A Free Lunch? How Changing Childcare Defaults Increases Parental Labor Supply

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Abstract

I analyze a policy reform that shifts public lunchtime childcare in kindergartens and schools in Zurich, Switzerland, from an opt-in to an opt-out system. In the new system, children are automatically signed up for low-cost lunchtime childcare unless parents actively deregister their child. Leveraging the staggered rollout in an event-study design, I document that the policy increases public childcare enrollment by on average 2.3 additional hours per week, compared to a baseline of 6.0 hours. The policy increases the employment rate of mothers with children in kindergarten by 4.8% and total earnings by 7.5%, and has a small positive effect on the extensive margin of fathers. The effects are particularly pronounced among low-income families, immigrant parents, and families with a male breadwinner model. Investigating the underlying mechanism, I document that parents react to the default component of the policy, suggesting that behavioral frictions play an important role in public childcare uptake.

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1 Introduction

The unequal division of childcare responsibilities between mothers and fathers is a central driver of gender inequality in the labor market (Goldin, 2014; Goldin et al., 2022; Kleven et al., 2019). Public childcare has the potential to increase maternal employment and narrow these gender gaps. Yet the existing literature finds that expansions in public childcare access and subsidies often yield only modest increases in maternal labor supply (Baker et al., 2008; Havnes and Mogstad, 2011; Kleven et al., 2024).

One possible explanation that has received little attention is that most public childcare systems rely on opt-in designs: children are not enrolled unless parents actively sign up. In standard household decision models, such institutional features should matter little, as fully informed and rational parents would select the option that maximizes their welfare regardless of the default. In contrast, insights from behavioral economics suggest that individuals often confront behavioral frictions in opt-in designs, and that default assignment can overcome these frictions (Chetty, 2015; Duflo, 2017).

This paper studies the effects of changing public childcare defaults using a unique natural experiment in Zurich, Switzerland: the introduction of a policy that automatically enrolls children in public lunchtime childcare at a low cost. Before the reform, public lunchtime childcare was available at all kindergartens and schools, but children were expected to return home or attend private childcare for lunch unless parents opted in. With the reform, children stay in public lunchtime childcare unless parents opt out. The reform also replaced income-based lunchtime fees with a uniform daily fee of CHF 6, lowering prices for high-income households but leaving costs unchanged for low-income families. In the analysis, I disentangle the default from the price change by leveraging information on subsidy eligibility prior to the reform. The policy was implemented through a staggered rollout across kindergartens and schools starting in 2016.

I combine rich administrative data to examine the effects of the policy on childcare usage and parental labor supply. I use records from Zurich's Department of Education that track public childcare enrollment by child, day of the week, and school-year for the universe of children in the public education system from 2011 to 2021. I link children to administrative

data on their parents' labor-market outcomes, allowing me to estimate effects on both the extensive and intensive margins for mothers and fathers. The main analysis covers 69,134 children and 74,435 parents.

I exploit the staggered rollout in a stacked event-study design that compares outcomes in adopting schools to clean control schools not exposed to the reform (Cengiz et al., 2019; Dube et al., 2025). A key concern for identification is parents' potential selection into reform schools. In Zurich, however, there is no school choice and children are allocated to schools based on their residence, limiting scope for parental sorting. Consistent with this institutional feature, I document no differential pre-trends in childcare enrollment or parental labor supply and no systematic changes in observable characteristics of children and parents at schools that adopted the reform. The results are robust to restricting the sample to families residing at the same address since the reform was announced.

The reform substantially increases public childcare usage. Lunchtime childcare enrollment rises by 22.3 percentage points, a 41.8% increase. Aggregating across all public childcare services, time spent in public childcare increases by 2.3 hours per week, relative to a baseline of 6.0 hours. Part of this increase is explained by behavioral spillovers: parents become more likely to enroll their children in after-school care on days when they are automatically enrolled in lunchtime childcare, even though after-school childcare remains opt-in and its price is unchanged. In addition, exposure of one child in a household increases public childcare enrollment among siblings attending non-adopting schools, consistent with the reform shifting household-level childcare choices.

These changes in public childcare usage translate into meaningful labor supply responses, concentrated among mothers of younger children. For parents of kindergarten-aged children, the reform increases employment on the extensive margin by 2.9 percentage points. Both mothers and fathers of children in this age group increase their labor supply on the extensive margin, by 3.7 percentage points and 2.0 percentage points, respectively. No significant effects are observed on the extensive margin for parents of older children. The policy also increases labor supply on the intensive margin among mothers with children in kindergarten and lower grades of primary school. They report higher monthly earnings driven by increased full-time equivalents and hours worked, without a corresponding rise in hourly wages. Com-

bining extensive and intensive margin effects, the policy increases annual earnings of mothers with children in kindergarten by CHF 2,971 on average from a baseline of CHF 39,000, while the effects on fathers are statistically not significant.

I then separate the default from the price change and find that the introduction of the opt-out default plays an independent and quantitatively important role. Low-income families or families receiving the full subsidy before the reform do not experience a reduction in lunchtime childcare prices which allows me to isolate the effect of the default change. In this group, the reform increases public childcare usage and raises labor supply on the extensive margin. Additional analyses further highlight the importance of the default change in the wider population. First, the post-reform distribution of lunchtime enrollment displays pronounced bunching at the new default option: a majority of children are enrolled in exactly the default number of lunchtime days, whereas before the reform many children were enrolled in none (the former default). Second, default adherence persists over time at the individual level. Even four years after initial exposure, children remain substantially more likely to be enrolled exactly at the default. Finally, the implied price elasticities vary sharply across income groups and are difficult to reconcile with a standard demand response to price alone.

What behavioral frictions could generate these effects? The gradual decline over time in the share of families remaining at the default suggests a role for adjustment frictions and inertia. At the same time, families are not fully passive: they actively adjust beyond the default by adding after-school care, enrolling siblings in non-reform schools in lunchtime childcare, and increasing their labor supply. Furthermore, the reform strongly increases public childcare usage and maternal labor supply among families with a high pre-reform reliance on home care, such as families with non-Swiss parents and male-breadwinner households. These patterns are consistent with defaults shifting perceived norms about children staying at school for lunch and, in turn, about maternal employment. Supporting this interpretation, I show that peer public childcare usage affects take-up prior to the reform, and that the reform has larger effects in classes with low baseline peer childcare usage, suggesting that the reform partly substitutes for these coordination patterns. By contrast, I find less support for a pure information mechanism: families with and without prior public childcare use respond similarly, suggesting that the reform's impact is not primarily driven by learning

about childcare availability or the process of how to enroll.

In the final part of the paper, I provide a stylized welfare analysis using the marginal value of public funds (MVPF) framework (Hendren and Sprung-Keyser, 2020). Combining estimated earnings impacts with administrative cost data, additional tax revenue offsets a meaningful share of reform costs, especially for kindergarteners. Under conservative assumptions about parental willingness to pay, the implied MVPF exceeds 1, with larger gains for families with younger children.

In summary, this paper demonstrates that default enrollment into public childcare can substantially increase both take-up and maternal labor supply. The findings imply that behavioral frictions play an important role in parents' childcare decisions and highlight the importance of policies that reduce such frictions, particularly for those households who stand to benefit the most in terms of labor force participation.

This paper contributes to two main strands of literature. First, research on the impact of subsidized public childcare has typically analyzed opt-in designs, in which children do not attend public childcare unless parents actively enroll them (Baker et al., 2008; Fitzpatrick, 2010, 2012; Havnes and Mogstad, 2011, 2015; Kottelenberg and Lehrer, 2017; Felfe and Lalive, 2018; Kleven et al., 2024).[1] This literature, reviewed by Olivetti and Petrongolo (2017) and Albanesi et al. (2023), finds modest effects of public childcare access on maternal labor supply. By documenting behavioral frictions to public childcare, my findings provide a new explanation for these muted responses and relate to recent studies that emphasize the role of beliefs and misperceptions around childcare and maternal labor supply (Boneva et al., 2024; Cortés et al., 2024; Costa-Ramón et al., 2026).

Second, the results link to a behavioral economics literature that has documented default effects across a range of domains, such as organ donation, exercise, or retirement savings (Abadie and Gay, 2006; Della Vigna and Malmendier, 2006; Chetty et al., 2014; Chetty, 2015; Duflo, 2017; Blumenstock et al., 2018; Shepard and Wagner, 2025). Chetty et al. (2014) show that defaults increase pension savings more effectively than subsidies because many individuals are passive savers and do not change their default savings rates. I contribute to

[1] Attanasio et al. (2022) and Humphries et al. (2024) focus on applicants to oversubscribed childcare programs.

this literature by demonstrating that default effects extend to childcare decisions — a high-stakes choice with immediately experienced consequences — and by showing that default assignment can induce subsequent active behavioral changes that are not directly shifted by default assignment, such as increases in parental labor supply.

The paper is organized as follows. Sections 2 and 3 introduce the data and institutional background. Section 4 outlines the empirical strategy, while Section 5 presents the empirical results on childcare enrollment, labor market outcomes, heterogeneity across individuals, and discussion of mechanisms. Section 6 provides a stylized welfare analysis, before the last section concludes.

2 Institutional Setting and Policy

In this section, I describe the institutional setting and the day-school policy.

School system In the city of Zurich, mandatory enrollment into the education system commences with kindergarten at the age of four. In the preschool years, parents can enroll their children in public or private daycare centers. Children spend two years in kindergarten, followed by six years of primary school, before progressing to lower secondary school for three years. Importantly, parental school choice does not exist. The Education Department assigns children to schools based on factors such as proximity to the child's residence, capacity constraints, and considerations of diversity.

Pre-reform childcare The city of Zurich offers universal public childcare in all kindergartens and schools. These services include childcare options before the school day commences (7-8 am), midday childcare during lunchtime hours (12-1:30 pm), and after-school childcare extending from 3:30 to 6 pm. Parents enroll their children in childcare services before the start of the academic year. Once enrolled, children stay in the same childcare services throughout the academic year.

Childcare fees differ by time block. Before-school childcare costs CHF 3 per day, while lunchtime childcare is priced at CHF 33, and after-school childcare at CHF 40. The city of Zurich provides subsidies to assist families in accessing lunchtime and after-school childcare

services. These subsidies are income-sensitive, contingent upon taxable household income, taxable wealth, and the number of children. With the full subsidy, parents pay CHF 4.5 for lunchtime and CHF 3 for after-school childcare per day. To access these subsidies, parents have to request an assessment with the city. Absent this subsidy request and assessment, parents bear the full costs for childcare services, irrespective of their income status (e.g., CHF 33 for lunchtime childcare).

Parents choose between public childcare, private external childcare or childcare at home during all hours that public childcare is available, both before and after the reform. In the pre-reform years, public childcare was characterized by an opt-in system: by default, children were expected to return home or to private external childcare for lunch unless parents took action and actively enrolled them in public childcare. Figure A1 shows that, in the pre-reform years 2011–2015, approximately 46% of parents enrolled their child in at least one public childcare service. Among non-users, the modal alternative was home care (37%), while 15% relied on private external childcare. Reliance on home care varies systematically by family background: low-income households, households with non-Swiss parents, and male-breadwinner households exhibit the highest pre-reform reliance on parental care at home.

Day-school policy With the day-school policy, the city of Zurich intended to increase the compatibility of family and work for parents. The day-school policy is characterized by several key features.

First, the policy entails automatic enrollment of children to remain at school during lunchtime on days when afternoon classes are scheduled, shifting lunchtime childcare from an opt-in to an opt-out system. The number of affected lunches increases with the grade level – ranging from two lunches in K2 to four lunches in grades 5-9. Children receive a warm meal and are looked after by school personnel during lunchtime. Importantly, while participation in lunchtime childcare is automatic, parents retain the option to opt out by notifying the school administration before the academic year commences (typically by email). If parents choose to opt out, the child is deregistered from all lunchtime childcare, i.e. parents cannot select specific days of the week at the flat lunchtime price. However, parents who opt out can still enroll their child in lunchtime childcare on selected days under the pre-reform fee

schedule. Figure A2 in the Appendix illustrates how the policy changes lunchtime childcare enrollment for children in the second kindergarten year.

Second, all families pay a daily fixed price of CHF 6 for lunchtime childcare. The financial impact, however, varies depending on family income and previous receipt of childcare subsidies. Low-income households already benefiting from subsidies before the reform witness either no change or a marginal increase in lunchtime childcare costs, while high-income families or those who did not apply for the subsidy before experience a reduction of up to CHF 27 in daily lunchtime childcare expenses. Figure A3 shows how parents' out-of-pocket cost for lunchtime childcare changes with the implementation of the day-school policy.

Finally, the day-school policy leaves public childcare supply unchanged. In pre-reform schools, children already have access to before-school, lunchtime, and after-school childcare services. The policy only automatically enrolls children in lunchtime childcare and changes the cost associated with lunchtime childcare. This contrasts with many previous studies that have analyzed expansions of public childcare services.

The day-school policy was first officially proposed in September 2014 as part of a project plan to introduce the policy in six schools. The Education Department selected schools of varying sizes and socio-economic backgrounds across school districts. Following this, each school district was invited to nominate additional schools for the policy's further implementation. Expecting an increase in lunchtime childcare usage after the reform, the Education Department prioritized schools with the physical infrastructure already in place to offer lunchtime childcare to all students.

The municipal council approved the reform for the initial six schools in March 2015. Expansion efforts continued with a new proposal in August 2017, and the policy received popular vote endorsement in June 2018, leading to its adoption in an additional 20 schools. The implementation occurred in phases: five schools adopted the policy in 2016, one in 2017, twelve in 2019, and eight more in 2020 and 2021.

3 Data and Outcomes

The analysis is based on combined data from several administrative registers. The data covers all parents and children in the public school system in the city of Zurich from 2011 to 2022.

School and childcare enrollment For the study of public childcare enrollment, I collaborate with the Education Department of the city of Zurich, which provided comprehensive register data on all pupils enrolled in public institutions within the city, from kindergarten entry (K1) through to the end of lower secondary school (grade 9). This data enables the tracking of each student's educational trajectory (school, grade) and includes detailed information on childcare enrollment, such as the type of childcare (before-school, lunchtime, or after-school), the specific days of service (e.g., Monday), and the childcare costs borne by parents after subsidies. Crucially, this data contains the social security numbers of the parents, allowing for linking multiple datasets on parents' backgrounds and labor market outcomes at the individual level. The primary childcare outcomes analyzed in this study are whether a child is enrolled in any lunchtime childcare and the number of days a child spends in lunchtime childcare per week.

The reform can substitute either childcare provided at home or external private childcare. Effects on parental labor supply are likely to be larger if the reform primarily replaces childcare at home, rather than private alternatives. However, the Education Department does not collect systematic data on the use of external private childcare. To address this limitation, I use data from the Swiss Labor Force Survey, which annually surveys a random sample of the Swiss population on their childcare arrangements. Within this survey, I am able to obtain information on parents' full childcare arrangements (public, external private, or at home) for 471 parents from my sample.

Labor market outcomes First, I access social security register information for the years 2011 to 2021. The social security register tracks all individual earnings from both wage employment and self-employment, as well as any federal benefits received for maternity leave, unemployment, disability, military service, and other sources. This information is recorded

for periods of varying lengths (ranging from one day to one year) within each calendar year. I aggregate these periods to create an annual record for each individual. This data is available for all 74,435 parents in the sample. I use the social security data to determine whether a parent had any earnings from wage or self-employment in a given year, and I also measure total annual earnings from wage or self-employment with values above the 97.5th percentile adjusted to this threshold.

Second, I employ the Swiss Earnings Structure Survey to analyze effects on the intensive margin of labor supply. Conducted biennially by the Swiss Federal Statistics Office, this survey samples private and public enterprises and administrations across Switzerland. The survey data covers 37% of all parent-year observations across six waves between 2011 and 2020. Using this data, I analyze parents' monthly income, monthly income adjusted for hours worked, and weekly hours worked.

Sample restrictions I restrict the sample in two ways. First, out of the 100 schools and kindergartens in the city of Zurich, I exclude five that already followed a day-school model before the reform. These schools offer childcare services that are not available at regular schools, such as the option to enroll children for a full day of childcare (before-school, lunchtime, after-school) at the cost of CHF 70. 4 out of these 5 schools adopted the city-wide day-school policy until 2022. To ensure a consistent comparison, I exclude all five schools from the sample. Second, starting from the population of parents in the administrative data of the Education Department, I remove parents for whom social security numbers are not available, who do not have an entry in the Swiss Population and Households Statistics, or who are younger than 20 years or older than 64 years, at which point retirement schemes begin.

Table 1 presents the summary statistics. The main analysis covers 69,134 children and 74,435 parents.

4 Empirical Strategy

4.1 Childcare enrollment

I conduct my analysis at the individual level, estimating whether the introduction of the day-school policy leads to an increase in the likelihood that student i at school s in school year t enrolls in public childcare. Recent econometric literature has identified potential issues with the two-way fixed effect estimator when treatment is staggered, treatment effects are heterogeneous and there are dynamic treatment effects over time (Goodman-Bacon, 2021; Sun and Abraham, 2021; De Chaisemartin and d'Haultfoeuille, 2020; Baker et al., 2022; Dube et al., 2025). To address these concerns, I employ a stacked event study approach similar to Cengiz et al. (2019) and Dube et al. (2025), which serves as my baseline specification.

I create 22 data sets, one for each school d introducing the day-school policy. The data sets include all students at school d introducing the policy and students from all clean control schools for an eight-year panel by event time. Clean control schools are those schools that belong to the same school district as treated school d and that do not introduce the day-school policy until three years after the respective adoption year of school d (the first year after the event window); other schools are dropped from data set d.

My baseline specification takes the following form:

$$Y_{istd} = \sum_{j=-5}^{2} \beta_j DaySchool^{j}_{std} + \mu_{sd} + \theta_{td} + \epsilon_{istd} \tag{1}$$

where Y_{istd} are childcare-enrollment outcomes of interest. The events in my setting represent a staggered, absorbing treatment, where schools adopt the day-school policy and remain treated. Out of the 95 schools and affiliated kindergartens, 22 schools introduce the policy until the end of school year 2021/22, while 73 schools remain untreated. $DaySchool^{j}_{std}$ is equal to 1 for school s j years from the introduction of the day-school policy and 0 otherwise. In the event studies, the omitted time period is $j = -1$, that is, the last year before the reform. μ_{sd} and θ_{td} represent school and year fixed effects, respectively. To address serial correlation in the error term ϵ_{istd}, I adjust standard errors for clustering at the school level (across data sets d). Taken together, my specification compares the probability of enrolling

in childcare in the years before and after the introduction of the day-school policy, in schools with and without the policy introduction.

After tracing out the event effects dynamically, I move to a static regression of the following form:

$$Y_{istd} = \beta_0 DaySchool_{std}^{(0,2)} + \mu_{sd} + \theta_{td} + \epsilon_{istd} \tag{2}$$

In this specification, my main parameter of interest is β_0 which takes a value of 1 for schools after the introduction of the day-school policy and 0 otherwise. The omitted comparison periods consist of the five pre-reform years contained in each data set d.

4.2 Labor market outcomes

To analyze the impact of the day-school policy on parents' labor market outcomes, I apply a similar methodology as used in my analysis of childcare enrollment, relying on the event study specifications outlined in equations (1) and (2). For this part of the analysis, I first identify the parents of each child included in the dataset. Then, I estimate whether the introduction of the day-school policy influences labor market outcomes – such as employment status, hours worked, and earnings – for parents whose child is attending school s during school year t. Since labor market data is available until the school year 2020/21, this analysis focuses on the 20 schools introducing the policy until the end of school year 2021/22.

Given that a parent may have multiple children enrolled in school during the same year, which would lead to multiple observations for the same parent, I adjust for this by weighing each parent-year observation by the inverse number of children attending school that year. This weighting ensures that each parent contributes equally to the analysis, regardless of the number of school-enrolled children they have. To ensure that my findings are not driven by the weighting approach, I replicate my results for the subset of parents with only one child enrolled in the public school system.

4.3 Threats to identification

My specification relies on two identification assumptions. First, students, parents, and schools should not respond in anticipation of the day-school policy (*no anticipation*). Second, the specification requires that after controlling for school and year fixed effects, childcare enrollment and parental labor supply in schools with introduction of the day-school policy and in schools without introduction would move in parallel in the absence of the introduction of the policy (*parallel trends*). A potential violation of this assumption would occur if the introduction of the day-school policy was systematically correlated across schools with other changes affecting the probability of childcare enrollment or parental labor supply.

One might be concerned that the selection of schools that introduce the day-school policy is endogenously driven by dynamics of local demand. An increase in childcare demand at specific schools may prompt the Education Department to introduce the day-school policy at these schools. Another concern is that the composition of parents and students at schools that introduce the day-school policy changes with the introduction of the policy, i.e. parents with a preference for public childcare services select into treated schools by 1. moving into the catchment areas of these schools or 2. petition the Education Department to be reassigned to a different school once the school assignment is communicated.

To investigate potential violations of the identification assumptions, I proceed in three steps. First, I use the event study specification to verify the absence of differential trends in childcare enrollment or labor market participation probabilities across schools before the introduction of the day-school policy. The absence of such trends supports the notion that the Education Department selected schools based on predetermined factors, rather than in response to changing parental demand for public childcare. Second, I document that the composition of students based on observables does not change with the introduction of the day-school policy. Figure A4 shows that none of the effects is statistically significant. Finally, in order to address the concern that parents move into catchment areas of the schools introducing the day-school policy, I replicate my main results for the subsample of families who live at the same address as in 2014, the first year the policy was officially announced.

5 Results

5.1 Childcare enrollment

Any lunchtime childcare Figure 1 compares whether a child is enrolled in any lunchtime childcare pre- and post-implementation of the day-school policy across schools with and without the reform. The graph on the left displays the raw data, with schools organized by the year of policy introduction (see also Figure A6). The graph on the right combines this data into an event study.

As previously mentioned, one might be concerned that the Education Department selected schools for the reform endogenously in response to rising childcare demand at these schools. Reassuringly, however, Figure 1 shows that there is no significant difference in lunchtime enrollment rates between schools that later adopt the policy and those that do not in the period leading up to the policy's introduction, with none of the event-study coefficients being statistically significant. This suggests that the parallel trends assumption holds.

Immediately after the policy introduction, the enrollment rate into lunchtime childcare increases by 22.3 percentage points (p-value: 0.000), representing a substantial increase of 41.8%. The effects endure in subsequent years.

Days in lunchtime childcare Table 2, column 1 presents the results for the static DiD specification as outlined in equation (2). The results indicate that, following the implementation of the day-school policy, children spend 1.127 (p-value: 0.000) additional days in lunchtime childcare, which corresponds to a 69.1% increase relative to the baseline.

This effect is driven by an increase in days in lunchtime childcare at both the extensive and intensive margins, with slightly stronger effects on the extensive margin. In the year before the implementation of the policy, 54.6% of the children were enrolled in at least one childcare service at the schools that later introduced the policy. As shown in Table 2 column 2 and 3, children previously enrolled in public childcare increase their days in lunchtime by 0.94 in the first year after the reform (p-value: 0.000), while those without prior childcare enrollment stay 1.79 additional days in lunchtime childcare (p-value: 0.000).

Across all childcare services, the number of hours children spend in public childcare increases by 2.3 hours per week (p-value: 0.000) or 37.9% relative to the baseline. Figure A5 replicates the event-study graph for the weekly hours children spend in public childcare.

Robustness Table 2 demonstrates the robustness of the findings. First, we might be worried that the results are driven by the self-selection of schools into the reform. To address this concern, Model (4) uses the 15 schools that introduce the day-school policy between 2022 and 2024, right after the period of observation, as control schools. The results do not change. Second, another concern is that parents might move into the catchment areas of schools that introduce the day-school policy. Model (5) restricts the sample to families residing at the same address as in 2014 when the day-school policy was initially proposed. I find a positive effect of 1.27 additional hours per week in childcare (p-value: 0.000).

5.2 Labor supply response

My results so far show that public childcare usage increases at schools that introduce the day-school policy. In this section, I investigate whether the introduction of the day-school policy affects parental labor supply.

Results based on social security data Figure 2 shows the effect of the day-school policy on the share of parents with any earnings, using the stacked event study regression as described in equation (1). The regressions pool mothers and fathers together but are estimated separately by grades (K1-2, Grade 1-4, Grade 5-9). The regressions exploit the introduction of the day-school policy in 20 schools and kindergartens between 2016 and 2020.

Prior to the introduction, the difference in employment is close to zero and none of the event-study coefficients is significant, further alleviating concerns that the Education Department selected schools for the reform endogenously in response to the evolution of labor market outcomes at these schools. After the policy introduction, parents with children in kindergarten increase their employment rate by 2.9 percentage points (p-value: 0.02) or 3.4%. There is no effect on the extensive labor supply of parents with children in higher grades.

Table 3 investigates labor supply responses to the day-school policy more closely and splits the sample additionally by parent gender. Mothers with children in kindergarten increase their extensive labor supply by 3.7 percentage points (p-value: 0.04, +4.8%) in response to the policy. The effect on fathers is 2.0 percentage points (p-value: 0.007, +2.2%). For mothers, the introduction of the day-school policy also translates into significantly higher average earnings. Mothers with children in kindergarten earn on average CHF 2,971 more from a baseline of CHF 39,000, while the effects on fathers are statistically not significant. Figures A8 and A9 in the Appendix show the dynamic event studies corresponding to these results.

The 3.7 percentage point increase in maternal labor market participation is large compared to estimates from previous studies on the effects of public childcare. While Baker et al. (2008) find comparable employment effects in the context of Quebec's childcare expansion, most other studies report more limited impacts. For example, Havnes and Mogstad (2015) document a 1.1 percentage point increase in maternal employment following the expansion of state-funded childcare places in Norway in 1975. Similarly, Kleven et al. (2024) find no effect of the expansion of preschool care on maternal labor-force participation in Austria.

To further contextualize the result, this study differs from prior work in at least two important ways – both of which should make it less likely to detect any effect. First, while previous studies typically examine childcare introductions or expansions, the reform I study leaves overall public childcare supply unchanged. Even before the reform, parents had the option to use public childcare in all schools and kindergartens during the work day. Second, whereas earlier research has focused primarily on early childcare for very young children, I examine a policy targeting children in kindergarten or older, when childcare responsibilities are likely to be already less constraining for parents (Goldin et al., 2022).

Results based on Swiss Earnings Structure Survey Table 4 investigates labor outcomes based on the Swiss Earnings Structure Survey. I find that mothers with children from K1 to Grade 4 have higher monthly earnings. What is driving the increase in earnings? In contrast to the social-security data, the Earnings Structure survey also captures hours worked, and monthly earnings standardized by hours. While I do not find a significant effect

on earnings per hour, Table 4 shows that the higher earnings can be related to more hours worked. Mothers with children in kindergarten work 2.078 hours more per week or 9.0%, while those with children in Grades 1 to 4 work 1.15 hours more (5.0%). I do not find any intensive margin effects on fathers.

Robustness Table A1 demonstrates the robustness of the findings to alternative specifications similar to the ones used for childcare usage. Model (4) restricts the sample to parents who only have one child enrolled in the public school system in a given year. Model (5) only uses later adopting schools as control schools. Model (6) only keeps the families in the sample that live at the same address as in the year 2014. The results remain unchanged.

5.3 Heterogeneity

My results indicate that parents with children in kindergarten increase their labor supply on the extensive margin. Mothers with children in lower grades also respond on the intensive margin. In this section, I analyze how different types of households react to the day-school policy.

I define a household as the set of parents registered for a given child in the Education Department data. I classify households along four dimensions. m households in which both parents were working.

I distinguish households along several dimensions. A household is defined as the set of parents registered as parents of a common child in the data provided by the Education Department. First, I construct a measure of household earnings using social security records. Low-income households are more likely to be affected exclusively by the default change, whereas high-income households additionally benefit from the reduction in lunchtime childcare costs of the reform. I group households into low-, middle-, and high-income categories based on income terciles in a given school year. I measure household earnings two to three years prior to year of observation. The results are robust to alternative ways of measuring household earnings, such as measuring earnings in the year before kindergarten entry. Second, I classify households by parental birthplace. I distinguish between households with (i) at least one Swiss-born parent, and (ii) no Swiss-born parent. This information is obtained

from the Swiss Population and Households Statistics, which includes data on all individuals residing in Switzerland.

In addition, I use two further variables to distinguish households. First, I distinguish single-parent from two-parent households using the number of parents registered with the Education Department and parents' residential addresses. A household is classified as single-parent if only one parent is registered for the child, or if two parents are registered but reside at different addresses and the child resides with one parent; otherwise, it is classified as two-parent. Second, I classify households by breadwinner arrangement using wage and self-employment earnings two to three years prior to the outcome year, distinguishing male-breadwinner households (father has earnings, mother has zero earnings) from dual-earner households (both parents have earnings).

I find that the day-school policy increases childcare usage across family backgrounds, while labor supply responses on the extensive margin are concentrated among families with lower baseline labor market participation. Figure 3 presents the effects of the policy on childcare enrollment by household type. Public lunchtime childcare enrollment increases for every group, including those that typically make little use of public childcare, such as two-parent households and households with a non-working mother. For the latter group, the number of days per week that a child spends in lunchtime childcare rises by 1.01 (p-value: 0.000), an 85.6 percent increase relative to the baseline of 1.18 days.

In contrast, labor supply responses on the extensive margin are concentrated among groups with lower baseline participation, including low-income households, families with parents born outside Switzerland, and households with a more traditional division of labor. For low-income parents, labor force participation increases by 6.1 percentage points (p-value: 0.003). I find similar magnitudes for parents born outside Switzerland and for mothers in households with in male-breadwinner household. Finally, both single-parent and two-parent households increase their labor force participation in response to the reform.

These are precisely the households for whom the counterfactual arrangement is more likely to be home care rather than external private care. Prior studies have documented that public childcare can crowd out informal childcare arrangements (Havnes and Mogstad, 2011; Attanasio et al., 2022; Kleven et al., 2024). To characterize the alternative care arrangements

used before the reform, I link parents in my main sample to the Swiss Labor Force Survey, which surveys a random subset of the Swiss population annually. Using social-security identifiers, I match 471 parents from pre-reform schools in the main sample.

Figure A1 shows that for low-income families the primary alternative to public childcare is home care: 43.4% rely on home care pre-reform, compared to 22.0% among high-income families. Similarly, pre-reform reliance on home care is high among households with non-Swiss parents (45.5%) and among male-breadwinner households (56.6%).

5.4 Default change vs. price change

In this part, I investigate heterogeneity across households that were affected by to different degrees by the cost-reduction to better understand what is driving the impact of the policy.

The day-school policy combines two changes: it automatically enrolls children in lunchtime childcare and it sets a uniform daily fee of CHF 6, independent of household income and regardless of whether parents request a subsidy assessment.

To disentangle the effects of automatic enrollment and cost reduction, I exploit heterogeneity in how the reform changed out-of-pocket costs. I restrict the sample to families with children in kindergarten to Grade 4, since labor supply responses are concentrated among parents with younger children. The subsidy assessment allows to calculate the change in lunchtime childcare cost that parents experience. For families that received the full subsidy before the reform, the out-of-pocket cost for lunchtime childcare increases or does not change, allowing me to isolate the impact of the default enrollment. In contrast, families with a partial subsidy before the reform experience both the default change and a reduction in lunchtime childcare costs. I estimate regressions similar to equation (2) by cost-change quintile.

Households affected by auto enrollment but experiencing no change or an increase in lunchtime childcare cost increase lunchtime childcare by 0.48 days per week (p-value: 0.000), as shown in Figure 4. Focusing on this subset of families provides a clean test of the impact of automatic childcare enrollment. I provide two additional pieces of evidence that further highlight the importance of the default policy feature in the broader study population.

First, I find that estimated price elasticities across subsidy quintiles vary substantially

and are difficult to reconcile with a response to prices alone. Figure 4 shows that families that experience a larger reduction in lunchtime childcare cost increase their use of lunchtime childcare to a larger extent. The quintile of children that experiences an average cost reduction of CHF 17.1 spend additional 1.1 days in lunchtime childcare per week. This translates into an elasticity of lunchtime childcare use with respect to its price of -0.67, larger than the elasticity reported in Baker et al. (2008) on the introduction of heavily subsidized childcare in Canada. The implied elasticities changes substantially when moving along the cost-change distribution: the elasticity increases to -1.2 for the quintile experiencing a small reduction, before jumping discontinuously to +0.73 for the quintile of parents experiencing a small increase in the price. Finally, the quintile experiencing the highest increase in lunchtime childcare cost has an elasticity of 0.42. For this latter quintile, parents that experience a 10 percent increase in price increase their lunchtime childcare use by 4.2 percent per week.

What could explain this pattern? Figure 5 shows descriptively that most parents keep their children enrolled in the number of childcare slots that correspond precisely to the new automatic enrollment default in a grade. Depending on grade and the number of school days with afternoon classes, the day-school policy affects a different number of lunchtime childcare slots. Starting in K2 when children have two days with afternoon classes, children are automatically enrolled in two lunchtime childcare slots, increasing to three slots from Grades 1 to 4, and to four slots for Grade 5 and above. Before the reform, 50.2% of children were not enrolled in any lunchtime childcare, the former default. After the reform, 52.3% of children from K2 to the end of lower II attend the precise number of automatic childcare slots, while 84.9% attend either the default number or more days in lunchtime childcare.

5.5 Mechanisms

In standard household choice models with fully informed parents and no adjustment costs, default assignment should not affect childcare choices or labor supply: parents would choose the same allocation whether public childcare enrollment is opt-in or opt-out (Albanesi et al., 2023; Olivetti et al., 2024). Behavioral economics, in contrast, emphasizes that defaults can matter when households face frictions (Chetty, 2015; Duflo, 2017). The large effects of opt-out enrollment on public childcare usage and parental labor supply documented above sug-

gest that such frictions are empirically important in this setting. This section discusses three candidate channels—inertia, norms/coordination, and information—and presents suggestive evidence most consistent with a combination of inertia and norms/coordination mechanisms rather than a pure information explanation.

Inertia As expected by inertial behavior and adjustment costs, public lunchtime childcare enrollment exhibits substantial persistence at the default at the individual level, and slow adjustment away from it. Figure 5 presents a stacked event-study that follows children for up to nine years and focuses on cohorts exposed to the day-school policy for at least four years. Treated children are matched to control children from the same grade who are not exposed to the policy until after the end of the analysis window. The outcome is an indicator for whether a child is enrolled in the default number of lunchtime slots. In the first year after exposure, children are 52.0 percentage points more likely to be enrolled at the default; four years after exposure, they remain 35.0 percentage points more likely to be enrolled at the default. Importantly, while default adherence declines over time, the reform's effect on total weekly childcare hours remains stable, with some parents increasing childcare beyond the default, others reducing it (right panel of Figure 5).

Active responses At the same time, parents are not fully passive. The reform triggers immediate active responses that go beyond automatic lunchtime enrollment. Most clearly, parents change their labor supply. Moreover, parents adjust along childcare margins that the reform did not directly change.

Because the regular school day ends at 3:30pm, households that expand labor supply may require additional afternoon coverage. Crucially, the reform did not change either the enrollment default or the price of after-school childcare. Table 5 reports impacts on lunchtime and after-school childcare enrollment by weekday. Lunchtime estimates mirror the policy design: enrollment rises on days with automatic lunchtime enrollment (Monday, Tuesday, Thursday, Friday, see the K2 schedule in Figure A2) and remains unchanged on Wednesday, the only weekday never affected by the reform. Turning to after-school childcare, I find modest but statistically significant increases on Monday and Friday, concentrated among children in

kindergarten and the first years of primary school. In addition, I document spillovers within households: families increase childcare use for other children even when those children attend non-reform schools (see also Table 5). Taken together, these patterns indicate that opt-out enrollment shifts households' broader childcare-work arrangements beyond what we would expect from adjustment frictions and inertial behavior.

Norms and coordination A second set of patterns points to norm- and coordination-related mechanisms. Extensive-margin labor-supply responses are strongest among families with high pre-reform reliance on home care, such as low-income households, families with non-Swiss parents, and male-breadwinner households, consistent with opt-out enrollment shifting perceived norms about children staying at school for lunch and maternal employment.

To further probe a coordination-based interpretation, I study whether peer childcare behavior predicts individual take-up prior to the reform and whether the reform operates differently in peer environments with low versus high baseline childcare usage. A growing literature emphasizes peer behavior as an important channel for norm changes (Albrecht et al., 2024; Fernández, 2025). For example, Dahl et al. (2014) show that exogenous increases in fathers' parental leave take-up raise take-up among coworkers and brothers, with peer effects propagating over time.

Motivated by this literature, I implement a Hoxby-style peer-effects strategy that exploits idiosyncratic cohort-to-cohort variation in the share of peers with prior lunchtime childcare use within school units, controlling for school-unit fixed effects, cohort fixed effects, school-unit-specific time trends, and the student's own prior lunchtime childcare usage. I focus on peer composition in Grade 1 of primary school, when students enter new school and class environments after kindergarten, and examine how Grade-1 peer childcare histories affects individual lunchtime childcare enrollment in Grade 3. I find economically meaningful peer effects: a 10 percentage point increase in the share of peers with prior lunchtime childcare usage increases a student's probability of enrolling in lunchtime childcare in Grade 3 by 1.6 percentage points. I then show that the reform interacts strongly with baseline peer environments: Figure 7 shows that in classes with low pre-reform peer childcare usage,

the reform increases Grade-3 lunchtime childcare usage by 42 percentage points, whereas in classes with high baseline peer usage the effect is 16 percentage points. Together, the results suggest that coordination plays an important role for public childcare uptake in the pre-reform context and the reform partly substitutes for this channel.

Information Finally, an information mechanism receives limited support. If opt-out enrollment primarily operates by informing families about the existence of public childcare or the enrollment process, effects should be strongest among families with no prior public childcare use. Instead, families with and without prior public childcare use respond similarly to the reform. Moreover, families that received the full subsidy before the reform, increase both childcare enrollment and labor supply on the extensive margin (Figure 4).

6 Welfare Analysis

I follow the approach developed by Hendren and Sprung-Keyser (2020) and compute the marginal value of public funds (MVPF) of the day-school policy reform for parents of 1. children across all grades 2. children in kindergarten. This exercise allows me to assess the welfare effects of the policy both overall and specifically for those families that have the strongest labor supply response to the policy.

The MVPF is defined as the ratio of marginal benefit to marginal cost, where the marginal benefit is measured in terms of individual willingness to pay (WTP) and the marginal cost is given by the difference between the overall cost of the policy and any fiscal externality that it generates. Following Hendren and Sprung-Keyser (2020), the MVPF is calculated as:

$$MVPF = \frac{\text{WTP}}{\text{Mechanical Costs - Fiscal externality}}$$

The mechanical cost of the day-school policy is CHF 1,455 per child per year. This amount is calculated using data provided by the Education Department of the city of Zurich on the costs incurred by the 18 schools that introduced the policy by 2020. The total yearly additional cost for these 18 schools due to the day-school policy amounts to CHF 6.7 million, broken down into CHF 2.0 million in setup costs (infrastructure changes, administrative ex-

penses, project management), CHF 0.4 million in salary costs for school leadership to manage the new lunchtime organization, and CHF 4.3 million in direct childcare costs (personnel, food, rent, and additional subsidies). The direct childcare cost are net of the price parents pay for public childcare. To compute the cost per child, I divide the total yearly cost by the average number of students at these schools between 2018 and 2020.

The overall fiscal externality is CHF 145 per child and CHF 579 per child in kindergarten. I calculate the marginal tax rate on the additional earnings generated by the reform using tax information provided by the Swiss tax authorities (FTA, 2024). To arrive at the marginal tax rate, I use average values observed in the sample for mothers with children in kindergarten. Based on these assumptions, I estimate a marginal tax rate of 19.5% on the additional earnings. As previously shown, the day-school policy increases mothers' earnings by CHF 2,971 for children in kindergarten. With a marginal tax rate of 19.5%, this results in a fiscal externality of CHF 579 per child in kindergarten. In contrast, the fiscal externality is 0 for children beyond kindergarten since there are no significant parental earnings effects for this age group. Correcting the fiscal externality by the share of kindergarten children in the data, the overall fiscal externality per child is CHF 145. Combined with the mechanical cost, this yields overall marginal costs of CHF 1,310 per child, and CHF 876 per child in kindergarten.

Following Humphries et al. (2024), an important conceptual question for the welfare analysis is whether parents' earnings gains should be included in WTP for the reform. If parents choose labor supply optimally, the benefits of higher earnings are fully offset by the marginal disutility of work, so earnings gains should not be included in the WTP. By contrast, if parents face behavioral barriers or other frictions that prevent them from choosing their preferred work–childcare bundle, then increases in earnings can reflect the value of relaxing those constraints and should be included in WTP. In what follows, I present results for both approaches.

Assuming that the utility of higher earnings are fully offset and parents value the reform at the net cost of provision, I calculate an overall MVPF 1.1. This implies that each additional CHF 1 spent on public childcare for children across all grades generates CHF 1.1. Focusing specifically on children in kindergarten, the MVPF increases to 1.66. Assuming that parents value the reform at their earnings gains, the MVPF increases to 3.4.

Together, these results suggest that while the reform yields more modest returns on average, it generates substantially larger welfare gains for parents of younger children.

This welfare analysis has two caveats. First, it does not account for the effect of the policy on children. Second, the relevant mechanical cost of the policy is likely lower. The aggregate childcare costs of CHF 4.3 million (out of CHF 6.7m) used in the calculation include the additional subsidies necessary to reduce the lunchtime childcare cost to a fixed price of CHF 6 for all families. However, my analysis shows that the default component drives the effects of the policy, in particular for those parents who adjust their labor supply on the extensive margin.

7 Conclusion

This paper provides evidence on the impact of public childcare policies on parental labor supply, focusing on a reform in Zurich, Switzerland that automatically enrolled children in low-cost public lunchtime childcare. The introduction of this policy iled to a significant increase in childcare usage and increased parental labor supply among mothers with kindergarten-aged children. The effects are more pronounced among low-income families, immigrant parents, and families initially following a male breadwinner model. Investigating the underlying mechanisms, I document that the default component of the policy impacts both childcare usage and parental labor supply. These findings suggest that parents face behavioral frictions when deciding whether to enroll their children in public childcare and that reducing these frictions, such as shifting public childcare from an opt-in to an opt-out system, can effectively increase public childcare take up and parental labor supply.

The insights from this study contribute to the broader literature on the impact of public childcare on parental labor supply. As governments seek to design policies that facilitate labor market participation for parents, the results underscore the value of policy features that account for potential behavioral frictions to public childcare usage that parents may face.

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Figures

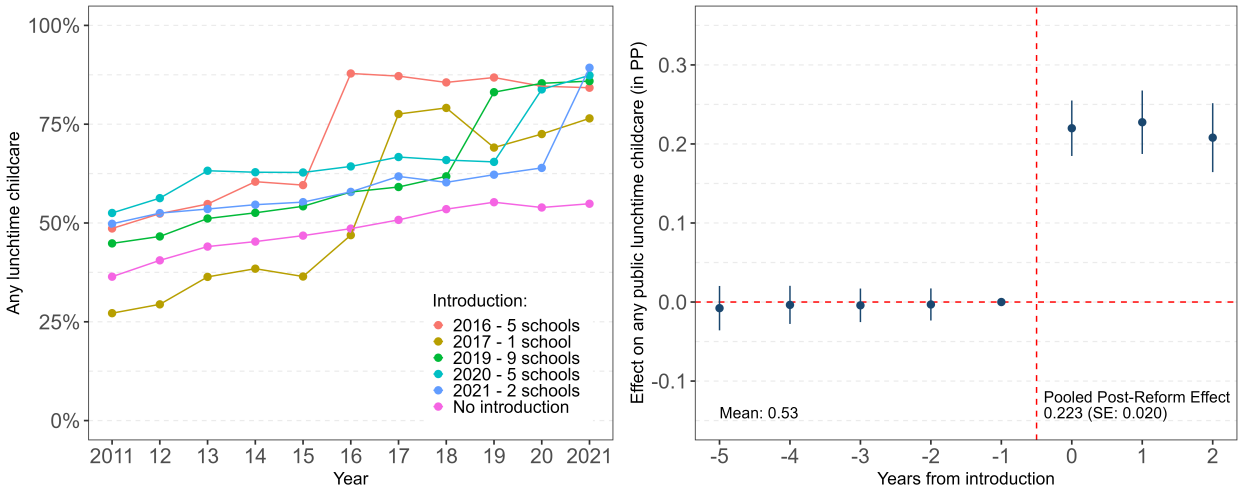


Figure 1: Impact of the Day-School Policy on Enrollment in Public Lunchtime Childcare

Notes: The figure shows the effect of the day-school policy on the share of students enrolling in any public lunchtime childcare. The left panel plots the raw data, grouped by introduction year of the policy. The right panel displays the effect of the policy using a stacked event study regression as described in equation (1), controlling for school and year fixed effects. The regression exploits the introduction of the day-school policy in 22 schools and kindergartens between 2016 and 2022. The sample mean is 53.4%. 95% confidence intervals (vertical lines) based on standard errors clustered at the school level are displayed.

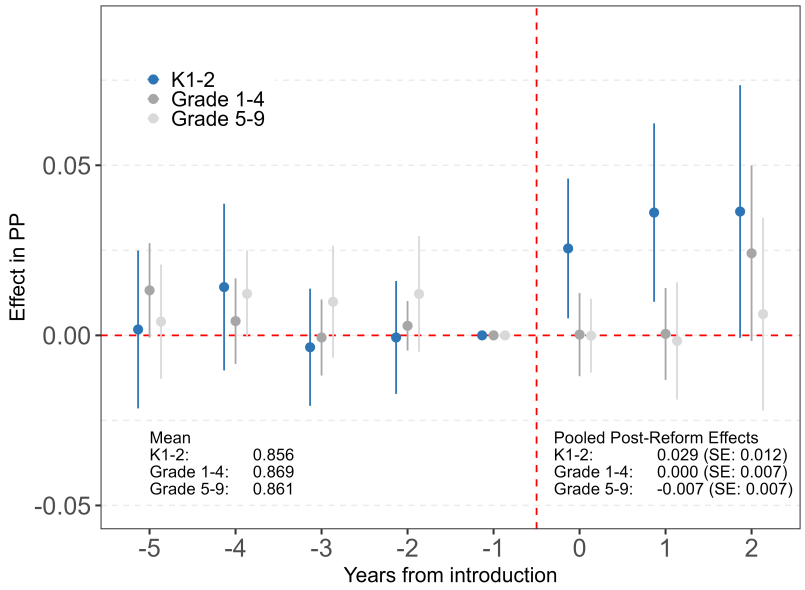


Figure 2: Impact of the Day-School Policy on Parents' Extensive Labor Supply by Grade

Notes: The figure shows the effect of the day-school policy on the share of parents with any earnings, using the stacked event study regression as described in equation (1). The regressions are estimated separately by grades (K1-2, Grade 1-4, Grade 5-9). The regressions exploit the introduction of the day-school policy in 20 schools and kindergartens between 2016 and 2020. The sample means for the respective regressions are 85.6% (K1-2), 86.9% (Grade 1-4), and 86.1% (Grade 5-9). 95% confidence intervals (vertical lines) based on standard errors clustered at the school level are displayed.

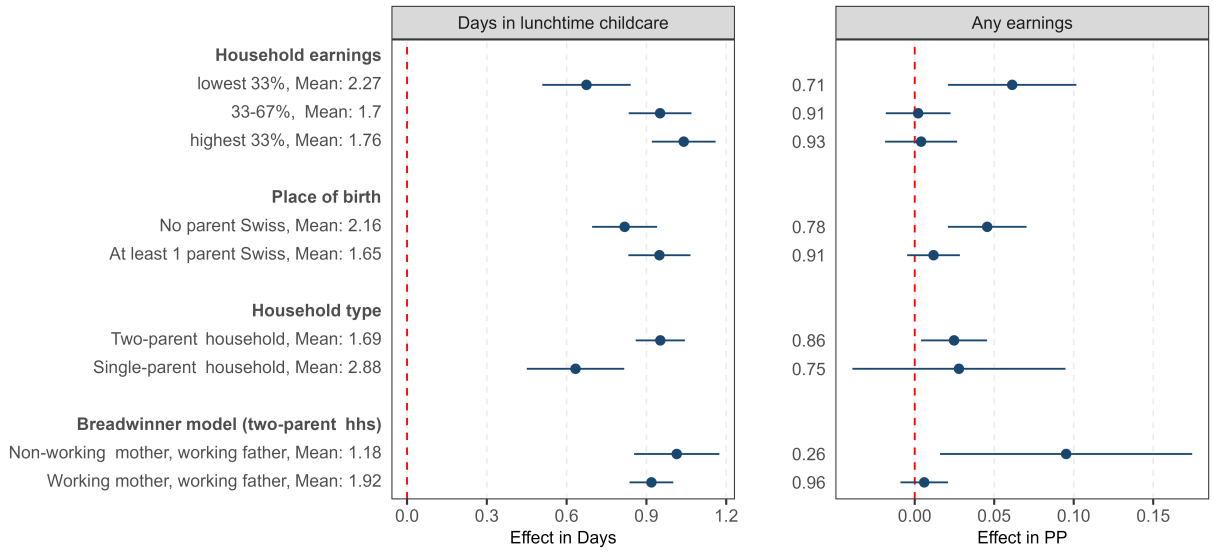


Figure 3: Impact of the Day-School Policy by Family Type

Notes: The figure shows the effect of the day-school policy across different family types. The sample consists of children in kindergarten and their parents. The left panel displays the effect of the policy on the likelihood that a child is enrolled in any lunchtime childcare. The right panel plots the effect of the policy on the likelihood that a parent has any earnings in a year. Household earnings refer to all earnings from wage and self-employment 2 to 3 years before the year of observation. Similarly, breadwinner model is measured 2-3 years before. For the labor market effects by breadwinner model, only outcomes for mothers are shown. The effects are estimated using a stacked event study regression as described in equation (2), controlling for school and year fixed effects. The regression exploits the introduction of the day-school policy in 22 schools and kindergartens between 2016 and 2022 for the childcare results and in 20 schools and kindergartens between 2016 and 2020 for labor-supply results. 95% confidence intervals (horizontal lines) based on standard errors clustered at the school level are displayed.

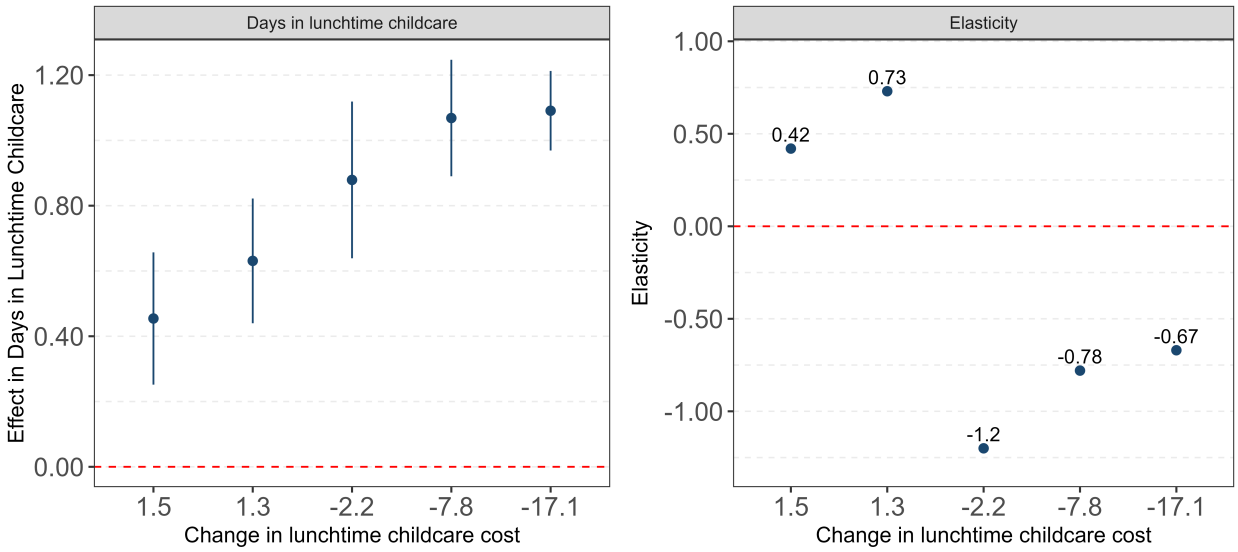


Figure 4: Impact of the Day-School Policy By Cost-Change Quintile

Notes: The figure shows the effect of the day-school policy for families that are affected by the default change and experience different changes in lunchtime childcare cost. The sample consists of children who attend grades K1 to Grade 4 and who had a subsidy assessment in the years before the reform. The subsidy assessment allows to calculate the change in lunchtime childcare cost that the parents experience. The figure splits the sample by cost-change quintile. The left panel plots the effect of the policy on the number of days that a child is enrolled in lunchtime childcare. The right panel plots the corresponding elasticities of lunchtime childcare use with respect to its price. The effects are estimated using a stacked event study regression as described in equation (2), controlling for school and year fixed effects. The regressions exploit the introduction of the day-school policy in 22 schools and kindergartens between 2016 and 2022. 95% confidence intervals (vertical lines) based on standard errors clustered at the school level are displayed.

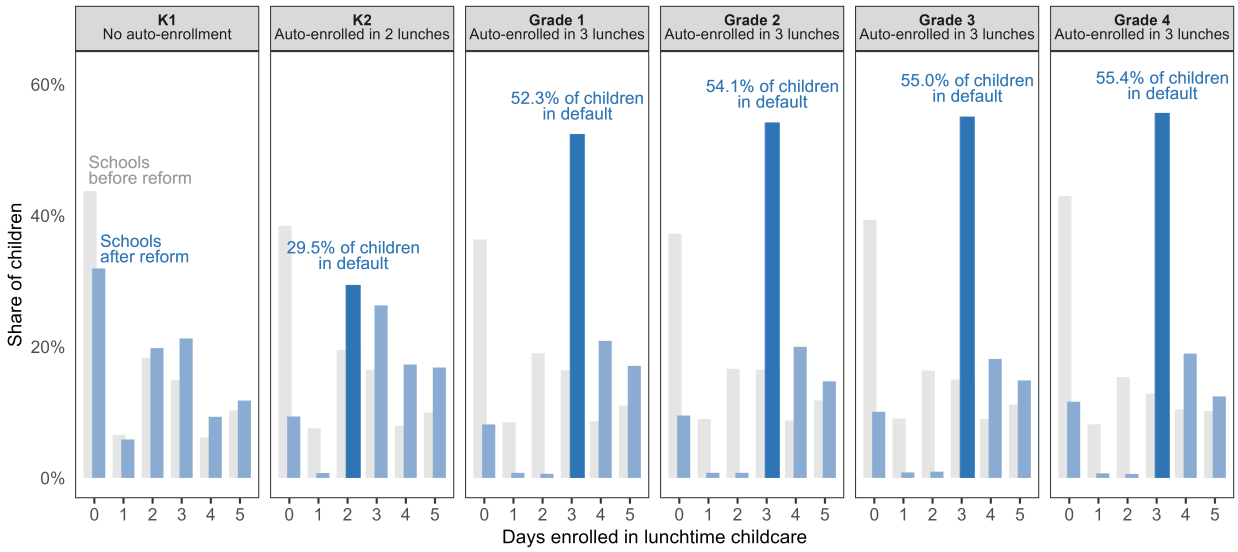


Figure 5: Share of Children Enrolled in Default of Lunchtime Childcare Slots

Notes: The figure presents raw data on the number of lunchtime childcare slots children are enrolled in, by grade. The sample includes all children who attended any of the 22 schools and kindergartens that introduced the day-school policy until 2022. The day-school reform shifts lunchtime childcare enrollment from an opt-in to an opt-out system. With the reform, children are automatically enrolled in public lunchtime childcare on those school days with afternoon classes unless parents opt out. Auto-enrollment begins in K2 with two lunchtime-childcare slots, increasing to three slots from Grades 1 to 4. On average, 52.3% of children remain enrolled in the default number of slots after the reform, while 32.6% enroll in additional slots and 15.1% enroll in fewer. Before the reform, 50.2% of children did not use any lunchtime childcare, the former default.

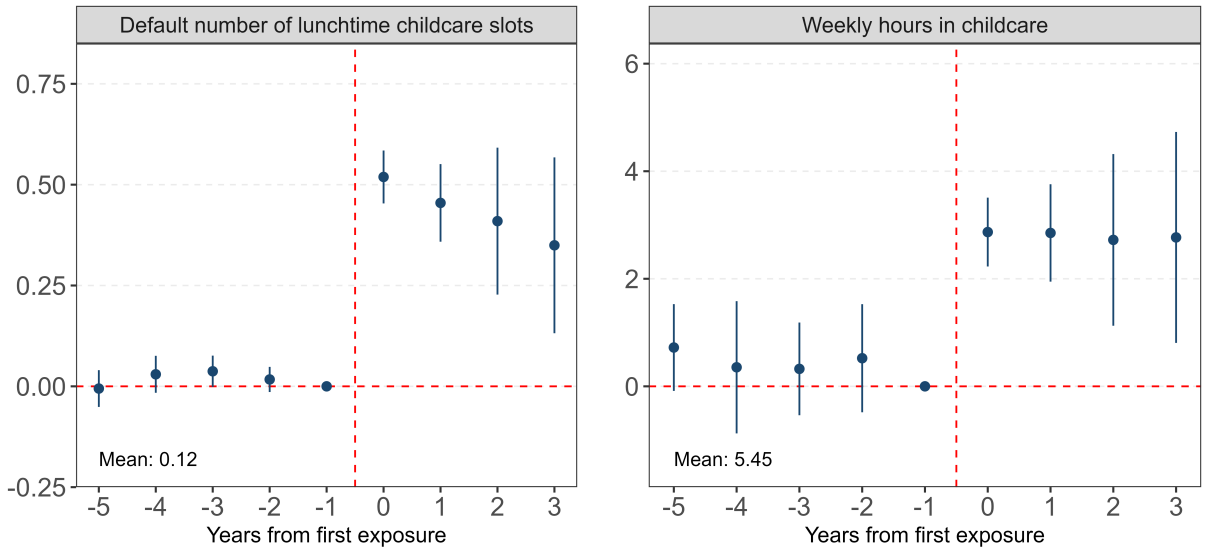


Figure 6: Children Remain Enrolled in Default Number of Days in Lunchtime Childcare

Notes: In the left panel, the figure shows the effect of the day-school policy on a dummy variable indicating whether a student is enrolled in the number of lunchtime childcare slots that corresponds to the new default; the right shows the effect on weekly hours in childcare. Both panels are based on a stacked event study regression on the individual level, controlling for child and year fixed effects. The regression exploits the introduction of the day-school policy in 22 schools and kindergartens between 2016 and 2022. The sample consists of all treated children that can be observed from 5 years before to 4 years after first exposure to the day-school policy, plus all control children that can be observed over 9 years and who attend the same grades but are not exposed to the day-school policy. 95% confidence intervals (vertical lines) based on standard errors clustered at the school level are displayed.

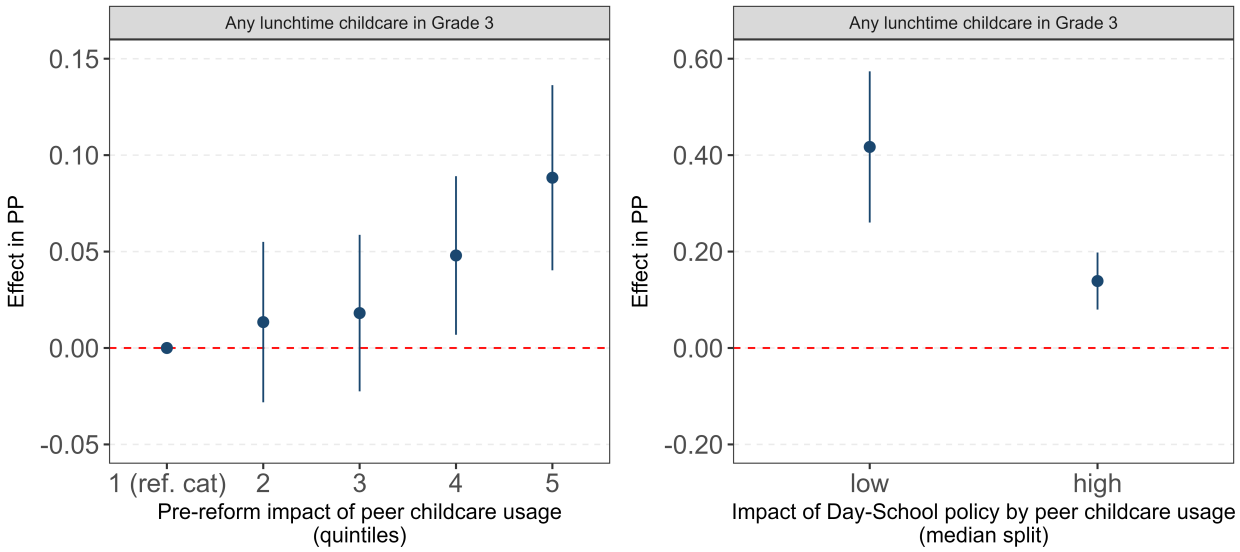


Figure 7: Stronger Impact of the Day-School Policy in Classes with Lower Baseline Peer Childcare Usage

Notes: The left figure shows the impact of peer childcare usage on the likelihood that a student enrolls in any public lunchtime childcare in Grade 3. The sample consists of all students attending Grade 1 between 2013 and 2019 in schools before the reform. Peer childcare usage measures the leave-out share of classmates in Grade 1 who used any public lunchtime childcare in K2 (by quintile). The regression includes school-unit fixed effects, cohort fixed effects, school-unit-specific linear, quadratic, and cubic time trends, and controls for individual lunchtime childcare usage in K1 and K2 and class size (deciles). The right figure shows the impact of the introduction of the day-school policy on the likelihood that a student enrolls in any public lunchtime childcare in Grade 3, by Grade 1 peer childcare usage (median split). The sample consists of all students attending Grade 1 between 2013 and 2019 in schools, both before and after the reform. The regressions include school-unit and cohort fixed effects, measured in Grade 1. 95% confidence intervals (vertical lines) based on standard errors clustered at the school-unit level are displayed.

Tables

Table 1: Summary Statistics

Variable	N unique obs.	Mean	q10	q90
Children				
Any public childcare enrollment	69,134	0.53	0.00	1.00
Any before-school childcare	69,134	0.06	0.00	0.00
Any lunchtime childcare	69,134	0.52	0.00	1.00
Any after-school childcare	69,134	0.33	0.00	1.00
Public childcare hours	69,134	6.05	0.00	18.00
Household characteristics				
Both parents born in CH	41,862	0.28	0.00	1.00
1 parent born in CH, 1 not	41,862	0.23	0.00	1.00
No parent born in CH	41,862	0.49	0.00	1.00
Low-income households	43,401	0.33	0.00	1.00
Medium-income households	43,401	0.34	0.00	1.00
High-income households	43,401	0.33	0.00	1.00
Father working, mother working	43,401	0.74	0.00	1.00
Father working, mother not working	43,401	0.20	0.00	1.00
Father not working, mother working	43,401	0.04	0.00	0.00
Mothers				
Age	38,984	40.66	32.00	49.00
N children in public school	38,984	1.46	1.00	2.00
Social security data				
Any earnings	38,984	0.80	0.00	1.00
Annual earnings	38,984	41,330.39	0.00	90,717.00
Swiss Earnings Structure Survey				
Monthly earnings	17,938	4,574.62	1,401.52	8,057.37
Monthly earnings, adjusted for hours	17,938	8,632.00	4,285.00	13,062.20
Weekly hours worked	17,938	23.08	0.00	41.25
Fathers				
Age	35,451	43.83	35.00	53.00
N children in public school	35,451	1.48	1.00	2.00
Social security data				
Any earnings	35,451	0.93	1.00	1.00
Annual earnings	35,451	95,213.52	9,491.50	190,026.00
Swiss Earnings Structure Survey				
Monthly earnings	18,474	8,342.43	4,127.20	13,539.24
Monthly earnings, adjusted for hours	18,474	10,380.18	4,990.00	16,593.20
Weekly hours worked	18,474	36.68	21.63	42.00

Table 2: Impact of the Day-School Policy on Public Childcare Usage

Specification	**DV: Days in lunchtime childcare**					**DV: Weekly childcare hours**
	Baseline	No prior childcare	Prior childcare	Early vs. late	Residence 2014	
	(1)	(2)	(3)	(4)	(5)	(6)
$DaySchool^{(0,2)}$	1.127***	1.791***	0.942***	1.142***	1.27***	2.351***
	(0.049)	(0.082)	(0.055)	(0.047)	(0.058)	(0.181)
Mean	1.63	0.28	2.65	1.76	1.52	6.08
N, unique stud.	64,412	30,675	31,557	28,230	46,173	64,412

Notes: The table shows the point estimates of the static analysis based on equation (2). All models control for school and year fixed effects. Model (1) represents my baseline specification and shows the effect of the day-school policy on the average number of days children spend in lunchtime childcare per week. (2) and (3) split the sample by childcare use before the reform. (4) uses the 15 schools that introduce the day-school policy between 2022 and 2024 as control schools. (5) only keeps the families in the sample that live at the same address as in the year 2014. (6) shows the effect of the reform on the average number of hours children spend in childcare per week. Standard errors adjusted for clustering on the school level are displayed.* p<.1, ** p<.05, *** p<.01

Table 3: Impact of the Day-School Policy on Parental Labor Supply (Social Security Data)

	Mothers			**Fathers**		
Grade	K1-2	1-4	5-9	K1-2	1-4	5-9
Any earnings						
$DaySchool^{(0,2)}$	0.037**	-0.005	0.003	0.02***	0.005	0.007
	(0.018)	(0.01)	(0.006)	(0.007)	(0.008)	(0.006)
N months with earnings						
$DaySchool^{(0,2)}$	0.474**	-0.055	0.021	0.318***	0.066	0.113
	(0.188)	(0.121)	(0.079)	(0.108)	(0.114)	(0.087)
Annual earnings (in CHF 1000's)						
$DaySchool^{(0,2)}$	2.971**	1.115	0.866	0.642	2.932	1.567
	(1.326)	(1.43)	(0.764)	(1.771)	(1.805)	(1.254)
Mean, Any earn.	0.77	0.8	0.8	0.94	0.94	0.93
Mean, Months earn.	8.67	9.16	9.1	10.92	10.88	10.78
Mean, Annual earn.	39.52	42.46	41	96.91	99.15	94.71
N, unique parents	22,715	24,079	21,730	21,320	22,117	19,265

Notes: The table shows the point estimates of the static analysis based on equation (2). The regressions are estimated separately for mothers and fathers. All models control for school and year fixed effects. All outcomes are based on social security data. Standard errors adjusted for clustering on the school level are displayed.* p<.1, ** p<.05, *** p<.01

Table 4: Impact of the Day-School Policy on Parental Labor Supply (Swiss Earnings Structure Survey)

	Mothers			**Fathers**		
Grade	K1-2	1-4	5-9	K1-2	1-4	5-9
Monthly gross earnings (in CHF 1000's)						
$DaySchool^{(0,2)}$	0.464***	0.3**	-0.041	-0.006	0.089	0.183
	(0.138)	(0.133)	(0.168)	(0.212)	(0.23)	(0.183)
Weekly hours worked						
$DaySchool^{(0,2)}$	2.078***	1.144*	0.032	0.874	-0.484	-0.143
	(0.755)	(0.654)	(0.858)	(0.765)	(0.475)	(0.632)
Monthly earnings, adjusted for hours (in CHF 1000's)						
$DaySchool^{(0,2)}$	-0.03	-0.041	-0.378	-0.368	0.099	0.083
	(0.195)	(0.154)	(0.246)	(0.273)	(0.275)	(0.294)
Mean, Monthly earn.	4.73	4.76	4.4	8.13	8.26	7.73
Mean, Weekly hours	23.14	23.08	23.08	36.39	36.54	36.95
Mean, Adj. earn.	8.77	8.76	8.12	10.05	10.24	9.48
N, unique parents	7,042	9,229	8,881	8,638	10,276	8,698

Notes: The table shows the point estimates of the static analysis based on equation (2), restricting the sample to parents that have worked in the two years before. All outcomes are based on the Swiss Earnings Structure Survey, which the Swiss Federal Statistics office carries out every two years among a random sample of private and public enterprises and administrations in Switzerland. The regressions are estimated separately for mothers and fathers. All models control for school and year fixed effects. Standard errors adjusted for clustering on the school level are displayed.* p<.1, ** p<.05, *** p<.01

Table 5: Spillovers of the Day-School Policy on After-School Childcare Enrollment and Siblings in Non-Reform Schools

Outcome	Monday	Tuesday	Wednesday	Thursday	Friday
Panel A: Direct effects					
Enrolled in lunchtime childcare					
$DaySchool^{(0,2)}$	0.434***	0.175***	-0.002	0.143***	0.513***
	(0.012)	(0.021)	(0.009)	(0.024)	(0.018)
Enrolled in after-school childcare					
$DaySchool^{(0,2)}$	0.022**	0.001	-0.008	-0.011	0.025***
	(0.009)	(0.008)	(0.006)	(0.01)	(0.008)
Mean, Lunchtime	0.37	0.4	0.16	0.39	0.31
Mean, After-school	0.17	0.19	0.11	0.19	0.14
N, unique students	59,476	59,476	59,476	59,476	59,476
Panel B: Effects on siblings in non-reform schools					
Enrolled in lunchtime childcare					
$DaySchool^{(0,2)}$	0.098***	0.056***	0.021*	0.065***	0.099***
	(0.021)	(0.018)	(0.011)	(0.021)	(0.02)
Enrolled in after-school childcare					
$DaySchool^{(0,2)}$	0.04***	0.01	0.019*	0.013	0.038***
	(0.012)	(0.01)	(0.01)	(0.012)	(0.01)
Mean, Lunchtime	0.34	0.37	0.18	0.37	0.28
Mean, After-school	0.19	0.21	0.13	0.21	0.16
N, unique students	63,166	63,166	63,166	63,166	63,166

Notes: Panel A: shows the point estimates of the static analysis based on equation (2). The sample comprises all children from K2 to end of lower secondary school. The columns show the effect of the day-school policy on the likelihood that a child is enrolled in lunchtime or after-school childcare at the respective day of the week. Panel B: The sample comprises all children in schools that do not introduce the reform until 2022. The treatment takes a value of 1 for all years that a sibling is in a school that has introduced the reform, and 0 otherwise. All regressions include school and year fixed effects. Standard errors adjusted for clustering on the school level are displayed.* p<.1, ** p<.05, *** p<.01

Appendix

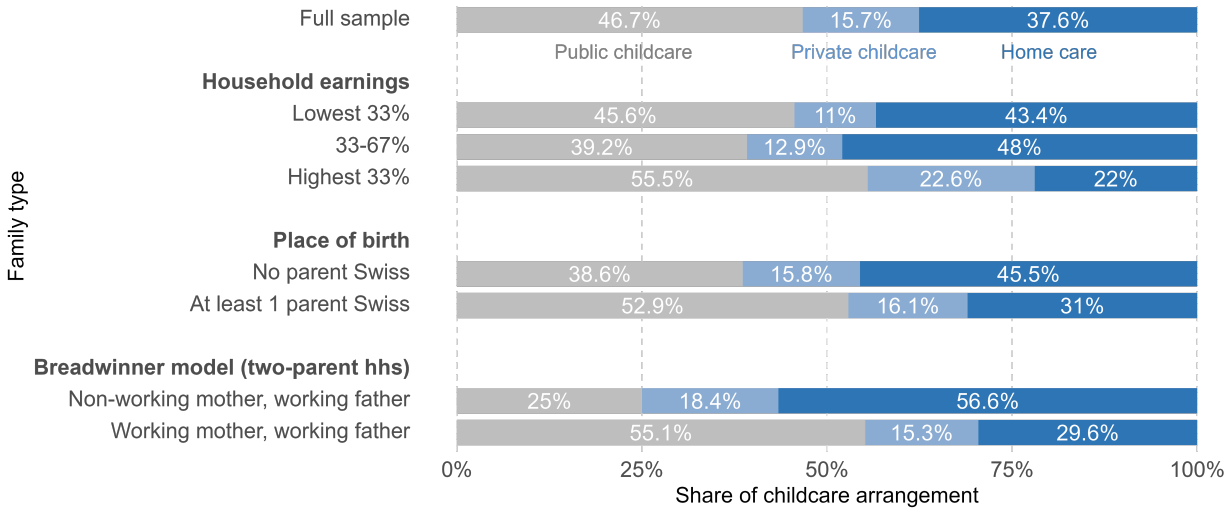


Figure A1: Pre-reform Childcare Arrangements by Family Background

Notes: The figure displays descriptive statistics for childcare arrangements by household income, migration background, and family model. The analysis is based on 471 parents that can be identified in the Swiss Labor Force Survey, which every year randomly surveys a subset of the Swiss population, between 2011 and 2015.

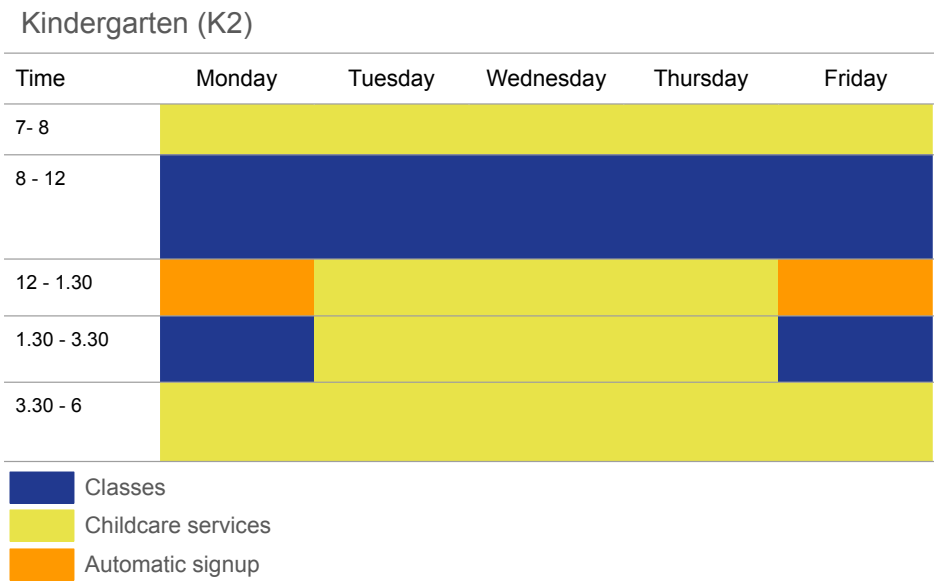


Figure A2: Class Schedule in Second Year of Kindergarten (K2)

Notes: The figure illustrates the weekly class schedule for children in their second year of kindergarten. Public childcare services are available before school, over lunchtime, and after school (shown in yellow) both before and after the reform. The day-school policy introduces a default enrollment into lunchtime childcare on the two days when children have afternoon classes (highlighted in orange), without expanding the overall availability of public childcare. In higher grades, the number of days with afternoon classes increases: children in grades 1–4 are automatically enrolled in lunchtime care on three days, and those in grades 5–9 on four days (not shown in the figure).

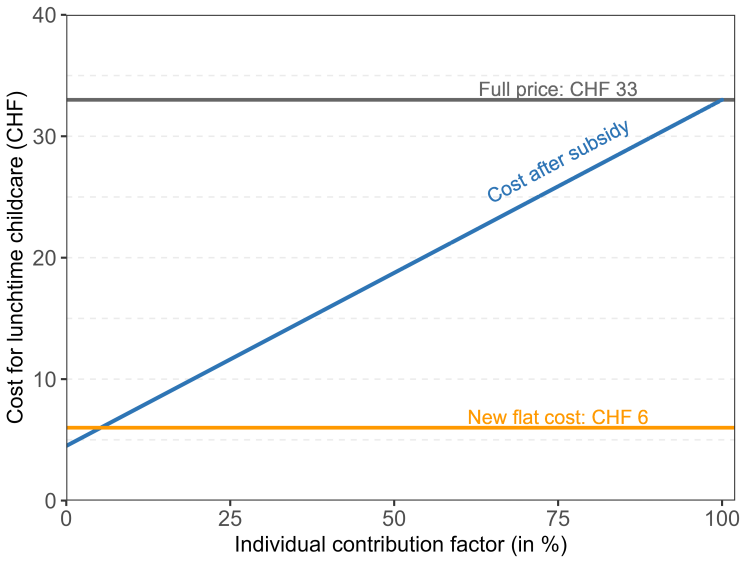


Figure A3: Parents' Cost of Lunchtime Childcare

Notes: The figure displays parents' cost of lunchtime childcare as a function of the individual contribution factor, which determines the share of childcare costs borne by parents. To obtain the individual contribution factor, parents have to request an assessment with the Education Department. Prior to the reform, parents who did not request an assessment paid the full cost of CHF 33 per day. Assessed families with a contribution factor of 0 (typically low-income families) paid CHF 4.50. Following the introduction of the day-school policy, all parents pay a flat fee of CHF 6 per day, regardless of income or assessment status.

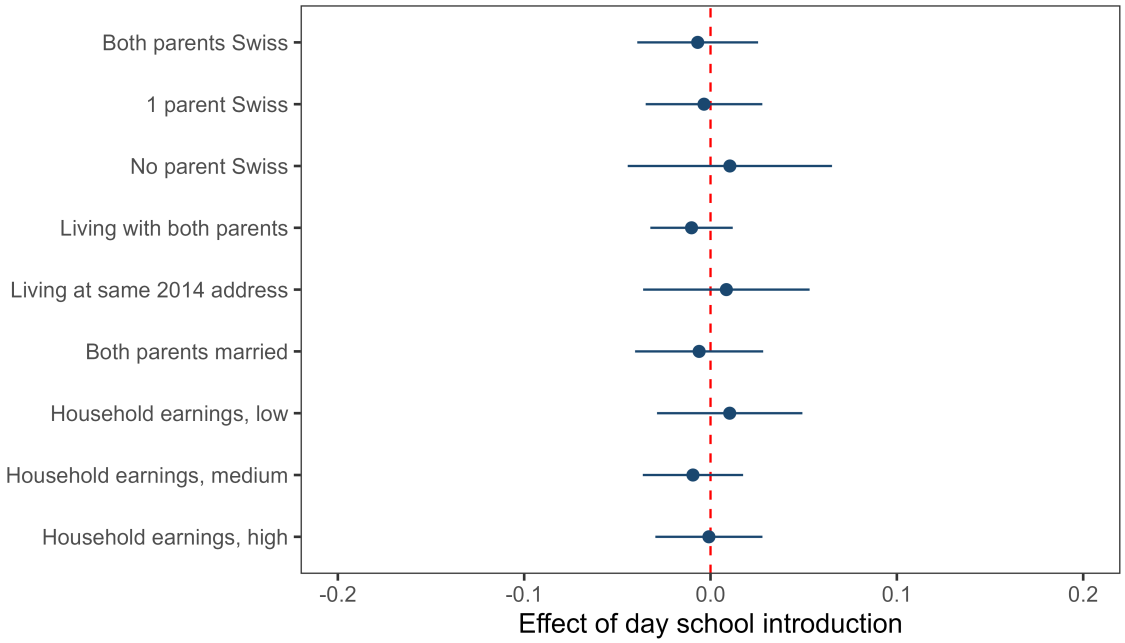


Figure A4: Effect of Day-School Policy on Observables

Notes: The figure shows the effect of the day-school policy on observables. The point estimates are derived from regressions of the respective background variables on a day-school dummy, similar to equation (2). 95% confidence intervals (horizontal lines) based on standard errors clustered at the school level are displayed.

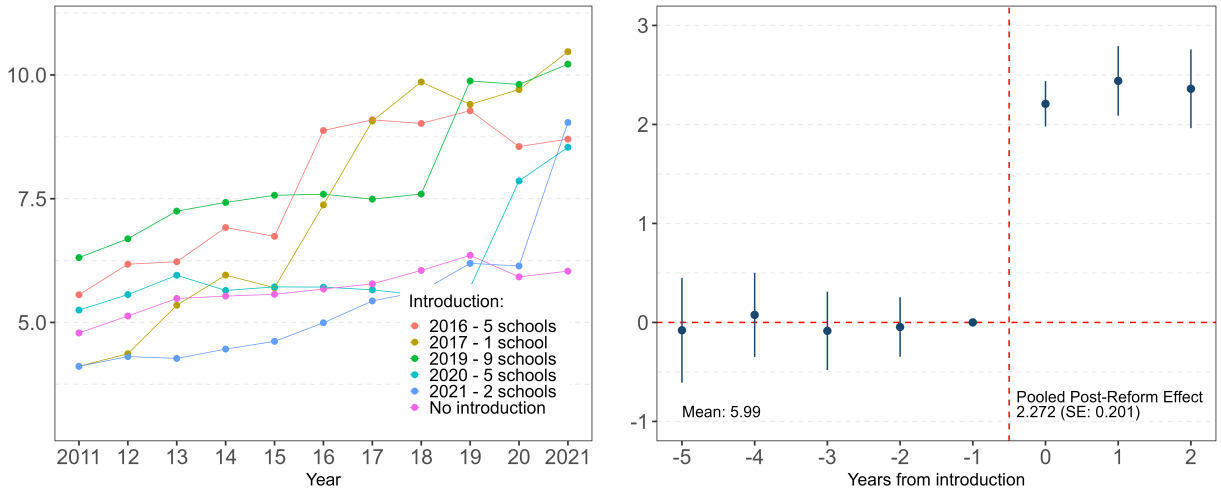


Figure A5: Impact of the Day-School Policy on Weekly Hours in Public Childcare

Notes: The figure shows the effect of the day-school policy on the weekly hours students are enrolled in public childcare. The left panel plots the raw data, grouped by introduction year of the policy. The right panel displays the effect of the policy using a stacked event study regression as described in equation (1), controlling for school and year fixed effects. The regression exploits the introduction of the day-school policy in 22 schools and kindergartens between 2016 and 2022. The sample mean is 53.4%. 95% confidence intervals (vertical lines) based on standard errors clustered at the school level are displayed.

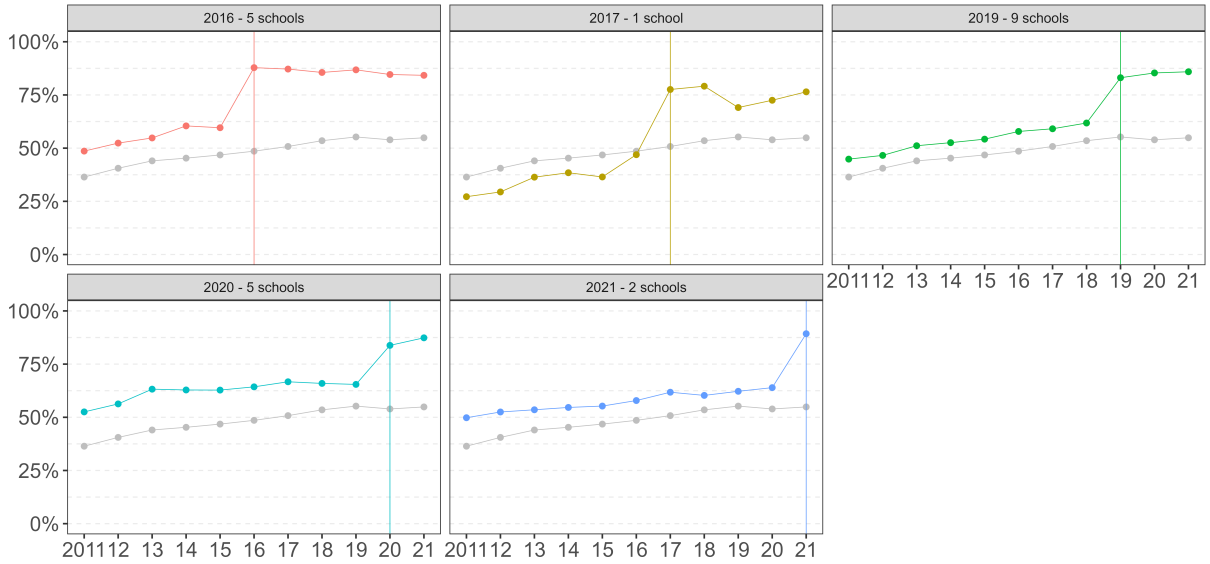


Figure A6: Share of Students With Any Lunchtime Childcare in Schools With and Without Introduction of the Day-School Policy

Notes: The panels plot the raw data, grouped by introduction year of the policy. The figure shows the share of students that are registered for at least 1 lunchtime childcare service. The sample consists of all students enrolled in the public kindergartens and schools in the city of Zurich. The gray line represents schools without any introduction of the day-school policy until the end of the school year 2021/22. The policy was introduced at 5 schools in 2016, 1 school in 2017, 9 schools in 2019, 5 schools in 2020, and 2 schools in 2021.

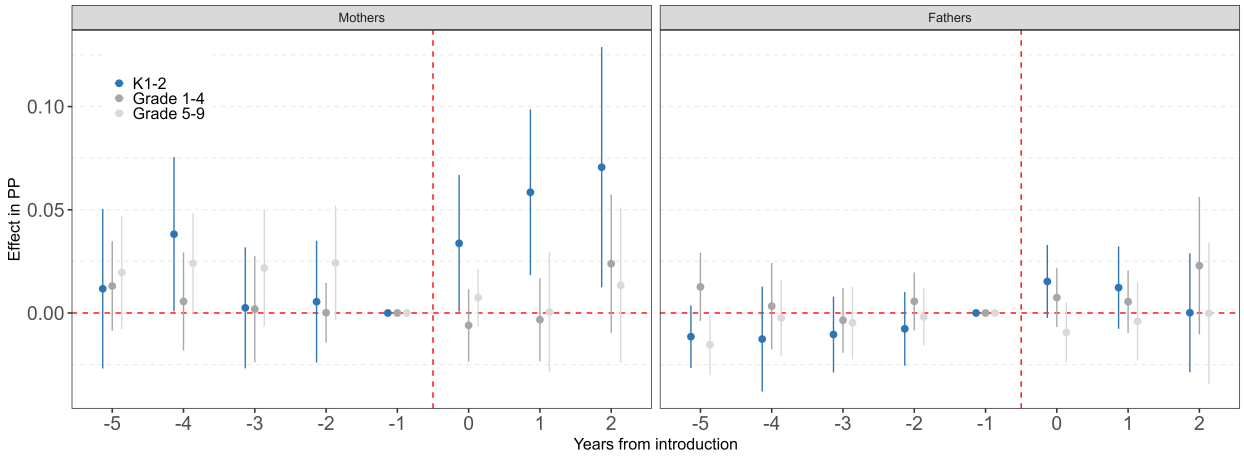


Figure A7: Impact of the Day-School Policy on Parents' Extensive Labor Supply by Grade and Parental Gender

Notes: The figure shows the effect of the day-school policy on the likelihood that parents have any earnings in a given year, using the stacked event study regression as described in equation (1). The regressions are estimated separately by grades (K1-2, Grade 1-4, Grade 5-9) and for fathers and mothers. The regressions exploit the introduction of the day-school policy in 20 schools and kindergartens between 2016 and 2020. 95% confidence intervals (vertical lines) based on standard errors clustered at the school level are displayed.

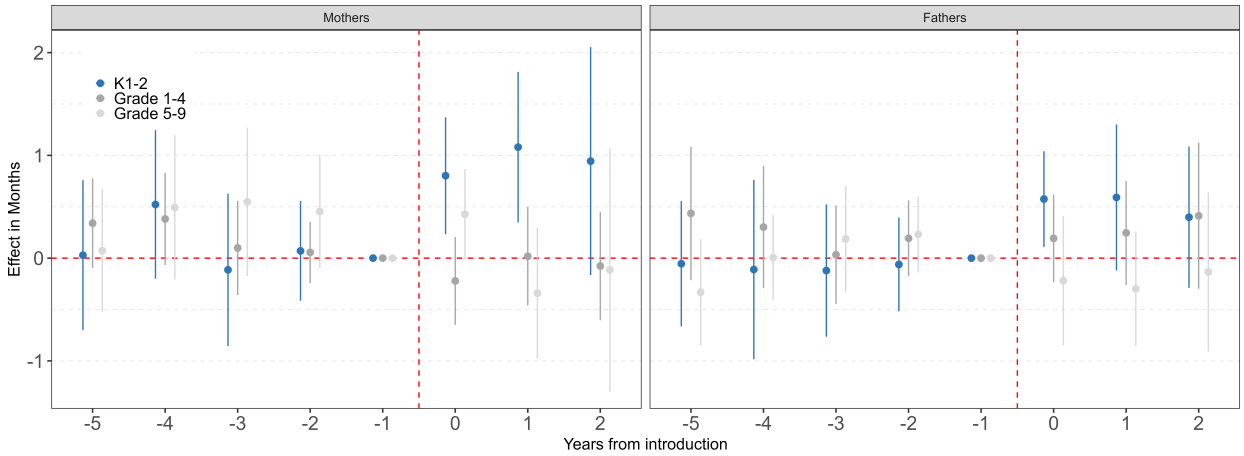


Figure A8: Impact of the Day-School Policy on Months Worked by Grade and Parental Gender

Notes: The figure shows the effect of the day-school policy on average number of months with any earnings in a given year, using the stacked event study regression as described in equation (1). The regressions are estimated separately by grades (K1-2, Grade 1-4, Grade 5-9) and for fathers and mothers. The regressions exploit the introduction of the day-school policy in 20 schools and kindergartens between 2016 and 2020. 95% confidence intervals (vertical lines) based on standard errors clustered at the school level are displayed.

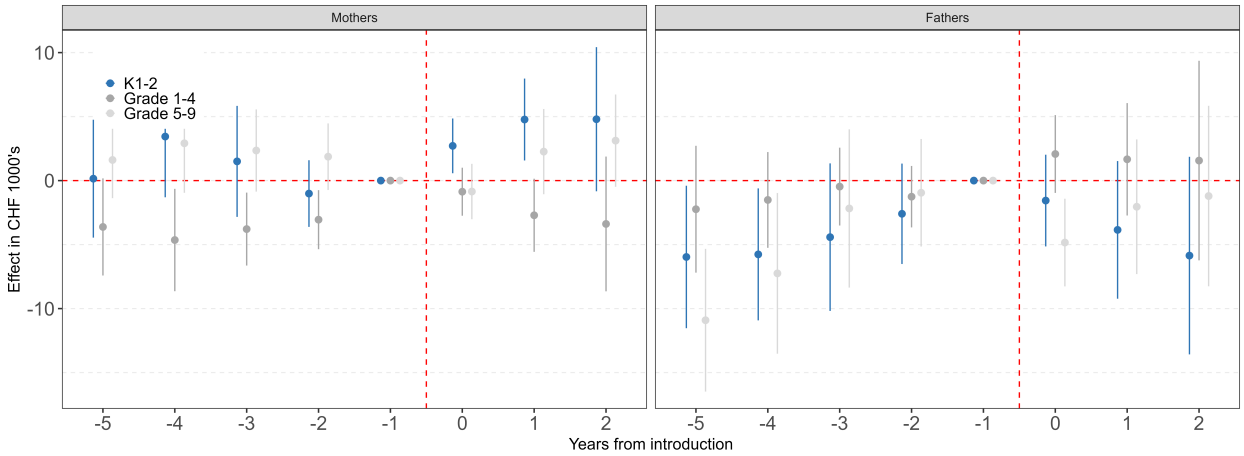


Figure A9: Impact of the Day-School Policy on Parents' Annual Earnings by Grade and Parental Gender

Notes: The figure shows the effect of the day-school policy on parents' annual earnings, using the stacked event study regression as described in equation (1). The regressions are estimated separately by grades (K1-2, Grade 1-4, Grade 5-9) and for fathers and mothers. The regressions exploit the introduction of the day-school policy in 20 schools and kindergartens between 2016 and 2020. 95% confidence intervals (vertical lines) based on standard errors clustered at the school level are displayed.

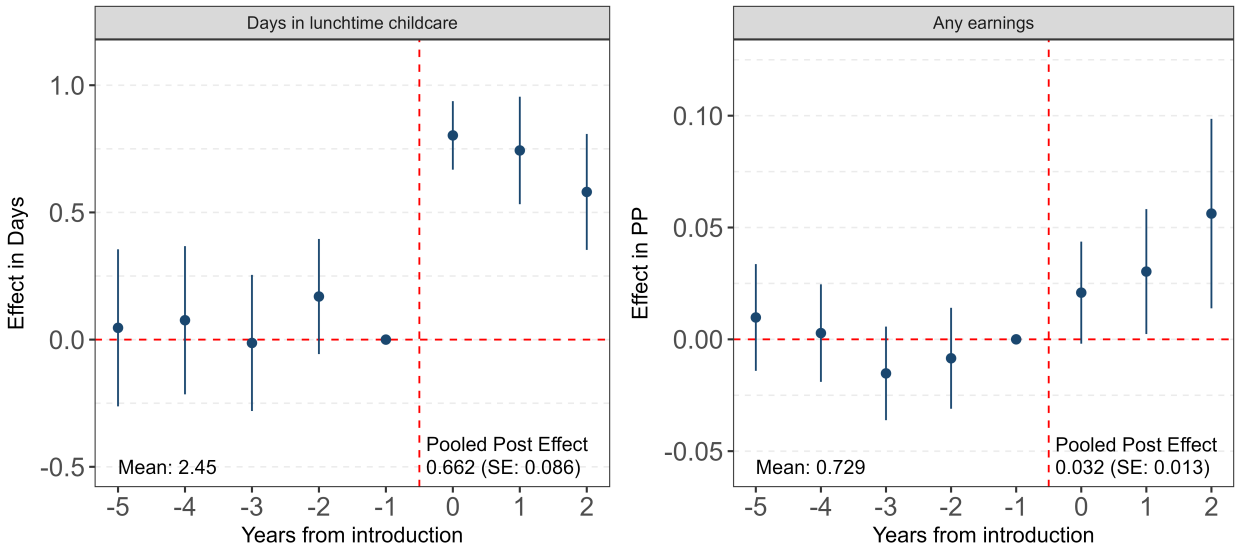


Figure A10: Impact of the Day-School Policy on Families With Default Change and Increase/No Change in Lunchtime Cost

Notes: The figure shows the effect of the day-school policy for families that are affected by the default change and experience an increase or no change in lunchtime childcare cost. The sample consists of children and their parents that attend grades between K1 and Grade 4. The left panel plots the effect of the policy on the likelihood that a child is enrolled in any lunchtime childcare. The right panel plots the effect of the policy on the likelihood that a parent has any earnings in a year. Household earnings are measured in the year before the reform. The effects are estimated using a stacked event study regression as described in equation (1), controlling for school and year fixed effects. The regression exploits the introduction of the day-school policy in 22 schools and kindergartens between 2016 and 2022 for the childcare results and in 20 schools and kindergartens between 2016 and 2020 for labor-supply results. 95% confidence intervals (horizontal lines) based on standard errors clustered at the school level are displayed.

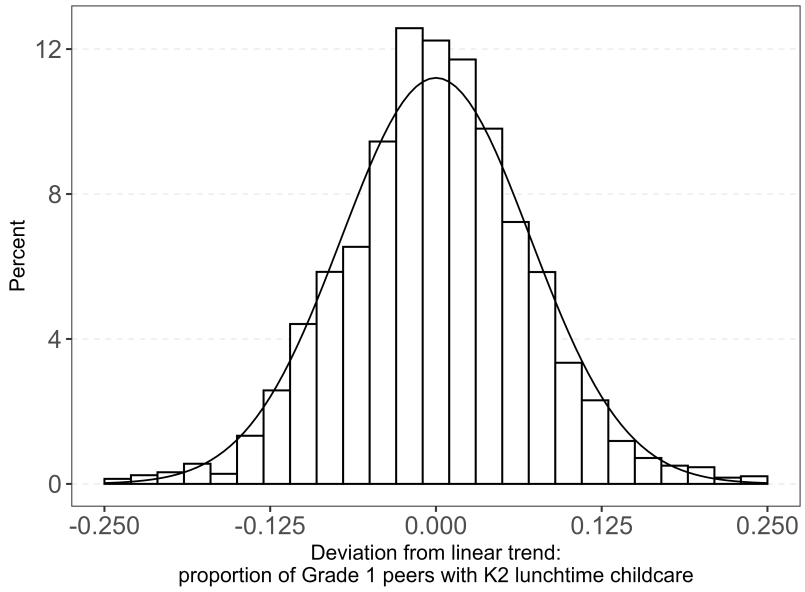


Figure A11: Deviation of Peer Childcare Usage from School-Unit-Specific Linear Trend

Notes: This figure presents the year-to-year variation in the proportion of peers in Grade 1 (same school unit, same cohort, same class type) who used any public lunchtime childcare in K2, plotted along with the normal distribution. More precisely, it plots residualized peer childcare usage from a regression of the proportion of Grade 1 peers with K2 lunchtime childcare usage on cohort and school-unit fixed effects and school-unit-specific linear time trends.

Table A1: Robustness: Impact of the Day-School Policy on the Extensive Labor Supply of Parents With Children in K1-2

Specification	**DV: Any earnings**					
	Baseline	Balanced	Only 2016	Weights=1	Early vs. late	Residence 2014
$DaySchool^{(0,2)}$	0.029**	0.029**	0.05	0.03**	0.026**	0.028**
	(0.012)	(0.012)	(0.032)	(0.012)	(0.012)	(0.011)
Mean	0.86	0.86	0.85	0.86	0.87	0.87
N, parents	44,034	43,440	26,404	34,063	17,281	30,622

Notes: The table shows the point estimates of the static analysis based on equation (2). The sample consists of all parents with children in K1-2. All models control for school and year fixed effects. Model (1) represents my baseline specification and shows the effect of the day-school policy on the likelihood that parents have with any earnings (from wage and self-employment). (2) shows the effect of the policy when I restrict the sample to the schools that are observable for all years between 2011 and 2020. (3) shows the effect of the policy on the 4 schools that introduce the policy in 2016. (4) restricts the sample to parents who only have one child enrolled in the public school system. (5) uses the 15 schools that introduce the day-school policy between 2022 and 2024 as control schools. (6) only keeps the families in the sample that live at the same address as in the year 2014. Standard errors adjusted for clustering on the school level are displayed.* p<.1, ** p<.05, *** p<.01

Table A2: Balancing Test: Does Lunchtime Childcare Usage Predict Peer Lunchtime Childcare Usage?

	DV: Share of G1 peers with any K2 lunchtime childcare				
Model	(1)	(2)	(3)	(4)	(5)
Any K2 lunchtime childcare	0.0003	-0.001	-0.001	-0.001	-0.002
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Mean	0.56	0.56	0.56	0.56	0.57
N	20,913	20,913	20,913	20,913	13,194
School-unit fixed effects	Yes	Yes	Yes	Yes	Yes
Cohort fixed effects	Yes	Yes	Yes	Yes	Yes
School-unit time trends	None	Linear	Quadratic	Cubed	Cubed
Individual controls	No	No	No	No	Yes

Notes: The dependent variable in all columns is the leave-out share of classmates in Grade 1 (same school unit, same cohort, same class type) who used any public lunchtime childcare in K2. The sample consists of all students attending Grade 1 between 2013 and 2019 in schools before the reform. Following the Guryan, Kroft, and Notowidigdo (2009) correction method, I control for the leave-out mean of the proportion of peer childcare usage in K2 across cohorts within a school unit. Standard errors clustered at the school-unit cohort level are reported. * p<.1, ** p<.05, *** p<.01

Table A3: Impact of Peer Childcare Usage on Lunchtime Childcare Enrollment in Grade 3

	DV: Any lunchtime childcare in Grade 3				
Model	(1)	(2)	(3)	(4)	(5)
Peer childcare usage	0.199***	0.114**	0.137***	0.162***	0.168***
	(0.048)	(0.053)	(0.052)	(0.048)	(0.054)
Mean	0.62	0.62	0.62	0.62	0.61
N	10,391	10,391	10,391	10,391	9,793
School-unit fixed effects	No	Yes	Yes	Yes	Yes
Cohort fixed effects	No	Yes	Yes	Yes	Yes
School-unit time trends	None	Linear	Quadratic	Cubed	Cubed
Individual controls	No	No	No	No	Yes

Notes: The table shows the impact of peer childcare usage on the likelihood that a student enrolls in any public lunchtime childcare in Grade 3. Peer childcare usage measures the leave-out share of classmates in Grade 1 (same school unit, same cohort, same class type) who used any public lunchtime childcare in K2. The sample consists of all students attending Grade 1 between 2013 and 2019 in schools before the reform. All regressions control for individual lunchtime childcare usage in K1 and K2. Standard errors clustered at the school-unit cohort level are reported. * p<.1, ** p<.05, *** p<.01