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# Paying Moms to Stay Home: Short and Long Run Effects on Parents and Children

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**Abstract**

We study the impacts of a policy designed to reward mothers who stay at home rather than join the labor force when their children are under age three. We use regional and over time variation in child home care allowance to show that home care allowance decreases maternal employment in both the short and long term, with almost three-quarters of the supplement amount offset by lost labor income. The effects are large enough for the existence of home care benefit system to explain the higher child penalty in Finland than comparable nations. Home care benefits also negatively affect the early childhood cognitive test results of children at the age of five, increase the likelihood of choosing vocational rather than academic secondary education track, and increase youth crimes. We confirm that the mechanism of action is changing work/home care arrangements by studying a a day care fee (DCF) reform had the opposite effect of raising incentives to work. We find that this policy increased the labor force participation of mothers and participation of children to day care, and improved child early test and schooling outcomes. This parallel set of findings suggests that on average in Finland, shifting child care from the home to the market increases labor force participation and improves child outcomes.[1]

JEL: J13, J21, J38

Keywords: home care allowance, employment, child development, schooling

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# 1 Introduction

The past fifty years has seen an explosion of market work by mothers of young children around the world. In the U.S., for example, the labor force participation rate for mothers of children under age 6 has risen from 39% in 1976 to 65% today. Figure 1 shows the share of mothers with children under age 6 who are working across the OECD; the share varies 27% (Turkey) to 80% (Netherlands), with a weighted mean of 62%, and only four countries below 50%.[2]

This rapid rise in maternal work has not been accompanied by a comparable reduction in the work of fathers – leading to a huge rise in the share of children cared for by others. In the U.S., for example, currently 40% of pre-school age children are cared for primarily by a parent, with 30% in center-based child care, 10% in non-relative home care, and 19% in relative's care.[3] Given the substantial use of market child care, there has been a rapid growth as well in child care subsidies; explicit subsidies and tax credits amount to $13 billion/year in the U.S., and there are ongoing arguments for additional subsidies to make child care more affordable.[4] For example, most of the leading contenders for the Democratic Presidential nomination in the U.S. in 2020 proposed expensive new child care subsidy programs, and it was a central plank of President Biden's Build Back Better (BBB) proposal.

At the same time, others have argued that the pendulum has swung too far towards market work for mothers- and that children are suffering as a result. As U.S. Senator Michael Bennet recently said, "Caregiving is the most meaningful work a parent can do, but for some reason we've made it harder and harder for families".[5] These individuals argue that we should not be additionally subsidizing the ability of mothers to work outside the home, but should rather be delivering money to families with children regardless of mother work status.

The existing economics literature has largely taken two approaches to addressing this question. The first is to study policies which make child care more affordable. Studies of such policies have shown mixed effects on both maternal labor supply and child outcomes. The second is to study the effect of parental leave policies after childbirth; this literature has found generally positive impact on child outcomes.

This literature has faced three limitations. First, while most studies have focused

[2]Historical data for the US: https://www.dol.gov/agencies/wb/data/mothers-and-families.The2020data:https://ec.europa.eu/eurostat/web/microdata/european-union-labour-force-survey.

[3]https://nces.ed.gov/programs/digest/d18/tables/dt18_202.30.asp

[4]https://www.clasp.org/sites/default/files/publications/2020/09/2020_Child%20Care%20Assistance%20and%20Participation%202018.pdf and https://www.irs.gov/statistics/soi-tax-stats-individual-income-tax-returns-publication-1304-complete-report.

[5]"Stay-at-Home Parents Work Hard. Should They Be Paid?," New York Times, October 3, 2019.

either on effects of parental leave, or formal child care arrangements for older children, we know still relatively little about effects of programs that subsidize mothers to stay home, beyond paid leave programs that generally cover only the first year after birth. Such programs may provide the closest parallel to what some policy-makers claim that countries should be doing to offset the rush to maternal work. Second, most previous studies focus either on the effects of child care on immediate outcomes in early childhood, or on long-term outcomes. There is limited evidence on how the policy affects *both* early childhood outcomes as well as outcomes later in life when children grow older and join the labor force; the impact of such programs may be mitigated or strengthened through time. Finally, studies typically address one program in a vacuum, making it hard to separate program-specific effects from the general mechanisms through which they operate.

We address these shortcomings by studying the Finnish Home Care Allowance program (HCA). This program provides substantial payments to mothers who stay home with their children from age ten months through 3 years old, rather than placing the children in (almost exclusively publicly-financed) child care. The program has a long tradition in Finland. It was introduced in 1985 and more than 80% of mothers in Finland utilize the HCA. As a result, the share of children in formal child care is much lower in Finland than in other Nordic countries (see Table .17). We are able to utilize over time and across regions varying supplements to HCA that provide a credibly plausible causal identification, building on work by Kosonen (2014) that showed the strong negative short-term effects of these supplements on maternal labor supply for mothers who have a child eligible for HCA.

We use several different register data sets of parents and their children from Finland during 1988-2019. The most novel data are the early outcome measures that originate from development test of children done at Finnish child health clinics. The tests measure cognitive development and the readiness for school, and are done during our observation period in the same way for all children across Finland at ages 4 or 5 years old. We also provide data on a suite of longer-term child outcomes including: enrolling in academic versus vocational high school track; enrolling in college when children are 18 through 23 years old; and youth crimes. All these measures are linked with Finnish registers containing full population (FOLK) for 1988-2019, that allow us to study the labor market histories of parents and different outcome measures for their children, and link this with rich set of demographic characteristics.

Our results show that higher levels of HCA through municipal supplements lead to mothers significantly delaying their return to the labor market. We estimate that each 100 euro rise in the supplement reduced maternal work by 1.6 percentage points when the youngest child is one year old. Our results suggest that almost three-quarters of the income delivered by increased HCA allowances on the margin is offset by lower labor income. We also show that in the longer term the reduction in maternal earnings persists even after the HCA expires.

To put this result in a well-known context, we estimate a child penalty for mothers in Finland to be initially much larger, at over 70% than the estimates by (Kleven et al. 2019b,a) for Denmark at around 30% (in the year following child birth). The higher child penalty lasts for several years after the birth of first child in Finland. Our dynamic estimates utilizing supplement variation in HCA are sizeable enough to explain the entire difference in the short run child penalty between Finland and Denmark.

We then examine the impact on children. We find that municipal supplements led to a significant decline in child performance on early childhood cognitive development tests from the child health clinics. Our dynamic differences-in-differences graphs, which are a generalization of more commonly known event studies to a setting continuous variation in incentives in multiple locations and different points of time, show that there are no differing trends across municipalities prior to the change in supplements.

We find significant negative longer-term results as well. Higher subsidies decrease the likelihood of the child enrolling in an academic high school track as opposed to vocational track (or no secondary education). In the Finnish schooling system most children enrolling to college have finished the academic high school track. Thus, it is not surprising that we find a similar result for enrolment to college, which declines for children who were eligible for higher HCA through supplements when they were young (although the findings there are less robust due to limitations on coverage of later cohorts). Furthermore, we find significant increases in juvenile crime due to higher HCA, that is, an increase in the number of children who have been convicted by a court when they were between the ages 15 to 18 years old.

Since the literature on child outcomes generally finds that higher income improves child outcomes ((Dahl and Lochner 2012; Løken et al. 2012; Hoynes et al. 2016)), this suggests that the mechanism of action for this result is maternal work and the associated mode of child care. But we can confirm this conclusion by turning to another source of exogenous variation in family care: a day care fee reform in 1997. This reform nationally unified a previously municipality-specific schedule of day care fees, providing significant variation across households in the price of child care. The effect of the reform was that for some individuals the day care fees were reduced, while for others they were increased. Reduced day care fees improve the incentives for parents to be employed and not stay at home taking care of their children. We use the variation in reduced day care fees and find that as a result opposite effect to the home care allowance, increasing maternal work. Also, maternal usage of HCA decline and labor earnings increase due to reduced day care fees.

And we find results for children from this alternative source of variation which are largely the opposite of our findings for the HCA, albeit statistically weaker, that therefore support the same mechanisms. The child outcomes improve both in short (early test fail rate declines) and long run (enrolment to academic high school increases), and in magnitudes that are comparable to the HCA variation. Thus, these results are consistent with the conclusion that subsidizing market care for children improves outcomes, and paying parents to stay at home with children worsens them, even as income rises in both cases.

The paper proceeds as follows. In Section 2 we provide a review of the relevant previous literature. In section 3, we present the Finnish home care allowance and parental leave scheme. In section 4, we describe the data. Section 5 presents the empirical strategy and the main results, and also discusses the potential issues with two-way fixed effects estimates and evidence for robustness against these issues. Section 7 presents the results from day care fee reform in 1997, and Section 8 concludes.

# 2 Literature review

The dramatic shift from home care to child care for young children around the world has inspired a vast literature that have investigated the impact of family policies on both mother's and children's outcomes. We review briefly the literature here, starting with literature focusing on mother's careers and then moving to result on children.

## 2.1 Family policies and mother's careers

Many papers have examined how extension of paid parental leave affect mother's careers. Much of this literature focuses on short-extensions of a relatively short family leave, and find modest long-run effects of maternal leave on mother's careers (Schönberg and Ludsteck 2014; Lalive et al. 2014; Dahl et al. 2016). Related literature has examined how expansion of formal child care expansion has affected mother's labor supply. These studies find mixed results: Havnes and Mogstad (2015) find no impacts for Norway and Lundin et al. (2008) do not find any effect on maternal labor supply from reduced day care fees in Sweden, while Baker et al. (2008); Gelbach (2002); Lefebvre and Merrigan (2008) find positive labor supply responses in the US and the Canada. The negligible labor supply responses in Norway can be partly explained by the fact that the counterfactual for the formal child care often used to be non-formal child care arrangements, not home care provided by parents.

Recently, literature has documented that parenthood is associated with a long-lasting earning reduction for mothers (Angelov et al. 2016; Kleven et al. 2019b; Andresen and Nix 2022; Sieppi and Pehkonen 2019). Consequently, several papers have examined whether changes in family policies can contribute to this child penalty. Kleven et al. (2020) and Andresen et al. (2022) found that parental leave extensions have not affected child penalty in Austria and Norway. However, Andresen et al. (2022) find that expansion of public child care has decreased child penalty in Norway.

A small subset of countries have introduced child home care subsidies that parents can use if they want to take care of children after the formal parental leave, that is, family policies similar to the one we study in this article. Studies examining the impacts of the child home care subsidies in Germany (Collischon et al. 2022; Gathmann and Sass 2018), Norway (Naz 2004; Schøne 2004; Drange et al. 2015; Thoresen and Vattø 2019), Sweden (Giuliani and Duvander 2017) document negative effects on maternal employment.

The Finnish home care subsidy policy have been previously studied by one of the authors in the current study: Kosonen (2014) show that higher supplements reduce maternal labor supply. The current study utilizes the same data on municipal supplements, and extends it to year 2014, while Kosonen (2014) observed supplements until 2005, and considers impacts on a wider array of outcomes including longer-term mothers' outcomes and child outcomes. Two papers have also mimicked the same institutional setting from municipal supplements to study different outcomes: Österbacka and Räsänen (2022) consider heterogeneous effects in short-term maternal employment of the supplements along multiple dividing factors, and Riukula (2018) examines the impact of this policy on marital stability. The latter study finds that home care allowance increases marital stability.

## 2.2 Family policies and children's outcomes

A wide literature has examined how form of child care affects children. The evidence on the effects of parental leave extensions on child outcomes is mixed. Carneiro et al. (2015) find that initial introduction of paid maternal leave in Norway affected positively children's long-term outcomes (decline in high school dropout), while (Dahl et al. 2016; Dustmann and Schönberg 2012) find that later extensions of the Norwegian or in German parental leave had no impacts on child outcomes. Danzer et al. (2022) examines a parental leave reform in Austria that extended parental leave length from child's first birth date until the second one. They find that extension had no effects on longer term labor market or educational outcomes, but it improved children's health outcomes.

Even a larger literature has focused on the role of early education or formal child care on child outcomes (For reviews see Cascio (2015); Baker (2011); Elango et al. (2015)). Overall, it seems that targeted programs, such as Head start, have beneficial effects on children Currie and Thomas (1993); Garces et al. (2002); Carneiro and Ginja (2014) and that children of more disadvantaged families benefit more from universal programs Felfe and Lalive (2018); Cornelissen et al. (2018); on the other hand, broader programs of universal child care have more mixed effects. Some studies find that placing children to child care can cause negative wellbeing or development among children (Baker et al. 2008, 2019; Fort et al. 2020). Studies using data from Northern European countries, on the other hand, tend to find either positive or zero impacts of expansion of universal child care on cognitive outcomes (Havnes and Mogstad 2015, 2011; Silliman and Mäkinen 2022), and no effects on noncognitive outcomes (Gupta and Simonsen 2010). Most these studies focus on policies affecting children that are close to school starting age (Havnes and Mogstad 2015), where the counterfactual mode of care can also be informal care. Papers focusing on the effects of formal care of 1-2 years-old children (Drange and Havnes 2019; Fort et al. 2020; Felfe and Lalive 2018) find mixed results.[6]

Again, the literature most closely related to our paper is the one examining the effects of explicit programs to reward mothers who stay home with their children after paid maternal leave. Overall, programs subsidizing children's home care are less common and there has been just handful of countries utlizing such policies, namely Germany and other Nordic countries. Bettinger et al. (2014) examine a program similar to the Finnish one, a home care allowance subsidy in Norway. They look at the introduction of a bonus for staying at home, and for identification purposes look only at the older siblings of those children for whom the stay at home bonus applies and find that home care subsidy positively affects older siblings. Gathmann and Sass (2018) examine how introduction of child home care subsidy for parents who do not take their 2-year old to public childcare in east German state Thuringia affected children's early outcomes. The results show that child home care subsidies decreased maternal employment and affected negatively the cognitive and non-cognitive skills of boys, but not girls. Finally, Collischon et al. (2022) examine effects of home care subsidy that is eligible for parents of one and two year-old children. They find that increase in home care subsidy reduced mother's labor supply and increased the use of exclusive parental care, but only in the former Western Germany. They also find that the subsidy improved children's development, using data from one West German state.

Our study makes several contributions to existing literature. First, we analyze unique and long-lasting policy that subsidizes mothers to stay home for considerably longer than in other nations. The home care allowance has a long history in Finland, dating back

[6] Drange and Havnes (2019) exploit a lottery for available child care slots and find that enrolment at ages 1-2 improves language and mathematic skills at ages 6-7. Fort et al. (2020) find that early child care reduces children's intelligence scores. Felfe and Lalive (2018) find that children from more disadvantaged background benefit from early child care.

to the mid-1980s; perhaps as a result, caring for children at home has become the norm in Finland, and the share of mothers staying home with under school age children in Finland exceeds most other northern European countries, as shown in Figure 1.

The consequences of this policy on both mother's and children can thus differ from studies that are focusing on policies with lower take-up, shorter duration and a less well-defined counterfactual. A small number of studies have already documented that Finnish women decrease their labor supply as a results of this allowance (Kosonen 2014; Österbacka and Räsänen 2022; Riukula 2018). Our paper extends this analysis to a longer time period, and analyze, in the same framework, its impacts on child outcomes. Descriptive evidence indicates a positive correlation with formal child care use and children's outcomes in Finland (Hiilamo et al. 2018).

Second, our paper studies the effects of the home care allowance on rich set of outcomes on both mothers and children. Importantly, we are able to follow mother's careers several years after child birth, and thus indicate the total consequences on women's careers. Similarly, we can follow children from birth to early adulthood and thus investigate the effect of the policy on both early and late outcomes of these children. Our child outcome measures vary from a unique self-collected early childhood cognitive test results, to medium and long-term outcomes available from administrative records. Our study can thus build a much richer picture on the effects of child home care subsidies than previous studies.

Finally, we place the impacts of the home care allowance policy in context by examining a parallel policy that provided the opposite incentives by subsidizing formal child care. By comparing these findings, we can confirm the general conclusion that changing the relative incentives for home and market care in Finland had important effects on both mothers and their children.

# 3 Institutions

The Finnish government provides financial assistance for parents who want to take care of their children at home. The earnings-related maternity leave is paid to mothers from one-month prior to birth until the child is 9 months old. After that, one of the parents is eligible for the home care allowance (HCA), which is a relatively high subsidy for parents with a child under 3 years of age who is not in municipal or private (both publicly subsidized) day care. The home care allowance was introduced in Finland in 1985; takeup is predominantly among mothers.

The amount of HCA a family is eligible for depends on the family's characteristics and ranges from 300 to 700 euros per month. There is a fixed amount of 255 to 315 euros per month (depending on the year), which does not depend on income. In addition, there is a means-tested part targeted at medium- to low-income families, not exceeding 180 euros per month, and a sibling supplement, which is from 60 to 100 euros per month per sibling cared for at home. On top of these allowances, some municipalities in some years provide supplements to the HCA.

We exploit this variation in the municipality-specific supplements to identify the causal effect of the monetary variation of HCA mothers' and on children's outcomes. Some municipalities have no supplement policy, while others have introduced it at different points in time. In addition, supplements vary in their nominal amount per month and on the child-age threshold (until which age the supplement can be paid). Some municipalities also have a prior (child birth) employment requirement for the supplement. A typical supplement is under 200 euros per youngest child per month plus a sibling extra of 50 euros per month.

Table 1 describes the supplement data in municipal and annual level, weighted by population of one-year old children in the municipality over time. Column "Supplement" shows that the average amount of supplement per month is 200 euros conditional on having the supplement policy. The "Age thresh" column shows the average upper age threshold in years, which is 2.1 years. Sibling suppl. refers to the sibling supplement in euros per month when the policy is in place. Income dep. refers to whether or not supplement is conditional on family income, which occurs in 2% of observations. Prior work condition are imposed in 9% of municipalities. In 18% of observations all children, including older under school age siblings, are required to be taken care of at home.

If parents choose not to take care of their children themselves, they can either place their children in public or private daycare. Both child care options are subsidized by the government. Public day care is the predominant choice of day care in Finland. Every child under the age of 7 (school starting age in Finland) is entitled to a public day care place if their parents request it. Day care fees are subsidized by the government and families pay only a small share of total costs. The fees in public day care depend on family income: low income families might have zero fees, and after a threshold the fees increase with family income but are capped at quite low level (under 300 euros per child per month) even for the highest income families.

Private day care is subsidized by the private daycare allowance, and some municipalities pay a supplement to the private day care allowance. However, in the majority of cases private day care is more expensive than public day care. Only a small share of children attend private day care nationwide, most children instead attend public day care provided by the municipality if they are not in home care.

The quality of both private and public day care is controlled by legislation, for instance by setting the minimum number of workers per child. There is also a requirement for minimum number of child care teachers per number for children, and they are generally required to have college education. Day care centers also provide pre-school education for 6 years old. Municipal child care quality could vary because some municipalities want to invest in child care and hire more teachers, while others provide only the minimum quality threshold mandated by legislation.

# 4 Data

For the empirical analysis, we use administrative data from multiple sources containing information on the population of residents in Finland and spanning more than two decades, from 1988 to 2019. The rich ecosystem of data available in Finland allows us to undertake a wid- ranging exploration of the impact of child policies.

## 4.1 Data on Labor Force Participation, Income and Benefits

The main administrative data source is the Finnish Linked Employer Employee Data (FLEED), which provides information on various labor market and background characteristics of all 16-70 year-old residents in Finland (such as the starting and ending dates of employment spells, yearly income, plant, education etc). The sample we have includes all women born in 1948 or thereafter, and their spouses. We observe all (biological or non-biological) children living with these women in the same household. We then link additional data registries to these base data.

To identify the birth of children, we utilize the Medical birth registry for 1987-2018, which has information on all live births in Finland. We also match complete taxation and benefits records, which include earned and capital income, as well as the usage of HCA at the annual level by each individual.

The information on municipality-specific supplements to child home care allowance is obtained from two sources. First, we obtained information about the level of municipality specific supplement from Finnish Social Insurance Institution (KELA). This information was updated in the scond step with information collected directly from municipalities (by phone calls and email inquiries). For later years information was obtainable also directly from the webpages of the municipalities. The information consists of both the level of the supplement, as well as information of different municipality specific rules about the eligibility of the supplement (age threshold).

We also have some information about the supplements that we use to exclude some municipalities from the analysis. We categorically exclude municipalities that have discretionary rules about eligibility for supplements as in these cases we cannot determine based on information in administrative data which individuals are eligible for the supplement. We also exclude some municipalities for which we deem the information about supplements unreliable. These tend to be smaller municipalities, and as a consequence excluding or including them does not impact the main results.

In our analysis we exploit variation in the amount of supplement that depend on both the level of the supplement, as well as changes in age threshold. For example, in some municipalities the supplement may be only paid for children who are eligible for home care allowance and below 2, while in some it is paid to all those that are eligible for Home care allowance (i.e. under 36 months). In the empirical specification we include controls for child's age in order exploit just the variation in municipality specific rules that are not related to child characteristics.

Table 1 describes the supplement data. Figure 2 shows on map in red the municipalities that had a supplement policy in place in years 1995, 1998, 2001 and 2005, respectively. As is clear, the supplement was growing in popularity over time, throughout the country.

## 4.2 Early childhood data

Our early childhood data are collected from maternity clinics in Finland. The purpose of these clinics is to provide health and development checks for all children in Finland from just after birth until children go to school. This is a service provided publicly to all, and every child is expected to visit the clinic. Children typically visit these tests multiple times at different ages before entering school. The motivation for this program for children older than two years is to be able to treat any conditions that hinder neurological development.

In the child health clinic children are given tests for motor skills, cognitive development, ability to focus, and other tests to indicate the rate of neurological development. The individual tests are performed according to nationally set guidelines in the same way for each child by a nurse or a medical doctor in the maternity clinic. The tests are not used individually as indicators of poor age-specific development, but rather all the tests are taken together and evaluated comprehensively.

Our main early outcome originates from a comprehensive development check, which is conducted at age 5 prior to 2010 and at age 4 after that. The comprehensive test differs from the tests done at other ages in that all children are very emphatically encouraged to do this test. It is institutionally important as this is the main test that check whether children are ready for starting school.

We obtain the data from tests from information entered in an electronic system. We do not observe all children in a given year and municipality in these files, potentially because the information was not recorded in the electronic files (they might have used old-fashioned paper-files in these instances).

We use all measures from the comprehensive test at age five or four that could be associated with cognitive development, ability to focus or social skills. The individual tests we utilize are described in Table 2. The table shows for each test the average of a dummy indicating failing that individual test, the standard deviation and number of observations for that test. Individual tests are marked as fail or pass in the data. Our main outcome combines the tests in the table, for which we observe either plus or minus in the records and for children for which we observe all the tests. We use as an outcome in one of sensitivity tests a variable that complements the plus/minus observations with text notes.

The individual tests we consider for four years olds (from 2010 onwards) are Cross (needing to draw a cross, where the two lines intersect), Ask (the child is able to ask following types of questions: when and where?), Details (the child is able to explain details from a specific picture), and Colors (the child is able to identify three out of four main colors from a color card). The tests for five years old (prior to 2010) are Circle (the child can cut a circle from a paper with scissors), Square (the child is able to draw a square on paper), Human (the child can draw human that has at least head, body and limbs come out of body, not from head), and Instruct (the child is able to follow three-part instructions).

To increase the number of children observed observed (which is necessary for our day care fee analysis), we also use an alternative measure, which combines information from any observed tests at ages four through six years old. As the latest data from some municipalities is from year 2014, we need to restrict the birth cohorts to those born in 2008 the latest when using this outcome (this is due to using tests for six years old, while the main outcome uses tests for four years old for later years). An additional disadvantage with this measure is that we don't observe all children at the same age (e.g. one child may fail at age 4 and another at age 6),

Our alternative outcome measure uses the same tests as in the main outcome, and additionally more tests observed from different visits to child health clinic at different ages. The individual tests that are used to compose this alternative outcome measure are described in Appendix, Table A.13. Additionally, although the bulk of our observations are marked as pass or fail, for some observations these indicators are missing and instead there are open text comments describing how the test went. We include in this measurethee open text comments, when they seem to indicate that the child failed the test. An example would be that the open text field remarks (in Finnish) that: "Child only got two out of three correct" or that "Child did not want to do the test".

### 4.3 Data on later child outcomes

For longer term child outcomes we utilize Statistics Finland's EDUC-Student data. These data contain information on post-compulsory school enrolment from years 1991-2019. From these records we create three variables. First, we create indicators whether individual enrolled to vocational or academic high school track in the fall following compulsory school ending, at age 16 or 17. We also create an indicator whether the individual has enrolled to college (university or other tertiary education) during the study period or by given upper age limit.

We also examine the incidence of youth crimes, which we observe from Sentence Records for 1987-2019. These include minor and more severe offences (either convicted or not) that are handed in courts for all offenders that are 15 years or older. In the records we observe the type of crime accused of, exact timing of the crime, whether convicted or not and the nature of penalty. We use these data to create our measure of youth crime, whether you were convicted at district court at least once after age 15. We use as the upper age restriction 18 years of age in the main analysis and use different upper age limits in sensitivity analysis. We create indicator variables taking value one if individual ever (or by given age) appears in the records and zero otherwise.

### 4.4 Descriptive evidence

We first describe how maternal employment and home care develop by youngest child's age, when the youngest child is eligible for HCA. Figure 4 shows the profile of exiting from HCA for two different reasons: either having the next child or exiting to employment. The figure also shows the aggregate profile compiled by counting these two separate exiting reasons together. Moreover, the figure shows the birth of a next child, which triggers another maternity leave period. The combined profile of these two events, stop using the family benefits or having the next child, shows mothers stopping to use family benefits due to presumably returning to work. The data source for the figure is population data of usage of family benefits from SII, which the authority that administers different social benefits in Finland, including family leaves.

This latter profile shows that when the child is about 10 months old and the paid maternity leave stops and HCA period starts, fewer than 20% of mothers exit from maternity leave. Just prior to the child turning 36 months and HCA period ending, almost 90% of mothers have exited and returned to work or have had another child.

# 5 Empirical Strategy and Results

## 5.1 Dynamic and Standard Differences-in-Differences

To identify the effect of home care allowance on children's and their parent's outcomes we use a differences-in-differences (DiD) strategy with continuous treatment. Specifically, we compare later outcomes of children who were between 9 months and 3 years old when their family was eligible for different amount of municipal specific supplements to HCA. We rely on standard DiD assumption of common pre-trends – that municipalities changing their supplements develop similarly to municipalities that do not make such changes.

To show the validity of the municipal supplements as identification strategy, we start with dynamic DiD graphs that are akin to event studies but for changes in continuous amount. These are based on regressions of leads and lags of a change in the municipal supplement policies, measured in 100 euros per month. The most common change occurs when a municipality takes up the supplement policy for the first time as the usual pattern is to take up the supplement and keep the policy constant at least for a few years; there are also large changes in the data such as removing the supplements or increasing the upper age threshold by a year. We study the effect of change in home care supplement that took place in municipality $m$ in year $t-k$ on mother and child outcomes at time $t$.[7] We let the change in supplement amount affect outcomes in the municipality three years prior to the change occurs, and three years afterwards. Thus, our dynamic difference-in-differences specification measures how a 100 euros change in supplement amount affects child outcomes in the years around the supplement change. The empirical specification takes the following form:

$$Y_{it} = \theta_t + \mu_m + \sum_{k=-3}^{3} \beta_k \Delta Supp_{mk} \; + \rho_a + \mathbf{X}_{it}\gamma + \varepsilon_{it}, \qquad (1)$$

where $\Delta Supp_{mk}$ are indicators for *the change* in the supplement amount $k$ years ago in 100 euros a family is eligible for in municipality $m$. $\mu_m$ is a municipality fixed-effect and $\theta_t$ is a (calendar) year fixed-effect. Our main interests lies in the coefficients $\beta_k$ that shows how outcomes evaluated around the time of supplement change. The specification also controls for child age dummies $\rho_a$ (in months at the end of the year) and mother characteristics from pre-birth year, such as mother's age, level of education, and number of children. Note that the municipalities that did not have at a change in their supplement policy are in the data as 0 change, and they allow us to control for common calendar year effects. We present the results from this estimation in graphs, where we scale the

[7] The results are not sensitive to using the number of children as control, and when fertility is examined explicitly as an outcome, we do not find a statistically significant effect

coefficients such that the effect in time -1 from the change in supplements is zero.

We also estimate the following (reduced form) standard DiD specification to quantify the effects and to show the estimates on leads and lags of outcomes.

$$Y_{i\tau} = \beta_\tau Supplement_{m,a,t} + \mathbf{X}_{it}\gamma + \alpha_m + \theta_t + \rho_a + \epsilon_{i\tau}, \tag{2}$$

where $Y_{ij\tau}$ is the outcome for parent or child $i$ measured in different periods $\tau = t+\delta$ (i.e. leads and lags of parent labor market outcomes, for children early cognitive development at age 4,5 or 6, school grade at 16, crime outcomes at 15-18). $Supplement_{m,a,t}$ is the amount of HCA supplement that a parent living in the municipality $m$, whose child was $a$ months old in the end of the calendar year, was eligible for in year $t$. We focus on the amount of supplement when the youngest (living) child was between 12 and 23 months at the end of year. $\mathbf{X}_{it}$ is the vector of pre-treatment covariates for mothers from period $t$. These include dummies fo mother's age (in years), level and field of education, and number of children. $\alpha_m$ is municipality fixed effects, $\theta_t$ are time fixed effects, and $\epsilon_{i\tau}$ is the residual error term. The specification includes also dummies for child's age at time of the subsidy (month dummies).The standard errors are clustered at the municipal level.

## 5.2 Impact on Maternal Outcomes

We begin our analysis by considering the effect of the HCA on maternal outcomes, in four ways. First we show the result of the Dynamic DiD exercise (equation 1) on maternal employment in Figure 5. The outcome variable is an indicator for maternal employment during the year when child is 1 year old. The specification follows municipalities over time, so that we can see whether the supplement increase is related to employment of mothers that are eligible for the home care allowance[8]. In the figure, the change in the supplement occurs at year zero, and there is no differential prior trend in employment of mothers having 1 year old child associated with supplements between those municipalities where the supplement change occurs and other municipalities. Thus, supplement increases do not appear to be responding to changes in underlying tastes for work among mothers. Maternal labor supply then falls by about 1.5% for each 100 euro increase in the homecare allowance and remains at that level. So supplements are clearly reducing maternal work in favor of at home care, and the effect corresponds to about 5 percentage reduction when compared with mean share of employment of mothers of one-year-old children.

Second, a natural question is whether this fall in maternal labor supply is sufficient to actually lower family income, or whether the income loss is less than the gain from the

[8]Sample includes all mothers of one year old children in Finnish municipalities in the years 1994-2015. The data excludes municipalities that used discretionary criteria for home care allowance supplements.

HCA supplement. As figure 6 shows, the latter is true; maternal labor income falls, but total income rises due to the HCA. The figure also shows that higher supplements lead to an increased use of HCA, which is in some sense first-stage effect of the policy. This outcome is opposite and consistent effect from reduced labor supply.

Third, an interesting question is whether the short run declines in maternal labor supply have long run impacts – e.g. that might lead family income to fall in the long run. Figure 7 shows the coefficients from separate DiD regressions of equation 2 on leads and lags of maternal employment and earnings. Note that this is different from the previous figure in that it follows mothers over time rather than provinces over time. Our previous analysis followed repeated cross-sections of mothers with one-year old children over time. This figure follows mothers from four years prior to birth of first child to 12 years after, and highlights the impact of municipal supplements the mother is eligible for when the child is one year old.

The results suggest important long run impacts of these shorter run home care allowances. Maternal employment and earnings in Figure 7 show a significant long run decline; the extra time at home due to the HCA appears to be "habit forming" in that it has an effect that persists long after it is expired.

Finally, our findings have an interesting relation to the literature on the "child penalty". Figure 8 shows in black the child penalty for Finland, which is measured by comparing earnings profiles of mother and fathers surrounding the birth of first child and normalized by counterfactual earnings that do not take the influence of child into account. The figure shows that the child penalty is quite high initially in Finland, dipping to around -70% in the year child is one year old, and only slowly increasing to a long-run level of about -20%. When compared to child penalties is Denmark or Sweden, the penalty is much higher initially in Finland, although the long run penalty is roughly similar (Kleven et al. 2019).

In fact, our results imply that the much larger short-run child penalty in Finland compared to other Nordic countries is almost completely due to the home care allowance. To illustrate this, we consider the child penalty shown in black and "add back" the implied effect of the home care allowance on longer run maternal supply from Figure 7. These estimates are expressed in terms of 100 euros of supplement per month, and the average HCA amount received is roughly 500 euros per month, so we multiply the estimates by five before adding them back. Doing so, we obtain the red line a "policy excluded" child penalty which is much lower. This red line is obviously a linear extrapolation, but illustrates clearly how our estimates are enough to bring up the child penalty in short run in Finland to Danish child penalty levels of about -20%. This result is interesting because Denmark does not have HCA system in place, but otherwise has quite similar system of child-related benefits and publicly subsidized child care.

### 5.3 Impact on Child Outcomes

We next turn to the impact of the HCA on children. Figure 9 presents the results for our key shot-term outcome, failing at least one of the standardized cognition tests conducted at child health clinics at age of five (after year 2010 the age when the comprehensive test is done is four) years old. The pre-period pattern is relatively flat, and there is a clear jump in the year when the supplement is changed. The figure thus indicates that it becomes more prevalent to fail the cognition test at age five or four for the children that were eligible for higher HCA in the form of supplements at age one.

Figure 9 also presents alternative measure for failing an early cognition test: collecting all tests we observe from health clinic visits during ages four through six years old. Due to last tests are observed for some municipalities in 2014, we need to restrict this outcome to birth cohorts of 2008. In any case, the effect is similar to our main outcome, and the point estimates are even slightly higher when using this outcome measure.

Figure 10 shows the dynamic DiD for two key long term outcomes, attending college between 18 to 23, and enrolling to academic high school (instead of vocational high school or dropping out completely) at ages 15 to 17 years old. These two outcomes are meaningful to study together, as going to academic high school is the typical track for going to college after that. The pre-trends are once again flat except small deviations three years prior to the change in supplements. After an increase in supplement when one year old, both enrolling to academic high school and enrolling to college at ages 18 to 23 declines. In this case the reaction grows over time, with the odds of going to college falling slightly in year 0 and significantly by year 2. There is some decline in the effect of enrolling to academic high school three years after the supplement change which we cannot fully explain but potentially due to consequent change in supplement policies. As we discuss below, we need to interpret the college enrolment result very cautiously as we do not observe all the cohorts in the analysis until the age of 23.

The results for another long-term outcome, being convicted of criminal offence or receiving a fine during ages 15 years through 20 years old, is shown in Figure 11. The pre-trend here is somewhat noisier than for other outcomes, but close to zero on average, and there is a clear upward shift at year 0; the noise likely reflects the much smaller incidence of this outcome, with a baseline youth crime rate of only 5%.

## 5.4 Quantifying the effect of HCA on child and mother outcomes

The dynamic DiD approach was useful to show that the validity of DiD assumptions are fulfilled in our institutional setting. We now turn to quantifying the effects by estimating the standard DiD specifications presented in equation (2).

Table 3 shows the result of estimating equation (2) on maternal outcomes; all coefficients show the effect of a 100 euros/month rise in the HCA. We first, in column (1), show the impact on HCA receipt: each 100 euros/month leas to 271 Euros/year in additional HCA receipt. This suggests a quite significant first-stage on average from the effect of municipal supplements, the amount of HCA used increases roughly 10% from the baseline. The continuous measure in euros utilized here captures both the extensive margin of whether or not mother uses any HCA as well as the intensive margin of for how long mother stays at home with the HCA. The intensive margin might be more relevant here as even in the absence of supplements around 80% of mothers utilize some HCA. But we do also observe an extensive margin response. Indeed, the odds of receiving HCA (not shown here) are 1.4 percentage points higher for each 100 euro supplement. The overall response suggests a sizeable elasticity of the decision to stay home with respect to the government rewards for doing so.

Column (2) shows that the impact of receiving supplement in 100 euros per month when the eligible child is one year old on employment of mothers is -1.27 percentage points, which is a roughly 5% decline in the odds of working. Column (3) shows that the impact on annual labor earnings is –194 Euros. Given the increase in HCA of 271 Euros in column (1), this suggests a more than two-thirds "crowdout" of the income benefits of HCA; that is, for every dollar of HCA received, mothers offset 72 cents in less labor income. Column (4) shows the effect of supplement on all income including earnings and taxable income transfers (including HCA and supplements). The effect on this outcome is 237 Euros. The figure does not exactly match column (1) as there are other social security that could be affected by staying at home instead of working, such as housing allowance.

Table 4 shows the results for early child outcomes. The table presents the impact of supplement in 100 euros per month on our main early outcome: failing at least one of the cognition tests in column (1), which is 1.78 percentage points and statistically significant. The effect size represents about 7% increase from the baseline failing rate shown in the bottom row of the table.

Column (2) presents failing any of the early tests we observe in child health care clinics during different visits to child health clinics during ages four through six years old, described in data section and Appendix, A.13. This is the alternative broad outcome we use in the analysis. Using this alternative measure, we observe an increase in the failing rate due to higher supplements very much in line with the cognitive test in the full sample.

Column (3) presents the impact on cross motor skills at child health clinics, and this estimate is close to zero and statistically insignificant. This estimate might be informative about mechanisms in the sense that motor skills test measures different kind of neurological development as the tests in our main early outcome. According to this result it appears that cross motor skills are not affected by supplements, unlike the cognitive test results.

Tables B.14 through B.16 report the effect of supplement when using single tests as the outcome, the tests from four throigh six year old tests are considered. These estimates show that we do not have enough observations in individual tests to estimate a statistically significant test with the exception of "Cut circle" or "Square" in the five year old tests. The tables also demonstrate that none of the estimates is negative and statistically significant.

Table 5 shows differences-in-differences results for child long-term outcomes. Column (1) shows that higher HCA in form of supplements when child is 1 year old leads to -.52 percentage point decline in that child enrolling in an academic high school, which is 1 percent of the mean. Column (2) shows a corresponding rise in vocational schools. The net result is a sizeable decline in college going of nearly 0.7 percentage points in column (3), which is about 1.7% of the mean, as academic high school is the main route to college in the Finnish education system. Again, the college result is not robust to limiting the analysis to cohorts for which we observe everyone until the age of 23, as shown in Table D.29.

Column (4) indicates that we find an increased effect on youth crime by age 18 years old in sentence records during the follow up period of .22 percentage points, off a mean of 4 percentage points, a roughly 6% effect which is comparable to the impact on test scores but larger than the impact on education. This variable gets value one if individual appears sentenced in court.[9]

Thus, Table 5 shows that for both educational attainment and rates of youth crime, there are significant negative long run effects of higher HCA supplements.

As highlighted earlier, a concern with the college outcomes is that the time span of our follow up period. In particular, we do only observe a balanced sample of those attending college through age 23 for those born in 1993-1996; as a result, our main result includes those younger than 23 as non-completers even if they will finish by age 23. In

[9]Note that the cohorts that we are focusing on long term analysis are different from the early childhood analysis, since we only have the educational outcomes at age 16 for the cohorts that were 1-year-old in years 1994-2004. The regressions in columns 3 and 4 use only cohorts that were one year old between 1994-2000, i.e. cohorts that were 19-25 in the latest observation year 2019.

Appendix Table D.29 and Figures D.18 through D.21, we assess the robustness to this restriction by restricting the data to those born in cohorts 1993-1997. In doing so, we find no effect on college enrollment. This may be due to limiting the variation or data used, but it certainly offers caution in drawing inferences from our college results. This is not a concern with the high school or crime outcome, as all cohorts through 2001 are observed through age 18.

It is of some interest to find out whether the effect of the policy varies across families, given that the earlier literature has found larger effects from child-related policies among children coming from less well-off families more disadvantaged families benefit more from universal programs Felfe and Lalive (2018); Cornelissen et al. (2018); Havnes and Mogstad (2015). Our institutional setting has merits in exploring the heterogeneous effect, because the HCA is so universally utilized with over 80% of mothers using it for at least for couple of months.

Tables C.17 through C.28 in Appendix, Section C explores the heterogeneity of our results by splitting the sample in two along two family characteristics: whether or not mother has college or higher as the highest education attainment, and by family earnings measured the year before child birth.

For mothers the split by college degree attained does not produce any differences. When splitting by pre-birth family income, the estimates on employment and earnings are in absolute value lower for low earnings group, but still negative and statistically significant. However, the baseline mean for low earnings families are also lower, non-surprisingly. Thus, for mothers we did not find great heterogeneity in the effect in an economic sense, all examined groups responded to the policy by increasing their HCA usage which led to an increase in disposable income, and by reducing their employment and labor earnings. Given results in some earlier studies the strong response by high-educated and high-earning mothers in somewhat intriguing. But as mentioned above, the HCA policy is widely utilized also by these groups, and the results suggest that mothers in these groups respond to the financial incentives to stay for longer periods at home taking care of their children.

For longer term child outcomes the results are slightly more mixed. The point estimates are larger in absolute value for academic high school enrolment for children having high-educated mother or high-earning family. But their baseline mean is also quite a bit higher. The point estimates on youth crime are quite similar, but the baseline for the better-well of group is lower. However, all estimates go to the same direction as the main estimates, but in some cases they are not statistically significant (such as college enrolment, which has also other robustness worries). In summary, we did not find any very clear heterogeneity in any of our heterogeneous results that would allow us to conclude that some group clearly does not respond to the policy in the same way as the other. Instead, all groups respond, and the effect of HCA is estimated as negative on all sub-groups (or not statistically significant).

## 6 Alternative specifications

In the above analysis we have compared how outcomes of children and their parents change around the time the municipal supplement to home care allowance changes. The institutional setting creates variation in incentives across municipalities at different points of time allowing us to control flexibly for general time trends common to all municipalities. However, the recent literature has identified also potential problems associated with this type of two-way fixed effects regressions, which we have thus far ignored (Goodman-Bacon (2021), Sun and Abraham (2021) and Callaway and Sant'Anna (2021)).

To investigate whether these concerns raised by the recent literature apply to our setting, we conduct several different robustness checks in this Section. The more careful analysis that provides evidence of robustness against these problems is provided in Appendix, Section E. First, a potential problem is that weights associated with treatment at different points of time might have different weights in two-way fixed-effects regression, and some weights could even be negative producing significant bias in the estimation. To investigate the treatment-weights in our two-way fixed effects analysis, we plot the distribution of weights that the main estimations of the effect of supplements on different outcomes actually utilize in Figures E.22 through Figure E.24. These histograms show that the weights are not completely homogenous, but that there is a large spike at small positive weight; there are indeed some negative weights, but these constitute less than one per cent of the weights.

We next provide evidence from staggered DiD design, which addresses the worry of subsequent changes in supplements. We begin by simplifying our current set up, and focus on one event (one change in the supplement) at a time for each municipality, and use as a comparison group municipalities that did not change their supplement to home care allowance during the entire study period. The supplement variation is continuous as in our baseline estimates: the treatment variables, as in specification (1), describes the change in the municipality specific supplement in 100 euros. The method is explained in the Appendix, Section E.

The results are presented in Figures E.25 through Figure E.33. We confirm our previous findings of clear negative consequences on mother's labor market outcomes and on early childhood cognition. For longer term child outcomes the effects are robust with respect to switching to vocational high school and for crime, but less so for college enrolment, once again reflecting the limitations of our data for this outcome.

The third robustness analysis we conduct is to implement the Callaway and Sant'Anna (2021) estimator, which accounts for both the treatment-weights and consequent changes in supplements problems. This estimator relies on distinct events, so to translate to our context we use as the key event the first occurrence of at least 40 euros per month increase in supplements, which mostly means the year when the supplement was first implemented in the municipality. The explanatory variable in this estimation is event indicator marking the first occurence of at least 40 euros increase in supplement rather than the continuous variation used thus far in the analysis. As our main analysis uses a richer variation in the supplements, this specification is more restrictive. The estimation procedure compares all municipalities that had an increase in supplements in certain year to municipalities that are never treated over the same years, that is, it creates for each increase in supplements a differences-in-differences analysis with one treatment and one control group. The estimation procedure also treats treatment weights such that the heterogeneous weights are controlled for.

We present results from the above analysis in Tables E.32 through E.35 in the Appendix, which shows that we get close to zero before-treatment effects for our outcomes, and post-treatment effects that are generally consistent with our main findings. The effect on HCA usage and employment are larger in absolute value than the main estimates, while the non-college results are similar; once again, the college enrolments results are the least robust. The event-study coefficients show that whenever we observe significant estimates, the effects seem quite stable after the treatment.

In summary, we observe similar results for children from this exercise as our main estimation results, but that some of the outcomes are not quite as statistically significant as in the main analysis. However, this could also be due to restrictions needed to impose in order to perform this analysis, and not necessarily evidence of two-way fixed effects problems.

# 7 Confirming Mechanism: The Day Care Fee Reform in 1997

Our results thus far suggest that incentivizing mothers to stay home led to mothers staying out of labor force for longer, with negative impacts on child outcomes. Since we find that, at least in the near term, family income went up (which should have improved child outcomes), our findings suggest that the mechanism of action is the change in maternal labor supply and associated child versus home care decisions.

As previewed in the literature review section, this paper is one of many that shows that a particular intervention had positive or negative effects on mothers and children. But our setting is unique in that we have a means of confirming the mechanism of action through another equal and opposite government intervention. In particular, we now turn to analyzing a reform in 1997 which lowered the fees for day care in Finland. Lower day care fees incentivize children to enter day care sooner, but do not affect the disposable income in families where children stay in home care.

Municipal day care centers in Finland are publicly subsidized and organized by the municipalities, but there are fees imposed on enrolled families. Prior to 1997, these fees varied around the nation, but a reform in 1997 unified the fees. Both before and after reform, day care fees relied on the same general structure, a step-wise system that depended on the family size and family income – but before the reform, the income and family size thresholds determining the size of the fee varied across municipalities. In the reform the system was unified across municipalities and also made somewhat simpler, but still depended on the same variables, family size and family income. Due to the pre-1997 municipal variation, the reform created exogenous variation that did not depend on the actions of the family. The variation is potentially quite rich as there were more than 400 municipalities in Finland at the time out of which we have day care fee schedules for 350 municipalities.

Of course, this variation depended on family income, which is endogenous. We solve the endogeneity problem by evaluating the change in child care costs for each family based on their predicted income – where predictions are based solely on exogenous characteristics. In particular, we regress family income against family size, age of the mother with three-year bin fixed effects, indicators for the age of the youngest child in months, level of education of mother, whether mother's native language is Finnish, Swedish or some other, whether or not mother has a spouse, the education level and age indicators of a possible spouse and tan indicator of he size group of the municipality of residence of mother (five groups). We predict from this regression the family income each mother would have based on the characteristics mentioned above. We then apply the same predicted income to different fee structures before and after the reform, thus removing the influence of any actual change in family income due to the reform. We obtain from this procedure the day care fee each family needs to pay before and after the reform based on the same family income, family composition and municipality or residence. We take the difference of these two fees and use it as the main explanatory variable.

Figure 12 shows the variation we have in the difference between the predicted day care fees before and after the reform. We observe that there is quite a bit of variation, with considerably mass in the range of +/- 100 euros per month per child in day care and some mass extending to +/- 200 euros per month per child. The variation shown here is comparable to the variation in municipal supplements used in the analysis above.

We analyze the impact of the above defined change in day care fees in regression framework on outcomes of mothers and children. The explanatory variable of interest is the change in day care fees interacted with event-time indicators in the event-study specification or before/after indicator in the DiD regressions. The change in day care fees is specified in 100 euros per month to be comparable with the monetary variation in supplements. We include in the same specification the dynamic DiD leads and lags for municipal supplements as some of the changes in supplements took place during the years we follow in the day care fee reform. As supplements affect the same behavioral margins, it is important to take their contribution into account. Equation 3 presents what we estimate.

$$Y_{it} = \theta_t + \mu_m + \sum_{d=-3}^{3} \beta_d \Delta DCF_i \times \text{eventtime}_d + \sum_{k=-3}^{3} \beta_k \Delta Supp_{mk} \; + \rho_a + \mathbf{X}_{it}\gamma + \varepsilon_{it}, \quad (3)$$

where $\Delta DCF_i \times \text{eventtime}_d$ are indicators for the change in day care fees each individual is facing due to the reform in 100 euros per month interacted with event-time indicators $d$ years from the DCF reform in 1997. We omit year 1996 from the specification. $\mu_m$ is a municipality fixed-effect and $\theta_t$ is a (calendar) year fixed-effect. Our main interests lies in the coefficients $\beta_d$ that shows how outcomes evaluated around the time of supplement change. The specification includes $\Delta Supp_{mk}$ that are the dynamic DiD variables for municipal supplements. The specification also controls for child age dummies $\rho_a$ (in months at the end of the year) and mother characteristics from pre-birth year, such as mother's age, level of education, and number of children.

Figure 13 shows the event study resulting from estimating equation 3 for maternal employment and Figure 14 the effect on HCA usage. Both figures show two lines, one corresponding to the change in day care fees, that is $\beta_d$ , and another corresponding to the change in supplements, that is $\beta_k$. There seems to be a flat pre-trend and an increase in maternal employment in the group facing reduced day care fees directly when the reform took place. The higher employment level stays in place for this group in the subsequent years relative to those for which the day care fees increased. There is a slight pre-trend on HCA usage, but also a clear drop after the reduction in day care fees. The HCA usage is not a directly related to day care participation, but is negatively associated with employment, thus this outcome serves as confirming the employment result. Therefore, this quasi-experiment provides a nice complement to the home care allowances which had an opposite effect on employment. Moreover, the supplements in the same specification provide a similar effect as before, which the figures show to be opposite from the impact of lower day care fees, which conforms with our hypothesis.

Table 6 shows the DiD results for maternal employment utilizing the same continuous change in day care fees as the event study specification above with $\Delta$ DCF the change in day care fees interacted with a dummy for after the reform in 1997. This specification also controls for municipal supplements. Column (1) presents the impact on maternal employment, with a clear increase of 1.6 percentage points from the DCF, while column (2) shows that the DCF increase earnings by about 345 euros; both estimates are comparable and opposite signed to what we saw from a 100 euro HCA supplement in Table 3.

There is a significant decline in the amount of home care allowance received, as opposed to the increase in Table 3; this is consistent with mothers working more and therefore getting less HCA. Overall, however, income of the mother goes up with reducing day care fees, as the rise in labor income is larger than the decline in HCA. Therefore, the DCF quasi-experiment allows us to ask: how did an alternative intervention that increased income by the same amount, but which had the opposite effect on mother's labor force participation, impact children's outcomes?

For short term outcomes, we focus here on our alternative measure of whether the child fails any test; as we have quite limited number of observations in the early test data from years surrounding the DCF reform, and therefore do not have enough statistical power to concentrate only on cognitive test score from one particular test. We have reported the comparable result for supplements in Table 4, column (3). Figure 15 shows the event study for failing any test at child health clinic as the early child outcome. Table 7 shows the DiD regression results for the DCF reform. We see that reduced day care fees lead to lower failing rate in these tests precisely at the time of the reform. Moreover, the supplement effect is the opposite estimated from dynamic DiD included in the same specification and utilizing the same year-range surrounding the 1997 DCF reform.

Figure 16 shows the event study for enrolling to academic high school (as opposed to vocational education or dropping out entirely from secondary education), and again find a similar pattern where the effect of reducing day care fees in the 1997 reform have an opposite effect from supplements, although the dynamic pattern in the figure is not very clean.

Table 8 shows the DiD results for long term impacts on children. Here we find the opposite pattern of the HCA effects on educational outcomes. Column (1) shows that the effect on high school enrolment is positive at .55 percentage points and column (2) shows that the effect on vocational secondary education enrolment is negative at .58 percentage points. Both of these estimates just fall short of significance at 95% level. The effects are opposite and of similar magnitude in absolute value than the effects of supplement variation in the same units, in 100 euros per month. Column (3) and (4) show that we do not find a statistically significant estimate on college attendance or youth crime, although the effects are in the expected direction and are the opposite of the supplement results.

# 8 Conclusion

This paper investigates the effect of generous child home care allowance policy on child and maternal outcomes. The Finnish home care allowance allows parents to remain home with their children on paid leave until child is three years old. As a result, Finnish children are less likely to attend formal day care when they are under three years old than in other Nordic countries where the paid parental leave ends when the child turns either 12 or 18months (e.g. 27% in formal day in Finland with 54% in Norway, 47% in Sweden, and 65% in Denmark). We exploit the variation in municipality specific supplements to home care allowance to show that: mothers reduce their labor supply significantly in response to supplements, with lost labor earnings offsetting almost three quarters of the incremental benefits; this labor supply reduction persists in the long run, and can explain the much larger near term child penalty in Finland; and child outcomes are worsened in both the short (cognitive testing) and long (schooling and crime) terms.

We then go beyond most previous studies to confirm our findings through an equal and opposite government intervention that subsidized formal child care. We find that these subsidies had comparable impacts on maternal income, but operated through raising maternal labor supply. And we confirm our conclusions by showing that this policy significantly improved child outcomes.

Taken together, this set of findings clearly demonstrates that promoting home care in Finland was harmful to children. This finding confirms some and contradicts other findings in the literature on government policy towards maternal labor supply. This large and mixed literature suggests that there may be few general international lessons from such policy analyses, and that conclusions are best drawn on a country-by-country basis. Our paper can set a template for such analyses by showing the value of a rich analysis of a suite of government policies to draw consistent conclusions about impacts on mothers and children.

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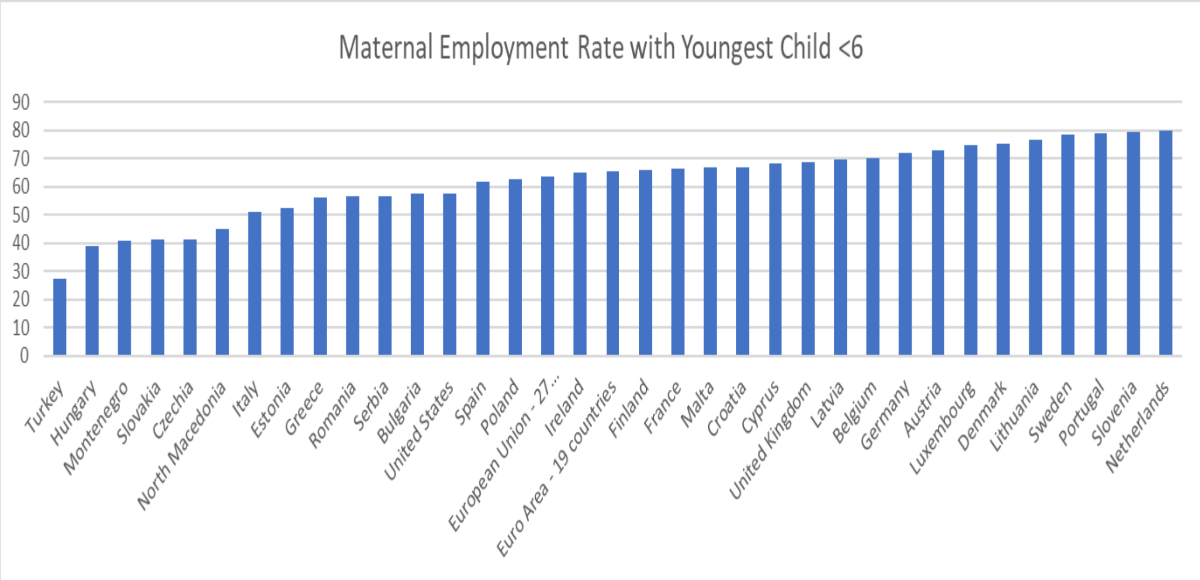
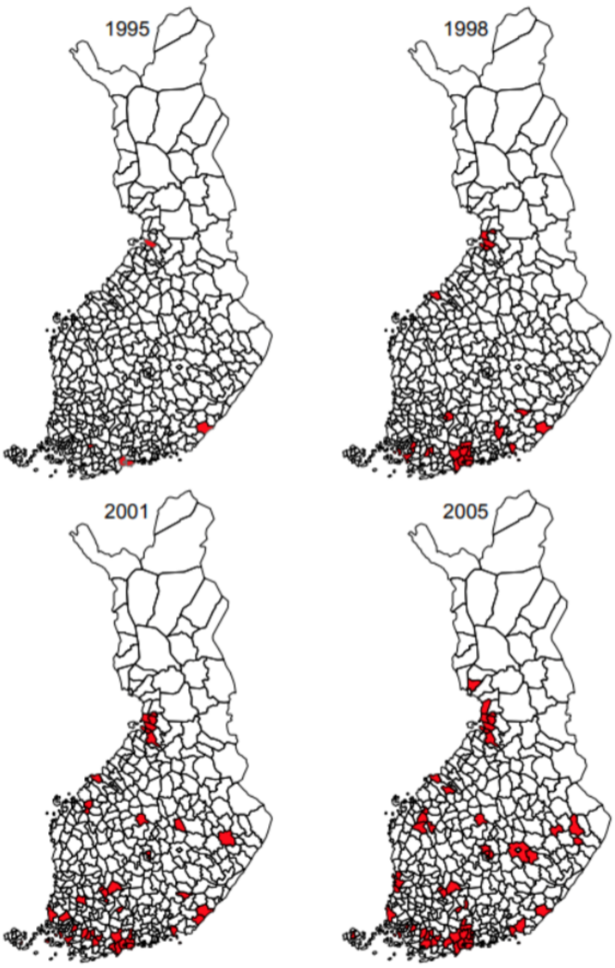
# 9 Figures

Figure 1: Maternal employment with youngest child below 6 years of age across countries



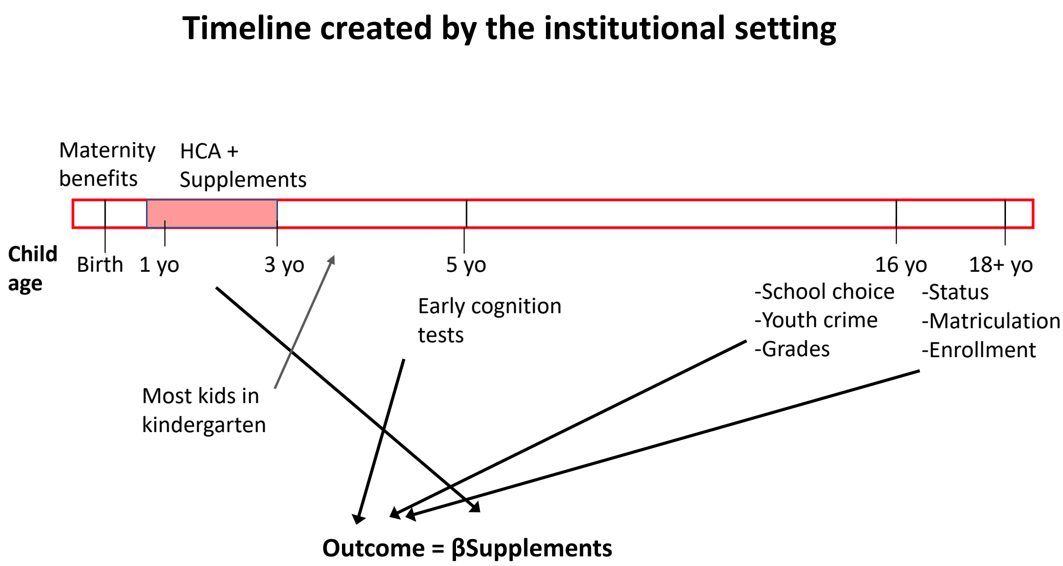

*Note*: The graph shows a maternal employment rates with youngest child below age of six across countries. Source: Eurostat, data for 2020.

Figure 2: Municipalities having supplements in different years on map



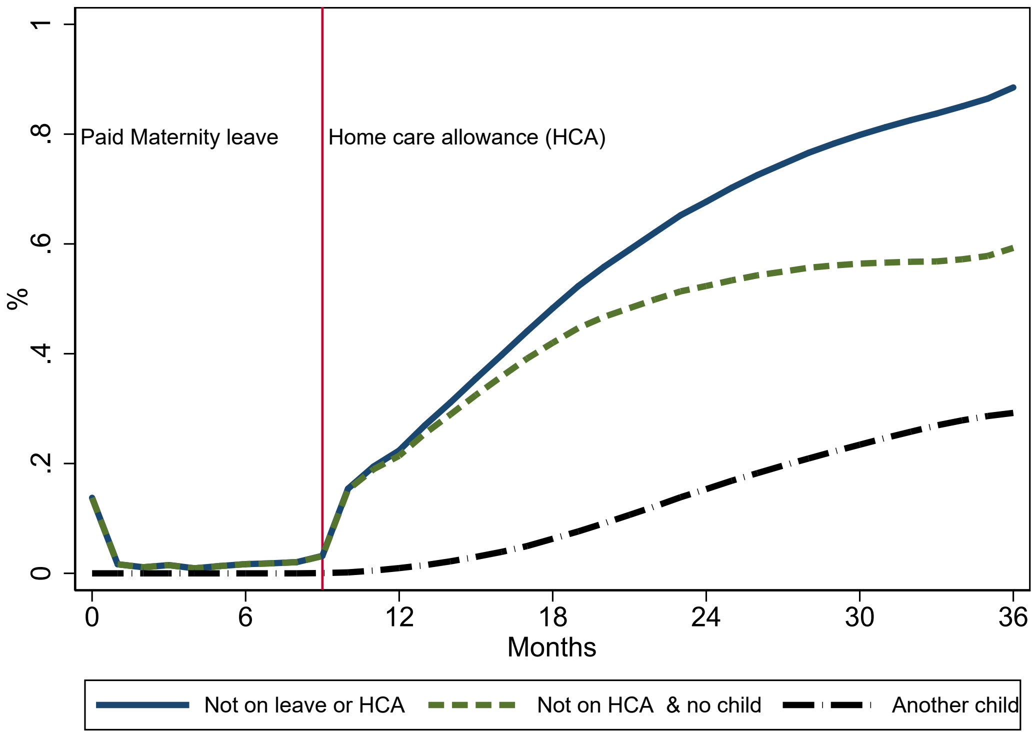

*Note*: Maps of Finland showing in red municipalities that provide supplement to the national HCA, different maps for years 1995, 1998, 2001 and 2005, respectively.

Figure 3: Illustrating the timeline in our setting



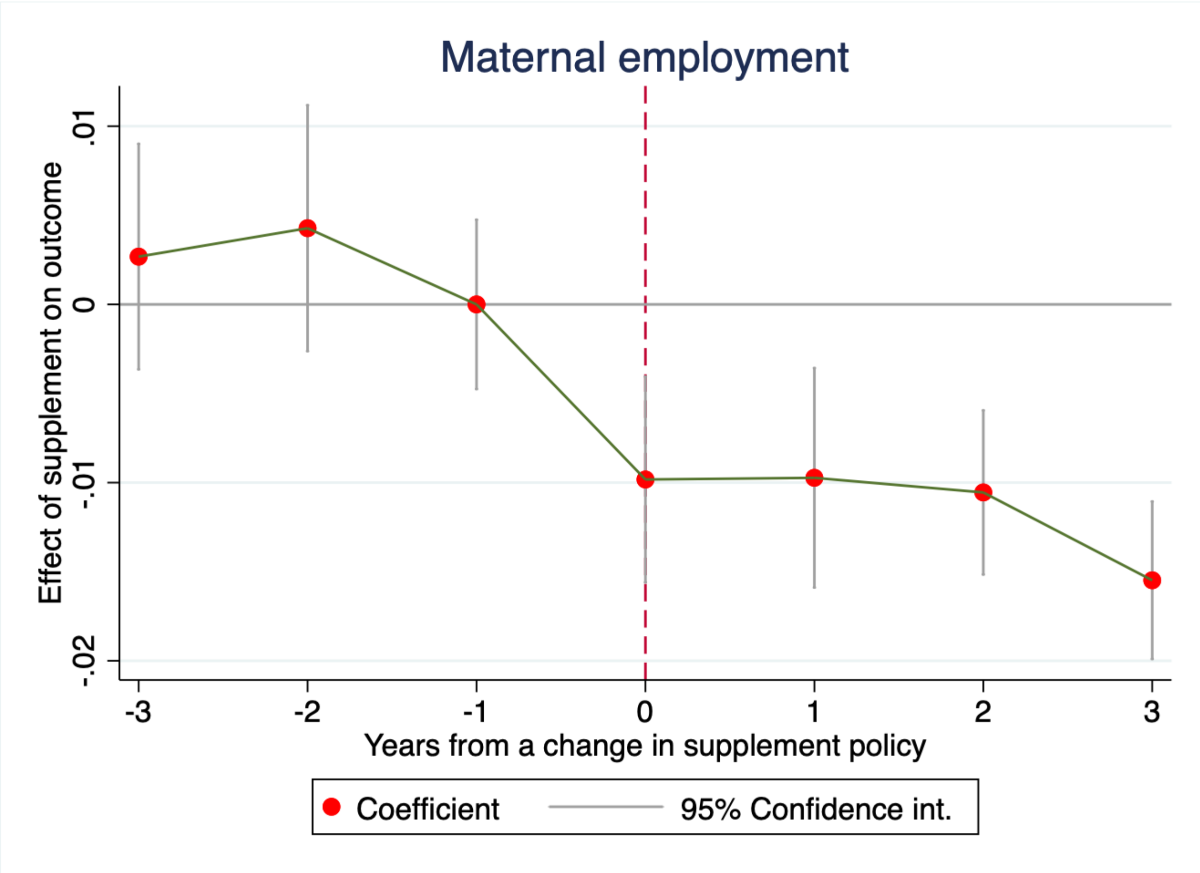

*Note*: The graph illustrates the timeline in our setting to estimate the effects of HCA (or DCF) on parental and child outcomes. The treatment occurs when the child is one year old and some outcomes for parents and all outcomes for children are observed some years later.

Figure 4: The rate of exiting HCA to work or having next child by child's age



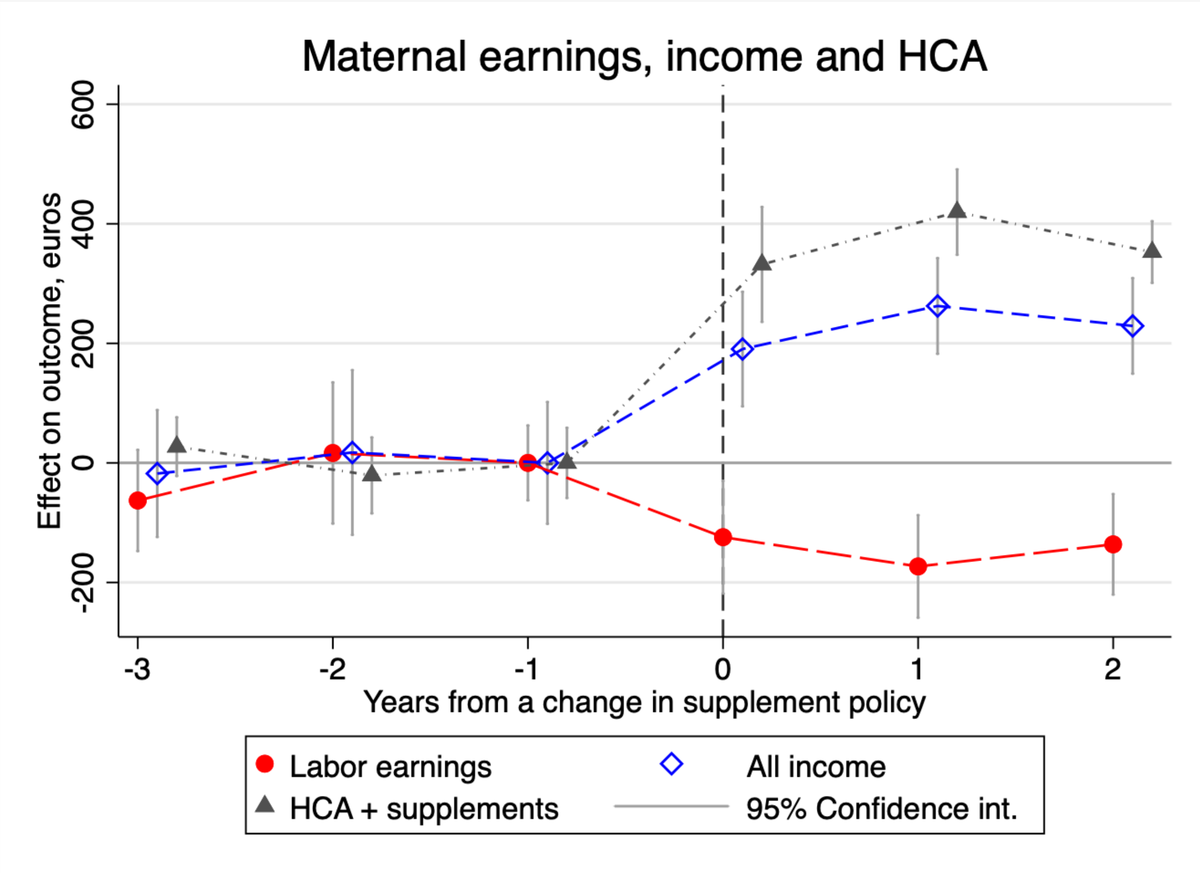

*Note*: Data from SII and own calculations. The graph describes by the age of children in months the fraction of parents not using parental leave or having another child. Population data for mothers who had a child in 2015.

Figure 5: Dynamic DiD: Mother employed



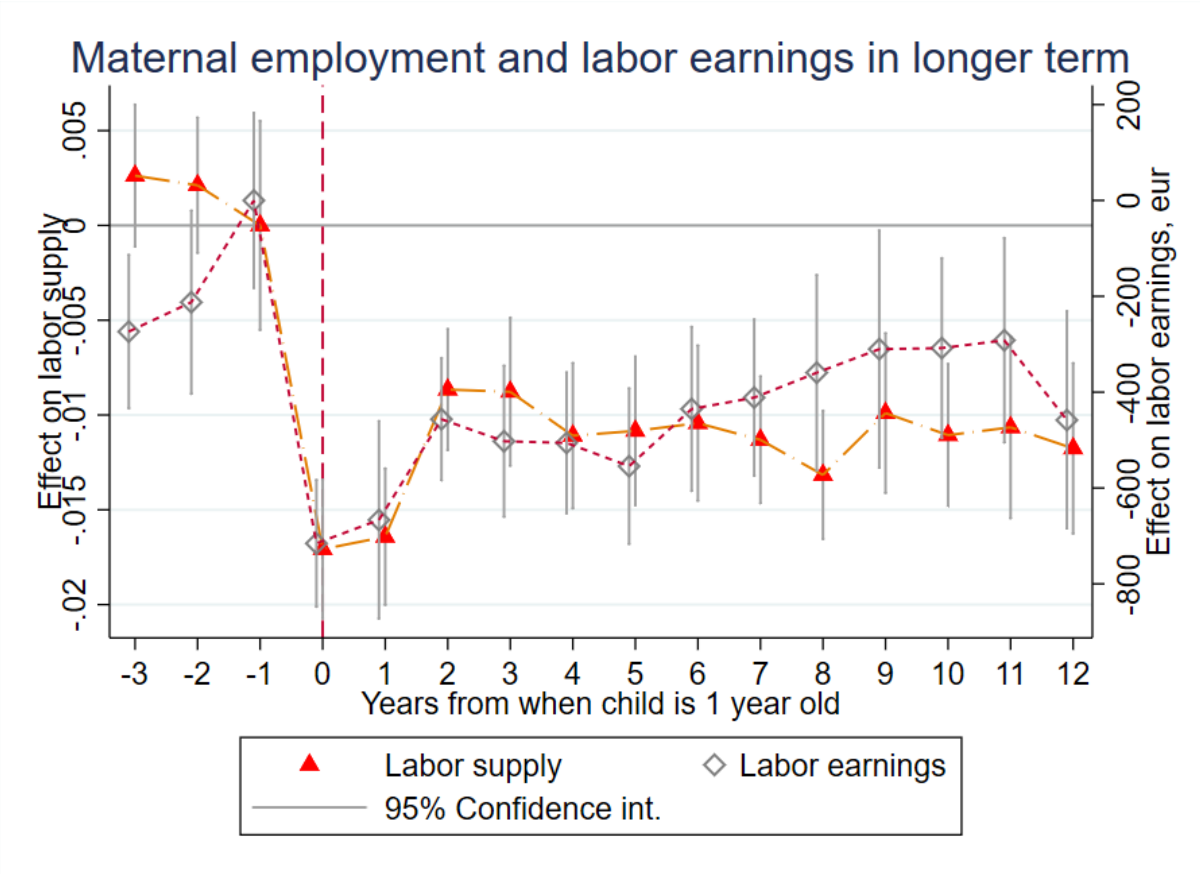

*Note*: The graph plots coefficients and confidence intervals that are estimates of leads and lags in years from changes in the supplement amount in 100 euros in year 0 as shown in equation (1), which are normalized to year -1 level by subtracting year -1 estimate from all other estimates. The specification controls for common year effects and municipality fixed effects. The outcome is an indicator for a mother being employed in that year, which is defined as having more annual earnings than the median earnings of all women who have children below the age of 6 years old.

Figure 6: Dynamic DiD: Mother's earnings, income and HCA usage



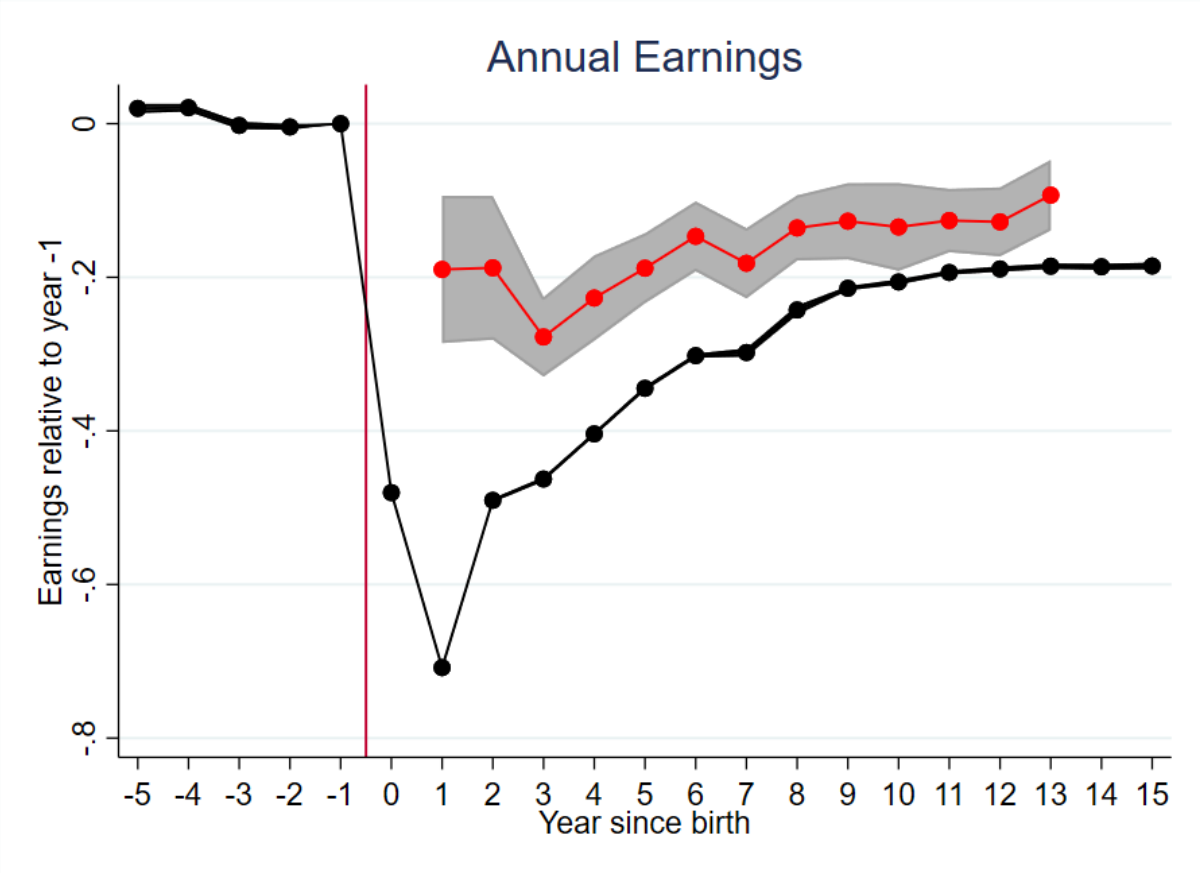

*Note*: The graph plots coefficients and confidence intervals that are estimates of leads and lags in years from changes in the supplement amount in 100 euros in year 0 as shown in equation (1), which are normalized to year -1 level by subtracting year -1 estimate from all other estimates. The specification controls for common year effects and municipality fixed effects. The outcomes are in euros (annual) for labor earnings, income including benefits and HCA used including supplements.

Figure 7: Longer term effects of supplements on mothers: labor supply and earnings



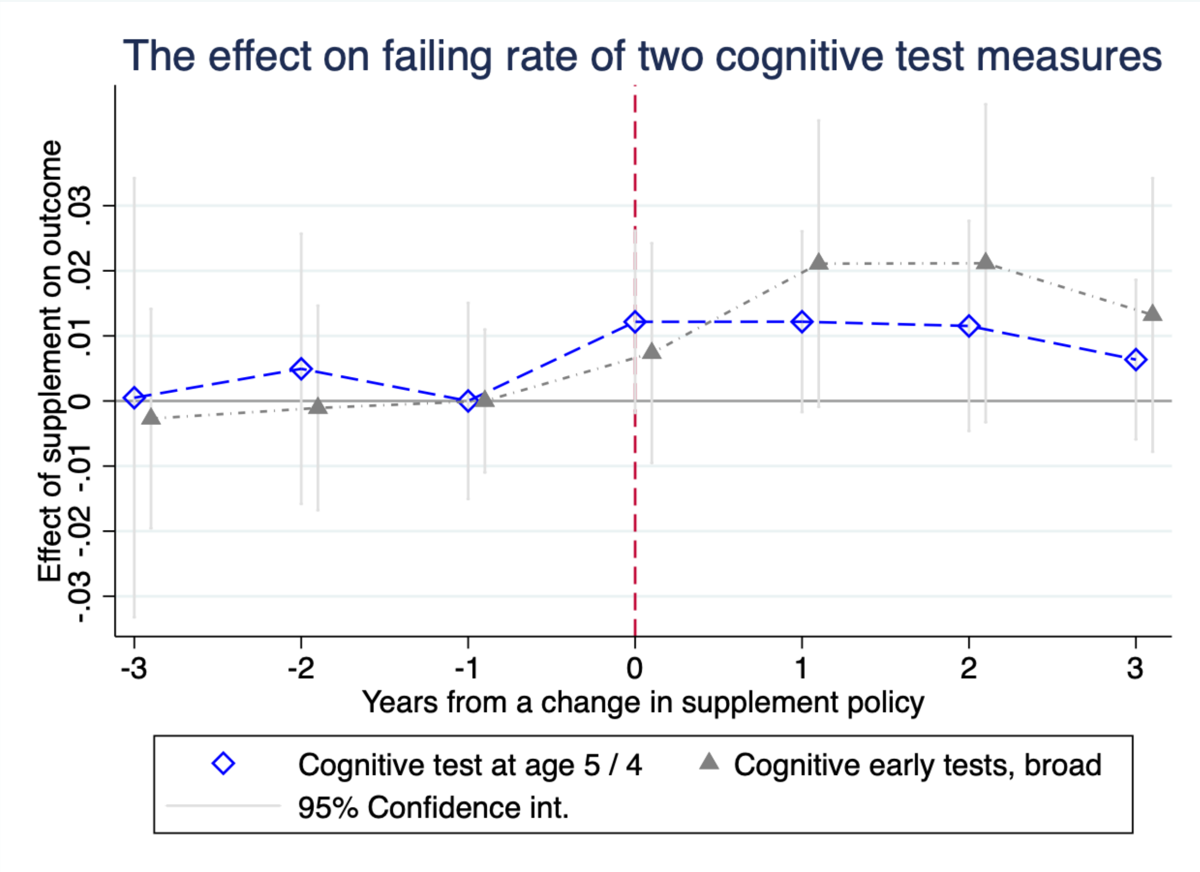

*Note*: The graph plots coefficients and confidence intervals that are estimates of separate DiD estimations where outcomes are maternal employment measured $k$ years before or after the first child was born.

Figure 8: Child penalty for Finland in labor earnings and the influence of supplements



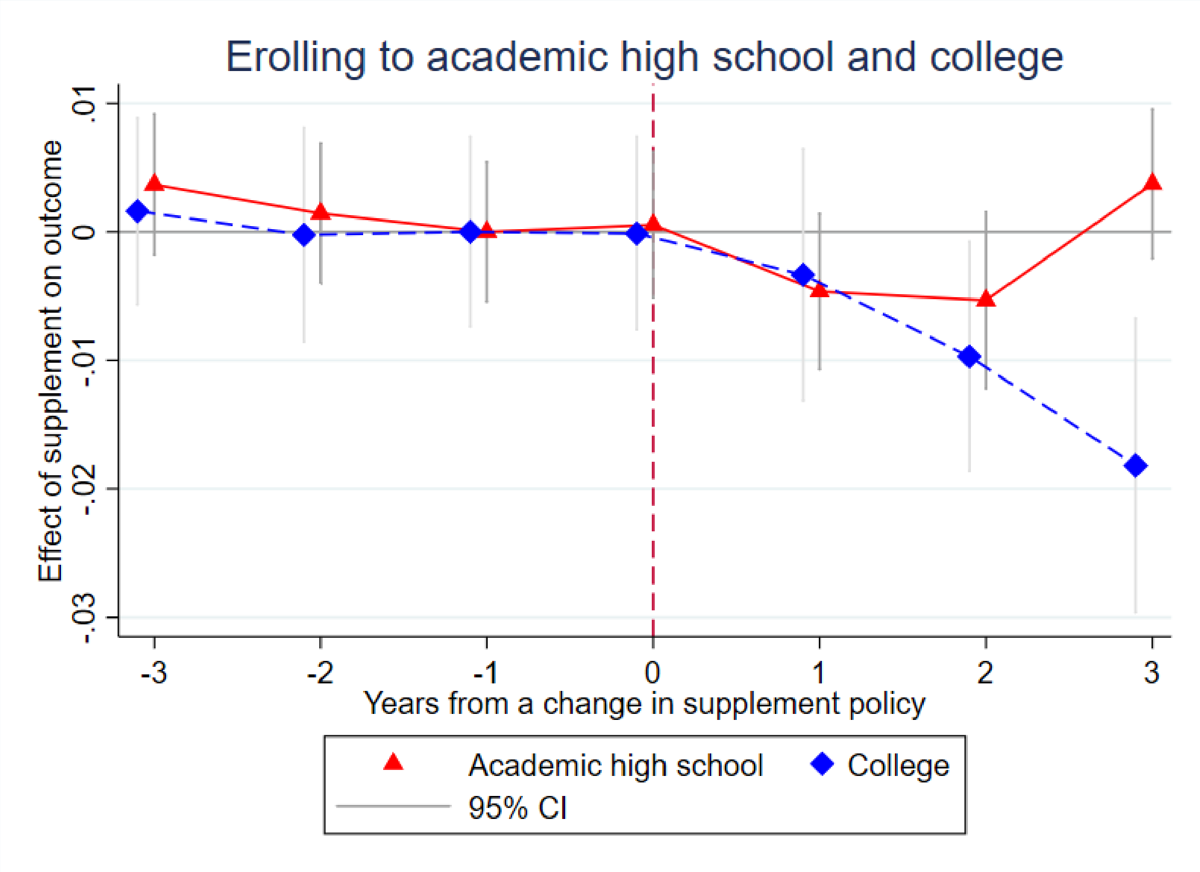

*Note*: The graph plots child penalty in our estimation sample, which is difference between labor earnings of mothers and fathers relative to a counterfactual earnings prediction. The graph also subtracts from this the longer term DiD estimates utilizing supplements in 100 euros per month. The graph includes an extrapolation where previous estimates are multiplied by five, because average total HCA received is about 500 euros per month.

Figure 9: Dynamic DiD: Failing cognition tests at ages 5/4 and failing any test at ages 4 through 6



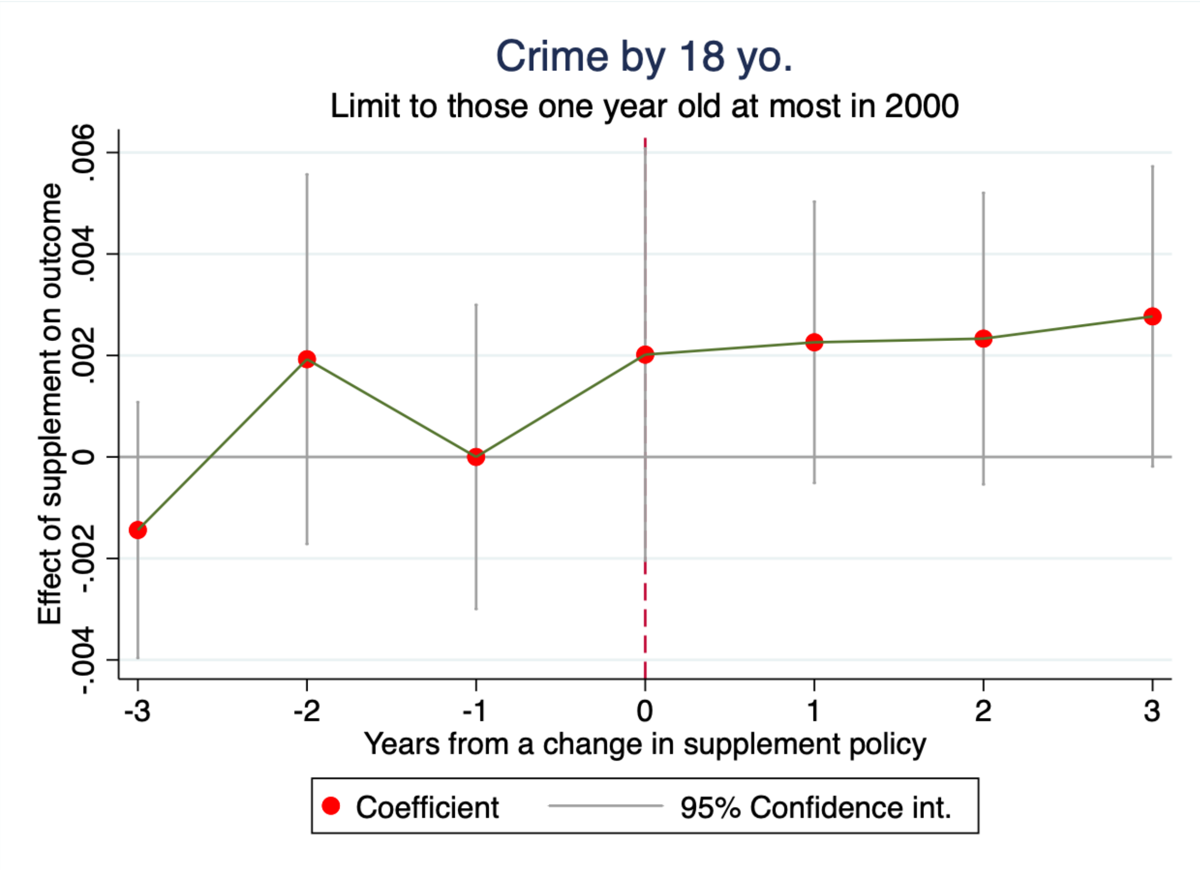

*Note*: The graph plots coefficients and confidence intervals that are estimates of leads and lags in years from changes in the supplement amount in 100 euros in year 0 as shown in equation (1), which are normalized to year -1 level by subtracting year -1 estimate from all other estimates. The specification controls for common year effects and municipality fixed effects. The outcomes are indicators for failing different early tests, first one is the main outcomes used in analysis, second is an indicator for failing at least one of the tests interpreted as measuring cognitive skills that are done for children aged 4 through 6 years old, but only observed until cohort born in 2009 for checking robustness of potentially changing test conditions over time.

Figure 10: Dynamic DiD: Enrolling to academic high school and college



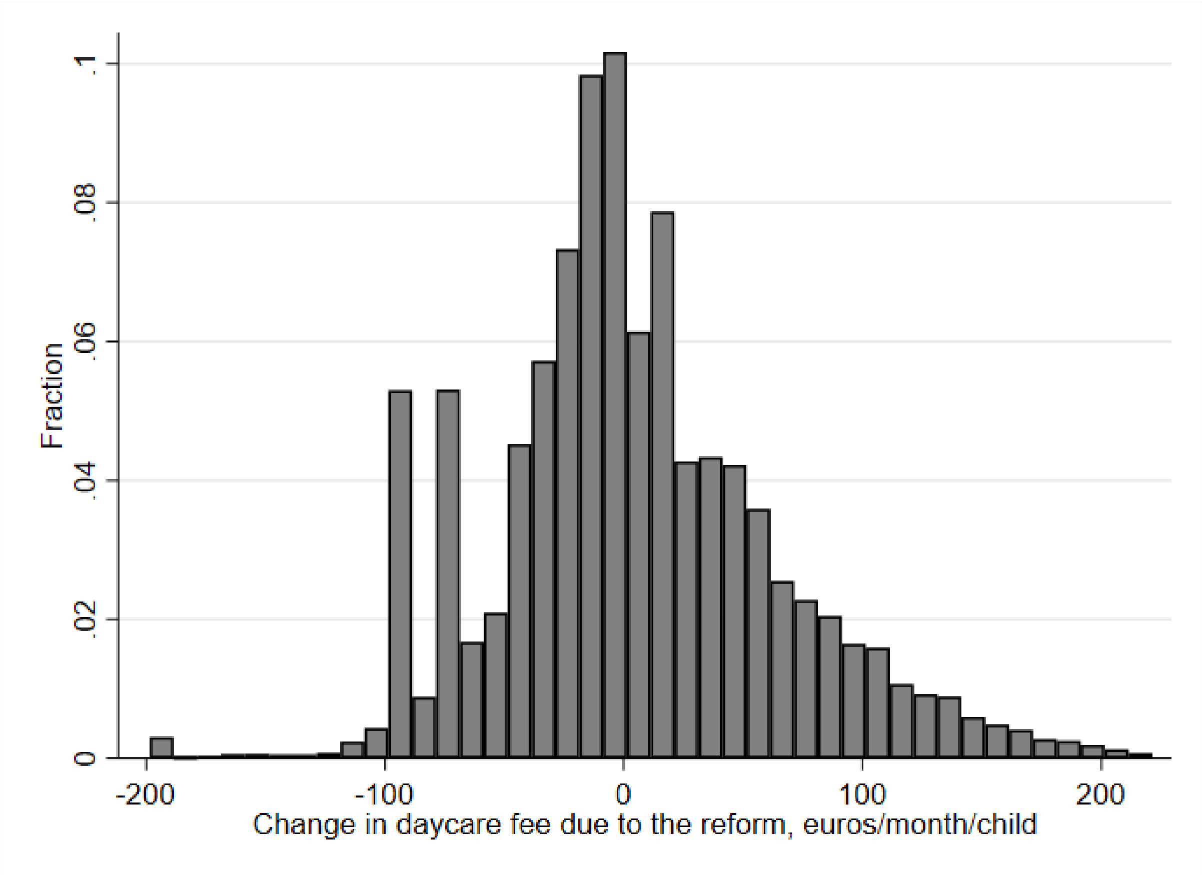

*Note*: The graph plots coefficients and confidence intervals that are estimates of leads and lags in years from changes in the supplement amount in 100 euros in year 0 as shown in equation (1), which are normalized to year -1 level by subtracting year -1 estimate from all other estimates. The specification controls for common year effects and municipality fixed effects. The first of two outcomes is an indicator for enrolling to academic high school ( at ages 15 to 17 years old) rather than choosing vocational secondary education or not observed enrolling to secondary education. The second outcome is enrolling to college, measured at ages from 18 to 23 years old.

Figure 11: Dynamic DiD: Committing a crime, ages from15 to 18 years old



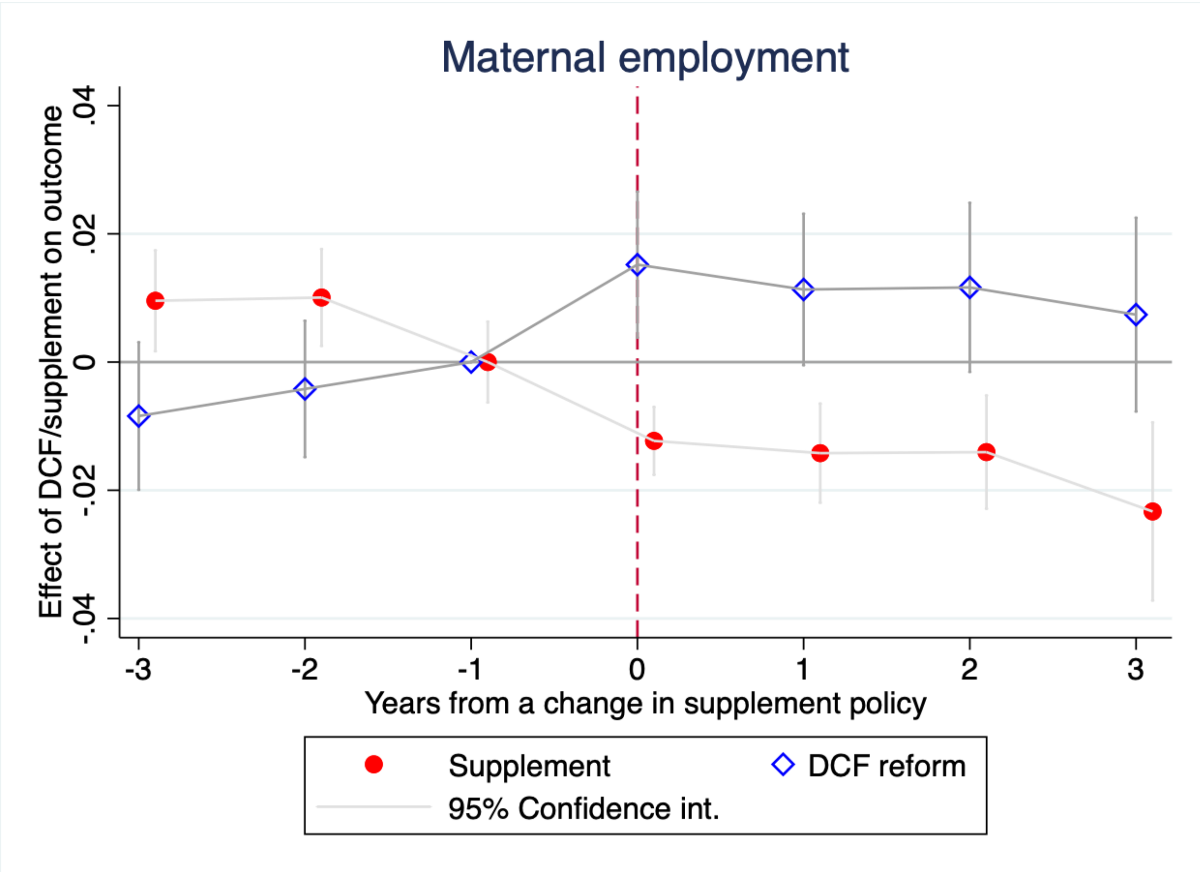

*Note*: The graph plots coefficients and confidence intervals that are estimates of leads and lags in years from changes in the supplement amount in 100 euros in year 0 as shown in equation (1), which are normalized to year -1 level by subtracting year -1 estimate from all other estimates. The specification controls for common year effects and municipality fixed effects. The outcome is an indicator for having been sentenced from committing a crime between the ages 15 through 18 years old.

Figure 12: Distribution of change in day care fees due to the reform



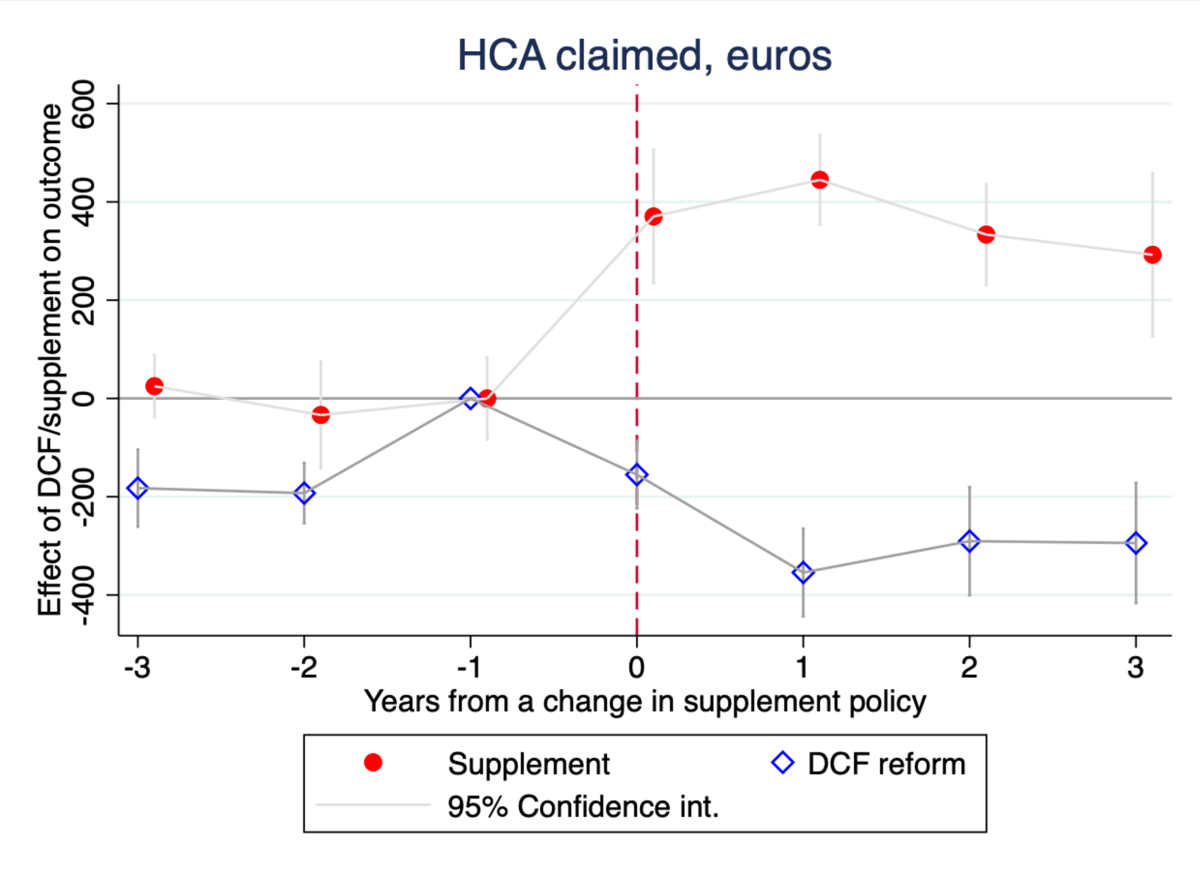

*Note*: The graph shows a histogram of the predicted change in day care fees due to the 1997 reform. The amount of change depends solely on how the fee schedule changed in the municipality, but varies across different households within municipalities because the fees depend differently on family characteristics before and after the reform.

Figure 13: Event study: The impact of day care fees on maternal employment



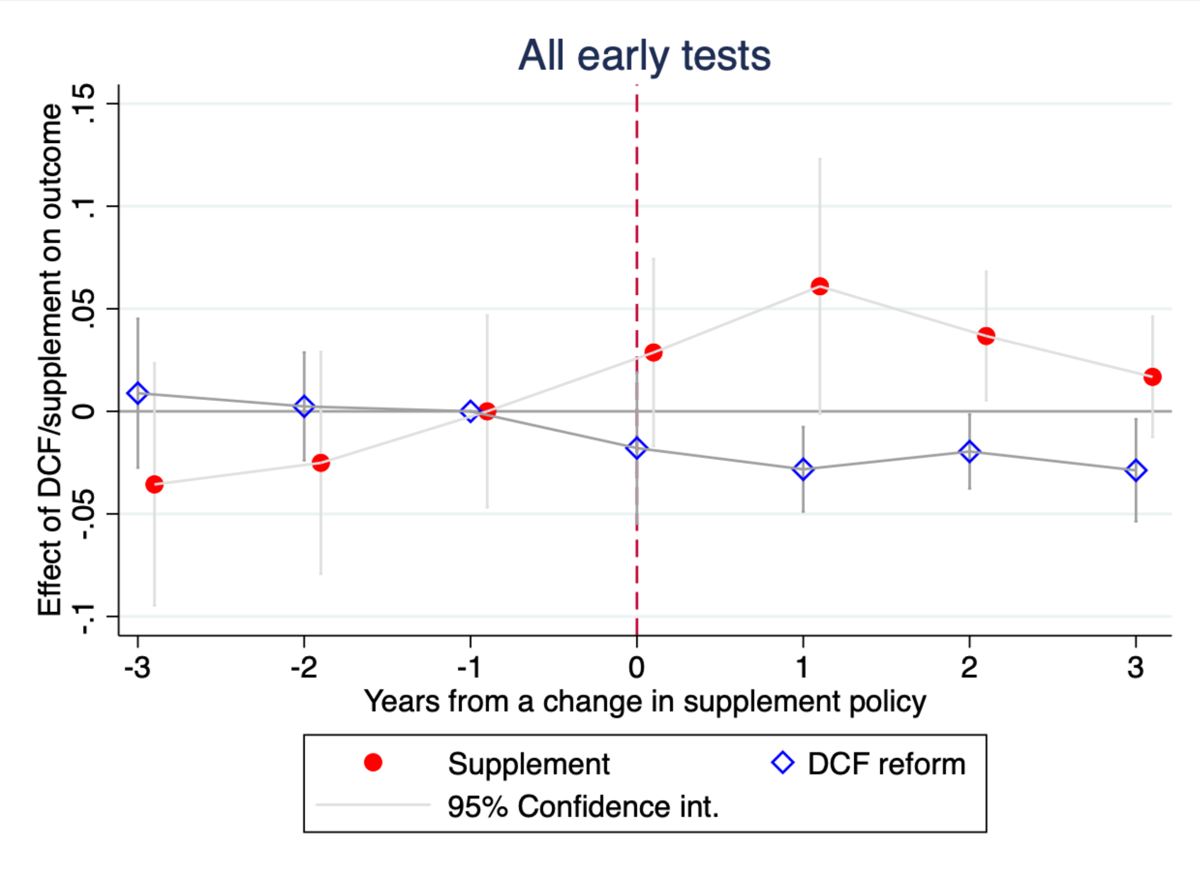

*Note*: The figure plots the coefficients on events of day care fee reform that reduced day care fees in 1997. Controls are reported under table 3.

Figure 14: Event study: The impact of day care fees on HCA usage



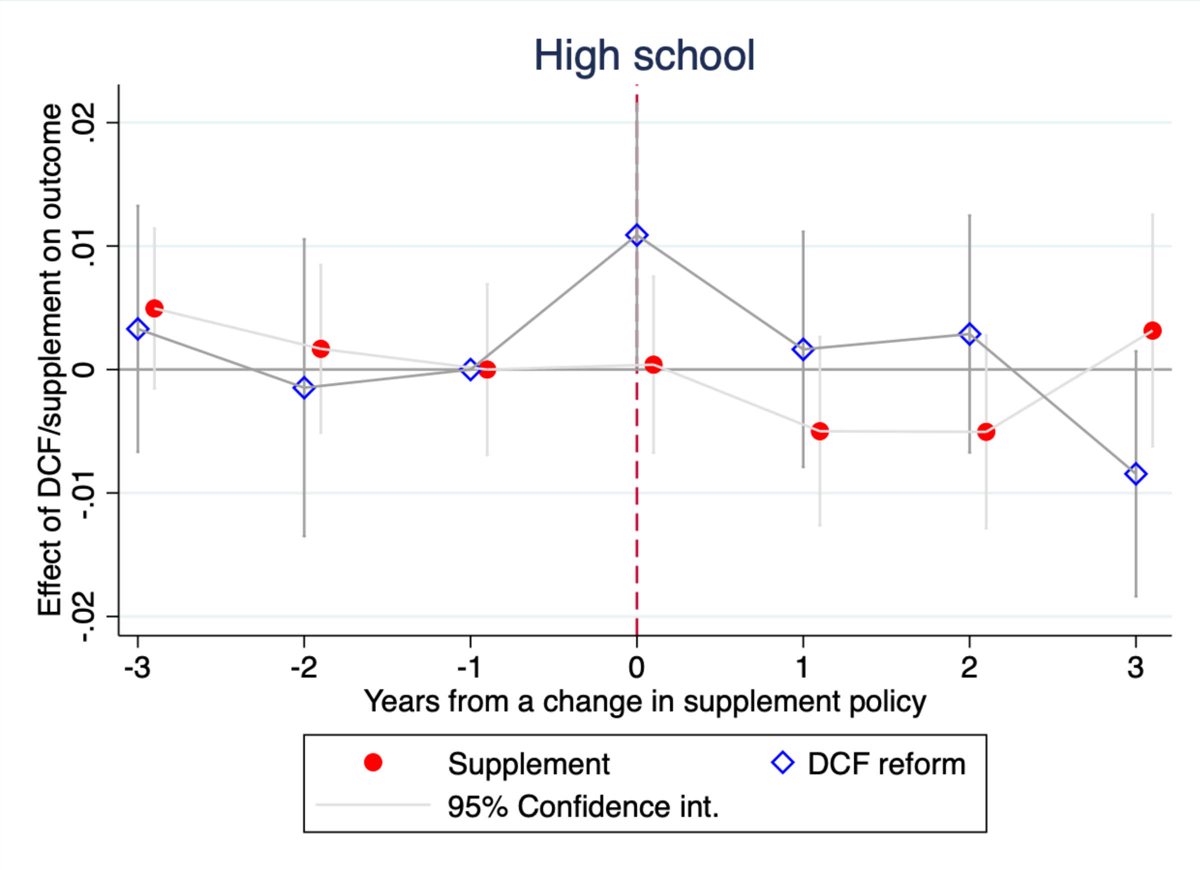

*Note*: The figure plots the coefficients on events of day care fee reform that reduced day care fees in 1997. Controls are reported under table 3.

Figure 15: Event study: The impact of day care fees on failing any early child test




*Note*: The figure plots the coefficients on events of day care fee reform that reduced day care fees in 1997. Controls are reported under table 3.

Figure 16: Event study: The impact of day care fees on enrolling to academic high school




*Note*: The figure plots the coefficients on events of day care fee reform that reduced day care fees in 1997. Controls are reported under table 3.

# 10 Tables

Table 1: Supplement amounts and rules in municipality and year data

| VARs | (1) Supplement | (2) Age thresh. | (3) Sibling suppl. | (4) Income dep. | (5) Prior work cond. | (6) Older child at home |
|---|---|---|---|---|---|---|
| Unit | eur/kk | year | eur/kk | 0/1 | 0/1 | 0/1 |
| Mean | 200 | 2.1 | 82 | 0.02 | 0.09 | 0.18 |
| SD | 47 | 0.58 | 23 | 0.14 | 0.28 | 0.39 |
| N (Mun and Year) | 1056 | 1056 | 395 | 86 | 441 | 598 |

*Note*: Descriptives of the supplement policies in municipal-year and population weighted data. Supplement is the amount of main supplement part in euros per month. Age thresh is the upper age threshold for the supplement in years. Sibling suppl. is the sibling supplement in euros per month. Income dep. is an indicator for whether the supplement is in any how conditioned on family income, Prior work cond. is an indicator for whether one has need to be in work prior to child birth in order to be eligible for the supplement, Older child at home is an indicator for whether older but under school age siblings need to be also taken care of at home in order to be eligible for the supplement.

Table 2: Description of early child tests

| VARs | (1)<br>Main<br>outcome | (2)<br>Broad<br>outcome | (3) | (4) | (5) |
|---|---|---|---|---|---|
| Mean | 0.25 | 0.28 | | | |
| SD | 0.43 | 0.45 | | | |
| N | 164224 | 292474 | | | |
| Age 4<br>(2010 →) | Cross | Ask | Details | Colors | Notes |
| Mean | 0.11 | 0.019 | 0.021 | 0.047 | 0.09 |
| SD | 0.31 | 0.14 | 0.14 | 0.21 | 0.29 |
| N | 195084 | 179015 | 193848 | 201856 | 16433 |
| Age 5<br>(→ 2009) | Circle | Square | Human | Instruct | Notes |
| Mean | 0.055 | 0.069 | 0.069 | 0.037 | 0.1 |
| SD | 0.23 | 0.25 | 0.25 | 0.19 | 0.3 |
| N | 165111 | 163082 | 154595 | 174524 | 18271 |

*Note*: Columns (1) through (4) give the mean failing rate, standard deviation and number of observations in individual tests. Column (5) gives these for the indicator of whether there were text notes indicating failing a test. Upper panel gives the main descriptives of the main outcome measure used in the analysis, as well as the broad outcome used as supplementary outcome measure. Mid-panel is for tests that are done at age four, used as the main outcome for 2010 onwards and lower panel are tests done at age five used as the main outcome from tests done up until 2009.

Table 3: The effect of HCA on parental outcomes

| VARs | (1)<br>HCA | (2)<br>Employment | (3)<br>Earnings | (4)<br>Income |
|---|---|---|---|---|
| Supplem. | 273.1*** | -0.0127*** | -194.2*** | 237.1*** |
| | (50.3) | (0.0015) | (62.3) | (47.0) |
| N | 1,045,364 | 1,045,364 | 1,045,364 | 1,045,364 |
| $R^2$ | 0.31 | 0.16 | 0.19 | 0.23 |
| out mean | 2787 | 0.27 | 6454 | 14342 |

*Note*: Dependent variables are: HCA added with possible supplements in column (1), maternal employment in column (2), maternal labor earnings in column (3), mother's disposable income including earnings and income transfers in column (4). Each column is separate regression for different outcomes. Supplement refers to municipality specific supplement to home care allowance in 100 euros. The specification controls for year and municipality dummies, as well as mother's age dummies, mother's education level dummies, dummy for mother living with a spouse, dummy for being an immigrant, child age dummies (in months at the end of the year when supplement is measured).

Table 4: The effect of HCA on failing an early outcome test

| VARs | (1)<br>Cognitive | (2)<br>Cognitive<br>4-6 yo tests | (3)<br>Motor |
|---|---|---|---|
| Supplem. | 0.0178*** | 0.0159*** | 0.0032 |
| | (0.0044) | (0.0053) | (0.0045) |
| N | 153,653 | 221,071 | 205,593 |
| $R^2$ | 0.0439 | 0.0327 | 0.0504 |
| out mean | 0.25 | 0.27 | 0.10 |

*Note*: Dependent variables are: a dummy for failing at least one of cognitive tests conducted at child health clinics at age 5 or 4 years old in column (1), failing any of cognitive tests done at different visits to child health clinics during ages 4 through 6 observed until 2011 in column (2) and failing at least one of cross motor skills tests as part of the same neurological examination in column (3). Supplement refers to municipality specific supplement to home care allowance in 100 euros. The specification controls for year and municipality dummies, as well as mother's age dummies, mother's education level dummies, dummy for mother living with a spouse, dummy for being an immigrant, child age dummies (in months at the end of the year when supplement is measured).

Table 5: The effect of HCA on child long-term outcomes

| VARs | (1)<br>High school | (2)<br>Vocational | (3)<br>College by 23 | (4)<br>Convict by 18 |
|---|---|---|---|---|
| Supplem. | -0.0060*** | 0.0055*** | -0.0070*** | 0.0022*** |
| | (0.0016) | (0.0016) | (0.0025) | (0.0006) |
| N | 491,165 | 475,035 | 348,408 | 359,245 |
| $R^2$ | 0.1405 | 0.1451 | 0.1407 | 0.0169 |
| out mean | 0.52 | 0.46 | 0.41 | 0.04 |

*Note*: Dependent variables are: child enrolling to academic high school (typically at ages 15 to 17 years old) in column (1), child enrolling to vocational secondary education in column (2), child enrolling to college by age 23 in column (3) and having been sentenced from crime committed at ages 15 to 18 years old in column (4). Supplement refers to municipality specific supplement to home care allowance in 100 euros. The specification controls for year and municipality dummies, as well as mother's age dummies, mother's education level dummies, dummy for mother living with a spouse, dummy for being an immigrant, child age dummies (in monhts at the end of the year when supplement is measured).

Table 6: Effect of Day care fee reform in 1997 on mothers' outcomes

| VARs | (1)<br>Employment | (2)<br>Earnings | (3)<br>Income | (4)<br>HCA |
|---|---|---|---|---|
| Δ DCF | 0.0157*** | 345.2*** | 343.4*** | -155.35*** |
| | (0.0042) | (61.91) | (48.33) | (50.48) |
| Supplement | -0.0126*** | 285.67*** | -74.11 | 298.21*** |
| | (0.0036) | (65.10) | (54.28) | (62.87) |
| N | 291,394 | 291,394 | 291,394 | 291,394 |
| $R^2$ | 0.1654 | 0.24 | 0.25 | 0.32 |
| out mean | 0.37 | 4838 | 11637 | 2726.46 |

*Note*: DiD regressions for the effect of the day care fee reform in 1997 on mothers' outcomes. Δ DCF refers to a change in imputed day care fees in 100 euros due to the reform. Each column is a separate regression. Column (1) is for maternal employment indicator, column (2) for labor earnings, column (3) for income including income transfers, and column (4) for HCA usage in euros per year. The specification controls for variation in HCA supplements, the main effect of day care fees, year and municipality dummies, as well as mother's age dummies, mother's education level dummies, dummy for mother living with a spouse, dummy for being an immigrant, child age dummies (in months at the end of the year when supplement is measured).

Table 7: Effect of Day care fee reform in 1997 on short-term child outcomes

| VARs | (1)<br>Cognitive | (2)<br>Any early | (3)<br>Motor |
|---|---|---|---|
| Δ DCF | -0.0070 | -0.0250*** | -0.0216 |
| | (0.0153) | (0.0089) | (0.0134) |
| Supplement | 0.0110 | 0.0417*** | -0.0182 |
| | (0.0152) | (0.0144) | (0.0123) |
| N | 10,359 | 29,698 | 16,033 |
| $R^2$ | 0.0786 | 0.0580 | 0.0492 |
| out mean | 0.24 | 0.86 | 0.16 |

*Note*: DiD regressions for the effect of the day care fee reform in 1997 on early child tests. Δ DCF refers to a change in imputed day care fees in 100 euros due to the reform. Each column is a separate regression. Dependent variables are: child failing one of the cognitive tests at age 5 in column (1), child failing at least one of the early tests conducted at ages 4 through 6 in column (2), child failing an early motor skills tests in column (3). The specification controls for variation in HCA supplements, the main effect of day care fees, year and municipality dummies, as well as mother's age dummies, mother's education level dummies, dummy for mother living with a spouse, dummy for being an immigrant, child age dummies (in months at the end of the year when supplement is measured).

Table 8: Effect of Day care fee reform in 1997 on long-term child outcomes

| VARs | (1)<br>High school | (2)<br>Vocational | (3)<br>College by 23 | (4)<br>Convict by 18 |
|---|---|---|---|---|
| Δ DCF | 0.0055* | -0.0058* | 0.0011 | -0.0012 |
| | (0.0030) | (0.0032) | (0.0030) | (0.0012) |
| Supplement | -0.0056*** | 0.0058*** | -0.0048** | 0.0023*** |
| | (0.0020) | (0.0020) | (0.0022) | (0.0006) |
| N | 283,335 | 274,646 | 282,705 | 291,394 |
| $R^2$ | 0.1696 | 0.1750 | 0.1364 | 0.0172 |
| out mean | 0.52 | 0.47 | 0.43 | 0.04 |

*Note*: DiD regressions for the effect of the day care fee reform in 1997 on long-term child outcomes. Δ DCF refers to a change in imputed day care fees in 100 euros due to the reform. Each column is a separate regression. Dependent variables are: child enrolling to academic high school (typically at ages 15 or 16 years old) in column (1), child enrolling to vocational secondary education in column (2), child enrolling to college by age 23 in column (3), having been sentenced or fined from crime committed from ages 15 to 20 in column (4) and appearing in police arrest records between from ages 15 to 20 in column (5). The specification controls for variation in HCA supplements, the main effect of day care fees, year and municipality dummies, as well as mother's age dummies, mother's education level dummies, dummy for mother living with a spouse, dummy for being an immigrant, child age dummies (in months at the end of the year when supplement is measured).

# APPENDIX FOR ONLINE PUBLICATION

Figure .17: Child care enrollment rate

| Age | Denmark | Finland | France | Germany | Norway | Sweden | United States |
|---|---|---|---|---|---|---|---|
| 0-2 | 58.5 | 26 | 47 | 26 | 52 | 46 | 25 |
| 3 | 97 | 67 | 99 | 89 | 94 | 94 | 40 |
| 4 | 98 | 73 | 100 | 95 | 96 | 97 | 64 |
| 5 | 98 | 77 | 100 | 97 | 97 | 98 | 91 |

*Note*: Source: OECD Family data base. The table shows enrolment rate to child care by the age category of children in different rows and for different countries in the columns.

## A Detailed information on child health care clinic data

In Finland there is a system of periodic health checks for the expecting mothers and children under the school age that are organized in maternity or child health care clinics. Municipalities are responsible for organizing the health checks and providing them for free for all mothers and children who live in the municipality. Our early outcome data are collected from electronic records of child health care clinics. Especially important as an outcome for children are so called extensive health checks at ages 4 months, 18 months and 4 years (5 years prior to 2010). The extensive health checks aim at providing comprehensive picture of the health status of the children and parents. Both parents can participate to the extensive health check, and they involve physiological and neurological testing as well as psychological evaluation. The extensive health checks are not mandatory, but given that they are a free service for the family and the child health clinics contact the family and make the appointment to the health check, it is not surprising that a very large fraction of families do go to the health checks.

In the child health care clinic children undergo different tests designed to detect any slow development in speech, neurological, physiological or psychological/emotional areas. The tests are conducted by nurse and usually in connection with the extensive health checks, also a medical doctor. The exact tests vary by age. Our data originate from electronic files that the nurse records after the tests using electronic forms. It is possible that some child health care clinics do not use the electronic forms to record all the tests they do. They can also use old fashioned paper files, in which case we do not observe the children having done the tests. In case we do observe the child in the tests, the information about them is recorded by the child's personal identity number. Thus even if they change municipalities, we are able to follow them and have the information about consequent tests connected to the same child. There are three or four different companies that provide the electronic forms used to record the information in child health care clinics, and we have bought the data from two of the largest. Some rare municipalities used an electronic systems to record the child development tests in 1990s, and the usage of electronic files has become much more common over time. Even in the most recent years we do not observe all children in the data, either because the municipality used some other electronic system than that we have data from, or because some of the tests were not recorded in the system, although the system in principle is in use.

The data coverage is described in Table A.9 overall, in Table A.10 for the largest municipalities individually, and Table A.11 by birth year of children. Table A.9 shows in first row the amount of children in the data for years 1994 to 2014, the second row the amount of children that appear somewhere in the child health care clinic data for tests done between ages 3 and 6 years old, and the third row shows the amount of children for which we observe the extensive health check, the Lene test, the main outcome. First column is for all the observations in the data and the second column for years between 1998 and 2012 and discarding tiny municipalities, when the electronic files were more common. Table A.10 shows the amount of children and share of children in our child outcome data for largest municipalities individually. The table includes years from 2000 to 2011, when the coverage of our data is the greatest. Table A.9 shows the amount of children in population, the amount of children in child health clinic data and the amount of children for which we have the Lene test by birth year of children. Clearly the share for which we observe the child health clinic data becomes larger over time, except for the latest years when the children start to become too young to appear in some of the tests.

Table A.9: Amount of children in data overall and in child health care clinic data

| Var | All | Y 1998 - 2012 |
|---|---|---|
| N children | 1170383 | 645685 |
| N data | 343402 | 300289 |
| N Lene | 214781 | 193817 |

Our raw data are variables from electronic forms that are filled in after the health check is over. Although the forms are slightly different for different companies from which we collected the data, the tests itself are the same across municipalities. The individual test results are recorded in the same way: +/- for whether or not the test is passed, and some comments in open text form for each test. Child health care clinic visit at different ages are scheduled to have different tests, and the tests for the same age are done in the same way for all children across Finland. The neurological tests utilize specific tools, such as cubics, show cards and paper and pen that are provided centrally to the child health

Table A.10: Amount of children and share in child health data in the largest municipalities

| Municipality | Share in data | Lene dummy | N children |
|---|---|---|---|
| Helsinki | .8530374 | .6646039 | 58981 |
| Espoo | .5836636 | .2055892 | 34316 |
| Vantaa | .1088851 | .0643255 | 26055 |
| Tampere | .813534 | .5970075 | 22122 |
| Turku | .8589528 | .7218859 | 17838 |
| Jyväskylä | .6443348 | .3716784 | 11553 |
| Lahti | .8711591 | .6443991 | 10284 |

care clinics and thus are similar across the country. In the extensive health checks medical doctors record growth of children, measure their hearing and eyesight, other physiological measures, as well as some of the neurological measures.

For this study we have information about the neurological tests that we use as an early outcome. The aim of the neurological tests is to understand in part the cognitive development of children, and in part cross or fine motor skills as well as emotional and psychological development. We focus on tests that are supposed to test for the cognitive development of children.

When the extensive health check was done at age five, prior to 2010, the neurological testing consisted of twelve individual tests. These were; Jump on one foot, Cut a circle from a paper, Draw a square, Draw human being with 4 to 5 different parts, Do a pencil grasp, Whether child is left or right handed, Speaking normally without deformations, Child can follow three-part instructions, Child role plays, Child focuses on one thing only at least for a while, Child has the ability to co-operate and Child is able to form friend relationships. We select as our main outcome as describing cognitive development of children; Cut a circle, Draw a square, Draw human being and Follow instructions. The other measure either physiological aspects or are not based on clean cut test conducted at the clinic, rather than asked from a parent. In 2010 and thereafter the extensive test is done at age 4, and the tests done at that age are different (less demanding due to younger age of children). The tests are; Walk a line, Draw a cross, Ask where and when, Speaking is comprehensible, Explain details from a picture in a show card, Can identify 3 out of 4 main colors in a show card, Plays with other children, Mimics a parent (of same sex). For the same reasons as above, we choose as our main outcome; Draw a cross, Asks when or where, Explains details and Identifies main colors.

We construct a dummy taking value one if child fails at least one of the tests and zero otherwise, at the extensive health check at age five or four (depending on year the test is conducted). We complement the +/- information for failing an individual test with information from the open text remarks connected to the individual test in question. The

Table A.11: Amount of children and share in child health data by birth year of children

| Year | N children | N child data | N Lene |
|---|---|---|---|
| 1996 | 17110 | 3006 | 1911 |
| 1997 | 36010 | 4487 | 3113 |
| 1998 | 40829 | 7808 | 3956 |
| 1999 | 41728 | 12231 | 6385 |
| 2000 | 43465 | 14934 | 10651 |
| 2001 | 43318 | 18137 | 11814 |
| 2002 | 43304 | 18977 | 12296 |
| 2003 | 43327 | 19266 | 12306 |
| 2004 | 44757 | 20991 | 12909 |
| 2005 | 45274 | 22011 | 14211 |
| 2006 | 45901 | 22410 | 14558 |
| 2007 | 47232 | 24292 | 15543 |
| 2008 | 48032 | 25251 | 15788 |
| 2009 | 48675 | 25719 | 16274 |
| 2010 | 49836 | 25504 | 18456 |
| 2011 | 49971 | 24450 | 16478 |
| 2012 | 48619 | 20293 | 13410 |
| 2013 | 44396 | 15801 | 11645 |
| Total | 781784 | 325568 | 211704 |

reason for this is that in the data the data does not include the binary outcome, or the test is marked as pass, but the open text form clearly indicates that there was some problem with the test. We include these as failing the test in the dummy constituting the main outcome. We needed to code the open text fields to have a numerical variable indicating a negative remark for that test. This was done by capturing words that indicate a good performance, such as "well" or "good" ("hyvin" in Finnish). If such words were found in the text field, that text field was not taken into consideration. Other text fields not containing such words were interpreted as indications of failing the test.

Table A.12 shows descriptive statistics of the individual tests and the dummy we use as main outcome that is formed based on the individual tests and the open text remarks. Upper panel in the table is for tests done at age four and lower panel for tests at age five. Variable "Comb" gives the combined rate of failing at least one of the tests and "Comb + notes" the combined rate of either failing one of the tests or having negative remarks. Our main outcome has a failing rate of 23% in test at age four and 21% in tests at age five.

Table A.12: Description of early child tests

| | (1) Cross | (2) Ask | (3) Details | (4) Colours | (5) Notes | (6) Comb | (7) Comb +notes |
|---|---|---|---|---|---|---|---|
| Mean | .15 | .02 | .027 | .045 | .089 | .18 | .23 |
| Sd | .36 | .14 | .16 | .21 | .29 | .38 | .42 |
| N | 45872 | 41491 | 46116 | 47318 | 48511 | 48511 | 48511 |
| | Circle | Square | Human | Instruct | Notes | Comb | Comb +notes |
| Mean | .058 | .071 | .072 | .041 | .099 | .15 | .21 |
| Sd | .23 | .26 | .26 | .2 | .3 | .36 | .41 |
| N | 128172 | 122839 | 120197 | 133556 | 144358 | 144358 | 144358 |

Table A.13: Description of early child tests used to build the alternative cognitive tests

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| | | | | Tests at 4 years old | | | | |
| Tests | Cross | Asks | Details | Colors | Compreh. | Play | | |
| Av. fail | .109 | .019 | .021 | .047 | .029 | .008 | | |
| Count | 195084 | 179015 | 193848 | 201856 | 201251 | 194714 | | |
| | | | | Tests at 5 years old | | | | |
| | Circle | Square | Human | Instruct | Focus | Tell | Cooper. | Friends |
| Av. fail | .055 | .069 | .069 | .037 | .01 | .03 | .006 | .006 |
| Count | 165111 | 163082 | 154595 | 174524 | 186964 | 183003 | 186061 | 174739 |
| | | | | Tests at 6 years old | | | | |
| | Human2 | Triangle | Repeat | Tell2 | Planned | Group | | |
| Av. fail | .021 | .032 | .044 | .01 | .005 | .006 | | |
| Count | 134768 | 153089 | 140415 | 139009 | 148770 | 153751 | | |

# B Additional child outcome tests at the child health clinic

Table B.14: The effect of supplement on individual child outcome tests at four years old tests

| VARs | (1) Walk line | (2) Compr.dable | (3) Cross | (4) Ask2 | (5) Details | (6) Play | (7) Colors | (8) Mimick |
|---|---|---|---|---|---|---|---|---|
| Supplem. | 0.0013** | 0.0006 | -0.0035 | 0.0008 | -0.0005 | -0.0008 | 0.0003 | 0.0005 |
| | (0.0006) | (0.0013) | (0.0052) | (0.0010) | (0.0013) | (0.0005) | (0.0012) | (0.0004) |
| N | 177,331 | 188,697 | 182,966 | 167,900 | 181,670 | 182,751 | 189,307 | 143,124 |
| $R^2$ | 0.0049 | 0.0112 | 0.0327 | 0.0108 | 0.0197 | 0.0049 | 0.0210 | 0.0042 |
| out mean | 0.01 | 0.03 | 0.11 | 0.02 | 0.02 | 0.01 | 0.05 | 0.01 |

Table B.15: The effect of supplement on individual child outcome tests at five years old tests

| VARs | (1) Jump 1 leg | (2) Pencil | (3) Dom. hand | (4) Explain | (5) Cut circle | (6) Square |
|---|---|---|---|---|---|---|
| Supplem. | 0.0034 | 0.0017 | -0.0007 | 0.0013 | 0.0081** | 0.0044* |
| | (0.0025) | (0.0017) | (0.0009) | (0.0016) | (0.0039) | (0.0026) |
| N | 161,083 | 168,122 | 169,848 | 170,659 | 153,617 | 151,519 |
| $R^2$ | 0.0169 | 0.0077 | 0.0031 | 0.0235 | 0.0217 | 0.0145 |
| out mean | 0.08 | 0.05 | 0.02 | 0.03 | 0.06 | 0.07 |

| VARs | (7) Human | (8) Instruct | (9) Role play | (10) Focus | (11) Cooperate | (12) Friends |
|---|---|---|---|---|---|---|
| Supplem. | 0.0035 | 0.0054 | -0.0008 | -0.0001 | 0.0004 | -0.0002 |
| | (0.0033) | (0.0037) | (0.0008) | (0.0005) | (0.0004) | (0.0005) |
| N | 144,458 | 162,684 | 146,564 | 174,353 | 173,568 | 163,201 |
| $R^2$ | 0.0183 | 0.0366 | 0.0041 | 0.0083 | 0.0045 | 0.0048 |
| out mean | 0.07 | 0.04 | 0.01 | 0.01 | 0.01 | 0.01 |

Table B.16: The effect of supplement on individual child outcome tests at six years old tests

| VARs | (1)<br>Sports | (2)<br>Clearspeech | (3)<br>Human2 | (4)<br>Triangle | (5)<br>Repeat | (6)<br>Explain2 | (7)<br>Planned |
|---|---|---|---|---|---|---|---|
| Supplem. | 0.0013 | 0.0030 | -0.0008 | -0.0019 | 0.0021 | 0.0009 | 0.0005 |
| | (0.0009) | (0.0023) | (0.0010) | (0.0017) | (0.0021) | (0.0008) | (0.0005) |
| N | 146,887 | 145,426 | 125,649 | 142,366 | 131,008 | 129,433 | 138,568 |
| $R^2$ | 0.0089 | 0.0129 | 0.0092 | 0.0125 | 0.0243 | 0.0114 | 0.0045 |
| out mean | 0.01 | 0.10 | 0.02 | 0.03 | 0.04 | 0.01 | 0.01 |

# C Heterogeneity in DiD results by parents' characteristics

Table C.17: The effect of HCA on parental outcomes: College educated mother

| VARs | (1) HCA | (2) Employment | (3) Earnings | (4) Income |
|---|---|---|---|---|
| Supplem. | 280.8*** | -0.0156*** | -238.2*** | 135.9*** |
| | (40.8) | (0.0032) | (75.8) | (52.2) |
| N | 375,176 | 375,176 | 375,176 | 375,176 |
| $R^2$ | 0.2663 | 0.147 | 0.173 | 0.168 |
| out mean | 2352.8 | 0.40 | 8850.7 | 16882.3 |

*Note*: Results for a sample where mother has college education attained. Dependent variables are: HCA added with possible supplements in column (1), maternal employment in column (2), maternal labor earnings in column (3), mother's disposable income including earnings and income transfers in column (4),earnings of a spouse in column (5) and total family income in column (6). Each column is separate regression for different outcomes. Supplement refers to municipality specific supplement to home care allowance in 100 euros. The specification controls for year and municipality dummies, as well as mother's age dummies, mother's education level dummies, dummy for mother living with a spouse, dummy for being an immigrant, child age dummies (in months at the end of the year when supplement is measured).

Table C.18: The effect of HCA on parental outcomes: Less than college educated mother

| VARs | (1)<br>HCA | (2)<br>Employment | (3)<br>Earnings | (4)<br>Income |
|---|---|---|---|---|
| Supplem. | 272.2*** | -0.0135*** | -300.583*** | 138.7*** |
| | (44.7) | (0.0019) | (82.6) | (51) |
| N | 521,452 | 521,452 | 521,452 | 521,452 |
| $R^2$ | 0.2750 | 0.142 | 0.168 | 0.187 |
| out mean | 2427.5 | 0.40 | 9666.2 | 17843.2 |

*Note*: Results for a sample where mother does not have college education attained. Dependent variables are: HCA added with possible supplements in column (1), maternal employment in column (2), maternal labor earnings in column (3), mother's disposable income including earnings and income transfers in column (4),earnings of a spouse in column (5) and total family income in column (6). Each column is separate regression for different outcomes. Supplement refers to municipality specific supplement to home care allowance in 100 euros. The specification controls for year and municipality dummies, as well as mother's age dummies, mother's education level dummies, dummy for mother living with a spouse, dummy for being an immigrant, child age dummies (in months at the end of the year when supplement is measured).

Table C.19: The effect of HCA on failing an early outcome test: College educated mother

| VARs | (1)<br>Cognitive | (2)<br>Cognitive<br>4-6 yo tests | (3)<br>Motor |
|---|---|---|---|
| Supplem. | 0.0188*** | 0.0143** | 0.0063 |
| | (0.0059) | (0.0057) | (0.0057) |
| N | 62,327 | 106,761 | 82,853 |
| $R^2$ | 0.0394 | 0.0258 | 0.0418 |
| out mean | 0.20 | 0.23 | 0.10 |

*Note*: Results for a sample where mother has college education attained. Dependent variables are: a dummy for failing at least one of cognitive tests conducted at child health clinics at age 5 or 4 years old in column (1), failing at least one of motor skills tests as part of the same neurological examination in column (2) and failing any of early tests done at ages 4 to 6 observed until 2011 in column (3). Supplement refers to municipality specific supplement to home care allowance in 100 euros. The specification controls for year and municipality dummies, as well as mother's age dummies, mother's education level dummies, dummy for mother living with a spouse, dummy for being an immigrant, child age dummies (in months at the end of the year when supplement is measured).

Table C.20: The effect of HCA on failing an early outcome test: Less than college educated mother

| VARs | (1) Cognitive | (2) Cognitive 4-6 yo tests | (3) Motor |
|---|---|---|---|
| Supplem. | 0.0176*** | 0.0178*** | 0.0019 |
| | (0.0046) | (0.0065) | (0.0043) |
| N | 91,376 | 114,310 | 122,904 |
| $R^2$ | 0.0380 | 0.0282 | 0.0592 |
| out mean | 0.28 | 0.31 | 0.10 |

*Note*: Results for a sample where mother mother does not have college education attained. Dependent variables are: a dummy for failing at least one of cognitive tests conducted at child health clinics at age 5 or 4 years old in column (1), failing at least one of motor skills tests as part of the same neurological examination in column (2) and failing any of early tests done at ages 4 to 6 observed until 2011 in column (3). Supplement refers to municipality specific supplement to home care allowance in 100 euros. The specification controls for year and municipality dummies, as well as mother's age dummies, mother's education level dummies, dummy for mother living with a spouse, dummy for being an immigrant, child age dummies (in months at the end of the year when supplement is measured).

Table C.21: The effect of HCA on child long-term outcomes: College educated mother

| VARs | (1) High school | (2) Vocational | (3) College by 23 | (4) Convict by 18 |
|---|---|---|---|---|
| Supplem. | -0.0059*** | 0.0049** | -0.0071*** | 0.0018** |
| | (0.0022) | (0.0021) | (0.0026) | (0.0008) |
| N | 210,215 | 205,560 | 145,423 | 148,562 |
| $R^2$ | 0.0798 | 0.0888 | 0.0740 | 0.0073 |
| out mean | 0.69 | 0.30 | 0.55 | 0.02 |

*Note*: Results for a sample where mother has college education attained. Dependent variables are: child enrolling to academic high school (typically at ages 15 or 16 years old) in column (1), child enrolling to vocational secondary education in column (2), child enrolling to college (typically at ages 18 to 20 years old) in column (3) and having been sentenced from crime committed at ages 15 to 18 years old in column (4). Supplement refers to municipality specific supplement to home care allowance in 100 euros. The specification controls for year and municipality dummies, as well as mother's age dummies, mother's education level dummies, dummy for mother living with a spouse, dummy for being an immigrant, child age dummies (in months at the end of the year when supplement is measured).

Table C.22: The effect of HCA on child long-term outcomes: Less than college educated mother

| VARs | (1)<br>High school | (2)<br>Vocational | (3)<br>College by 23 | (4)<br>Convict by 18 |
|---|---|---|---|---|
| Supplem. | -0.0047** | 0.0042** | -0.0038 | 0.0023*** |
| | (0.0022) | (0.0021) | (0.0032) | (0.0009) |
| N | 280,950 | 269,475 | 202,985 | 210,683 |
| $R^2$ | 0.0536 | 0.0569 | 0.0530 | 0.0148 |
| out mean | 0.39 | 0.59 | 0.31 | 0.05 |

*Note*: Results for a sample where mother does not have college education attained. Dependent variables are: child enrolling to academic high school (typically at ages 15 or 16 years old) in column (1), child enrolling to vocational secondary education in column (2), child enrolling to college (typically at ages 18 to 20 years old) in column (3) and having been sentenced from crime committed at ages 15 to 18 years old in column (4). Supplement refers to municipality specific supplement to home care allowance in 100 euros. The specification controls for year and municipality dummies, as well as mother's age dummies, mother's education level dummies, dummy for mother living with a spouse, dummy for being an immigrant, child age dummies (in months at the end of the year when supplement is measured).

Table C.23: The effect of HCA on parental outcomes: High income family

| VARs | (1)<br>HCA | (2)<br>Employment | (3)<br>Earnings | (4)<br>Income |
|---|---|---|---|---|
| Supplem. | 269.3*** | -0.0111*** | -209.1*** | 270.3*** |
| | (56.2) | (0.0017) | (78.3) | (52.6) |
| N | 670,188 | 670,188 | 670,188 | 670,188 |
| $R^2$ | 0.315 | 0.092 | 0.162 | 0.24 |
| out mean | 3029.9 | 0.19 | 5113 | 12919.4 |

*Note*: Results for a sample where family has higher than medium earnings in year before the child was born. Dependent variables are: HCA added with possible supplements in column (1), maternal employment in column (2), maternal labor earnings in column (3), mother's disposable income including earnings and income transfers in column (4),earnings of a spouse in column (5) and total family income in column (6). Each column is separate regression for different outcomes. Supplement refers to municipality specific supplement to home care allowance in 100 euros. The specification controls for year and municipality dummies, as well as mother's age dummies, mother's education level dummies, dummy for mother living with a spouse, dummy for being an immigrant, child age dummies (in months at the end of the year when supplement is measured).

Table C.24: The effect of HCA on parental outcomes: Low income family

| VARs | (1)<br>HCA | (2)<br>Employment | (3)<br>Earnings | (4)<br>Income |
|---|---|---|---|---|
| Supplem. | 249.3*** | -0.0064*** | -87.6*** | 263.7*** |
| | (62.6) | (0.0013) | (22) | (40.1) |
| N | 523,912 | 523,912 | 523,912 | 523,912 |
| $R^2$ | 0.347 | 0.096 | 0.121 | 0.184 |
| out mean | 3144.6 | 0.14 | 3257.7 | 10856.6 |

*Note*: Results for a sample where family has lower than medium earnings in year before the child was born. Dependent variables are: HCA added with possible supplements in column (1), maternal employment in column (2), maternal labor earnings in column (3), mother's disposable income including earnings and income transfers in column (4),earnings of a spouse in column (5) and total family income in column (6). Each column is separate regression for different outcomes. Supplement refers to municipality specific supplement to home care allowance in 100 euros. The specification controls for year and municipality dummies, as well as mother's age dummies, mother's education level dummies, dummy for mother living with a spouse, dummy for being an immigrant, child age dummies (in months at the end of the year when supplement is measured).

Table C.25: The effect of HCA on failing an early outcome test: High income family

| VARs | (1)<br>Cognitive | (2)<br>Cognitive<br>4-6 yo tests | (3)<br>Motor |
|---|---|---|---|
| Supplem. | 0.0181*** | 0.0140** | 0.0030 |
| | (0.0042) | (0.0059) | (0.0043) |
| N | 83,420 | 116,007 | 109,378 |
| $R^2$ | 0.0380 | 0.0277 | 0.0473 |
| out mean | 0.23 | 0.24 | 0.09 |

*Note*: Results for a sample where family has higher than medium earnings in year before the child was born. Dependent variables are: a dummy for failing at least one of cognitive tests conducted at child health clinics at age 5 or 4 years old in column (1), failing at least one of motor skills tests as part of the same neurological examination in column (2) and failing any of early tests done at ages 4 to 6 observed until 2011 in column (3). Supplement refers to municipality specific supplement to home care allowance in 100 euros. The specification controls for year and municipality dummies, as well as mother's age dummies, mother's education level dummies, dummy for mother living with a spouse, dummy for being an immigrant, child age dummies (in months at the end of the year when supplement is measured).

Table C.26: The effect of HCA on failing an early outcome test: Low income family

| VARs | (1)<br>Cognitive | (2)<br>Cognitive<br>4-6 yo tests | (3)<br>Motor |
|---|---|---|---|
| Supplem. | 0.0174*** | 0.0191*** | 0.0039 |
| | (0.0055) | (0.0062) | (0.0052) |
| N | 70,262 | 105,064 | 96,349 |
| $R^2$ | 0.0481 | 0.0352 | 0.0551 |
| out mean | 0.28 | 0.30 | 0.11 |

*Note*: Results for a sample where family has lower than medium earnings in year before the child was born. Dependent variables are: a dummy for failing at least one of cognitive tests conducted at child health clinics at age 5 or 4 years old in column (1), failing at least one of motor skills tests as part of the same neurological examination in column (2) and failing any of early tests done at ages 4 to 6 observed until 2011 in column (3). Supplement refers to municipality specific supplement to home care allowance in 100 euros. The specification controls for year and municipality dummies, as well as mother's age dummies, mother's education level dummies, dummy for mother living with a spouse, dummy for being an immigrant, child age dummies (in months at the end of the year when supplement is measured).

Table C.27: The effect of HCA on child long-term outcomes: High income family

| VARs | (1)<br>High school | (2)<br>Vocational | (3)<br>College by 23 | (4)<br>Convict by 18 |
|---|---|---|---|---|
| Supplem. | -0.0066*** | 0.0058*** | -0.0046 | 0.0016 |
| | (0.0020) | (0.0020) | (0.0028) | (0.0011) |
| N | 243,242 | 237,685 | 173,649 | 177,450 |
| $R^2$ | 0.1258 | 0.1327 | 0.1036 | 0.0091 |
| out mean | 0.62 | 0.37 | 0.49 | 0.03 |

*Note*: Results for a sample where family has higher than medium earnings in year before the child was born. Dependent variables are: child enrolling to academic high school (typically at ages 15 or 16 years old) in column (1), child enrolling to vocational secondary education in column (2), child enrolling to college (typically at ages 18 to 20 years old) in column (3) and having been sentenced from crime committed at ages 15 to 18 years old in column (4). Supplement refers to municipality specific supplement to home care allowance in 100 euros. The specification controls for year and municipality dummies, as well as mother's age dummies, mother's education level dummies, dummy for mother living with a spouse, dummy for being an immigrant, child age dummies (in months at the end of the year when supplement is measured).

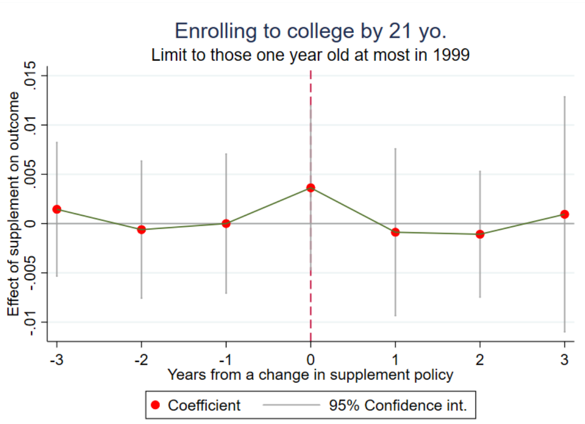
Table C.28: The effect of HCA on child long-term outcomes: Low income family

| VARs | (1) High school | (2) Vocational | (3) College by 23 | (4) Convict by 18 |
|---|---|---|---|---|
| Supplem. | -0.0044* | 0.0045* | -0.0064** | 0.0024* |
| | (0.0026) | (0.0025) | (0.0029) | (0.0013) |
| N | 247,923 | 237,350 | 174,759 | 181,795 |
| $R^2$ | 0.1001 | 0.1051 | 0.0926 | 0.0173 |
| out mean | 0.42 | 0.56 | 0.33 | 0.05 |

*Note*: Results for a sample where family has lower than medium earnings in year before the child was born. Dependent variables are: child enrolling to academic high school (typically at ages 15 or 16 years old) in column (1), child enrolling to vocational secondary education in column (2), child enrolling to college (typically at ages 18 to 20 years old) in column (3) and having been sentenced from crime committed at ages 15 to 18 years old in column (4). Supplement refers to municipality specific supplement to home care allowance in 100 euros. The specification controls for year and municipality dummies, as well as mother's age dummies, mother's education level dummies, dummy for mother living with a spouse, dummy for being an immigrant, child age dummies (in months at the end of the year when supplement is measured).

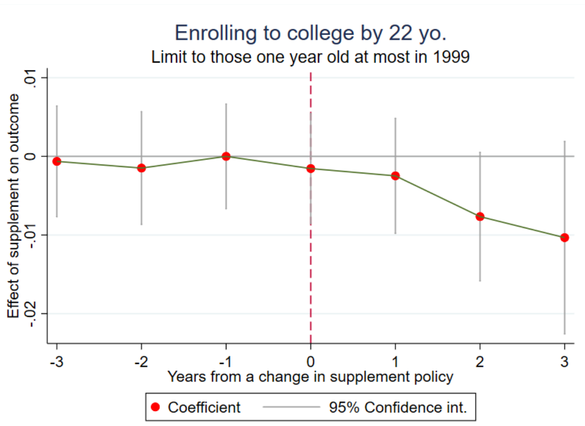
# D Additional DiD results

Figure D.18: Dynamic DiD: Enrolling to college by age 21 and limiting cohort to 1999



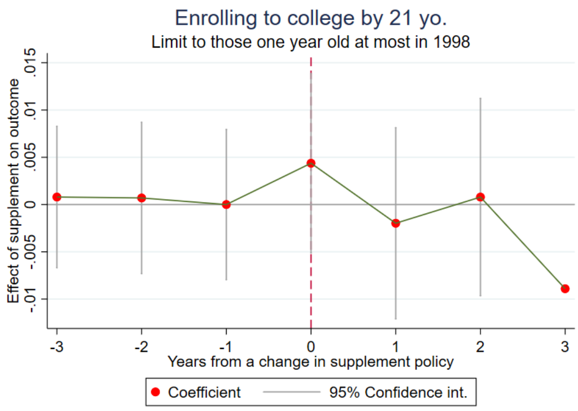

*Note*: The graph plots coefficients and confidence intervals that are estimates of leads and lags in years from changes in the supplement amount in 100 euros in year 0 as shown in equation (1). The specification controls for common year effects and municipality fixed effects. The outcome is an indicator for enrolling to college from ages 15 through 21 years old.

Figure D.19: Dynamic DiD: Enrolling to college by age 21 and limiting cohort to 1999



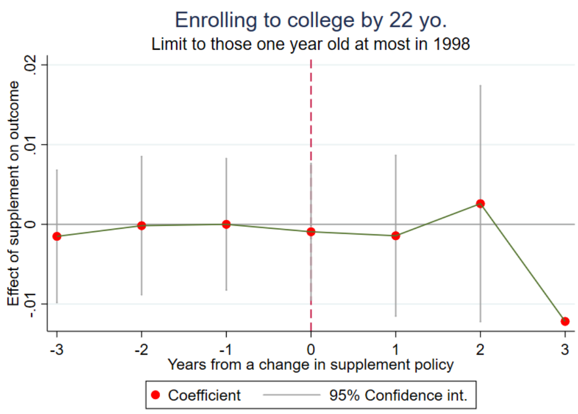

*Note*: The graph plots coefficients and confidence intervals that are estimates of leads and lags in years from changes in the supplement amount in 100 euros in year 0 as shown in equation (1). The specification controls for common year effects and municipality fixed effects. The outcome is an indicator for enrolling to college from ages 15 through 22 years old.

Figure D.20: Dynamic DiD: Enrolling to college by age 21 and limiting cohort to 1998




*Note*: The graph plots coefficients and confidence intervals that are estimates of leads and lags in years from changes in the supplement amount in 100 euros in year 0 as shown in equation (1). The specification controls for common year effects and municipality fixed effects. The outcome is an indicator for enrolling to college from ages 15 through 21 years old.

Figure D.21: Dynamic DiD: Enrolling to college by age 21 and limiting cohort to 1998




*Note*: The graph plots coefficients and confidence intervals that are estimates of leads and lags in years from changes in the supplement amount in 100 euros in year 0 as shown in equation (1). The specification controls for common year effects and municipality fixed effects. The outcome is an indicator for enrolling to college from ages 15 through 22 years old.

Table D.29: Limiting to cohorts of one year old the latest in 1999 or 1998

| VARs | (1)<br>End cohort 1999<br>College by 21 | (2)<br><br>College by 22 | (3)<br>End cohort 1998<br>College by 21 | (4)<br><br>College by 22 |
|---|---|---|---|---|
| Supplem. | 0.0000 | -0.0024 | 0.0002 | -0.0002 |
| | (0.0025) | (0.0024) | (0.0026) | (0.0026) |
| N | 301,255 | 301,255 | 254,457 | 254,457 |
| $R^2$ | 0.0948 | 0.1044 | 0.0949 | 0.1047 |
| out mean | 0.36 | 0.41 | 0.36 | 0.42 |

*Note*: The outcome in column (1) is college enrolment observed the latest by 21 years old, and in column (2) is college enrolment observed the latest by 22 years old. The end cohort is limited to those who are one year old in 1999 the latest. Columns (3) and (4) repeat this analysis, but limits the end cohort those who were one year old in 1998 the latest. Each column is separate regression for different outcomes. Supplement refers to municipality specific supplement to home care allowance in 100 euros. The specification controls for year and municipality dummies, as well as mother's age dummies, mother's education level dummies, dummy for mother living with a spouse, dummy for being an immigrant, child age dummies (in monhts at the end of the year when supplement is measured).

Table D.30: The effect of HCA on child long-term outcomes, alternative outcomes

| VARs | (1)<br>College22 | (2)<br>Convict by 20 | (3)<br>Fines by 20 |
|---|---|---|---|
| Supplem. | -0.0045* | 0.0021** | 0.0040*** |
| | (0.0024) | (0.0010) | (0.0013) |
| N | 348,408 | 359,245 | 359,245 |
| $R^2$ | 0.1328 | 0.0239 | 0.0285 |
| out mean | 0.39 | 0.06 | 0.15 |

*Note*: Dependent variables are: child enrolling to academic high school (typically at ages 15 to 17 years old) in column (1), child enrolling to vocational secondary education in column (2), child enrolling to college by age 22 in column (3), having been sentenced from crime committed at ages 15 to 22 years old in column (4) and appearing in police arrest data by age 22 in column (5). Supplement refers to municipality specific supplement to home care allowance in 100 euros. The specification controls for year and municipality dummies, as well as mother's age dummies, mother's education level dummies, dummy for mother living with a spouse, dummy for being an immigrant, child age dummies (in monhts at the end of the year when supplement is measured).

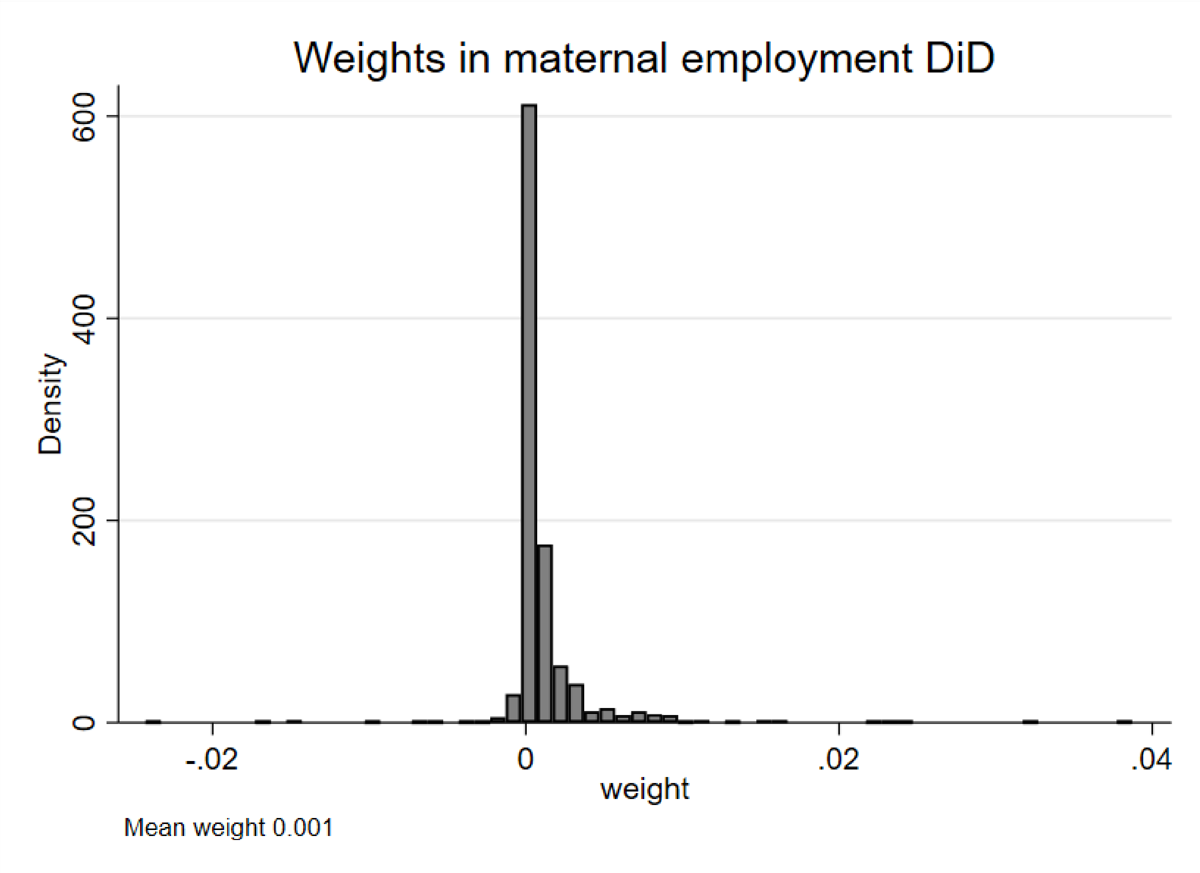
Table D.31: Effect of Day care fee reform in 1997 on earnings and income in longer term

| VARs | (1)<br>S.emp1-5 | (2)<br>Earn 1-5 | (3)<br>Inc. 1-5 | (4)<br>Fam Inc. 1-5 |
|---|---|---|---|---|
| Δ DCF | 0.0476*** | 1,889.3*** | 1,598.1*** | 3,539.5*** |
| | (0.0130) | (297.67) | (224.24) | (1,180.94) |
| N | 241,455 | 241,455 | 241,455 | 241,455 |
| $R^2$ | 0.2186 | 0.25 | 0.25 | 0.28 |
| out mean | 2.01 | 36545.18 | 54777.40 | 1.4e+05 |

*Note*: DiD regressions for the effect of the day care fee reform in 1997 on mothers' outcomes. Δ DCF refers to a change in imputed day care fees in 100 euros due to the reform. Each column is a separate regression. The outcome in column (1) is sum of maternal earnings when child is one or two years old, in column (2) is sum of maternal earnings when child is three to five years old, in column (3) is sum of maternal earnings when child is one to five years old, in column (4) is average of maternal income when child is one to two years old, column (5) is average of maternal income when child is three to five years old, (6) is sum of maternal income when child is one to five years old, in column (7) sum of family income when child is one to two years old, in column (8) sum of family income when child is three to five years old, and in column (9) sum of family income when child is one to five years old. The specification controls for the main effect of day care fees, year and municipality dummies, as well as mother's age dummies, mother's education level dummies, dummy for mother living with a spouse, dummy for being an immigrant, child age dummies (in monhts at the end of the year when supplement is measured).

# E Dealing with two-way fixed effects problems

Figure E.22: Weights in DiD regression using supplements as dependent variable and maternal employment as the outcome

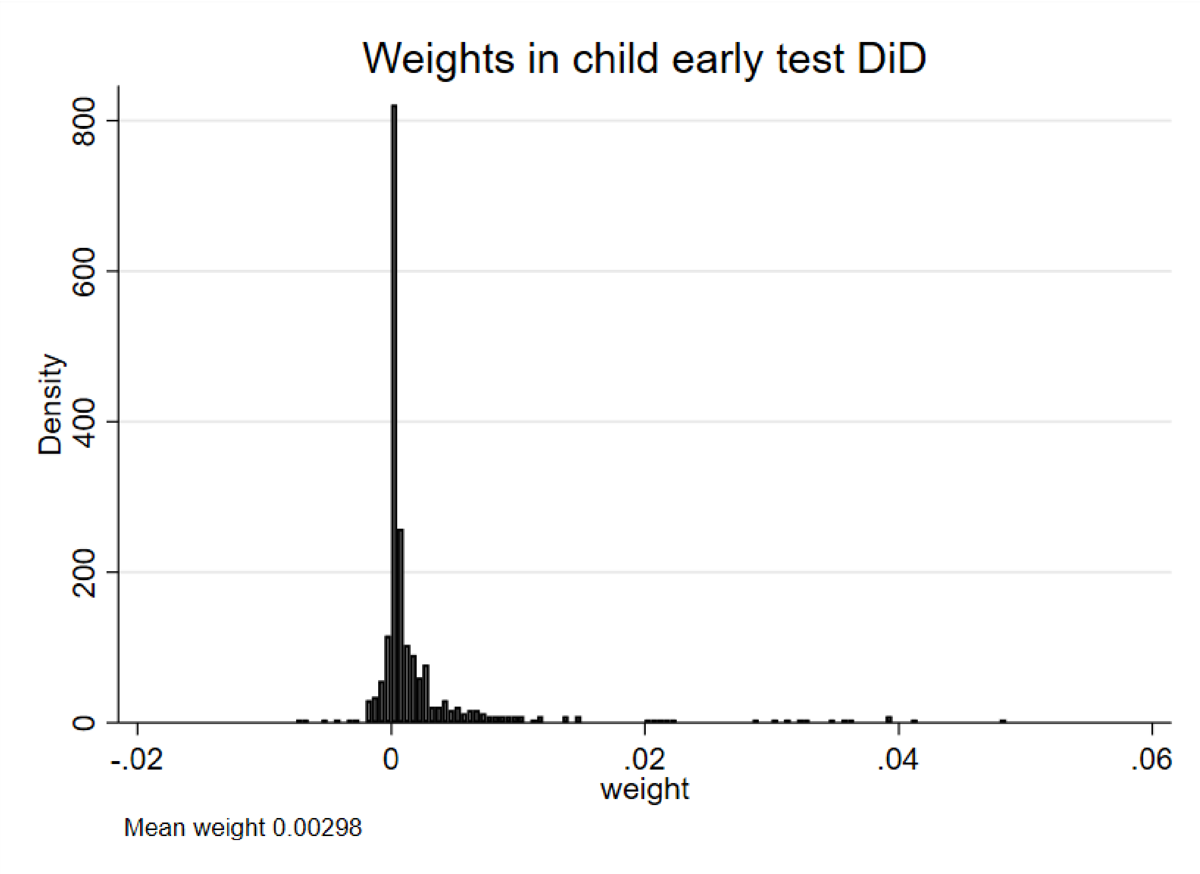



*Note*: Distribution of DiD weights used in main specification where dependent variable is supplement and outcome maternal employment.

Figure E.23: Weights in DiD regression using supplements as dependent variable and maternal earnings as the outcome


*Note*: Distribution of DiD weights used in main specification where dependent variable is supplement and outcome maternal earnings.

Figure E.24: Weights in DiD regression using supplements as dependent variable and failing early cognitive test as the outcome




*Note*: Distribution of DiD weights used in main specification where dependent variable is supplement and outcome failing early cognitive test.

This section describes the staggered DiD design and results from it. We begin by simplifying our current set up, and focus on one event (one change in the supplement) at a time for each municipality, and use as a comparison group municipalities that did not change their supplement to home care allowance during the entire study period. We label the year when the home care supplement changes as an event-year $b$. We follow each municipality three years before and after each change in municipality supplement occurs. If municipality has a new change in supplement policy we stack it to the data as a new observation. The empirical specification takes the following form:

$$Y_{it} = \theta_{bk} + \mu_{mb} + \sum_{k=-3, j\neq -1}^{3} \beta_k \Delta Supp_{mk} \ + \rho_a + \mathbf{X}_{it}\gamma + \varepsilon_{it}, \tag{4}$$

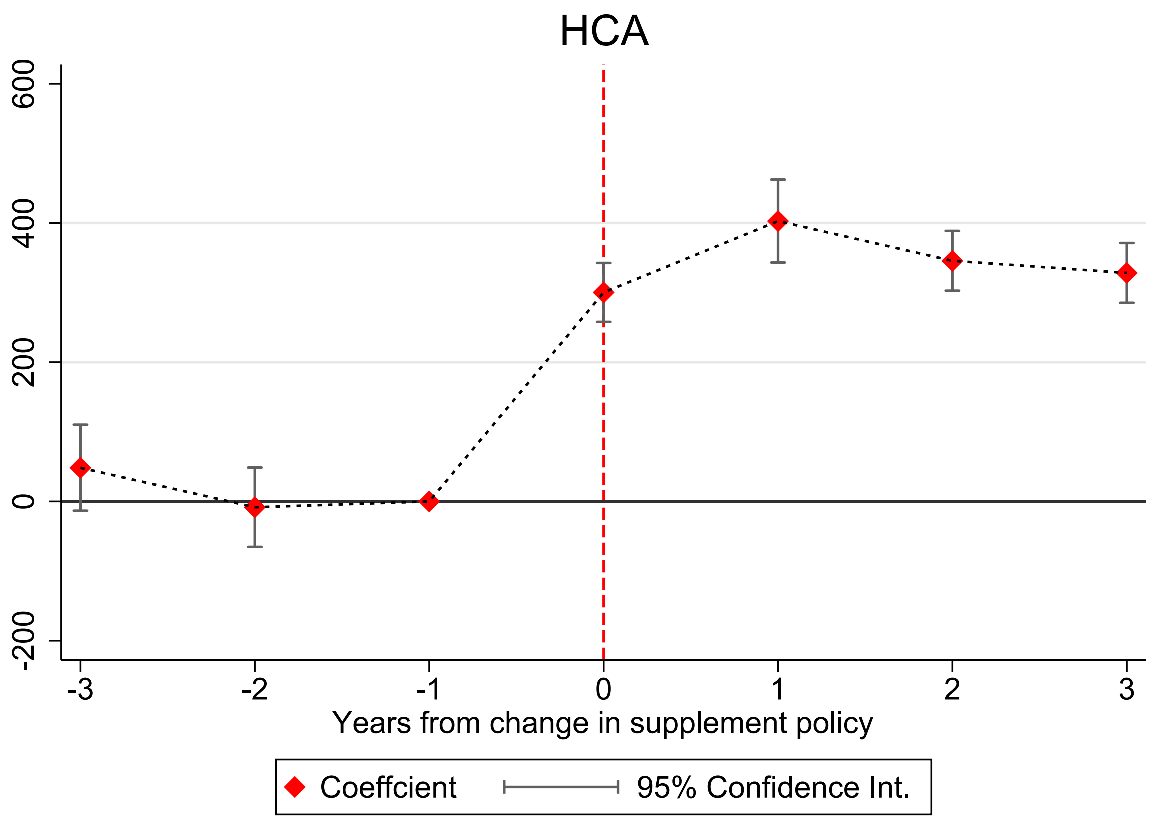
where $\Delta Supp_{mk}$ are indicators for *the change* in the supplement amount $k$ years ago in 100 euros a family is eligible for in municipality $m$. $\mu_m b$ is a event-year ($b$) -specific-municipality fixed-effect and $\theta_{bk}$ is an indicator specific to each time-since-event-year for each event year cohort. Our main interests lies in the coefficients $\beta_k$ which will identify the effect of supplement change relative to the year -1. The specification also controls for child age dummies $\rho_a$ (in months at the end of the year) and mother characteristics from pre-birth year, such as mother's age, level of education, and number of children. Note that the municipalities that did not have at a change in their supplement policy are in the comparison group in each base-year with 0 change in their home care allowance supplement.

Figure E.25 shows how the amount of home care allowance evolve around the time of the municipality specific supplement change. The Figure plots the point estimates that we obtain from (equation 4). The results indicate that amount of home care allowance received by mother's of one year old children increases with the increase in supplement amount. In Figures E.26 and E.27 show how employment and earnings of mother's of one year old children change around the time of the supplement change. The results confirm our previous findings. Mother's strongly decrease their labor supply in the municipalities when the amount of home care allowance increases.

Next we focus on children's outcomes. Figure E.29 show that increase in supplement amount at the time when child is one year old coincides with an increase in the probability that child fails his early cognition test at age five. The results on children's long-term outcomes are less clear: The probability of choosing a vocational high school track increases with the increase in the municipality specific supplement (measured when child was one years old) as shown in Figure E.31. We find an increase in criminal behavior between ages 15 and 18 (Figure E.33), although the pre-trends are not super clean. However, we find no evidence that children's college enrolment (Figure E.32).

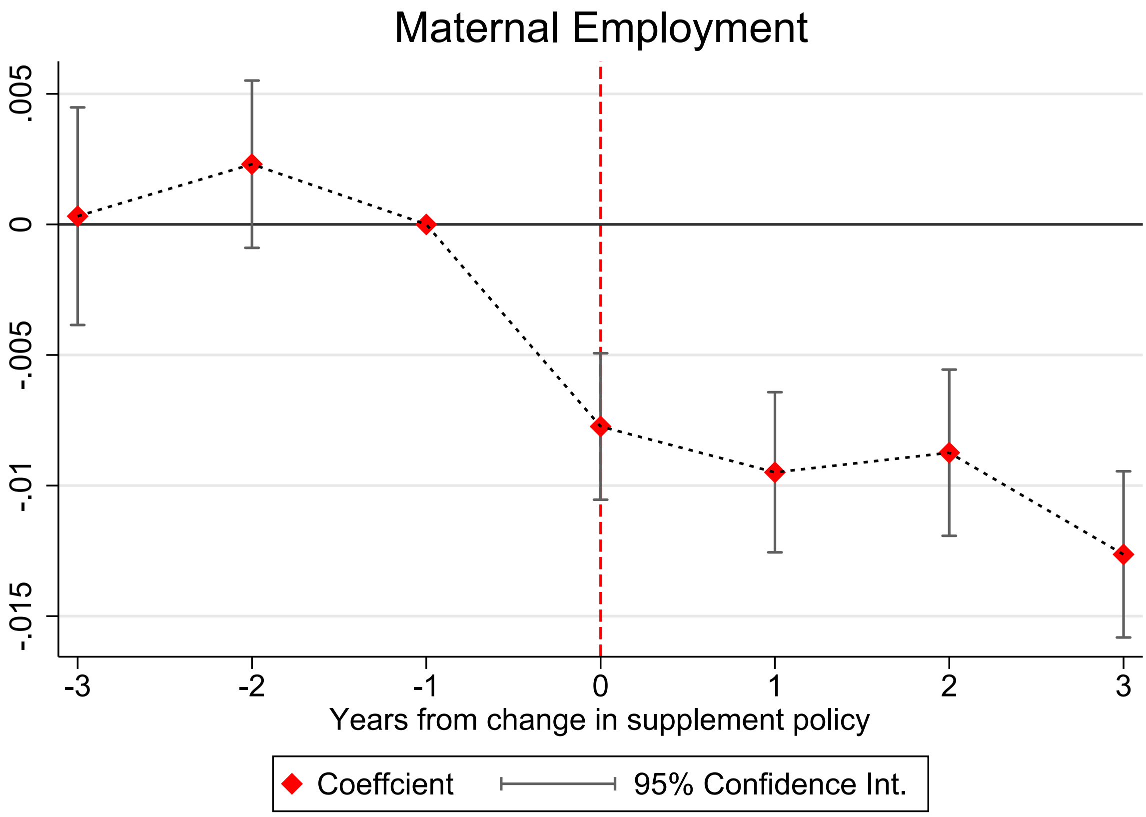
In summary, the results confirm our previous findings, indicating clear negative consequences on mother's labor market outcomes. For children the results show that they do worse in early childhood cognition tests, that they enrol less often to academic high school track and that we observe more youth crimes as a response to higher HCA in the form of municipal supplements when they were one year old. Thus, our conclusion is that according to this analysis the fact that sometimes municipal supplements change close to each other does not create significant bias in our main estimations.

Figure E.25: Balanced DiD: Amount of home care allowance



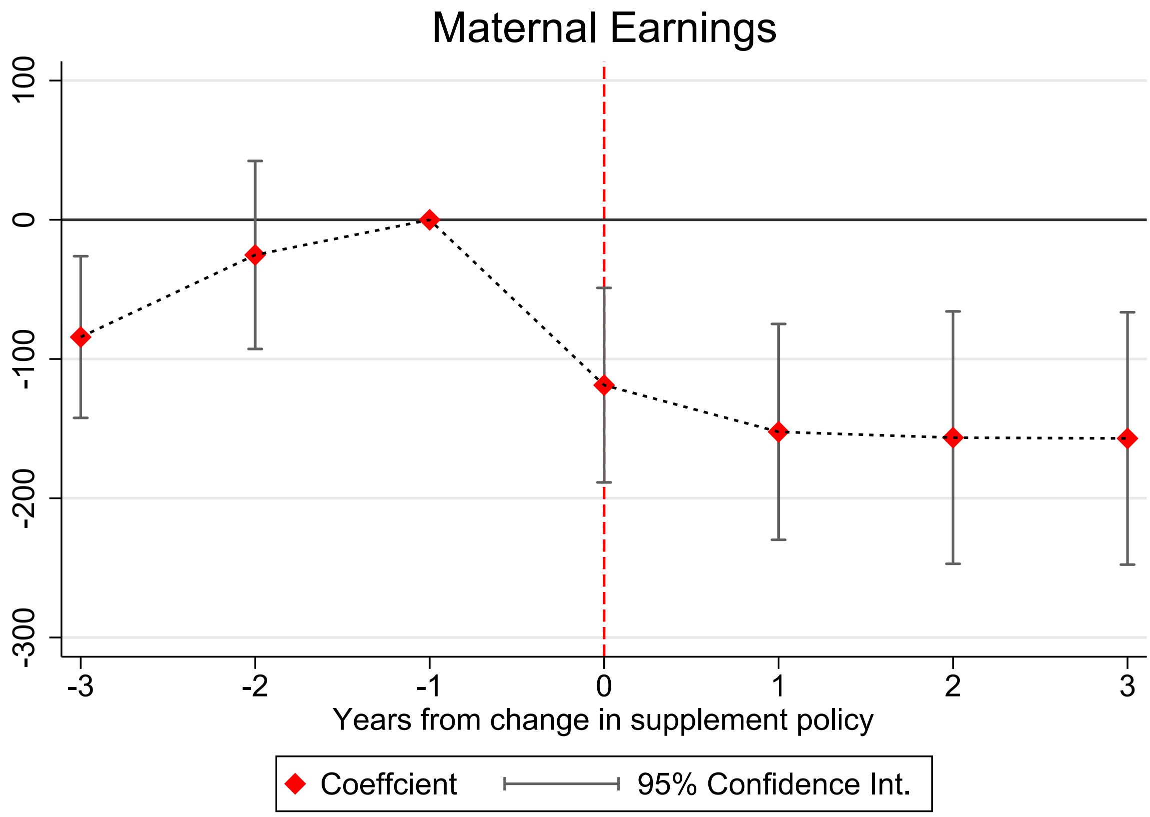

*Note*: The graph plots coefficients and confidence intervals that are estimates of 100 euros change in supplement amount occurring in year 0 as shown in equation (4). The specification controls for common year effects and municipality fixed effects, mother's age, child age in months. The explanatory variable is amount of supplement change in 100 Euros.

Figure E.26: Balanced DiD: Maternal Employment



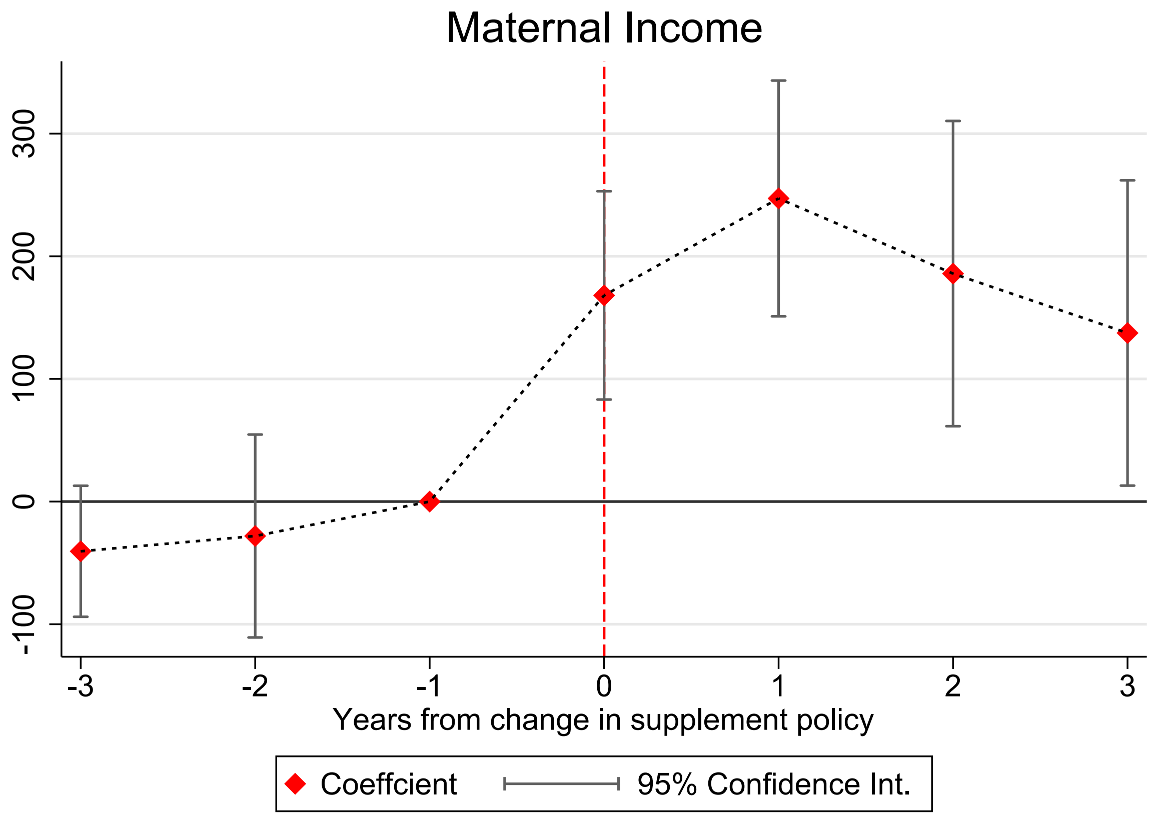

*Note*: The graph plots coefficients and confidence intervals that are estimates of 100 euros change in supplement amount occurring in year 0 as shown in equation (4). The specification controls for common year effects and municipality fixed effects, mother's age, child age in months. The explanatory variable is amount of supplement change in 100 Euros.

Figure E.27: Balanced DiD: Maternal Earnings



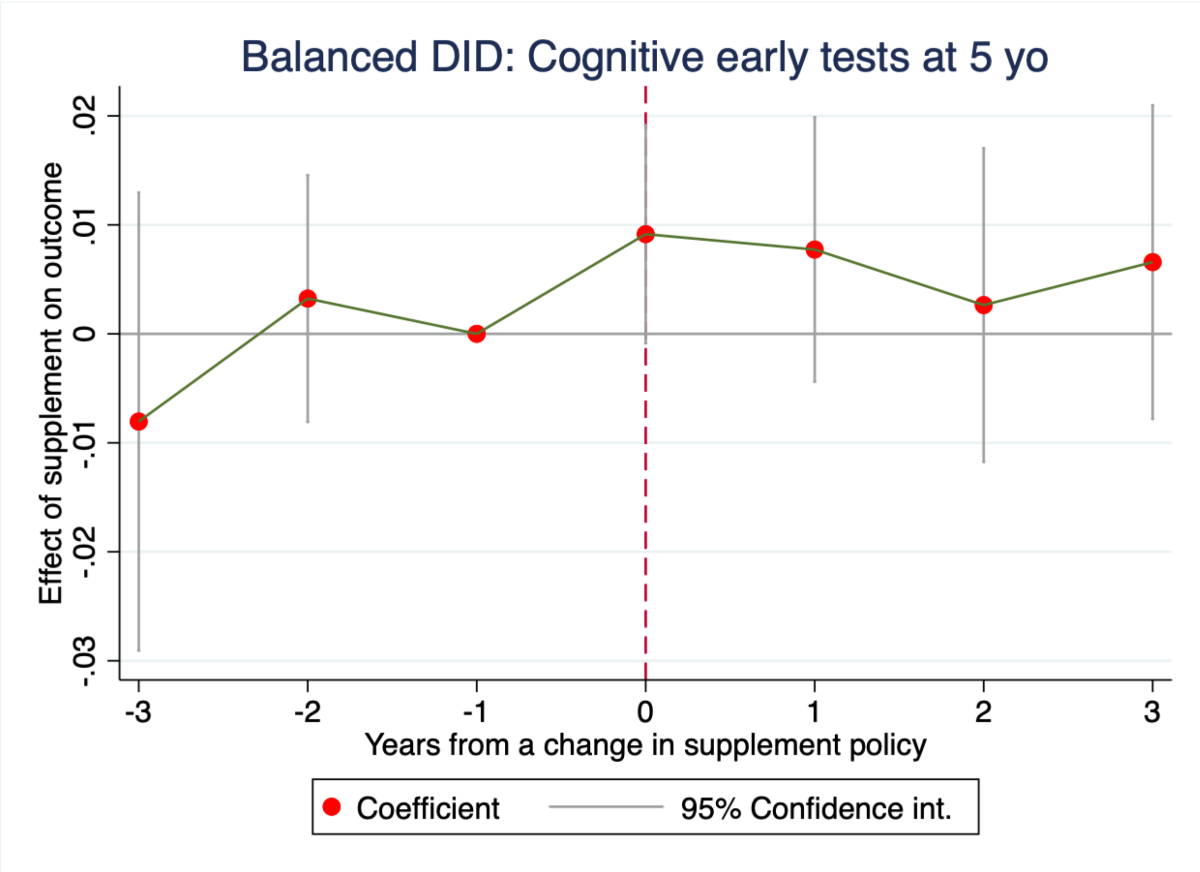

*Note*: The graph plots coefficients and confidence intervals that are estimates of 100 euros change in supplement amount occurring in year 0 as shown in equation (4). The specification controls for common year effects and municipality fixed effects, mother's age, child age in months. The explanatory variable is amount of supplement change in 100 Euros.

Figure E.28: Balanced DiD: Income



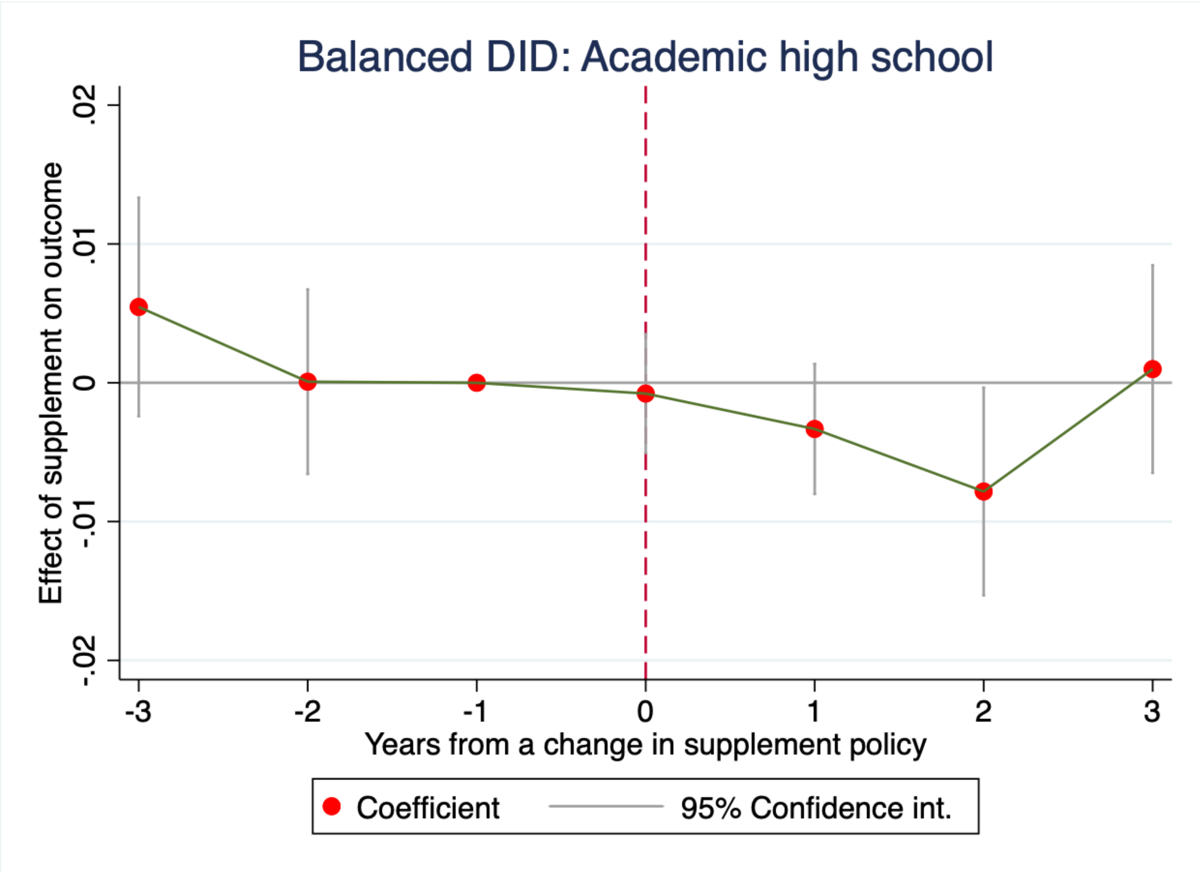

*Note*: The graph plots coefficients and confidence intervals that are estimates of 100 euros change in supplement amount occurring in year 0 as shown in equation (4). The specification controls for common year effects and municipality fixed effects, mother's age, child age in months. The explanatory variable is amount of supplement change in 100 Euros.

Figure E.29: Balanced DiD: Failing early test



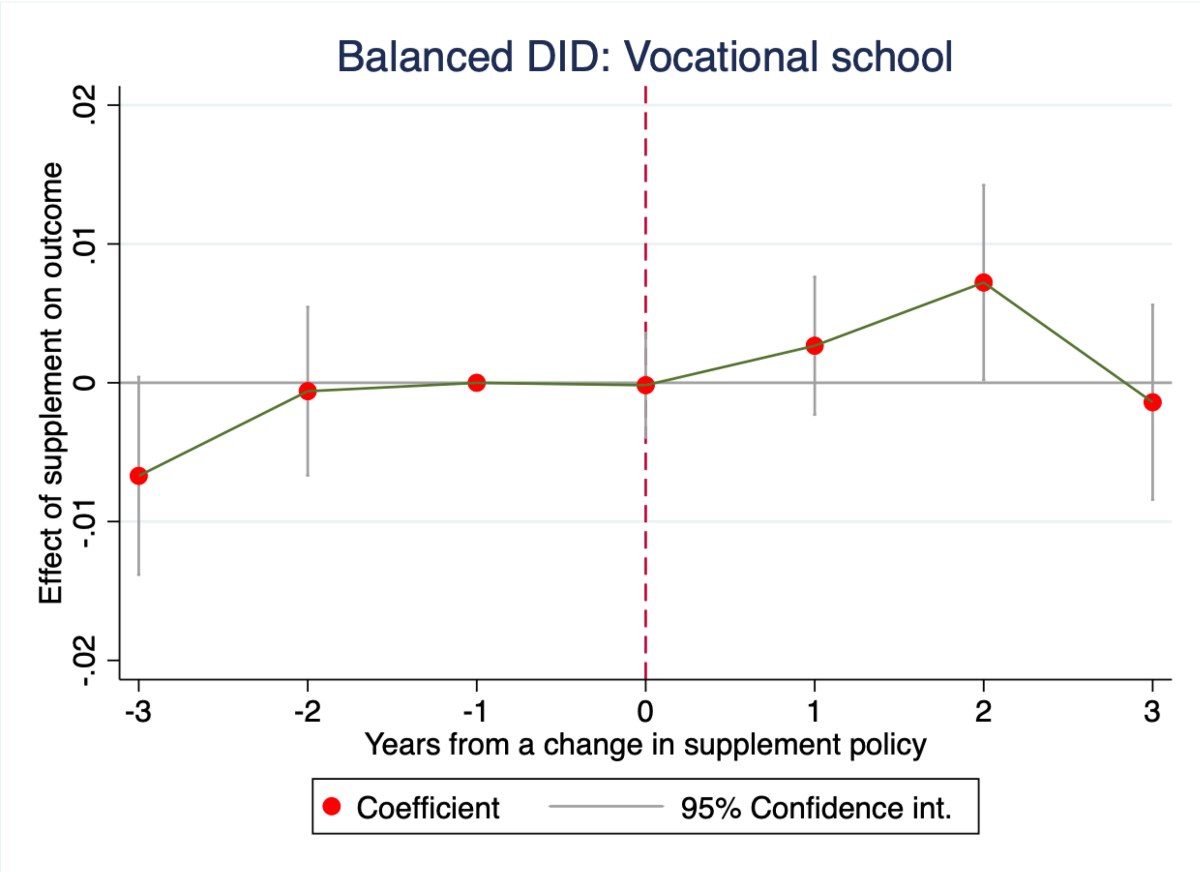

*Note*: The graph plots coefficients and confidence intervals that are estimates of 100 euros change in supplement amount occurring in year 0 as shown in equation (4). Outcome variable gets value one if child fails the cognition test at age 5. The specification controls for common year effects and municipality fixed effects, mother's age, child age in months. The explanatory variable is amount of supplement change in 100 Euros.

Figure E.30: Balanced DiD: Academic high school track



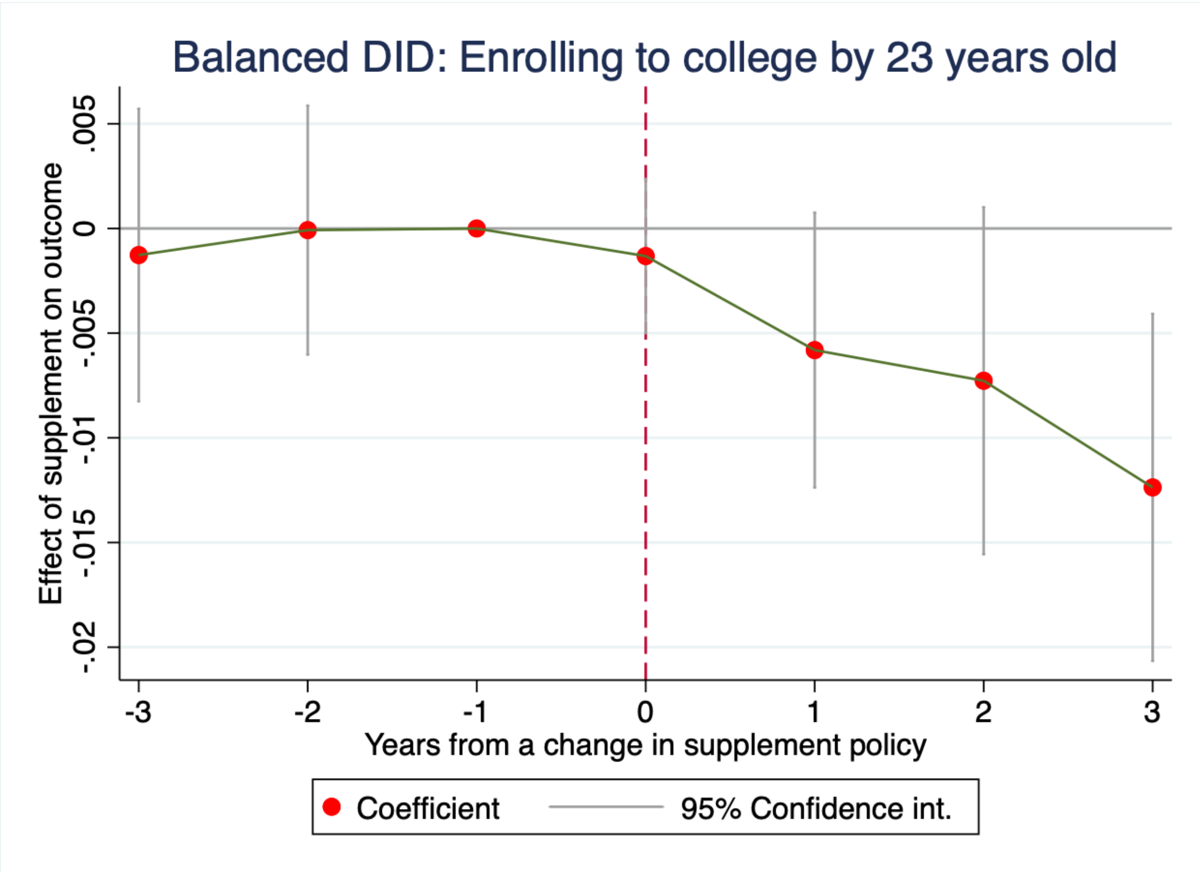

*Note*: The graph plots coefficients and confidence intervals that are estimates of 100 euros change in supplement amount occurring in year 0 as shown in equation (4). The specification controls for common year effects and municipality fixed effects, mother's age, child age in months. The explanatory variable is amount of supplement change in 100 Euros.

Figure E.31: Balanced DiD: Vocational track



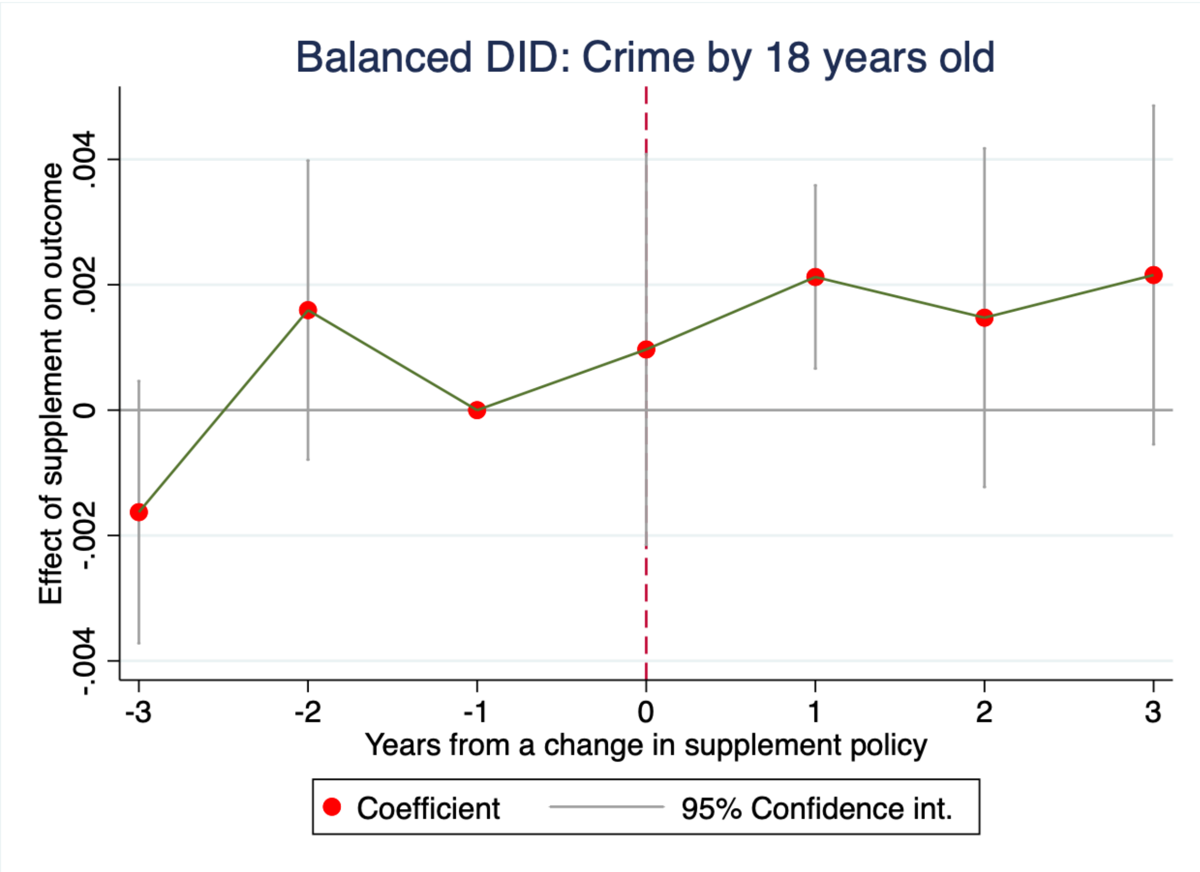

*Note*: The graph plots coefficients and confidence intervals that are estimates of 100 euros change in supplement amount occurring in year 0 as shown in equation (4). The specification controls for common year effects and municipality fixed effects, mother's age, child age in months. The explanatory variable is amount of supplement change in 100 Euros.

Figure E.32: Balanced DiD: College enrolment




*Note*: The graph plots coefficients and confidence intervals that are estimates of 100 euros change in supplement amount occurring in year 0 as shown in equation (4). Outcome variable gets value one if an individual has enrolled to college by age 21. The specification controls for common year effects and municipality fixed effects, mother's age, child age in months. The explanatory variable is amount of supplement change in 100 Euros.

Figure E.33: Balanced DiD: Any crime age 15-18




*Note*: The graph plots coefficients and confidence intervals that are estimates of 100 euros change in supplement amount occuring in year 0 as shown in equation (4). The outcome variable gets value one if the individual committed any crime between ages 15-18 by district court records.The specification controls for common year effects and municipality fixed effects, mother's age, child age in months. The explanatory variable is amount of supplement change in 100 Euros.

This section describes the Callaway and Sant'Anna (2021) estimator, which accounts for both the treatment-weights and consequent changes in supplements problems. This estimator relies on distinct events, so to translate to our context we use as the key event the first occurrence of at least 40 euros per month increase in supplements, which mostly means the year when the supplement was first implemented in the municipality. As our main analysis uses also subsequent changes in supplements in the same municipality as well as decreases in supplements, this specification is more restrictive. The estimation procedure compares all municipalities that had an increase in supplements in certain year to municipalities that are never treated over the same years, that is, it creates for each increase in supplements a differences-in-differences analysis with one treatment and one control group. The estimation procedure also treats treatment weights such that the heterogeneous weights are controlled for.

We show results from the above procedure that yield ATT estimates in the average pre- and post-estimates, as well as the event-study coefficients. Table E.32 shows the average pre- and post-treatment estimates for mother outcomes. We observe that the before-treatment effects are close to zero and not statistically significant, except for small negative coefficient for HCA significant only at 10% level. The post-coefficients are highly statistically significant and similar to our main DiD estimates. The effect on HCA usage and employment are larger in absolute value than the main estimates. Table E.34 shows the event-study coefficients indicating that the effects arise at the time of change in supplements and that the effects persist for the three examined years after the increase in supplements.

Table E.33 shows the results for child outcomes. The result for the early cognitive test in column (1) is similar to the main estimation, but not statistically significant. The result for enrolling to academic high school in column (2) and enrolling to vocation secondary education in column (3) are in line with the main estimation results and statistically significant at 5% level. The result for college enrolment is not very robust here in columns (4) and (5), whereas the result for youth crime in column (6) is similar to main estimation results and statistically significant. The event-study coefficients in Table E.35 show that whenever we observe significant estimates, the effects seem quite stable after the treatment. In summary, we observe similar results for children from this exercise as our main estimation results, but that some of the outcomes are not quite as statistically significant as in the main analysis. However, this could also be due to restrictions needed to impose in order to perform this analysis, and not necessarily evidence of two-way fixed effects problems.

Table E.32: CSDID Parent outcomes +40 euros change in supplement as treatment

| VARs | (1)<br>HCA | (2)<br>Employment | (3)<br>Earnings | (4)<br>Income |
|---|---|---|---|---|
| Pre average | -36.11* | 0.000409 | 41.72 | -3.333 |
| | (19.46) | (0.00162) | (34.16) | (40.82) |
| Post average | 601.4*** | -0.0216*** | -274.1*** | 331.1*** |
| | (60.25) | (0.00400) | (66.20) | (85.93) |

*Note*: Regressions implementing Callaway and Sant'Anna (2021) estimators. Treatment is the first occurrence of at least + 40 euros increase in municipal supplements. Row Pre average shows the sum of coefficients prior to the change. Post average shows sum of coefficients for five years after the change and is the ATT estimate. Column (1) is for maternal employment, column (2) for maternal earnings, column (3) for maternal income including benefits, column (4) for spouse's earnings and column (5) for family income (aggregating both spouses and including benefits).

Table E.33: CSDID Child outcomes +40 change in supplement as treatment

| VARs | (1)<br>Cognitive | (2)<br>High school | (3)<br>Vocational | (4)<br>College by 23 | (5)<br>Convict by 18 |
|---|---|---|---|---|---|
| Pre avrg | 0.00600 | 0.00191 | -0.00175 | 0.0150*** | -0.00264* |
| | (0.00771) | (0.00365) | (0.00352) | (0.00512) | (0.00143) |
| Post avrg | 0.0176 | -0.00602** | 0.00662** | -0.00533 | 0.00377** |
| | (0.0150) | (0.00299) | (0.00265) | (0.00521) | (0.00154) |

*Note*: Regressions implementing Callaway and Sant'Anna (2021) estimators. Treatment is the first occurrence of at least + 40 euros increase in municipal supplements. Row Pre average shows the sum of coefficients prior to the change. Post average shows sum of coefficients for five years after the change and is the ATT estimate. Column (1) is for early cognitive test, column (2) for enrolling to vocational high school, column (3) for enrolling to academic high school, column (4) for enrolling to college, column (5) for enrolling to college by age 23, column (6) for being sentenced or fined for any crime from 15 to 20 years old and column (7) for appearing in any police records from 15 to 20 years old.

Table E.34: CSDID Parent outcomes +40 change in supplement as treatment, event study coefficients

| VARs | (1) HCA | (2) Employment | (3) Earnings | (4) Income |
|---|---|---|---|---|
| Tm3 | -24.31 | 0.00548 | 31.53 | 49.12 |
| | (55.79) | (0.00391) | (88.20) | (90.33) |
| Tm2 | -98.10* | -0.00151 | 29.41 | -81.94 |
| | (53.11) | (0.00406) | (82.11) | (81.24) |
| Tm1 | 14.08 | -0.00275 | 64.23 | 22.81 |
| | (37.98) | (0.00480) | (80.55) | (72.76) |
| Tp0 | 509.9*** | -0.0167*** | -232.8*** | 262.6*** |
| | (56.14) | (0.00341) | (79.21) | (72.26) |
| Tp1 | 692.5*** | -0.0201*** | -334.5*** | 365.0*** |
| | (82.17) | (0.00505) | (110.4) | (119.3) |
| Tp2 | 620.1*** | -0.0217*** | -239.2*** | 393.6*** |
| | (64.90) | (0.00435) | (78.02) | (110.8) |
| Tp3 | 583.2*** | -0.0280*** | -289.8*** | 303.2** |
| | (75.98) | (0.00633) | (104.6) | (139.8) |

*Note*: Regressions implementing Callaway and Sant'Anna (2021) estimators. Treatment is the first occurrence of at least + 40 euros increase in municipal supplements. Rows show the event study coefficients with Tp0 being the first treatment. Column (1) is for maternal employment, column (2) for maternal earnings, column (3) for maternal income including benefits, column (4) for spouse's earnings and column (5) for family income (aggregating both spouses and including benefits).

Table E.35: CSDID Child outcomes +40 change in supplement as treatment, event study coefficients

| VARs | (1)<br>Cognitive | (2)<br>High school | (3)<br>Vocational | (4)<br>College by 23 | (5)<br>Convict by 18 |
|---|---|---|---|---|---|
| Tm3 | 0.0382 | 0.00929 | -0.00955 | 0.0398*** | -0.0106*** |
| | (0.0255) | (0.00895) | (0.00814) | (0.0111) | (0.00332) |
| Tm2 | -0.00949 | 0.00192 | -0.000784 | 0.0152** | 0.00360* |
| | (0.0161) | (0.00569) | (0.00561) | (0.00737) | (0.00211) |
| Tm1 | -0.0107 | -0.00548 | 0.00508 | -0.00990 | -0.000967 |
| | (0.0114) | (0.00539) | (0.00585) | (0.00679) | (0.00167) |
| Tp0 | 0.0226** | -0.000762 | 0.000422 | 0.00514 | 0.00339* |
| | (0.0115) | (0.00351) | (0.00342) | (0.00326) | (0.00194) |
| Tp1 | 0.0153 | -0.00650 | 0.00714* | 0.00714 | 0.00280 |
| | (0.0131) | (0.00407) | (0.00371) | (0.00667) | (0.00211) |
| Tp2 | 0.0194 | -0.00940** | 0.0101** | -0.00599 | 0.00411** |
| | (0.0201) | (0.00435) | (0.00415) | (0.00838) | (0.00203) |
| Tp3 | 0.0132 | -0.00741 | 0.00884* | -0.0276*** | 0.00477* |
| | (0.0229) | (0.00475) | (0.00453) | (0.00599) | (0.00246) |

*Note*: Regressions implementing Callaway and Sant'Anna (2021) estimators. Treatment is the first occurrence of at least + 40 euros increase in municipal supplements. Rows show the event study coefficients with Tp0 being the first treatment. Column (1) is for early cognitive test, column (2) for enrolling to vocational high school, column (3) for enrolling to academic high school, column (4) for enrolling to college, column (5) for enrolling to college by age 23, column (6) for being sentenced or fined for any crime from 15 to 20 years old and column (7) for appearing in any police records from 15 to 20 years old.