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

Karin Edmark[1,2,3*] and Lovisa Persson[4,5,6]

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.

JEL classification H44, I2, J31, J42

Keywords Monopsony power for-profit provision, Preschool workers, Gender gap

1 Introduction

During the last decades, 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 unusual in the early 1990 s, but currently, private providers cater to 22 percent of preschool children, to 16 percent of children at the primary and lower secondary levels, and to 31 percent of upper secondary students.[1] The breaking up of public sector monopolies in service production translates into the breaking up of public sector monopsonies on the labour markets for service professions (Le Grand 1991). The subsequent decrease in employer concentration is predicted to put upwards pressure on wages via increases in labour supply elasticities (Azar and Marinescu 2024) and/or stronger worker bargaining power (Jarosch et al. 2024). An increase in employer options locally could be particularly beneficial for women, who dominate the workforce in welfare services, and whose labour markets are already smaller geographically because of a reluctance to commute.[2]

The expectation that wages will increase after a quasi-market reform relies on the assumption that a public

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[1] The statistic for preschool is obtained from Skolverket (2025) and the statistic for primary- and secondary school is obtained from Skolverket (2024a).

[2] The reason is more extensive household responsibilities among women. Caldwell and Danieli (2024) find that 20 percent of the gender earnings gap in Germany can be attributed to gender differences in, primarily, implicit commuting costs. In general, Robinson (1933) suggested that differences in labour supply curves could be a source of the gender-wage gap. Ransom and Oaxaca (2010) suggest that lower supply elasticity among women explains lower wages among women in retail in the United States.

monopsony employer suppresses wages, and that entering employers will bid up wages. However, amendments to standard monopsony theory must be made that consider differences in employer organisational form. A public employer receives directions from politicians, who can be positively inclined, depending on their political leanings, towards union demands, leading to higher wages compared to those offered by a private monopsony employer. Furthermore, rather than being profit-motivated, public organisations are proposedly mission-oriented, i.e. workers care about the quality of service, and managers might accordingly care about providing sustainable working conditions (Besley and Ghatak 2005; Francois 2000).

In contrast, high-powered profit-motivated organisations will prioritise cost-minimization over quality-enhancing investments if there are substantial quasi-market failures, e.g. unobservability of non-contractable quality, weak reputation-building mechanisms, and switching costs (Hart et al. 1997). Under functioning quasi-markets, however, profit-motivated actors can be expected to innovate in the dimension of non-contractable quality by bidding up the wages of highly qualified workers (Hoxby 2002). Whether for-profit providers will engage in a bidding war over incumbent municipal preschool workers and contribute to higher wages overall cannot be settled by theory alone.

We study whether a policy reform that decreased public sector employer domination affected the wage level in the Swedish preschool sector. The reform, which was implemented in 2006, abolished the municipalities' right to veto private preschools to enter their markets. It resulted in an expansion of preschools organized as for-profit firms into municipalities which previously had no for-profit preschools. We use an event study differences-in-differences design and compare the wage development in municipalities who experienced for-profit entry in direct connection to the reform (treatment) with municipalities where for-profit entry occurred later in time (control).

The case of for-profit preschool expansion in Sweden has several interesting features. The municipalities were practically the sole local providers of preschool until quasi-market reforms implemented in the 90 s and 00 s gradually introduced the right to voucher funding for private providers of all organisational forms. The stated purposes of the reforms were primarily to increase preschool choice but also to improve working conditions and efficiency via provider competition. The reforms generated substantial variation in employer concentration over time and across local labour markets. Furthermore, we study a specific configuration of monopsony and challenger types; a public (municipal) monopsony employer and entering for-profit employers. A review by Azar and Marinescu (2024) addresses how antitrust regulation, measures that facilitate job switching, and minimum wages can reduce the wage suppressing effect of monopsony power. Our paper deals with a perspective on monopsonistic conditions and predictions that is rarely explored, namely the employer organizational form. Finally, we contribute by using variation in employer concentration induced by a policy reform. Most previous empirical studies in the monopsony literature rely on structural modelling, or case-studies of mergers, and lack quasi-experimental variation.[3]

The results from our analysis show no sign of any positive impact on the wage level despite significant reform-induced decreases in employer concentration in treated municipalities. Our wage estimates are close to zero and robust to alternative definitions of treatment and control groups. The parallel trend assumption is supported by an analysis that shows no differential pre-trends in the outcome variables.

The zero-result holds for the full sample, as well as for subgroups of workers, such as incumbent versus newly hired employees, and workers with different qualification levels. For instance, we do not observe an effect for experienced certified preschool teachers with high upper secondary school grades. We thus find no support for the argument that for-profit organizations invest in quality by bidding up wages of highly qualified workers, but we cannot rule out that for-profits are willing to pay more for workers with alternative qualifications that are unobservable to us. For instance, Hoxby (2002) finds that teachers in areas with a high degree of school choice are paid more for self-reported effort and extracurricular work.

We rely on descriptive evidence and institutional features of the Swedish preschool market to discuss possible mechanisms. First, we find that entering for-profit providers hire fewer certified preschool teachers, as well as less experienced and younger workers. For-profits also hire a substantial share of their workers from outside the preschool sector. These hiring practices mitigate the increase in bargaining power of incumbent workers and contribute to lower wages via selection. Second, workers in for-profit preschools negotiate their wages with a private employer organisation, who could potentially be a tougher negotiator than the public employer organisation. Edmark and Persson (2026) shows that for-profits generally set lower wages conditional on observable and unobservable worker characteristics.[4] It is also possible that the municipal employer did not use its market power

[3] For review articles, see Card (2022) and Ashenfelter et al. (2022). Staiger et al. (2010) utilizes a nurse pay reform and finds evidence of monopsony on the labour market for nurses. See also Thoresson (2024), who exploit policy-driven variation in the Pharmacy market, for other related studies.

[4] While Edmark and Persson (2026) performs a comparison between workers in different preschool organisations while controlling for observable

to supress wages because of political or other institutional reasons.

Our results can be contrasted with Thoresson (2024), who finds that the reduction in employer concentration that followed a deregulation of the Swedish pharmacy industry had a positive impact on wages, and Hensvik (2012), who estimates that a higher voucher school share is associated with increasing teacher wages, especially for high-skilled teachers and teachers entering the profession. The absence of a wage impact in our study can stem from weaker regulation of worker qualifications and fragmented monitoring in the preschool sector compared to the pharmacy and school sector. Hensvik (2012) furthermore captures competition from all private providers – including non-profits, who arguably have weaker cost-cutting incentives than for-profits.

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 weak requirements relating to worker qualifications. 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 that the municipalities rely on to fund their own preschools. The municipalities' main sources of revenue are the local income tax and central government grants. Municipal funding of preschools is complemented with a parental fee which is very low and regulated by the state. The same rate applies irrespective of the chosen preschool provider – municipal or private. Approximately 96 percent of all children in ages 4–5 attend preschool (Skolverket 2024b).

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 implemented 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) (Skolverket 2025). The National Agency for Education 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 average 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 is considerable 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

3.1 Reform history

During the 1980 s 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. In an effort to stem such a development, the Social Democrats and the Left Party passed a law in 1984 that restricted municipalities from channelling public funds to private providers other than in the following exceptions: parental cooperatives, non-profit organizations or actors offering alternative pedagogy (e.g. Waldorff) – worker cooperatives

and unobservable characteristics, the present paper instead uses variation induced by a reform that changes the competitive pressure.

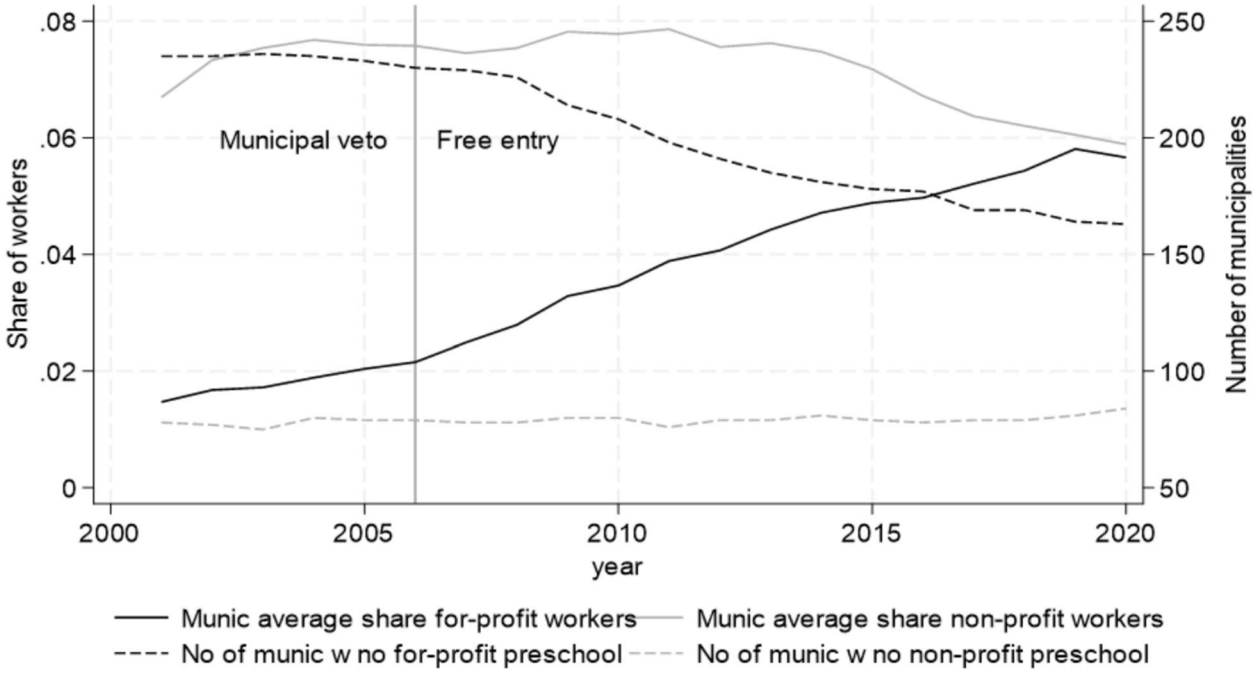

Fig. 1 Trends in market shares and for-profit presence before and after the 2006 reform. Municipal level unweighted averages, 288 municipalities

were added to the list in 1991.[5] For-profit companies in the form of limited companies (*Aktiebolag*) were however explicitly prohibited. Accordingly, municipalities had full discretion over whether to approve and fund private preschools as long as they were on the list of approved organisations.

The ban on for-profits was strongly contested by the centre-right parties. When they formed a coalition government after the 1991 election, they promptly lifted the ban, meaning that also for-profits could be allowed to operate and receive public funding from municipalities subject to the municipality's approval (Government bill 1991/92:65). The centre-right government had also prepared to implement a free-entry law that would abolish the municipal veto, but this law was reversed before coming into effect when a Social Democratic minority government regained power after the 1994 election.

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, over-riding the Social Democratic minority government (Education committee report 2005/06:UbU13). The law specified that municipalities must approve and provide public funding to any private preschool able to fulfil certain quality requirements. Funding could be denied only if the municipality could show that a new preschool would have significant negative economic consequences, or if the parental fee was unreasonably high compared to that levied by the municipal preschools. After the election in 2006, the centre-right parties formed a majority government coalition, and the free-entry regulation was further consolidated in 2009, when the right to municipal funding on equal terms was clarified (Government bill 2008/09:115). The presence of the for-profit motive is still being debated, and the largest opposition party, the Social Democratic Party, currently wants to abolish the for-profit motive in preschool.[6]

The stated purpose of the quasi-market reforms in Swedish preschool has primarily been to increase the freedom to choose a preschool suited to family preferences. Quality improvements and efficiency gains due to provider competition were also anticipated when the ban on for-profit preschools was lifted in 1991. As it pertains to workers, policymakers expressed hope that more outside options would lead to better working conditions and employer-employee-matches: "Preschool teachers and childcare assistants will have more employers to choose from. It gives them a stronger position in relation to improving working conditions and to choose under which circumstances they want to work" (Government bill 1991/92:65, pp 4, our translation).

As can be seen in Fig. 1, the 2006 reform had a substantial impact on the local presence of for-profit preschools. The average municipal share of preschool

[5] Approved organisations were listed in Government bill 1983/84:177 and employee cooperatives were added in Government bill 1990/91:38. See also Linnarsson (2017), Larsson and Westberg (2020), and Martin-Korpi (2015) for more details.

[6] https://www.socialdemokraterna.se/nyheter/nyheter/2025-09-04-vinstforbud-i-forskolan---s-lagger-forslaget-pa-riksdagens-bord

Table 1 The likelihood of at least one for-profit or one non-profit preschool in the municipality, 2001–2020

	> 0 For-profit	> 0 Non- profit
	(1)	(2)
The Moderate Party	1.562***	0.368
	(0.315)	(0.233)
2006#Moderate Party	−0.914***	−0.263
	(0.252)	(0.169)
Ln age 1–5 population	1.704***	1.720***
	(0.189)	(0.256)
Ln tax base	−1.227	−2.208
	(1.654)	(1.790)
Female employment	0.702	2.146***
	(0.691)	(0.566)
Male employment	−0.302	−1.869***
	(0.565)	(0.460)
Share Swedish- Born	−0.302	0.174
	(0.222)	(0.248)
Urban Municipality	0.758*	1.020
	(0.456)	(0.854)
Mid-sized municipality	0.447	0.491
	(0.319)	(0.313)
Municipality-year observations	5,759	5,759

The variable Moderate Party is the seat share for the Moderate Party in the municipal council. The seat share, as well as the variables for female and male employment and the share of Swedish-born, are 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

workers employed in for-profits tripled after 2006, and the number of municipalities with no for-profit preschool dropped rapidly a few years into the reform.[7] For the non-profit private sector there is however no discernible positive impact of the 2006 reform; the average share of non-profit workers instead starts to decline after 2014. The reforms have purposedly boosted choice possibilities on average, although the decrease in non-profits in later years can be seen as a decline in variety. Neither the outcomes for workers nor children have been thoroughly investigated.

3.2 Geographical and political patterns in for-profit entry

The reforms have increased preschool choice and provider variety, albeit to varying extent in municipalities. The results in Table 1, which shows the correlation between for-profit and non-profit preschool presence and municipal characteristics, highlights the role of urbanization and politics. Regarding the latter, the table shows that the presence of for-profit preschools was, prior to 2006, strongly correlated with the municipal council seat share for the largest of the centre-right parties – the Moderate Party – which had a long history of advocating for for-profits in the preschool sector.[8] 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 1.56-point higher log-odds, or an increase in the odds ratio of nearly 5, 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 pattern 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.

Column 2 shows that the political dimension is only present for for-profits; for the non-profits, 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, and, among the for-profits, the dummy variable for urban municipality, defined as large cities and municipalities within a substantial share of commuters to a large city. This is not surprising, given that service providers prefer markets with a larger potential customer base. The presence of non-profits is also positively associated with female employment, and negatively with the male employment rate.

4 Empirical method

The fundamental methodological problem that we need to address is that an observed municipal-level correlation between the for-profit market share 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 population size and Moderate party vote share, which were shown to be positively correlated with for profit preschool presence in Table 1. 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). Using the reform has two benefits: First, it provides substantial variation in the for-profit preschool share (Fig. 1). Second, because of the design of the reform, it is plausible that the for-profit increase after 2006 stemmed from for-profits being

[7] 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. The more immediate increase in the share of for-profit workers might reflect an expansion of already existing establishments. 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 expansion among prospective private providers further.

[8] Table 5 in the appendix reproduces Table 1 with step-wise addition of the righthand side variables.

allowed to enter municipalities where they had previously met substantial political barriers (as reflected in the decreased association between the for-profit advocating Moderate party and for-profit presence after 2006 in Table 1), rather than from sudden changes in the development of municipal characteristics that determine the attractiveness of the market.

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

Our panel of municipalities spans 2001–2020. 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. 2007–2013. We do not view year 2006 as a pure treatment year since the reform took effect 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 these "later-treated" municipalities constitute a relevant counterfactual to the "early-treated" group. The underlying assumption is that the timing of for-profit entry 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.

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. The reform was unexpected in the sense that it was voted through parliament by the opposition against the sitting government, and it is reasonable to expect that it would take some time for for-profit actors to adjust to the new situation and complete the process of starting a new preschool. For completeness, we also show the event study coefficients for the subsequent seven years, 2014–2020.

Figure 2 shows the for-profit worker share for the treatment and control groups respectively. 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 municipal markets in the two groups are similar in terms of attractiveness for for-profits but differ in timing of for-profit entry. The maximum difference between the treatment and control groups is around 7 percentage points.

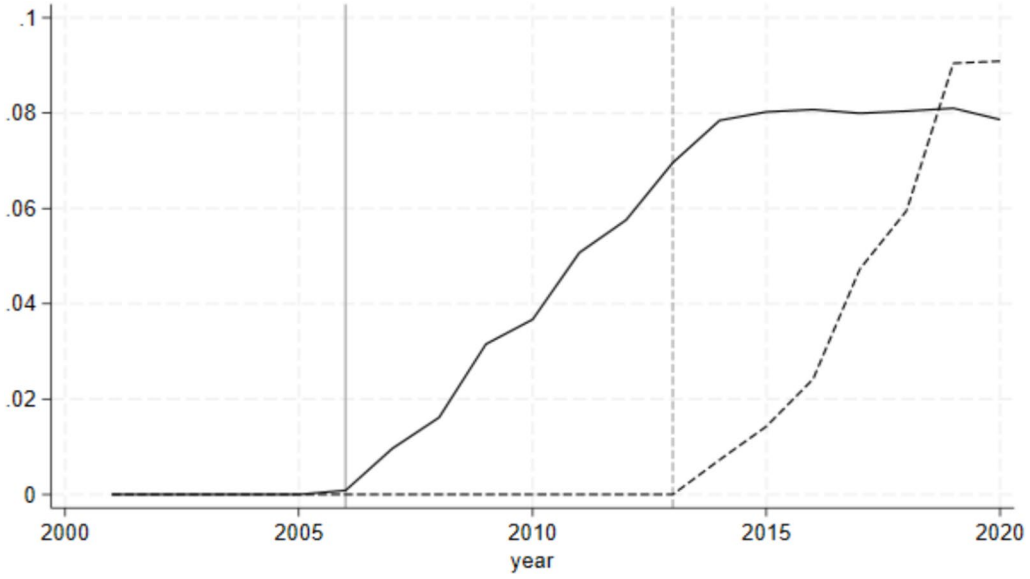

Fig. 2 For-profit worker shares in treated (solid) and control (dash) municipalities. Note the different scales on the y-axis. Municipal level unweighted averages. The vertical solid grey line indicates the timing of the free-entry reform, and the vertical dashed grey line indicates the end of the period used for determining the treatment and control group. The treated group contains 51 municipalities and the control group 28 municipalities

We consider this difference to be large enough to expect differential preschool market outcomes, in particular since we also capture variation at the extensive margin; from no to some for-profit preschools.

For-profit expansion is expected to reduce employer concentration as more firms enter the local labour market. Appendix Fig. 4 shows that an increase in the for-profit share is associated with a substantial reduction in the Herfindahl–Hirschman index (HHI), calculated using worker shares. The HHI decreases rapidly after 2006 in the early-treated municipalities, from around 0.85 to 0.75 (Fig. 4a).[9] The right-hand panel, Appendix Fig. 4b, where the HHI is used as an outcome in event study regressions, establishes that the decrease is significantly different from the development in the control municipalities.[10]

As was noted above, our empirical strategy hinges on the assumption that the entry of for-profits in the treated municipalities after 2006 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, it can be informally evaluated by studying whether the development of municipal characteristics follow parallel trends in the treated and control group around the reform. In Fig. 6 in the Appendix we show how the log of the local preschool age population, the income level (measured by the income taxbase), and the local government seat shares for the Moderate Party and for the two centre-left parties, the Social Democrats and the Left party, developed over the studied period. Although the levels sometime differ substantially, they overall follow roughly similar trends. There are thus no immediately visible signs that the parallel trends assumption is violated. Event study estimates available in Appendix Fig. 7, which provides a more formal test of the assumption, 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 group. 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 (Fig. 19) show that the estimates are insensitive to removing the municipal covariates; suggesting that the estimates are not affected by the differential population trends.

4.2 Event study estimation equation

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

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

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 ($l=0$). Our empirical strategy is thus a difference-in-difference estimation where $D_{mt} = \tau_t \times T_m$, i.e. interactions between year dummies τ_t and T_m, which is a treatment dummy variable that takes on values 1 if the municipality is in the treated group and 0 otherwise. The validity of difference-in-difference estimation hinges on the parallel trends assumption. 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 (Eq. 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 worker observations 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.

[9] A perfect monopsony case corresponds to a HHI of one. The average HHI is below one in treated municipalities before the reform because of a small presence of non-profits. A decrease in the HHI before 2006 signals growing market shares among non-profits; the trends in HHI are seemingly parallel before the reform but diverge after.

[10] Results that are available in Appendix Figure 5 shows that the for-profit increase is associated with some reduction in the non-profit worker share. 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.

Table 2 Descriptive statistics 2001–2013

	All	Treated	Control
Wage	23,367	23,485	23,148
Ln wage	10.05	10.06	10.04
Municipality characteristics			
Ages 1–5 population	1,750	2,072	1,077
Ln ages 1–5 pop	6.85	7.30	6.65
Taxable income	175,137	176,169	169,359
Ln taxable income 12.06		12.07	12.03
Seat share The Moderate Party	0.189	0.190	0.184
Seat share Soc. Dem. and Left Party	0.442	0.440	0.439
Worker characteristics			
Preschool teacher	0.446	0.454	0.452
Childcare assistant	0.264	0.263	0.256
Age	43.36	43.46	43.44
Female	0.9734	0.9734	0.9704
Swedish-born	0.94	0.93	0.95
Number of municipalities	288	51	28
Municipality-year observations	3,743	663	364

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

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" (*Aktiebolag*) are classified by us as being "for-profit".[11] 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 unlikely to be a 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.[12] 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 Sect. 4.2 for how these variables are defined and included in the analyses.

[11] According to Government Report 2025:37, 17 percent of limited companies in the school sector distributed profits in 2024. However, this number does not include transfers within a corporate group. These kinds of transfers are common among the larger companies and are, due to complicated company structures, difficult to quantify.

[12] 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 €).

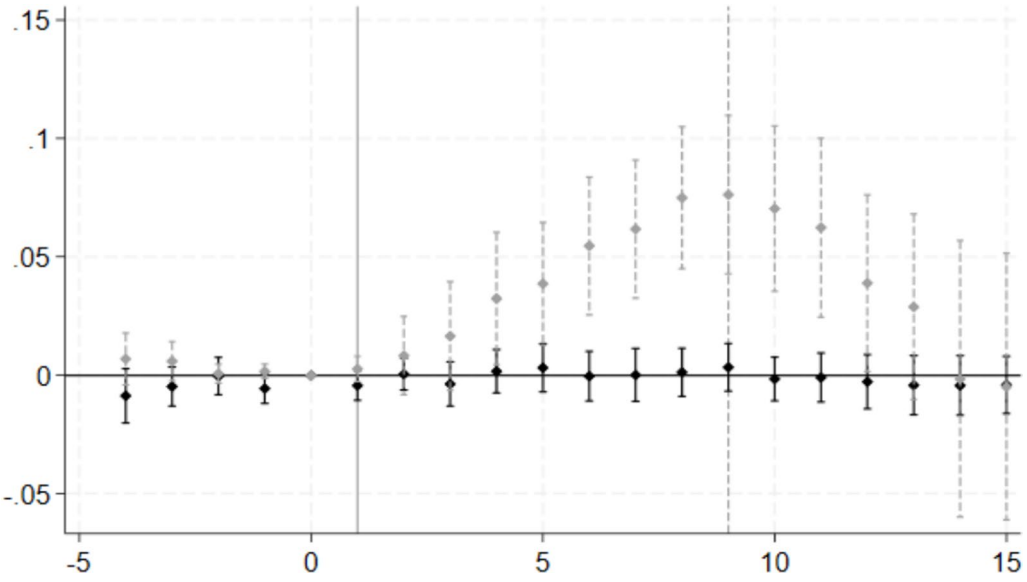

Fig. 3 Event study estimates for ln wage (black) and for-profit worker share (grey). Observations are on worker level but are weighted with the inverse of the municipality-year number of workers, such that equal weight is given to all municipalities. The number of individual-year observations are 389,899, and there are 51 municipalities in the treated group and 28 in the control group. 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

5.2 Descriptive comparison between treated and control municipalities

Table 2 presents summary statistics regarding worker outcomes and worker characteristics. The individual level data is first aggregated to municipality-by-year averages and then averaged by treatment status.[13] We also show a set of municipal level characteristics. 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. Treated municipalities have a somewhat higher taxable income level. On the political side, the seat shares of the Moderate Party and the sum of Social Democrats and the Left Party are balanced.

6 Results

6.1 Main results

In this section we present the results of the event study regression (Eq. 1). Our main results can be found in Fig. 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.[14]

In Appendix Figs. 8 and 9, we obtain a null effect also when restricting the analysis to workers employed by a preschool the previous year, and newly hired workers, respectively. This suggests 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. The null result also holds when we isolate the sample to workers in municipal preschools (Figs. 10 and 11).

It is possible that the zero-impact on wages found in Fig. 3 masks heterogeneous effects across workers with different qualifications. To test if this is the case, we re-estimate the event study regression (Eq. 1) on split samples according to educational background, worker experience, upper secondary grades, and combinations of the three. The results for all the subgroup analyses are that the zero coefficients from the baseline estimations remain (Appendix Figs. 12, 13, 14 and 15). Notably, we obtain a zero result when studying the effects for workers

[13] The regression analysis will be run on individual level data, but we will apply weights to give each municipality equal weight, in order to avoid that large municipalities get a larger influence on the estimates.

[14] In a prior working paper version of this paper, we included results using alternative definitions of treatment and control that relied on different rates of post-reform growth of the for-profit share. These are excluded from this version, as they showed signs of different pre-reform trends. The results of the alternative strategy showed the same zero wage effect as the results reported here (Edmark and Persson 2025).

Table 3 Background characteristics of preschool workers in treated municipalities 2007–13

	Municipal preschools	For-profit preschools	Difference (Municipal–For-profit)
Preschool teacher	0.484	0.347	0.138*
Childcare assistant	0.238	0.264	−0.026*
None of the above	0.277	0.389	−0.112*
Preschool work experience	5.833	4.637	1.196*
Age	44.068	39.483	4.585*
High grade	0.137	0.107	0.030*
Mid-grade	0.401	0.409	−0.008
Low grade	0.117	0.148	−0.031*
Missing grade	0.345	0.336	0.009

The table shows the average characteristics of workers in municipal and for-profit preschools, in the treated municipalities, in 2007–2013, as well as the average difference between them. * denotes that the difference is statistically significant at the 5% level. Preschool experience denotes the number of years employed in a preschool during the last eight years. Preschool experience and age is measured in years. The remaining variables are shown as shares

who are certified preschool teachers, who have more than five years of work experience in preschool and who have high upper secondary grades in combination, i.e. workers who are highly qualified in all observed dimensions do not experience a wage increase, see Fig. 13. Fig. 16 furthermore shows weak signs of an increase in the total number of preschool workers (per enrolled child) in the treated municipalities. The last result shall however be taken with a grain of salt, as we cannot observe worked hours, and thus cannot measure the number of full-time equivalent employees.

6.2 Robustness analysis

To increase the treatment dose, we re-estimate Fig. 3 and restrict the analysis to the top half of the treated municipalities in terms of average increase in the for-profit share 2007–2013. The for-profit worker share difference-in-difference at the end of the first treatment period increases from 8 to 13 percentage points. The results, which are shown in Appendix Fig. 17, mainly reproduce the null result of the main estimation. 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 (Fig. 18); removing municipality- and worker level covariates (Fig. 19), adding municipalities to the control group that were either never-treated during the studied period (Fig. 20a), and dropping a municipality that experienced a substantial public to private transformation after the reform (Fig. 20b). The results from these robustness checks are similar to the main results.

7 Mechanisms

The zero result for increased competition on the preschool market from for-profit providers diverge from standard predictions from monopsony theory (Azar and Marinescu 2024; Jarosch et al. 2024). In this section, we discuss possible alternative mechanisms, related to hiring patterns and regulatory legislation.

7.1 Differences in recruitment

Worker qualifications and certifications are less regulated in the preschool sector than in the school sector. Schools must always fill teacher positions with certified teachers above all other candidates, while preschools only require at least *one* certified preschool teacher on staff to teach the preschool curriculum. Moreover, the school sector is monitored by a national agency, the Swedish Schools Inspectorate. Regulation and monitoring are weaker in the preschool sector, while the monitoring of private preschools is more fragmented and in the hands of the municipalities.

There are thus more degrees of freedom to hire workers without relevant certifications and educational background in preschool. Certified preschool teachers have 3.5 years of tertiary education, and their wages are considerably higher, on average 19 percent, than unqualified preschool workers, whereas childcare assistant on average have around 3 percent higher wages than unqualified workers. In the absence of legislative requirements, worker qualifications become a dimension of non-contractable quality where for-profit providers can either minimize costs or invest in quality.

We now analyse recruitment behaviour among the entering for-profits in the treated municipalities in comparison with the incumbent municipal employers. Recruitment is a mediator for the competition effect; if for-profits prefer staff without formal qualifications and without prior preschool work experience, the change in bargaining power among incumbent workers is subdued.

In Table 3, which shows the characteristics of workers recruited by for-profit and municipal preschools in the treated municipalities in the post-reform period (2007–13), reveals that for-profits tend to hire a lower share of preschool teachers (35 percent) than municipal employers (48 percent). The share of childcare assistants is roughly the same, and the share of non-qualified workers is higher in for-profits. Workers in for-profits furthermore have shorter work experience in preschool,

Table 4 Previous employment of hires in for-profits in treated municipalities, 2007–13, %

Panel A: Employed in a preschool in the municipality in t-1	
Municipal preschool	23.03
Other for-profit preschool	3.77
Non-profit preschool	12.33
	∑ 39.13
Panel B: Employed in a preschool in another municipality in t-1	
Municipal preschool	7.07
Other for-profit preschool	2.96
Non-profit preschool	1.75
	∑ 11.78
Panel C: Employed in other sectors in t-1	
Health, welfare, other education	13.13
Trade, hotel, restaurant, transport, postal- and telecom. services	7.41
Finance, insurance, real estate, data processing, R&D, public sector	4.71
Lobbying/advocacy, religious activity, culture, sports, other services	2.83
Agriculture, forestry, manufacturing, construction, sewage, sanitation	2.09
Missing sector information	10.98
	∑ 41.15
Panel D: Other categories	
No employment in t-1	7.54
Missing info t-1	0.4

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

4.64 years in the last 8 years compared to 5.83 years in municipal preschools; they are younger, 39 years old compared to 44 years old, and they are somewhat less likely to have high upper secondary grades. Edmark and Persson (2026) show similar differences between for-profit and municipal while comparing the full population of preschool providers across the country in 2020 (lower share of certified preschool teachers, shorter work experience, younger, and lower grades in for-profits).

The differences in Table 3 are not extreme, but they do indicate that for-profits are more likely to recruit workers that lack both formal and informal qualifications. Since we lack direct measures of preschool quality, we cannot rule out that for-profit preschools are still 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).[15]

Our results on recruitment behaviour differ somewhat from Hensvik (2012), who finds that voucher schools, similarly to preschools, tend to hire workers with lower formal qualifications than municipal schools, but that they are *more* inclined to hire workers with higher cognitive skills. We show, however, that for-profit preschools tend to hire workers also with lower *informal* qualifications, as indicated by upper secondary grades. The differential recruitment behaviour among for-profit preschools is in line with the results in Knutsson and Tyrefors (2022), who find that private ambulance services perform worse in terms of non-contractable quality, i.e. mortality, and that the explanation is partly because of lower staff quality. Knutsson and Tyrefors (2022) note that private ambulance firms have higher turnover, fewer specialist nurses, and employees have lower work experience and cognitive skills. They also find that the hourly wage is lower.

We now turn to studying from where the for-profit providers recruited their staff. In Table 4 we show the employment status in year *t-1* among workers who were hired by for-profit preschools in year *t*, where *t* goes from 2007 to 2013. As shown in Panel A, 39 percent of the new hires came from preschools in the same municipality, and 23 percent were previously employed in a municipal preschool in the same municipality. 12 percent of new hires were recruited from preschools in other municipalities (Panel B). 41 percent of new hires were previously employed in another sector of the economy (incl. missing information sectors), mainly in health, welfare or non-preschool education or in other service sectors including e.g. trade, restaurant and hotel (Panel C), and 7.5 percent had no employment in *t-1* (Panel D). All in all, approximately half of all new hires came from outside the preschool sector. The substantial outside recruitment can be a partial explanation for the lack of wage impact, as it limits the competition for workers already employed in the preschool sector.

To summarize, we find that for-profit preschools recruited cheaper workers with lower formal and informal qualifications, and that almost half were recruited outside of the preschool sector. Because of weak

[15] 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 and Fryer (2013) find that formal teacher training and certification is not related to charter school outcomes. Harris and Sass (2011) find a positive relationship between teacher productivity and (early) work experience.

regulation regarding worker qualifications in preschool, worker qualifications constitute a non-contractable dimension of quality, and if quality-investments are not salient for-profit providers might select a different pool of workers that are less qualified and that have less preschool experience (Hart et al. 1997). We argue that the for-profit motivation to hire cheaper staff, in combination with weak regulation regarding worker qualifications in preschool, is a plausible contributing mechanism behind the zero result. It can be noted that the preschools that we study are all funded by a combination of public grants and a small regulated parental fee. This means that employers cannot finance higher wages through price-increases, something that may dampen the incentives to attract highly qualified workers.

The hiring of cheaper and less qualified workers could give rise to poor preschool outcomes if non-contractable quality is not upheld by the competitive pressure of parental choice. Whereas we cannot observe quality, nor the degree of demand side competitive pressure, one can note that the period that we study is characterized by growing preschool age cohorts; the number of 1–5-year-olds was 460 702 in 2001, and 579 514 in 2013, (Statistics Sweden database). When preschool is in an expansionary phase because of growing demographic demand, it is plausibly easier to run at full capacity in spite of weak quality, resulting in less competitive pressure from parental choice.

7.2 Institutional and other features

It is also possible that the lack of a wage impact stems from differences in the legislative and institutional features among municipal versus for-profit organizations. For one, worker unionization tends to be higher in the public sector (OECD 2025). In Sweden, all workers in the public sector are covered by collective bargaining agreements, while ca 82 percent of the workforce in the private sector is covered (Swedish National Mediation Office 2025). In addition, for-profit companies may be tougher wage negotiators than the municipal organizations. This could be due to the explicit profit motive in the former, or to municipal politicians aligning with the preferences with the unions. All these factors could contribute to lower wage rates among the for-profits, thus dampening their attractiveness as outside options for the municipal workers.[16]

The public insight into the economy and activities of private establishments is weaker. Public agencies in Sweden adhere to far-reaching freedom of information laws, including public employers in preschool, whereas private providers of publicly financed services do not. Workers in the public sector are also protected by whistleblower laws. Protection for whistleblowers was extended to include workers in private organisations providing publicly funded services in 2017. These factors could also contribute to a generally weaker bargaining position of the private workers towards their employer.

To summarize, institutional and legal features suggest that public and private employers would behave differently as monopsony employers. It has been pointed out in the monopsony literature that de facto monopsony power can be dampened by institutional features of the labour market (Manning 2021; Sokolova and Sorensen 2010), this study is to our knowledge the first that explicitly recognizes the link to organizational form (Azar and Marinescu 2024).

Finally, it has been proposed that public organisations are mission-oriented, in contrast to profit-motivated private organisations (Besley and Ghatak 2005; Francois 2000). Mission-orientation implies that workers care about the quality of service, i.e. the "mission". If that is the case, jobs in the public sector attract a selected group of workers who seek alignment between their own motivation and the employer. For-profit organizations might therefore not be regarded as an equivalent outside option for some municipal workers. On the other hand, Edmark and Persson (2025) find that sick leave and prescription of medicine for mental health conditions is lower in both for-profits and non-profits compared to municipal preschools. More research is required regarding worker health outcomes in different types of preschools.

8 Conclusions

Policymakers anticipated that the introduction of quasi-markets in preschool would lead to a stronger bargaining position for workers and a subsequent improvement in working conditions. However, our analysis yields no evidence that reduced employer concentration, in the form of an increased presence of for-profit preschool providers, altered the average wage level among preschool workers. The null result, which is stable across groups of workers and for a set of robustness tests, contrasts to the predicted positive wage effect of decreased employer concentration in theoretical models on employer monopsony and outside options.

A potential explanation behind the lack of a positive wage effect might stem from the specific configuration of increased for-profit provision in a setting where a public provider is the dominant provider. Politics,

[16] A report by the Swedish Municipal Workers' Union (2025), based on register data, shows that temporary- and part-time employment contracts for childcare assistants are more common in for-profit preschools.

mission-orientation, higher unionization rates or other legislative features could contribute to less wage suppression when the monopsony employer is a public employer. The configuration of an incumbent public employer and cost-minimizing incentives among entering for-profit providers may have counteracted the wage-boosting effect of lower concentration.

Our data shows that the entering for-profit preschools hired half of their workers from other sectors than the preschool sector, and that the recruited workers to for-profit preschools are on average less qualified and experienced. Such hiring practices decrease the average wage level via selection but also via attenuation of the competitive pressure for incumbent and experienced workers. The institutional barriers against reducing costs through hiring low-qualified personnel are relatively weak in the Swedish preschool setting.

Lower labour supply elasticities among the female worker population or averseness to for-profit providers because of a perceived conflict in terms of motivation (mission vs for-profit), or risk-taking in terms of work security and employment conditions, 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.

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. 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 consider as they may attenuate or strengthen the consequences of monopsony market power.

It must be noted, however, that our study does not include the full universe of perks and work conditions that affect worker welfare. This is an area where future studies can contribute. Other areas of research that remain to be studied concern the impact on preschool quality, as well as the role of alternative providers and employer concentration in other institutional contexts.

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

A1. Municipality characteristics and the expansion of for-profit and non-profit preschools

Table 5 The likelihood of at least one for-profit or one non-profit preschool in the municipality

	> 0 For-profit				> 0 Non- profit			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
The Moderate	1.816***	1.775***	1.663***	1.562***	1.012***	0.387	0.452**	0.368
Party	(0.238)	(0.301)	(0.312)	(0.315)	(0.223)	(0.236)	(0.226)	(0.233)
2006#Moderate	−0.550***	−1.026***	−0.955***	−0.914***	−0.105	−0.212	−0.294*	−0.263
Party	(0.205)	(0.251)	(0.256)	(0.252)	(0.174)	(0.150)	(0.163)	(0.169)
Ln age 1–5		1.787***	1.714***	1.704***		1.630***	1.756***	1.720***
population		(0.173)	(0.185)	(0.189)		(0.248)	(0.259)	(0.256)
Ln tax base		0.207	−0.280	−1.227		−0.978	−1.554	−2.208
		(1.550)	(1.651)	(1.654)		(1.532)	(1.563)	(1.790)
Female			0.622	0.702			2.075***	2.146***
employment			(0.659)	(0.691)			(0.536)	(0.566)
Male			−0.168	−0.302			−1.721***	−1.869***
employment			(0.556)	(0.565)			(0.444)	(0.460)
Share Swedish-			−0.405*	−0.302			0.113	0.174
Born			(0.210)	(0.222)			(0.230)	(0.248)
Urban				0.758*				1.020
Municipality				(0.456)				(0.854)
Mid-sized				0.447				0.491
municipality				(0.319)				(0.313)
Municipality-year observations	5,759	5,759	5,759	5,759	5,759	5,759	5,759	5,759

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. Female and male employment and the share of Swedish-born are also 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

A2. Herfindahl–Hirschman index and worker shares

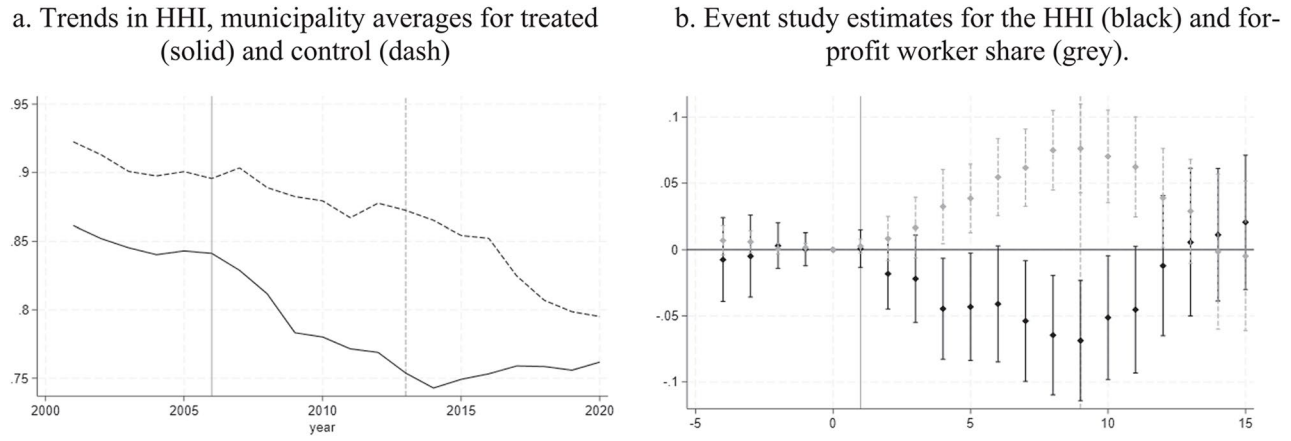

Fig. 4 The Herfindahl–Hirschman index. Measures of HHI include market shares among non-profit preschools. An HHI-value below one in the period before reform signals non-zero market shares among non-profits. The HHI is calculated such that each firm or municipality is viewed as a separate employer, and firms who belong to the same corporate group are treated as the same employer. The regression equation includes municipality and year fixed effects. 95% confidence intervals, based on standard errors are clustered on municipality, are shown

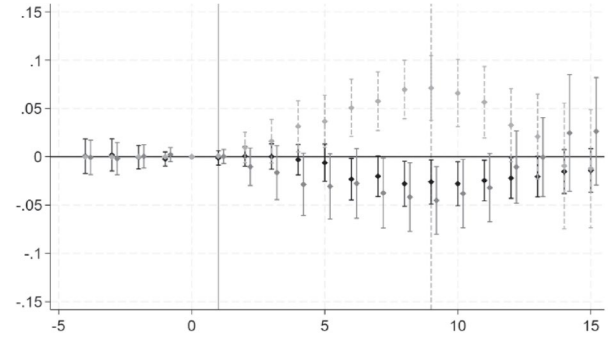

Fig. 5 Event study estimates for the share of non-profit workers (black), municipal workers (grey, solid), and for-profit workers (light grey, dash) The event study regressions are weighted with the inverse of the municipality-year number of workers. The regression equation includes municipality and 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

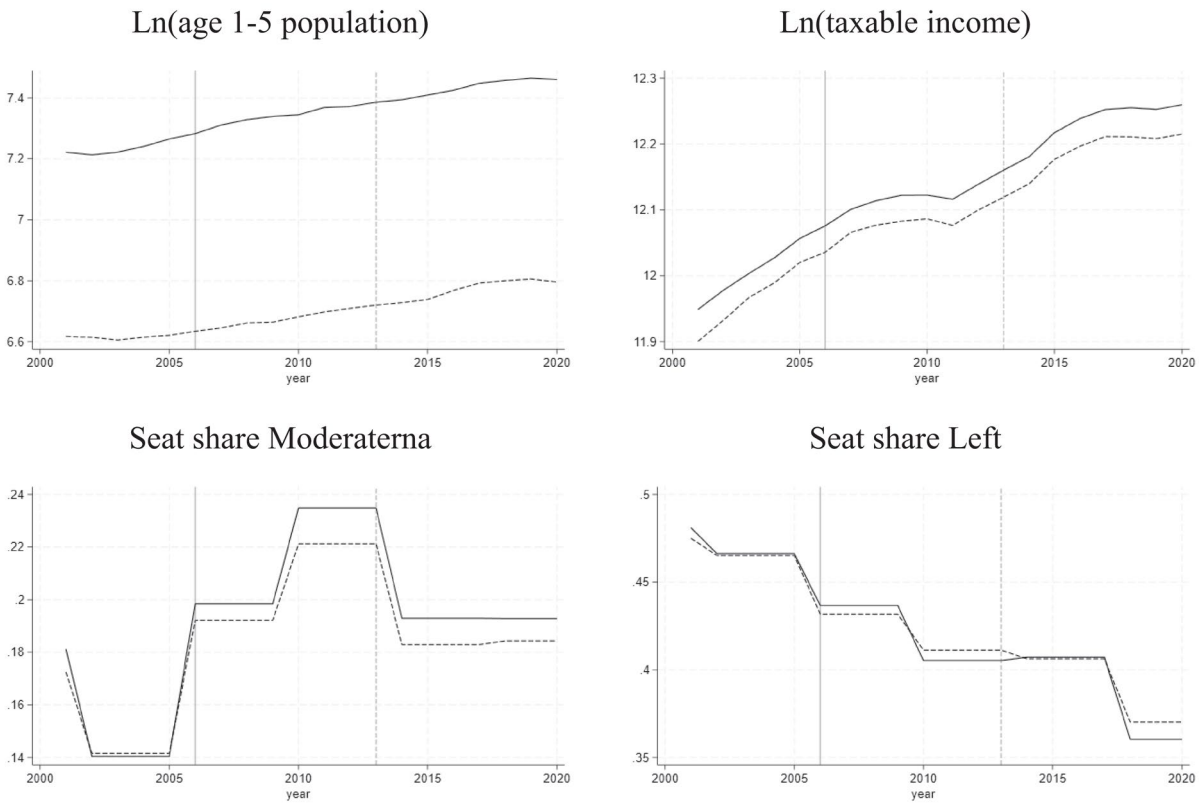

Fig. 6 Trends in municipal characteristics for treated (solid) and control (dash).Note that the scale of the y-axis differs between the figures

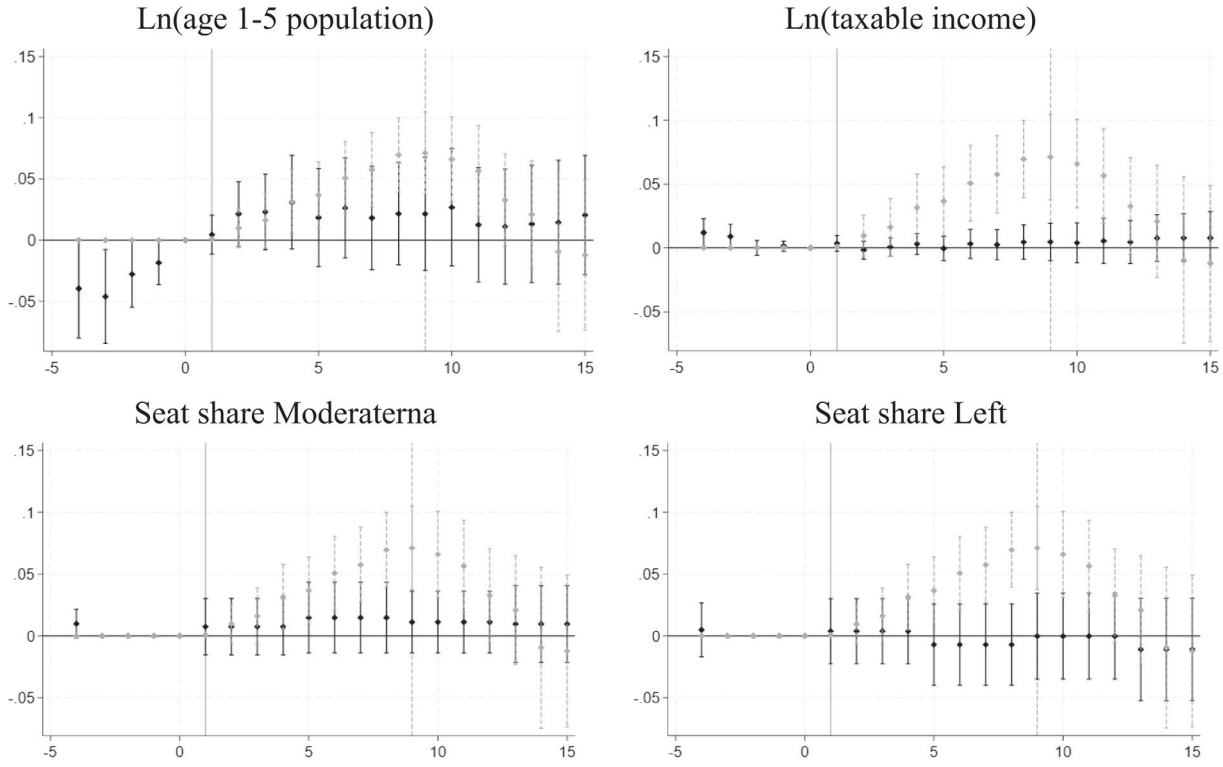

Fig. 7 Event study estimates for municipal characteristics. The grey markers show the event study estimates for the for-profit share of preschool workers. The estimations include municipality and year and municipality fixed effects, but no covariates. The figures show 95% CI, based on standard errors clustered on municipality

A4. Additional event-study regressions

A4.1 Sub-group analysis – ln(wage) as outcome

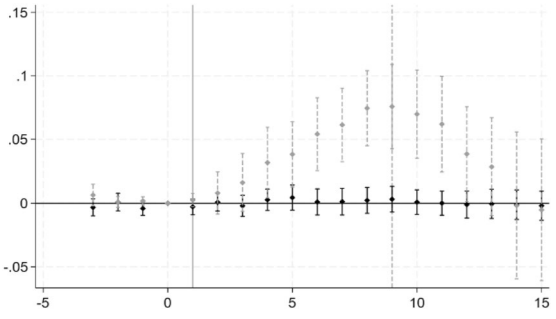

Fig. 8 All incumbent workers (worked in a preschool in the municipality the previous year). See note to Fig. 3 in the main text. *The grey dashed lines show estimates for the for-profit worker share*

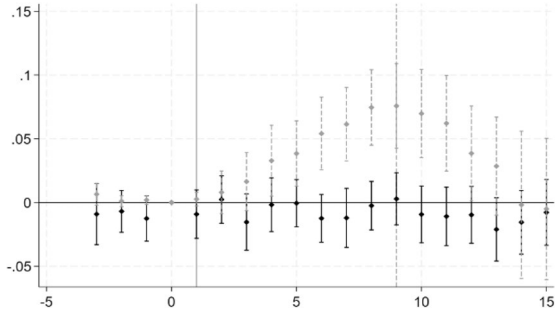

Fig. 9 All new hires (did not work in a preschool in the municipality the previous year). See note to Fig. 3 in the main text. *The grey dashed lines show estimates for the for-profit worker share*

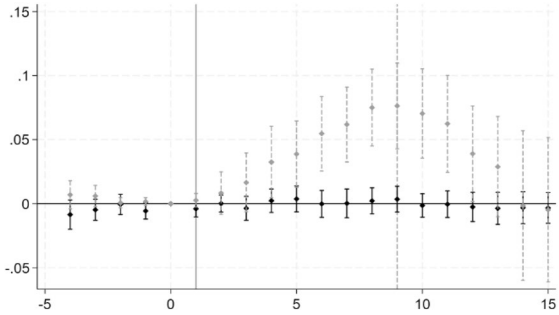

Fig. 10 Municipal preschool workers. See note to Fig. 3 in the main text. *The grey dashed lines show estimates for the for-profit worker share*

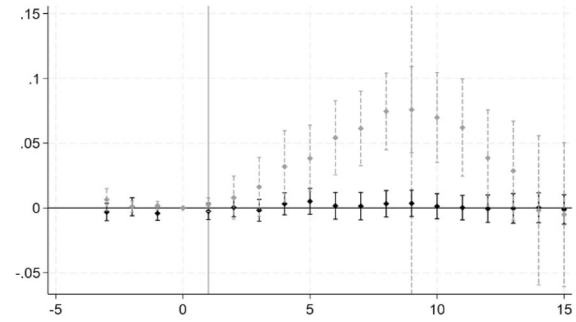

Fig. 11 Municipal incumbents (worked in municipal preschool in the municipality the previous year). See note to Fig. 3 in the main text. *The grey dashed lines show estimates for the for-profit worker share*

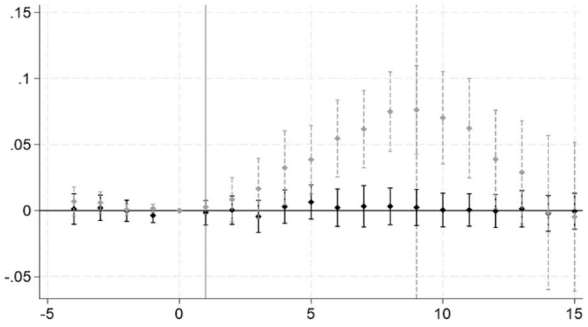

Fig. 12 Preschool teachers. See note to Fig. 3 in the main text. *The grey dashed lines show estimates for the for-profit worker share*

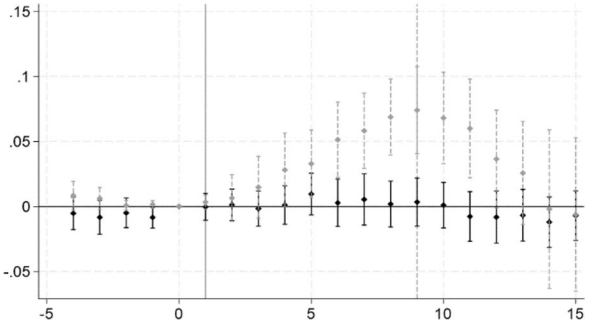

Fig. 13 Preschool teacher degree, > 5 years experience and high grades. See note to Fig. 3 in the main text. *The grey dashed lines show estimates for the for-profit worker share*

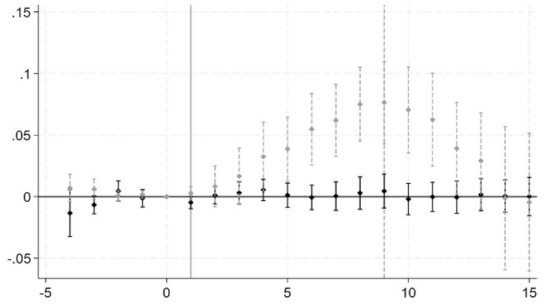

Fig. 14 Childcare assistant degree. See note to Fig. 3 in the main text. *The grey dashed lines show estimates for the for-profit worker share*

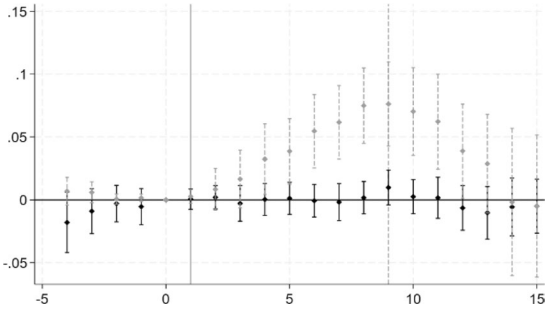

Fig. 15 Unqualified workers (neigher preschool teacher nor childcare assistant degree). See note to Fig. 3 in the main text. *The grey dashed lines show estimates for the for-profit worker share*

A4.2 Number of preschool workers per enrolled child in the municipality

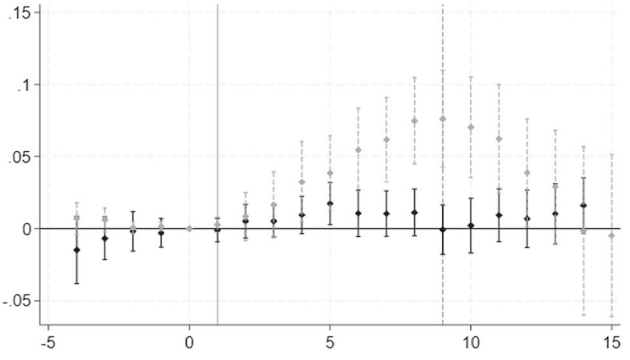

Fig. 16 Outcome: Number of preschool workers per enrolled child. Observations are on worker level but are weighted with the inverse of the municipality-year number of workers. Municipality- and year fixed effects are included. Municipality covariates: ln age 1–5 population in level and squared; ln taxable 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. *The grey dashed lines show estimates for the for-profit worker share*

A5. Robustness checks – event study estimates for ln(wage)

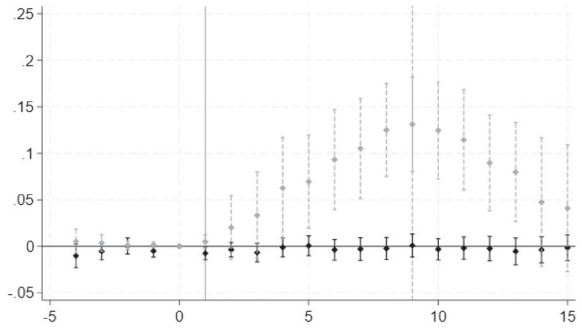

Fig. 17 Increasing treatment dose: Top half early treated and all later treated. See note under Fig. 3 in the main text. The treatment group includes only municipalities with the largest increase in the for-profit share between 2007 and 2013; the control group is identical to the control group in the main estimations. The grey dashed lines show estimates for the for-profit worker share

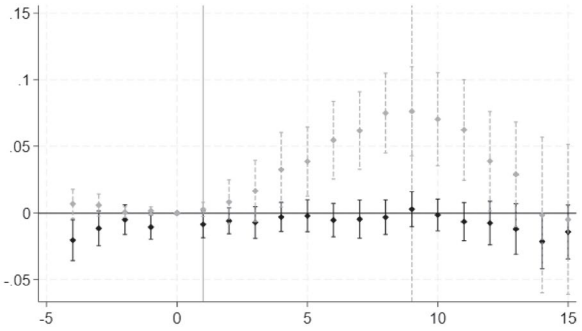

Fig. 18 Using weights corresponding to the inverse of the wage sampling probability. See note under Fig. 3 in the main text. Observations are weighted with the inverse of the wage sampling probability to correct for the oversampling of municipal and larger preschool organizations in the wage sample

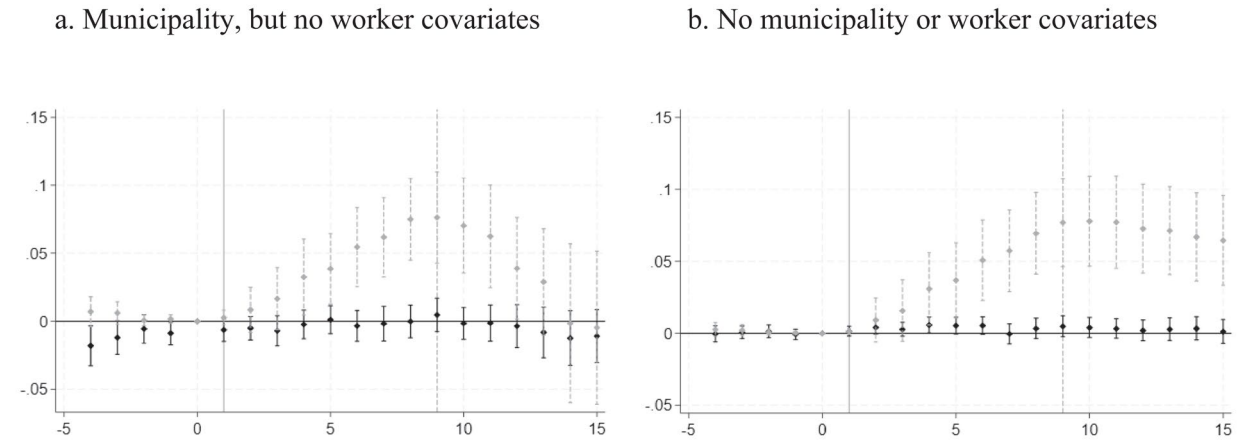

Fig. 19 Varying municipal and worker covariates. The grey dashed lines show estimates for the for-profit worker share. Observations are on worker level but are weighted with the inverse of the municipality-year number of workers. Municipality- and year fixed effects are included. Municipality covariates: 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

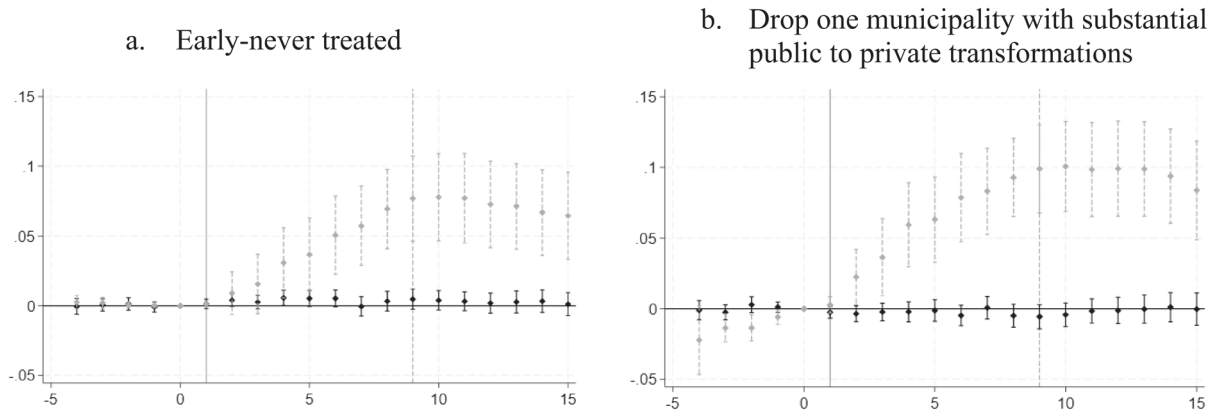

Fig. 20 Alternative control group definitions. The estimations include municipality and year and municipality fixed effects, but no covariates. Adding municipalities to the control group that were not treated with for-profits at any point, including 2014–2020). See also note to Fig. 3 in the main text. The figure is based on the same estimations as Fig. 3, with the exception that one municipality, with substantial public-to-private preschool transformations taking place during the studied period, have been excluded. See also note to Fig. 3 in the main text

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Author contributions
Both authors have contributed equally to all parts of the manuscript.

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Data availability
This paper makes use of official administrative data, based on registers held by Statistics Sweden, which we are not permitted to share. We can provide all the Stata 'do' files used to process and analyse the data. We can also guide researchers on how to obtain the data sets used in our analysis.

Declarations

Ethics approval
The project has been approved by the Swedish Ethical Review Authority (Dnr 2020–04676).

Competing interests
The authors declare that they have no competing interests.

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