WORKING PAPER 2025:18

Increasing participation in preschool

Evidence from a default enrollment policy

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ISSN 1651-1166

Increasing participation in preschool

Evidence from a default enrollment policy[a]

by

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September 22, 2025

Abstract

While all children in Sweden are entitled to free, universal preschool from age 3, enrollment rates for children with an immigrant background – and especially for those in newly-arrived families – remain well below the 95 percent national average. At the same time, studies suggest that these children have particularly high returns from preschool attendance. Thus, policies that increase preschool enrollment among children with an immigrant background have potential to improve long-term educational and labor market outcomes and narrow the gap to natives. We evaluate a reform in 2023 that required municipalities to offer preschool slots to 3–5-year-olds in newly-arrived immigrant families without the parents needing to apply – so-called automatic offers. Using a difference-in-differences specification, we compare next-year enrollment rates for currently non-enrolled children in newly-arrived families and other families and find that the policy substantially increased preschool enrollment. Our results suggest that administrative hurdles prevent some immigrant families from enrolling their children and that simplifying the application process is an effective policy tool for increasing preschool participation in this group.

Keywords: preschool participation; foreign background; default enrollment
JEL-codes: I24; I28

[a] We thank Chris Karbownik, Sara Roman, Olof Åslund and seminar participants at IFAU and the UIL Urban Lab HEFUU 2025 workshop for valuable comments. We thank the Swedish Research Council for Health, Working Life and Welfare (FORTE; grant no. 2024-00851) for funding.
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1 Introduction

Reducing disparities in learning outcomes between students from different socioeconomic backgrounds is a central policy goal in many countries. Economic theory, such as the work of Cunha and Heckman (2007) and Heckman (2007), emphasizes the critical role of early-life human capital investments, arguing that skills developed early significantly benefit continued learning. Consistent with this perspective, many studies have found that participation in preschool programs improves later educational outcomes, particularly for children from socioeconomically disadvantaged or immigrant backgrounds (see, e.g., Berlinski et al. 2009; Havnes and Mogstad 2011; 2015; Cascio and Schanzenbach 2013; Gray-Lobe et al. 2022 as well as reviews by Dietrichson et al. 2020 and Duncan et al. 2023). However, studies on early childhood education have also shown that children who are likely to experience the highest returns in terms of overall school readiness, such as those with immigrant background, tend to participate to a lesser extent (Cornelissen et al. 2018; Corazzini et al. 2021). Across countries, children from immigrant or socioeconomically disadvantaged backgrounds are consistently underrepresented in preschool, and such enrollment gaps persist even in systems where preschool is universal and free of charge (OECD 2025).[1] This so-called "Matthew effect" in social policy – the tendency for programs intended for broad access to be disproportionally utilized by more advantaged social groups – dampens intergenerational social mobility. Increasing participation in preschool programs among children with disadvantaged backgrounds therefore holds promise as a policy tool for promoting equality of opportunity.

This study evaluates a policy intervention in Sweden, introduced in 2023, that offers a reserved preschool slot to immigrant children aged 3–5 without the parents needing to apply – a so-called automatic offer – and examines its effect on enrollment. The offer concerns a slot in universal preschool with a curriculum focused on school-preparatory activities, free of charge, for 15 hours per week.[2] The policy provides immigrant parents with a 'default option' to enroll their child: parents only need to respond to the offer; no additional application procedure is needed. Such a policy may increase enrollment by removing administrative hurdles and cognitively demanding tasks associated with the application process, which could otherwise discourage some parents from applying for a preschool slot. In addition, it informs (or reminds) parents that their child is eligible for free universal preschool, that preschool attendance is beneficial for their child, and

[1] Many countries offer universal preschool programs free of charge, yet enrollment gaps persist by socioeconomic background and immigration status. OECD (2025) reports that in 8 out of 28 countries surveyed, preschool enrollment gaps have widened for children aged 3–5, as enrollment rates have increased more among children from advantaged backgrounds.

[2] Note that all 3–5-year-olds in Sweden are entitled to 15 hours of free preschool per week. But parents who are not targeted by the reform must actively apply for a slot for their child. If they apply, a slot is guaranteed within 4 months.

that it is the typical choice for children aged 3 and older. The target group consists of children who are not enrolled in preschool at the beginning of the year in which they turn 3, 4 or 5 and who are either foreign-born themselves or have parents who are foreign-born and immigrated to Sweden within the last 5 years. This specific target group enables us to construct a control group consisting of children who are also not enrolled in preschool at the beginning of the year in which they turn 3, 4 or 5 but who do not have an immigrant background as defined above.

In most European OECD countries, children from disadvantaged backgrounds attend preschool at lower rates than more advantaged children (OECD 2025). In Sweden, this enrollment gap is strongly linked to immigration status: while 95 percent of 3–5-year-olds born in Sweden are enrolled, the rate is 80 percent for children born abroad (SOU 2020:67). From this study, we learn about the role of information deficits and administrative barriers for explaining the lower enrollment rates among children with immigrant background, as well as about a policy measure that might help more children access the benefits of early childhood education.

We find that the policy increased the next-year preschool enrollment rate in the target group by 5 percentage points from a baseline of 29 percent, corresponding to a 17 percent increase.[3] Automatically offering a reserved preschool slot thus appears to be an effective policy tool for increasing preschool participation among immigrant children. Further analysis aimed at disentangling the effect of removing administrative hurdles from the informational component suggests that providing information about preschool eligibility alone does not increase enrollment, rather the information must be combined with an offer of a reserved preschool slot to have an effect. Thus, the reason for non-enrollment does not appear to be a lack of knowledge about the universal preschool system.

While the policy is effective overall, a closer examination of which families respond indicates that the increase is primarily driven by children whose mothers have at least high school education and those whose mothers immigrated for reasons other than refugee-related ones, such as to work or study. Thus, children from the most disadvantaged backgrounds, who may have the most to gain from preschool activities, appear to have been harder to influence, although we note that the differences between groups are not statistically significant. It is also worth emphasizing that the target group as a whole is disadvantaged relative to the general population.

Since the policy implies that virtually all administrative and informational obstacles have been removed, we interpret families not affected as having preferences for not using preschool, or that there are other types of barriers preventing them from enrolling their child, such as lack of trust in institutions. Among newly-immigrated children who do not attend preschool, 66 percent

[3] Note that the target group, as well as the control group, consists of children who are *currently non-enrolled*. This explains the rather low baseline of 29% in terms of next-year enrollment. As mentioned in the text, most immigrant children (80%) are already enrolled and are thus not part of the target group.

remained non-enrolled despite receiving the offer, a reduction of only 7 percent relative to the previous year's rate of 71 percent.

This paper contributes to several strands of the literature. First, we contribute to understanding the mechanisms behind the lower preschool enrollment rates among immigrants, or more generally, disadvantaged children in universal preschool systems. While previous studies show that removing financial barriers increases participation rates among minority children (Drange and Telle 2015), our study is the first to show that simplifying the preschool application process also significantly increases enrollment. This finding suggests that administrative hurdles prevent some immigrant families, and potentially also native families with a weaker socioeconomic status, from enrolling their children in preschool. This finding aligns with studies in the higher education literature, showing that simplifying the application process and providing assistance with financial aid applications can increase college enrollment among students from disadvantaged backgrounds (Bettinger et al. 2012; French and Oreopoulos 2017; Dynarski et al. 2021). At the same time, the majority of the target group did not accept the automatic preschool offer, suggesting that other factors – such as cultural norms or lack of trust in institutions – may remain barriers to preschool enrollment for some immigrant groups, as suggested in several qualitative studies (e.g., Vesely 2013; Garvis 2021; Shomary 2022).

Second, our results contribute to the choice architecture literature and particularly studies showing that the likelihood that people choose a societally preferred option increases when that option is presented as a pre-selected choice – a 'default' (Jachimowicz et al. 2019). From other settings, we know that going from an opt-in to an opt-out system could be a potent way to increase program take-up (see e.g., retirement savings: Madrian and Shea 2001; organ donation: Abadie and Gay 2006; vaccination: Elinder et al. 2023). Our study is the first to demonstrate the relevance of these ideas for closing the preschool enrollment gap.[4] Our findings are in line with earlier results in the psychological literature, suggesting that the default option is interpreted as an implicit recommendation by society (McKenzie et al. 2006), or that it directly affects the meaning of the enrollment choice (Davidai et al. 2012). In our case, automatic offers may be perceived by parents as a reflection of the prevailing social norm in Sweden, namely that 3-year-olds should attend preschool. In addition, the choice architecture may be important: When a child is offered a slot by default, parents are confronted with the choice to opt out, which may be seen as deviating from the norm. Conversely, without an automatic offer, the burden shifts to parents to actively apply for a slot they do not necessarily perceive as valuable.

[4] Another way to close the enrollment gap is of course to make preschool compulsory from the age of 3, as France did in 2019 (Herbaut et al. 2025), but in most countries this is not a politically feasible policy option. In those cases, automatic offers could be a compromise.

Finally, our study relates to the literature about nudging within education (see, e.g., Mayer et al. 2019; Damgaard and Nielsen 2020). An example of a somewhat promising policy in the preschool context is to send text messages to parents about the importance of attending preschool (Díaz et al. 2020). Our study complements this literature by focusing on the extensive margin, namely, to enroll children in preschool in the first place.

The paper is organized as follows. We begin by describing the Swedish preschool system and the policy of automatic preschool offers (section 2). We then describe our data and outline the empirical strategy (section 3). The main results are presented in section 4, and in section 5 we discuss mechanisms. Section 6 concludes.

2 Institutional context

Before presenting the policy evaluated, we briefly describe the Swedish preschool system.

2.1 Swedish universal preschool

Sweden has a decentralized education system, where the responsibility for organizing both schools and preschools lies at the local municipal level[5], although the actual providers can be either public or private. How preschools/childcare centers[6] operate is largely regulated by the central government. For example, all preschools must follow a national curriculum, they are highly subsidized, and there is a national cap on the preschool fee.[7] Municipalities are obligated to provide a preschool slot for all children from age 1 if their parents are working, studying, or applying for jobs. The number of hours parents are entitled to depends on how much they work or study. Starting in the fall semester of the year a child turns 3, all children, regardless of parental employment status, are entitled to preschool free of charge for 15 hours per week (typically organized as 3 hours per day).[8] This universal preschool follows the compulsory school calendar (i.e., from late August to early June)[9] and is considered an important preparatory stage for compulsory school, which begins in the fall of the year the child turns 6 (SOU 2020:67).

Although all children are entitled to universal preschool from age 3, a slot is only provided once the parents have submitted an application. Applications are typically submitted through the

[5] There are 290 municipalities in Sweden.

[6] Early childhood education and childcare is combined in Sweden; hence, ‘childcare’ and ‘preschool’ refer to the same thing in the Swedish context.

[7] The fee for a preschool slot depends on family income and number of children enrolled: A family pays at most 3% of their monthly income for the first child, 2% for the second, and 1% for the third. The cap on the fee in 2023 corresponded to SEK 1,645 (EUR 143) for the first child, SEK 1,097 for the second, and 548 for the third child. The fourth child attends for free.

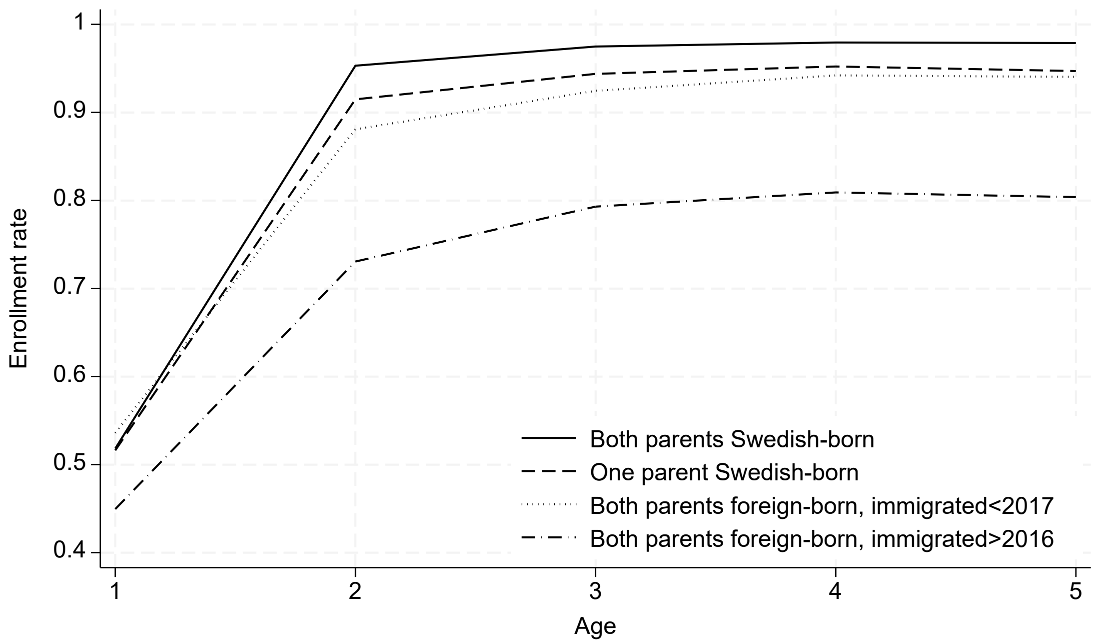
[8] For parents using more than 15 hours per week, the total fee is reduced by an amount corresponding to these 15 hours.

[9] See Prop. 2008/09:115 and paragraphs 4,16, and 20 in chapter 8 of the Swedish School Law (2010:800).

municipality's online application system, where parents rank multiple alternatives. The application usually needs to be submitted a few months before the desired start date. If the number of applications exceeds the available slots at a specific preschool, municipalities generally prioritize children who have a sibling already enrolled and those residing in close proximity to the preschool, but all children are guaranteed a slot in some preschool in the municipality from the fall the year they turn 3 (given that the application is submitted in time).

Figure 1 presents the preschool enrollment rate in 2022 by child age and parental immigrant background. More than 95 percent of all 3-years-olds with at least one Swedish-born parent are enrolled. The enrollment rate among children with two foreign-born parents who immigrated more than 5 years ago is just a few percentage points lower, while the rate for those whose parents immigrated in the last 5 years is markedly lower, corresponding to around 80 percent.

Figure 1 Preschool enrollment rates in Sweden in 2022 by age and parental immigration background



Note: The figure is based on children born 2017–2021 who were registered as residents in Sweden on December 31, 2022. Preschool enrollment is measured on October 15, 2022. The figure is based on own calculations using nationwide register data from Statistics Sweden.

Children who do not attend preschool differ from those who do in terms of more than just migration background. Table A1 in the Appendix presents background characteristics of children aged 3–5 in 2022, by preschool enrollment status. Non-enrolled children are much more likely to have parents with lower education levels and low (or no) earnings from work, especially mothers, and the parents are also more likely to receive welfare benefits. Thus, not attending preschool is strongly associated with socioeconomic disadvantage.

2.2 The policy of automatic preschool offers

Since 2023, Swedish municipalities have been required to engage in outreach efforts to increase preschool enrollment among children with an immigrant background, with the primary aim of supporting their Swedish language development (Prop. 2021/22:132).[10] The policy has two components: First, municipalities must contact the guardians of *all* children aged 3–5 who are not enrolled in preschool and inform them about the purpose of preschool and their child's entitlement. Second, they are obligated to automatically offer all 3–5-year-olds in newly-arrived immigrant families (who immigrated less than 5 years ago) a reserved 15-hour-per-week free preschool slot. Note that families with non-enrolled 3–5-year-olds who are not targeted by the automatic offers still receive information. This is important since these families will constitute the control group in our analysis. (In section 3.2, we discuss how this affects the interpretation of our estimates.) The automatic offer requirement means that municipalities must reserve a slot for the targeted children, even if the child's guardians have not applied for one. This offer must be from the fall of the year the child turns 3, i.e., when the child first becomes entitled to 15 hours per week free of charge.

Offers are sent by mail in April or May, and parents are asked to respond "yes" or "no" on a reply stub and return it using an enclosed envelope. If there is no reply, the municipality must still keep the slot reserved for one month after the suggested start date. During that month, parents can bring their child to the preschool and claim their slot even if they did not respond to the offer. If the guardians do not take up the offer, a new offer must be made for each subsequent fall until and including the year the child turns 5.

The Swedish National Agency for Education has prepared information material in different languages (e.g., English, Arabic, Dari, Persian, and Somali) for municipalities to use in their outreach activities, along with a step-by-step guide on how to identify the correct target group of children to be offered a preschool slot using population register data.[11] Municipalities can adapt their approaches to reach and inform guardians based on local needs and circumstances. In Uppsala municipality, for example, preschool principals make an additional contact with the guardians by phone during June–September to welcome them to preschool and inform them that the family can visit the preschool. An example of how the information in the preschool placement offer may look is shown in Appendix C.

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[10] The proposition was approved in the parliament in June 2022 and applied for the first time on education starting after July 1, 2023.

[11] The information material can be found here: [Kommuners arbete för fler barn i förskolan - Skolverket](#).

3 Data and empirical strategy

3.1 Data sources

Our analyses are based on a database that combines administrative data from several registers maintained by Statistics Sweden. The database includes all Swedish residents (ages 0–74), with the various registers linked through unique personal identification numbers. The data include information on individuals' education, income, and municipality of residence, as well as immigration background, such as country (or region) of birth, immigration date, and type of residence permit. The immigration date is defined as the date the individual was first registered as a resident in Sweden.[12] Children are linked to their parents through the multigenerational register, and we can observe their sex, date of birth, and country of birth. Additionally, a nationwide preschool register, available for 2014–2023, provides information on whether a child is registered in a preschool on October 15 each year. The register also includes the date of the first enrollment with the preschool provider.

3.2 Empirical strategy

Our main strategy to identify the impact of automatic preschool offers on enrollment is to compare differences in preschool enrollment rates in the fall between the target group of non-enrolled children in newly-arrived immigrant families and a control group of children who also did not attend preschool in the spring of the year in which they turned 3, 4 or 5 but were not targeted to receive an automatic offer, before and after the 2023 reform.

Sampling. For the fall 2023 preschool offers, we construct our treatment and control groups by selecting all children born 2018–2020 who met the following criteria: they were registered as residents in Sweden on December 31 of both 2022 and 2023 (i.e., both the treatment year and the year before)[13], and they were not enrolled in preschool according to the October 15, 2022, edition of the preschool register. Moreover, we exclude children who appear in the 2023 preschool register but whose start date is before August 2023, as an earlier start indicates that an application had already been submitted before the municipalities identified the target group and sent out offers (offers were not sent to children who had pending applications). We then distinguish between treated children (who received information *and* an offer) and control children (who received information *but not* an offer) based on migration information for the child and the parents. According to the law, only families who immigrated during the last 5 years should receive the

[12] This date may differ from the individuals' date of arrival, especially for asylum seekers who often have to wait a rather long time before they receive a resident permit.

[13] We condition on residency also at the end of 2023 to avoid including children who emigrated during the treatment year. It happens that families emigrate without notifying the authorities, which means that they may still be registered as residents after leaving the country. To further investigate this aspect, we also condition on the parents having some type of registered income in one of our robustness checks; see section 4.2.

offer, but the interpretation of the 5-year rule is not precisely specified. However, our contact with a number of municipalities indicates that January 1, 2018 was used as the immigration date cutoff when identifying the target group in the population register.

We then construct analogous samples for each year going back to 2015. For each year, this means that we select children turning 3–5 who were not enrolled in preschool in the spring, and define the potential treatment group using the same 5-year immigration window relative to that year. Each annual sample consists of approximately 12,500–14,500 children, with 17–25 percent belonging to the group that would have received an offer if the policy had been in place (i.e. children in newly-arrived families); see Table A2.

Table 1 presents background characteristics of the treated children from newly-arrived families and the control group of other non-enrolled children of the same age. As expected, the family members in the newly-arrived families are to a larger extent foreign-born, with Syria, Iraq, and Somalia being the most common countries of origin. Approximately half of these families migrated to Sweden for refugee-related reasons (having received residence permits as refugees or relatives of a refugee), while one-quarter arrived for work or study purposes (one or both parents hold a residence permit based on work or study). The remaining quarter consists mostly of migrants from other EU countries. Parental earnings and educational attainment are significantly lower among the newly-arrived families than in the control group: 88% of mothers and 50% of fathers in this group have no earnings from work, and around 60% of both mothers and fathers have not completed high school.

Table 1 Mean of the variables in the analysis sample

	Newly-arrived (1)	Others (2)	Difference (1)−(2)	Test of diff. (p-value)
Age and gender				
Child's age	3.86	3.73	0.13	0.00
Child is a boy	0.51	0.51	0.00	0.17
Birth country (the most common)				
Child born in Sweden	0.22	0.96	−0.74	0.00
Mother born in Sweden	0.00	0.57	−0.57	0.00
Father born in Sweden	0.00	0.53	−0.53	0.00
Child born in Syria	0.13	0.00	0.13	0.00
Mother born in Syria	0.20	0.03	0.17	0.00
Father born in Syria	0.19	0.03	0.17	0.00
Child born in Iraq	0.03	0.00	0.03	0.00
Mother born in Iraq	0.06	0.05	0.02	0.00
Father born in Iraq	0.06	0.05	0.01	0.00
Child born in Somalia	0.01	0.00	0.01	0.00
Mother born in Somalia	0.04	0.04	−0.00	0.00
Father born in Somalia	0.03	0.04	−0.01	0.00
Reason for residence				
Child refugee	0.18	0.00	0.18	0.00
Mother refugee	0.23	0.08	0.15	0.00
Father refugee	0.28	0.13	0.15	0.00
Child refugee-connection	0.20	0.08	0.12	0.00
Mother refugee-connection	0.23	0.20	0.03	0.00
Father refugee-connection	0.08	0.11	−0.03	0.00
Mother, work/study	0.24	0.04	0.20	0.00
Father, work/study	0.25	0.05	0.20	0.00
Earnings and education				
Mother zero earnings	0.88	0.51	0.37	0.00
Father zero earnings	0.50	0.23	0.27	0.00
Earnings of mother (2023 SEK in 1,000)	22.91	107.61	−84.70	0.00
Earnings of father (2023 SEK in 1,000)	182.20	328.64	−146.44	0.00
No high school (mother)	0.63	0.25	0.38	0.00
No high school (father)	0.56	0.26	0.30	0.00
Some tertiary education (mother)	0.25	0.41	−0.16	0.00
Some tertiary education (father)	0.29	0.34	−0.05	0.00
Total number of children:	24,321	96,568		

Note: The sample includes children who turned 3, 4 or 5 in 2015–2023 and who were not enrolled in preschool in the spring of that year. Newly-arrived families are families who have been in Sweden less than 5 years when the child is sampled. Earnings are measured in t-1.

Model specification. To estimate the causal effect of the policy, we use the following model:

$$y_{it} = \sum_{t=2015,\, t\neq 2022}^{2023} \beta_t 1[Year = t] \times Offer_i + \gamma Offer_i + \rho_t + \boldsymbol{\theta X}_i + \varepsilon_{it} \qquad (1)$$

y_{it} is an indicator for whether child *i* is enrolled in preschool in the fall of year *t*. $Offer_i$ indicates whether he/she belongs to the group that would receive an automatic offer if the policy were in place, thereby controlling for general differences in future enrollment decisions between the target and control children. β_{2023}, our parameter of interest, captures whether there is a differential impact of belonging to the target group after the new policy was introduced in 2023, thereby capturing the effect of receiving an automatic preschool offer, under the assumption that enrollment trends would have developed in parallel in the absence of the policy change. Importantly, to the best of our knowledge, there were no other policy changes in 2023 that could have had a differential impact on the enrollment rates in the treatment and control groups. Identification further requires that the families in the control group were not indirectly affected by the offers given to the treatment group, i.e. the classic SUTVA assumption. Since we expect limited social interactions between families in the treatment and control groups, this assumption is likely to be fulfilled. Note that both the target and control group received information about universal preschool and their eligibility, which means that our effect estimate captures the impact of the automatic offer over and above this informational component.[14] β_{2015}–β_{2021} are placebo estimates, providing a test of the parallel trends assumption.[15] ρ_t represent calendar year fixed effects, and ε_{it} is the error term. Our preferred specification also includes several pre-determined covariates to account for compositional changes in the sample over time.[16] This is crucial in this context, as migration flows to Sweden have shifted significantly during our sampling period (particularly in the pre-pandemic years) in response to geopolitical conflicts and changes in national migration policies. By controlling for these factors, we only need to assume parallel trends conditional on these observables. Standard errors are clustered at the municipality level, as the policy is implemented by the municipalities.

[14] In section 5, we present results – based on an alternative difference-in-differences strategy – suggesting that the information campaign alone had no impact on enrollment.

[15] Some of the placebo years overlap with the COVID-19 pandemic (officially classified as a societal threat in Sweden from February 2020 to March 2022). However, as Sweden kept education for younger children open as usual throughout the pandemic (see, e.g., National Agency for Education 2022; and Hall, Hardoy, and Lundin 2022 for a description of how the pandemic affected the Swedish education system), we have no strong reason to believe that the pandemic introduced significant compositional shifts in the population of non-enrolled children that would render these years unsuitable for placebo analysis.

[16] We control for: immigration timing, birth country of the child and the parents, type of residence permit of the child and the parents, municipality fixed effects, parental age, education and income, and age, gender and birth month of the child. In Table A3, we show that we obtain a similar treatment effect also without these background controls.

As a complement, we also provide estimates from a regression discontinuity (RD) design that exploits the immigration date cutoff for policy eligibility (less than 5 years of residency in Sweden). The immigration date cutoff (January 1, 2018) can only be used in an RD-setting when the child is born in Sweden[17] (in 2018–2020) and both parents are born abroad. In our sample, there are 472 children born in Sweden in 2018–2020 whose parents immigrated after the cutoff (and thus received a preschool offer) and 4,006 children born in Sweden in 2018–2020 whose parents immigrated before the cutoff. Restricting the sample to families who immigrated in 2017 or 2018 (i.e., a one-year bandwidth), leaves us with 234 children who received an offer and 269 children who did not.

Finally, we present results from an alternative difference-in-differences strategy that, instead of comparing 3–5-year-olds who were eligible or ineligible for the policy based on migration background (newly-arrived versus not), compares changes in enrollment rates over time across different ages within the group of newly-arrived children. Specifically, we compare changes over time for 3-year-olds (eligible for the offer after 2023) and 2-year-olds (who remained ineligible), controlling for the same set of background characteristics as in our main specification. Note that, in contrast to our main model in which the control group also received information about preschool and their entitlement, the control group in this age-based difference-in-differences approach (the 2-year-olds) was completely unaffected by the new policy. This allows us to examine the importance of the information component of the new policy.

4 Results

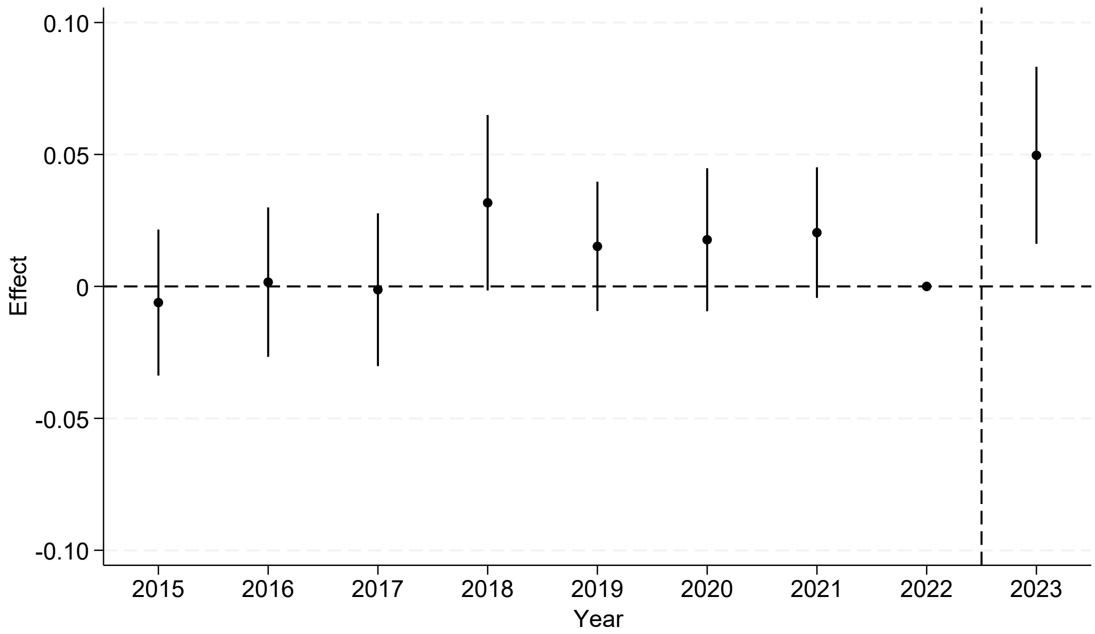
4.1 Main result

Figure 2 presents the β-estimates from equation (1), showing that receiving an automatic preschool offer increases the enrollment rate by 5 percentage points (see the estimate for 2023). This effect is significant at the 1% level. Relative to the enrollment rate for the target group in the year before the policy was introduced (0.29; see col. 2 in Table A2), the effect corresponds to an approximate increase of 17%. The placebo estimates are closer to zero and are all statistically insignificant at the 5% level (only 2018 is significant at the 10% level). An F-test fails to reject that the placebo estimates are jointly equal to zero (p-value = 0.4476), providing support for the validity of the identifying assumption. Moreover, the 2023-estimate remains significant at the 5%

[17] All children who were born abroad in 2018–2020 and then immigrated to Sweden have been in Sweden less than 5 years and thus there is no relevant cutoff for children born abroad. Since most children targeted by the policy are born abroad, we cannot use the RD design as our main empirical strategy.

level when all pre-reform years (i.e., 2015–2022), rather than just 2022, are used to define the reference period.[18]

Figure 2 Effects of the default enrollment policy on enrollment



Notes: The figure shows estimates of β_{2015}–β_{2023} from equation 1. Year 2022 is the reference year. The estimates for years 2015–2021 are placebo estimates. 120,882 observations are included in the estimation. The vertical lines are 95% confidence intervals based on standard errors clustered at the municipality level.

In the subsections that follow, we test the robustness of this result to a range of sample restrictions and alternative specifications. We also examine whether responses to the policy vary by child and parental characteristics.

4.2 Robustness checks

Since the policy was implemented by municipalities, and the regulations allow some room for interpretation regarding implementation details, we examine whether the results are sensitive to how we define preschool enrollment and the target group. We also test robustness to the set of control variables included and the choice of control group. The results from these robustness checks are presented in Table A3. Finally, we discuss the results from the RD-strategy, which relies on a different identifying assumption. These results are provided in appendix B.

The policy requires municipalities to reserve preschool slots from the beginning of the fall semester (late August) and hold them for one month, or until parents respond. We observe enrollment as of October 15 – more than 6 weeks later – which should imply that recorded enrollments

[18] The estimate is 0.0389 with a standard error of 0.0160.

reflect actual enrollment, not just reserved slots. However, we do not know the exact timing of when municipalities allocate slots. If some municipalities allocate slots with start dates in late September, it is possible that some children recorded as enrolled may still have had only a reserved slot awaiting parental response. To address this concern, we perform an estimation where children with a start date after September 14 are excluded. Reassuringly, the results remain very similar (Table A3, column 1).

Another potential concern is that some families in our sample may have emigrated from Sweden, even though we require the children to be registered as residents both the year before treatment and the treatment year. First, conditioning on remaining registered in Sweden until the end of 2023 turns out not to be crucial; the results are similar without this restriction (column 2). Second, it is possible that some individuals emigrated without notifying the authorities. To assess whether our results are sensitive to this issue, we restrict the sample to families with positive disposable income in the year before treatment (about 94% of the original sample), as this indicates continued presence in the country. The results are very similar to the baseline estimates also with this restriction (column 3).

To make the target and control groups more similar, we perform an analysis where the control group is restricted to only include children with foreign-born parents. This restriction reduces the sample size by half but does not change our findings (column 4). In column (5), we further restrict the control group to parents who immigrated to Sweden no more than 10 years ago. Again, the point estimate remains stable although statistical significance is lost due to worse precision. We also obtain similar results if we estimate a model without controls for background characteristics (column 6).

To further test the validity of this empirical design, we present results from a placebo analysis, in which we estimate the same difference-in-differences model using a sample consisting of all 1- and 2-year-old children not enrolled in preschool. Children in newly-arrived families in this age group did not receive automatic preschool offers in 2023; thus, there is no reason to expect changes in enrollment over time for this group relative to other 1–2-year-olds currently not enrolled. Indeed, the placebo estimate is close to zero and statistically insignificant (column 7).

Alternative strategy: a regression discontinuity design. As discussed in section 3.2, an RD design can be applied to children born in Sweden in 2018–2020 whose parents are both foreign-born and who were not enrolled in preschool in the spring of 2023. In this research design, the parents' immigration date relative to January 1, 2018 serves as the running variable.[19] If the parents immigrated after January 1, 2018, their child was targeted by the automatic offer policy

[19] If the parents immigrated on different dates, we use the earlier date to the define the running variable. As noted in section 3.2, the exact cutoff date is not formally stipulated in the law but reflects the cutoff we identified from communication with some municipalities regarding the implementation of the 5-year rule.

in 2023. Conversely, if the parents immigrated before January 1, 2018, they only received information about preschool and their child's entitlement to attend preschool 15 hours per week free of charge. Relying on the assumption that the exact immigration date around January 1, 2018, is as if randomly assigned, we exploit this policy discontinuity to estimate the effect of automatically being offered a preschool slot on subsequent preschool enrollment.

The RD estimation is shown visually in Figure B1. In the baseline specification, we use a 1-year bandwidth and include a linear control for the running variable, allowing the slope to differ on each side of the cutoff. We employ a uniform kernel and robust standard errors. As shown in the figure, the discontinuity at the cutoff (i.e., the estimate of the effect of the automatic offer) is almost 10 percentage points.[20] Hence, the estimate is qualitatively consistent with our main result from the difference-in-differences model, although it is not statistically significant.

The conditions for an RD analysis are not ideal, as the number of observations close to the cutoff is rather limited (see Figure B2). That said, we find no evidence of bunching or discontinuities in predetermined background characteristics around the cutoff (Figure B2 and Figure B3), which supports the validity of the design.[21] Moreover, varying the model specification – by changing the functional form, kernel, or bandwidth – yields qualitatively similar results, although the point estimates vary in magnitude (generally exceeding our baseline estimate), and are often not statistically significant (see Table B1).

4.3 Heterogeneous treatment effects

So far, we have shown that automatic preschool offers, on average, increase enrollment rates in the target group. Given that non-enrollment tends to be more prevalent among children with disadvantaged backgrounds (see, e.g., Table A1 and OECD 2025), an important question is whether the most vulnerable families within the target group responded to the policy. While the target group of newly-arrived immigrant families is disadvantaged as a whole, there is also heterogeneity within this group; some families face greater barriers to labor market integration and self-sufficiency, particularly those with lower educational attainment and those who immigrated for refugee-related reasons (as opposed to work or study). We therefore examine heterogenous effects along these two dimensions, splitting the sample by the mother's educational attainment (any tertiary education versus less, and at least high school versus less) and her reason

[20] The exact estimate is 0.098 with a robust standard error of 0.074.
[21] Conducting placebo-analyses by estimating the same RD-regression for the years prior to the reform, yields estimates that, for the most part, are closer to zero and statistically insignificant; see Figure B4.

for migration (refugee-related versus other reasons).[22] We then estimate our main difference-in-differences specification (equation 1) separately for these subgroups.

The results are presented in Table 2 where we show the estimate of β_{2023} for the different subgroups. For perspective, remember that our sample consists of children who were not enrolled in preschool during the spring. Although the share of non-enrolled children who start preschool in the fall are relatively similar across the different groups (see row "Mean in 2022" in Table 2), the share of children in newly-arrived immigrant families already enrolled (and who are therefore not in our sample) differs significantly depending on both the mother's education level and the mother's reason for immigration. For example, among newly-arrived families, the preschool enrollment rate of 3–5-year-olds is 87.6% if the mother has some tertiary education compared to 70.3% if she lacks high school education, and 85.6% if the mother's reason for migration is refugee-related (including being the relative of a refugee) compared to 77.8% if she received residence permit for other reasons.

With this selection into the analysis sample in mind, we return to the estimates and observe that the policy seems to have a greater impact if the mother is more educated and when the family arrived for other than refugee-related reasons. In all cases, however, estimates for the different groups are not significantly different from each other, suggesting that the results should be interpreted with caution. That said, the results suggest that, if anything, the automatic preschool offers may have been less effective in increasing enrollment among children from the most socioeconomically disadvantaged backgrounds – who may have the most to gain from preschool attendance.

Table 2 Heterogeneity based on mother's education and reason for migration

Column: Sample:	(1) Any tertiary	(2) No tertiary (or missing)	(3) At least high school	(4) No high sch. (or missing)	(5) Refugee	(6) Not refugee
DiD-effect	0.0812*** (0.0280)	0.0377 (0.0234)	0.0720*** (0.0254)	0.0186 (0.0298)	0.0129 (0.0326)	0.0594*** (0.0164)
Mean in 2022[a]	0.2576	0.2955	0.2676	0.2953	0.3010	0.2779
Observations	45,336	75,537	81,342	39,532	38,242	82,625

Note: The table shows estimates of β_{2023} from equation 1. Standard errors clustered on municipality are in parentheses and */**/*** refers to statistical significance at the 10/5/1 percent level. [a]Mean in 2022 refers to the mean outcome for the newly-arrived, i.e. the share starting preschool in the fall among those who were not enrolled in the spring.

[22] 25% of the mothers in the target group have some tertiary education, compared to 40% in the control group (see Table 1). 46% of the mothers in the target group immigrated for refugee-related reasons, a substantially larger share than for the control group (28%).

We have also examined potential heterogeneity in responses to the policy by the child's gender and age group, distinguishing between 3-year-olds and 4–5-year-olds; see Table A4. The results are similar across these groups (significant effects of around 4–6 percentage points), showing that the results are not driven by a specific age group or gender.

5 Administrative hurdles versus information deficits

While preschool participation has been shown to benefit children from disadvantaged backgrounds (e.g., Duncan et al. 2023), less is known about why non-enrollment remains more common among these children, even when preschool is free and universal. Understanding the reasons behind this pattern is essential for designing effective policies to increase enrollment. In this section, we shed light on the underlying mechanisms behind non-enrollment.

The policy of automatic preschool slot offers reduces the administrative hurdles associated with preschool application, but it also informs (or reminds) parents that their child is eligible for free universal preschool, that preschool is beneficial for their child, and that most children aged 3 and older attend. Here, we use an age-based difference-in-differences strategy to investigate the relative importance of removing administrative hurdles versus providing information.

First, we compare children in newly-arrived immigrant families who differ in policy eligibility based on age (instead of time since immigration). Specifically, we compare changes in enrollment over time for 3-year-olds (who were targeted by the automatic offer and the information campaign after 2023) and 2-year-olds (who were never targeted). Again, the sample consists of children who were not enrolled in preschool in the spring of the year under investigation. Note that the comparison between treated and controls in this analysis differs from the one in the main specification: In the main analysis, the comparison is between children who received an automatic preschool offer and those who did not, within a group that had all been targeted with information about universal preschool and their eligibility.[23] Thus, our main specification captures the effect of the automatic offer on top of this information. Here, we capture the combined effect of both the information campaign and the preschool offer.

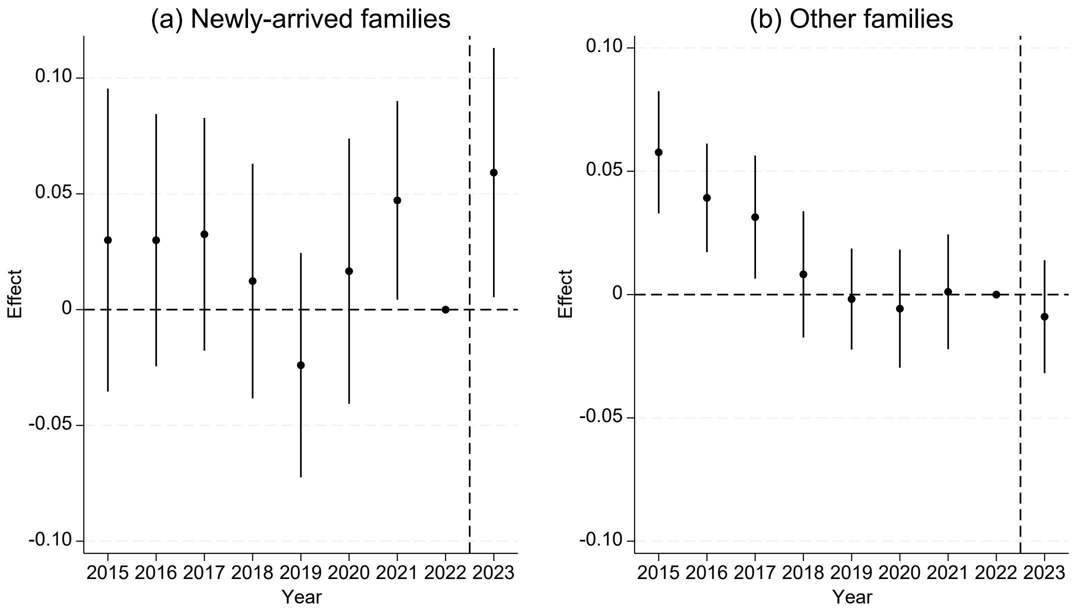
Panel (a) of Figure 3 presents the results. As before, the estimate for 2023 captures the effect of interest, while the estimates for earlier years serve as placebo tests. The result for 2023 is very similar to the estimate from our main analysis, although the placebo estimates (particularly the one for 2021) indicate that this is a somewhat less reliable empirical design. The fact that the effect is similar suggests that the information component alone had limited impact on enrollment.

[23] As described in section 2.2, all 3–5-year-olds not enrolled in preschool received this information.

To further shed light on the role of information, panel (b) of Figure 3 presents results from the same age-based analysis for non-enrolled children *not* belonging to the newly-arrived immigrant category (i.e., children with Swedish-born parents or immigrant parents with longer residence in Sweden than 5 years). For parents of 3-year-olds in 2023, the treatment in this case consists only of receiving information about their child's entitlement to universal preschool and the benefits of preschool for children. Again – as indicated by the small and insignificant effect for 2023 – the results suggest that providing information alone did not affect enrollment, although the presence of some significant placebo tests for the earliest years calls for some caution in the interpretation.

Taken together, the results presented in this section suggest that the administrative burden associated with applying for a preschool slot is more important than lack of information about eligibility for explaining why some children do not attend universal preschool. Hence, additional information campaigns are unlikely to significantly increase enrollment among the currently non-enrolled.

Figure 3 Effects of the policy change in 2023 on preschool enrollment. Estimates based on an age-based difference-in-differences design



Note: The figure shows estimates from a difference-in-differences analysis that compares changes over time for 3-year-olds (eligible after 2023) and 2-year-olds (ineligible). Year 2022 is the reference year. The estimates for years 2015–2021 are placebo estimates. The analysis in panel (a) is based on 29,300 observations. The p-value of the F-test that the placebo estimates are jointly equal to zero is 0.1096 for the analysis in panel (a). The analysis in panel (b) is based on 240,128 observations. The p-value of the F-test that the placebo estimates are jointly equal to zero is 0.0000 for the analysis in panel (b).

6 Conclusion

Children who are expected to benefit the most from universal preschool are less likely to attend (e.g., Cornelissen et al. 2018). In this study, we provide insights into why this remains the case even when preschool is free and available for all, and we demonstrate how policy can increase enrollment. To this end, we evaluate a policy that offers a reserved slot in free universal preschool to immigrant children, even if their parents have not applied for one. Parents only need to respond to the offer; no additional application is required. The policy targets children aged 3–5 who are *not enrolled* in preschool and who are either foreign-born themselves or have foreign-born parents who immigrated to Sweden within the past five years.

Our main conclusion is that automatic preschool offers significantly increase enrollment: the likelihood of enrolling the next semester rises by 5 percentage points, a 17 percent increase from a baseline of 29 percent. Further analysis suggests that this effect is driven by the removal of administrative hurdles associated with preschool application rather than the informational content of the offer. Hence, simplifying the application process appears to be a more effective policy tool for increasing preschool participation among immigrant families than providing information alone.

While the policy was effective in increasing enrollment, the majority of the targeted children still did not respond. Our results also indicate that the policy may have been less effective in increasing enrollment among children from the most disadvantaged backgrounds, who arguably have the most to gain from preschool participation. Given that all 3–5-year-olds are entitled to universal preschool, free of charge, and that virtually all administrative and informational barriers were removed thanks to the automatic offers, we interpret the remaining non-enrollment among newly-arrived families as reflecting either preferences for alternative forms of childcare or other types of barriers, such as lack of trust in institutions. These explanations seem plausible for newly-arrived immigrants, who may have limited familiarity with Swedish institutions. In this context, it is noteworthy that enrollment rates are rather similar among natives and children with immigrant parents who have lived in Sweden for more than 5 years (see Figure 1).

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Appendix A: Additional tables

Table A1 Background characteristics of children aged 3–5 in 2022, by preschool enrollment status.

	Enrolled	Not enrolled
Child born in Sweden	0.963	0.777
Mother born in Sweden	0.692	0.380
Father born in Sweden	0.681	0.374
Mother zero earnings	0.131	0.524
Father zero earnings	0.070	0.257
Earnings of mother	269,696	120,409
Earnings of father	431,674	303,748
No high school (mother)	0.102	0.326
No high school (father)	0.131	0.287
Some tertiary education (mother)	0.581	0.417
Some tertiary education (father)	0.438	0.388
Child has younger sibling	0.435	0.464
Number of siblings	1.371	1.704
Parents separated	0.138	0.172
Mother on welfare	0.035	0.081
Father on welfare	0.027	0.059
Age of mother	35.029	34.566
Age of father	37.943	38.439
Observations	332,469	15,006

Note: The table is based on children born 2017–2019 who were registered as residents in Sweden on December 31, 2022. Preschool enrollment is measured on October 15, 2022. The income variables are measured in 2022. The statistics are based on own calculations using nationwide register data from Statistics Sweden.

Table A2 Number of children and share in preschool (in the fall) per year and treatment category

Year	Newly-arrived		Other	
	(1)	(2)	(3)	(4)
	# children	Share in preschool (fall)	# children	Share in preschool (fall)
2015	2,508	0.42	12,117	0.33
2016	2,594	0.43	11,268	0.33
2017	3,662	0.49	11,010	0.32
2018	3,317	0.42	10,956	0.31
2019	2,913	0.35	10,367	0.32
2020	2,657	0.33	10,337	0.31
2021	2,346	0.32	10,436	0.32
2022	2,235	0.29	10,112	0.29
2023	2,089	0.34	9,965	0.29

Note: The sample includes children who turned 3, 4 or 5 in the respective years and who were registered as residents in Sweden on December 31 in the current year as well as in the preceding year. In addition, the children should not be enrolled in preschool in the spring of the respective years. Newly-arrived families are families who have been in Sweden less than 5 years when the child is sampled.

Table A3 Robustness checks

Column: Model:	(1) <sep15	(2) t-1 sample	(3) Inc. in t-1	(4) Foreign	(5) Foreign <10 years	(6) No controls	(7) Placebo
DiD-effect	0.0517***	0.0592***	0.0421**	0.0427**	0.0414	0.0588**	0.0013
	(0.0156)	(0.0152)	(0.0176)	(0.0184)	(0.0266)	(0.0238)	(0.0074)
Mean in 2022[a]	0.2509	0.2477	0.3105	0.2855	0.2855	0.2855	0.4136
Observations	116,374	132,504	113,200	57,321	36,800	120,889	1,133,838
Baseline (from Figure 2)	0.0497*** (0.0171)						
Mean in 2022[a]	0.2855						
Observations	120,882						

Note: The table shows estimates of β_{2023} from equation 1. Standard errors clustered on municipality are in parentheses and */**/*** refers to statistical significance at the 10/5/1 percent level. The lower panel shows the baseline estimate from Figure 2. Col. (1) excludes children with a start date later than September 14. Col. (2) only requires that the child is registered in Sweden on December 31 in t-1 to be included in the sample. Col. (3) requires that the family has some disposable income in t-1 to be included in the sample. Col. (4) restricts the control group to children with foreign-born parents. Col. (5) further restricts the control group to children with foreign-born parents having migrated to Sweden less than 10 years ago. Col. (6) shows the estimate from a model without controls. Col. (7) shows results from a placebo-regression on 1–2-year-olds. [a]Mean in 2022 refers to the mean outcome for newly-arrived, i.e. the share starting preschool in the fall among those who were not enrolled in the spring.

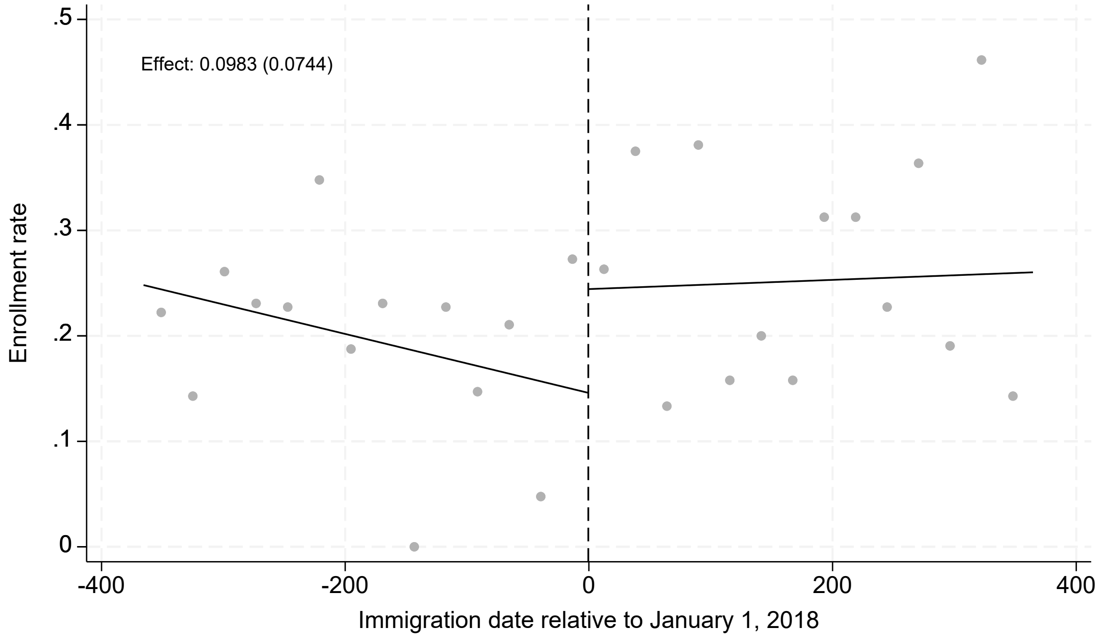
Table A4 Heterogeneity based on child characteristics

Column: Sample:	(1) Boys	(2) Girls	(3) Age 3	(4) Age 4–5
DiD-effect	0.0451**	0.0544**	0.0602**	0.0409*
	(0.0217)	(0.0237)	(0.0244)	(0.0213)
Mean in 2022[a]	0.2839	0.2870	0.3552	0.2345
Observations	61,250	59,624	58,152	62,721

Note: The table shows estimates of β_{2023} from equation 1. Standard errors clustered on municipality are in parentheses and */**/*** refers to statistical significance at the 10/5/1 percent level. [a]Mean in 2022 refers to the mean outcome for the newly-arrived, i.e. the share starting preschool in the fall among those who were not enrolled in the spring.

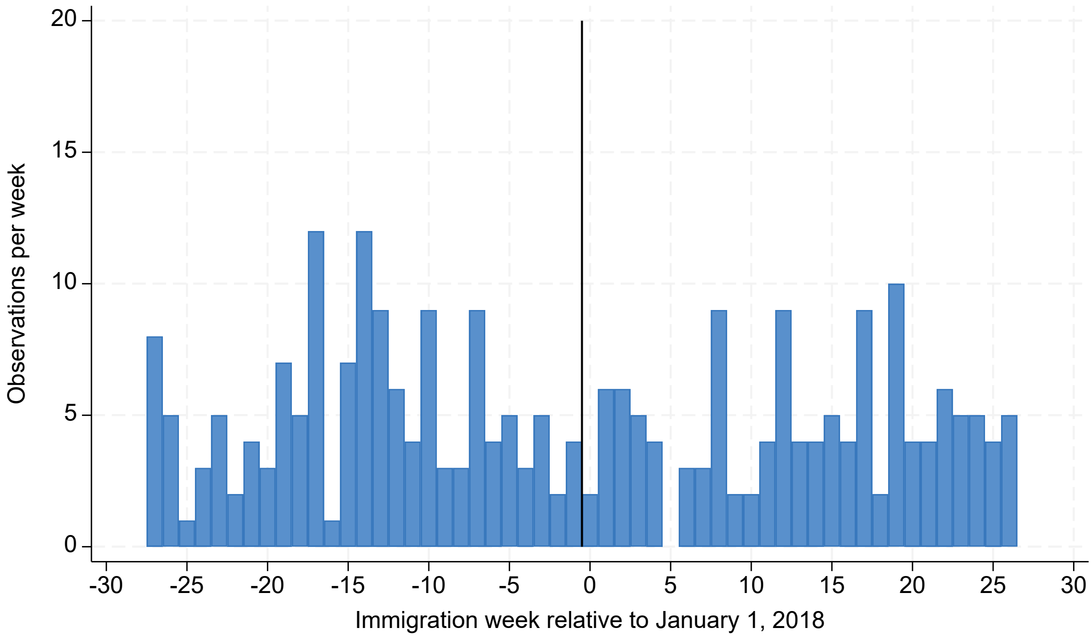
Appendix B: Analysis based on a regression discontinuity design

Figure B1 RD-plot for preschool enrollment in the fall of 2023



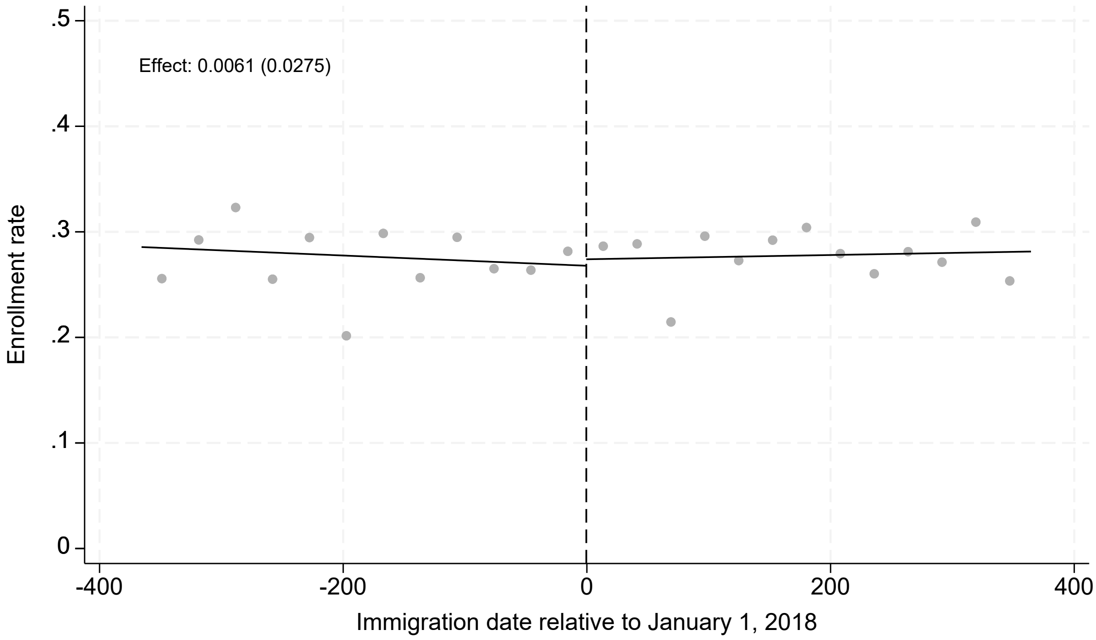
Note: The sample in the RD estimation is based on children born in Sweden 2018–2020 who were not enrolled in preschool in the spring of 2023. Both parents are required to be born abroad and the sample is restricted to parents who immigrated to Sweden in 2017 or 2018. The sample consists of 269 children to the left of the cutoff (did not receive the automatic offer) and 234 children to the right of the cutoff (received the automatic offer). The outcome takes the value 1 if the child was enrolled in preschool in the fall of 2023 and 0 otherwise. The RD model in the figure is based on a linear specification with a uniform kernel. A 365-day bandwidth is used. The estimate of the discontinuity at the cutoff is printed in the upper left corner, with the robust standard error in parenthesis.

Figure B2 Number of children per week (parents' week of immigration relative to cutoff)



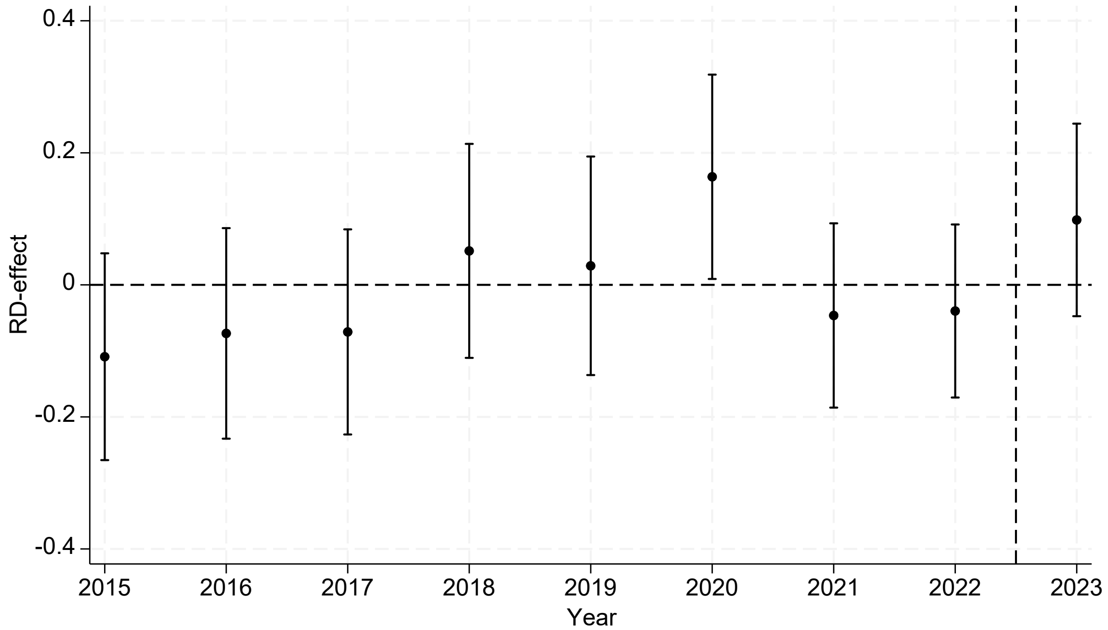
Note: The sample is based on children born in Sweden 2018–2020 who were not enrolled in preschool in the spring of 2023. Both parents are required to be born abroad and the sample is restricted to parents who immigrated to Sweden in 2017 or 2018. The sample consists of 269 children to the left of the cutoff (did not receive the automatic offer) and 234 children to the right of the cutoff (received the automatic offer). The figure shows the number of children whose parents immigrated in a certain week relative to January 1, 2018.

Figure B3 RD-plot for predicted enrollment based on predetermined characteristics



Note: To construct a composite measure of predetermined characteristics that are relevant for preschool enrollment, we used data from 2015–2022 and regressed preschool enrollment on a set of predetermined characteristics. For both mothers and fathers we include birth country, education, income, reason for residence and age. In addition, we include age and gender of the child. We then used the coefficients from the model to predict preschool enrollment for the 2023 sample. The 2023 sample is the same as in Figure B1. The RD model in the figure is based on a linear specification with a uniform kernel. A 365-day bandwidth is used. The estimate of the discontinuity at the cutoff is printed in the upper left corner, with the robust standard error in parenthesis.

Figure B4 Placebo (and actual) RD-effects (linear model, uniform kernel, 365-day bandwidth)



Note: The estimate for 2023 is the actual RD estimate, while the estimates for 2015–2022 are placebo estimates. The 95 % confidence intervals are based on robust standard errors. The RD model is based on a linear specification with a uniform kernel. A 365-day bandwidth is used.

Table B1 Sensitivity of the RD-estimate to model variations

Column:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. Linear model								
Effect	0.098	0.054	0.133	0.149**	0.122	0.102*	0.126	0.132**
	(0.074)	(0.054)	(0.110)	(0.066)	(0.082)	(0.058)	(0.125)	(0.066)
Observations	503	1093	253	693	503	1093	253	844
Kernel	Uniform	Uniform	Uniform	Uniform	Triang.	Triang.	Triang.	Triang.
Bandwidth (days)	365	730	180	449[a]	365	730	180	536[a]
Control intercept	0.146	0.176	0.173	0.117	0.134	0.136	0.171	0.121
B. Quadratic model								
Effect	0.165	0.176**	0.115	0.142	0.154	0.164*	0.093	0.161*
	(0.120)	(0.081)	(0.172)	(0.093)	(0.129)	(0.086)	(0.192)	(0.088)
Observations	503	1093	253	873	503	1093	253	1062
Kernel	Uniform	Uniform	Uniform	Uniform	Triang.	Triang.	Triang.	Triang.
Bandwidth (days)	365	730	180	550[a]	365	730	180	701[a]
Control intercept	0.111	0.0753	0.168	0.118	0.144	0.107	0.193	0.113

Note: Robust standard errors are in parentheses and */**/*** refers to statistical significance at the 10/5/1 percent level. The estimate in column (1) of panel A corresponds to our baseline estimate depicted in Figure B1 [a] Optimal bandwidth (given other model options).

Appendix C: Example of offer

Figure C1 Example of offer from the municipality of Uppsala, which was sent out in both Swedish and English

Uppsala kommun

The preschool works for your child to play, develop and prepare for school

Welcome to preschool

Uppsala municipality is providing your child a place in the preschool.
The place is optional. The place is free of charge. You pay nothing.
On September 2, your child can start preschool.

Preschool is fun, safe and educational for all children.
Your child develops and learns language, numbers and games.
The preschool has trained staff on site.
Preschool is good for your child prior to starting school.
Most children in Sweden do attend preschool.

How to respond to a place offer

In the letter with the offer there is a reply stub that you fill in and send back with the enclosed envelope. You make the decision yourself whether your child should start preschool or not. It is important that you answer with a yes or no to the offer of a place in preschool.
Scan the QR-code or read more at uppsala.se/allkids
Or call 018-727 00 00

uppsala.se/allkids