio, but no binding regulations.

Preschool worker wages are determined through a combination of individualized negotiations and collective agreements between employer and employee organizations. Most collective agreements outline expected wage increases at the firm or sector level in percentage terms, while others focus solely on establishing the procedures for wage negotiations without specifying any figures. Regardless of the type of agreement, there remains room for individual wage discussions between employees and employers within the collective agreement. This combination of individual and collective bargaining has been in place since the mid-1990s.

3. The 2006 free entry reform

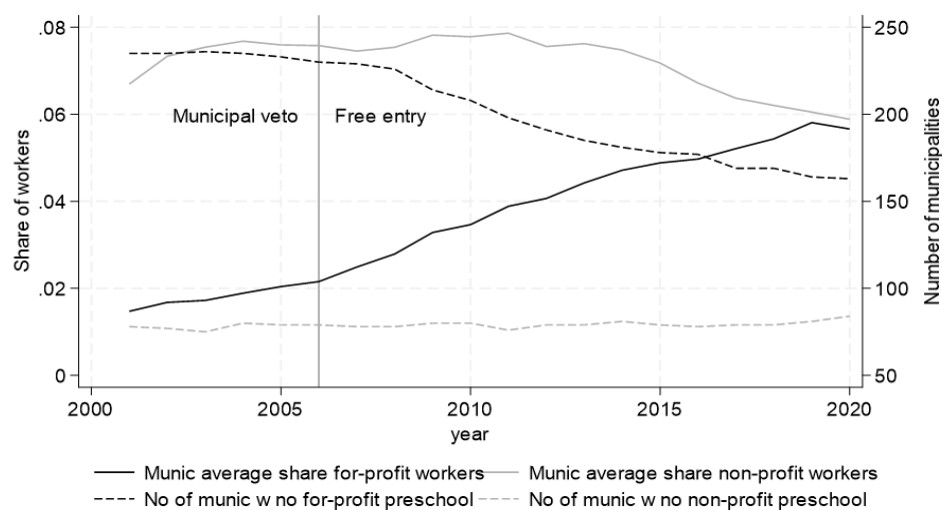
During the 1980s there was growing political pressure from centre-right opposition parties (liberals – in the European sense – and/or conservatives), and from some municipalities, to deregulate the preschool market and allow public preschool grants to be channelled to private providers. The controversy was the strongest regarding for-profit preschool enterprises – they were opposed by the centre-left parties (the Social Democrats and the Left Party) but advocated by the centre-right. By the 2000s, a series of political decisions had led to a system where it was up to the municipal governments to decide, on a case-by-case basis, whether private providers would be allowed to operate and receive public funding in the municipality. The centre-right parties, however, continued to push for nation-wide free entry, and in 2006, having gained the support by the Green party, they managed to vote through a parliamentary bill that abolished the municipal veto, overriding the Social Democratic minority government.⁸ The law specified that municipalities must approve and provide public funding to any private preschool that could show that it would be able to fulfil the quality requirements specified by laws and regulations. Funding could be denied only if the municipality could show that a new preschool would have significant negative consequences

⁸ See Appendix section A1 for a more detailed exposition regarding the 2006 reform.

for the municipality's economic possibilities to provide preschool, or if the parental fee was unreasonably high compared to that levied by the municipal preschools.

As can be seen in Figure 1, the 2006 reform had a substantial impact on the local presence of for-profit preschools. The average municipal share of preschool workers employed in for-profits increases faster after 2006, and the number of municipalities with no for-profit preschool starts to drop rapidly a few years after the reform.⁹ For the non-profit private sector there is however no discernible positive impact of the 2006 reform; the average share of non-profit workers among the municipalities remains at approximately the same level until around 2014, after which it declines somewhat. The number of municipalities without any non-profit preschool was low and constant during the studied period.

Figure 1. Trends in market shares and for-profit presence before and after the 2006 reform



Note: Municipal level unweighted averages, 288 observations.

⁹ The lagged response in the drop of municipalities with no for-profit preschool can be explained by the fact that the process of starting a new preschool takes time. On the other hand, the more immediate increase in the share of for-profit workers might partly reflect an expansion of already existing establishments. In addition, the free-entry regulation was consolidated in 2009, when the right to municipal funding was clarified through the introduction of a voucher system. This may have spurred expectations among prospective private providers further.

Table 1 shows that the presence of for-profit preschools was, prior to 2006, strongly correlated with the municipal council seat share for the largest party among the centre-right parties – the Moderate Party; the party arguing most strongly in favour of allowing for-profit firms to enter the preschool market. The top coefficient of Column 1 suggests that a ten-percentage point increase in the municipal council seat share for the Moderate Party is associated with almost 1.8-point higher log-odds, or an increase in the odds ratio of nearly 6, of there being at least one for-profit preschool in the municipality. After 2006, the correlation is reduced by more than half (the second coefficient of Column 1). This suggests that, prior to the reform, local politics restricted for-profit entry and expansion in markets with characteristics that otherwise made them attractive to for-profit providers. In other words, for-profits could not freely select local markets before the reform, and the degree to which they were restricted was largely determined by local political preferences.

Table 1. The likelihood of at least one for-profit or one non-profit preschool in the municipality

	(1) >0 For-profit	(2) >0 Non-profit
The Moderate Party	1.775*** (0.301)	0.387 (0.236)
Post 2006 # The Moderate Party	-1.026*** (0.251)	-0.212 (0.150)
Ln ages 1-5 population	1.787*** (0.173)	1.630*** (0.248)
Ln tax base	0.207 (1.550)	-0.978 (1.532)
Observations	5,759	5,759

Note: The variable Moderate Party is the seat share for the Moderate Party in the municipal council. It is scaled such that a one-unit increase reflects a ten-percentage point increase. The regressions include year fixed effects. Standard errors are clustered at the municipality level. ***<0.01, **<0.05, *<0.1.

Column 2 shows no similar pattern for the non-profit preschools; the coefficient for the Moderate Party is small and insignificant and does not change significantly after 2006. Table 1 also shows that both for- and non-profit preschools are more likely to be present in larger municipalities, here measured by the size of the preschool age (1–5 years) population. This is not surprising, given that service providers prefer markets with a larger potential customer base.

4. Empirical method

The fundamental methodological problem that we need to address is that an observed correlation between for-profit market share in a municipality and the wage level need not mean that the former caused the latter. Instead, there may be other underlying local factors that affect both the wage level and the for-profit share, such as the population size, urbanization rates, and income levels. Reverse causality is also a possibility. For instance, for-profits might prefer to enter local markets where wages are lower.

We solve these identification issues by making use of the 2006 free entry reform in an event study design (see Miller, 2023, for an overview of the event study design). Using the reform has two benefits: First, it provides substantial variation in the for-profit preschool share (Figure 1). Second, given the design of the reform, it is plausible that the for-profit increase after 2006 stemmed from for-profits being allowed to enter municipalities where they had previously met substantial political barriers, rather than from sudden changes in the development of municipal characteristics that determine the attractiveness of the market. We make use of the variation induced by the reform to define treatment and control groups based on the observed development of the for-profit presence in the municipalities over time, a strategy that resembles the methods used by e.g. Havnes & Mogstad (2011a and 2011b) and Silliman & Mäkinen (2022).

Before we turn to the more detailed definitions of treatment and control groups, empirical strategy, and underlying assumptions, one can note that our focus on variation between municipalities implies that we treat municipalities as separate local labour markets. Our data suggests that this is a reasonable choice: the share of preschool employees in our data who work in the municipality where they reside is almost 90 percent. The risk that the results are attenuated by spill-over effects from treatment to control municipalities is thus low.

4.1 Definition of treated and control municipalities

We use two sets of treatment and control groups in our panel of municipalities spanning 2001–2020. First, we define as treated all municipalities that had no for-profit private preschool in the five years leading up to the reform year, i.e. in 2001–2005, but who got at least one for-profit preschool in the seven years following the reform year, i.e. during 2007–2013. We do not view year 2006 as a pure treatment year since the reform took effect in the middle of the year, on July

1st. We then construct a control group of municipalities with no for-profit preschool during 2001–2013, but where at least one for-profit preschool entered during the seven subsequent years, 2014–2020. We argue that using these “later-treated” municipalities as a control group improves the comparability of the treatment and control group. The underlying assumption is that the timing of the increase in for-profit preschools is near random with respect to underlying trends in the outcome variable; in other words, that both sets of municipalities are in principle equally attractive markets for for-profit providers, but that entry is distributed exogenously over time with respect to local wage trends.

We complement the above approach with a second set of treatment and control groups. Here, we draw inspiration from Havnes & Mogstad (2011a and 2011b), who study the impact of a childcare expansion reform in Norway on children’s educational outcomes and labour force participation (2011a), as well as the labour force participation of the mothers (2011b). They rank municipalities according to the post-reform expansion in childcare and split into two groups at the median. We apply this approach to our case by dividing municipalities into a treatment and a control group based on the growth in the for-profit preschool worker share during the seven years following the free-entry reform, i.e. during 2007–2013.¹⁰

The second treatment-control definition, “high-low growth”, provide a complementary source of variation, which takes into account the full rate (intensive margin) of for-profit expansion after the reform, instead of only relying on extensive margin changes as we do in the first definition, “early-later treated”.¹¹ Before ranking municipalities according to preschool share growth, however, we drop the very small municipalities with no for-profit preschool at any point during 2001–2013. These very small municipalities are likely to differ in many respects from the larger municipalities

¹⁰ The political associations with for-profit entry documented in Table 1 might lead one to consider assigning municipalities into treated and control groups based on the local council seat share of the Moderate Party. The control group would then be “always-treated” in the sense that they were always open to for-profit preschools, whereas the treated group would become treated first after the reform. Although this is in theory a possible alternative, in practice it is unfeasible. Having municipalities where the share of for-profits changes both before and after the reform as the control group would make it hard to quantify effects and would require us to make strong assumptions. In particular, we would need to assume that the for-profit impact is not dynamic or heterogeneous – otherwise the impact of the pre-reform for-profits in the “always-treated” control group could turn up in the post-reform years and bias the results.

¹¹ The early-later treated groups consist of 51 municipalities in the treatment group and 28 in the control group, The high-low-growth groups consist of 49 treated and 48 control municipalities. There is some overlap: 29 of the treated municipalities in the early-later group are included as controls in the high-low growth definition, and 20 municipalities are included in the treated group in both definitions.

that experienced substantial increases in the for-profit share after the reform, meaning that the likelihood that they were to be on parallel trends with the treated municipalities in the absence of treatment is probably lower than for municipalities with some for-profit preschool entry¹².

Our preferred choice of treatment period (2007–2013) is relatively long. This is motivated by the fact that there is likely some inertia in for-profit expansion following the reform. Given that the reform was unexpected in the sense that it was voted through parliament by the opposition against the sitting government, it is reasonable that it took some time for for-profit actors to adjust to the new situation. Furthermore, the process of starting a new preschool is in itself likely to take some time. For completeness, we also show the event study coefficients for the subsequent seven years, 2014–2020, i.e. outside of the period for which the treatment groups are defined.

Figures 2a and 2b show the for-profit worker share for the two alternative sets of treatment and control groups respectively. In Figure 2a, the for-profit worker share in the early treated municipalities increases from zero at the time of the reform to around seven percent by 2013 and then increases a bit more before levelling out at around eight percent at the end of the period. The share in the control group is (by construction) zero until 2013 and after that increases to similar levels at around 9 percent. The fact that the control and treatment group eventually end up with similar levels of for-profit worker share can be seen as an indication that the groups are similar in terms of the scope for for-profit entry but differ in timing.

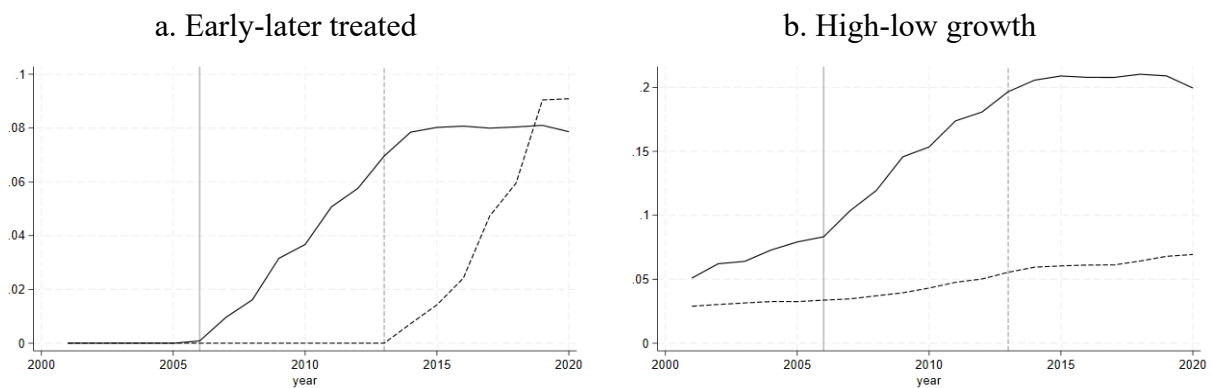
Figure 2b shows that the for-profit worker share increases from 8 percent, at the timing of treatment, to almost 20 percent in 2013 in the high-growth municipalities. For the control group we observe a much smaller increase, from 3.4 percent in 2006 to 5.5 percent in 2013. The figure shows that the for-profit shares in the two groups diverged substantially in the period after the reform. Although treated municipalities experienced a somewhat stronger for-profit growth share than the control group also before the reform, the kink at the timing of the reform is clearly visible.

For-profit expansion is expected to reduce employer concentration as more firms enter the local labour market. Appendix Figure A1 indeed shows that an increase in the for-profit share is associated with substantial reductions in the Herfindahl-Hirschman index (HHI), calculated using

¹² They are however included as controls according to the first definition (given that they were “treated with” for-profit preschool entry after 2013) since they in that case act as controls to treated municipalities that are more similar.

worker shares. In the top panel of Appendix Figure A1, the HHI decreases rapidly after 2006 in the early-treated municipalities, from around 0.85 to 0.75¹³, and from almost 0.7 to a bit over 0.55 among the high-growth municipalities. The lower panels of Appendix Figure A1, where the HHI is used as an outcome in event study regressions, establishes that these decreases are significantly different from the development in the control municipalities.¹⁴

Figure 2. For-profit worker shares in treated (solid) and control (dash) municipalities.



Note: Note the different scales on the y-axis. Municipal level unweighted averages. The vertical solid grey line indicates the timing of the free-entry reform, and the vertical dashed grey line indicates the end of the period used for determining the treatment and control group.

As was noted above, our empirical strategy hinges on the assumption that the entry of for-profits in the treated municipalities after 2006 (Figure 2a), or the larger increase in the treated municipalities (Figure 2b), was due to the abolishment of the veto – and not to other changes in municipal characteristics that are correlated with the local wage level. While this assumption cannot be formally tested, we employ the following informal ways of evaluating it:

First, Figures A3 and A4 in the Appendix show how the log of the local preschool age population and taxbase, and the local government seat shares for the Moderate Party and for the two center-left parties, the Social Democrats and the Left party, developed over the studied period. Although

¹³ A perfect monopsony case corresponds to a HHI of one, since all workers are then employed with one single employer. The average HHI is below one in both the early- and the later-treated municipalities because of a small presence of non-profits.

¹⁴ Results that are available in Appendix Figure A2 shows that the for-profit increase is associated with some reduction in the non-profit worker share, especially in the early-later treated specification. This decrease is however substantially smaller than the for-profit increase. As shown by the HHI, the net change is a decrease in the employer concentration.

the levels sometime differ substantially, they overall follow roughly similar pre-reform trends over time. There are thus no immediately visible signs that the parallel trend assumption is violated. Event study estimates available in Appendix Figures A5 and A6, which provide a more formal test of the assumptions, shows a similar picture; for most of the variables there are no signs of diverging trends. The exception is the log of the preschool age population, where there are indications of a somewhat faster increase in the treatment group than the control in the early-later treated case. This could give rise to an upward bias in the wage estimates, as increased demand for childcare improves the bargaining position for preschool workers. We, however, control for the preschool population size in the regressions, along with the other municipal variables. In addition, results available in the Appendix (Figure A18) show that the estimates are insensitive to removing the municipal covariates; suggesting that the estimates are not affected by the differential population trends. Finally, the differential trend in preschool age population size is not present in the high-low growth case. This speaks for the added value of using alternative treatment and control group definitions as a robustness check.

4.2 Event study estimation equation

We use the above generated sets of treated and control municipalities to estimate the following event study regression equation:

$$y_{imt} = \sum_{l=\underline{l}}^{\bar{l}} \beta_l \cdot D_{imt}^l + \alpha_m + \tau_t + \gamma \cdot X_{it} + \delta \cdot M_{mt} + \varepsilon_{imt} \quad (1),$$

where y_{imt} denotes the outcome of worker i , employed in a preschool in municipality m in year t . β_l contains the coefficients that capture the differences in the outcome variable between the treatment and control group in each year (lags and leads) compared with the difference in the reference year 2005 ($l=0$). The β_l -coefficients with $l < 0$ provide an informal test of the parallel trends assumption; if they are non-zero, there are indications of diverging wage trends between the treated and control municipalities already before the reform. The impact of the reform is captured by the β_l -coefficients with $l > 0$. Although 2006 is technically the first treatment year we do not expect any substantial effects since the reform took effect in the middle of the year.

Furthermore, in Equation 1, α_m and τ_t denote municipality and year fixed effects. X_{it} is a set of worker covariates, including age (in level and squared) and indicator variables for being female, Swedish-born, having a preschool teacher degree, and having a child assistant degree. Finally,

M_{mt} is a set of time varying municipality level covariates, including the log of the preschool age (age 1–5) population (in level and squared), the log of taxable income, and the seat shares in the municipal council for the liberal-conservative Moderate Party as well as for the two left-wing parties, the Social Democrats and the Left Party. The standard errors are clustered at the municipality level, i.e. the panel variable (Miller, 2023). We rely on individual-level data when estimating the regression equation, but we apply weights corresponding to the inverse of the number of workers per municipality-year so that each municipality gets equal weight in the regression. This is done because Swedish municipalities vary considerably in size, and we want to avoid that large municipalities have a dominating influence on the results. Furthermore, using the municipality as the entity of observation is reasonable as it is the level where political decisions that impact the structure of the preschool market are made.

5. Data and description

5.1 Data sources and definitions

The analysis is based on data on workers and preschools stemming from the nation-wide registers on workers and workplaces from Statistics Sweden. Workplaces in the preschool sector are further classified by us as belonging to the municipal, non-profit or for-profit sector depending on their organizational form. While the classification into municipal and private sector is trivial, the classification of the private preschools into non- and for-profit requires some discretion. Preschools that are coded as limited companies are classified by us as being “for-profit”. The non-profits are made up of all other forms of private organization, including economic and other associations, foundations, interest groups, and a small number of non-corporate firms. Economic association is the most common form of private organization in preschool after limited companies (*Ekonomisk förening* in Swedish). Economic associations are not restricted from making a profit and giving out dividends to its members, but we expect profit-making to be a rather unusual primary objective of economic associations in the preschool sector since most economic associations are organized either as parental- or worker cooperatives. In 2020, the national level share of workers in for-profit preschools was around 11 percent, while the share of workers in non-profit preschools was around 7 percent. The remaining share was employed by municipalities.

We only include in our sample workers that have a strong employment attachment to their preschool employer, i.e. very sporadic part-time workers – for instance summer substitutes – are excluded. A “strong” employment attachment is defined as having annual earnings from a preschool employer that surpasses one income base amount.¹⁵ We furthermore restrict the sample to only include workers with occupations related to working with children, so that other preschool staff working in the kitchen, with cleaning or with administrative tasks are excluded.

Our main outcome variable is the full-time equivalent wage as observed at the individual level in the Statistics Sweden’s wage register. The register covers the whole of the public sector, and a subsample of the private sector. The private sector subsample is obtained via stratified random sampling, with substantially lower coverage for very small organizations. In for-profit preschools, wages are observed for around 30 percent of workers. In the non-profit sector, where organizations are on average smaller, the coverage is only around 5 percent. In the baseline analysis we do not account for the sampling probability, with the consequence that municipal workers are overweighted compared to their true representation in the preschool worker population. Results where we apply weights corresponding with the inverse of the sampling probability are provided as a robustness check.

From the individual level registers of Statistics Sweden we have access to the following worker characteristics that we include as covariates: worker age and gender, country of birth, upper secondary grades and highest educational degree. We use information about worker degrees to construct two indicator variables to capture worker qualifications: one for having a preschool teacher degree and one for having a childcare assistant or related degree. Remaining “unqualified” workers are in the reference category. From the publicly available database of Statistics Sweden we obtain information about municipal level characteristics: the number of residents of preschool age (1–5 years), taxable income, and the seat shares in the local government assembly for the Moderate Party, the Social Democrats and the Left Party, see Section 4.2 for how these variables are defined and included in the analyses.

¹⁵ The income base amount (*Inkomstbasbelopp*) reflects the aggregate income development and is set by the Swedish Pensions Agency to determine social insurance benefits. In 2024, the amount was 76 200 SEK (approx. 6900 €).

5.2 Descriptive comparison between groups

Table 2 presents summary statistics for the outcome variable and covariates in all municipalities, as well as for the treatment and control groups. The individual data is aggregated to municipality-by-year averages.

Table 2. Descriptive statistics 2001–2013

	(1)	(2)	(3)	(4)	(5)
		<u>Early-later treated</u>		<u>High-low growth</u>	
	All	Treated	Control	Treated	Control
Wage	23,367	23,485	23,148	23,560	23,465
Ln wage	10.05	10.06	10.04	10.06	10.05
<i>Municipality characteristics</i>					
Ages 1–5 population	1,750	2,072	1,077	3,292	3,498
Ln ages 1–5 pop	6.85	7.30	6.65	7.52	7.81
Taxable income	175,137	176,169	169,359	192,783	182,425
Ln taxable income	12.06	12.07	12.03	12.15	12.11
Seat share The Moderate Party	0.189	0.190	0.184	0.270	0.215
Seat share Soc. Dem. and Left Party	0.442	0.440	0.439	0.380	0.426
<i>Worker characteristics</i>					
Preschool teacher	0.446	0.454	0.452	0.419	0.444
Childcare assistant	0.264	0.263	0.256	0.260	0.261
Age	43.36	43.46	43.44	42.58	42.82
Female	0.9734	0.9734	0.9704	0.9702	0.9703
Swedish-born	0.94	0.93	0.95	0.90	0.92
Number of municipalities	288	51	28	49	48
Municipality-year observations	3,743	663	364	637	624

Note: The table shows the averages of municipality-year means. One municipality (Dorotea) is lacking in 2001, due to incomplete data. Two municipalities (Uppsala and Knivsta) are excluded as they were part of a secession during the period.

As seen in the table, there are a few notable differences between groups. First, treated municipalities are on average twice as large as control municipalities when using the “early-later treated” definition, while treatment and control municipalities are more equally sized when using the “high-low growth” definition. Treated municipalities have a somewhat higher taxable income

level using both classifications. On the political side, the seat shares of the Moderate Party and the sum of Social Democrats and the Left Party are balanced between treated and control using the “early-later treated” definition. The support for the Moderate Party is higher, especially in the treated group, using the “high-low growth” definition.

Furthermore, there are differences between the two sets of treatment and controls. Municipalities using the “high-low growth” definition in Columns 4 and 5 are substantially larger in terms of population and seat share for The Moderate Party, indicating that there are compositional differences between the two sets of groups. We view the differences between the sets as beneficial in terms of robustness to the extent that the two cases provide similar results.

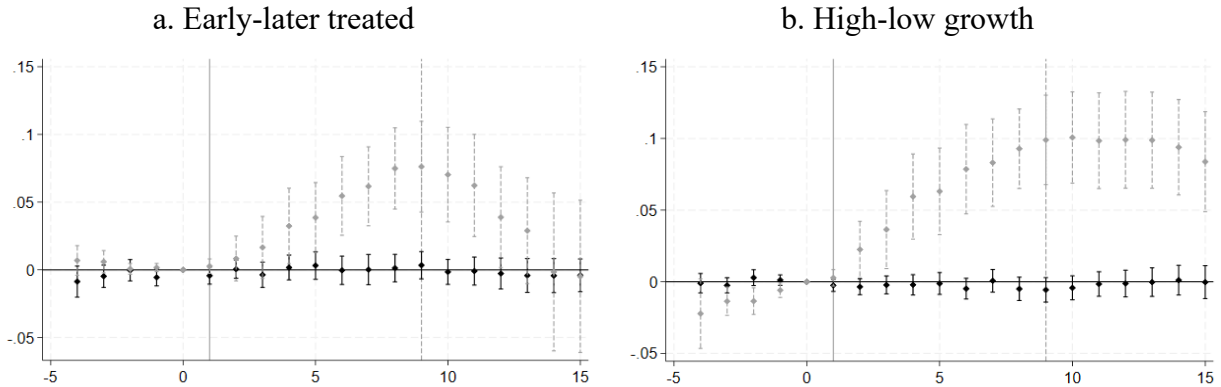
6. Results and discussion

6.1 Main results

In this section we present the results of the event study regression (Equation 1). We present two analyses side-by-side using the two alternative definitions of treatment and controls.

Our main results can be found in Figure 3, where the event study estimates using the log wage as outcome variable are shown in black solid lines. Estimates using the for-profit share as outcome variable are shown in grey dashed lines to facilitate comparison with the average treatment dose. The wage coefficients are very flat over time and precisely estimated around zero for both treatment definitions. The results hold for all studied periods; pre-reform as well as during the first and last seven-year-period after the reform. Taken together, we view this as a null result, suggesting that the for-profit worker share has no impact on the municipal average wage among preschool workers.

Figure 3. Event study estimates for ln wage (black) and for-profit worker share (grey).



Note: Observations are on worker level but are weighted with the inverse of the municipality-year number of workers, such that equal weight is given to all municipalities. Municipality- and year fixed effects, and the following worker covariates are included: age (level and squared); and dummies indicating female, Swedish born, preschool teacher degree, and child assistant degree. The following municipality covariates are included: ln age 1–5 population (level and squared); ln taxable income; Moderate Party seat share, Social Democrats and the Left Party seat share. Standard errors are clustered on municipality. 95% confidence intervals are shown.

What can explain the lack of a clear wage impact? A possible explanation lies in differences in hiring practices between for-profit- and other providers. Appendix Table A1 shows that for-profit workers in treated municipalities have shorter preschool-related work experience and lower grades than workers in municipal and non-profit preschools, and they are less likely to have a preschool teacher degree compared to municipal preschool workers. Appendix Table A2 furthermore shows that for-profits in treatment municipalities often hired workers from outside of the preschool sector or that were previously non-employed; 49 percent in early-treated and 55 percent in high-growth.¹⁶ These hiring practices can be expected to lower the local average wage level unconditional on worker characteristics as less qualified workers are paid lower wages, i.e. negative selection. Furthermore, if for-profits do not prioritize recruiting workers with experience, the competitive pressure for incumbent workers is attenuated. In Appendix Figure A7, we obtain a null effect also when restricting the analysis to workers employed by a municipal preschool the previous year, implying that the main null result is not solely a consequence of negative selection via recruitment of low-wage workers, but also by weak competitive pressure for incumbent workers.

¹⁶ Workers with missing information on prior sectors (6–11 percent) are included in these numbers.

The recruitment of workers with lower experience and qualifications by for-profits could be the result of cost-cutting behaviour induced by the profit-motive. However, the explanation can also lie on the supply side, i.e. incumbent and more experienced workers could be averse to supplying their labour to the entering for-profit preschools. In general, the predominantly female worker population might be less inclined to switch jobs that increase commuting time, or to switch jobs altogether, resulting in lower labour supply elasticity. Additionally, if incumbent preschool workers have distaste for for-profit provision, specifically, for-profits will have to pay a wage premium to attract incumbent workers, or recruit from elsewhere.¹⁷

6.2 Sub-group analysis

Irrespective of the relative importance of the supply and demand channels, the seemingly different hiring patterns in for-profits relative to incumbent preschools might bring about differential wage effects among sub-groups of workers. If for-profits were eager to hiring experienced and highly qualified workers, they would be willing to bid up their wages. On the other hand, if for-profits are relatively less inclined to hire qualified workers, as our data seem to suggest, the wages of qualified workers might instead decrease as demand shifts towards less qualified workers.

We therefore re-estimate the event study regression Equation (1) on split samples according to educational background, worker experience, upper secondary grades, and combinations of the three. None of the results we obtain from the sub-group analysis, shown in Appendix Section A5, are robust to alternative definitions of treatment and control. There are indications of a negative wage effect for workers with high grades (Figure A10b), and a negative effect for workers with a combination of having a preschool teacher degree, long preschool work experience and high grades (Figure A11b), but these indications are only present in the high-low growth specification. When looking at changes in the composition of preschool worker population in high-growth municipalities, we find, correspondingly, indications of a decreased share of workers with high grades, and an increased share of workers with low grades (Figure A14b). And finally, there are

¹⁷ It can be noted that the increase in the preschool age cohorts that characterized our studied period should furthermore make it harder to recruit experienced staff (and should in addition contribute to an upward wage pressure), but this holds for all providers and is not specific to the for-profits.

indications of a lower share of workers with a preschool teacher degree, and a higher share with no childcare-related degree in early-treated municipalities (Figure A12a).

The results from our sub-group analyses are far too weak and non-robust to be viewed as conclusive evidence. However, to the extent that our results point in any direction, they are consistent with the descriptive evidence on hiring patterns, i.e. that for-profits on average tend to hire workers with lower formal qualifications, shorter work experience and lower grades. Given that many subsamples are used, we should also not pay too much attention to isolated significant results – they may well occur by chance. Appendix Figure A15 furthermore shows no clear impact on the total number of preschool workers (per enrolled child) in the treated municipalities. There are some signs of a positive effect in early-treated municipalities, but no such impact is found in high-growth municipalities. Concludingly, there is no robust evidence that the for-profit expansion significantly altered the aggregate preschool worker composition or magnitude.

6.2 Robustness analysis

To increase the treatment dose, we re-estimate Figure 3 and restrict the analysis to the top half of the treated municipalities. The for-profit worker share difference-in-difference at the end of the first treatment period increases from 8 to 13 percentage points using the early-later definition, and from 10 to 16 percentage points using the high-low definition. The results are shown in Appendix Figure A16; they mainly reproduce the same zero-result. If anything, there is a tendency to a negative – albeit not statistically significant at the five-percent-level – effect at the end of the treatment window using the high-low growth definition.

Further robustness checks include: using weights corresponding to the inverse of the sampling probability in the wage data, i.e. correcting for the overrepresentation of workers in municipal and larger private preschool units (Figure A17); removing municipality- and worker level covariates (Figure A18), adding municipalities to the control group that were either never-treated during the studied period (early-later treated), or they were never-treated with any for-profits at all in the treatment period (high-low growth) (Figure A19), and dropping two municipalities from the analysis altogether that experienced a substantial public to private transformation after the reform (Figure A20). The results from these robustness checks are similar to the main results.

7. Conclusions

Our analysis yields no evidence that reduced employer concentration, in the form of an increased presence of for-profit preschool providers in Swedish municipalities, altered the average wage level among preschool workers. The null result is stable across alternative treatment and control group definitions, and for a set of robustness tests. The results are a contrast to theoretical models on employer monopsony and workers' outside options, which predict a positive wage effect from decreased employer concentration.

A potential explanation behind the lack of positive wage effects might stem from the specific configuration of increased for-profit provision in a setting where a public provider is the dominant provider. The wage-boosting effect of lower employer concentration may in such a setting have been counteracted by stronger cost-minimizing incentives in for-profit providers compared to the incumbent public provider. Cost-minimizing can take the form of offering lower wages conditional on worker characteristics, or selecting workers with characteristics that require lower wages: i.e. workers who are younger, have lower formal qualifications, shorter work experience and/or lower abilities. Our data shows that the entering for-profit preschools hired substantial shares of their workers from other sectors than the preschool sector, and the recruited workers to for-profit preschools are on average less qualified and less experienced. Such hiring practices are likely to decrease the average wage level via selection but also via attenuation of the competitive pressure for incumbent and experienced workers. Lower labour supply elasticities among the female worker population could furthermore imply that it takes a large wage premium to attract highly qualified workers from the incumbent preschools, a premium that cost-minimizing for-profits might prefer to avoid. In a wider sense, our results support the notion that the specifics of the market structure, its institutions, regulations and employer types are relevant to take into account as they may attenuate or strengthen the consequences of monopsony market power.

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Appendix: *Does competition from for-profit firms raise wages for preschool workers?*

A1. Background to the 2006 free entry reform¹⁸

The reform had its roots in a long-standing debate regarding the role of private providers of preschool, and whether they were to be allowed to operate alongside the municipal preschools. During the 1980s, the center-right parties had started to propagate for allowing private actors, including for-profit firms, to operate and receive public funding. The debate intensified in 1983 after a newspaper revealed that the Swedish manufacturing company Electrolux planned to start a commercial preschool, with the support of the Swedish Employers' Association. The Social Democratic party, which governed during most of the 1980s with the support of the Left Party, countered by passing a law in 1984 that prohibited municipalities from using central government preschool grants to fund preschools operated by private organizations in the form of for-profit companies or employee cooperatives. The Social Democratic government and the Left Party were strongly opposed to the idea of for-profit preschools, and in 1984 passed a law that was given the nickname "Lex Pysslingen", since the preschool company it was aimed at stopping was named "Pysslingen Preschools Inc." (*Pysslingen förskolor AB*) (see e.g. Linnarsson, 2017) and Larsson and Westberg (2020), that restricted municipalities from channelling public funds to private providers other than the following list of exceptions: private actors organized as parental cooperatives and organizations with a non-profit focus, or offering alternative pedagogical orientations (e.g. Waldorff). In 1991, employee cooperatives were added to the list of approved actors. For-profit companies in the form of limited companies (*aktiebolag*) were however explicitly prohibited (1990/91:38). Accordingly, municipalities decided whether to approve and fund private preschools, as long as they were on the list of approved organizations.

The ban on for-profits was strongly contested by the four centre-right parties. When they formed a coalition government after the 1991 fall election, they promptly lifted the ban – meaning that any private preschool provider, including for-profits, could be allowed to operate and receive public funding from the municipalities. The law specified that the private preschools should maintain sufficient quality and that the parental fee should not unreasonably exceed the municipal preschool fee. In effect, this meant that also for-profit companies could enter the preschool market with public funding, but only subject to the municipality's approval. The center-right government coalition had also prepared to implement a free-entry law that would abolish the municipal veto,

¹⁸ Large parts of this section draw heavily from Martin-Korpi, B (2015). *Förskolan och politiken – om intentioner och beslut bakom den svenska förskolans framväxt*, Publication from the Department of Education; Government bills (*propositioner*): 1983/84:177; 1990/91:38; 1990/91:39; 91/92:64, and *Utbildningsutskottets betänkande* 2005/06:UbU13.

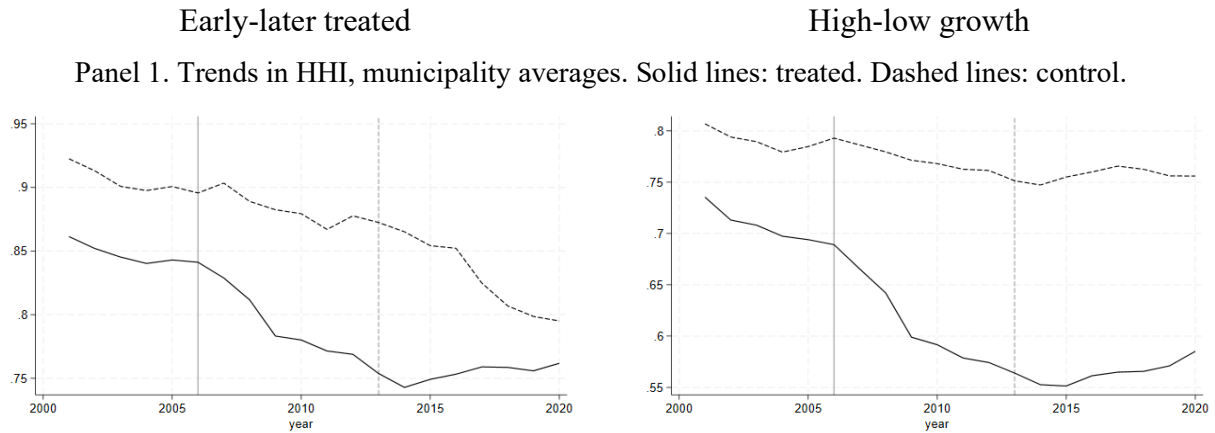
but this law was reversed before coming into effect when a Social Democratic minority government regained power following the 1994 election.

Municipal discretion over the approval and funding of private preschool providers thus prevailed. In 2006 however, the parliamentary situation was such that the center-right parties could, together with the support of the Green party, implement the free entry law against the will of the Social Democratic minority government. Starting from 1 July 2006, municipalities could no longer veto a private actor that wanted to set up a preschool in their municipality, but were instead obliged to provide funding to any private provider that fulfilled the quality criteria (Skollag 1985:100, Chap §13 and §17).

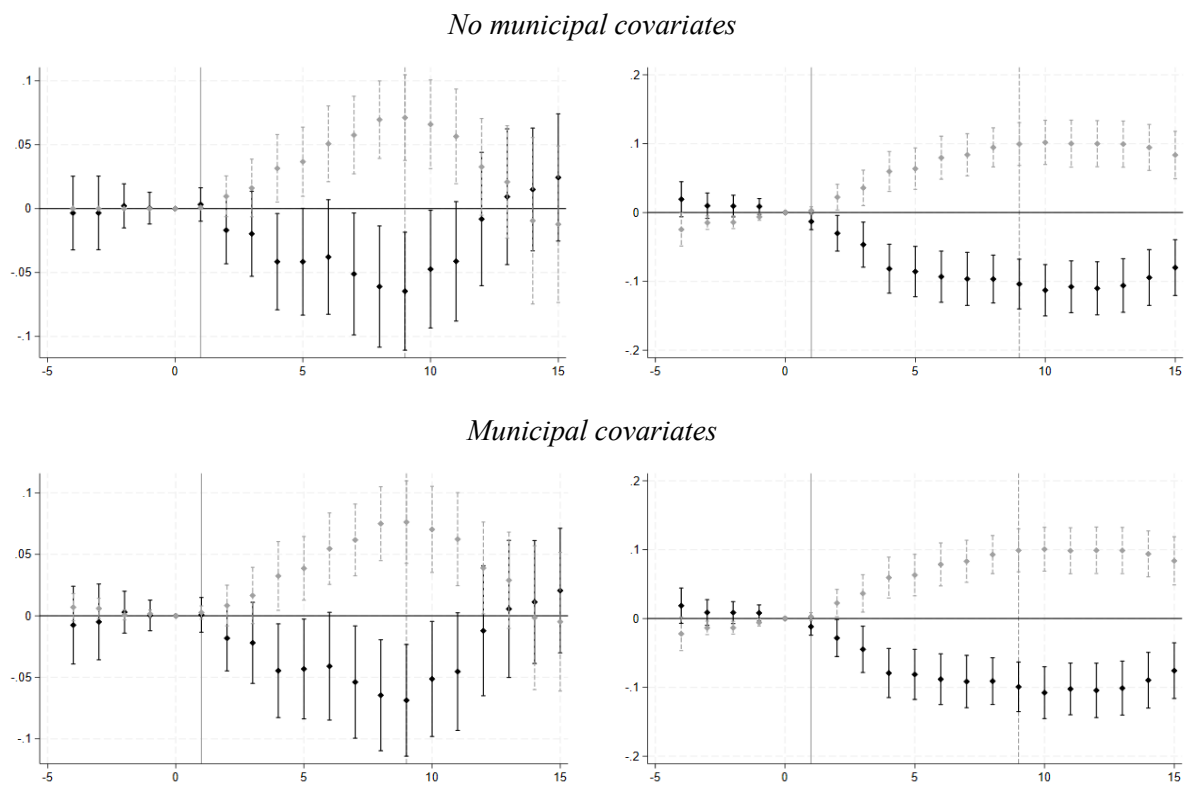
Funding could be denied only if the municipality could show that a new preschool would have significant negative consequences for the municipality's economic possibilities to provide preschool, or if the parental fee was unreasonably high compared to that levied by the municipal preschools (see e.g. p.4 *Finansutskottets yttrande 2005/06:FiU1y Etableringsfrihet för förskolor*). We have no information on how often municipalities reject entry of new preschools on the basis of these arguments. However, Angelov and Edmark (2016) report that rejections are very rare at the primary school level for years 1997–2000, which is governed by a similar rule. In addition, the parliamentary Education Committee report preceding the 2006 decision stated that rejection based on 'significant negative consequences' should be used restrictively (p.48 bet. 2005/06:UbU13). The free-entry regulation was consolidated in 2009, when the right to municipal funding was clarified through the introduction of a voucher system (Prop. 2008/09:115).

A2. Herfindahl-Hirschman index

Figure A1. The Herfindahl-Hirschman index.

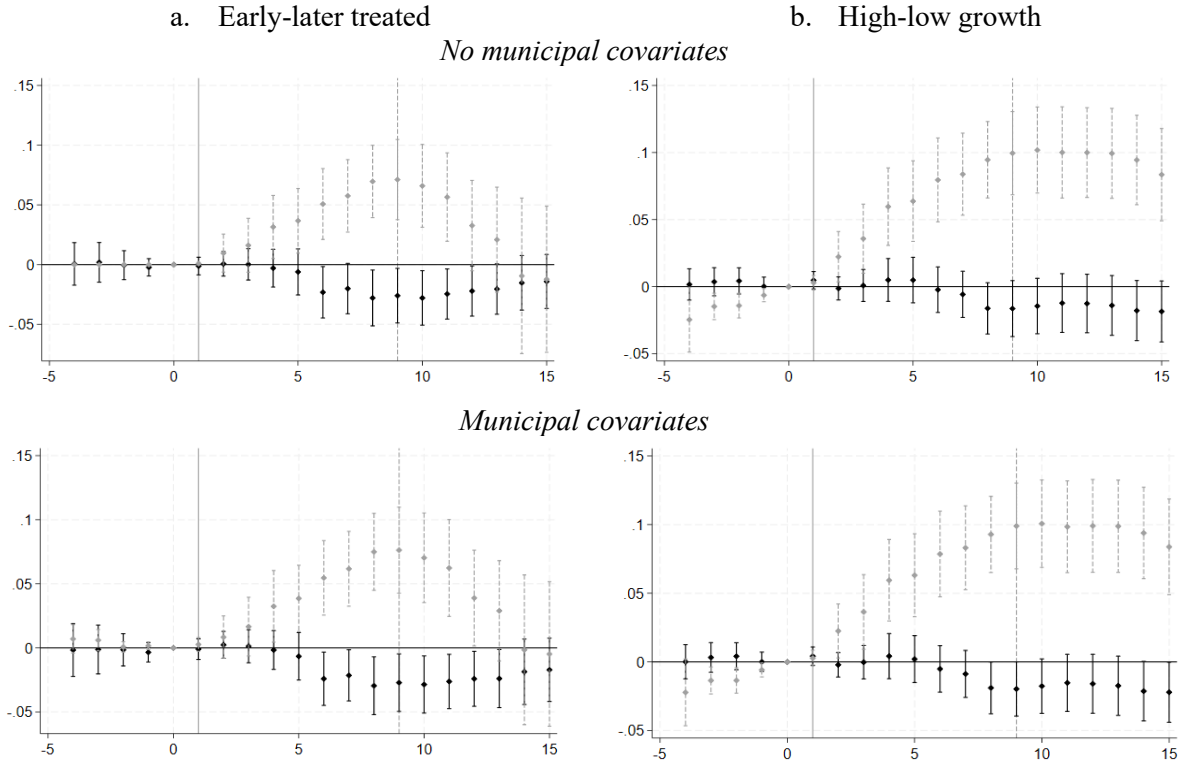


Panel 2. Event study estimates for the HHI (black) and for-profit worker share (grey).



Note: Note that the scale of the y-axis differs between the figures. The HHI is calculated such that each firm or municipality is viewed as a separate employer, and firms who belong to the same corporate group are treated as the same employer. The regression equation includes municipality and year fixed effects. The lower panel also include municipality level covariates: $\ln$ age 1–5 population (in level and squared); $\ln$ taxable income; seat share Moderate Party, and seat share Social Democrats and the Left Party. 95% confidence intervals, based on standard errors are clustered on municipality, are shown.

Figure A2. Event study estimates for the share of non-profit preschool workers.



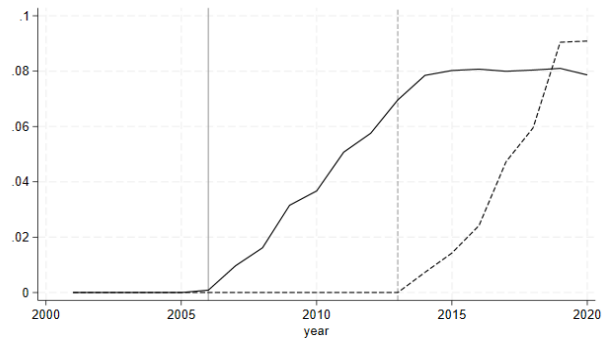
Note: The event study regressions are weighted with the inverse of the municipality-year number of workers. The regression equation includes municipality and year fixed effects. The lower panel also include municipality level covariates: $\ln$ age 1–5 population (in level and squared); $\ln$ taxable income; seat share Moderate Party, and seat share Social Democrats and the Left Party. 95% confidence intervals, based on standard errors are clustered on municipality, are shown.

A3. Trends in municipal characteristics

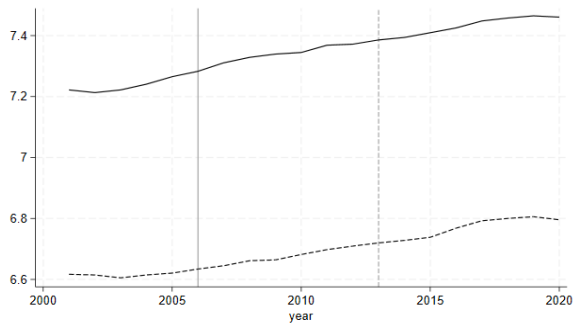
Figure A3. Trends in municipal characteristics – Early-later treated.

Solid lines: treated municipalities, dashed lines: control municipalities. Note that the scale of the y-axis differs between the figures.

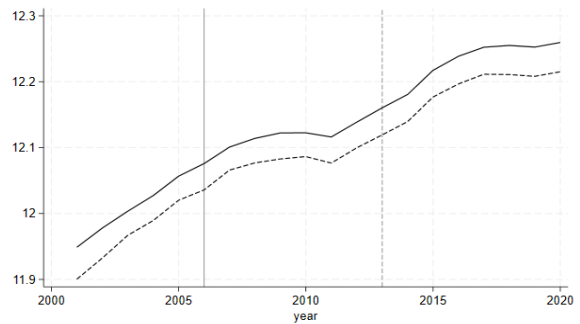
Preschool worker share for-profits



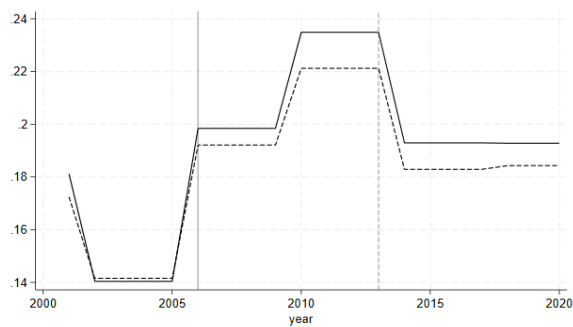
Ln(age 1-5 population)



Ln(taxable income)



Seat share Moderaterna



Seat share Left

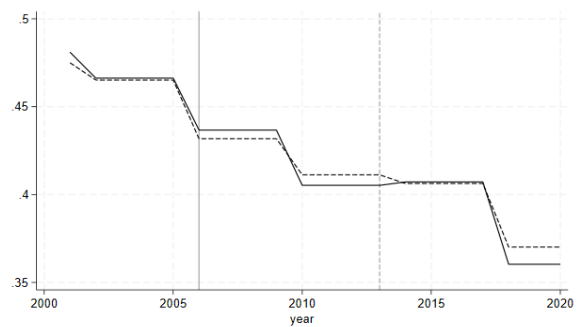


Figure A4. Trends in municipal characteristics – High-low growth
 Solid lines: treated municipalities, dashed lines: control municipalities. Note that the scale of the y-axis differs between the figures.

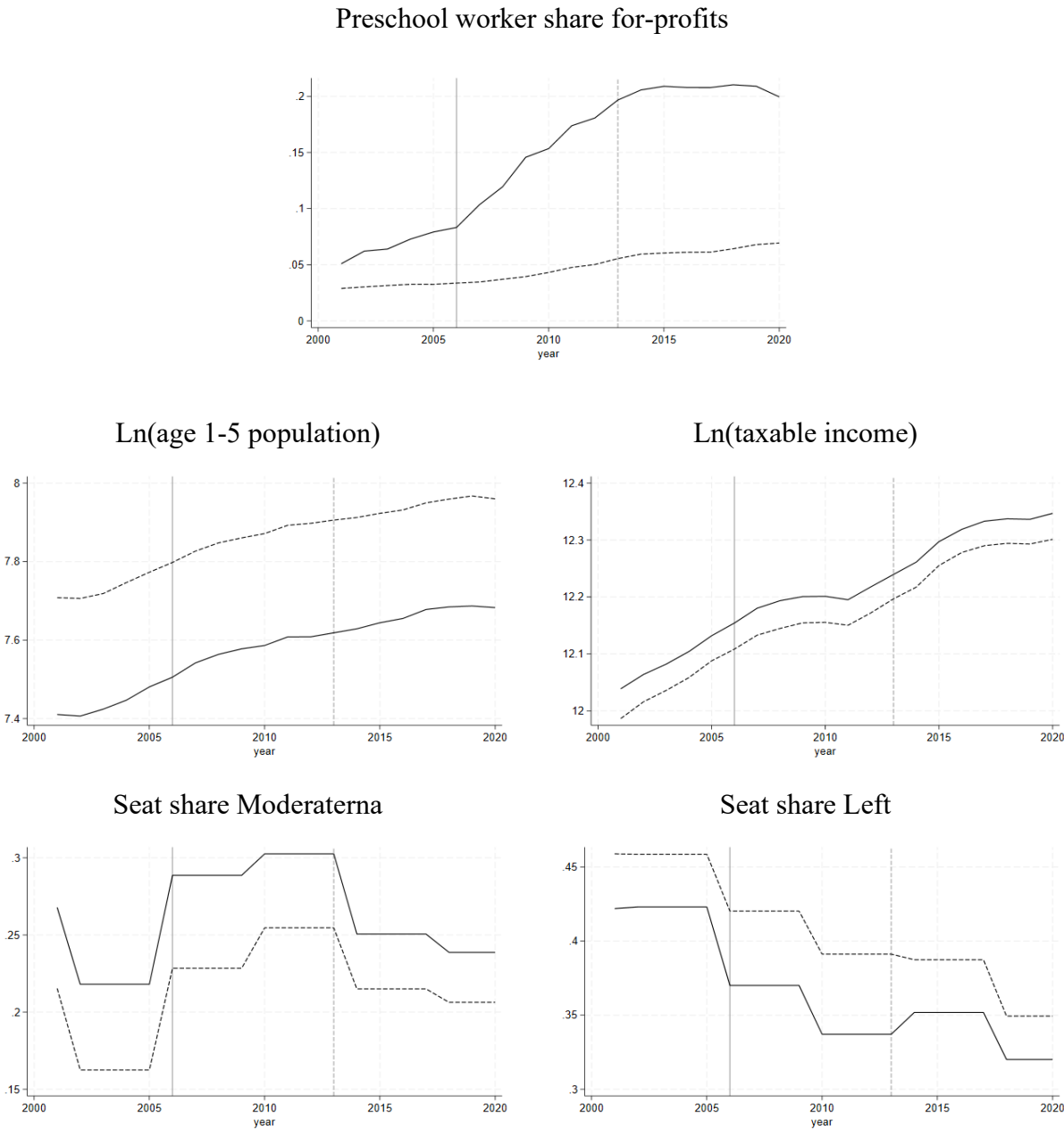
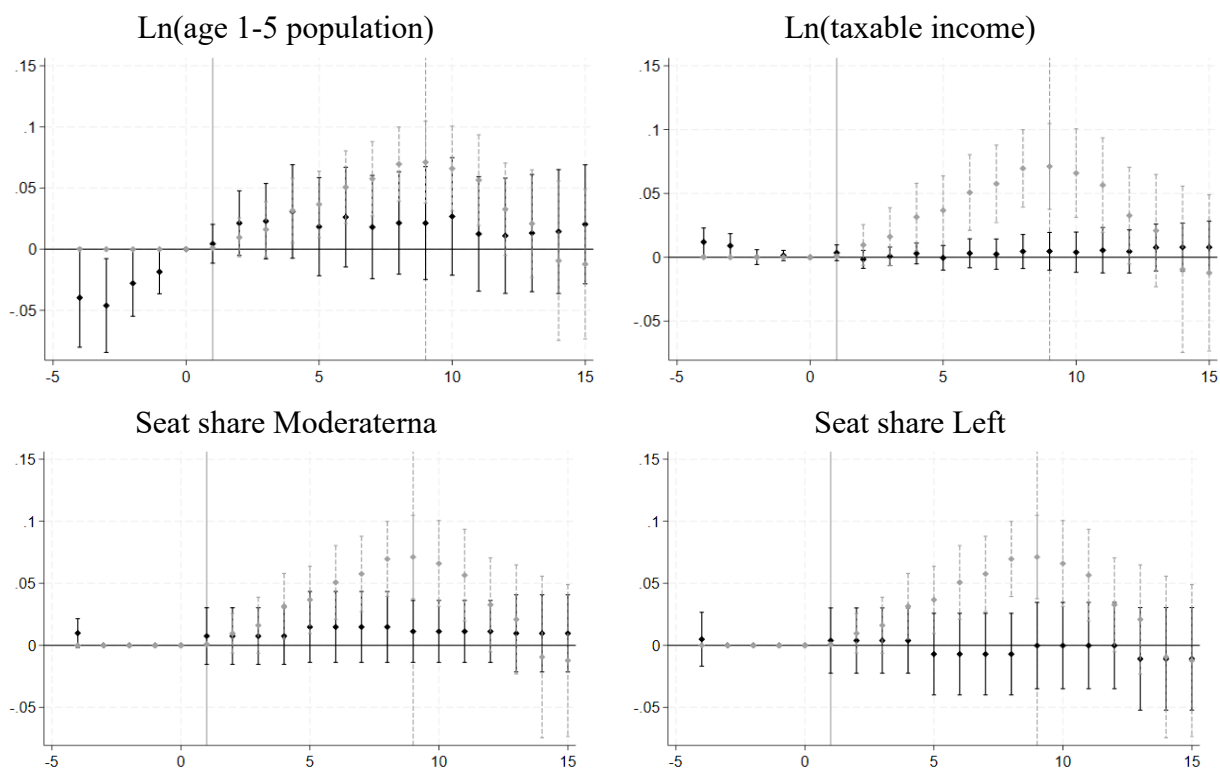
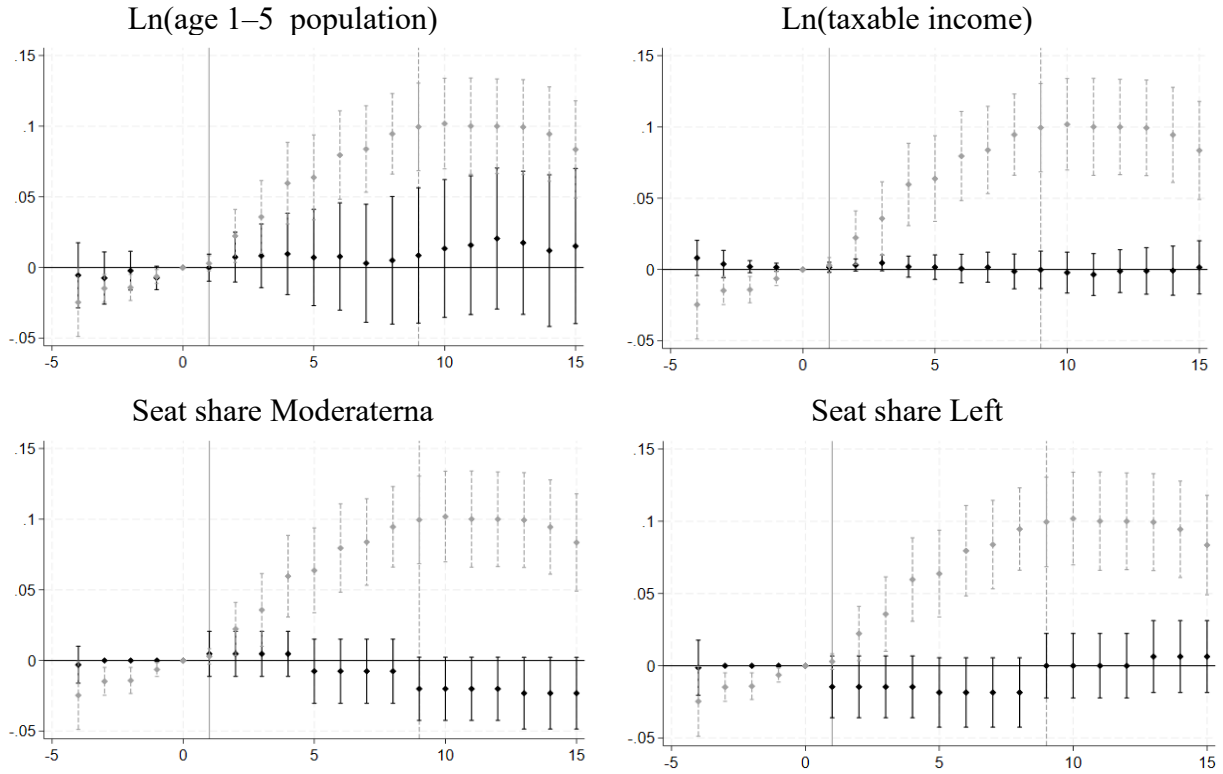


Figure A5. Event study estimates for municipal characteristics – Early-later treated.
The grey markers show the event study estimates for the for-profit share of preschool workers.



Note: The estimations include municipality and year and municipality fixed effects, but no covariates. The figures show 95% CI, based on standard errors clustered on municipality.

Figure A6. Event study estimates for municipal characteristics – High-low growth
The grey markers show the event study estimates for the for-profit share of preschool workers.



Note: The estimations include municipality and year and municipality fixed effects, but no covariates. The figures show 95% CI, based on standard errors clustered on municipality.

A4. Differences in hiring practices

Table A1 illuminates differences in hiring practices between different types of preschools in the treated municipalities. The comparison is restricted to the time-period directly after treatment. The comparison shows that the share preschool teachers is higher in the municipal preschools than in the non-profit and for-profit preschools. The share of workers who lack both preschool teacher degree and childcare assistant education is higher in both non-profits and for-profits. For-profits stand out by their hiring of workers with shorter work experience in preschool (PS), and by having somewhat lower grades.

Table A1. Background characteristics of preschool workers in treated municipalities 2007–13.

	Early treated municipalities			High growth municipalities		
	Municipal preschool workers	Non-profit preschool workers	For-profit preschool workers	Municipal preschool workers	Non-profit preschool workers	For-profit preschool workers
Preschool teacher	0.48	0.34	0.35	0.40	0.29	0.28
Childcare assistant	0.24	0.26	0.26	0.23	0.25	0.23
None of the above	0.28	0.39	0.39	0.37	0.46	0.49
PS work experience	5.83	5.65	4.64	5.48	5.63	5.02
Age	44.07	39.63	39.48	43.04	40.78	38.72
High grade	0.14	0.15	0.11	0.12	0.13	0.11
Mid grade	0.40	0.45	0.41	0.34	0.36	0.36
Low grade	0.12	0.15	0.15	0.13	0.14	0.16
Missing grade	0.34	0.25	0.34	0.41	0.37	0.37

Table A2 shows the employment status in year $t-1$ among workers that were hired by for-profit preschools in the treated municipalities in year t , where t goes from 2007 to 2013. As shown in Panel A, around 40 percent of the new hires in the early-treated municipalities, and almost 30 percent in the high-growth municipalities, came from preschools in the same municipality. Among them, 23 (early-treated) or 13 (high-growth), percent were employed in a municipal preschool. Panel B shows that a smaller share of new hires were recruited from preschools in other municipalities. Note that our preschool worker sample is restricted to workers in occupations relating to working with children; e.g. administrative, kitchen or cleaning staff are excluded.

Panel C shows that 41/44 percent of new hires were employed in another sector of the economy (including missing sector information), mainly in health, welfare or non-preschool education or in other service sectors including e.g. trade, restaurant, hotel and other services. Around 11/6.3

percent were employed in workplaces with missing sector information; these could include some preschools. Panel D shows that around 7.5/11 percent had no employment in $t-1$, and for very small shares we lack sufficient information to categorize the $t-1$ activity.

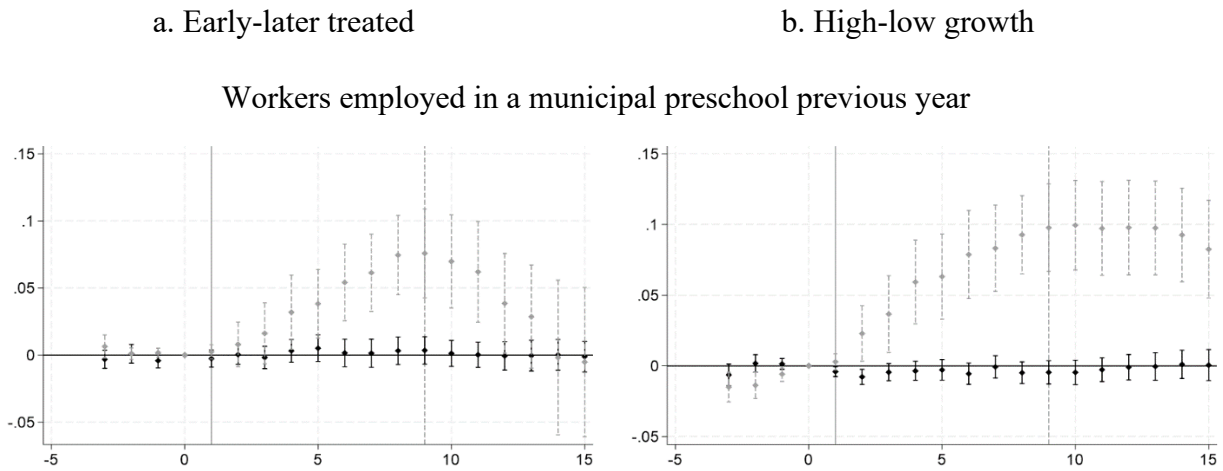
Table A2. Previous employment for new hires in for-profits in treated municipalities, 2007–13.

	Percent (%)	
	Early treated	High growth
Panel A: Employed in a preschool in the municipality in $t-1$		
Municipal preschool	23.03	13.02
Other for-profit preschool	3.77	8.97
Non-profit preschool	12.33	5.65
Σ	39.13	27.64
Panel B: Employed in a preschool in another municipality in $t-1$		
Municipal preschool	7.07	9.06
Other for-profit preschool	2.96	5.43
Non-profit preschool	1.75	2.5
Σ	11.78	16.99
Panel C: Employed in other sectors in $t-1$		
Health, welfare, other education	13.13	15.7
Trade, hotel, restaurant, transport, postal- and telecom. services	7.41	10.5
Finance, insurance, real estate, data processing, R&D, public sector	4.71	6.88
Lobbying/advocacy, religious activity, culture, sports, other services	2.83	2.95
Agriculture, forestry, manufacturing, construction, sewage, sanitation	2.09	1.89
Missing sector information	10.98	6.32
Σ	41.15	44.24
Panel D: Other categories		
No employment in $t-1$	7.54	10.82
Missing info $t-1$	0.4	0.32

Note: Analysis of the 1,485 new hires to for-profit preschools during 2007–13 in treated municipalities.

A5. Sub-group analysis – $\ln(\text{wage})$ as outcome

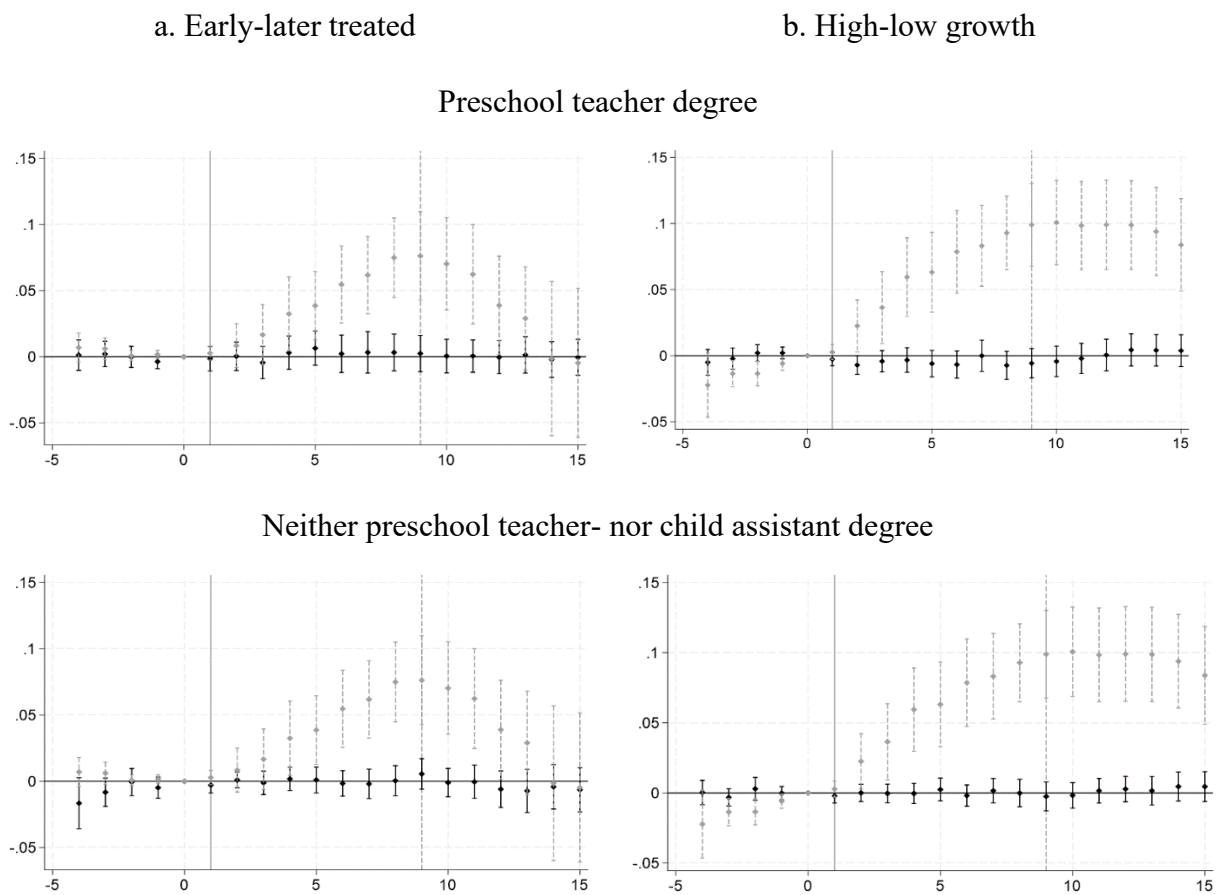
Figure A7. Workers employed in a municipal preschool previous year
The grey dashed lines show estimates for the for-profit worker share.



Note: See note to Figure 3 in the main text.

Figure A8. Educational background

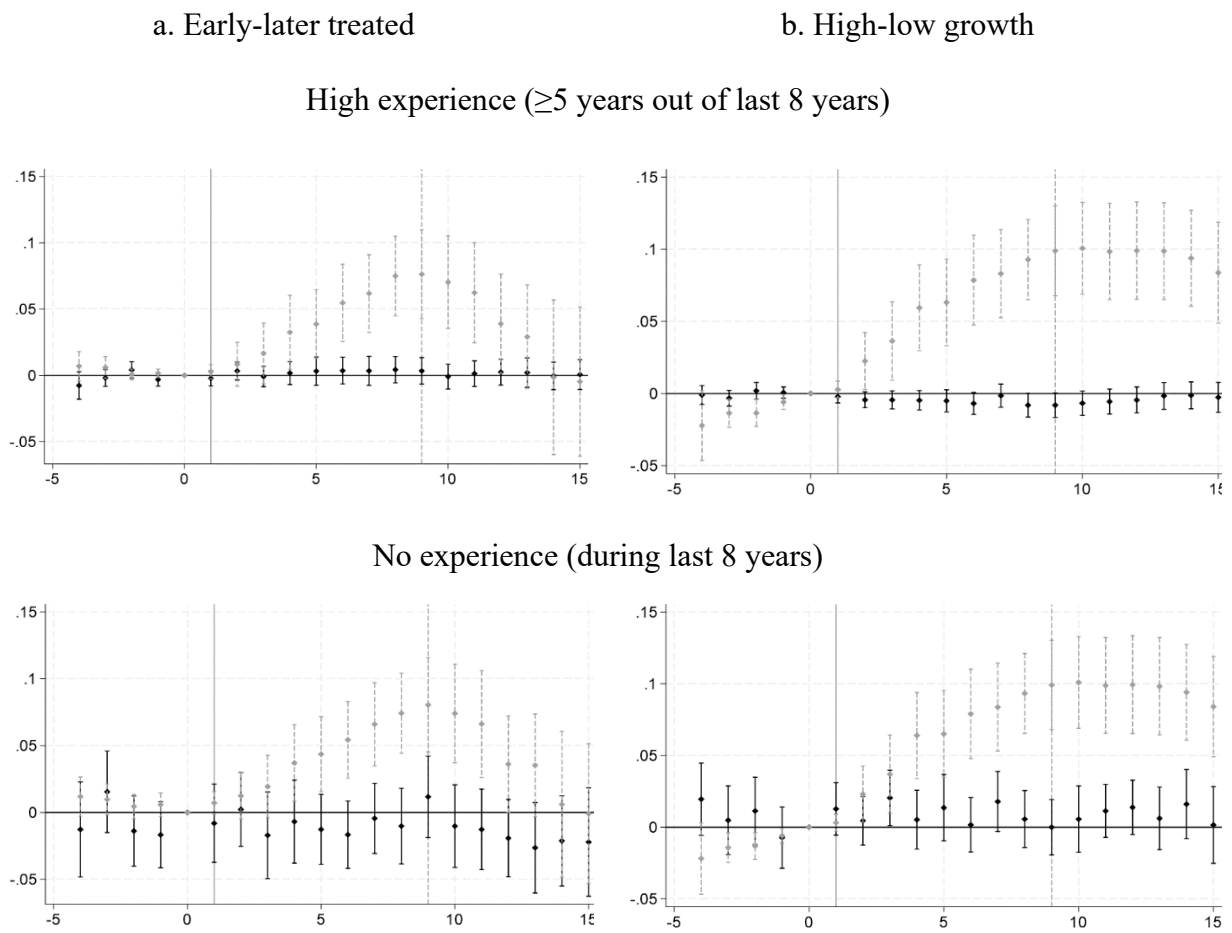
The grey dashed lines show estimates for the for-profit worker share.



Note: See note to Figure 3 in the main text.

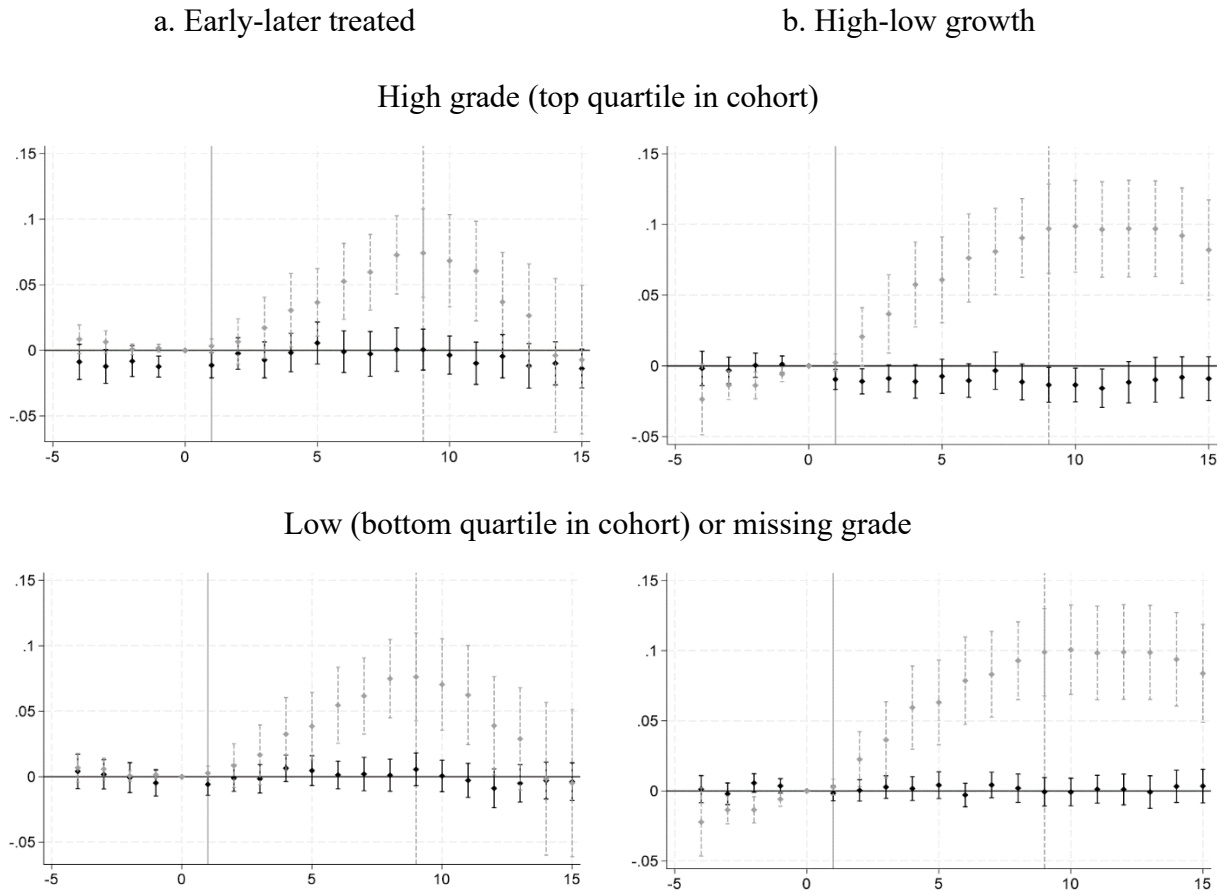
Figure A9. Work life experience

The grey dashed lines show estimates for the for-profit worker share.



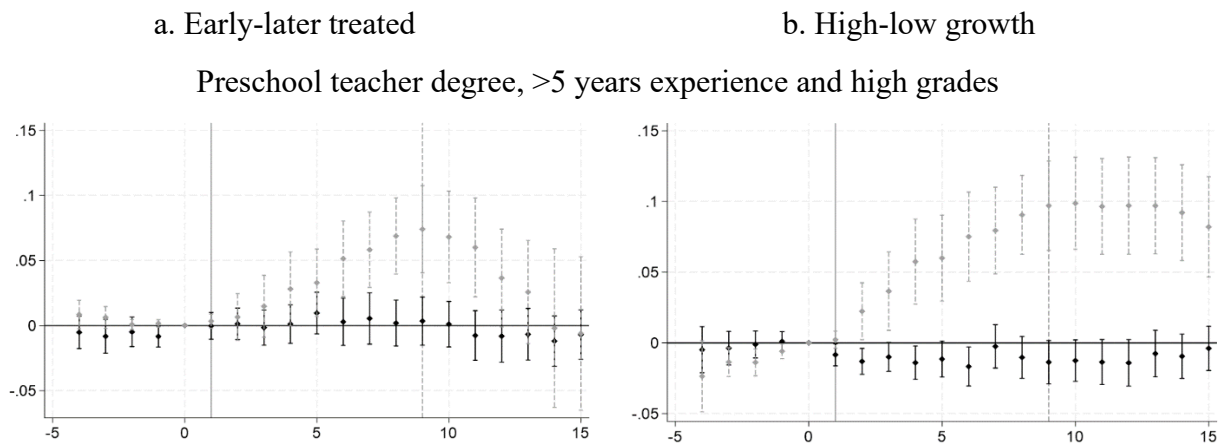
Note: See note to Figure 3 in the main text.

Figure A10. Upper secondary grades
The grey dashed lines show estimates for the for-profit worker share.



Note: See note to Figure 3 in the main text. Top and low grades are defined as the top and bottom quartiles of the cohort wide grade distribution. The missing grade category includes students who lack final grade due to dropping out of upper secondary school or due to having immigrated to Sweden without upper secondary school grades.

Figure A11. Combination of worker characteristics
The grey dashed lines show estimates for the for-profit worker share.

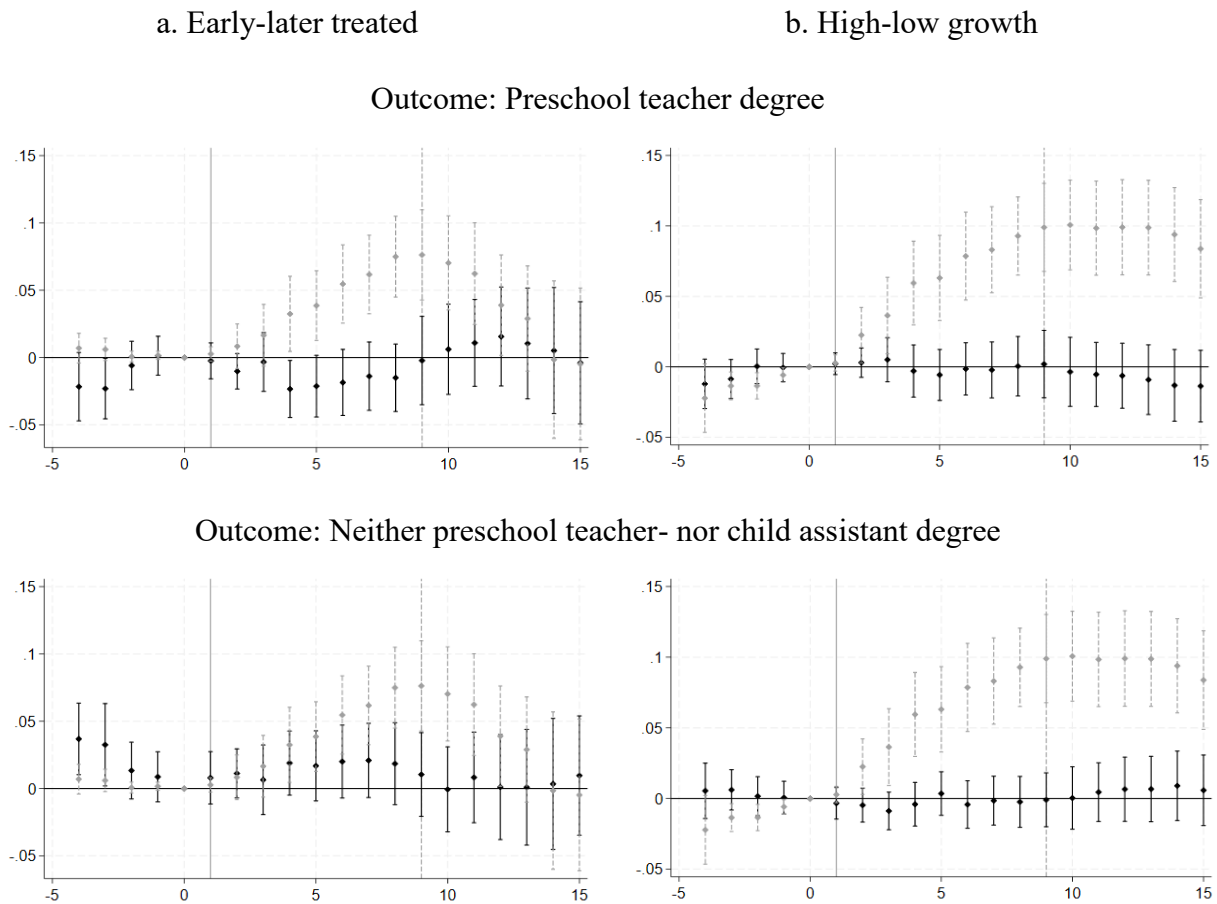


Note: See note to Figure 3 in the main text.

A6. Subgroup analysis – worker characteristics as outcomes

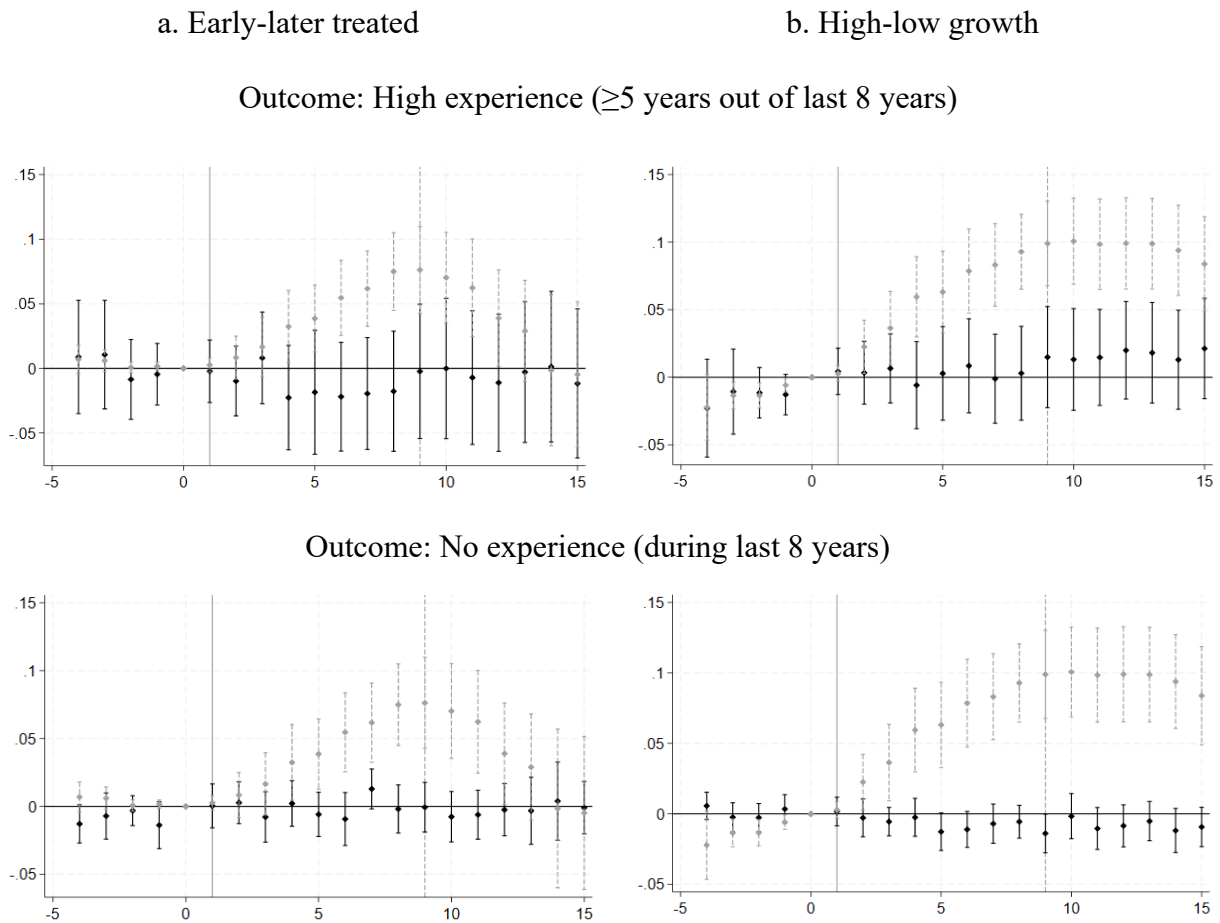
Worker covariates are not included in the event study regressions in this section.

Figure A12. Outcomes based on educational background
The grey dashed lines show estimates for the for-profit worker share.



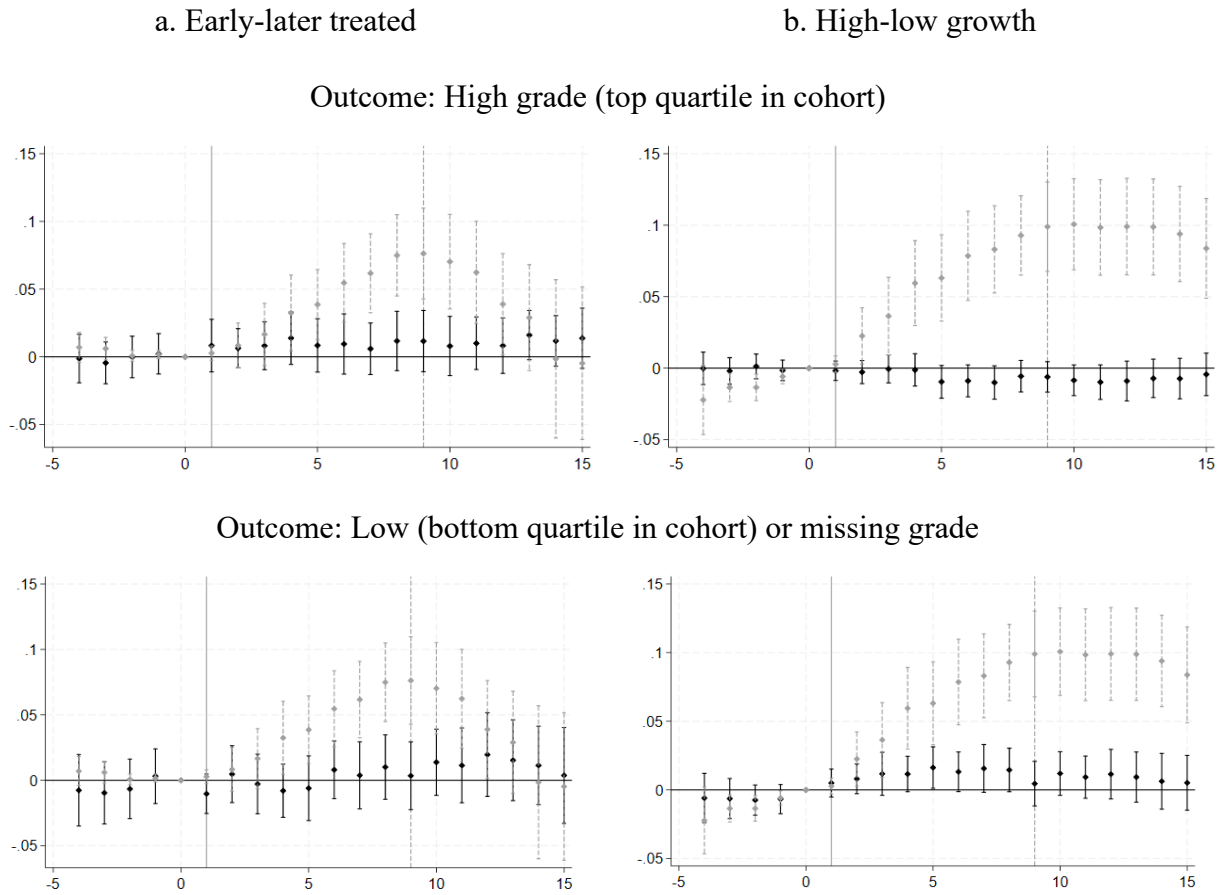
Note: Observations are on worker level but are weighted with the inverse of the municipality-year number of workers. Municipality- and year fixed effects are included. Municipality covariates: $\ln$ age 1–5 population in level and squared; $\ln$ taxable income; local council seat share for the Moderate Party, and the total seat share of the Social Democrats and the Left Party. Standard errors are clustered on municipality. 95% confidence intervals are shown.

Figure A13. Outcomes based on work life experience
The grey dashed lines show estimates for the for-profit worker share.



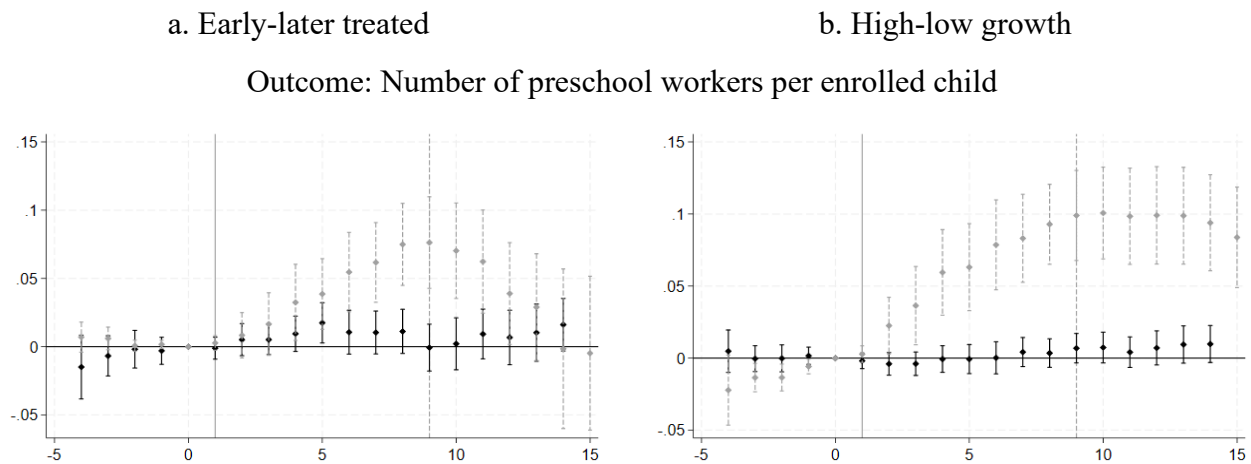
Note: Observations are on worker level but are weighted with the inverse of the municipality-year number of workers. Municipality- and year fixed effects are included. Municipality covariates: $\ln$ age 1–5 population in level and squared; $\ln$ taxable income; local council seat share for the Moderate Party, and the total seat share of the Social Democrats and the Left Party. Standard errors are clustered on municipality. 95% confidence intervals are shown.

Figure A14. Outcomes based on upper secondary grades
The grey dashed lines show estimates for the for-profit worker share.



Note: Observations are on worker level but are weighted with the inverse of the municipality-year number of workers. Municipality- and year fixed effects are included. Municipality covariates: $\ln$ age 1–5 population in level and squared; $\ln$ taxable income; local council seat share for the Moderate Party, and the total seat share of the Social Democrats and the Left Party. Standard errors are clustered on municipality. 95% confidence intervals are shown. Top and low grades are defined as the top and bottom quartiles of the cohort wide grade distribution. The missing grade category includes students who lack final grade due to dropping out of upper secondary school or due to having immigrated to Sweden without upper secondary school grades.

Figure A15. Number of preschool workers per enrolled child
The grey dashed lines show estimates for the for-profit worker share.

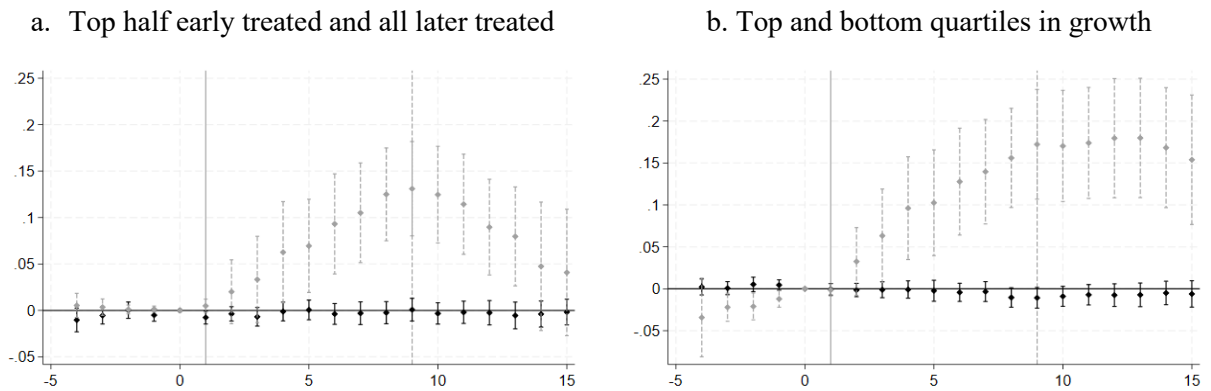


Note: Observations are on worker level but are weighted with the inverse of the municipality-year number of workers. Municipality- and year fixed effects are included. Municipality covariates: $\ln$ age 1–5 population in level and squared; $\ln$ taxable income; local council seat share for the Moderate Party, and the total seat share of the Social Democrats and the Left Party. Standard errors are clustered on municipality. 95% confidence intervals are shown.

A7. Robustness checks – event study estimates for $\ln(\text{wage})$

Figure A16. Increasing treatment dose

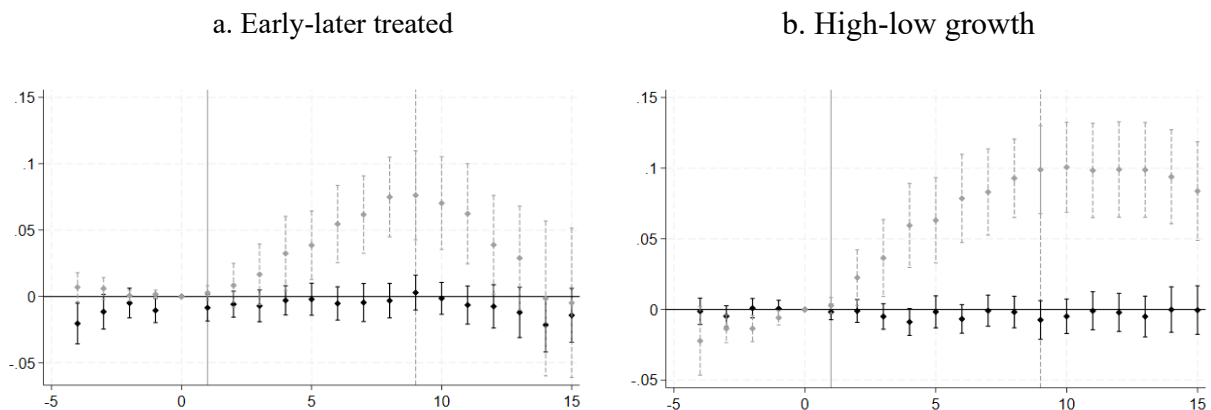
The grey dashed lines show estimates for the for-profit worker share. Note that the y-axis differs from that in Figure 3.



Note: See note under Figure 3 in the main text. The treatment group in Figure a. includes only municipalities with the largest increase in the for-profit share between 2007 and 2013; the control group is identical to the control group in the main estimations. The treatment group in Figure b. includes only the municipalities in the highest quartile in terms of for-profit growth between 2007–2013, and the control group includes municipalities in the bottom quartile.

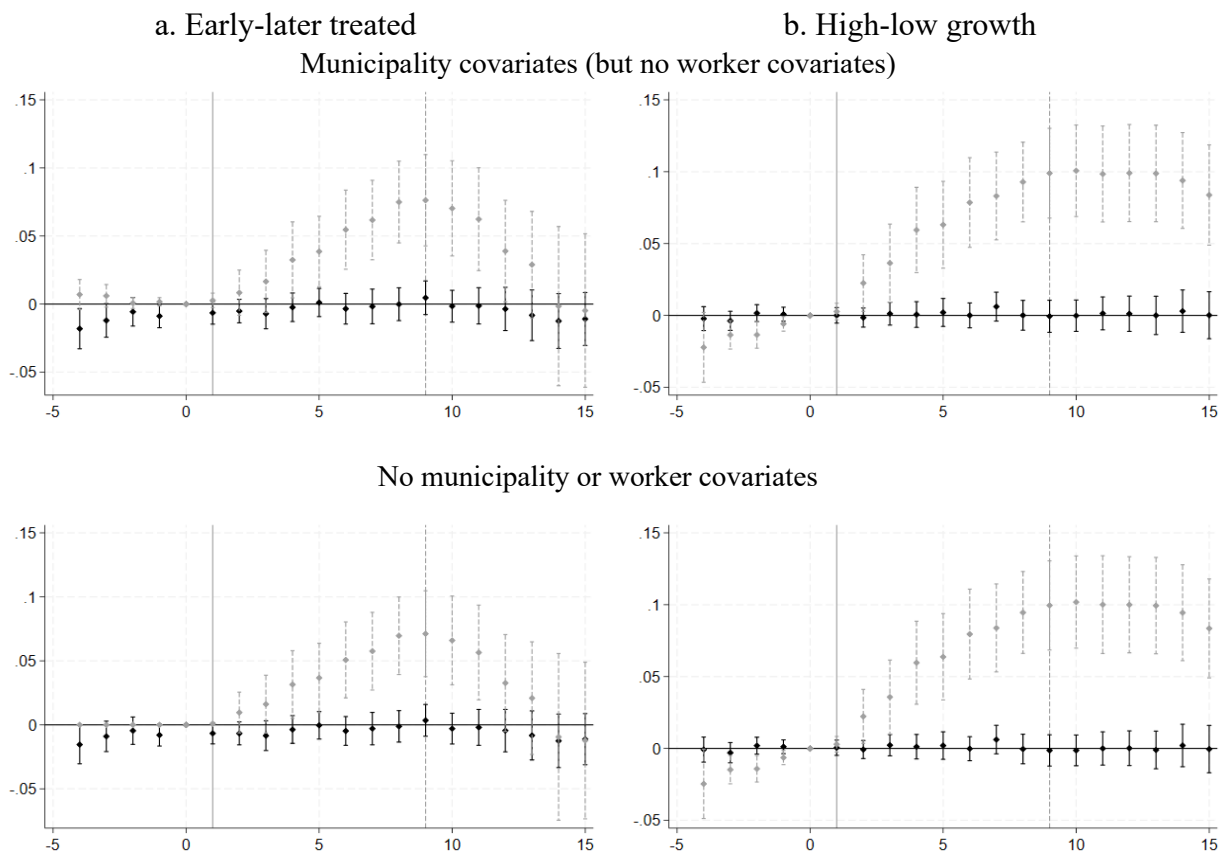
Figure A17. Using weights corresponding to the inverse of the wage sampling probability.

The grey dashed lines show estimates for the for-profit worker share.



Note: See note under Figure 3 in the main text. Observations are weighted with the inverse of the wage sampling probability to correct for the oversampling of municipal and larger preschool organizations in the wage sample.

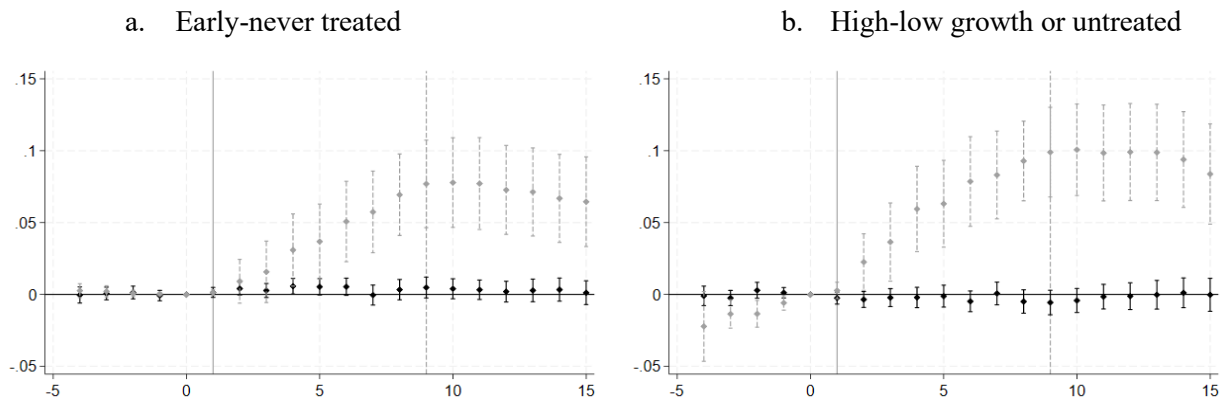
Figure A18. Varying municipal and worker covariates.
The grey dashed lines show estimates for the for-profit worker share.



Note: Observations are on worker level but are weighted with the inverse of the municipality-year number of workers. Municipality- and year fixed effects are included. Municipality covariates: $\ln$ age 1–5 population in level and squared; $\ln$ taxable income; local council seat share for the Moderate Party, and the total seat share of the Social Democrats and the Left Party. Standard errors are clustered on municipality. 95% confidence intervals are shown.

Figure A19. Alternative control group definitions

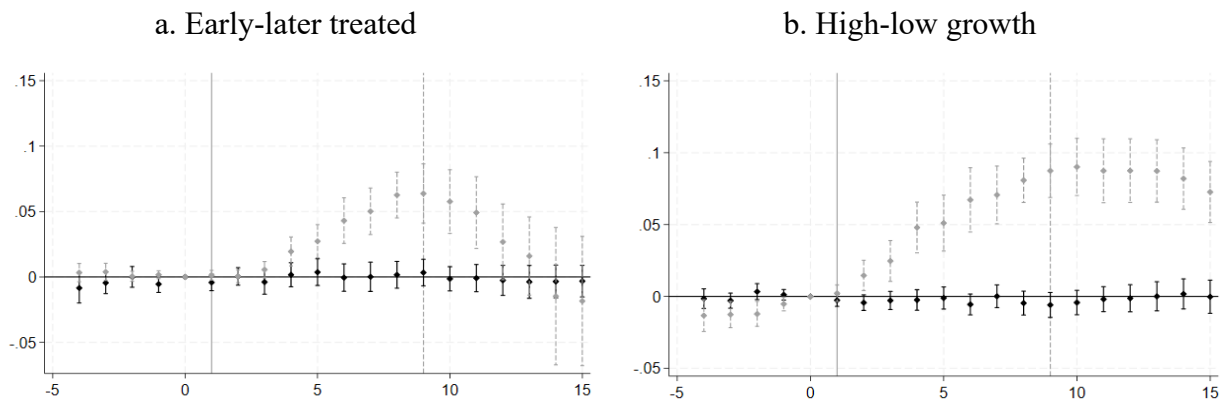
The estimations include municipality and year and municipality fixed effects, but no covariates.



Note: Early-never treated: Adding municipalities to the control group that were not treated with for-profits at any point, including 2014–2020). High-low growth or untreated: Adding municipalities to the control group with no for-profit preschool during 2001–2005 and 2007–2013. See also note to Figure 3 in the main text.

Figure A20. Drop two municipalities with substantial public to private transformations

The estimations include municipality and year and municipality fixed effects, but no covariates.



Note: The figures are based on the same estimations as Figure 3, with the exception that two municipalities, with substantial public-to-private preschool transformations taking place during the studied period, have been excluded. See also note to Figure 3 in the main text.