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Increased flexibility in childcare arrangements: impacts on parents' careers and children's school performance

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Increased flexibility in childcare arrangements: impacts on parents' careers and children's school performance[a]

by

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Abstract

We study the effects of access to a child home care allowance on parents' labor supply and childcare decisions, as well as their longer-term earnings trajectories and children's school performance. To establish causality, we exploit variation in the availability of the allowance over time and across municipalities. The analysis is based on rich administrative data for the entire Swedish population. Our results suggest a negative impact of benefit eligibility on mothers' labor supply and earnings as well as on children's enrollment in childcare during the time the family is entitled to the benefit (until the child turns 3 years old). Negative impacts on mothers' earnings persist after benefit eligibility has expired and are still visible 7 years after childbirth. These effects are driven by mothers who lacked earnings before childbirth, while there are no lasting impacts for mothers with prior employment. We find no effects on fathers' earnings. As for the children, our results suggest that access to the home care allowance had a negative impact on boys' scores on standardized tests at age 10, while there are no significant effects on girls' test scores.

Keywords: Home care allowance; labor supply; earnings; childcare; preschool; school performance
JEL-codes: J13, J18, J24

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1 Introduction

Subsidized childcare of high quality is often argued to be critical for both increasing female labor supply and for creating more equal opportunities for children from diverse backgrounds (e.g., Olivetti and Petrongolo 2017). However, an argument raised against subsidizing childcare, is that parents are best equipped to decide the appropriate care for their child, and that it is unfair that all parents must pay, through taxes, for a universal solution some do not desire for their own child (Duvander and Ellingsaeter 2016).[1] Consequently, some countries have introduced a child home care allowance (HCA), providing financial support for families who choose to not use subsidized childcare.[2] HCA schemes have in turn been criticized for being harmful for women's careers, and also, in some cases, for children, who may have benefited from preschool activities rather than home care (ibid). Prior studies of HCA:s have primarily focused on short-term labor supply responses (e.g. Naz 2004; Schøne 2004; Kosonen 2014; Giuliani and Duvander 2017); more knowledge about how the provision of HCA affects women's long-run careers as well as children's school outcomes is needed to assess the validity of these arguments.

In this paper, we study the impact of providing an HCA in Sweden. The Swedish HCA could be used when the child was between ages 1 and 3, and enabled parents to prolong their job-protected parental leave.[3] To receive the benefit, which amounted to around 300 euros per month, the child could not attend subsidized childcare. Sweden is characterized by a strong norm to use formal childcare from an early age: around 90 % of all 2-year-olds are enrolled in center-based childcare (Swedish National Agency for Education 2021), and most mothers stay in the labor force after giving birth. Being a 'stay-at-home mom' is in fact a rare phenomenon, as illustrated by the employment rate among women being just a few percentage points (pp) lower than that of men.[4] Swedish formal childcare is of high quality in international comparisons (e.g. OECD 2014) and childcare/preschool centers[5] have the dual tasks of both enabling parents to combine parent-hood with work and to promote children's development and learning. Childcare is also heavily subsidized: parents only pay a small fraction of the total cost of a childcare slot. Hence, intro-ducing an HCA in this context implies an opportunity for parents to choose an alternative child-care arrangement than the common norm. We document how the HCA is used and investigate

[1] See Swedish Social Insurance Agency (2013) for a summary of the debates in the Nordic countries in relation to providing a home care allowance.
[2] See Collischon et al. (2020) for a review of home care allowance (or 'cash for care') systems in Europe.
[3] As of 2016, the HCA is no longer in place. However, its re-introduction remains high on the political agenda of the most conservative parties.
[4] Since the early 2000s, the female employment rate has been 3–5 pp below that of men; see statistics from the Swedish Labor Force Surveys, https://www.ekonomifakta.se/Fakta/arbetsmarknad/Jamstalldhet/Kvinnor-pa-arbetsmarknaden/.
[5] We use the terms 'childcare' and 'preschool' interchangeably as early childhood education and childcare is combined in Sweden.

how benefit eligibility affects women's long-run earnings trajectory as well as children's performance in primary school.

We begin by presenting estimates of the direct effect of HCA eligibility on the take-up rate, parents' earnings while eligible, and children's enrollment in formal childcare, which helps us understand how the benefit is used. Thereafter, we move on to more long-run impacts on mothers' earnings trajectories up to 8 years following childbirth, and children's school results in 3rd grade (age 10). We also examine impacts on fathers' earnings and future fertility decisions. The analysis is based on rich administrative data for the full population, including yearly information on employment, earnings, benefit take-up and a wide range of demographic background variables. We have access to results from standardized national tests in Swedish and mathematics for all children in 3rd grade, and for around 40 % of the municipalities we can also observe childcare enrollment. To identify the causal impact of HCA eligibility, we exploit that the availability of this benefit has varied over time and across municipalities in a difference-in-differences analysis based on a matched sample of municipalities.

We show that eligibility for the HCA had a negative impact on mothers' labor supply and earnings as well as children's enrollment in childcare during the time the family could use the benefit. The estimated short-run average effect on earnings is rather small in magnitude, corresponding to a reduction of 6 % the second year after childbirth. However, considering that benefit take-up was also rather low (around 6 % among those eligible), the results clearly show that the HCA induced most recipients to stay home longer instead of working. Thus, the HCA does not appear to have represented a windfall gain to any large extent in the Swedish setting. More importantly, negative impacts on mothers' earnings remain when the family is no longer eligible for the HCA and are still visible 7 years after the child was born. Moreover, we find that these effects are driven by mothers who lacked earnings before childbirth, while there are no lasting impacts for mothers with a stronger labor market position. Hence, for mothers who were established in the labor market before giving birth, the option to extend the job-protected parental leave with the HCA did not affect their later earnings trajectory. However, for mothers without prior employment, the HCA delayed labor market entrance and had a lasting negative impact on earnings. We find no effect of HCA eligibility on fathers' earnings or fertility choices. As for the children, our results suggest that access to the HCA had a negative impact on boys' test scores, while there are no significant effects for girls.

Our study contributes to the literature on childcare prices and female labor supply, as the provision of an HCA changes the relative price of formal childcare vs. home care. How childcare prices affect female labor supply is a long-standing research question.[6] Studies that have exploited

[6] See Blau and Currie (2006) for a review of early work on this topic.

exogenous changes in childcare prices to identify causal effects have typically found small to modest labor supply responses (e.g., Brink et al. 2007; Lundin et al. 2008; Blau and Tekin 2007). There is also a closely related literature that specifically focuses on the provision of HCA in various European countries (see, e.g., Naz 2004 and Schøne 2004 for Norway; Kosonen 2014, Österbacka and Räsänen 2022, and Gruber et al. 2022 for Finland; Piketty 2005 for France; and Giuliani and Duvander 2017 for Sweden). This literature has mainly focused on short-term labor supply responses, and the results generally suggest modest effects on mothers' labor supply and no effects on fathers. An important contribution to this literature is that we are some of the first to employ a longer follow-up horizon, allowing us to assess not only immediate labor supply responses during the time the family is eligible for the benefit, but also whether the additional time out of work has lasting impacts on parents' career trajectories.[7] Understanding the potential long-term consequences of families' decisions regarding labor supply during the child's early years is important to be able to assess the full impact of providing this type of benefit.

Our findings on the long-term impacts of HCA provision for mothers' earnings also have implications for the child penalty literature (Angelov et al. 2016; Kleven et al. 2019). 'Child penalty' is commonly used to refer to the fact that children cause large reductions in earnings for mothers, but not for fathers. Importantly, our results contribute to the ongoing debate over whether family policy can reduce the size of the child penalty (e.g., Kleven et al. forthcoming; Andresen and Nix 2022). Kleven et al. (forthcoming) find that neither extensions of parental leave nor childcare subsidies have affected the child penalty in Austria. Andresen and Nix (2022) draw similar conclusions regarding parental leave extensions in Norway but find that publicly provided childcare reduces the child penalty. In line with Gruber et al. (2022), who study a similar HCA policy in Finland, our results imply that the HCA in Sweden increases the child penalty by negatively affecting female but not male earnings. Although the Swedish HCA is not widely used, we find negative effects on mothers' average earnings up to 7 years after childbirth. However, there is no increased child penalty in the long run for those mothers who were employed before childbirth and hence had an employer to return after the leave. In Sweden, paid parental leave is generally job-protected and the compensation largely depends on previous earnings, which has probably contributed to the high labor force participation among women both before and after childbirth. The HCA is an exception in this regard, and it is possible that this is precisely why we see long-term negative effects on women's earnings among those who were not established on the labor market before the extended parental leave.

Only a couple of studies have investigated how the provision of HCA affects children (see Gathmann and Sass 2018; Collischon et al. 2020; Gruber et al. 2022). Both Gathmann and Sass

[7] Gruber et al. (2022) show similar long-term negative labor market impacts for the Finnish HCA.

(2018) and Collischon et al. (2020) focus on Germany and conclude that the German HCA to a large extent represents a windfall gain for families that would have opted out of formal childcare regardless of the HCA, although it also led to a modest decline in childcare participation. Using data from school entrance exams, Collischon et al. (2020) find no evidence of any important impact on children's cognitive development. Conversely, Gruber et al. (2022) find that offering HCA in Finland increased home care and negatively affected early childhood development test results. The negative effects on children appear to persist into the teenage years: children impacted by the HCA are less likely to enroll in an academic high school track and have a higher likelihood of criminal convictions. Our study is closely related to these studies, but we study the provision of HCA in a context characterized by a strong norm to use formal childcare and a long tradition of high female labor supply also among mothers with small children.[8] This is likely to contribute to the relatively low take-up of HCA in the Swedish context; only 6 % of those eligible used the allowance, compared to 80 % in Finland (Gruber et al. 2022) and 60 % in Germany (Collischon et al. 2020).[9] For the Swedish families who used the allowance, we find a substantial reduction in childcare enrollment. In line with the results from Finland, we find that access to the HCA had a negative impact on school outcomes for boys, but we find no evidence of any impacts for girls.

Our study also contributes to the literature on early childhood education. Recent surveys conclude that early childhood education programs for children aged 3–5 tend to have both short- and long-run positive effects on educational outcomes, at least for disadvantaged children (Elango et al. 2015; Dietrichson et al. 2018; Duncan et al. 2022). For younger children (ages 0–2), the results are more mixed. Some findings suggest that one-to-one interactions with adults benefit young children's human capital development, and that home care therefore is better than center-based care for the youngest children; see, e.g., Fort et al. (2020) and Ginja et al. (2019). Other studies find that also children below age 3 benefit from center-based childcare, with the effects being stronger for children from disadvantaged backgrounds (Drange and Havnes 2019; Yamaguchi et al. 2018). Hence, although preschool for older children (above age 3) seems to be beneficial for their future academic progress, the jury is still out regarding the optimal age for letting young children start preschool/childcare. Our findings are in line with beneficial effects of formal childcare also before age 3 for children with less educated parents; however, we only observe this positive impact for boys.

[8] The norm to use formal childcare is much weaker in Germany: in 2016, only about one third of all children below age 3 was enrolled in formal childcare (Collischon et al. 2020). Gruber et al. (2022) state roughly similar numbers for Finland (see their online appendix).

[9] Differences in take-up rates across countries may also depend on compensation rates, childcare fees, and how the HCA interacts with the parental leave system. Based on the benefit amounts stated in these papers, the Swedish HCA appears to provide less financial support than the Finnish HCA but more than the German one.

The rest of the paper is organized as follows: We start by describing the institutional details surrounding the HCA and its implementation (Section 2) as well as its expected effects on labor supply, choice of childcare, and long-run outcomes (Section 3). Thereafter, we present the data (Section 4) and empirical strategy (Section 5). The results are presented in Section 6, and Section 7 concludes.

2 The child home care allowance

In an international comparison, Swedish children tend to start childcare/preschool at an early age (see, e.g., OECD 2017). Most enroll between ages 1 and 2 when the mother or father returns to work after a longer period of paid parental leave; parents are jointly entitled to 480 days of paid (and job-protected) parental leave per child, which can be used with a great amount of flexibility.[10] Close to 90 % of all 2-year-olds and 50 % of all 1-year-olds were enrolled in center-based child-care in 2014 (Swedish National Agency for Education 2014). The high enrollment rate is likely a result of great access to high-quality subsidized childcare: municipalities are obliged to provide childcare to all children from age 1 if the parents are working, studying, or applying for jobs. This also means that informal care plays an insignificant role (OECD 2021).[11]

A legislative change on July 1, 2008 gave municipalities the right to introduce a municipal home care allowance for parents who, instead of using formal childcare, wanted to care for their small child at home. The HCA made it possible to stay at home with job protection until the child turned 3 and at the same time receive a tax-free economic compensation. The center-right government justified the new law as a means of providing families with more options and increased flexibility when it comes to combining work and family life (Government Bill 2007/08:91). The introduction of this benefit had been strongly pushed for by a small conservative party, often (both locally and nationally) in coalition with other center-right parties. The allowance was withdrawn in February 2016[12] by the left-green coalition government, which feared that it led to worse labor market attachment among women with a weak position on the labor market (Government Bill (2014/15:147). However, re-introducing the allowance remains high on the political agenda of the most conservative parties.

[10] Although the uptake of parental leave by fathers has increased over time, mothers continue to take the majority of paid leave. For children born in 2016, the fathers' share of paid leave during the child's first two years of life amounted to 22 % on average (Swedish Social Insurance Agency 2020).
[11] According to OECD's Family Database, the use of informal care in Sweden is close to zero both for 0–2-year-olds, and 3–5-year-olds.
[12] After February 1, 2016 it was no longer possible to approve new applications.

The HCA amounted to max SEK 3,000 (~300 EUR) per month, which corresponded to 11 % of the mean wage in 2008[13] and more than twice of the maximum amount a family would pay for a (full-time) childcare slot.[14] Since the purpose of the allowance was to enable a longer period of care at home, a family could not receive the full amount if the child was enrolled in formal child-care. The allowance would also not be paid out if the parent at the same time received other benefits, such as unemployment, sickness, or parental leave benefits. However, the HCA could be combined with work as long as the child did not attend subsidized childcare. To be entitled to the HCA, the parent also had to reside in a municipality that had chosen to introduce the benefit and have used up at least 250 days of paid parental leave.

The share of municipalities offering HCA increased rapidly during the 12 months following the legislative change. Figure 1 shows the share of municipalities offering the allowance on an annual basis. Around 14 % of Sweden's 290 municipalities introduced the allowance already during the latter part of 2008 (usually towards the end of the year), a share that increased to 33 % in 2009, and to 38 % in 2010.[15] Thereafter, there are only minor year to year changes.[16]

[13] This is a vastly lower amount than the replacement rate provided during parental leave, both for employed parents, who received approximately 80 % of their wage in parental benefits, and non-employed, who received a flat rate of compensation amounting to SEK 5,490 per month. These rates are paid out during 390 days of parental leave. On top of these 390 days, both groups of parents were entitled to an additional 90 days of parental leave at a compensation of SEK 5,490 per month. The amounts cited in the text correspond to those valid in 2008.

[14] The fee for a childcare slot depends on family income and number of preschool children: A family pays at most 3 % of their monthly income for the first child, 2 % for the second, and 1 % for the third. There is cap on the fee corresponding to SEK 1,260 (EUR 126) for the first child, SEK 840 for the second, and 420 for the third child. The fourth child attends for free.

[15] Most municipalities that introduced the HCA in 2008 did so during the fall, with some as late as December. By contrast, the vast majority adopting the HCA in 2009 did so early in the year, typically in January (SCB 2012). Hence, the policy adoption was much less staggered than the figure (based on yearly data) suggests.

[16] The vast majority of the municipalities that introduced the HCA kept offering the allowance for the entire period; only around 10 % of the municipalities changed treatment status at some point between 2010 and 2015.

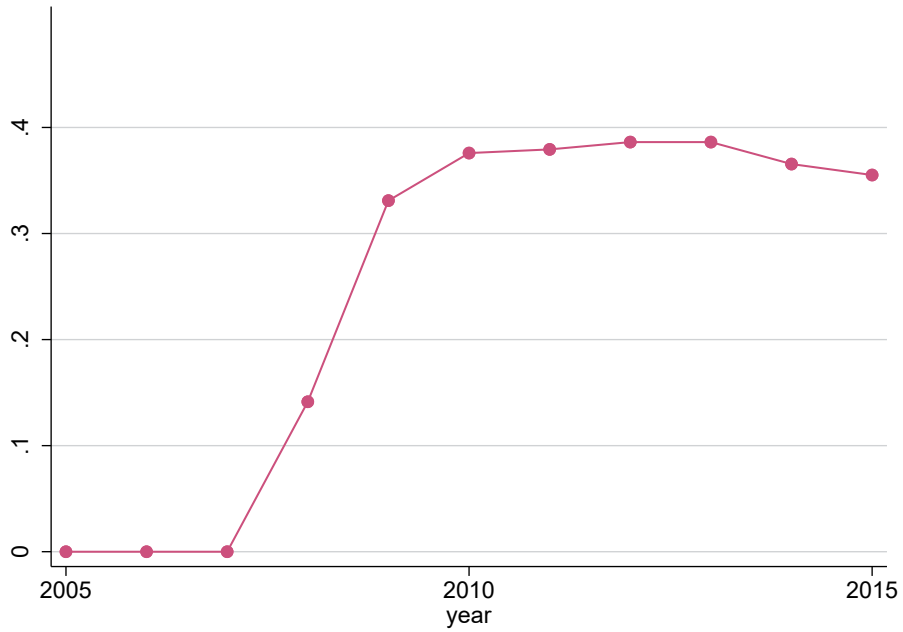

Figure 1. Share of municipalities offering home care allowance 2005–2015

Note: The figure shows the share of all Swedish municipalities that offered the HCA during at least some part of the year. *Source*: The data originates from Statistics Sweden (2012) and Statistics Sweden (2017).

Since it was a local political decision whether to introduce the HCA, the municipalities offering the allowance do not constitute a random selection of municipalities. In Section 5, we show how the probability that a municipality offers the HCA is related to a number of municipality characteristics and discuss the empirical strategy we use to account for these differences.

3 Expected effects on labor supply, choice of childcare, and long-run outcomes

As outlined in Gathmann and Sass (2018), an HCA is theoretically equivalent to an increase in the price of formal childcare, which is entirely compensated by an income subsidy. The implications of such a policy for families' childcare and labor supply choices in the short run will depend on their preferences for formal childcare. Some families would never consider using formal childcare for children below age 3. For this type of family, the introduction of an HCA represents a windfall gain of EUR 300. This will result in an income effect which may reduce the parents' labor supply (increase the demand for leisure). If the parents otherwise would have used informal childcare, it is possible that the income effect also reduces their demand for this type of care in favor of taking care of the child at home. However, informal childcare is rarely used in Sweden (OECD 2021), why this is unlikely to constitute an important margin in the Swedish setting.

For families who would have used formal childcare in the absence of the HCA, which is the group that represents most Swedish families, the introduction of this benefit implies an increase in the price of formal care in relation to the price of other childcare solutions. Essentially, these parents now pay EUR 300 more per month for (full-time) formal childcare – a large increase considering that the maximum fee for formal childcare was EUR 126 at the time. But the HCA at the same time compensates families for this price increase. Still, the altered relative prices might affect their choice of childcare. For these families, the demand for formal childcare can be expected to decline, and, as a consequence, the parents may reduce their labor supply to care for the child at home.[17]

Taken together, we expect that eligibility for the HCA on average reduces parents' (primarily mothers') labor supply and children's attendance in formal childcare in favor of being cared for at home.

Changes in labor supply and/or childcare arrangements due to the HCA may also have long-run consequences for both parents and children. Taking time out of work may have lasting effects on parents' labor market opportunities and earnings due to skill depreciation and/or foregone experience (e.g., Edin and Gustavsson 2008). It is important to acknowledge that, for parents who are employed, the HCA provides not only financial compensation but also job-protected leave. Hence, employed parents have the possibility to return to their previous job, which should, to some extent, protect them from an earnings penalty from extended leave. For non-employed parents, the earnings penalty can be expected to be larger as extended leave may also have a negative impact on their chances of getting hired. In both cases, we cannot exclude that a longer period at home also changes parents' preferences for work vs. home life; for instance, leading them to desire to work less (cf. Engström et al. 2006).

For the children, receiving care at home instead of formal center-based care may affect the development of both cognitive and non-cognitive skills, with potential lasting effects on school performance. The expected effects depend on how stimulating and enriching the home environment is in relation to the childcare environment, in terms of, e.g., learning activities and the quality and frequency of interactions with adults and peers. In general, children from disadvantaged backgrounds can be expected to benefit more from formal childcare compared to children from more advantaged backgrounds (see, e.g., the review by Duncan et al. 2022). If the HCA on average leads to lower parental earnings and thereby reduces family income, we can expect a negative impact on children's school results through this channel. Reduced income means that parents have fewer financial resources to invest in the child's environment, and could also imply economic

[17] In theory, it is also possible that they would switch from formal to an informal childcare solution (e.g., rely on grandparents or a nanny to care for the child). But, again, since informal childcare is rarely used by Swedish families, this is unlikely to constitute an important margin.

stress which may have a negative impact on their parenting abilities (see, e.g., Cooper and Stewart 2021).

4 Data

Most of our empirical analyses are based on a database that combines administrative data from several registers held at Statistics Sweden. This database includes all individuals residing in Sweden from the 1990s to 2022 (ages 0–74), and the various registers are linked through unique personal identification numbers. Included is information on, e.g., the individuals' education, age, immigration background, municipality of residence and sources of income (wage earnings and various benefits on a yearly basis). From 2010 onwards, we can observe receipt of HCA.[18] Children are linked to their (biological) parents through the multigenerational register, and the database includes information on children's year of birth, sex, order among siblings as well as results on standardized test in Swedish and mathematics taken in third grade (available since 2010).

We are interested in mothers' labor supply responses on both the extensive and the intensive margin. To capture these responses, we use information on annual wage earnings. We construct dummy variables for whether the mother has positive earnings the relevant year, and for whether she has earnings exceeding half of the median annual earnings of a 45-year-old female worker.[19] The first outcome is intended to capture whether the mother works at all, and the second whether she works at least part-time (or part of the year). We also examine the impact on total annual wage earnings, which captures the combined effect on the extensive and intensive margins. To examine impacts among fathers we use outcome variables defined in the same way.

To capture impacts on children's school performance we use results on third grade standardized tests in Swedish and mathematics. We calculate the mean over all available subtests in each subject, and then combine the subject scores by taking the mean of the two.[20] We also perform analyses of the two subjects separately. To account for potential changes in test content and grading standards over time, we standardize scores within test cohort to have mean zero and standard deviation one. It is important to highlight that the tests are designed to assess whether the student has reached the lowest acceptable level of knowledge. This means that many students receive

[18] The HCA is reported by child in our data, which means that we cannot observe if it was the mother or father that received the benefit. However, according to Statistics Sweden (2012) it was primarily women who applied for the benefit; e.g. 91.2 % of the recipients were women during the last six months of 2011. If HCA has been paid out for a given child, we therefore assume that it was the mother who applied for the benefit.

[19] A similar cut-off has been used in several previous studies; e.g. Forslund et al. (2017); Åslund et al. (2006).

[20] The data from Statistics Sweden contain test scores for 6 subtests (out of 7) in mathematics each year, and 2 subtests (out of 8) in Swedish. All subtests are given equal weight in the calculation of the mean scores within subjects. Students who lack results on all subtests within either subject are excluded from the analyses (around 6 % of the sample). We have analyzed whether access to the HCA affected test taking but find no indication of such effects. We have also analyzed effects on the share of subtests passed, as this allows us to include all subtests rather than the subset for which we have information on test scores. This analysis generates results that are qualitatively similar to those for test scores.

high scores, particularly in Swedish, and that the test results therefore cannot be expected to capture differences in skills among students high up in the ability distribution.

The database described above does not contain information on children's participation in childcare/preschool. Formal childcare is organized by the municipalities and there is no national register on childcare enrollment before 2014. The analysis of childcare participation will instead be based on data collected from the municipalities, which have been incorporated into another database. This 'childcare database' is partly overlapping with the database described above when it comes to population and variables included (e.g., demographic variables and parent-child link), but the two databases cannot be linked (they have been anonymized with different sets of personal identification numbers).[21] 113 out of Sweden's 290 municipalities have delivered data on children's registered childcare enrollment to this database, most of them for the time period 2003–2010.[22] This means that we can study childcare participation only for the first few cohorts of children that had access to the HCA. We measure childcare participation the years a child turns one, two, and three, and define a child as participating if he/she has been registered in childcare at any point during the year in question.

The quality of the childcare data does not necessarily live up to the high standards guaranteed in Statistics Sweden's registers and may differ across municipalities. We therefore perform additional robustness checks for all analyses based on the childcare data; we report these in Appendix C.

5 Empirical strategy

To identify the causal impact of access to the HCA, we exploit that the availability of this benefit has varied over time and across municipalities in a difference-in-differences analysis based on a matched sample of municipalities.

5.1 Comparable municipalities

As described earlier, it was a local political decision whether to introduce the HCA or not, which means that the municipalities offering this allowance are not a random selection of municipalities. Panel A of Table 1 shows how the probability that a municipality offers the HCA is related to several municipality characteristics. First of all, and as expected, providing HCA is closely related to having one of the most conservative parties in the local government. But there are also other

[21] The preschool data collected from the municipalities can only be combined with variables that the municipalities gave permission for at the time of data collection. This is the reason it cannot be combined with the database we use in our other analyses.

[22] Most municipalities reported data between 2000 and 2010, but due to differences in the way this was done, data are missing for some cohorts before 2003. We therefore restrict our analysis sample to children enrolled in preschool 2003–2010.

differences between HCA and non-HCA municipalities: provision of HCA is positively related to the population size as well as the education level. There is also a significant correlation between HCA and the local unemployment rate – introducing HCA is more common in municipalities with a (previously) lower unemployment rate.[23]

Differences in local labor market conditions pose challenges for identifying the effects of HCA eligibility. Using a difference-in differences design, we account for time-invariant differences, but if labor market conditions also evolve differently over time in the two groups of municipalities, this is likely to confound our estimates. To limit this potential problem, we start by creating a more balanced sample of municipalities in terms of labor market conditions using nearest neighbor matching (see Appendix A for a detailed description). We estimate the propensity score for each municipality based on a few variables that are likely to be crucial for the development of local labor market conditions but also for arriving at a more balanced sample of parents in terms of background characteristics. More specifically, we use the local unemployment rate (measured the year before municipalities could choose to introduce the HCA), the share of inhabitants with post-secondary education, and the share with immigrant background. For each treated municipality (HCA municipality), within the region of common support[24], we then select the closest untreated neighbor in terms of the propensity score to also be included in the sample. Hence, the idea is to create two groups of municipalities that are similar in terms of local labor market conditions and demographics, despite their differences in political preferences (which determines HCA status, and which we assume are constant over time).[25] As can be seen in panel B of Table 1, this simple matching procedure results in a sample of municipalities that is well-balanced in terms of unemployment and demographics. In Figure 3, we see that these two groups of municipalities also exhibit similar trends in earnings during the pre-reform period. All analyses will be carried out based on this matched sample of municipalities, which consists of roughly half (47 %) of Sweden's 290 municipalities.

[23] The unemployment rate is here measured in 2007, i.e., the year before it became possible for municipalities to introduce the HCA.

[24] Municipalities with propensity score>0.6 are excluded since there are very few municipalities without HCA in this region; see Figure A1. The fact that we cannot find good matches for all HCA municipalities means that the estimated effects presented in the paper may differ from the average effects for all HCA municipalities. As can be seen in Table 1, the HCA municipalities that are included in our estimation sample have, on average, a higher unemployment rate and a lower education level compared to the full sample of HCA municipalities.

[25] The idea is akin to the idea behind synthetic control methods for aggregated data (e.g., Abadie et al. 2010; Abadie 2021), but instead of generating weights to create a synthetic control for each treated municipality, we match each treated municipality to its closest neighbor. In contrast to synthetic control methods, we do not match municipalities based on pre-treatment outcomes, only on pre-treatment characteristics. Pre-treatment outcomes are instead used to assess the parallel trends assumption.

Table 1. Comparison of municipalities with and without HCA, before and after matching

	(1) Municipalities with HCA (at some point)	(2) Municipalities without HCA
A. Full sample of municipalities:		
Population size	39,370	26,430
Average age	41.68	43.38
Share with three-year upper secondary education	0.210	0.206
Share with post-secondary education	**0.261**	**0.213**
Share born abroad	**0.106**	**0.100**
Share openly unemployed[a]	**0.029**	**0.038**
Conservative party in the governing majority	0.843	0.283
No. of observations	123	167
B. Matched sample of municipalities:		
Population size	32,116	28,708
Average age	42.75	43.36
Share with three-year upper secondary education	0.209	0.208
Share with post-secondary education	**0.223**	**0.216**
Share born abroad	**0.098**	**0.096**
Share openly unemployed[a]	**0.035**	**0.034**
Conservative party in the governing majority	0.882	0.309
No. of observations	68	68

Note: The table is based on municipality data from Statistics Sweden and the Public Employment Service. Municipality characteristics are measured in 2008, except unemployment which is measured in 2007. Conservative party refers to the Christian Democrats and the Swedish Democrats (however, no municipality reported having the latter of these parties in the governing majority at this point in time). Variables in **bold** are those used in the estimation of the propensity score. [a]The unemployment rate is measured as a share of the (working age) population.

5.2 Estimating the impact on mothers' labor market outcomes

5.2.1 Sampling and descriptive statistics

We sample all mothers, residing in our matched sample of municipalities, who gave birth from 2000 to 2013.[26] We focus primarily on mothers since it was rare that fathers took up the HCA (Statistics Sweden, 2012). The observation unit when examining the impact on mothers' labor market outcomes is mother-child, i.e. the mother enters the sample each time she gives birth (given that the birth takes place within the given time interval). We then follow the mothers' labor market outcomes at different ages of the child to capture both immediate effects during the time the mother was entitled to the HCA (ages 1–3) and more long-term effects (above age 3).

To limit the potential problem that availability of the HCA can give rise to residential sorting – that is, that families that are interested in using the allowance may choose to relocate across

[26] Births taking place before the mother is 16 years old are not included due to an age restriction in the income register.

municipality borders to become entitled – we define municipality of residence based on the municipality the mother lived in the year she gave birth. For mothers who immigrated after giving birth, we define municipality of residence as the first municipality we can observe in our data.[27]

Our sampling procedure results in a sample of 646,262 births from 404,883 different mothers. Table 2 shows background statistics for births taking place in 'HCA' and 'non-HCA municipalities', where municipalities are categorized as 'HCA' if offering this allowance at least at some point. We can see that the matching approach has been successful in creating a balanced sample of mothers: average characteristics are similar in both groups of municipalities.[28]

[27] Municipality of residence is observed December 31 each year. In Appendix C (section 2), we examine whether the HCA induced families to relocate across municipality borders but find no evidence of this being the case.

[28] In Appendix Table B1, we show descriptive statistics separately depending on if the child was conceived before or after it was possible for municipalities to offer HCA, i.e., before or after 2009. The table shows that the small differences that do exist between HCA and non-HCA municipalities (e.g., mother's level of education) are very similar in both periods. Hence, there are no apparent reasons to suspect that the introduction of HCA caused any larger compositional changes by affecting residential choices, which is in line with the results presented in Appendix C.

Table 2. Descriptive statistics for the sample of mothers, separately for HCA and non-HCA municipalities

	(1) HCA	(2) No HCA
Mother's age at childbirth	30.11	30.32
	(5.18)	(5.22)
Mother born in non-Nordic country	0.21	0.21
	(0.41)	(0.41)
Immigrated <3 years before childbirth	0.04	0.04
	(0.21)	(0.21)
Mother has at most compulsory education[a]	0.12	0.11
	(0.32)	(0.31)
Mother has at most upper secondary educ.[a]	0.43	0.41
	(0.50)	(0.49)
Mother has post-secondary education[a]	0.40	0.42
	(0.49)	(0.49)
Missing data on mother's education[a]	0.06	0.06
	(0.24)	(0.24)
Mother's first child	0.43	0.45
	(0.50)	(0.50)
Mother's second child	0.36	0.35
	(0.48)	(0.48)
Mother's third or later child	0.21	0.20
	(0.41)	(0.40)
Child is a girl	0.49	0.49
	(0.50)	(0.50)
Child's age at immigration[b]	1.41	1.38
	(1.16)	(1.16)
Number of births	*339,658*	*306,604*

Note: The table reports sample means and standard deviations (within parenthesis) for mothers residing in the matched sample of municipalities. 'HCA municipalities' refers to municipalities that offered HCA at some point; 'no HCA' refers to municipalities that never offered this benefit. [a]Education is measured at child birth. [b]Calculated for children who immigrated to Sweden between age 0–3.

Figure 2 shows the take-up of HCA during the child's second year of life among women residing in HCA and non-HCA municipalities. It is clear that this benefit is not widely used. Only around 5–7 % of the mothers in HCA municipalities used this benefit during the child's second year of life for the period 2010–2015.[29] On average, the benefit recipient claimed HCA for nine months on a full-time basis (see Table B2). In Figure 3, which is constructed in the same manner, we see that the take-up of HCA is mirrored by lower average earnings two years after childbirth during the same time period. This indicates that the benefit did not just replace unpaid leave, but also had an impact on women's labor supply.

[29] Some mothers who lived in non-HCA municipalities at childbirth have moved to an HCA municipality by the time we measure HCA take-up in this figure; this explains why take-up is not strictly zero in non-HCA municipalities.

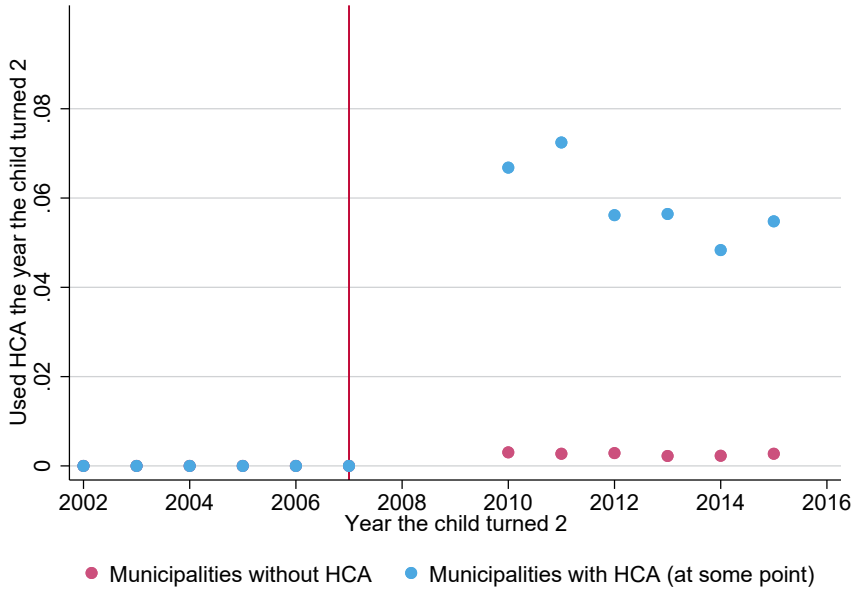

Figure 2. HCA take-up the year the child turns 2 in municipalities with and without HCA

Note: The figure is based on mothers residing in the matched sample of municipalities. The vertical line marks the year *before* it was possible for municipalities to offer HCA. HCA take-up is not included in our data until 2010.

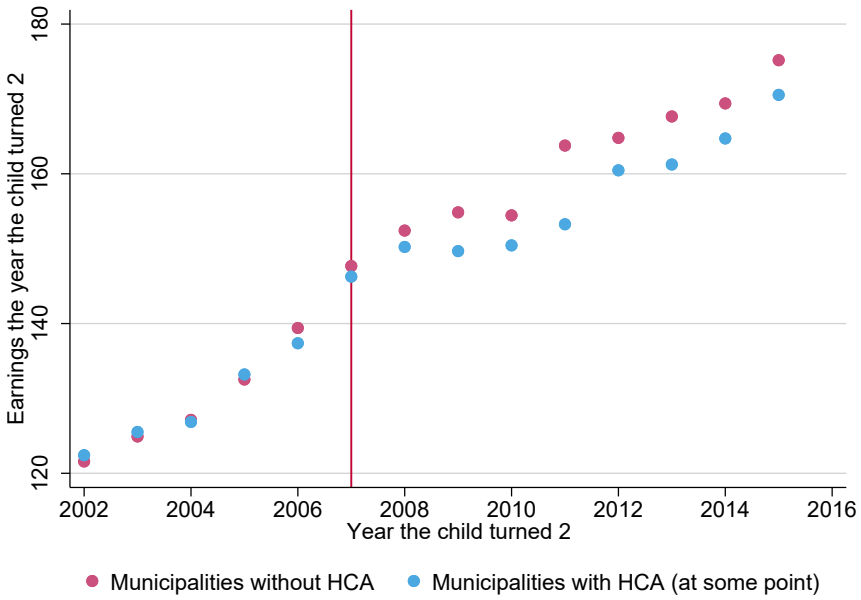

Figure 3. Earnings the year the child tuns 2 in municipalities with and without HCA

Note: The figure is based on mothers residing in the matched sample of municipalities. The vertical line marks the year *before* it was possible for municipalities to offer HCA. Earnings are reported in 1,000 SEK (2017 value).

5.2.2 Empirical model

To capture short- and long-run behavioral responses to HCA eligibility, we estimate the following difference-in-differences model year by year following childbirth:

$$Y_{imt} = \alpha + \beta Eligible\_HCA_{mt} + \gamma_m + \delta_t + \theta X_i + e_{imt} \quad (1)$$

Y_{imt} is the outcome for mother *i*, residing in municipality *m* in calendar year *t*. $Eligible\_HCA_{mt}$ is a dichotomous variable taking the value 1 if the municipality of residence (defined in the year of childbirth) offered the benefit, and 0 otherwise. The definition of this variable varies depending on if the purpose is to capture short- or long-run effects: When estimating short-run impacts (1–3 years following childbirth, i.e., at the time the mother could use the benefit), $Eligible\_HCA_{mt}$ takes the value 1 if the municipality offered the benefit the current year; when estimating longer run impacts (>3 years later), the variable takes the value 1 if the municipality offered the benefit at any time during year 1–3 following childbirth. The parameter of interest, β, gives the estimated (long- or short-run) impact of HCA eligibility. γ_m and δ_t represent municipality and calendar year fixed effects, respectively, and e_{imt} is the error term. Standard errors are clustered at the municipality level since both the sampling and the treatment is at this level (cf. Abadie et al. 2023). We generally show results separately for first-time mothers and mothers who had their second or later child.

Our preferred model specification also includes a few pre-determined[30] demographic covariates (X_i) – the mother's education level, immigration background, and age – to adjust for potential divergent demographic changes over time in HCA vs. non-HCA municipalities. To improve precision, our preferred model also includes a control for the mother's earnings before childbirth (measured two years before the birth of the first child). In practice, we exclude year 2008 (the first year the municipalities could offer HCA) from the estimation as parents had limited access to the benefit this year.[31]

β can be given a causal interpretation under the assumption that mothers' labor market outcomes would have developed similarly in HCA and non-HCA municipalities in the absence of the introduction of this benefit. Figure 3 indicated that female earnings developed in a similar way in HCA and non-HCA municipalities before the reform; hence, supporting the parallel trends assumption. In Section 6.2, we provide more formal support for this assumption. We also show that there is no indication of divergent labor market trajectories in the two municipality groups

[30] Measured at childbirth.
[31] Although several municipalities introduced HCA already this year, the introduction often came towards the end of the year; see Section 2. In the estimation of long-run impacts this means that we exclude individuals who were only eligible this specific year.

after the reform for women who had not yet had children. In our main specification, the treatment variable is allowed to vary over time within a given municipality. Section 6.2 also includes results from a model that instead considers a municipality to be a 'HCA municipality' if offering the HCA at any point in time. Since treatment status is rather constant over time during the period the benefit existed (see Section 2), this change in definition does not affect our findings. This also means that the staggered adoption of the HCA does not contribute much to identifying the effect of benefit eligibility; allowing us to draw conclusions without the need to invoke stronger assumptions or employ alternative difference-in-differences estimators (cf. de Chaisemartin and D'Haultfœuille 2020; Roth et al. 2023).

5.3 Estimating the impact on childcare participation and children's school performance

When estimating the impact of HCA eligibility on child outcomes, our sample consists of the children of the mothers included in the above sample. Data on standardized tests are available for the period 2010–2019 and 2022 (children born 2000–2009 and 2012)[32], resulting in a sample of in total 496,022 individuals; see Table B3 for descriptive statistics. Data on childcare enrollment are available for 44 out of the 136 municipalities that are included in our matched sample[33] for the period 2003–2010 (children born 2002–2009). That we lack childcare data for many municipalities could of course affect the balance in characteristics between HCA and non-HCA municipalities. Table B4 shows descriptive statistics for the 172,093 children who are included in the childcare sample. The table shows that the share of mothers with at most upper secondary education is somewhat higher in HCA compared to non-HCA municipalities, while the opposite is true for the share of mothers with post-secondary education and first-born children. These differences are, however, similar in both periods, which implies that our difference-in-differences estimates should not be biased due to this imbalance in characteristics.

In Figure 4, we see that enrollment in childcare was somewhat higher in HCA compared to non-HCA municipalities in the period before this benefit was introduced. This is expected since the HCA municipalities in general have a higher education level and lower unemployment rate (see Table 1). Despite different levels, the trends are similar in both groups of municipalities before the reform. After the reform, they converge, suggesting that HCA eligibility delayed childcare enrollment for some children. The pattern is clearest in 2009 and 2010 when most HCA municipalities had introduced this benefit.

[32] The standardized tests were cancelled in 2020 and 2021 due to the COVID-19 pandemic.
[33] Six additional municipalities have delivered data but of questionable quality and are therefore excluded; see Appendix C.

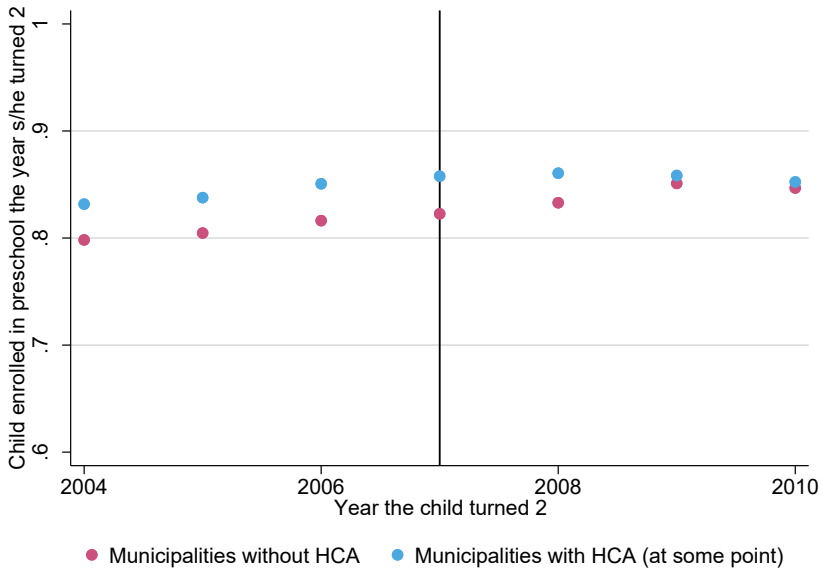

Figure 4. Enrollment in childcare the year the child turns 2 in municipalities with and without HCA

Note: The figure is based on children residing in matched municipalities. The vertical line marks the year *before* it was possible for municipalities to offer HCA.

To estimate the impact of HCA eligibility on child outcomes we use the same regression model as above (equation 1) but replace the outcome variable with an indicator for enrollment in child-care as well as the child's results on the standardized tests taken in third grade. Hence, the index i in equation 1 now refers to the child. When estimating the impact on childcare enrollment, the treatment variable $Eligible\_HCA_{mt}$ takes the value 1 if the municipality offered the benefit the current year (in line with how we estimate the short-run impact for the mothers). When estimating impacts on test scores, $Eligible\_HCA_{mt}$ takes the value 1 if the municipality offered the benefit at any time during year 1–3 following childbirth (in line with how we capture long-run impact for mothers). The vector of covariates includes background characteristics of both the mother and the father as well as the child's sex and month of birth.

6 Results

We start by presenting the estimated short-run impacts of HCA eligibility, i.e., impacts during the period families could use the benefit (Section 6.1). Understanding how the change in the relative price of formal childcare vs. home care affected parents' labor supply is interesting in itself, but also essential to be able to characterize how access to the HCA impacted families' childcare arrangements (i.e., if the allowance induced parents to spend more time at home or not). These results are followed by some robustness checks and placebo analyses (Section 6.2). Thereafter,

we analyze whether past benefit eligibility had any long-term effects on parents' labor market outcomes and children's cognitive skills (Section 6.3).

6.1 Short-term effects: How do families use the HCA?

6.1.1 Effects for the full sample of mothers and children

Table 3 displays the estimated short-run impact of HCA eligibility on benefit take-up (panel A) and mothers' total yearly earnings (panel B). Since some individuals have zero earnings, the earnings variable has been transformed using the inverse hyperbolic sine (IHS) transformation (see, e.g., Burbidge et al. 1988). The IHS transformation allows for a similar interpretation of the results as the log transformation but can also be applied to zeros.[34] The effects on benefit take-up are positive and significant all three years families are eligible, but the HCA is most frequently used the calendar year the child turns two. 6 % of eligible mothers used the benefit at least at some point during this year. The lower take-up during the first year is expected, given that parents are entitled to 480 days of paid parental leave with a substantially higher replacement rate (see Section 2). In line with the pattern in Figure 3, the results show that the HCA affected mothers' labor supply: HCA eligibility is estimated to have reduced total yearly earnings by on average 4.5–6.3 %, although the confidence intervals are wide. The largest impact again occurs during the child's second year of life. In Table B6, which displays results for the additional earnings variables, we see that the impact on earnings stems from responses at both the extensive and the intensive margin.[35] The fact that the percentage decline in yearly earnings is similar (or larger) than the percent of mothers using the HCA, suggests that the HCA induced most recipients to stay home longer instead of working.

In Table B7 we show the corresponding results for fathers. There is no indication that access to the HCA affected fathers' earnings in the short run. This finding is not surprising since fathers rarely used the HCA (see Statistics Sweden 2012).

Since eligibility for HCA induced some mothers to work less, we should expect it to also reduce childcare enrollment.[36] This is also what we find for municipalities where childcare data is available; see panel C of Table 3. The estimates suggest that eligibility for the HCA reduced

[34] The IHS transformation has been shown to have a major weakness in that it is not invariant to the units of measurement (e.g., Aihounton and Henningsen 2021; Chen and Roth 2024). We follow the recommendation in Aihounton and Henningsen (2021) and use the R^2-criterion to choose the most appropriate units of measurement; see Table B5. We also show effect estimates separately for the extensive and the intensive margin (cf. Chen and Roth 2024); see Table B6.

[35] Note that, since we only have annual data on earnings, the estimates in panel A of Table B6 will not capture the full response at the extensive margin. We only observe an effect on the probability of having positive earnings in cases where HCA eligibility postponed labor market return for a full calendar year. As shown in Table B2, the average duration of HCA take-up was 9 months only.

[36] If mothers had used the benefit without changing their labor supply, the interpretation would be that the HCA was mainly used by mothers who had stayed home for an equally long period regardless of this benefit. In such a case, we should not necessarily expect any effects on the child's participation in childcare.

childcare participation during the calendar year the child turned one (by 2.1 pp, or 4.5 %) and two (by 2.7 pp, or 3.2 %), although only the latter estimate is statistically significant.[37] Note that our definition of childcare participation (i.e., being registered at any point during the relevant calendar year) implies that we should not expect any effects the third year, as the possibility to receive HCA ended the day the child turned three (see Section 2). Table B8 shows that the estimated effects are rather similar for boys and girls.[38]

The overall pattern of results in Table 3 is stable across specifications, but the magnitude of the earnings estimates is reduced when the model includes demographic controls (see col. 2, 5 and 8). This seems to be due to a slight divergent compositional change in the share of foreign (non-Nordic) born mothers in the two groups of municipalities compared. However, given that the model includes demographic controls, the results are not affected if we also control for the mother's earnings before childbirth (measured two years before the birth of the first child) (see col. 3, 6 and 9). This suggests that the few demographic variables included manage to account for the relevant compositional changes. Since controlling for pre-birth earnings improves the precision of some of the estimates, the third model is our preferred specification.

[37] Note that, given our definition of childcare enrollment, the estimates in panel A and C should not be expected to exactly correspond. The enrollment variable takes the value 1 if the child was enrolled in childcare at any point during the relevant calendar year, and 0 otherwise. Thus, if HCA eligibility causes a delay in childcare enrollment within the same calendar year (e.g., from April to September), we will not capture this delay. However, the take-up variable will capture also such behavioral changes.

[38] The effect of benefit eligibility on take-up is also very similar for boys and girls (not shown).

Table 3. Short run effects of HCA eligibility on take-up , mother's earnings, and the child's participation in childcare, different child ages

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
		age 1			age 2			age 3	
A. HCA take-up									
Eligible HCA	0.045***	0.045***	0.045***	0.061***	0.061***	0.061***	0.027***	0.026***	0.027***
	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)	(0.005)	(0.002)	(0.002)	(0.002)
Observations	537,086	537,086	537,086	542,174	542,174	542,174	549,811	549,811	549,811
R-squared	0.043	0.044	0.045	0.056	0.058	0.058	0.026	0.027	0.028
Outcome mean[a]	0.000	0.000	0.000	0.001	0.001	0.001	0.001	0.001	0.001
B. Earnings (IHS transf.), effect in %									
Eligible HCA	-0.069	-0.050*	-0.045**	-0.094*	-0.062**	-0.063***	-0.080*	-0.051**	-0.054***
	(0.052)	(0.028)	(0.020)	(0.054)	(0.028)	(0.022)	(0.045)	(0.021)	(0.017)
Observations	583,456	583,456	583,456	588,754	588,754	588,754	596,164	596,164	596,164
R-squared	0.011	0.234	0.348	0.011	0.295	0.402	0.010	0.284	0.364
C. Childcare enrollment									
Eligible HCA	-0.018	-0.022	-0.021	-0.028**	-0.028***	-0.027***	-0.002	-0.001	-0.001
	(0.015)	(0.015)	(0.015)	(0.011)	(0.010)	(0.010)	(0.007)	(0.006)	(0.006)
Observations	149,895	149,895	149,895	126,652	126,652	126,652	104,896	104,896	104,896
R-squared	0.013	0.292	0.293	0.016	0.076	0.081	0.019	0.041	0.045
Outcome mean[a]	0.470	0.470	0.470	0.833	0.833	0.833	0.864	0.864	0.864
Demographic controls	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Control for pre-birth earnings[b]	No	No	Yes	No	No	Yes	No	No	Yes

Notes: Year 2008 is excluded from all estimations. Data on take-up is missing for 2008 and 2009. All regressions control for year and municipality fixed effects. The following demographic controls are included in panel A & B col. 2, 3, 5, 6, 8 and 9: mother's age, age^2, foreign background (10 categories), immigrated <3 years before childbirth, education level (3 categories), and missing data on education level. The following demographic controls are included in panel C col. 2, 3, 5, 6, 8 and 9: mother's and father's age, age^2, foreign background (10 categories), immigrated <3 years before childbirth, mother's and father's education level (3 categories), missing data on education level, and sex and birth month of the child. Robust standard errors, clustered on municipality, in parentheses. *** p<0.01, ** p<0.05, * p<0.1. [a]Outcome means are calculated for all untreated observations. [b]Pre-birth earnings refers to yearly earnings two years before the birth of the first child. We also control for missing data on this variable.

The estimated impacts on both labor market outcomes and childcare participation may seem small in magnitude, but we need to keep in mind that the effects reported are reduced form estimates, i.e., effects of benefit eligibility, and that the impact on HCA take-up was only around 3–6 pp. Dividing the reduced form impacts by the estimated effect on benefit take-up in panel A (the 'first stage'), gives us an IV-estimate of the impact of HCA take-up on the various outcomes, for mothers who were induced to use the benefit when they became eligible.[43] The exclusion restriction that needs to hold for this interpretation to be valid is that HCA eligibility only affected the outcomes (mothers' labor supply and earnings and children's childcare participation) through its effect on benefit take-up. Although this seems like a reasonable assumption, it may not be strictly fulfilled if the introduction of an HCA affected other decisions such as career choices also among non-recipients.[44] This type of calculation would imply that claiming HCA reduced the mother's probability of having positive earnings during the second year after childbirth by 15 pp (-0.009/0.061, see Table B6) and the probability that the child attended childcare by 45 pp (-0.027/0.06, see Table 3). However, since we are limited to annual data, these extensive margin impacts will not capture the full behavioral response.

To sum up, offering families the possibilities of using the HCA did not have a major impact on mothers' labor supply or earnings on average, nor did it imply a large decline in the average childcare participation rate. But the impacts on these outcomes were substantial for the families who chose to use the benefit.

6.1.2 First vs. later births and impacts on fertility choices

So far, we have presented average effects of HCA eligibility for all childbirths, but impacts may differ for first compared to later born children. One important reason is that parents get access to additional parental leave benefits (480 days) for each childbirth. This means that parents who give birth to an additional child have the possibility of staying home also with their first-born child, even if the parental leave benefits for that child have run out. It is therefore possible that access to HCA would be less important for these families. Table 4 shows effects of HCA eligibility (at child age 2), separately for first and later born children. In line with our reasoning above, the HCA is used somewhat less frequently for first-born children (5 pp compared to 7 pp for later born children), and the point estimates suggest that mothers' earnings and children's participation in childcare are less impacted in these families.

[43] Angrist and Imbens (1995) refer to this effect as the 'average causal response'. See Hudson et al. (2017) for a formal outline of this type of 'instrumented difference-in-differences design'.

[44] As in all IV-analyses we also need to assume monotonicity, i.e., in this case that benefit eligibility never reduced the probability of using the benefit. This assumption should be satisfied by construction in our setting.[45] As when we study longer-run outcomes, the eligibility variable takes the value 1 if the municipality offered the benefit at any time during year 1–3 following childbirth. Hence, the parameter of interest, β, gives the estimated impact of HCA eligibility for the first child on the probability of having a second child within a given time interval.

Table 4. Short-run effects of HCA eligibility, separately for first and later born children

	(1) HCA take-up	(2) Earnings (IHS transf.), effect in %	(3) Enrolled childcare
A. First child			
Eligible HCA	0.050***	-0.045*	-0.019*
	(0.004)	(0.023)	(0.011)
Observations	237,504	258,093	68,199
R-squared	0.048	0.394	0.076
Outcome mean[a]	0.001		0.820
B. Later child			
Eligible HCA	0.069***	-0.079***	-0.034***
	(0.006)	(0.026)	(0.009)
Observations	304,670	330,661	58,453
R-squared	0.066	0.417	0.092
Outcome mean[a]	0.001		0.849

Notes: Outcomes are here measured the year the child turned 2. Year 2008 is excluded from all estimations. Data on take-up is missing for both 2008 and 2009. All regressions control for year and municipality fixed effects as well as mother's age, age^2, foreign background (10 categories), immigrated <3 years before childbirth, education level (3 categories), missing data on education level, earnings 2 years before the birth of the first child as well as missing data on this variable. Col. 3 also includes all mentioned controls measured for the father as well as an indicator for missing father information and controls for sex and birth month of the child. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. [a]Outcome means are calculated for all untreated observations.

It is conceivable that access to the HCA would also affect fertility choices, e.g., the probability or timing of a second child. While fertility outcomes are interesting in themselves, such effects would also make the results presented above for later born children harder to interpret, as the presence and timing of a second (or later) child would then be endogenous. To investigate if eligibility for the HCA impacted fertility choices, we estimate the same model as before, but with the outcome being an indicator of whether the mother has given birth to a second child, evaluated at different years following the birth of the first child.[45] Table B9 displays the results for years 2–6 following childbirth. All estimates are statistically insignificant and of small magnitude, suggesting that fertility patterns were unaffected.

6.1.3 What types of families use the HCA?

Next, we take a closer look at what types of families that used the HCA and examine how it affected mothers' labor market participation and families' choice of childcare in various subgroups. Table 5 shows estimated effects of HCA eligibility on benefit take-up, mothers' earnings, and children's participation in childcare (the year the child turned 2) across different groups. The results indicate that the HCA is used somewhat more often in socioeconomically weaker groups

[45] As when we study longer-run outcomes, the eligibility variable takes the value 1 if the municipality offered the benefit at any time during year 1–3 following childbirth. Hence, the parameter of interest, β, gives the estimated impact of HCA eligibility for the first child on the probability of having a second child within a given time interval.

(mothers with a lower level of education and immigrant background) and in particular among families where the mother had a weak attachment to the labor market before childbirth (zero earnings before childbirth).[46] The results also indicate that eligibility for the HCA led to greater reductions in earnings and childcare enrollment for those sub-groups that used the benefit most frequently, although the differences are generally not statistically significant. Moreover, relating the estimated earnings and childcare impacts to the impacts on HCA take-up, suggests that the differences in short-run impacts between sub-groups are largely attributable to the differences in benefit take-up.[47]

Table B2 provides further details about usage patterns by showing the average total number of months the benefit was claimed among the recipients as well as share of full-time usage. The average benefit duration is rather similar across the sub-groups examined, ranging from 8.6–10.4 months, and almost everyone seems to have claimed the benefit on full-time basis.

[46] Recent immigrants turn out to constitute an exception from this pattern; among mothers who immigrated less than three years before childbirth the take-up rate was only around 3 % (not shown in the table).
[47] When it comes to fathers' earnings, the impact is statistically insignificant for all sub-groups of families, except those where the mother worked before childbirth; see Table B10. For these families, there is a negative impact also on fathers' earnings.

Table 5. Effects of HCA eligibility on take-up, earnings and childcare, the year the child turns 2. Results for various sub-groups.

	(1) Did not work before childbirth[a]	(2) Worked before childbirth[a]	(3) Low education[b]	(4) High education[b]	(5) Immigrant[c]	(6) Native[c]
A. HCA take-up						
Eligible HCA	0.083***	0.054***	0.070***	0.049***	0.069***	0.058***
	(0.007)	(0.005)	(0.005)	(0.005)	(0.007)	(0.005)
Observations	112,891	429,283	319,876	222,298	109,404	432,770
R-squared	0.079	0.054	0.066	0.051	0.069	0.057
Outcome mean[d]	0.001	0.001	0.001	0.001	0.001	0.001
B. Earnings (IHS transf.), effect in %						
Eligible HCA	-0.091*	-0.054***	-0.074***	-0.039**	-0.063	-0.061***
	(0.046)	(0.018)	(0.027)	(0.020)	(0.048)	(0.021)
Observations	122,855	465,899	346,558	242,196	118,925	469,829
R-squared	0.154	0.148	0.379	0.304	0.362	0.249
Mean earnings[d] (SEK 1,000)	40.80	172.4	109.4	199.5	70.91	163.1
C. Childcare enrollment						
Eligible for HCA	-0.055***	-0.025**	-0.034***	-0.019*	-0.035**	-0.026**
	(0.013)	(0.010)	(0.010)	(0.011)	(0.014)	(0.010)
Observations	18,217	98,906	75,279	51,373	27,822	98,830
R-squared	0.057	0.036	0.097	0.045	0.134	0.053
Outcome mean[d]	0.770	0.864	0.815	0.859	0.768	0.851

Notes: Data on take-up is missing for 2008 and 2009. All regressions control for year and municipality fixed effects as well as mother's age, age^2, foreign background (10 categories), immigrated <3 years before childbirth, education level (3 categories), missing data on education level, earnings 2 years before the birth of the first child and missing data on this variable. Panel C additionally includes all mentioned controls measured for the father as well as an indicator for missing father information and controls for sex and month of birth for the child. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. [a]Earnings before childbirth are measured at year t-2. [b]Low education is defined as not having post-secondary education, and high education as having post-secondary education. [c]Mothers who are born in a Nordic country are defined as 'native', and mothers who are born outside the Nordic region are defined as 'immigrants'. [d]Outcome means and mean earnings are calculated for all untreated observations.

6.2 Assessing the identifying assumption

Event study analysis. To assess the assumption of parallel trends more formally, we complement our main empirical approach with an event study analysis which allows the impact of residing in an HCA municipality to vary over time – both before and after the reform. More specifically, we estimate the following model:

$$Y_{imt} = \alpha + \sum_{t=2002, t\neq 2007}^{2015} [\lambda_t(HCA\_municipality_m)] + \gamma_m + \delta_t + \theta X_i + e_{imt} \quad (2)$$

where Y_{imt} is the labor market outcome for mother i, who resides in municipality m, at calendar year t. We here focus on labor market outcomes measured the year the child turns two, i.e. the year HCA is most frequently used. $HCA\_municipality_m$ takes the value 1 if the mother resides in a municipality that *at some point* offered HCA, and 0 if she lives in a municipality that never offered this benefit.[48] The λ_t coefficients for the pre-reform period (λ_{2002} to λ_{2006}) trace out relative trends during the years preceding the reform, while the λ_t coefficients for the post-reform period (λ_{2008} to λ_{2015}) provide time-varying treatment effects. The reference year is 2007, which means that all λ_t estimates are relative to the last year before it became possible to offer HCA. γ_m and δ_t represent municipality and calendar year fixed effects, respectively. X_i includes demographic covariates (education, immigration background, and age) as well as earnings before childbirth, and e_{imt} is the error term. Standard errors are clustered at the municipality level.

Figure 5a shows the λ_t estimates for total yearly earnings, while the estimates for the other labor market outcomes examined are shown in Figure B1. The estimates for the pre-reform years are statistically indistinguishable from zero for all outcomes and exhibit no discernable trends, which supports the parallel trends assumption. For total earnings as well as the probability of having positive earnings, all post-reform estimates are statistically significant at least at the 10 % level. The estimates are also of rather similar size, apart from the estimate for 2011 which is substantially larger – in Figure 2 we see that this is also the year when the HCA was used the most. The estimates for working at least part-time exhibit more variation over time, and here we see no discernable impact before 2010. For the intensive margin, there is a tendency of a negative impact towards the end of the period, but most estimates are not statistically significant. Figure 5b replicates this analysis using the child's enrollment in childcare as the outcome. Reassuringly, there is no indication of divergent trends in the pre-reform period for this outcome either, while we see a clear reform impact in both 2009 and 2010.

[48] Note that the definition of treatment in this analysis differs from that in our main analysis, where the treatment variable (*Eligible_HCA*) takes into account whether HCA was offered in the home municipality the relevant year. Hence, in the main analysis municipalities are allowed to change treatment status over time in the post-reform period, while we here consider them either treated ('HCA municipality') or non-treated ('non-HCA municipality'). While this modification makes it more straightforward to compare the groups in the pre-reform period, it also introduces some measurement error in the treatment variable after the reform which should bias the estimates of λ_t towards zero. However, as only a small fraction of municipalities changes treatment status during the post-reform period (see Section 2) this has no meaningful impact on the results.

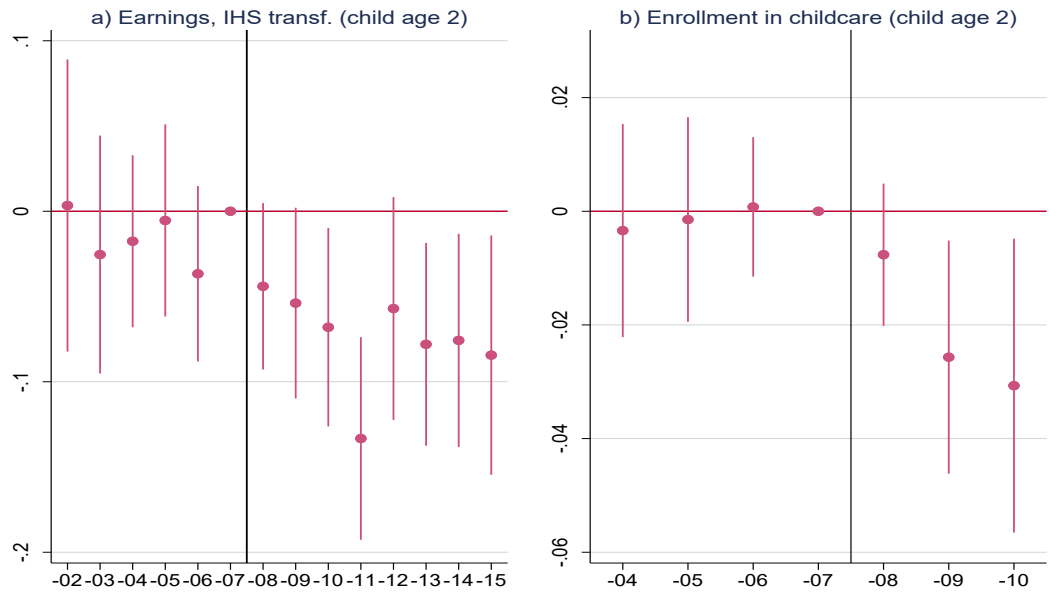

Figure 5. Time-varying effects of living in an HCA municipality on earnings

Note: 2007, the last year before the reform, is the reference year. 'HCA municipality' is defined as a municipality that offered HCA at least at some point. Effects on earnings are in percent.

Placebo analysis using data before childbirth. Another way to assess whether mothers' labor market outcomes would have developed similarly in municipalities with and without HCA in the absence of this benefit, is to examine how labor market outcomes developed during the time the HCA was in place for women who had not yet had children. To do this, we estimate our main model specification (model 1, including demographic covariates only) using earnings one, two and three years before the mother gave birth to her first child as the outcome.[49] If we were to see the same decline in earnings for women living in HCA municipalities also *before* childbirth, this would indicate divergent trends on the labor market in the two municipality groups; hence, violating the parallel trends assumption.

Figure 6 shows estimates from this exercise on yearly earnings. The estimates for child ages -3, -2, and -1 are thus placebo estimates, while the estimates for ages 1, 2 and 3 correspond to effect estimates of actual HCA eligibility (the same as in Table 3, col. 2, 5, and 8, but with the sample limited to first-time mothers). The estimate for age 0 can also be considered a placebo estimate, as parents could not claim the HCA before the child turned 1 year old. Reassuringly, all placebo estimates are small in size and never statistically significant. The effect estimates are in line with those shown in Table 3, but less precisely estimated for this reduced sample. Figure B2

[49] The eligibility variable (*Eligibility_HCA*) in the placebo-regressions takes the value 1 if the municipality of residence offered HCA during the calendar year the child turned 1, and 0 otherwise.

in Appendix presents the corresponding estimates for the other labor market outcomes examined. The results are in line with the conclusion from Figure 6.

Figure 6. Placebo analysis: Effects of HCA eligibility on earnings before and after childbirth

Note: Estimates of model 1 before and after childbirth. In the placebo-regressions (year -3, -2, -1, 0), the eligibility variable takes the value 1 if the mother will become eligible for HCA the year the child turns 1, otherwise it is 0. For the effect estimates (year 1, 2, 3), eligibility is defined as before, i.e. based on the current year, and year 2008 is excluded. The sample is limited to first time mothers. The figure shows effects in percent.

In Appendix C (section C3), we repeat both the event study analysis and the placebo analysis for all sub-groups examined in the previous section. Reassuringly, none of these analyses show patterns that indicate that the parallel trends assumption would not be fulfilled.

6.3 Long-term effects

The short-run impacts of HCA eligibility are expected given the design and purpose of the benefit: Benefit eligibility leads to reduced labor supply among mothers and a lower rate of childcare enrollment among their children. Even more interesting is to examine whether there are any long-term effects, i.e., lasting effects when the family is no longer eligible for the benefit. Does the possibility to stay home longer with the child affect labor market outcomes in the longer run, and does it have an impact on the child's school results? We start by presenting estimates for the mothers' labor market outcomes and thereafter for children's school performance.

6.3.1 Mothers' long-term labor market outcomes

Figure 7a shows estimated impacts of HCA eligibility on total yearly earnings up to 8 years following childbirth, for first-time mothers. The figure shows that HCA eligibility has a rather long-lasting labor market impact: From year 2 to year 7 after childbirth, there is a statistically signifi-

cant negative impact on mothers' yearly earnings, which are reduced by 3.7–4.9 %. In Figure B3 in the Appendix we can see that this pattern is driven by impacts on both the extensive and the intensive margins. Figure 7b, which repeats the analysis for mothers who give birth to a second or later child, shows a very similar pattern, although the effects are less precisely estimated.[50] Hence, for both groups of mothers, the negative impact seems to disappear around the time the child starts school.[51] In line with the short-run results, we find no long-run impact on fathers' earnings; see Figure B5.

It is possible that part of the long-term earnings effect for mothers is driven by benefit eligibility for later born siblings, as many mothers had the possibility of using the HCA for more than one child. However, in Figure B4, we show that we get very similar estimates if we control for benefit eligibility for later born siblings.

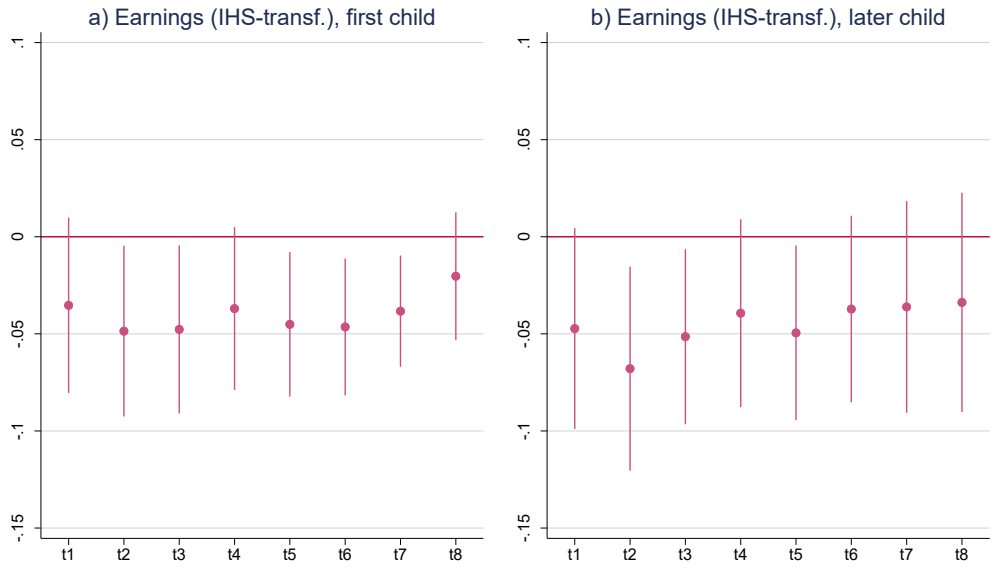

Figure 7. Long run impact of HCA eligibility, separately for first-time mothers and mothers who give birth to their second or later child

Note: Estimated effects in %. The figure shows estimates of model 1 with the full set of background controls. The eligibility variable takes the value 1 if the municipality of residence offered HCA at any time during year 1–3 following childbirth, and 0 otherwise. The estimated impact of HCA eligibility on the probability of HCA take-up (at any time year 1–3 following childbirth) is 0.048 (0.004) for first-time mothers, and 0.063 (0.005) for mothers who give birth to a second or later child.

Figure 8 presents results from the same type of analysis, but separately for various sub-groups of mothers: by pre-birth employment, education level, and immigrant background. While the diffe-

[50] For the full sample of mothers, the earnings effect is also statistically significant up to 7 years after childbirth.
[51] For most children in our sample, schooling was compulsory from the fall semester of the year they turned 7. However, the vast majority attended a voluntary pre-school class the year before. Starting in the school year 2018/2019, schooling became compulsory from the year a child turns 6.

rences in short-term impact between these sub-groups seemed to be largely explained by differences in benefit take-up, the differences in long-run impacts require additional explanations. First and foremost, it is clear that the rather long-lasting negative earnings impact that we saw for the full population of mothers is driven by those groups of mothers who typically have a weaker position on the labor market: mothers with a lower education level, immigrant background, and mothers who did not work before childbirth. For the latter two groups, the negative impact is particularly large, around -10 % from year 3 and onwards, and it remains statistically significant 8 years after childbirth. For mothers with post-secondary education, on the other hand, there is no evidence of any lasting effects (i.e., when the mother was no longer eligible for the HCA). A possible explanation is that mothers with higher education to a large extent had stable jobs that they could return to after they stopped using the benefit (recall that parents were entitled to job-protected leave while claiming HCA). The fact that there is little evidence of long-lasting labor market impacts also among those mothers who worked before giving birth (comprising more than 80 percent of the sample) supports this explanation. Hence, it seems that HCA eligibility contributes to worse labor market attachment for mothers with a weak position on the labor market before childbirth, while there is little evidence of long-lasting negative effects for women with a stronger labor market position.

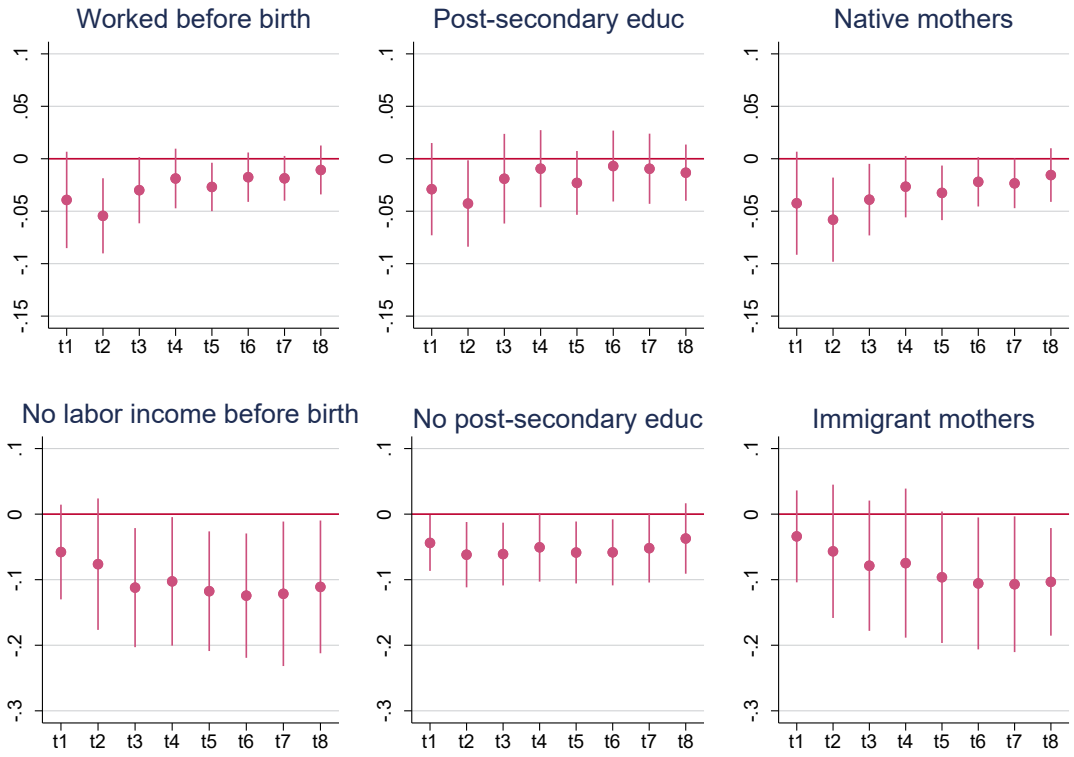

Figure 8. Impact of HCA eligibility on total yearly earnings (in %) for different sub-groups of mothers

Note: Effects on IHS transformed yearly earnings (i.e., effects in %). Both first-time mothers and mothers who give birth to a second or later child are included. The figure shows estimates of model 1 with the full set of background controls. The eligibility variable takes the value 1 if the municipality of residence offered HCA at any time during year 1–3 following childbirth, and 0 otherwise. The estimated impact of HCA eligibility on the probability of HCA take-up (at any time year 1–3 following childbirth) is 0.052 (0.005) for mothers with positive pre-birth earnings; 0.072 (0.007) for mothers without pre-birth earnings; 0.047 (0.005) for mothers with high education; 0.063 (0.005) for mothers with low education; 0.054 (0.005) for native mothers; and 0.062 (0.008) for immigrant mothers.

6.3.2 Effects on children's school results

So far, we have seen that eligibility for the HCA reduced childcare attendance as well as the mother's (but not the father's) earnings several years after the benefit eligibility expired.[52] Both of these channels may in turn affect the development of children's cognitive skills. While a lower family income can be expected to have a negative effect on children's school results (see Cooper and Stewart, 2021, for a review of this field), the expected impact of more home-care instead of formal childcare at age 1–2 is less clear.

Table 6 (panel A) shows the estimated impact of HCA eligibility on children's test scores on 3rd grade standardized tests in Swedish and math (combined), broken down by sex. Examining

[52] The HCA, along with saved childcare fees, partly compensated for the income loss during the period (on average 9 months) that families claimed the benefit. However, our results show that HCA eligibility reduced mothers' earnings for up to 7 years after childbirth. Note, however, that our measure of parental earnings does not give a complete picture of how the family's total income was affected as it does not include income from, e.g., unemployment or sick leave benefits.

boys and girls separately is motivated by prior research suggesting that boys have a heightened sensitivity to childhood circumstances (e.g., Bertrand and Pan 2013; Autor et al. 2019). The point estimate for girls is close to zero and statistically insignificant, indicating that access to the HCA had no discernible impact on their school performance. For boys, the estimated average effect is negative and statistically significant at the 10 % level, suggesting that eligibility for the HCA led to a decrease in their performance by 2.8 % of a standard deviation. The results are similar if we conduct separate analyses for mathematics and Swedish (see Table B11). Distinguishing between first- and later-born children (panels B and C), suggests that the negative effect for boys is primarily driven by first-born children, even though families used the HCA somewhat more often for later-born children (see Table 4). A potential reason might be that the home environment was less stimulating for the first child who did not have any older siblings at home.

If we consider that only about 5 % of eligible families utilized the benefit, an average decline of 2.8 % of a standard deviation represents a substantial impact.[53] This estimated impact comprises the combined effect of about 9 months less childcare attendance and 7 years of lower maternal earnings, together with potential additional channels that we do not document here. For instance, reduced income may influence residential choices, affecting which neighborhoods families live in, which in turn may have implications for school quality and peer groups. However, we should keep in mind that the confidence intervals are large, making it difficult to precisely determine the magnitude of the effect, and that the exclusion restriction needed for an IV interpretation of the estimates might not be strictly fulfilled (see discussion in section 6.1.1).

In Figure B6 (as well as Figure C4 for heterogeneity depending on the mothers' characteristics) we show results from an event study analysis, which allows us to examine the parallel trends assumption for test scores. While the figures show no clear indications of divergent trends for children born before the HCA was introduced, is should be noted that the point estimates are rather unstable in this cohort-by-cohort comparison, warranting some caution in the interpretation of the results for test scores.

[53] An IV interpretation of the estimate would thus suggest a decline of 56 % of a standard deviation for those who used the allowance. If we compare this estimate to previous studies of preschool/childcare interventions, it represents a large effect but falls within the range of previous findings. Duncan et al. (2022) review the literature on the impacts of preschool participation on cognitive outcomes (including interventions that vary greatly in terms of length, intensity, and quality) and report estimates ranging from negative to above 100 % of a standard deviation for targeted programs, and estimates ranging form negative to 0.71 for universal preschool programs.

Table 6. Effects of HCA eligibility on children's standardized test scores in grade 3

	(1) Girls	(2) Boys
A. All children		
Eligible for HCA	-0.005	-0.028*
	(0.015)	(0.014)
Observations	206,762	215,859
R-squared	0.133	0.118
Outcome mean[a]	0.0770	-0.0630
B. First child		
Eligible for HCA	-0.007	-0.041***
	(0.017)	(0.014)
Observations	90,848	95,464
R-squared	0.131	0.114
Outcome mean[a]	0.154	0.0123
C. Later child		
Eligible for HCA	-0.002	-0.019
	(0.016)	(0.016)
Observations	115,914	120,395
R-squared	0.134	0.120
Outcome mean[a]	0.0162	-0.123

Notes: Test scores have been standardized within test year (school cohort) to have a mean of 0 and a standard deviation of 1. All regressions control for cohort and municipality fixed effects as well as mother's and father's age, age^2, foreign born mother and father (10 categories), if mother and father immigrated <3 years before childbirth, mother's and father's education level (3 categories), mother's and father's earnings before childbirth, missing data on parents' education and earnings, missing father data, and birth month of the child. Children born 2005 have been excluded as they (although treated) had very limited access to the HCA. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. [a]Outcome means are calculated for all untreated observations. The estimated impact of HCA eligibility on the probability of HCA take-up (at any time year 1–3 following childbirth) is 0.051 (0.004) for all girls; 0.050 (0.004) for all boys; 0.047 (0.004) for first-born girls; 0.042 (0.003) for first-born boys; 0.053 (0.004) for later-born girls; 0.055 (0.005) for later-born boys.

In Table 7, we repeat the test analysis for various sub-groups, using the same sub-group divisions as when we examined impacts on mothers. Since access to the HCA led to a longer-lasting reduction of household income for children whose mothers are lower-educated, foreign-born, or lacked pre-birth earnings (see Figure 8), we might anticipate more pronounced negative impacts on school results for these sub-groups of children. There are also indications that childcare enrollment was reduced the most for the same sub-groups (see Table 5). Given that children from disadvantaged backgrounds are generally expected to benefit more from formal childcare compared to those from more advantaged backgrounds (as discussed in Section 3), these sub-groups might also be more negatively impacted through this channel.

Again, we find no evidence that access to the HCA impacted girls' school performance; the estimates are far from being statistically significant for all sub-groups (panel A). For boys, we find statistically significant negative effects on tests scores for several groups (panel B). Splitting the sample by the mother's education shows that the negative impact is solely driven by boys with lower-educated mothers. If we instead split by pre-birth employment, we find similar point esti-

mates irrespective of whether the mother worked before childbirth, indicating that pre-birth employment is not an important driver of heterogeneity in impacts. Notably, the negative effect for boys seems to be driven by those with native rather than foreign-born mothers. The lack of significant negative effects for the two sub-groups where mothers' earnings were reduced the most – foreign-born mothers and mothers without pre-birth earnings (see Figure 8) – suggests that the negative impact on boys' test scores is unlikely to be caused by lower household income. Reduced participation in formal childcare seems like a more plausible mechanism. The pattern being strongest for children of less educated mothers is consistent with a story in which the quality of the home environment is closely linked to the human capital of the mothers. It may seem surprising that that there is no effect for boys with a foreign background given that previous research has found that immigrant children particularly benefit from preschool attendance (Cornelissen et al. 2018; Drange and Telle 2017). However, it should be noted that foreign-born mothers are a heterogenous group in our study, both in terms of educational background, country of birth, and length of residence in Sweden. It is possible that part of the explanation for the absence of an effect is that that the HCA was rarely used by mothers who had recently immigrated (see section 6.1.3).

Finally, it is important to acknowledge that even though we do not find a statistically significant impact of benefit eligibility for many subgroups, the size of the confidence intervals precludes us from ruling out economically important effect sizes. We should also keep in mind that the standardized tests we examine may not capture differences in skills among students high up in the ability distribution (see discussion in section 4).

Table 7. Effects of HCA eligibility on children's standardized test scores in grade 3, various sub-groups

	(1) Mother did not work before childbirth	(2) Mother worked before childbirth	(3) Mother has low education	(4) Mother has high education	(5) Immigrant mother	(6) Native mother
A. Girls						
Eligible HCA	-0.021	-0.001	-0.005	-0.003	-0.031	0.001
	(0.032)	(0.012)	(0.017)	(0.013)	(0.038)	(0.013)
Observations	40,298	166,464	123,928	82,834	37,746	169,016
R-squared	0.106	0.103	0.086	0.070	0.117	0.100
Outcome mean[a]	-0.270	0.158	-0.0967	0.349	-0.291	0.153
B. Boys						
Eligible HCA	-0.038	-0.026*	-0.042**	-0.004	-0.002	-0.033**
	(0.030)	(0.014)	(0.019)	(0.013)	(0.031)	(0.014)
Observations	41,910	173,949	129,100	86,759	38,987	176,872
R-squared	0.097	0.096	0.072	0.067	0.106	0.094
Outcome mean[a]	-0.391	0.0133	-0.247	0.223	-0.419	0.00922

Notes: Test scores have been standardized within test year to have mean of 0 and standard deviation 1. All regressions control for cohort and municipality fixed effects as well as mother's and father's age, age^2, foreign born mother and father (10 categories), if mother and father immigrated <3 years before childbirth, mother's and father's education level (3 categories) and earnings before childbirth, missing data on parents' education and earnings, missing father data, and birth month of the child. Children born 2005 are excluded as they (although treated) had very limited access to the HCA. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. [a]Outcome means are calculated for all untreated observations. The estimated impact of HCA eligibility on the probability of HCA take-up **among boys** (at any time year 1–3 following childbirth) is 0.041 (0.004) for mothers with positive pre-birth earning; 0.086 (0.007) for mothers without pre-birth earnings; 0.039 (0.004) for mothers with high education; 0.057 (0.004) for mothers with low education; 0.047 (0.004) for native mothers; and 0.060 (0.006) for immigrant mothers. The corresponding estimates **among girls** are very similar.

7 Concluding discussion

We evaluate the impact of providing a child home care allowance on parents' decisions regarding labor supply and childcare, as well as on their longer-term earnings trajectories and children's development of cognitive skills. The context is Sweden, a country characterized by a strong norm to use formal childcare from an early age. The introduction of an HCA provided an opportunity for parents to choose an alternative childcare arrangement than the common norm. The HCA could be used when the child was between ages 1 and 3, and it allowed parents to prolong their job-protected parental leave. To identify the causal impact of eligibility for the HCA, we exploit variation in the availability of this benefit over time and across municipalities in a difference-in-differences analysis based on a matched sample of municipalities.

We find that being eligible for the HCA had a negative impact on mothers' labor supply and earnings during the time the family could use the benefit, while it had no impact on fathers. Qualitatively, these results are in line with studies of HCA in other European countries, which have generally focused on the immediate short-term effects of such benefits (e.g., Naz 2004; Piketty 2005; Schøne 2004; Kosonen 2014). Consistent with the results of a recent study from Finland

(Gruber et al. 2022), we show that a negative impact on mothers' earnings remains when the family is no longer eligible for the benefit and is still visible seven years after the child was born. Moreover, we show that this negative long-run effect is driven by mothers who lacked earnings before childbirth, while there is no lasting impact for mothers with previous employment. A plausible explanation for the absence of a lasting earnings impact for mothers with prior employment is that they were entitled to job-protected leave while claiming HCA. This might have shielded these mothers from long-term negative labor market impacts as they had the option to return to their previous job. For mothers needing to find a new job, on the other hand, the possibility of extended time at home had long-term negative impacts on labor market opportunities. This suggests that the long-term negative impacts of this type of policy potentially could be avoided by conditioning paid leave on pre-birth earnings.

As for the children, we find that eligibility for the HCA reduced enrollment in formal childcare. The HCA may thus have impacted children's development of cognitive skills both through the long-term reduction of household earnings and through reduced participation in childcare. We find no evidence that access to the HCA affected girls' test scores in math and Swedish at age 10. For boys, our results suggest a negative impact, driven by those with lower-educated mothers. A closer look at different sub-groups suggests that the negative impact on boys' test scores is more likely caused by reduced childcare participation than lower household income.

Only a couple of previous studies have investigated the impact of eligibility for an HCA on children's development of cognitive skills. The negative impact we document for boys aligns with the findings of Gruber et al. (2022) for Finland, who also argue that reduced childcare participation is the likely mechanism. Collischon et al. (2020), on the other hand, find no impact of the introduction of an HCA in Germany on children's results on school entry exams. This difference in results may be due to the fact that the German HCA mainly seems to have represented a windfall gain for families who would not have used formal childcare regardless of the HCA. In contrast to the German setting, most children in Sweden attend formal childcare at an early age. While the HCA was not widely used in the Swedish context, we show that for the families who used the allowance, there is a substantial decline in childcare participation.

Taken together, our results show that the increased flexibility the HCA policy offered to families, which was likely valued by those who used the allowance, also came with long-term costs in terms of lower earnings for mothers and, for boys, worse school outcomes. These costs were primarily borne by families with weaker socioeconomic backgrounds.

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Appendix A: The matching procedure

The purpose of the matching procedure is to arrive at two groups of municipalities – HCA and non-HCA municipalities – that are similar in terms of local labor market conditions and how these conditions develop over time. The key variable that we want to match on is therefore the local unemployment rate (measured before municipalities could choose to introduce the HCA). The hope is that we, by matching on the unemployment level, also will arrive at two municipality groups with similar trends in the pre-reform period. Since it is advantageous to also obtain a sample of individuals that is balanced in terms of background characteristics, we also include some key demographic variables (education level and immigrant background) in the estimation of propensity scores.

The matching is conducted as follows: 1) We estimate the propensity score for each municipality based on the local unemployment rate (measured the year before municipalities could choose to introduce the HCA), the share of inhabitants with post-secondary education, and the share with immigrant background. The propensity score is estimated using a probit model. 2) For each treated municipality (HCA municipality), within the region of common support, we select the closest untreated neighbor in terms of the propensity score to also be included in the sample. To avoid matching municipalities that are far apart in terms of the propensity score we set the maximum propensity score distance (caliper) that is tolerated to 0.1.[54]

Figure A1 visualizes the extent of common support by showing the density distribution of the propensity score in non-HCA (left figure) and HCA municipalities (right figure). There are few non-HCA municipalities with a propensity score>0.6, which is why also HCA municipalities in this region of support are excluded from the sample. The excluded HCA municipalities tend to be located in large city regions, have a comparably low unemployment rate and a highly educated population. Such municipalities tend to have a center-right wing local government, favoring the introduction of the HCA. The fact that it is not possible to find good matches for all HCA municipalities means that the estimated effects presented in the paper may differ from the average effects for all HCA municipalities; for instance, they may not be generalizable to the largest cities.

[54] The matching is carried out using the Stata package *psmatch2* (Leuven and Sianesi 2018).

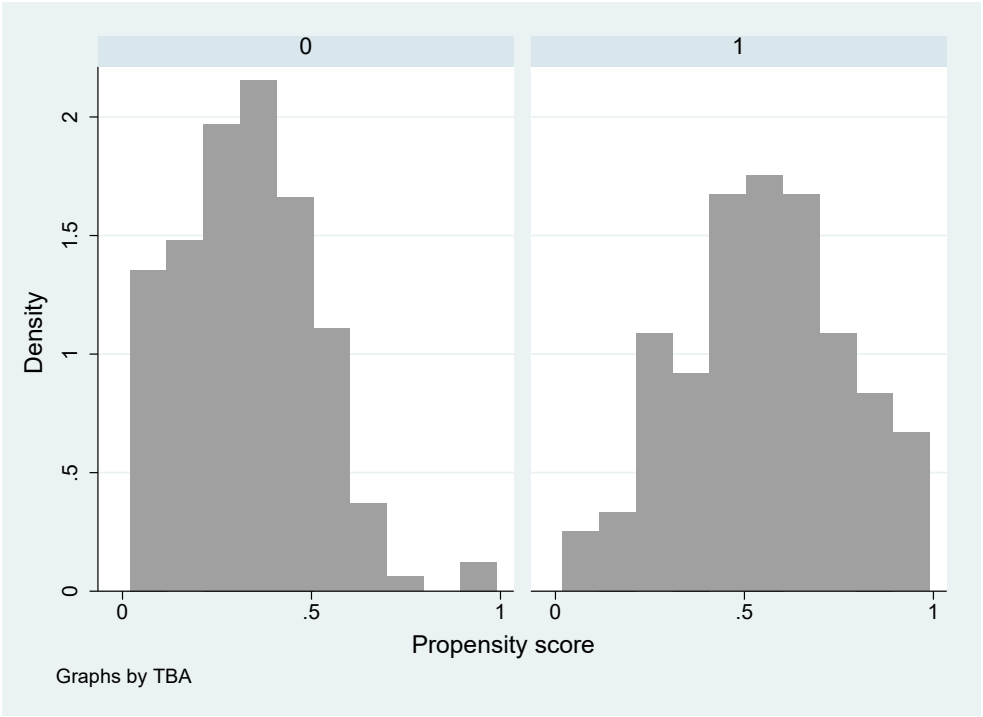
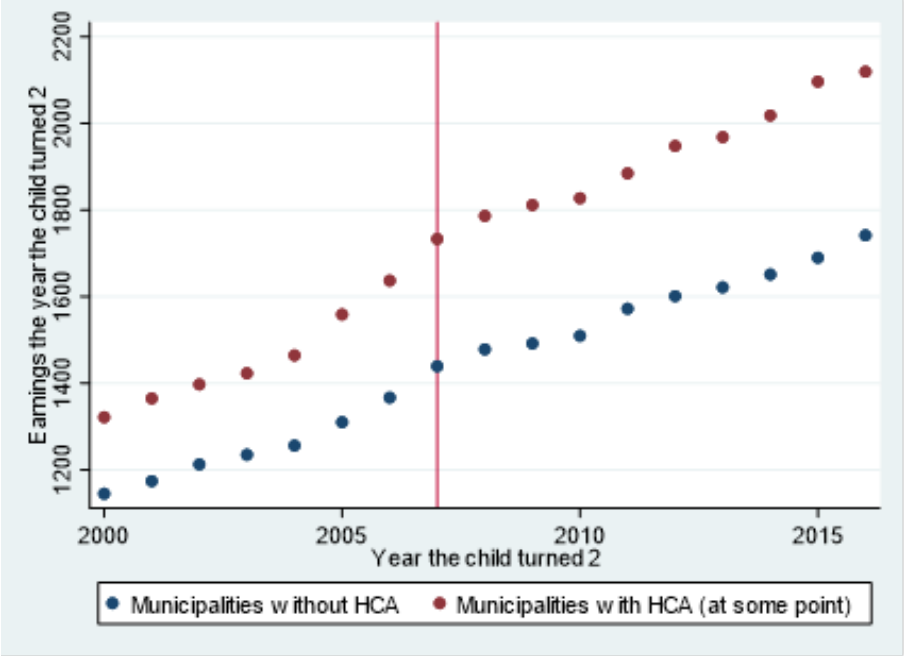


Figure A1. Density distribution of the propensity score in municipalities without HCA (left) and with HCA (right)

The matched sample of municipalities consists of roughly half of Sweden's municipalities. Table 1 (in the main text) shows that our simple matching procedure results in a sample of municipalities that is well-balanced in terms of unemployment and demographics. Figure A2, panel B, shows that mothers in matched HCA and non-HCA municipalities also exhibit similar trends in earnings during the pre-reform period. This pattern is in sharp contrast to the pattern that arises if we look at all municipalities; see panel A of Figure A2, which shows large difference in both levels of earnings and earnings trajectories in the pre-reform period. Allowing for a less strict matching (by relaxing the requirement of common support or adjusting the caliper upwards) increases the sample size but quickly results in less comparable municipalities in terms of earnings trends in the pre-period; hence violating the parallel trends assumption needed for the difference-in-differences analysis.

A. All Swedish municipalities



B. The matched sample of municipalities

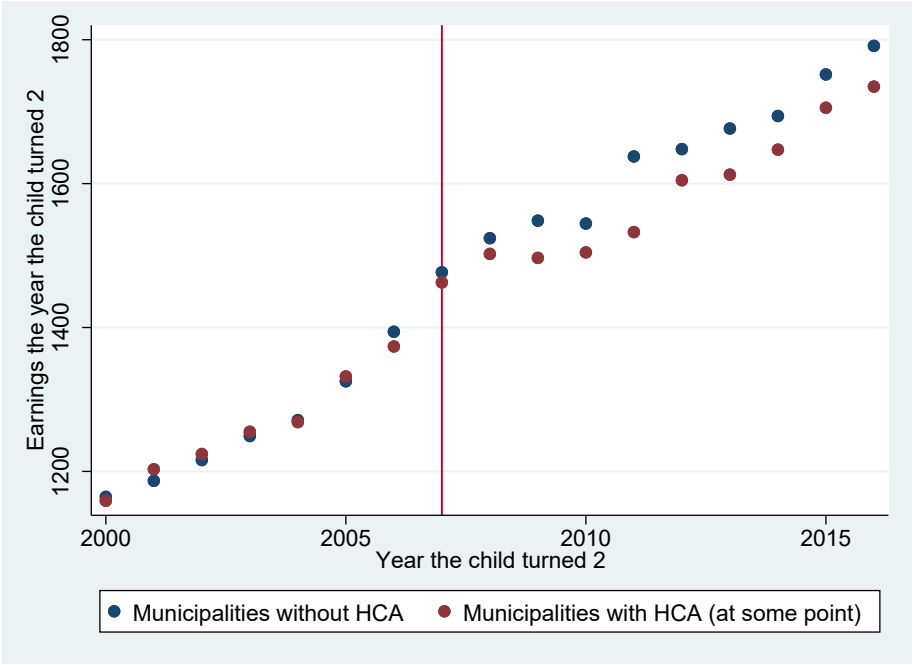


Figure A2. Mothers' earnings the year the child tuns 2 in municipalities with and without HCA.

Note: The vertical line marks the year *before* it was possible for municipalities to offer HCA. Earnings are reported in 1,000 SEK (2017 value).

Appendix B: Additional figures and tables

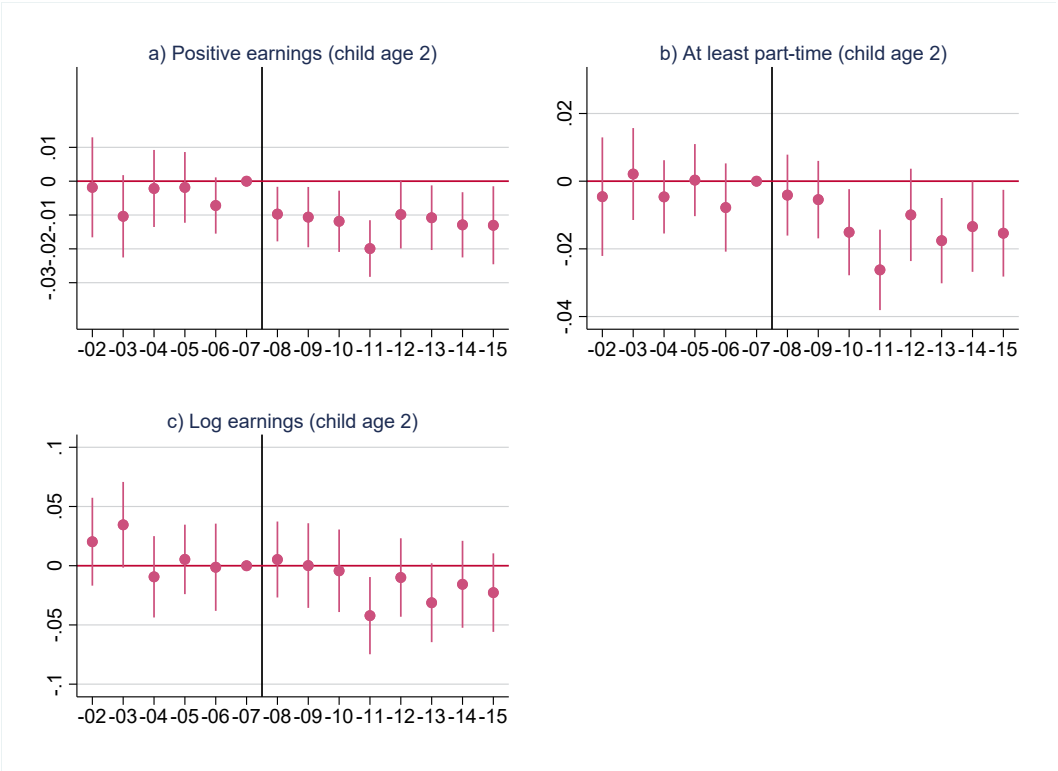


Figure B1: Time-varying effects of living in an HCA municipality. Additional earnings-related outcomes.

Note: 2007, the last year before the reform, is the reference year. 'HCA municipality' is defined as a municipality that offered HCA at least at some point.

Figure B2. Placebo-analysis: Effects of HCA eligibility before and after childbirth. Additional earnings-related outcomes.

Note: Estimates of model 1 before and after childbirth. In the placebo-regressions (year -3, -2, -1, 0), the eligibility variable takes the value 1 if the mother will become eligible for HCA the year the child turns 1, otherwise it is 0. For the effect estimates (year 1, 2, 3), eligibility is defined as before, i.e. based on the current year, and year 2008 is excluded. The sample is limited to first time mothers.

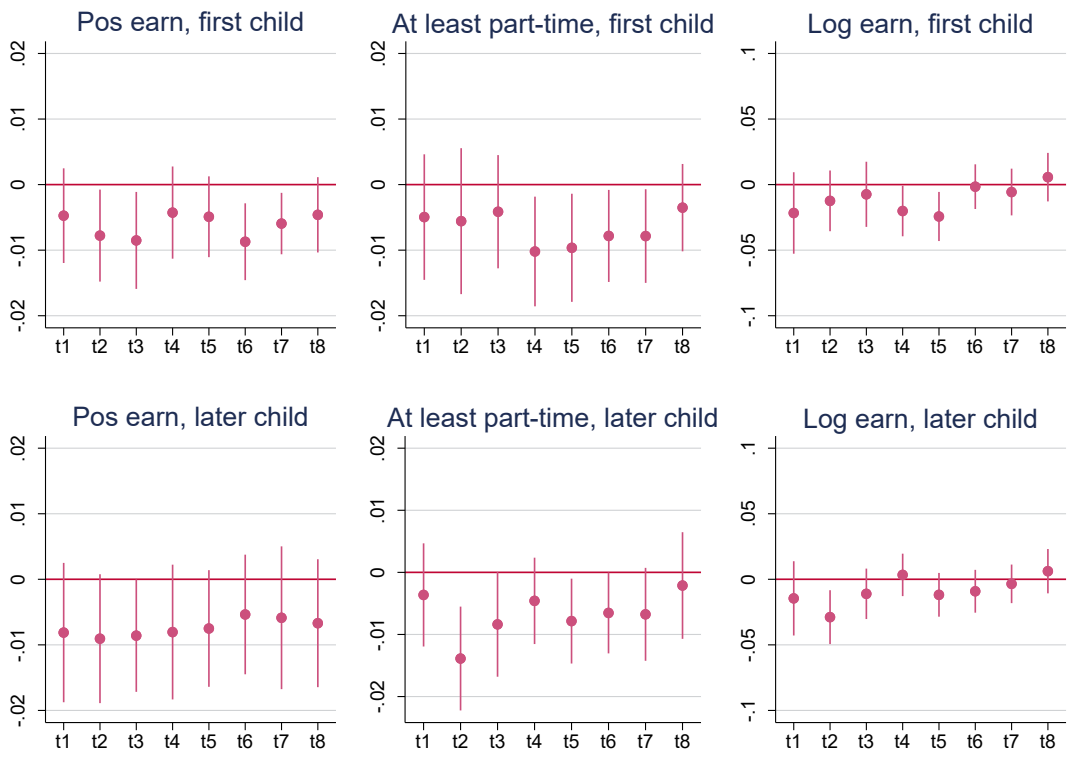


Figure B3. Long run impact of HCA eligibility, separately for first-time mothers and mothers who give birth to their second or later child. Additional outcomes.

Note: Estimates of model 1 with the full set of background controls. The eligibility variable takes the value 1 if the municipality of residence offered HCA at any time during year 1–3 following childbirth, and 0 otherwise. The estimated impact of HCA eligibility on the probability of HCA take-up (at any time year 1–3 following childbirth) is 0.048 (0.004) for first-time mothers, and 0.063 (0.005) for mothers who give birth to a second or later child.

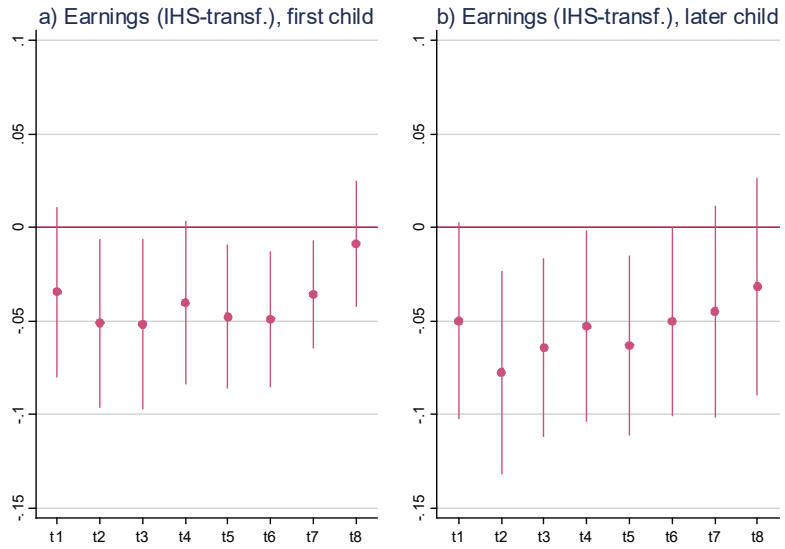


Figure B4. Long run impact of HCA eligibility, separately for first-time mothers and mothers who give birth to their second or later child. Controlling for benefit eligibility for later born siblings.

Note: Estimated effects in percent. The figure shows estimates of model 1 with the full set of controls, as well as a control for benefit eligibility status for later born children. The eligibility variable takes the value 1 if the municipality of residence offered HCA at any time during year 1–3 following childbirth, and zero otherwise. The estimated impact of HCA eligibility on the probability of HCA take-up (at any time year 1–3 following childbirth) is 0.048 (0.004) for first-time mothers, and 0.063 (0.005) for mothers who give birth to a second or later child.

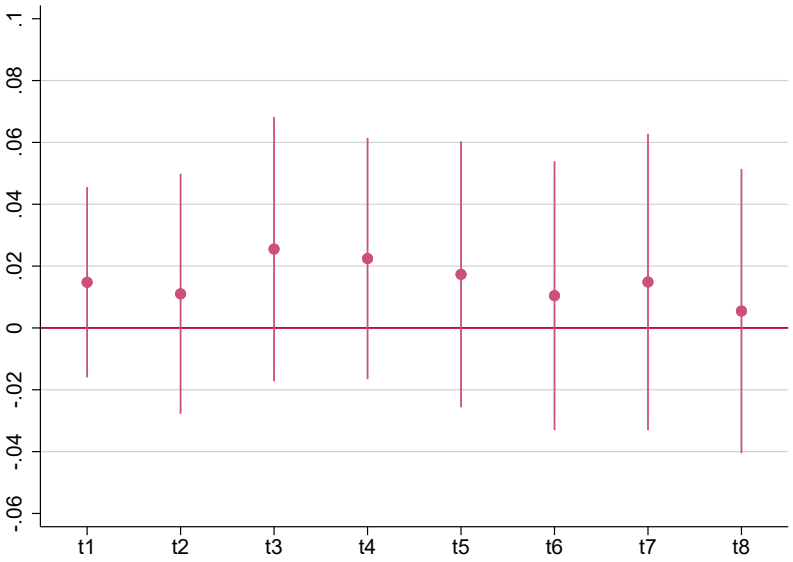


Figure B5. Long run impact of HCA eligibility on the father's earnings (IHS-transf., effects in %)

Note: Estimates of model 1 with the full set of controls. Both first- and later-born children are included. The eligibility variable takes the value 1 if the municipality of residence offered HCA at any time during year 1–3 following childbirth, and zero otherwise.

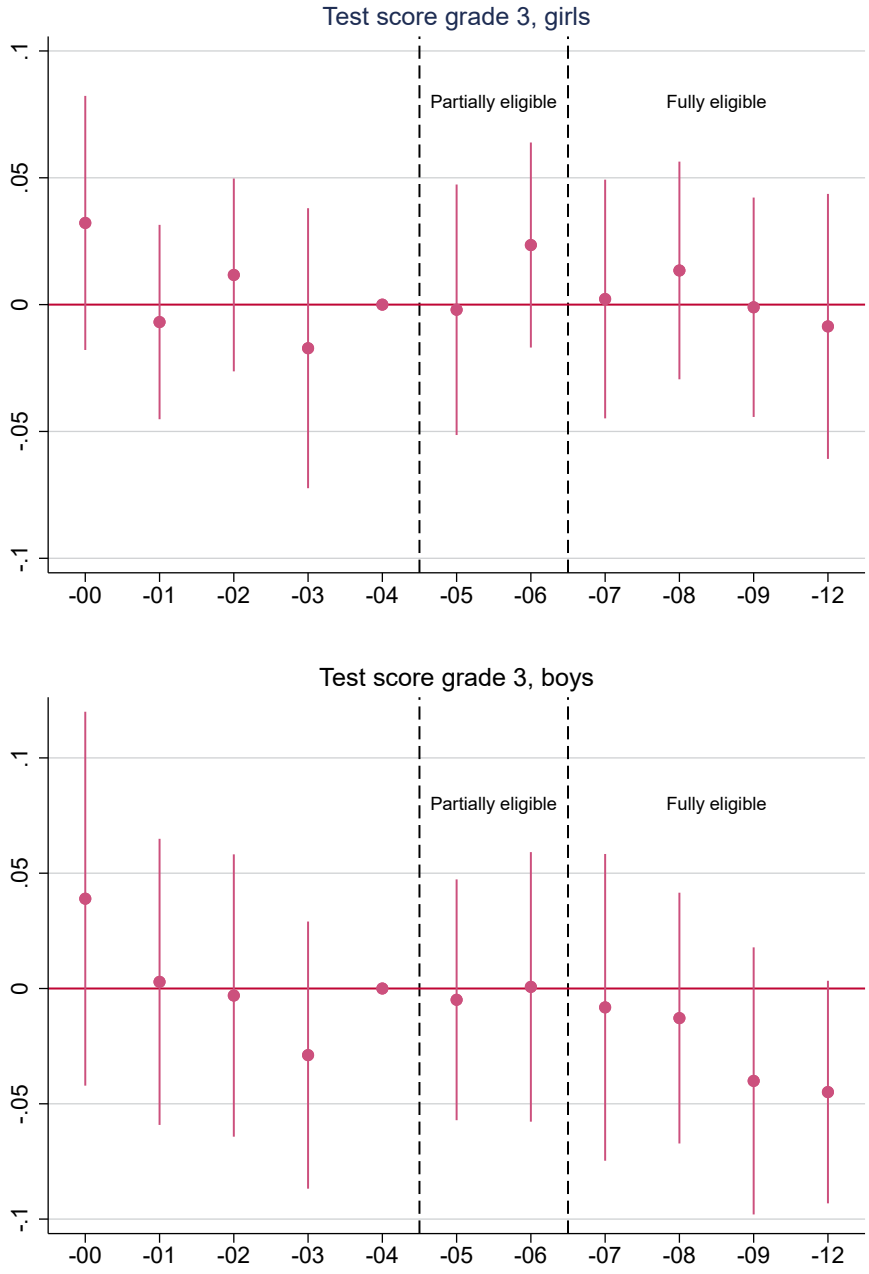


Figure B6. Effects of living in an HCA-municipality on 3rd grade test scores, separately by birth cohort

Note: Birth cohort 2004 (the last cohort that was completely unaffected by the HCA) is the reference cohort. The first dashed line marks difference between no treatment and partial treatment (cohort 2005 is largely untreated even if living in an HCA municipality, while cohort 2006 is partially treated); the second dashed line marks the difference between partial and full treatment (cohorts 2007–2012 were fully eligible for the HCA, i.e. from age 1–3). Cohort 2010 and 2011 are excluded since the national tests were cancelled during the covid 19-pandemic. The regressions include the full set of background controls (see Table 6).

Table B1. Descriptive statistics for the sample used for estimation of effects on mothers' labor market outcomes, separately for births taking place before and after 2009.

	(1) HCA *before 2009*	(2) No HCA *before 2009*	(3) HCA *2009 and later*	(4) No HCA *2009 and later*
Mother's age at childbirth	30.09	30.27	30.13	30.39
	(5.10)	(5.14)	(5.30)	(5.35)
Mother born in non-Nordic country	0.18	0.19	0.26	0.25
	(0.39)	(0.39)	(0.44)	(0.43)
Immigrated <3 years before childbirth	0.04	0.04	0.06	0.06
	(0.19)	(0.19)	(0.23)	(0.23)
Mother has at most compulsory education[a]	0.11	0.11	0.12	0.11
	(0.32)	(0.31)	(0.32)	(0.32)
Mother has at most upper secondary educ.[a]	0.46	0.44	0.38	0.36
	(0.50)	(0.50)	(0.49)	(0.48)
Mother has post-secondary education[a]	0.37	0.40	0.43	0.46
	(0.48)	(0.49)	(0.50)	(0.50)
Missing data on mother's education[a]	0.05	0.05	0.07	0.07
	(0.22)	(0.22)	(0.26)	(0.26)
Mother's first child	0.43	0.45	0.43	0.45
	(0.50)	(0.50)	(0.49)	(0.50)
Mother's second child	0.36	0.35	0.37	0.36
	(0.48)	(0.48)	(0.48)	(0.48)
Mother's third or later child	0.21	0.20	0.21	0.19
	(0.41)	(0.40)	(0.41)	(0.39)
Child is a girl	0.48	0.49	0.49	0.49
	(0.50)	(0.50)	(0.50)	(0.50)
Child's age at immigration[b]	1.31	1.29	1.52	1.48
	(1.15)	(1.16)	(1.16)	(1.16)
Number of births	*207,847*	*187,774*	*131,811*	*118,830*

Note: The table reports sample means and standard deviations (within parenthesis) for mothers residing in the matched sample of municipalities. 'HCA municipalities' refers to municipalities that offered HCA at some point; 'no HCA' refers to municipalities that never offered this benefit. [a]Education is measured at child birth. [b]Calculated for children who immigrated to Sweden between age 0–3.

Table B2. Share claiming HCA among entitled families in our sample, and average number of months HCA was received among recipients, 2010-2015.

	All mothers	Mother worked before childbirth[a]		Mother has post-secondary education		Mother has immigrant background[b]	
		Yes	No	Yes	No	Yes	No
Claimed HCA, child age 2	0.07	0.06	0.11	0.06	0.08	0.08	0.06
Total number of months with HCA, ages 1–3 [c]	9.20	8.71	10.38	8.60	9.55	9.83	8.94
Share of full-time usage [c]	99.86	99.84	99.92	99.74	99.94	99.94	99.84
No. of obs.	132,036	102,273	20,373	58,331	73,705	31,692	100,344

Note: HCA take-up is not included in our data until 2010.[a] Earnings before childbirth are measured at year t-2. [b]Mothers who are born in a Nordic country are defined as 'native', and mothers who are born outside the Nordic region are defined as 'immigrants'. [c] Share of full-time and number of months with HCA refers to 2010 and are approximations based on a sub-sample (90–95 % of the original sample of recipients) for which we have reliable data on this variable. Share of full-time usage varies between 95 and 100% for all years and subgroups.

Table B3. Descriptive statistics for the sample used for estimation of effects on children's test scores, separately for births taking place before and after 2009.

	(1) HCA *before 2009*	(2) No HCA *before 2009*	(3) HCA *2009 and later*	(4) No HCA *2009 and later*
Mother's age at childbirth	30.11	30.29	30.17	30.46
	(5.07)	(5.11)	(5.26)	(5.32)
Born in non-Nordic country	0.17	0.17	0.23	0.22
	(0.38)	(0.38)	(0.42)	(0.42)
Immigrated <3 years before childbirth	0.03	0.03	0.05	0.04
	(0.17)	(0.17)	(0.21)	(0.21)
Child is a girl	0.49	0.49	0.49	0.50
	(0.50)	(0.50)	(0.50)	(0.50)
Mother has at most compulsory education	0.11	0.11	0.11	0.11
	(0.31)	(0.31)	(0.31)	(0.31)
Mother has at most upper secondary educ.[a]	0.47	0.45	0.39	0.37
	(0.50)	(0.50)	(0.49)	(0.48)
Mother has post-secondary education[a]	0.38	0.40	0.44	0.47
	(0.48)	(0.49)	(0.50)	(0.50)
Missing data on mother's education[a]	0.04	0.04	0.06	0.06
	(0.20)	(0.20)	(0.23)	(0.23)
Mother's first child	0.43	0.45	0.43	0.45
	(0.50)	(0.50)	(0.49)	(0.50)
Mother's second child	0.36	0.35	0.37	0.36
	(0.48)	(0.48)	(0.48)	(0.48)
Mother's third or later child	0.21	0.20	0.20	0.19
	(0.41)	(0.40)	(0.40)	(0.39)
Child's age at immigration[b]	1.30	1.26	1.47	1.49
	(1.16)	(1.17)	(1.16)	(1.16)
Number of children	*195,589*	*176,078*	*48,749*	*43,922*

Note: The table reports sample means, for continuous variables, and standard deviations (within parenthesis) for children residing in the matched sample of municipalities. 'HCA' refers to municipalities that offered HCA at some point; 'no HCA' refers to municipalities that never offered HCA. The sample includes children born 2000-2009 and 2012 (and a few born 2010, 2011, and 2013; test years 2010-2019, and 2022), [a]Education is measured at childbirth. [b]Calculated for children who immigrated between age 0–3.

Table B4. Descriptive statistics for the sample used for estimation of effects on enrollment in childcare., separately for births taking place before and after 2009.

	(1) HCA before 2009	(2) No HCA before 2009	(3) HCA 2009 and after	(4) No HCA 2009 and after
Mother's age at childbirth	30.20	30.59	30.13	30.82
	(5.15)	(5.11)	(5.33)	(5.23)
Born in non-Nordic country	0.21	0.23	0.25	0.25
	(0.41)	(0.42)	(0.43)	(0.43)
Immigrated <3 years before childbirth	0.04	0.04	0.04	0.04
	(0.19)	(0.19)	(0.20)	(0.20)
Mother has at most compulsory education[a]	0.12	0.11	0.12	0.11
	(0.33)	(0.31)	(0.33)	(0.31)
Mother has at most upper sec. education[a]	0.44	0.39	0.40	0.34
	(0.50)	(0.49)	(0.49)	(0.48)
Mother has post-secondary education[a]	0.38	0.44	0.42	0.49
	(0.48)	(0.50)	(0.49)	(0.50)
Missing data on mothers' education[a]	0.06	0.06	0.06	0.06
	(0.24)	(0.24)	(0.24)	(0.24)
Mother's first child	0.51	0.55	0.46	0.50
	(0.50)	(0.50)	(0.50)	(0.50)
Mother's second child	0.38	0.36	0.38	0.36
	(0.49)	(0.48)	(0.49)	(0.48)
Mother's third or later child	0.10	0.09	0.16	0.14
	(0.30)	(0.29)	(0.37)	(0.34)
Child is a girl	0.49	0.48	0.49	0.49
	(0.50)	(0.50)	(0.50)	(0.50)
Child's age at immigration[b]	1.88	2.06	1.59	1.70
	(2.26)	(2.63)	(1.51)	(1.62)
Number of children	*71,329*	*77,348*	*11,100*	*12,316*

Note: The table reports sample means and standard deviations (within parenthesis) for children residing in matched municipalities. 'HCA' refers to municipalities that offered HCA at some point; 'no HCA' refers to municipalities that never offered this benefit. Data come from the "preschool database" and includes children born 2002-2009 from 44 municipalities. [a]Education is measured at childbirth. [b]Calculated for children who immigrated to Sweden between age 0–3.

Table B5: Effects of HCA eligibility on mothers' earnings the year the child turns 2. Sensitivity of the IHS transformation of the outcome variable to units of measurement.

Unit of measurement	Coef.	s.e.	Obs.	R-squared
SEK 1	-0.128***	0.045	535,379	0.298
SEK 10	-0.107***	0.037	535,379	0.303
SEK 50	-0.113***	0.039	535,379	0.301
SEK 100	-0.086***	0.030	535,379	0.307
SEK 500	-0.071***	0.024	535,379	0.309
SEK 1,000	**-0.065*** **	**0.022**	**535,379**	**0.310**
SEK 5,000	-0.050***	0.017	535,379	0.308
SEK 10,000	-0.043***	0.015	535,379	0.306

Notes: Year 2008 is excluded from all estimations. All regressions control for year and municipality fixed effects as well as mother's age, age^2, born in non-Nordic country, immigrated <3 years before childbirth, education level (3 categories), missing data on education level, and earnings two years before the birth of the first child. Robust standard errors, clustered on municipality, in parentheses. *** p<0.01, ** p<0.05, * p<0.1. **Bold** marks the option suggested by the R^2-criterion, as avocated in Aihounton and Henningsen (2021).

Table B6. Short run effects of HCA eligibility on additional labor market outcomes, different child ages

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
		age 1			age 2			age 3	
A. Positive earnings									
Eligible	-0.011	-0.007	-0.006*	-0.014*	-0.009*	-0.009***	-0.014*	-0.009**	-0.009***
HCA	(0.010)	(0.005)	(0.004)	(0.009)	(0.005)	(0.004)	(0.008)	(0.004)	(0.003)
Obs.	583,456	583,456	583,456	588,754	588,754	588,754	596,164	596,164	596,164
R^2	0.012	0.245	0.354	0.013	0.275	0.364	0.011	0.271	0.339
Outcome mean[a]	0.744	0.744	0.744	0.802	0.802	0.802	0.811	0.811	0.811
B. Working at least part-time/part of the year									
Eligible	-0.007	-0.006	-0.005	-0.016*	-0.011**	-0.011**	-0.010	-0.006	-0.006*
HCA	(0.006)	(0.004)	(0.004)	(0.009)	(0.005)	(0.004)	(0.007)	(0.004)	(0.004)
Obs.	583,456	583,456	583,456	588,754	588,754	588,754	596,164	596,164	596,164
R^2	0.009	0.085	0.113	0.007	0.175	0.239	0.005	0.164	0.208
Outcome mean[a]	0.208	0.208	0.208	0.505	0.505	0.505	0.509	0.509	0.509
C. Log earnings (earn >0)									
Eligible	-0.021	-0.023*	-0.024**	-0.019	-0.019*	-0.022**	-0.006	-0.007	-0.010
HCA	(0.015)	(0.013)	(0.012)	(0.013)	(0.010)	(0.009)	(0.012)	(0.010)	(0.009)
Obs.	432,158	432,158	432,158	469,972	469,972	469,972	482,228	482,228	482,228
R^2	0.020	0.064	0.089	0.022	0.111	0.145	0.018	0.102	0.122
Demo-graphic controls	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Control for pre-birth ear-nings[b]	No	No	Yes	No	No	Yes	No	No	Yes

Notes: Year 2008 is excluded from all estimations. All regressions control for year and municipality fixed effects. The following demographic controls are included in col. 2, 3, 5, 6, 8 and 9: mother's age, age^2, foreign background (10 categories), immigrated <3 years before childbirth, education level (3 categories), and missing data on education level. Robust standard errors, clustered on municipality, in parentheses. *** p<0.01, ** p<0.05, * p<0.1. [a]Outcome means are calculated for all untreated observations. [b]Pre-birth earnings refers to yearly earnings two years before the birth of the first child. We also control for missing data on this variable.

Table B7. Short run effects of HCA eligibility on fathers' labor market outcomes, different child ages

	(1) age 1	(2) age 1	(3) age 2	(4) age 2	(5) age 3	(6) age 3
A. Positive earnings	0.001	0.003	-0.001	0.002	-0.001	0.002
Eligible for HCA	(0.007)	(0.004)	(0.007)	(0.003)	(0.006)	(0.003)
Observations	575,117	575,117	579,725	579,725	584,291	584,291
R-squared	0.010	0.299	0.009	0.275	0.009	0.253
Outcome mean[a]	0.752	0.752	0.810	0.810	0.819	0.819
B. At least part-time	0.003	0.006*	-0.003	0.001	-0.003	0.001
Eligible for HCA	(0.007)	(0.003)	(0.008)	(0.003)	(0.008)	(0.004)
Observations	575,117	575,117	579,725	579,725	584,291	584,291
R-squared	0.011	0.308	0.011	0.315	0.010	0.305
Outcome mean[a]	0.210	0.210	0.511	0.511	0.515	0.515
C. Log earnings (earn > 0)	0.000	0.007	-0.004	0.003	-0.009	-0.003
Eligible for HCA	(0.009)	(0.010)	(0.008)	(0.008)	(0.008)	(0.006)
Observations	505,671	505,671	509,458	509,458	513,786	513,786
R-squared	0.009	0.192	0.012	0.192	0.014	0.188
D. IHS transf. earn., % effect	0.008	0.024	-0.012	0.010	-0.012	0.009
Eligible for HCA	(0.044)	(0.019)	(0.043)	(0.019)	(0.042)	(0.019)
Observations	575,117	575,117	579,725	579,725	584,291	584,291
R-squared	0.011	0.387	0.011	0.362	0.012	0.337
Covariates included	No	Yes	No	Yes	No	Yes

Notes: Year 2008 is excluded from all estimations. All regressions control for year and municipality fixed effects. Col. 2,4 and 6 additionally control for mother's and father's age, age^2, foreign born (10 categories), immigrated <3 years before childbirth, education level (3 categories), missing data on education level, father's yearly earnings two years before the birth of the first child with the mother in question, and missing data on this variable. Robust standard errors, clustered on municipality, in parentheses. *** p<0.01, ** p<0.05, * p<0.1. [a]Outcome means are calculated for all untreated observations.

Tabell B8. Effects of HCA eligibility on enrollment in childcare, separately for girls and boys

	(1) Girls age 1	(2) Boys age 1	(3) Girls age 2	(4) Boys age 2	(5) Girls age 3	(6) Boys age 3
Eligible for HCA	-0.028	-0.015	-0.027**	-0.028**	-0.001	-0.002
	(0.017)	(0.015)	(0.011)	(0.010)	(0.009)	(0.006)
Observations	72,801	77,094	61,289	65,363	50,993	53,903
R-squared	0.291	0.295	0.084	0.078	0.048	0.044
Outcome mean[a]	0.467	0.472	0.829	0.836	0.862	0.866
Demographic controls	Yes	Yes	Yes	Yes	Yes	Yes
Control for pre-birth earn.[b]	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Year 2008 is excluded from all estimations. All regressions control for year and municipality fixed effects as well as mother's and father's age, age^2, mother/father born in non-Nordic country, immigrated <3 years before child-birth, education level (3 categories), missing data on education level, child's gender and birth month, and missing father data. Robust standard errors, clustered on municipality, in parentheses. *** p<0.01, ** p<0.05, * p<0.1. [a]Outcome means are calculated for all untreated observations. [b]Pre-birth earnings refers to the mother's yearly earnings two years before the birth of the first child. We also control for missing data on this variable.

Table B9. Effects on the probability of having a second child, by different ages of the first child

	(1) age 2	(2) age 3	(3) age 4	(4) age 5	(5) age 6
Eligible for HCA	-0.001	-0.008	-0.011	-0.009	-0.010
	(0.004)	(0.007)	(0.008)	(0.009)	(0.009)
Observations	283,494	283,494	283,494	283,494	283,494
R-squared	0.061	0.157	0.222	0.257	0.276
Outcome mean[a]	0.281	0.449	0.527	0.562	0.581

Notes: All regressions control for year and municipality fixed effects as well as mother's age, age^2, foreign background (10 categories), immigrated <3 years before childbirth, education level (3 categories), and missing data on education level. Robust standard errors, clustered on municipality, in parentheses. *** p<0.01, ** p<0.05, * p<0.1. [a]Outcome means are calculated for all untreated observations.

Table B10. Effects of HCA eligibility on fathers' earnings (IHS-transf., % effect) , child age 2

	(1) Mother did not work before childbirth[a]	(2) Mother worked before childbirth[a]	(3) Mother has low education[b]	(4) Mother has high education[b]	(5) Immigrant mother[c]	(6) Native mother[c]
Eligible HCA	0.056	-0.056**	-0.001	0.014	0.030	-0.012
	(0.068)	(0.023)	(0.063)	(0.017)	(0.036)	(0.015)
Observations	114,329	465,899	115,037	464,688	339,895	239,830
R-squared	0.195	0.014	0.202	0.102	0.211	0.129

Notes: Year 2008 is excluded from all estimations. All regressions control for year and municipality fixed effects. Col. 2,4 and 6 additionally control for father's age, age^2, foreign born (10 categories), immigrated <3 years before childbirth, education level (3 categories), missing data on education level, yearly earnings two years before the birth of the first child with the mother in question, and missing data on this variable. Robust standard errors, clustered on municipality, in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Categorization into sub-groups is based on the *mother's* characteristics. [a]Mother's earnings before childbirth are measured at year t-2. [b]Low education is defined as the mother not having post-secondary education, and high education as the mother having post-secondary education. [c] Immigrant/native indicates whether the mother was born in the Nordic region or not.

Table B11. Effects of HCA eligibility on children's test scores in grade 3, Swedish and mathematics

	(1) Girls, maths	(2) Boys, maths	(1) Girls; Swedish	(2) Boys, Swedish
Eligible for HCA	0.001	-0.030*	-0.007	-0.023*
	(0.018)	(0.017)	(0.011)	(0.012)
Observations	207,383	217,695	207,215	216,323
R-squared	0.116	0.099	0.103	0.093
Outcome mean[a]	-0.00258	-0.0118	0.123	-0.0875

Notes: Test scores have been standardized within test year (school cohort) to have a mean of 0 and a standard deviation of 1. All regressions control for cohort and municipality fixed effects as well as mother's and father's age, age^2, foreign born mother and father (10 categories), if mother and father immigrated <3 years before childbirth, mother's and father's education level (3 categories), mother's and father's earnings before childbirth, missing data on parents' education and earnings, missing father data, and birth month of the child. Children born 2005 have been excluded as they (although treated) had very limited access to the HCA. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. [a]Outcome means are calculated for all untreated observations. The estimated impact of HCA eligibility on the probability of HCA take-up (at any time year 1–3 following childbirth) is 0.051 (0.004) for girls; 0.050 (0.004) for boys.

Appendix C: Additional analyses

C1. Childcare data: quality assessment and robustness tests

We assess the quality of the childcare data by comparing the observed number of 1–5-year-olds enrolled in childcare in the 52 matched municipalities between 2003 and 2010 with the corresponding numbers reported by the National Agency for Education (NAE).[55] For each year-observation and municipality, we calculate the response rate, defined as the number of children observed in childcare in our sample relative to the number of children enrolled in childcare as reported by the NAE. We consider an observation with a response rate of less than 0.7 as being of low quality. Table C1 shows the distribution of the number of low-quality year-observations (out of 8) among municipalities. Our main specification excludes municipalities with more than 1 low quality year-observation (6/52). The direction of our estimates for childcare enrollment is not sensitive to including all municipalities, but the magnitude of the estimates increases significantly (while the precision decreases). Excluding the three municipalities with lowest-quality data (7–8 low quality observations out of 8) yields estimates of somewhat greater magnitude for 1 and 2-year-olds than those reported in Table 3, as seen in Table C2.

Table C 1. Distribution of number of low-quality observations among municipalities

No. of low quality obs.	No. of municipalities	Percent municipalities
0	31	59.62
1	13	25.00
2	1	1.92
3	3	5.77
4	1	1.92
7	2	3.85
8	1	1.92

Note: The table shows the distribution of the number of low-quality year-observations among the 52 municipalities in our sample. A year-observations including less than 70 % of the number of children reported being enrolled in childcare by the NAE are defined as being of low quality.

[55] The NAE reports the number of 1–5-year-olds enrolled in preschool in October each year. The preschool data includes children enrolled in several types of childcare, including but not limited to preschool. Although the share of children enrolled in other forms of childcare is small (approximately 2 % in 2019), this means that the calculated percentages are likely to be slightly exaggerated.

Table C2. Effects of HCA eligibility on children's participation in childcare using a less restrictive sample of municipalities (lower requirement on data quality)

	(1) age 1	(2) age 1	(3) age 2	(4) age 2	(5) age 3	(6) age 3
Eligible for HCA	-0.025*	-0.028*	-0.037***	-0.037***	-0.010	-0.009
	(0.014)	(0.015)	(0.012)	(0.012)	(0.011)	(0.011)
Observations	160,294	160,294	135,557	135,557	112,282	112,282
R-squared	0.015	0.286	0.029	0.083	0.029	0.049
Outcome mean[a]	0.464	0.464	0.823	0.823	0.856	0.856
Controls	No	Yes	No	Yes	No	Yes

Notes: The three municipalities with the worst data quality have been excluded from the sample (i.e., 49 out of 52 municipalities are included). For these municipalities, 7-8 out of 8 year-observations have a response rate of <70 % as compared to the number of children enrolled in childcare reported by the NAE. Year 2008 is excluded from all estimations. All regressions control for year and municipality fixed effects. Col. 2,4 and 6 additionally control for mother's and father's age, age^2, foreign born (10 categories), immigrated <3 years before childbirth, education level (3 categories), and missing data on education level, earnings 2 years before childbirth, as well as child's gender and birth month and an indicator of missing father data. Robust standard errors clustered on home municipality in parentheses. *** p<0.01, ** p<0.05, * p<0.1. [a]Outcome means are calculated for all untreated observations.

Figure C2 displays the raw data for the same sub-sample of municipalities. Pre-reform trends in childcare enrollment are similar for HCA and non-HCA municipalities also for this less restrictive sample.

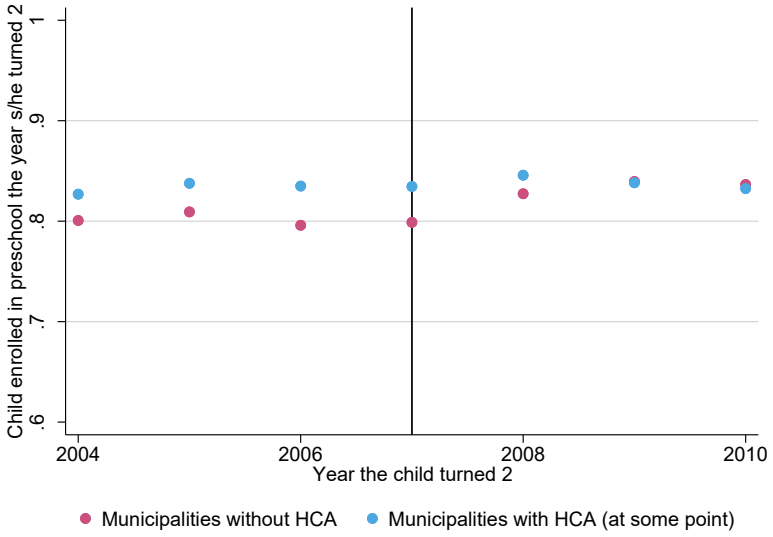


Figure C1. Childcare enrollment the year the child turns 2 for the less restricted sample of municipalities (lower requirement on data quality)

Note: Based on municipalities with at most one (yearly) observation with a response rate <=70 %, as compared to the number of children enrolled in childcare reported by the NAE (44 out of 52 municipalities). The vertical line marks the year *before* it was possible for municipalities to offer HCA.

C2. Impact on residential sorting

The fact that only some municipalities offered HCA may affect location choices: families with preferences for longer parental leave may choose to move to HCA municipalities be able to use this benefit. This type of behavior could then give rise to divergent compositional changes in municipalities with and without HCA, which may challenge a causal interpretation of the effect of HCA eligibility. To limit this potential problem, we have defined municipality of residence based on the municipality the mother lived in the year she gave birth. However, as mothers re-enter the sample each time they give birth, this type of compositional changes can still occur for mothers who have more than one child (i.e. if the family relocates to be able to use HCA for the first child, this will affect their municipality of residence at the birth of the second child.) To shed light on whether this type of sorting seems to be an issue, we investigate whether mothers become more likely to move from a 'non-HCA' to an 'HCA-municipality' after municipalities could offer this benefit. More specifically, we estimate the following model different years following child-birth:

$$Y_{imt+j} = \alpha + \varphi Non\_HCA_m + \lambda Post_t + \beta Non\_HCA_m * Post_t + \gamma_m + \delta_t + \theta X_i + e_{imt}$$

Y_{imt} is an indicator for whether mother *i*, who lived in municipality *m* at childbirth, has moved to an 'HCA municipality' *j* years after childbirth.[56] Again, a municipality is defined as an 'HCA municipality' if at any point offering this benefit. Non_HCA_m takes the value 1 if the mother lived in a 'non-HCA municipality' at childbirth, and 0 otherwise; hence, φ captures general moving patterns between 'non-HCA' and 'HCA municipalities'. $Post_t$ takes the value 1 for births taking place in the post-reform period, i.e. after 2007, and 0 for births taking place earlier.[57] The coefficient for the interaction term, β, thus captures if it became more common to move from a 'non-HCA' to an 'HCA-municipality' after the possibility of offering HCA was introduced. As in previous analyses, γ_m and δ_t represent municipality and calendar year fixed effects, and X_i includes demographic covariates (education, immigration background, and age). e_{imt} is the error term and standard errors are clustered at the municipality level.

The results, which are displayed in Table C3, do not indicate that the possibility of using HCA influenced families' location choices. The estimate for the interaction term has the opposite sign

[56] Hence, Y_{imt} takes the value 1 if the mother has moved to an 'HCA municipality', and 0 if she either has not moved or has moved to a 'non-HCA municipality'. Note that moves to *all* municipalities are included here, not only moves to municipalities that are included in our analysis sample.

[57] The full difference-in-differences model is here specified for simplicity. However, in practice, neither φ nor λ are identified as the model includes municipality of residence and time fixed effects.

from expected – i.e., showing that the probability of moving from a 'non-HCA' to a 'HCA municipality in fact declined following the reform – but in terms of size, it is of negligible importance.

Table C3. Effects on the probability of moving to an HCA municipality within 2 and 3 years after childbirth

	(1) t2	(2) t3
Non-HCA municipality * Post	-0.002*	-0.002
	(0.001)	(0.001)
Observations	619,733	617,932
R-squared	0.028	0.036
Outcome mean	0.048	0.062

Notes: Both regressions control for year and municipality fixed effects as well as mother's age, age^2, born in non-Nordic country, immigrated <3 years before childbirth, education level (3 categories), and missing data on education level. Robust standard errors, clustered on municipality, in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

C3. Event study analyses and placebo-estimates for various sub-groups

Figure C2 repeats the event study analysis (shown in Figure 5) separately for sub-groups of mothers, while Figure C3 repeats the placebo-analysis (shown in Figure 6) for the same sub-groups. Reassuringly, none of these analyses show patterns that indicate that the parallel trends assumption is likely to be violated.

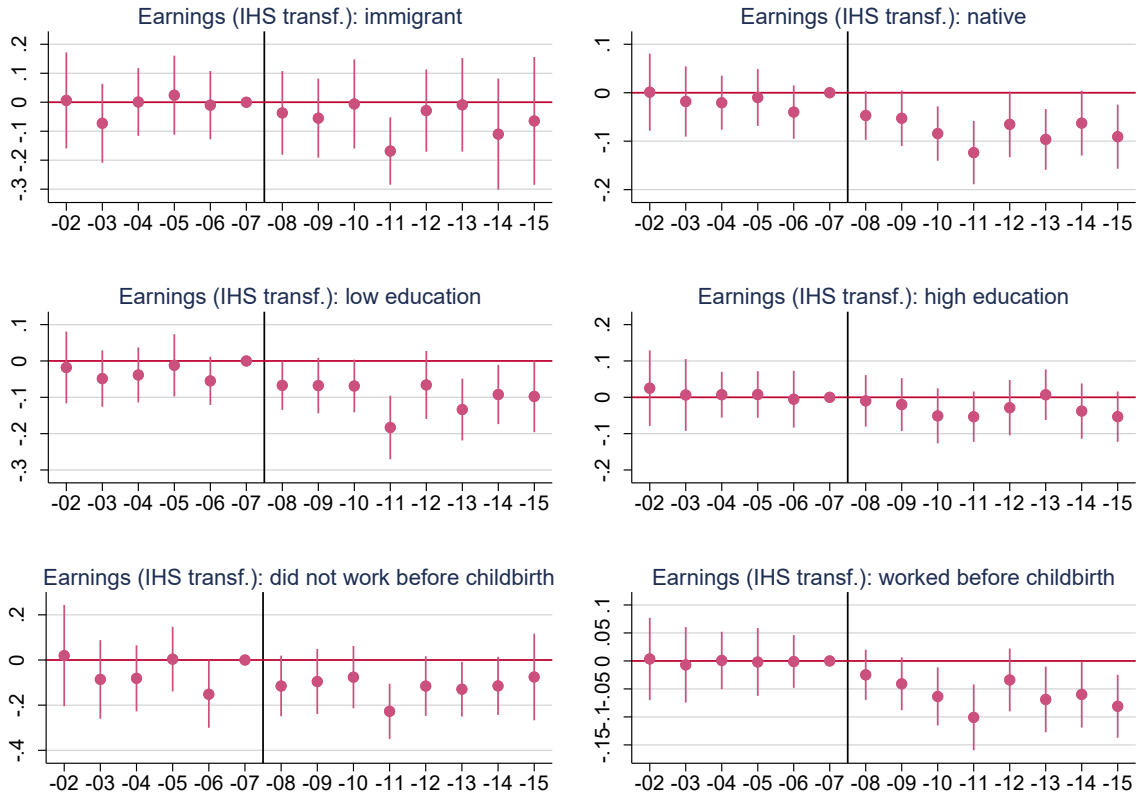


Figure C2: Time-varying effects of living in an HCA municipality, separately for sub-groups of mothers

Note: Effects on earnings are in percent. 2007, the last year before the reform, is the reference year. 'HCA municipality' is defined as a municipality that offered HCA at least at some point. Earnings are measured the calendar year the child turns two.

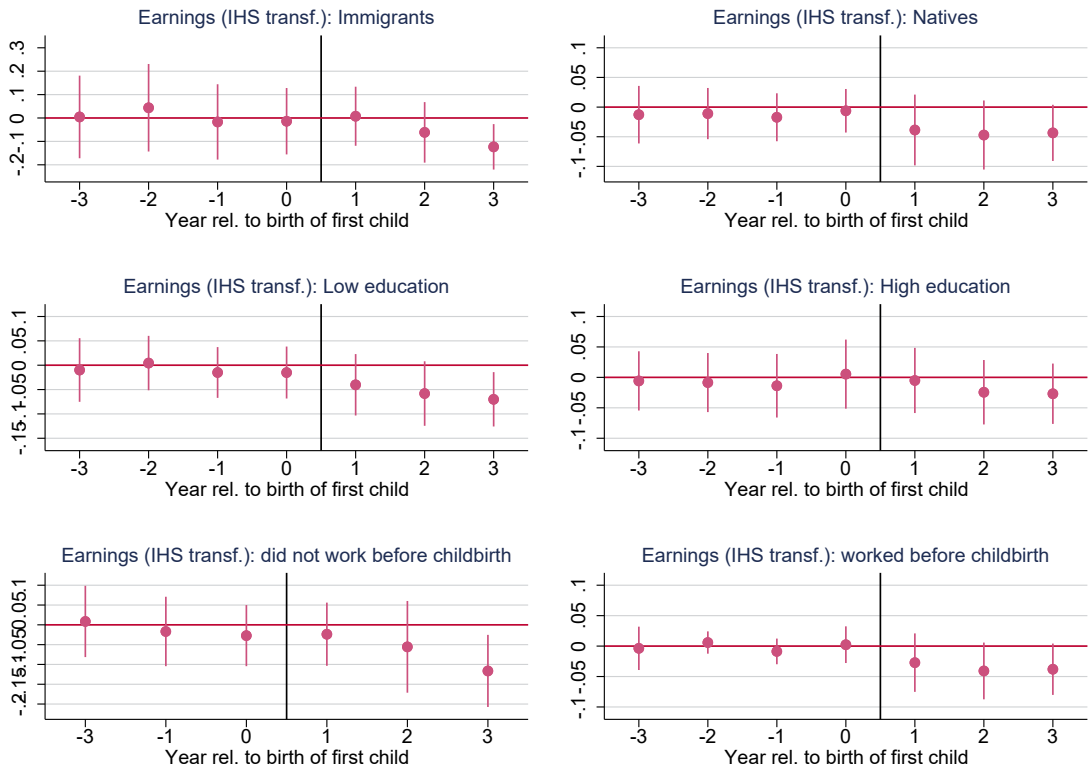


Figure C3. Placebo-analysis: Effects of HCA eligibility before and after childbirth, separately for sub-groups

Note: Effects on earnings are in percent. Estimates of model 1 before and after childbirth. In the placebo-regressions (year -3, -2, -1, 0), the eligibility variable takes the value 1 if the mother will become eligible for HCA the year the child turns 1, otherwise it is 0. For the effect estimates (year 1, 2, 3), eligibility is defined as before, i.e. based on the current year, and year 2008 is excluded. The sample is limited to first time mothers.

Figure C4 repeats the event study analysis for children's 3rd grade test scores (shown in Figure B7) separately for sub-groups. The point estimates are rather unstable in these cohort-by-cohort comparisons, but there are no clear indications of divergent trends among children born before the HCA was introduced.

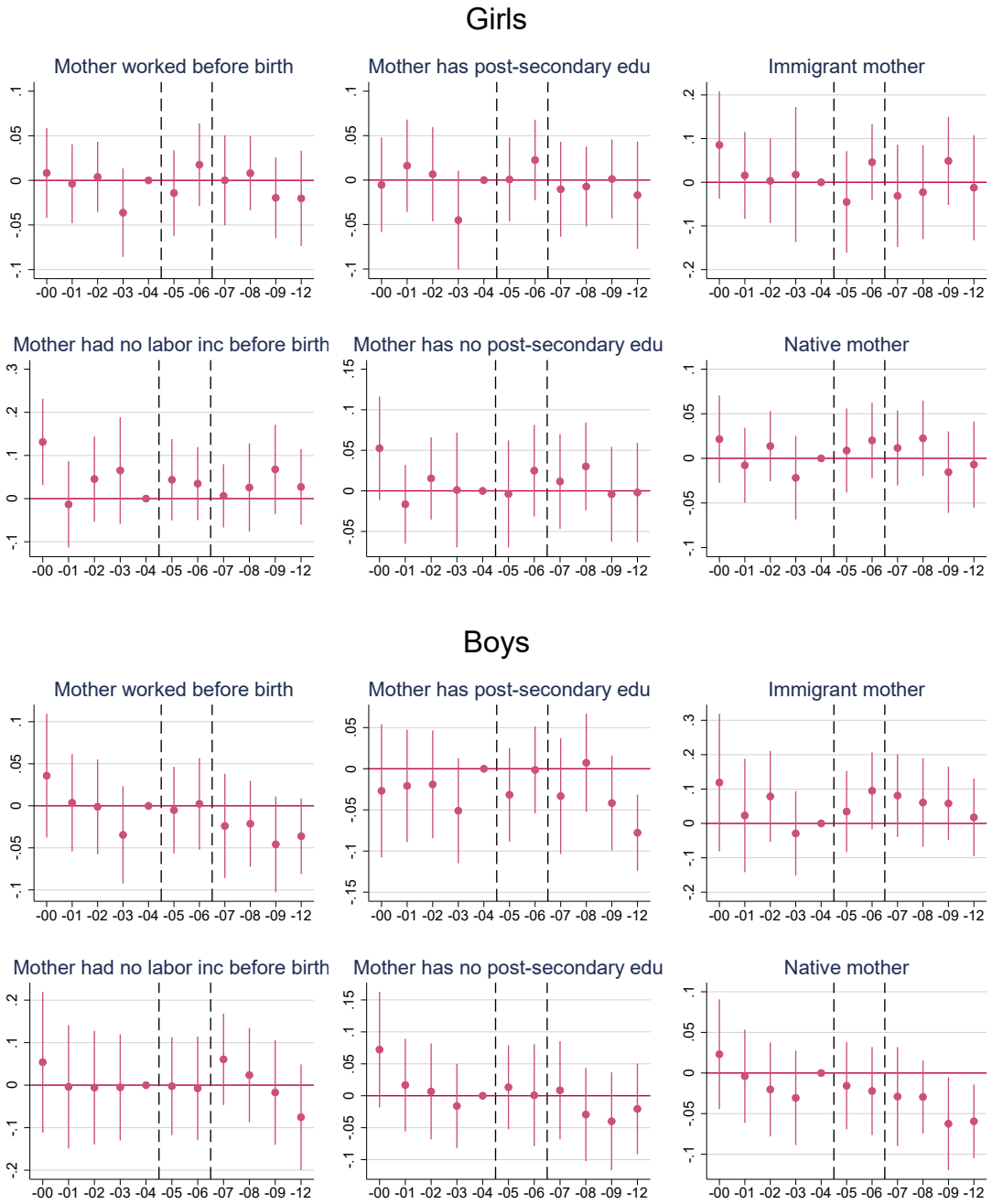


Figure C4. Effects of living in an HCA municipality on 3rd grade test scores, separately by birth cohort

Note: Birth cohort 2004 (the last cohort that was completely unaffected by the HCA) is the reference cohort. The first dashed line marks the difference between partial treatment and no treatment (cohort 2005 is largely untreated even if living in an HCA municipality, while cohort 2006 is partially treated); the second dashed line marks the difference between partial and full treatment (cohorts 2007–2012 were fully eligible for the HCA, i.e. from age 1–3). Cohort 2010 and 2011 are excluded since the national tests were cancelled during the covid 19-pandemic. The regressions include the full set of background controls (see Table 7).