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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 to show that the Finnish Home Care Allowance (HCA) decreases maternal employment in both the short and long term. The effects are large enough for the existence of home care benefit system to explain the higher short-term child penalty in Finland than comparable nations. Home care benefits also negatively affect the early childhood cognitive test results of children, decrease the likelihood of choosing academic high school, and increase youth crimes. We confirm that the mechanism of action is changing work/home care arrangements by studying a day care fee reform that had the opposite effect of raising incentives to work – with corresponding opposite effects on mothers and children compared to HCA. Our findings suggest that shifting child care from the home to the market increases labor force participation and improves child outcomes.

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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 A.11 shows the share of mothers with children under age 6 who are working across the OECD; the share varies from 27% (Turkey) to 80% (Netherlands), with a weighted mean of 62%, and only six 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 our suite of public policies has tilted too much in favor of market work for mothers relative to staying home to raise their children.

[2]Historical data for the US: www.dol.gov/agencies/wb/data. The 2020 data for Europe: ec.europa.eu/eurostat/web/microdata/.

[3]NCES education statistics: nces.ed.gov/programs/digest/d18/tables/

[4]CLASP report: www.clasp.org/sites/default/files/publications/ and statistics from the IRS: www.irs.gov/statistics/.

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

The existing economics literature has largely focused on two policies that change the relative return to work and staying home to care for small children. The first is 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 parental leave policies after childbirth; studies of these policies have also found mixed effects on child outcomes.

This literature has faced three limitations. First, we still know relatively little about the effects of programs that subsidize mothers to stay home, beyond paid leave programs that generally cover only the first year after birth. Second, most previous studies focus either on the effect of family policies 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 formal child care, which is almost exclusively publicly-financed and of relatively high-quality in international comparison. The HCA 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 A.9). We are able to utilize over time and across regions varying supplements to HCA that provide a credibly plausible causal identification.

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 two longer-term child outcomes: enrolling in academic versus vocational high school track; and youth crimes. All these measures are linked with Finnish registers containing full population 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 for nearby Denmark, which are around 30% (in the year following child birth) (Kleven et al. 2019b,a). 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). 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. Our results therefore suggest that lower maternal work and more child care at home lead to worse long and short run child outcomes.

We also investigate whether the effect is heterogeneous by parental background. We split the estimation sample by whether or not mother has attained any college (higher education), and whether or not family earnings is above median prior to child birth. Intriguingly, we do not find any large heterogeneity in maternal employment or in the impact of HCA on children.

Of course, this conclusion may reflect incentives particular to this program. To confirm the generality of the mechanism of action, we turn 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 increase the incentives for parents to be employed and not stay at home taking care of their children. We use the variation in reduced cay care fees to show that reduced day care fees have a result in the opposite direction of 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.

Overall, our findings indicate that subsidizing home care leads to worse maternal labor market outcomes in the short and longer term. We also find negative effects on child cognitive outcomes in the short term, and corresponding indicators of worse long

term outcomes in terms of educational attainment and crime. These negative effects may arise through either a shift in the locus of care from relatively high-quality publicly-provided market to home, or through reduced labor market attachment of mothers in the long run (and associated reduced income).

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 results for 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) no effect 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) find 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 the 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 in the near term, although no previous studies have studied longer run impacts on maternal labor supply.

The Finnish home care subsidy have been previously studied by one of the authors in the current study: Kosonen (2014) show that higher supplements reduce maternal labor supply in the short run. The current study utilizes the same data on municipal supplements, and extends it from 2005 to 2014, while considering a much 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 the 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 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 that it improved children's health outcomes.

An even 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]

[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

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, Sweden with a smaller subsidy and Norway with a subsidy that one can use for taking care of children not in subsidized public care. Bettinger et al. (2014) examine the Cash-for-care program in Norway 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; they find that Cash-for care 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 German home care subsidy for which parents of one and two years old children are eligible. They find that an 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 German state in the West.

Our study makes several contributions to existing literature. First, we analyze unique and long-lasting policy that subsidizes mothers to stay at home for considerably longer than in other nations. The home care allowance has a long history in Finland; 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 A.11. Moreover, the main alternatives for most mothers are to take care their children at home themselves or place their children in relatively high-quality public day care, with unofficial private day care significantly less

background benefit from early child care.

common than in other settings studied in the literature. The consequences of this policy on both mother's and children can thus differ from studies that are focusing on policies with lower takeup, shorter duration and a less well-defined counterfactual.

Second, our paper studies the effects of the home care allowance on a rich set of outcomes for both mothers and children. Importantly, we are able to follow mother's careers several years after child birth, allowing us to investigate 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 the variation in the municipality-specific supplements to identify the causal effect of the monetary variation of HCA on mothers' and 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 about 200 euros per youngest child per month plus a sibling extra of 50 euros per month.

Table 1 describes the supplement data by municipality/year, 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" is an eligibility criteria that requires parent to have been in work prior to child birth in order to be eligible for the supplement – it is 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 fairly 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 child, and teachers are generally required to have college education. Day care centers also provide pre-school education for 6 years olds. 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.

When one year old children enter public or private day care center, they are in a small group of their age-peers taken care of by at least three adults. Thus, they are likely to learn how to socially interact with their peers. There is also a pedagogical curriculum that the day care centers follow. Day care centers also provide basic care and meals for children.

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 three

decades, from 1988 to 2019. The rich ecosystem of data available in Finland allows us to undertake a wide-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 population data (FOLK), which are linked between employers and employees and provide 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, firm and work-site, 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 administrative data 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 and it's subcomponents at the annual level by each individual.

The information on eligibility rules of 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 the Finnish Social Insurance Institution (KELA). This information was updated in the second 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 (e.g. age threshold).

We also have 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 individual-level characteristics 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 and the 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 the home care allowance (i.e. under 36 months). In the empirical specification we include controls for child's age in order to exploit just the variation in municipality specific rules that are not related to child characteristics.

Table 1 describes the supplement data. Figure A.12 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 and child health 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 clinics multiple times at different ages before entering school. The motivation for this program for children 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 this is the main test to assess school readiness, and as a result all children are emphatically encouraged to take this test.

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.

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).

We also use an alternative measure which combines information from all tests taken at ages 4 through 6 years old (not just the consistent set of school readiness tests). The advantage of this measure is that it allows us to increases the number of children observed,

which is necessary for our day care fee analysis.[7] The disadvantage of 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 B.14. 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 measure the 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 for 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 for whether the individual has enrolled to higher education, but use this outcome only in the Appendix, because we do not observe this outcome to all cohorts we study.

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, which is an indicator for whether you were convicted at district court at least once after

[7] In addition, in the HCA analysis, as the latest data from some municipalities is from 2014, we need to restrict the birth cohorts to those born in 2008 at 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).

age 15. We use as the upper age restriction 18 years of age that allows us to use all available cohorts.

To summarize the child outcome analyses, Figure A.13 presents a timeline to illustrate the timing of the policy intervention and the associated child outcomes that we study.

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 A.14 is the opposite of a labor force participation graph, and shows the profile of exiting from either maternity leave or HCA. These exits can happen 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. 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 figure 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 home benefits. Just prior to the child turning 36 months and HCA period ending, almost 90% of mothers have exited the HCA (presumably to work) or 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 parents' outcomes we use a differences-in-differences (DiD) strategy with continuous treatment. Specifically, we compare later outcomes of children who were one year old when their families were eligible for different amounts 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 an 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.[8] 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. We let the change in supplement amount affect outcomes in the municipality three years prior the change occuring, 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:

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

where $\Delta Supp_{mk}$ are indicators for *the change* in (in 100 euros) the supplement amount k years ago for which family i is eligible for in municipality m. μ_m is a municipality fixed-effect and θ_t is a (calendar) year fixed-effect. Our main interests lies in the coefficients β_k that shows how outcomes evaluated around the time of supplement change. The specification also controls for child age dummies ρ_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.[9] Note that the municipalities that did not have at a change in their supplement policy are in the data as 0 change for every time period, and they help us to control for common calendar year effects. We present the results from this estimation in graphs, where we scale the coefficients such that the period before the change in supplements is zero.

[8]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.

[9]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

We also estimate the following (reduced form) standard DiD specification to quantify the effects:

$$Y_{imt} = \beta Supplement_{mt} + \mathbf{X}_{it}\gamma + \alpha_m + \theta_t + \rho_a + \epsilon_{i\tau}, \tag{2}$$

where Y_{imt} is the outcome for parent or child i. $Supplement_{mt}$ is the amount of HCA supplement that a parent living in the municipality m in year t was eligible for when their youngest (living) child was between ages 12-23 at the end of the calendar year. $\mathbf{X}_{it}$ is the vector of pre-treatment covariates for mothers from period t. These include dummies for mother's age (in years), level and field of education, and number of children. α_m is municipality fixed effects, θ_t are time fixed effects, and $\epsilon_{i\tau}$ is the residual error term. The specification includes also dummies for child's age at the time of the subsidy (month dummies). The standard errors are clustered at the municipal level.

In order to quantify how the effect of home care allowance evolves over time, we estimate the following adjusted DiD specification:

$$Y_{imt\tau} = \beta_\tau Supplement_{mt} + \mathbf{X}_{it}\gamma + \alpha_m + \theta_t + \rho_a + \epsilon_{i\tau}, \tag{3}$$

where $Y_{imt\tau}$ is the outcome for parent or child i measured in period τ, which is τ years before or after the current year, the effect of which is captured by coefficient β_τ. Otherwise this equation is the same as equation 2 above.

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 1. The outcome variable is an indicator for maternal employment during the year when child is one year old, which is defined as having more annual earnings than 60% of the median earnings of all women who have children. 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; the change in

the supplement occurs at year zero.

We find no differential prior trend in employment of mothers of one year old child associated with supplements changes. 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 in the municipalities that increased their supplement amount. So supplements are clearly reducing maternal work in favor of at home care, and the effect corresponds to about 5 percent 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 HCA supplement. As figure 2 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 at individual level – e.g. that might lead maternal earnings to fall in the long run. Figure 3 shows the coefficients from separate DiD regressions of equation 3 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 municipalities 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 3 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 4 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 initially much higher 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 3. 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 large enough to explain the entire difference between the Finnish and (for example) Danish levels of short run child penalties of 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 5 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 through 2010, and age four thereafter. 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 children become more likely to fail the cognition test at age four or five when their parents were eligible for higher HCA supplements at child's age 1.

Figure 5 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. Since the latest tests are observed for some municipalities in 2014, we need to restrict this outcome to birth cohorts of 2008 or earlier. 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 6 shows the dynamic DiD for two key long term outcomes, enrolling to academic high school (instead of vocational high school or dropping out completely) at ages 15 to 17 years old, and committing a youth crime between ages 15 to 18 years old. The two outcomes occur at similar ages, although those who respond in the margin may be very different individuals. The pre-trend for high school enrollment is relatively flat, but there are some deviations from zero for youth crime, although not statistically significant. After an increase in supplement when one year old, enrolling to academic high school declines and committing a youth crime increases. 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 may be due to consequent change in supplement policies. For youth crime there is a clear upward shift at year 0; the noise in pre-trends likely reflects the much smaller incidence of this outcome, with a baseline youth crime rate of only 4%.

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 leads 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 how long the 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 at home with respect to the government rewards for doing so.

Column (2) shows that the impact of receiving a 100 euros per month supplement when the eligible child is one year old is to reduce the employment of mothers by -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 273 Euros in column (1), this suggests an almost three-quarters "crowdout" of the income benefits of HCA; that is, for every dollar of HCA received, mothers offset 72 cents through lower labor earnings. 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 the sums of columns (1) and (3) as there are other transfer programs that could be affected by staying at home instead of working, such as the housing allowance.

Table 4 shows the results for early child outcomes. The table presents the impact of the supplement on our main early outcome: failing at least one of the cognition tests. In column (1), we show that for our main test measure a 100 euro per month increase in the supplement leads to a statistically significant 1.78 percentage point increase in the odds of failing a test; the effect size represents about 7% increase from the baseline failing rate shown in the bottom row of the table. Column (2) extends the analysis to our broader

alternative measure of failing any cognitive test during a visit from age 4-6. 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 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.

Tables C.15 through C.17 report the effect of supplement when using single tests as the outcome, all of the tests from four through six years 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 the child is one year old leads to -.6 percentage point decline in the odds of enrollment in an academic high school, which is about 1 percent of the sample mean. Column (2) shows a corresponding rise in vocational schools.

The expected consequence of children not enrolling to academic high school is that they later are not enrolling to college. We indeed find a negative effect on college enrollment of 0.7 percentage points in Appendix Figure E.16 and Table E.30. However, a concern with the college outcomes is that the time span of our follow up period is too short to observe all cohorts having had time to make the college enrollment decision. In particular, we 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-attenders even if they will attend by age 23. Moreover, most of the supplement variation takes place in 1997 and thereafter. When limiting the sample to cohorts for which we observe college enrollment by age 23 or limit college enrollment to be observed

until age 21, we do not find a statistically significant effect, which may be due to too little variation in observed enrollment. Thus, we interpret the college enrollment result very cautiously.

Column (3) of Table 5 indicates that we find an increased effect on youth crime by 15-18 years old, which takes a value of one if the individual is sentenced in court. We find that each 100 euros per month of supplement leads to a rise in youth criminal sentencing 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. 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.

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 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 universally utilized with over 80% of mothers using it for at least for couple of months.

Tables D.18 through D.29 in Appendix, Section D explores the heterogeneity of our results by splitting the sample in two along two family characteristics: whether or not the mother has college or higher as the highest education attainment, and by family earnings measured the year before child birth. Overall, we find remarkably little heterogeneity. Estimated effects for employment and earnings are lower for those with lower pre-birth family income, but comparable relative to baseline values. Similarly, for child outcomes, the point estimates are larger for higher earnings/education groups, but once again baseline means are higher for these groups as well. 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 very clear heterogeneity; 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; more detail on these checks is provided in the Appendix, Section F. First, a potential problem is that the weights that our estimates put on treatments at different points in time might be heterogeneous – and even possibly negative. To investigate the treatment-weights in our two-way fixed effects analysis, we plot the distribution of implied weights for three of our analyses in Figures F.17 through F.19. 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 a staggered DiD design, which addresses concerns around subsequent treatment periods acting as controls for earlier analyses. 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 using 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, the same as in specification (1), describe the change in the municipality specific supplement in 100 euros. The method is explained in the Appendix, Section F.

The results are presented in Figures F.20 through Figure F.26. We confirm our previous findings of clear negative consequences on mother's labor market outcomes and on

child outcomes. The dynamic patterns of coefficients resemble much the dynamic DiD results.

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 an increase in supplements of at least 40 euros per month, which generally indicates the year when the supplement was first implemented in the municipality. The explanatory variable in this estimation is an event indicator marking the first occurrence of an increase of at least 40 euros in the 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 accounts for heterogeneous weights.

We present results from the above analysis in Tables F.32 through F.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 results for children are similar, although the estimate on early cognitive test is not statistically significant here. The event-study coefficients show that whenever we observe significant estimates, the effects seem quite stable after the treatment.

7 Confirming Mechanism: The Day Care Fee Reform in 1997

Our results thus far suggest that incentivizing mothers to stay at home led to mothers staying out of labor force for longer, with negative impacts on child outcomes. 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 the reform, day care fees relied on the same general structure, a step-wise system that depended on 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, of which we were able to collect day care fee schedules for 350.

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 an indicator of the 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 of residence. We take the difference of these two fees and use it as the main explanatory variable.

Figure A.15 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 HCA supplements used in the analysis above.

We analyze the impact of the above defined change in day care fees in a regression analysis of the same outcomes for mothers and children that we used for our HCA analysis. The explanatory variable of interest is the change in day care fees interacted with event-time indicators in the event-study specification or a 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 HCA supplements as well, as some of the changes in supplements took place during the years we follow in the day care fee reform. As HCA supplements affect the same behavioral margins, it is important to take their contribution into account. We estimate equation 4:

$$Y_{imt} = \theta_t + \mu_m + \sum_{d=-3}^{3} \beta_d \Delta DCF_i \times \text{eventtime}_d + \sum_{k=-3}^{3} \beta_k \Delta Supp_{mk} \ + \rho_a + \mathbf{X}_{it}\gamma + \varepsilon_{it}, \quad (4)$$

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. μ_m is a municipality fixed-effect and θ_t is a (calendar) year fixed-effect. Our main interests lies in the coefficients β_d that shows how outcomes evaluated around the time of supplement change. The specification also includes $\Delta Supp_{mk}$ that are the dynamic DiD variables for municipal HCA supplements. The specification also controls for child age dummies ρ_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 7 shows the event study resulting from estimating equation 4 for maternal employment and Figure 8 shows the event study for HCA usage. Both figures show two lines, one corresponding to the change in day care fees, that is β_d , and another corresponding to the change in supplements, that is β_k. There is a flat pre-trend and an increase in maternal employment in the group facing reduced day care fees directly when the reform took place. There is a slight pre-trend on HCA usage, but also a clear drop after the reduction in day care fees. The HCA usage in Figure 8 is indirectly related to day care participation as most children attend day care when they are not taken care of at home by a parent with HCA. This outcome is negatively associated with employment, thus this outcome serves as confirming the employment result. Moreover, the effects of HCA supplements in both figures provide a similar result 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 Δ DCF interacted

with a dummy for years after the reform in 1997. This specification also controls for municipal HCA 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 the effects of HCA supplements in the same table and 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 in column (3), as opposed to the increase in Table 3; this is consistent with mothers working more and therefore getting less HCA. Overall, however, column (4) shows that the income of the mother goes up with reducing day care fees, as the rise in labor income is larger than the decline in HCA.

For short term outcomes for children, we focus here on our alternative measure of whether the child fails any test; as we have generally only a limited number of observations for the years surrounding the 1997 reform, and especially for our base measure of school readiness tests only. Figure 9 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 HCA supplement effect is of the opposite sign, consistent with our findings above but for this earlier time period.

Figure 10 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 that the effect of reducing day care fees in the 1997 reform have an opposite effect from supplements; enrolling to academic high school increases, although the estimates decline over time.

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 academic 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) shows that we do not find a statistically significant estimate on youth crime, although the effect is in the expected and opposite direction of the supplement results in the same specification.

8 Conclusion

This paper investigates the effect of a generous child home care allowance policy on child and maternal outcomes. The Finnish home care allowance allows one of the 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. We exploit the variation in municipality specific supplements to the 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 strongly suggests that promoting home care in Finland was harmful to both mothers and 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 limits to 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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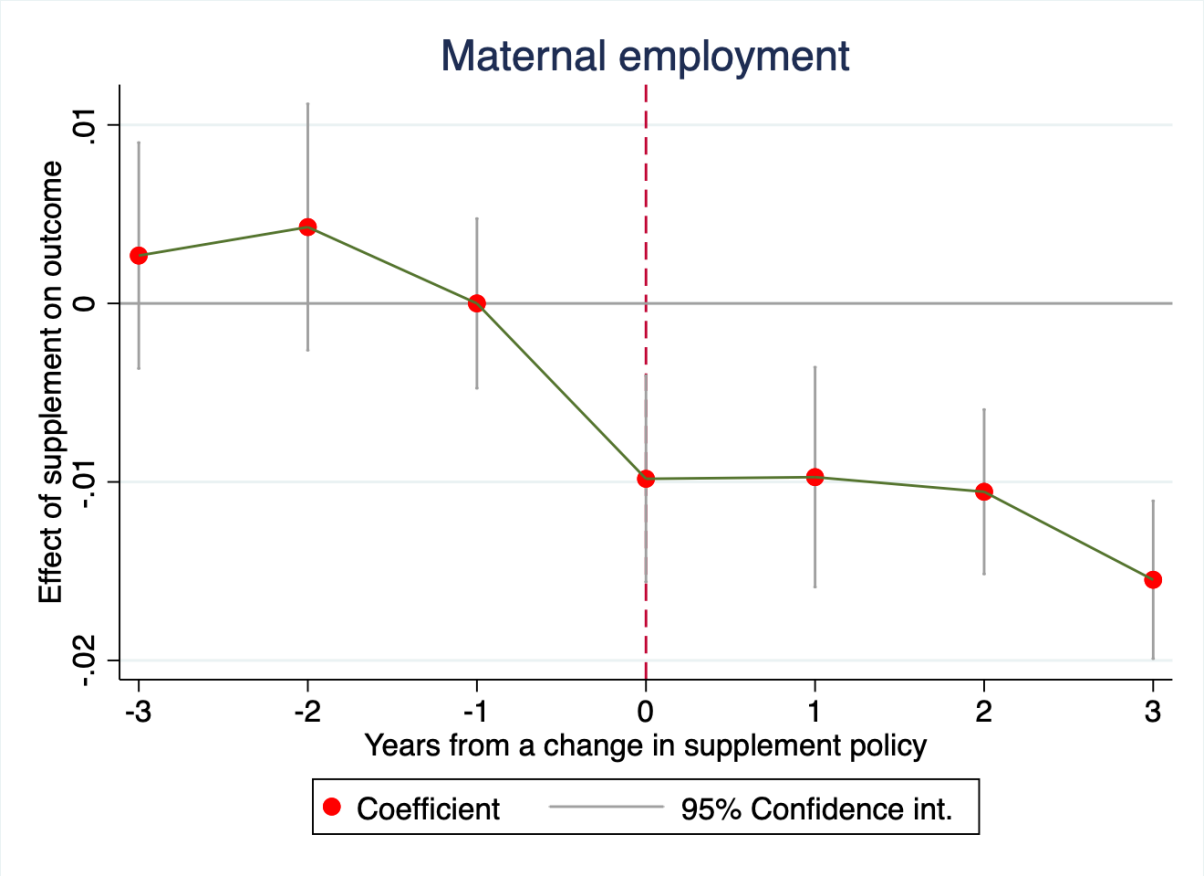
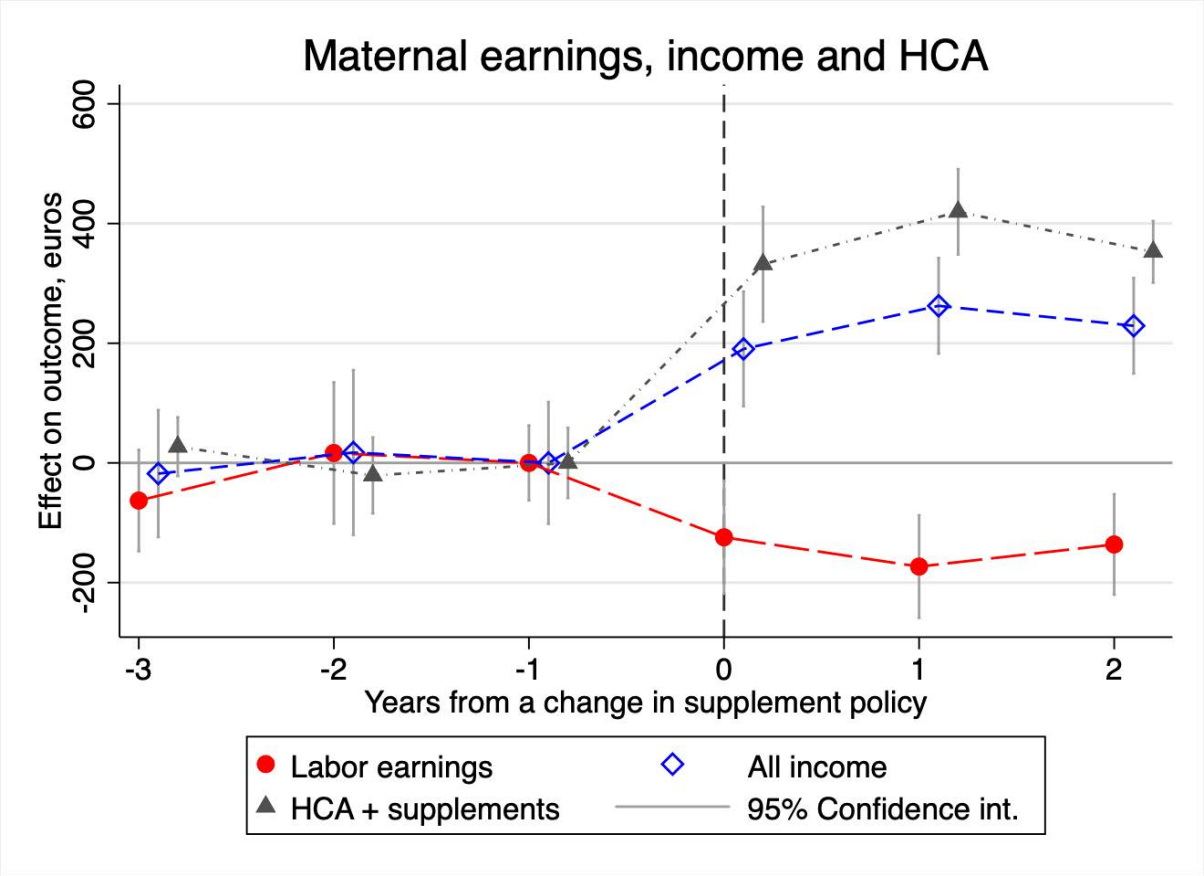
9 Figures

Figure 1: Dynamic DiD: Mother employed



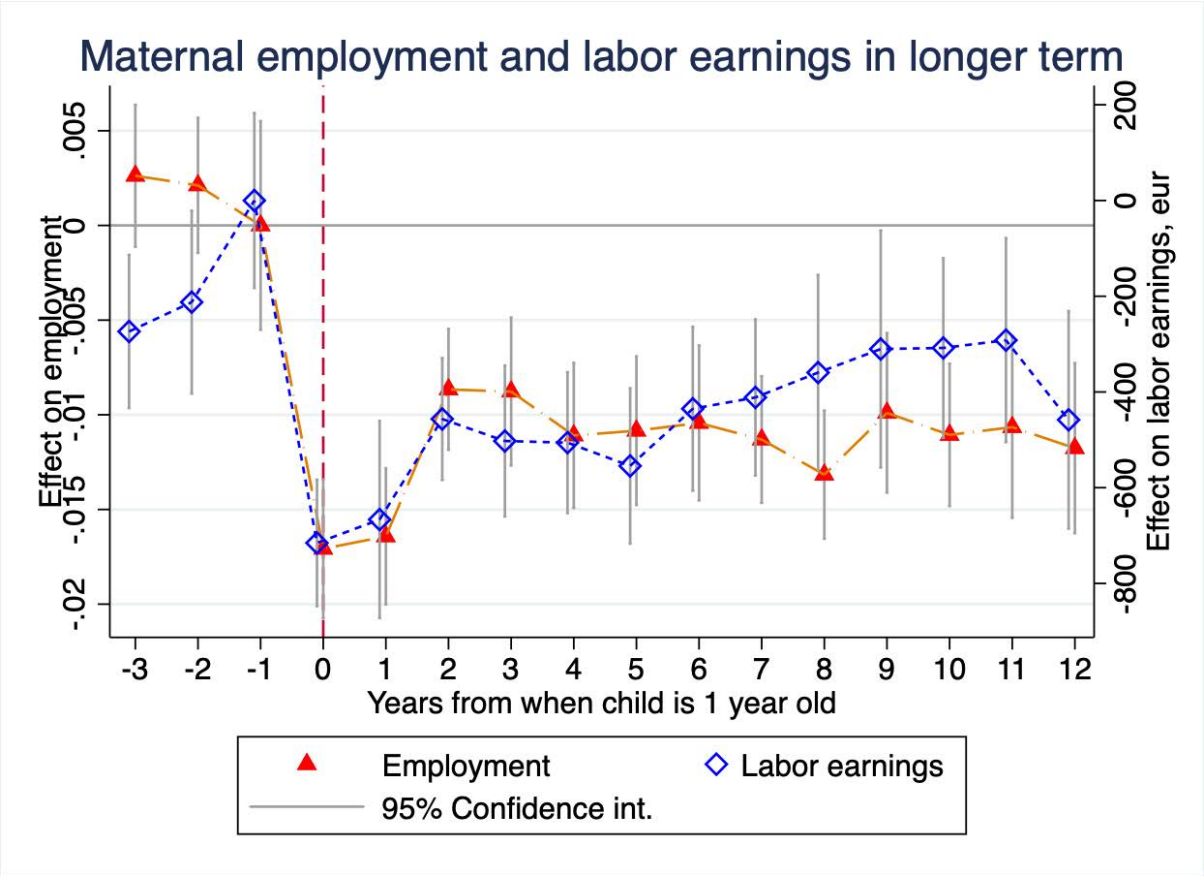
Note: The graph plots coefficients and confidence intervals that are estimates of leads and lags in years from changes in the supplement amount in 100 euros in year 0 as shown in equation (1), which are normalized to year -1 level by subtracting year -1 estimate from all other estimates. The specification 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 60% of the median earnings of all women who have children.

Figure 2: Dynamic DiD: Mother's earnings, income and HCA usage



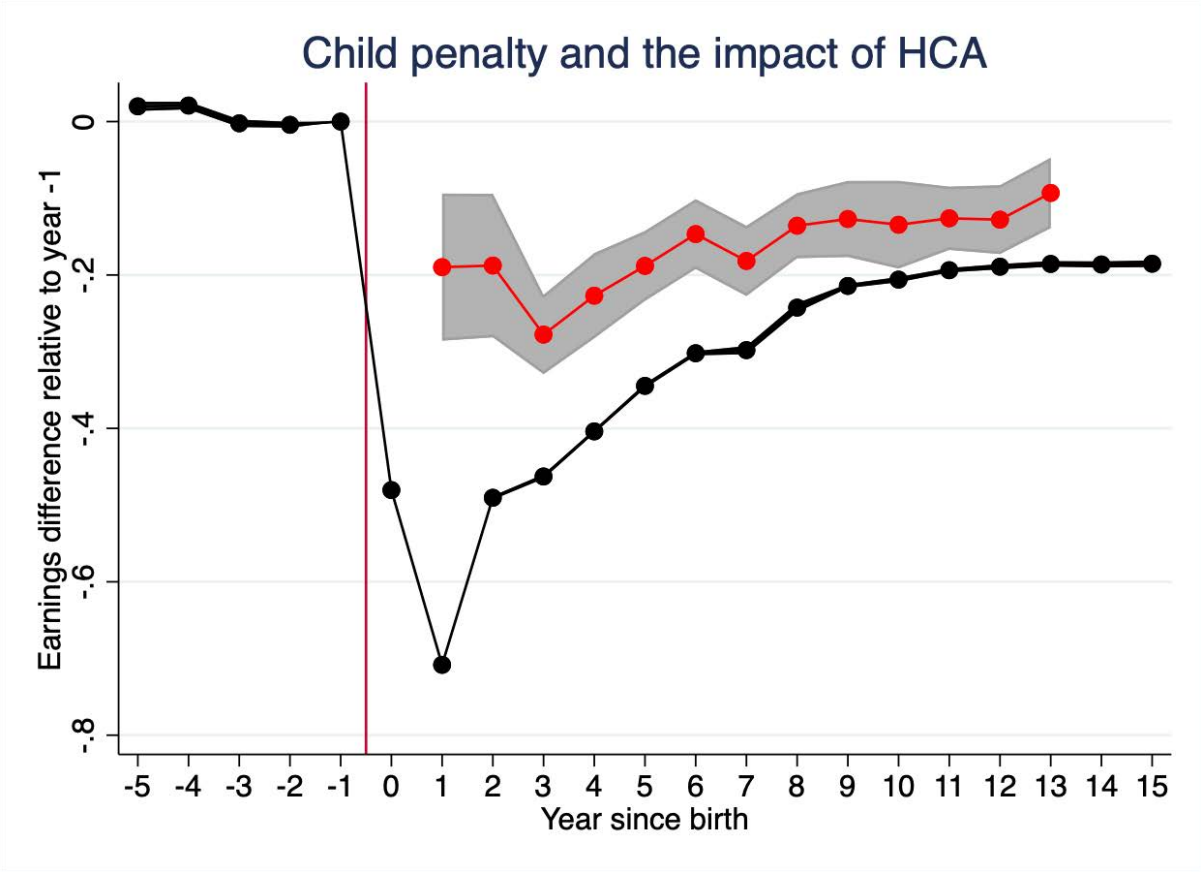
Note: The graph plots coefficients and confidence intervals that are estimates of leads and lags in years from changes in the supplement amount in 100 euros in year 0 as shown in equation (1), which are normalized to year -1 level by subtracting year -1 estimate from all other estimates. The specification 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 3: Longer term effects of supplements on mothers: labor supply and earnings



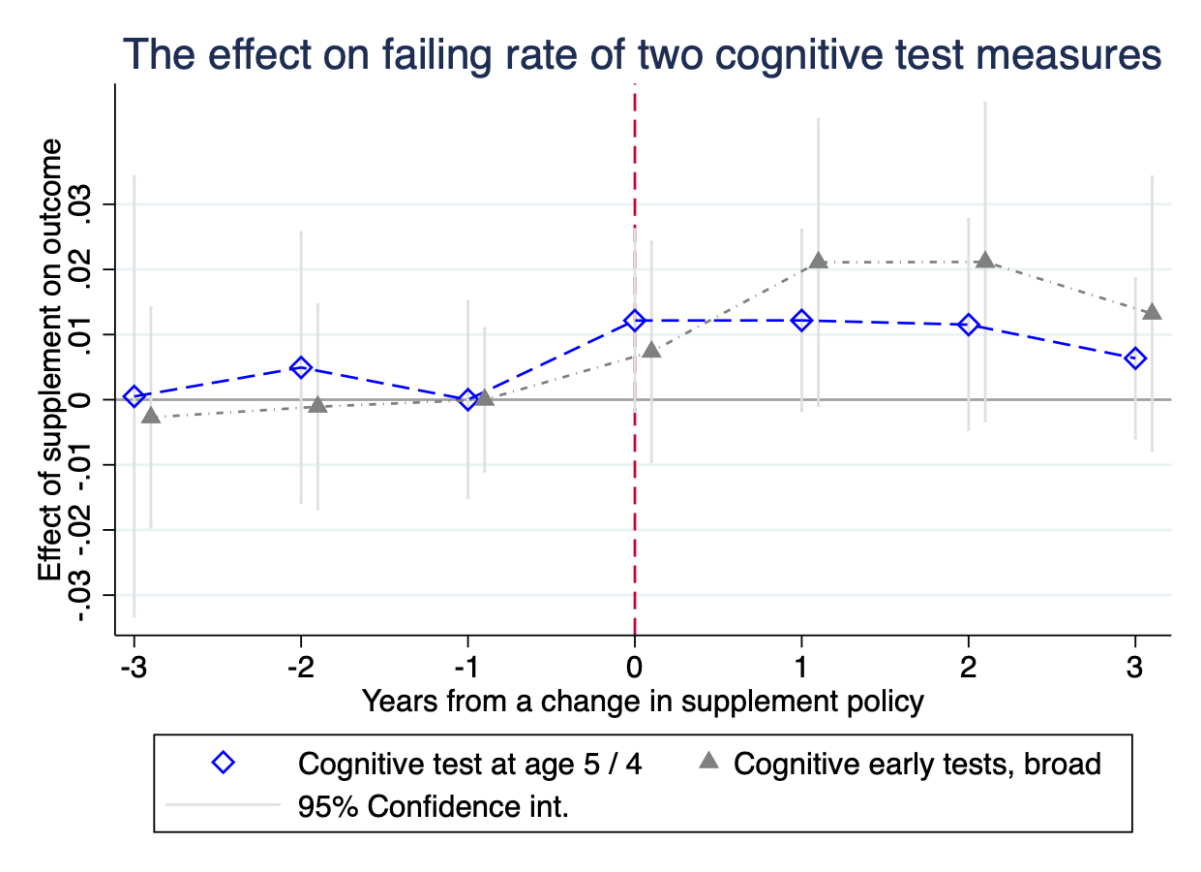
Note: The graph plots coefficients and confidence intervals that are estimates of separate DiD estimations where outcomes are maternal employment measured k years before or after the first child was born.

Figure 4: Child penalty for Finland in labor earnings and the influence of supplements



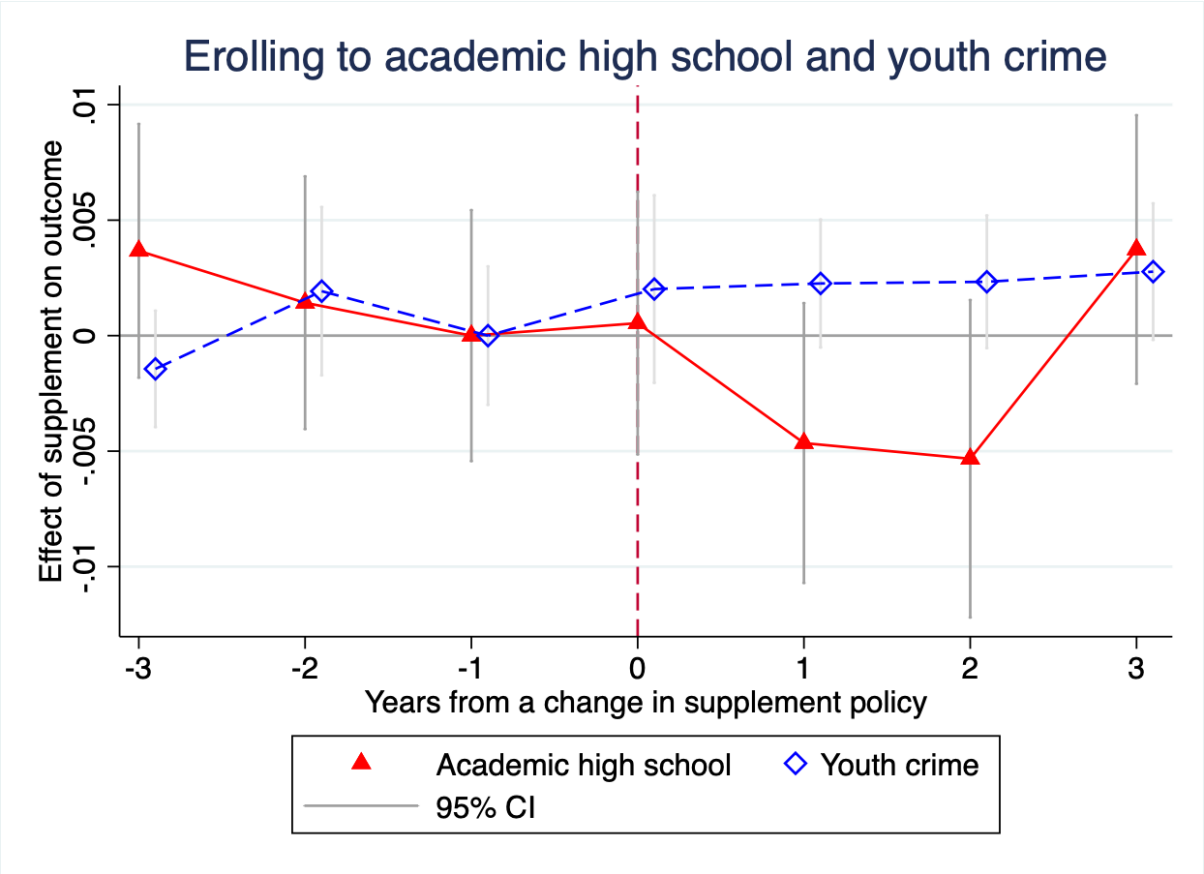
Note: The graph plots child penalty in our estimation sample, which is difference between labor earnings of mothers and fathers relative to a counterfactual earnings prediction. The graph also subtracts from this the longer term DiD estimates utilizing supplements in 100 euros per month. The graph includes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

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



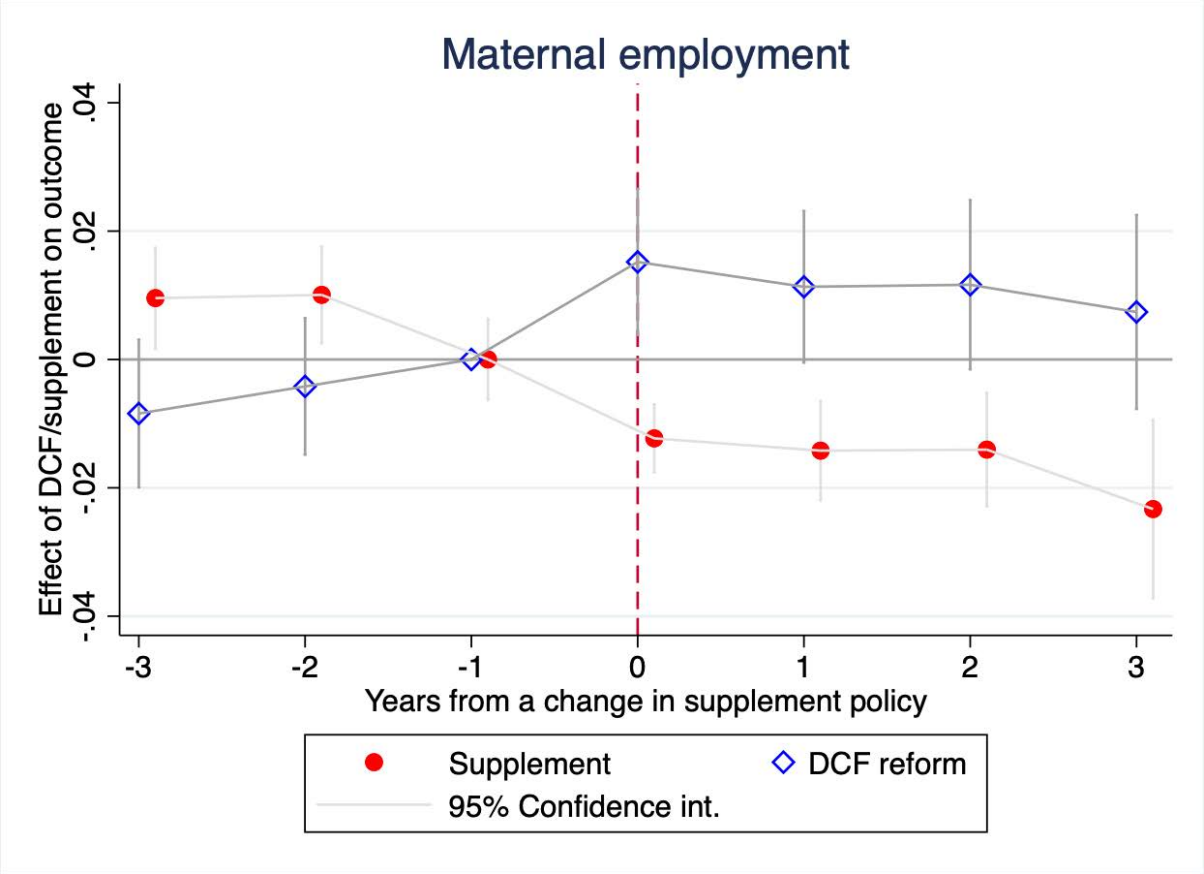
Note: The graph plots coefficients and confidence intervals that are estimates of leads and lags in years from changes in the supplement amount in 100 euros in year 0 as shown in equation (1), which are normalized to year -1 level by subtracting year -1 estimate from all other estimates. The specification 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 6: Dynamic DiD: Enrolling to academic high school and youth crime



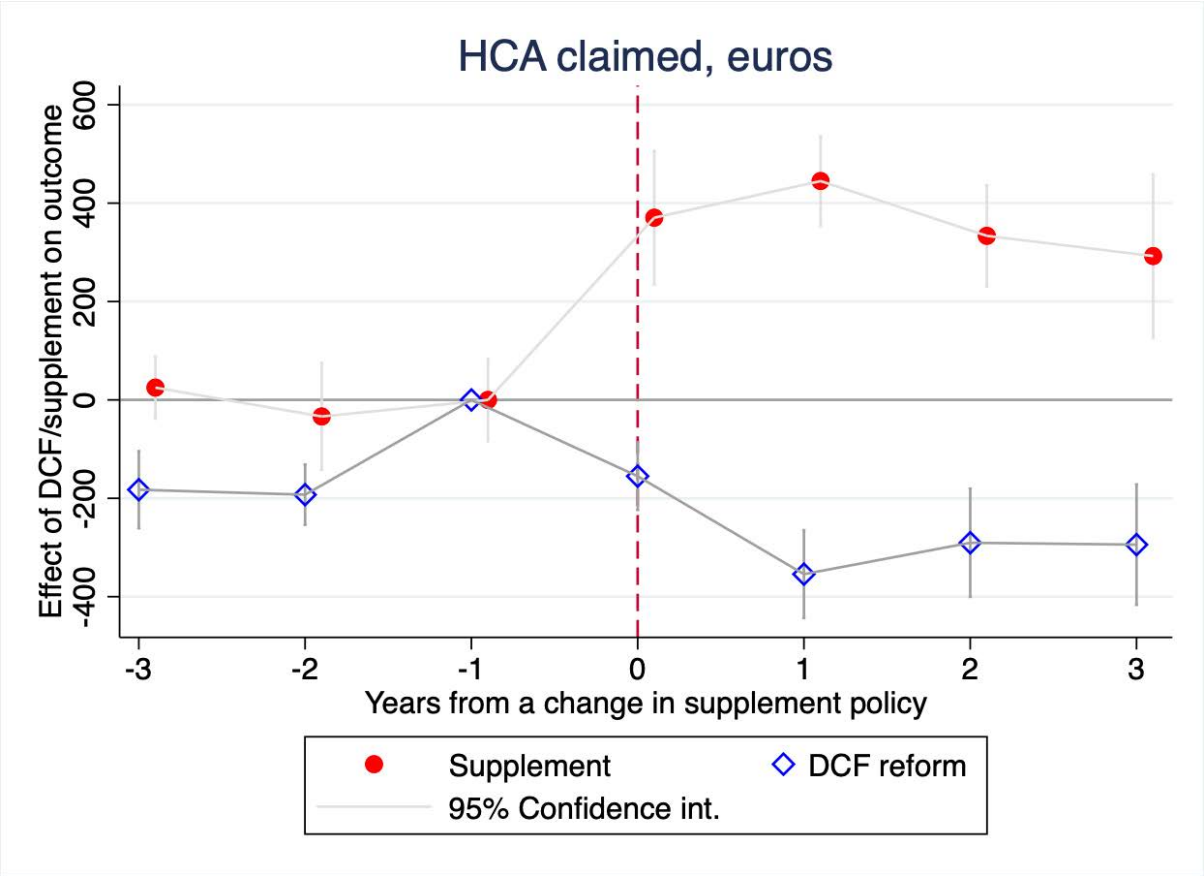
Note: The graph plots coefficients and confidence intervals that are estimates of leads and lags in years from changes in the supplement amount in 100 euros in year 0 as shown in equation (1), which are normalized to year -1 level by subtracting year -1 estimate from all other estimates. The specification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

Figure 7: Event study: The impact of day care fees on maternal employment



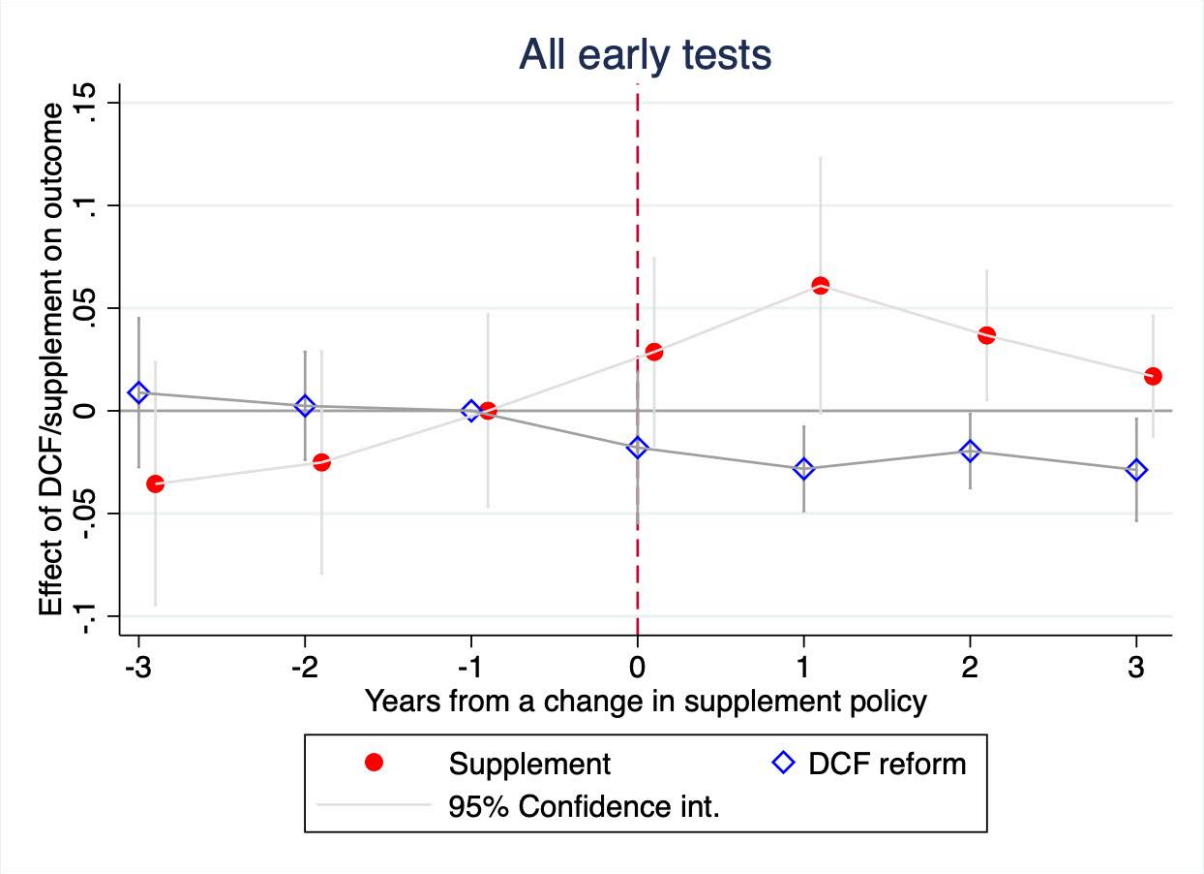
Note: The figure plots the coefficients on events of day care fee reform that reduced day care fees in 1997. Controls are reported under table 6.

Figure 8: Event study: The impact of day care fees on HCA usage



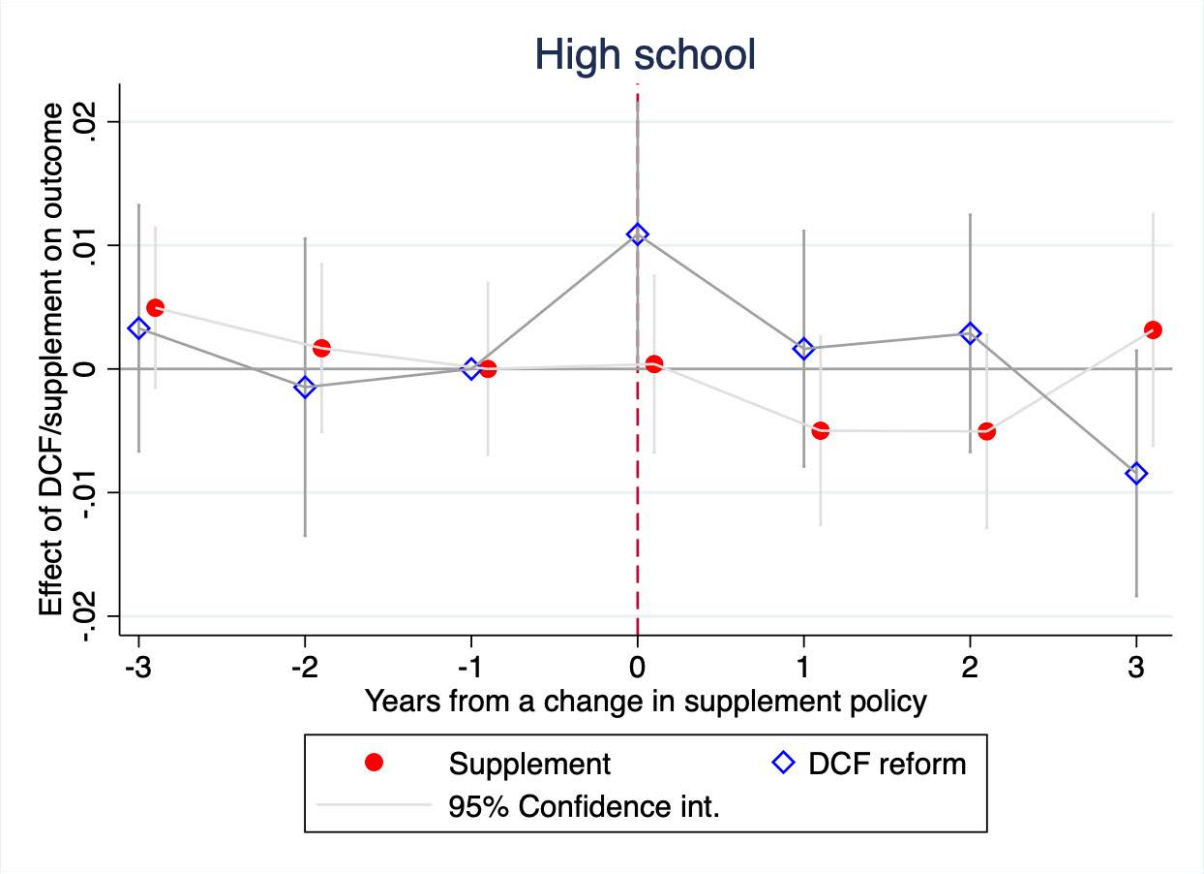
Note: The figure plots the coefficients on events of day care fee reform that reduced day care fees in 1997. Controls are reported under table 6.

Figure 9: 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 6.

Figure 10: 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 6.

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) Main outcome	(2) Broad outcome	(3)	(4)	(5)
Mean	0.25	0.28			
SD	0.43	0.45			
N	164224	292474			
Age 4 (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 (→ 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) HCA	(2) Employment	(3) Earnings	(4) 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) Cognitive	(2) Cognitive 4-6 yo tests	(3) 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 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) High school	(2) Vocational	(3) Convict by 18
Supplem.	-0.0060***	0.0055***	0.0022***
	(0.0016)	(0.0016)	(0.0006)
N	491,165	475,035	359,245
R^2	0.1405	0.1451	0.0169
out mean	0.52	0.46	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) and having been sentenced from crime committed at ages 15 to 18 years old 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 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) Employment	(2) Earnings	(3) HCA	(4) Income
Δ DCF	0.0157***	345.2***	-155.35***	343.4***
	(0.0042)	(61.91)	(50.48)	(48.33)
Supplement	-0.0126***	285.67***	298.21***	-74.11
	(0.0036)	(65.10)	(62.87)	(54.28)
N	291,394	291,394	291,394	291,394
R^2	0.1654	0.24	0.32	0.25
out mean	0.37	4838	2726.46	11637

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 HCA usage in euros per year, and column (4) for income including income transfers. 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) Any early	(2) Motor
Δ DCF	-0.0250***	-0.0216
	(0.0089)	(0.0134)
Supplement	0.0417***	-0.0182
	(0.0144)	(0.0123)
N	29,698	16,033
R^2	0.0580	0.0492
out mean	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 at least one of the early tests conducted at ages 3 through 6 in column (1), child failing an early motor skills tests in column (2). 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) High school	(2) Vocational	(3) Convict by 18
Δ DCF	0.0055*	-0.0058*	-0.0012
	(0.0030)	(0.0032)	(0.0012)
Supplement	-0.0056***	0.0058***	0.0023***
	(0.0020)	(0.0020)	(0.0006)
N	283,335	274,646	291,394
R^2	0.1696	0.1750	0.0172
out mean	0.52	0.47	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 and Supplement is the effect of 100 euros of municipal HCA supplement per month included in the same specification. 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) and having been sentenced from crime committed from ages 15 to 18 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).