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Early Childhood Policy Effects on Children, Their Families, and Childcare Providers

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Early Childhood Policy Effects on Children, Their Families, and Childcare Providers

DISSERTATION

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for the degree of

DOCTOR OF PHILOSOPHY

in Education

by

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2024

DEDICATION

To Julia.

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ABSTRACT OF THE DISSERTATION

Early Childhood Policy Effects on Children, Their Families, and Childcare Providers

By

Maria Sauval Algorta

Doctor of Philosophy in Education

University of California, Irvine, 2024

Distinguished Professor Greg Duncan, Co-Chair
Associate Professor Jade Jenkins, Co-Chair

Early childhood policies can affect a variety of actors in different ways, both intentionally or not. In this dissertation, I present three studies to measure the effects of early childhood interventions on outcomes for the children who benefit from the interventions, their families, and childcare providers. The studies are based on two interventions: North Carolina's pre-kindergarten program (NC Pre-K) and a randomized controlled trial of a child allowance for low-income families with young children (Baby's First Years). In Study 1, I estimate the effects of an unconditional, regular cash transfer on the employment participation and earnings of low-income mothers with young children. I use data from a randomized controlled trial, Baby's First Years, which randomized participants to receive either a \$333 or \$20 monthly cash gift during the first four years of their children's life. In Study 2, I study the effects of NC Pre-K's expansion on the childcare market. Taking advantage of the time and geographic variation of the program's rollout, I measure how the expansion of the NC Pre-K program affected the enrollment of children in childcare facilities across the state. In Study 3, I measure whether participating in NC Pre-K affected teen births. Using administrative data, including birth records, educational records, and pre-k participation linked at the individual level, I estimate the causal effects of pre-k enrollment on outcomes during females' teenage years.

Chapter 1

Introduction

In the last several decades, researchers across social sciences have developed substantial knowledge of the importance of early childhood experiences in shaping individuals' life trajectories. This has been accompanied by a growing interest in social investment in this period. However, while there is now substantial research-based agreement on the importance of supporting young children and their families, there is less agreement on concrete policy designs and how to implement them (Gormley Jr., 2007; Huston, 2005). This is partially due to the lack of a direct link between research findings and policy implications. In this sense, this dissertation aims to contribute to early childhood policy research, focusing on two early childhood policies in the United States that, for different reasons, are surrounded by substantial policy debate: pre-kindergarten programs and cash support for families with young children.

Importantly, this dissertation is also strongly informed by previous contributions in different disciplines such as economics, developmental psychology, sociology, and education. I aim to reflect on the interdisciplinarity nature of policy research and integrate these perspectives in the different research stages, including motivating the research questions, choosing the right methodological tools to answer them, and interpreting and drawing conclusions based on the findings, with a focus on informing policy discussion, design and implementation. Next, I describe some of the contributions of this dissertation.

First, *when policies might intend to support the development of young children, they can unintendedly affect the behaviors of their family members.* While research has demonstrated the link between child care and parental employment, the link between child allowances and employment is not well-studied. This has historically been a salient concern in the policy arena, and it has gained even more attention recently since the expansion of child tax credits as a transitory pandemic response. In chapter 2, I use a randomized control trial that provided low-income families with an unconditional cash gift, the Baby's First Years study, to measure whether receiving a $313 every month during the first four years after the birth of a child affected maternal employment and household earnings.

Second, *we do not have a clear understanding of how public involvement in childcare and education provision affects the childcare market.* This is particularly important in the context of the United States' fragmented ECE system, in which child care is provided by various private and public uncoordinated initiatives that often overlap and compete with each other (Karch, 2013). To contribute to this question, I evaluate how the enrollment of children under five years old in all licensed childcare centers was affected when North Carolina expanded pre-kindergarten slots for four-year-old children. This is presented in chapter 3.

Third, *how early childhood education policies affect individuals throughout the lifespan is still unclear.* While small demonstration programs such as Perry Preschool and Abecedarian have long-lasting effects, current scaled-up education-based programs lead to less clear conclusions. Moreover, most state-funded pre-kindergarten programs, who currently enrolled a large percentage of the four-year-old population, have not been evaluated for long-term outcomes. In chapter 4, I present the effects of the North Carolina pre-kindergarten program on teen births, which can be an important social marker of social and economic prospects for women (Kearney and Levine, 2012).

To address the previous gaps in the early childhood policy literature, I apply rigorous research designs to infer causality, including differences in differences, instrumental variables, fixed effects, and a randomized controlled trial. In terms of data, in two of the studies, I used statewide longitudinal datasets built by linking many years of administrative records of different sources at the individual level for the entire state of North Carolina. The availability of administrative data, which has become more widespread over the years, opens new opportunities to evaluate policies, providing several advantages that I exploit in these studies (e.g., its large sample size, its accuracy and richness, the possibility of creating a panel that follows all units over many years and combining variables from different sources). In the other study, I use data from a unique large randomized controlled trial conducted across four U.S. metropolitan areas, and its large response rates allowed us to make a unique contribution where experimental evidence is scarce and difficult to obtain.

In chapter 5, I summarize the findings and discuss the contributions of the three studies, focusing on their policy implications, as well as research questions that remain unanswered and will guide my future steps as a social policy scholar.

Chapter 2

Unconditional Cash Transfers and Maternal Employment: Evidence from the Baby's First Years Study

Chapter acknowledgments: The research in this paper is funded work by the Eunice Kennedy Shriver National Institute of Child Health and Human Development of the National Institutes of Health under Award Number R01HD087384. This is independently initiated work, and builds on the parent grant of Baby's First Years (BFY), which is a multidisciplinary collaboration of seven Principal Investigators: Kimberly Noble, Katherine Magnuson, Greg Duncan, Lisa Gennetian, Hirokazu Yoshikawa, Nathan Fox and Sarah Halpern-Meekin. This is a version of a submitted article, co-authored with BFY PIs. See `http://www.babysfirstyears.com` for more information about the study. Baby's First Years' datasets are available on the Inter-university Consortium for Political and Social Research (ICPSR) repository (Magnuson et al. 2022). The study was pre-registered in the AEA RCT Registry, with unique identification number AEARCTR-0003262. We thank the University of Michigan Survey Research Center, our partners in recruitment, data collection, and participant location and retention.

2.1 Introduction

Relative to the OECD average, the U.S. spends one-third as much of its GDP on cash and in-kind benefits for families with children (OECD, 2021). In the case of unconditional cash assistance, concerns in the U.S. about their potential adverse behavioral effects on adults have outweighed consideration of their potential benefits for children and led to a shift toward work-conditioned benefits such as earned income tax credits (Aizer et al., 2022; Hoynes and Schanzenbach, 2018). However, both the COVID-19 pandemic and more than 100 state and local basic income initiatives have revived interest in understanding how people in the U.S. may benefit from a basic level of income provided as unconditional cash transfers (Stanford Basic Income Lab, 2024).

Most of the empirical studies of labor supply responses of low-income parents to increased income are of programs like the Earned Income Tax Credit (EITC). The EITC leverages income gains that phase in at lower income levels and phase out at higher income levels, making it difficult to separate income and substitution effects.[1] Additionally, the EITC is disbursed annually through a lump sum payment, rather than monthly. For these reasons, it is unclear whether findings based on the EITC would be found in programs that transfer cash monthly without conditions or a phase-out.

The main contribution of this study is to estimate the labor market behavior of one important subpopulation of low-income individuals—mothers with young children— in response to an unconditional monthly cash transfer that does not count against income eligibility for receipt of benefits from most existing U.S. safety net programs.[2]

[1] Moreover, recent studies of the Child Tax Credit (CTC) extension in the American Rescue Plan (ARP) provide forecasts and estimates of labor supply changes for families with children but, owing to the elimination of the phasing-in of benefits at low levels of family income, the ARP-type expansion of the CTC also leads to both an income and a substitution effect (Goldin et al., 2021; Corinth et al., 2021; Ananat et al., 2022; Bastian and Lochner, 2022; Pilkauskas et al., 2022; Han et al., 2022; National Academies of Sciences, 2019).

[2] The BFY cash transfer is a gift funded by charitable organizations and is not taxable. Agreements were secured with state and local officials to ensure that the cash gifts were not considered to be countable income in determining eligibility for public benefits, including Temporary Assistance for Needy Families (TANF),

Economic theory predicts that unconditional cash transfers should reduce labor supply owing to a pure income effect. For the general population, several recent studies have estimated the magnitude of the responses to unconditional income to be small. A review by Marinescu (2018) concludes that the marginal propensity to earn (MPE) out of unearned income derived from lottery studies and negative income tax experiments is -0.10, i.e., an income increase of $100 leads to a $10 decrease in earnings. Data used in recent work based on U.S. lottery winners by Golosov et al. (2021) supported estimates of treatment effect heterogeneous by income level. The authors found a MPE of -0.31 for adults in the lowest quartile of the income distribution (whose pre-win annual earnings averaged $12,000), with no significant differences by gender. However, none of these studies focus on low-income parents with young children. Bibler et al. (2023) analyzed the short-term labor supply effects of the Alaska Permanent Fund, a universal cash transfer, and found that women were more likely to reduce hours worked following the transfer receipt. Noteworthy for this paper is that they found larger reductions for mothers of children under age five, but did not estimate separate labor supply responses for mothers in higher- vs. low-income households.

Our analysis focuses on a racially and geographically diverse sample of low-income mothers with younger children (under four years old) in the United States. The labor supply responses of this group are particularly relevant and expected to differ from those of other populations. On the one hand, a long delay in the return to work after birth can be costly: the child penalty that mothers experience when they interrupt their labor trajectories after the birth of a child may be an important cause of long-run gender gaps in earnings (Angelov et al., 2013; Kleven et al., 2019). For example, Kuka and Shenhav (2020) find that returning to work sooner after childbirth has long-term positive consequences in the labor market outcomes of low-income mothers, as shown by EITC-driven changes in work incentives. On

Supplemental Nutrition Assistance Program (SNAP), Medicaid, childcare subsidies, and Head Start. In the case of Supplemental Security Income and Section 8 Housing Choice Vouchers, federal rules stipulate that gift income may be counted in eligibility determination. This was explained to participating mothers before they consented to receive the cash gift.

the other hand, reducing the employment of parents of young children might be beneficial. Parents are typically in the early stages of their labor market careers, with higher economic instability and increased care burdens. Low-income workers are more likely to work irregular and unpredictable schedules, which are harder to balance with childcare responsibilities (Enchautegui et al., 2015). Mothers might face additional challenges in their labor market participation during the first few years after birth, when non-parental childcare is more expensive and less available (Herbst, 2022). However, the lack of access to paid maternity leave and pressing economic needs may force them to rush their return to the labor market following a birth. In 2019, more than half of single mothers with infants under one year old reported working (Bureau of Labor Statistics, 2021).

There is no clear consensus on the size of low-income mothers' labor supply and behavioral responses to a pure income effect. Exploiting an increase in TANF benefits in New Hampshire that occurred in 2017, Freedman and Kim (2022) estimated a 6% decrease in the employment of low-income single mothers, translating into an elasticity of -0.14. However, these benefits are conditional on studying or working. A few studies have measured the effects of unconditional support for low-income parents in the European context. Del Boca et al. (2021) and Aparicio Fenoll and Quaranta (2022) analyzed a cash transfer experiment with low-income parents of 0 to 6-year-old children in Italy, which had both conditional and unconditional treatment arms. They found no effects of receiving an unconditional cash transfer on labor market outcomes, while the employment participation was higher for males when the cash transfer was conditional on attending a job search training. Mothers' labor supply did not respond to either of the cash transfer treatment arms. On the other hand, Jensen and Blundell (2024) estimated a sizeable income elasticity for mothers with young children as a response to an (unconditional) child benefit reduction in Denmark, particularly at the extensive margin. They estimate an elasticity between -2.7 and -0.9.

Baby's First Years (BFY) is a randomized control study of an unconditional cash transfer made available to 1,000 low-income mothers who gave birth in 12 U.S. hospitals in four cities in 2018-19. Upon consent, mothers were randomized to receive a monthly unconditional cash gift of either \$333 or \$20, starting at their child's birth, for a duration of 40 months (subsequently extended to 76 months). The high-cash gift amount corresponds to 17% of baseline household income. Take-up of the cash gift was nearly universal (Gennetian et al., 2023), and the study has achieved 90+% response rates in each of its four annual waves of survey data collection.

Overall, we found mostly negative but not statistically significant effects of the high-cash gift on the extensive and intensive margins of labor supply, as well as self-reported annual earnings, over the four years of the study analysis period —which spans the interval between the focal child's birth and fourth birthday.

Our estimated four-year pooled impact on the probability of mothers working for pay was -1 percentage point, or -2.8%.[3] Although our estimates lacked precision to detect small effects, we can rule out effects larger than 6 percentage points, such as those estimated by Jensen and Blundell (2024) for Danish mothers. Moreover, the point estimate translates into an income elasticity of -.16, which is similar to findings on single heads of households in the negative income tax experiments (Robins, 1985) (ϵ=-.16), low-income single mothers in New Hampshire (Freedman and Kim, 2022) (ϵ=-.14), and married women and single mothers' responses to tax changes across the 70s and 80s (Blundell and Macurdy, 1999; National Academies of Sciences, 2019) (between ϵ=-.085 and ϵ=-.12). While still statistically non-significant, the estimated extensive margin coefficient was larger at the time of the child's first birthday (a 4-percentage-point group difference, corresponding to a 9% reduction in

[3] A possible concern could be the attenuation of the estimated effects due to the incidence of adjustment frictions related to childcare responsibilities (Gelber et al., 2020). These should diminish over time throughout the four years of analysis. However, it is possible that mothers in our sample had more children after the BFY focal child, meaning that the adjustment frictions could have lasted longer. We take this into consideration by running the analysis for the sample of mothers who did not have younger kids at the time of the Wave 4 survey, i.e., the BFY focal child is the youngest. This is the case for about 70% of the BFY mothers. The impact of the cash gift for this sample is also null (high cash gift group coefficient = 0.006, p-value = 0.801).

employment). This might be interpreted as a beneficial response in a context with low access to maternity leave and high infant childcare prices. The cash gift effect converged to zero in the following years.[4] Additionally, the cash gift did not affect the probability of living in a household with no employed adults (measured as a household with no earned income), which is the case for about 15% of our sample.

Our pooled estimates also allow us to rule out a decrease of more than 4.7 hours worked per week at the intensive margin. However, looking at each wave separately, we found a statistically significant decrease of 6.0 hours worked per week (-39%) at the time of the child's second birthday and in the midst of the COVID-19 pandemic, and null results at the time of the first, third, and fourth birthday. Our exploratory analysis suggests that the BFY cash gift facilitated behavioral adjustments to the pandemic.

Finally, we rule out changes in maternal annual earnings of more than \$1500 (i.e., a marginal propensity to earn out of the cash gift of -0.4). We estimated a statistically insignificant \$451.21 reduction in maternal earnings and \$177.74 reduction in non-maternal household earnings, corresponding to a marginal propensity to earn out of the cash gift of -0.15 and -0.10, respectively. These point estimates are quite similar to those found for the general population in other settings (Marinescu, 2018), although smaller than lottery-based estimates for low-income individuals (Golosov et al., 2021).

The COVID-19 pandemic occurred during the BFY study, which raises some concerns. The contraction of the economy and job losses during the early months of the pandemic might have attenuated labor supply responses to the cash gift, limiting the extent to which our results can generalize to other contexts. However, because we observed maternal employment over four years (from July 2019 to July 2023), the pandemic period prior to the widespread

[4]Stilwell et al. (2024) offer a closer look into the first year and found that the cash gift did not change breastfeeding, childcare, and employment patterns in the first twelve months or the likelihood of meeting their baseline intentions. There was a small (one-month) delay in entering formal home- or center-based childcare.

availability of vaccinations comprised only about one-quarter of the total reporting period. Compared with other recessions, the labor market recovered from the COVID-19 pandemic very quickly. The labor force participation rate, particularly for the "prime-age" group (ages 25-54), was already at the pre-pandemic levels in mid-2022 (The White House, 2023). Hence, our study enables us to estimate treatment effects across very different economic contexts.

The data also allowed us to explore the extent to which the pandemic interacts with the cash-gift effect, which is theoretically ambiguous. A higher cash gift could have allowed mothers to offset work participation or hours during the pandemic, at a time when in-person work may have been dangerous and child care was scarce. On the other hand, the cash gift could have supported mothers' work participation or hours during the pandemic because it could be used to cover pandemic-related costs of working (e.g., engaging in more protective behaviors, improving internet/technology, paying for alternative child care arrangements when schools closed, etc.).

As previously indicated, we found that mothers in the high-cash gift group reduced working hours and were significantly less likely to work full-time than mothers in the low-cash gift group (an effect of -6 p.p. in full-time work, or -27%) at the time of their child's second birthday, which was early in the COVID-19 pandemic when public health protocols and vaccination availability were rapidly evolving. Labor supply differences were not apparent in the two following waves of data collection. Moreover, mothers in the high-cash gift group were significantly more likely to report making major behavioral changes to limit contact with people outside their homes and losing income due to COVID. Finally, the reduction in weekly hours was not associated with their site-specific labor market conditions during the interview month. Together, these results suggest a possible role of cash support in addressing health and family concerns during the COVID-19 pandemic.

2.2 The Baby's First Years Study

Between May 2018 and June 2019, the Baby's First Years study recruited 1,000 low-income mothers in New Orleans, New York City, Omaha Metropolitan area, and the Twin Cities of Minneapolis and St. Paul. Potential participants at twelve hospitals were approached on postpartum wards after giving birth, assessed for study eligibility, and, if eligible, offered enrollment in the study.[5] Study recruitment was designed to provide an even flow of participants across the 12-month recruiting period. Details about the study design can be found on the study website, in Noble et al. (2021), and are pre-registered (Duncan et al., 2019).[6]. After completing a baseline survey, participants were invited to receive a cash gift with random assignment into a high-cash gift group (n = 400) or a low-cash gift group (n = 600). The former is receiving \$333 per month (\$3,996/year) and the latter \$20 per month (\$240/year) for an initially planned duration of 40 months, then extended for a total duration of 76 months, with no restrictions regarding how the money could be spent. The cash gifts were loaded onto a debit card with a "4MyBaby" logo printed on the card. The card was activated and given to the mothers in the hospital. Each month, either \$333 or \$20 is loaded on the same day of the month as the child's birth with an accompanying text message. A hotline was available to assist in using the BFY card, and most issues that arose were easily and quickly able to be resolved (e.g., replacement cards). Mothers were able to use the card effectively. For the subsample of mothers who consented to share card transaction information (n=900), as of July 2022, only 7 mothers in the low-cash gift group had failed to ever use their cards (and none in the high-cash group failed to do so), suggesting that high implementation success of the cash gift disbursement approach. Additional relevant

[5]Mother's eligibility was based on having a household income below the federal poverty line in the previous calendar year, speaking English or Spanish, living in the state of recruitment and not being "highly likely" to move to a different state or country in the next 12 months, being of legal age for informed consent, and having a baby admitted into the well-baby nursery and scheduled to be discharged into the custody of the mother.

[6]The study website is https://www.babysfirstyears.com/. See Magnuson et al. (2022) for public use data files and documentation. The American Economic Association's Registry information can be found here

information about the study implementation can be found in Gennetian et al. (2023).

Our ability to detect small income effects is hampered by the study's sample size. We have statistical power to identify treatment effects of a 6-percentage-point change in the probability of working for pay at a .05 significance level and 80% power (Bloom, 1995), given the control group employment rate of 48%, averaged across waves. Given that the cash gift represents a 17% increase in household income, our minimum detectable elasticity is 0.7. We can also detect a change of $1,500 in maternal annual earnings and $2,800 in household annual earnings, given a control group base of $10,970 and $23,320, respectively. This corresponds to minimum detectable propensities to earn (MPE) out of the cash transfer of -.40 and -.76 at the individual and household level respectively. On the other hand, the sample size does enable us to rule out some of the larger labor supply reductions suggested in policy discussions[7], as well as the elasticities found by Jensen and Blundell (2024) for Danish mothers, which were between -2.7 to -0.9.

2.3 Empirical Strategy

2.3.1 Participants

Some 976 out of 1,000 participants responded to at least one of the four follow-up surveys, which were scheduled to take place around the time of the children's 1st, 2nd, 3rd, and 4th birthdays.[8] Because recruitment was spread across a 12-month period, these follow-up

[7] As a recent example, concerns about work disincentives and large employment losses were raised in discussions around the expansion of the Child Tax Credit as part of the Build Back Better Act in 2021 (e.g., 167 Cong. Rec. H188; 167 Cong. Rec. H201; 167 Cong. Rec. S145; 167 Cong. Rec. S185). A prominent example from the older welfare reform debates was Anderson (1978)'s "average" estimate of the labor supply responses for female heads of household in the Negative Income Tax experiments of -46 percent.

[8] Although the intention was to complete the surveys in the month of the child's birthdays, some participants completed the survey later. And while the numbers in the high- and low-cash gift groups were balanced throughout the recruitment year, there were some group differences in the average age of the children at the time of the data collection: 13.1 (sd=2.1), 24.9 (sd=1.9), and 36.9 (sd=1.8) months at the time of the

surveys provide a fairly continuous time series of labor supply responses across the entire follow-up period.

Response rates among eligible mothers were consistently very high: 94% at Age 1 (931 respondents), 93% at Age-2 (922 respondents), 93% at Age-3 (922 respondents), and 90% at age 4 (888 respondents). More details about sample construction and reasons for non-response can be found in the CONSORT diagram in Appendix A.1. Reasons for non-response included a small number of sample exclusions (either the child or mother was deceased), ineligibility to complete a survey wave (when the mother was incarcerated or did not have custody of the child), as well as a number of participants who were not found or were unavailable or refused to participate in the survey.[9]

Age-1 interviews were scheduled to be conducted in person between July 2019 and June 2020. The COVID-19 pandemic began in March 2020; as a result, in-home interviews were suspended on March 12, and the study transitioned to phone interviews beginning on March 14. A total of 326 phone interviews (35 percent of the Age-1 total) were conducted, which means that nearly two-thirds of the Age-1 labor market information was gathered before the onset of the pandemic. All Age-2 and Age-3 interviews were completed over the phone. At Age-4, participants were scheduled for in-person data collection at university labs. The Age-4 outcomes for this paper come from a short survey that was conducted during the lab visit. For mothers who were not able to bring their children to a lab, the survey was conducted over the phone.

Descriptive statistics measured at baseline (i.e., pre-randomization) are included in Appendix

three data collect for the low-cash gift group, and 12.6 months (sd=1.5), 24.5 (sd=1.3), and 36.9 (sd=1.3), respectively, for the high-cash gift group.

[9]While the attrition number is small, a joint test of differential attrition using all baseline covariates showed that mothers who did not respond to surveys were significantly different from those who did (except for wave 3, when the differences were not significant). Specifically, mothers who did not respond to the surveys were more disadvantaged, for instance, in terms of their overall health and depression symptoms at baseline (see Appendix A.1). Robustness checks addressing potential attrition concerns are presented in Appendix A.2.

A.3, for the sample of participants who responded to any of the follow-up surveys (n=976), as well as each wave separately. The participating BFY study mothers were diverse along several dimensions, but well-balanced between the two cash-gift groups. Characteristics for the full sample of 1,000 enrolled mothers can be found in Noble et al. (2021). At the time of enrollment in the study, mothers in both the low- and high-cash gift groups reported similar levels of labor market participation and intentions to work in the future; about 57 percent of mothers in both groups worked while pregnant, 85 percent planned to work at some point during the next year, and more than half were planning to work within three months of their child's birth.[10] Joint significance tests of all the listed covariates predicting cash group assignment suggested no treatment-group differences across respondents in any of the four follow-up survey samples. Nevertheless, as described in Section 2.3.3, baseline variables were included in the regressions to improve the precision of the impact estimates.

2.3.2 Measures

We have several variables designed to capture maternal and household labor market participation, which we organize into three groups: whether mothers participate in the paid labor market (extensive margin), hours of paid work (intensive margin), and maternal and household annual earnings.

Mothers were asked about paid employment at the time of the survey interview: whether they were working for pay, whether they were self-employed[11], and how many hours they had worked in an average week in the last month. We create an indicator for working at

[10]The high percentage of mothers who were planning to work after birth is somewhat surprising (85%), especially when we consider the actual employment rates (between 40-50% depending on the survey wave). Stilwell et al (forthcoming) show that the cash gift did not affect the probability of meeting their baseline employment intentions during the first year, and this result did not vary by past labor market involvement, whether they had older children, or whether they lived with other adults in the household.

[11]Due to a survey implementation error at the beginning of data collection, self-employment was not asked to all respondents until December 10, 2019, when the error was fixed. For this reason, some of our outcomes have a smaller sample size. The main outcome, whether mothers are working for pay, was not affected.

least 35 hours per week.

Mothers were also asked to report their annual earnings from work as well as income from other sources during the previous calendar year. We construct a measure of earnings for the mother (available for all four waves) and a measure of earnings from all other adults in the household (available for waves 1-3).[12] To minimize the impact of outliers, all earnings measures are truncated at the 99th percentile. As a sensitivity test, we contrast the results with and without truncation in Appendix A.4. Calendar-year earnings correspond to either 2018 or 2019 at Age-1, 2019 or 2020 at Age-2, 2020 or 2021 at Age-3, and 2021 or 2022 at Age-4.[13] To assess whether unconditional cash support might increase the likelihood of children residing in households without any employed adults (Rachidi and Doar, 2019; Winship, 2021), we also estimate impacts on an indicator of whether the household has any positive earnings in the prior calendar year. While the amount of non-maternal earnings is not reported at Age-4, we do have an indicator variable for whether there were any non-maternal earnings in the household in the previous week.

Finally, the second year of follow-up survey data included a module of questions related to how the pandemic affected their income, health, and general behavior changes, which we explore in section 2.4.3.[14]. We provide more details on the alignments between the timing of relevant measures, data collection waves, and calendar time in Appendix A.2.

[12]At Age-1, the previous calendar year included some time prior to randomization, hence, Age-1 results should be interpreted with caution.

[13]While mothers reported significantly less income for the calendar year 2020, there was no significant effect of the cash gift group assignment on the year they reported income for. Additionally, there are no significant differences between cash gift group assignments on whether they lived with other adults that could have earnings as well (Appendix A.5).

[14]For more details, questionnaires are available on the study website.

2.3.3 Analysis

Because assignment to the high-cash gift group was random by design, securing an unbiased estimate of the impact of the higher cash gift (a difference of $313/month between groups) is relatively straightforward.

Our estimates pooling the four waves of data are derived from the following model:

$$Y_{isw} = \beta_0 + \beta_1 HighCashGift_{isw} + X_{isw} + \gamma_w + \delta_s + \varepsilon_{isw} \tag{2.1}$$

where i indexes the mother, s indexes the four sites, w indexes the wave of data collection, Y is the outcome of interest, $HighCashGift$ is an indicator for being assigned to receive $333 as opposed to $20 per month, X is a vector of covariates, γ_w is a vector of wave indicators, $delta_s$ is a set of four site fixed effects, and ε is the error term. Site fixed effects are needed since randomization occurred within site. Standard errors were clustered at the individual level. The coefficient β_1 provides an estimate of the causal effects on Y of being assigned to the high as opposed to low-cash gift group. Given mothers' nearly universal use of the money in both groups, the intention-to-treat estimates can also be interpreted as the effects of receiving a monthly cash gift of $313, i.e., treatment-on-the-treated (TOT) estimates. Regressions were estimated using Ordinary Least Squares, while the effects on the number of hours worked were also estimated using a tobit model. For annual earnings, we also control for the calendar year for which earnings are reported. Finally, we also estimate the same specification for each wave separately, plus a fully interacted model to assess whether the estimates differ across waves.

The pre-registered plan specified the following baseline covariates to be used to improve the precision of causal estimates: mother's age, maximum education level attained, race and ethnicity, marital status, general health, maternal depressive symptoms, cigarette and alcohol consumption during pregnancy, number of children born to mother, number of adults in the household, father living with the mother, household income, household net worth, and baby's weight and gestational age at birth. We added the following pre-birth (and therefore pre-random assignment) maternal employment measures: worked while pregnant, continued working until birth, and plans to return to work after birth. We also control for two post-randomization variables: a calendar month indicator and the time distance to the child's birthday within wave in months (i.e., how far away they were from the scheduled data collection).

2.4 Results

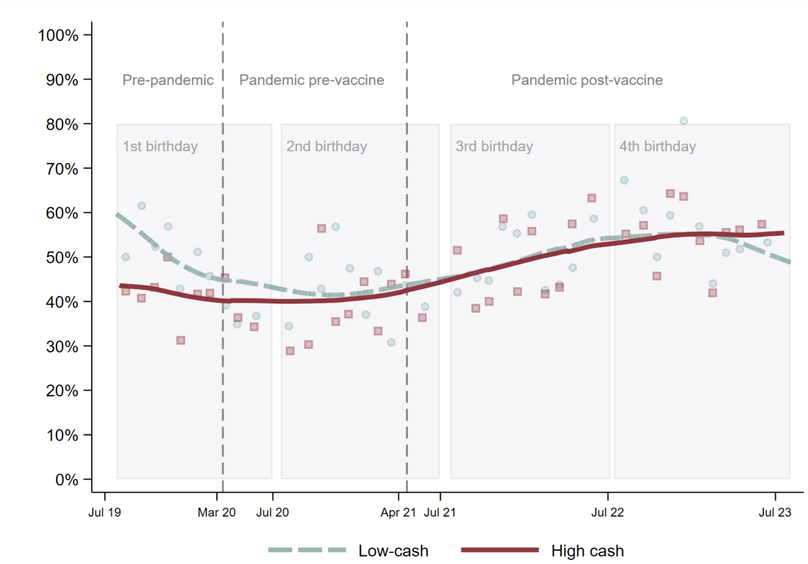
2.4.1 Descriptive Results

We first present unadjusted means of employment by month and cash-gift group (Figure 3.1). Because each mother is interviewed only once per year, around the time of her child's birthday, the within-wave monthly variation in Figure 1 comes from the fact that BFY sample recruiting and subsequent follow-ups were spread across roughly 12-month periods. Recruitment began in May 2018 and the Age-4 reinterview period ended in July 2023. Figure 3.1 shows the four annual data collection periods mapped against child age and three pandemic periods: pre-pandemic (before March 14, 2020, when data collection switched to phone interviews), pandemic pre-vaccine (between March 14, 2020, and April 19, 2021; when vaccines became available to all adults in the U.S.), and pandemic after vaccine (post April 19, 2021). As might be expected, the proportion of mothers reporting working for pay decreased during the pandemic months of 2020, but began to show a positive trend for both groups by the beginning of 2021. It appears that the high-cash gift group mothers were less likely to be working for pay during the first year, particularly those interviewed before

COVID. Employment rates seem to converge after this point. Approximately, between 40% and 60% of the mothers reported working for pay across this period. As a reference, the employment-population ratio of all women with children under 3, regardless of income, was .59 in 2021 (US Census Bureau, 2021).

Figure 2.1: Maternal employment by interview month

A. Fraction of mothers working for pay



B. Hours worked per week, if working

Notes: Markers in Panel A illustrate the percentage of participants who responded "yes" to the question "Are you currently working for pay?", by interview date, while those in Panel B represent the average hours worked per week in the four weeks prior to the interview. Hours are truncated at the 99th percentile. Each bin represents a calendar month. To reduce the noise caused by some sample unbalance at the beginning and the end of each wave, the first and last months are winsorized within waves. I.e., the first bin in each wave combines the interviews conducted before August and the last bin combines the interviews conducted after May. Lines represent a LOWESS smoothed trend in the variable with a 0.6 bandwidth. Each wave of data collection is indicated by the gray background rectangles. Vertical black lines illustrate three main "pandemic periods", based on the date when interviews switched from in-person to phone-based (March 14, 2020) and the date when all adults in the U.S. became eligible for COVID-19 vaccines (April 19, 2021). Data for Panel B is missing for the first months of data collection due to an error in the survey implementation.

2.4.2 Main Estimates of the BFY Cash Gift on Employment

Our main results summarizing the effects of the BFY cash gift on maternal and household employment and earnings estimated using Equation 2.1 are reported in Tables 2.1 and 2.2, with robustness checks shown in Appendix A.2. Table 2.1 shows the impacts on maternal employment. In all four waves, group differences at the extensive employment margin (columns 1-2) were not statistically significant. As suggested in Figure 1, the coefficient size is larger at wave 1 (b=-0.04). However, we cannot reject that the estimates for all waves are equal. In the pooled analysis that includes data from all four follow-up surveys (Panel A), we estimate a 1 percentage-point (-3%) lower proportion of high- vs. low-cash mothers in the likelihood of working for pay but cannot reject the null hypothesis of no difference ($p = 0.55$): the 95% confidence interval ranges from -5.6 and +2.9 percentage points. While null effects do not imply the absence of a labor supply response, our estimates rule out effects larger than 6 percentage points in the probability of working for pay.

At the intensive margin (columns 3-5), at the time of the Age-2 survey, which overlapped substantially with the pre-vaccine phase of the pandemic, there was a statistically significant 7 percentage-point reduction (-27%) in the probability of working full-time (more than 35 hours per week) ($p<0.05$) and a reduction of 6 hours worked per week (-39%). As shown by the hours' histograms in Appendix A.3, much of this reduction was concentrated at the 40-hour threshold. This decline is robust to different specifications related to covariates, adjustments to non-response, and effect bounds based on Lee (2009) (Appendix A.2). Impacts on the intensive margin were null at the time of the Age-1, Age-3, and Age-4 surveys.

Finally, the effects on maternal annual earnings were also small and not statistically significant at conventional levels. We estimated a decrease of $450 in annual earnings, which implies a marginal propensity to earn out of the cash gift of -.15.[15] Additional tests in Ap-

[15]The MPE is obtained by dividing the annual earnings reduction in dollars by the annual increase in income, i.e., $3756.

pendix A.4 show that these results are not sensitive to the inclusion of outliers. We also rule out a MPE larger than -0.4.

Table 2.1: Main effects of BFY cash gift on maternal employment

	Working for pay	Self-employed	Working full-time	Weekly Hours (OLS)	Weekly Hours (Tobit)	Annual earnings
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Pooled results						
High cash gift	-0.01	0.01	-0.03	-1.62+	-2.08	-451.21
	(0.02)	(0.02)	(0.02)	(0.90)	(1.70)	(532.78)
Observations	3661	3323	3283	3194	3194	3582
Low-cash gift group mean	0.48	0.15	0.31	17.91	17.91	10967.79
Effect in %	-2.76%	4.73%	-9.48%	-9.03%	-11.61%	-4.11%
Panel B. Wave 1						
High cash gift	-0.04	0.04	-0.01	-0.46	-0.51	204.10
	(0.03)	(0.03)	(0.03)	(1.53)	(3.20)	(508.07)
Observations	931	593	582	582	582	922
Low-cash gift group mean	0.45	0.11	0.19	13.41	13.41	7254.38
Effect in %	-9.29%	39.46%	-7.42%	-3.44%	-3.79%	2.81%
Panel C. Wave 2						
High cash gift	-0.02	0.01	-0.07*	-3.27**	-6.02*	-229.53
	(0.03)	(0.02)	(0.03)	(1.18)	(2.49)	(676.47)
Observations	921	921	908	908	908	907
Low-cash gift group mean	0.42	0.13	0.24	15.41	15.41	9868.70
Effect in %	-4.81%	7.10%	-27.56%	-21.21%	-39.05%	-2.33%
Panel D. Wave 3						
High cash gift	0.01	0.01	0.01	-0.32	0.77	-816.22
	(0.03)	(0.02)	(0.03)	(1.26)	(2.23)	(829.99)
Observations	922	922	910	910	910	913
Low-cash gift group mean	0.50	0.13	0.30	18.46	18.46	11391.83
Effect in %	2.39%	7.12%	1.88%	-1.74%	4.17%	-7.16%
Panel E. Wave 4						
High cash gift	-0.01	-0.02	-0.04	-2.04	-2.48	-1021.38
	(0.03)	(0.03)	(0.03)	(1.41)	(2.23)	(984.82)
Observations	887	887	883	794	794	840
Low-cash gift group mean	0.56	0.21	0.46	23.49	23.49	15816.61
Effect in %	-1.58%	-8.39%	-7.79%	-8.67%	-10.57%	-6.46%
Panel F. Additional Tests						
All waves are equal (p-val)	0.615	0.485	0.162	0.148	0.089	0.279
All waves are equal to zero (p-val)	0.670	0.602	0.091	0.067	0.096	0.426

Notes: + p<.10, * p<.05, ** p<.01. Robust standard errors in parentheses. All regressions include site fixed effects and the following covariates: mother's age, maximum education level attained, race and ethnicity, marital status, general health, maternal depressive symptoms, cigarette and alcohol consumption during pregnancy, number of children born to mother, number of adults in the household, father living with the mother, household income, household net worth, baby's weight and gestational age at birth, mother worked while pregnant, continued working until birth, plans to go back to work after birth, calendar month indicator, and the time distance to the child birthday within wave, in months (i.e., how far away they were from the scheduled data collection). Pooled estimates also include wave fixed effects and standard errors are clustered at the individual level. Working full-time is defined as working at least 35 hours per week.

The effects of the BFY cash gift on household earnings are reported in Table 2.2. For the age-1 to age-3 pooled analysis, point estimates were -178 (-1.4%) in non-maternal earnings, and -407 in total household earnings, implying a marginal propensity to earn out of the cash gift of -.10 at the household level.

The differences in the timing of measurement of employment status and earnings do not allow us to precisely estimate whether receiving the high cash gift had an effect on wage rates. However, since annual earnings are reported for the calendar year preceding the interview, we can approximate a wage rate using annual earnings reported in a given year and the hours reported in the previous year. We do not find a significant effect of the cash gift on this estimated wage rate.

Finally, we tested for differences in whether mothers reported that no members of their households had positive earnings in the calendar year prior to the Age-1 to Age-3 interviews. Some 15% of both low- and high-cash gift group households fell into this category. The point estimate of the BFY impact on this measure of the absence of work is negative (i.e., the high-cash gift group is less likely to have no earnings from work) but far from any threshold of statistical significance. Moreover, at the time of the Age-4 survey, there were no significant differences in the probability of living in a household with non-maternal earnings.

Table 2.2: Main effects of BFY cash gift on household earnings

	HH non-maternal earnings	Total HH earnings	HH has zero earnings (indicator)	HH has non-maternal earnings (prev.week)
	(1)	(2)	(3)	(4)
Panel A. Pooled (waves 1-3)				
High cash gift	-177.74	-406.90	-0.01	
	(853.71)	(1024.13)	(0.02)	
Observations	2654	2638	2638	
Low-cash gift group mean	12554.16	22322.04	0.15	
Effect in %	-1.42%	-1.82%	-4.66%	
Panel B. Wave 1				
High cash gift	-574.68	-467.77	0.00	
	(1004.71)	(1140.05)	(0.02)	
Observations	892	887	887	
Low-cash gift group mean	12995.78	20272.44	0.12	
Effect in %	-4.42%	-2.31%	0.74%	
Panel C. Wave 2				
High cash gift	-618.94	-674.21	-0.01	
	(1199.28)	(1513.98)	(0.02)	
Observations	879	873	873	
Low-cash gift group mean	12777.73	22906.13	0.15	
Effect in %	-4.84%	-2.94%	-6.16%	
Panel D. Wave 3				
High cash gift	549.88	-206.72	-0.01	
	(1202.54)	(1528.99)	(0.03)	
Observations	883	878	878	
Low-cash gift group mean	11883.79	23788.13	0.18	
Effect in %	4.63%	-0.87%	-6.65%	
Panel E. Wave 4				
High cash gift				0.01
				(0.03)
Observations				878
Low-cash gift group mean				0.38
Effect in %				3.89%

Notes: + p<.10, * p<.05, ** p<.01. Standard errors in parentheses. All regressions include site fixed effects and the following covariates: mother's age, maximum education level attained, race and ethnicity, marital status, general health, maternal depressive symptoms, cigarette and alcohol consumption during pregnancy, number of children born to mother, number of adults in the household, father living with the mother, household income, household net worth, baby's weight and gestational age at birth, mother worked while pregnant, continued working until birth, plans to go back to work after birth, calendar month indicator, the time distance to the child birthday within wave, in months (i.e., how far away they were from the scheduled data collection, and interview year. Pooled regressions also include wave fixed effects and standard errors are clustered at the individual level.

In sum, we do not detect statistically significant reductions in maternal employment as a response to the BFY cash gift, except for a reduction in the hours worked at the time of the Age-2 survey. This is further explored in the next section. Moreover, the results are robust to the exclusion of covariates or the inclusion of non-response weights (Appendix A.2). To further address attrition concerns, we provide Lee bounds to our estimates. Finally, we explored whether our results were hiding some relevant heterogeneity by population subgroup (Appendix A.6). In particular, we considered some variables that are arguably related to labor market participation outcomes: level of education, presence of a partner in the household, number of (older) children besides the BFY-focal child, and previous labor market involvement (during pregnancy). We found suggestive evidence that mothers who had a partner at the time of the baby's birth might have reduced their employment significantly more than those who did not have a partner, and mothers without high school completion also experienced a larger reduction in earnings.

2.4.3 The COVID-10 Pandemic, Cash Gifts, and Maternal Employment

The pandemic was an important context for the families in the study. While the timing of the pandemic is a valid concern regarding the external validity and interpretation of the results, the four-year duration of the data collection allows us to offer some insights into how the pandemic interacted with the cash gift.

Having four waves of data, we were able to observe the employment outcomes of mothers throughout a considerably long period of time. Survey interviews were completed: i) during the pre-pandemic period (between July 2019 and March 14, 2020; when data collection switched to phone interviews); ii) during the pandemic period before vaccines became widely available (between March 14, 2020, and April 19, 2021; and iii) the period after vaccines became widely available (between April 19, 2021, and July 2023). While the pandemic might have had an important influence during the end of wave 1 and wave 2, wave 3 and wave 4 were collected in the post-vaccine era, allowing us to better disentangle the incidence of the COVID-19 pandemic and increasing the external validity of our study. In fact, when the age-4 data collection started, employment levels across the country were already at the pre-pandemic levels.

Table 2.1 showed that group differences in maternal labor force participation were negative and statistically significant at Age-2 but had disappeared completely at the time of the Age-3 and Age-4 surveys. Because much of the Age-2 data collection overlapped with the pandemic pre-vaccine period, these findings suggest that the high-cash gift mothers reduced their labor market hours during the heightened and potentially riskiest time of the pandemic. Next, we further explored whether we can attribute the Age-2 results to the pandemic.

COVID-specific questions asked at Age-2 provide an additional opportunity to analyze whether mothers' employment behavior was associated with the pandemic. Table 2.3 shows that mothers in the high-cash gift group were 11.7% and 19.7% more likely than mothers in the low-cash gift group to live in a household where at least one person lost income or received unemployment benefits due to the pandemic, respectively. Both of these estimates are statistically significant at conventional levels. Moreover, although high-cash-gift mothers were not statistically significantly less likely to get COVID, they were 7 percentage points (9.8%) more likely to report having made major changes to their behavior, such as not going to school or work or limiting contact with people outside their home.

Table 2.3: Effects of the BFY cash gift on COVID-19-related outcomes

	Lost income due to COVID	Unemployment benefits	COVID diagnosis	Major changes to behavior
	(1)	(2)	(3)	(4)
Panel A: Anyone in the household				
High cash gift group	0.07*	0.07*	0.03	
	(0.03)	(0.03)	(0.03)	
Observations	918	911	916	
Low-cash gift group mean	0.60	0.36	0.16	
Effect in %	11.70%	19.72%	18.50%	
Panel B: Mother				
High cash gift group	0.06+	0.04	0.01	0.07*
	(0.03)	(0.03)	(0.02)	(0.03)
Observations	918	912	916	913
Low-cash gift group mean	0.42	0.27	0.13	0.69
Effect in %	15.05%	14.40%	4.72%	9.73%
Panel C: Other household member (not mother)				
High cash gift group	0.03	0.03	0.03	
	(0.03)	(0.03)	(0.02)	
Observations	918	911	918	
Low-cash gift group mean	0.30	0.17	0.11	
Effect in %	9.91%	16.30%	29.04%	

Notes: + p<.10, * p<.05, ** p<.01. Standard errors in parentheses. All regressions include site fixed effects and the following covariates: mother's age, maximum education level attained, race and ethnicity, marital status, general health, maternal depressive symptoms, cigarette and alcohol consumption during pregnancy, number of children born to mother, number of adults in the household, father living with the mother, household income, household net worth, baby's weight and gestational age at birth, mother worked while pregnant, continued working until birth, plans to go back to work after birth, calendar month indicator, and the time distance to the child birthday within wave, in months (i.e., how far away they were from the scheduled data collection). The outcome in Column (4) refers to the third option in the following question: *"Thinking back to when your community was most affected by the coronavirus pandemic, which one of the following statements describes how much you changed your behavior in response to social distancing and stay-at-home recommendations? 1- I made no changes to my behavior; 2-I made minor changes to my behavior (for example, going out less, seeing fewer friends); 3-I made major changes to my behavior (for example, not going to school or work or limiting my contact with people outside our home)"*

We then explore whether the employment conditions during the pandemic might be driving the Age-2 results. To do this, we interact the high-cash gift coefficient with the unemployment rate in the local area during the month of data collection. Arguably, variations in monthly unemployment rates reflect pandemic-related changes in employment conditions (see Appendix A.4).[16]

As observed in Table 2.4, a 1-percentage-point increase in local unemployment was associated with a 1-percentage-point decrease in maternal employment rate, and this association is similar in high- and low-cash gift groups. However, the coefficient on the interaction between being in the high cash gift group and unemployment rates is quite small and not statistically significant. It appears, then, that reductions in work for high-cash gift mothers when their children were two years of age may have been more responsive to health and family concerns than to local employment conditions.[17]

[16]We also conducted a similar analysis using a different local employment condition metric, namely, the number of workers employed in selected industries (retail, hospitality, leisure). The conclusions were similar.

[17]Although the context is different, our results are consistent with Ohrnberger (2022), who finds that a cash transfer program for low-income families with children in South Africa mitigated the adverse health effects of the pandemic, as well as other studies showing the role of cash transfers in mitigating the negative effects of unprecedented shocks (Adhvaryu et al., 2024).

Table 2.4: Effects of the BFY cash gift interacted with local monthly unemployment rate

	Working for pay		Self-employment		Working full-time	
	(1)	(2)	(3)	(4)	(5)	(6)
High cash gift	-0.015	0.007	0.007	-0.002	-0.031	-0.008
	(0.022)	(0.034)	(0.017)	(0.028)	(0.020)	(0.034)
Monthly unemployment rate (site-specific)	-0.013**	-0.012**	-0.003	-0.003	-0.006*	-0.005
	(0.003)	(0.004)	(0.003)	(0.003)	(0.003)	(0.003)
Unemployment rate X High Cash Gift		-0.004		0.002		-0.004
		(0.005)		(0.004)		(0.004)
Observations	3661		3323		3283	
Low-cash gift group mean	0.481		0.148		0.305	

Standard errors in parentheses

$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$

Notes: + p<.10, * p<.05, ** p<.01. Standard errors in parentheses. All regressions include site and wave fixed effects and the following covariates: mother's age, maximum education level attained, race and ethnicity, marital status, general health, maternal depressive symptoms, cigarette and alcohol consumption during pregnancy, number of children born to mother, number of adults in the household, father living with the mother, household income, household net worth, baby's weight and gestational age at birth, mother worked while pregnant, continued working until birth, plans to go back to work after birth, calendar month indicator, and the time distance to the child birthday within wave, in months (i.e., how far away they were from the scheduled data collection). Standard errors are clustered at the individual level.

In sum, based on the timing of the observed reductions in hours worked during the height of the pandemic, together with the COVID-specific questions collected at Age-2, and the additional analysis interacting the cash gift group with local employment conditions, we believe that the high cash gift allowed mothers to reduce their hours of paid work as a response to pandemic-driven disruptions in daily life.

2.5 Conclusion

This study examined the labor supply behavior of mothers with young children in response to a pure income effect created by the receipt of a monthly unconditional cash gift. Findings showed that receiving a $333 monthly cash gift (as opposed to $20) over the first four years after the child's birth led to null to small declines in maternal employment and household earnings. A reduction in maternal work hours was limited to the period around the child's second birthday which also coincided with the height of the COVID pandemic. Our study's exploratory analysis of potential COVID "contamination" suggests that the cash gift may have allowed low-income mothers to reduce work-based exposure to COVID or stay home altogether. At the time of the age-3 or age-4 surveys, which were implemented between July 2021 and July 2023, there were no differences in employment between the two groups at any margin.

A potential concern that would limit the external validity of our study is the design of the card, labeled "4MyBaby".[18] While we cannot directly test whether there is a labeling effect, a look at the transaction data (i.e., categories of expenditures directly derived from the use of the card), as well as a qualitative sub-study, suggest that mothers used the BFY cash gift in a large variety of ways, and mostly to cover existing expenses (Gennetian et al., 2022). Additionally, it is important to note that mothers in both the high- and the low-cash gift group received a similar card, with the only difference being the gift amount. In this sense, concerns regarding potential psychological responses triggered by the label, such as conveying the significance of investing in children, should be alleviated.

Even though we could not detect small income effects, we can rule out a 6 percentage point decrease in paid work participation, a 4.7 reduction in hours worked per week, and a $1500

[18] In other words, even though mothers knew that they could use the money in any way, some may have felt the need to spend it on their children, and labor supply responses might potentially be affected by this nudge.

reduction in maternal earnings (i.e., $125 per month). The main specification in our analysis yields a point estimate of -2.8% employment reduction over the study's first four years. Given that the cash gift represents a 17% increase in household income, our estimate corresponds to an elasticity of -.16. Although not statistically significant, this result is qualitatively similar to elasticities estimated with other income changes such as the -.16 elasticity found for single female heads of households in Negative Income Tax experiments (Robins, 1985), the -.14 elasticity of low-income single mothers in New Hampshire as a response to a change in TANF benefits (Freedman and Kim, 2022), or rough midpoints of -.12 for married women, and -.085 for single mothers used in the NAS report (National Academies of Sciences, 2019) based on Blundell and Macurdy (1999) review of tax changes across the 1970s and 1980s in the U.S. and Europe. While also not statistically significant with our sample, the estimated marginal propensity to earn out of unearned income was -.15 for maternal earnings and -.10 for household earnings. These estimates align with the prior literature on lottery winners and negative income tax experiments, and are smaller than the estimates reported by Golosov et al. (2021) for US lottery winners with similar annual incomes. We also ruled out the large elasticities found by Jensen and Blundell (2024).

Finally, by tracking both labor supply responses as well as impacts related to child development, the BFY study of unconditional cash benefits contributes to our understanding of both the benefits for children and the possible behavioral costs of such a policy (Aizer et al., 2022). And, despite concerns about its statistical power, this study makes an important contribution by estimating income effects for the sample population of low-income mothers with young children, which has received little attention in the literature and is subject to increasing public attention in the context of child allowance discussions, such as the expansion of child tax credits.[19]

[19] Adjustments to the child tax credits change work incentives, leading to a combination of both income and substitution effects.

Chapter 3

The Effects of North Carolina's Pre-K Program on the Childcare Market

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3.1 Introduction

State-funded pre-kindergarten programs (pre-k) have been growing constantly in the United States for the last two decades. They currently enroll about one-third of all 4-year-old children and constitute a financial investment of more than $9.5 billion in 2021-2022 (Friedman-Krauss et al., 2023). Current policy discussions suggest that these investments will continue to grow in the following years.[1] Despite their recent growth, state-funded pre-k centers are only one of several key actors in the broader early childhood education system, which includes a wide range of public and private childcare providers serving infants to preschoolers.[2] Hence, one natural question that follows its expansion is: how does an increase in state provision of pre-k education affect enrollment structures of all (state-funded or not) local providers?

The incorporation of state-funded pre-k slots may affect all childcare providers in the local market. Intuitively, state-funded pre-K education can be considered a subsidy that increases the number of free slots for four-year-old eligible children in centers that offer the program. This can attract eligible children who would not attend these centers otherwise. To accommodate such increased demand, pre-k providers that offer subsidized slots may increase their capacity and serve more children. Moreover, under optimization frictions such as capacity constraints, they might also respond by changing the classroom composition, depending on their preferences for subsidized versus non-subsidized slots and how pre-k eligible children are assigned to centers offering the program. The increase in state-funded pre-k slots near

[1] Friedman-Krauss et al. (2023) state that four states passed laws to provide universal preschool in 2022 (California, Colorado, Hawaii, and New Mexico); two state governors have announced their support and increased funding (Michigan and New Jersey); while other states are increasing their enrollment and funding to improve its quality (e.g., Alabama and Rhode Island).

[2] In 2002, 32% of four-year-old children attended a state-funded pre-k program; 6% were in a Head Start program, 3% in a special education program, 2% in another public program, and 57% were in either a private setting or not enrolled in any early childhood education program (Friedman-Krauss et al., 2023). On the other hand, the federal government offers funding to states in the form of Child Care Development Funds to subsidize childcare for low-income children contingent on parental work, as well as Social Services Block Grants and Temporary Assistance for Needy Families (TANF), which states could also use to subsidize childcare for low-income children.

them might also indirectly affect centers that do not offer subsidized slots. On the one hand, they could lose enrollment if eligible children switch to other local providers offering subsidized slots. On the other hand, they might face increased demand from ineligible children displaced from other pre-k centers that now offer subsidized slots and accommodate more eligible children. Ultimately, the overall effect is not theoretically clear.

This study provides empirical evidence to answer this question in the context of the North Carolina Pre-Kindergarten (NCPK) program, which offers subsidized pre-k education slots for children from low-income families. There is variation in how states design their pre-k programs. The NCPK is a program with a mixed-delivery system, i.e., state-funded pre-k slots can be offered in public and private childcare centers (a.k.a. community-based organizations). For simplicity, from now on, I will refer to centers offering subsidized NCPK slots as NCPK centers. The NCPK program started in 2001 as a pilot program in 100 classrooms and quickly expanded to all counties in the state. By 2012, even though the total number of NCPK centers at the state level had stabilized, there were still several underserved areas. New providers continued joining the program every year across all North Carolina counties. The typical NCPK expansion case constitutes an existing center that starts offering subsidized NCPK slots. Alternatively, there are also some new center openings under the NCPK program. North Carolina is an ideal context to study this question, given that its state-funded pre-k program has been in place for over two decades, it serves around one-fourth of all four-year-old children in the state, it has been recognized for its positive effects on child outcomes, and it has a high-quality data system.

To answer how NCPK might affect enrollment structures of NCPK and non-NCPK childcare providers, I exploit geographical and time variation in the rollout of the NCPK program, jointly with a rich combination of administrative childcare provider-year-level records. There are two primary sources of data. First, enrollment data and provider-level characteristics were obtained from the North Carolina Department of Health and Human Services (NCD-

HHS). For monitoring purposes, NCDHHS collects information on all licensed child-care providers in North Carolina through unannounced visits made at least once yearly by government officials. Second, specific information corresponding to NCPK centers was collected and provided by the University of North Carolina at Chapel Hill, which was in charge of monitoring the NCPK program. Finally, I also rely on data from the American Community Survey 5-year estimates at the census block level for socio-demographic characteristics of the relevant areas.

The research design is based on a difference-in-differences design that exploits the staggered and geographically heterogeneous incorporation of childcare centers to the NCPK program between 2012 and 2018 - the period for which the most detailed data is available. Specifically, I combine an event study via a two-way-fixed effect strategy with a ring design to define treatment and control areas (Currie and Walker, 2011). In short, this research design compares enrollment in childcare centers that had an NCPK center within a radius of 0.5 miles with presumably similar centers located a bit further away from an NCPK center (between 0.5 and 1 mile) before and after the center began offering NCPK slots. While I discuss this in detail in Section 3.5, the chosen size of the ring is consistent with recent related work that shows that childcare spillovers happen at a very local level (Brown, 2018). Therefore, the key identification assumption is that the timing of NCPK establishment is uncorrelated with other determinants of changes in enrollment in the area.

The empirical analysis leads to three main sets of results. First, when joining the program, NCPK centers concentrated more on serving eligible four-year-old children. On the one hand, they significantly increased the enrollment of four-year-olds by about 36%. On the other hand, the share of four-year-olds among all the children served in their centers also increased by about ten percentage points. Furthermore, there is suggestive evidence of a slight decrease in the absolute number of 0-3-year-old children enrolled in NCPK centers. Overall, these results indicate that when joining the program, NCPK centers reformulated their enrollment structure towards a larger share of the eligible population to the detriment of the ineligible population (either non-eligible four-year-old or 0-3-year-old children).

Second, when NCPK centers began offering the program, other non-NCPK childcare providers in their vicinity were also affected. Specifically, non-NCPK childcare centers within half a mile of an opening NCPK center increased the enrollment of four-year-old children by 25% and three-year-old children by almost 20%. These results are robust to several specifications, such as changes in the sample definition, model specification, and control/treatment ring size definition. Moreover, the effects are negatively associated with the distance: facilities located within shorter distances of the NCPK center had more significant increases in enrollment, and these effects quickly diminished and disappeared by about a 0.75-mile distance. Together, the first and second sets of main results suggest that ineligible three- and four-year-old children were crowded out from NCPK centers to non-NCPK centers in the local area.

Third, the growth in four-year-old enrollments in nearby facilities is explained almost exclusively by centers in lower-income areas. In other words, in lower-income areas, the increased demand from ineligible NCPK children faced by non-NCPK centers more than offsets what they lose in NCPK-eligible children who are switching to NCPK centers. This is not true for higher-income areas, where both forces seem to offset each other, meaning that overall enrollment remains unchanged. These results are consistent with the fact that low-income children are less likely to be in center-based childcare arrangements than their higher-income counterparts (U.S. Department of Education, National Center for Education Statistics, 2021; U.S. Department of the Treasury, 2021). It suggests that, in higher-income areas, NCPK is more likely to be substituting childcare from another provider than home-based or informal arrangements, which are more prevalent in low-income families.

Finally, an important implication of these findings is that, unintentionally, NCPK might have increased income segregation across childcare centers. While, unfortunately, I cannot observe the characteristics of the children enrolled in each center, the changes in enrollment counts imply that centers are altering the composition of their classrooms in a way that NCPK centers serve four-year-olds from eligible low-income families almost exclusively in four-year-old classrooms, while higher-income ineligible children are displaced to non-NCPK centers.

This study contributes to three strands of literature. First, I contribute to the literature evaluating childcare market responses to pre-k program expansions and, more specifically, an emerging literature looking at how they affect the availability of childcare for ineligible children. Two studies of universal pre-k programs —in Florida and New York City— showed that increasing pre-k slots for four-year-olds can unintentionally decrease the availability of care for three-year-olds (Bassok et al., 2016) and children younger than two (Brown, 2018). Brown (2018)'s findings also suggest that these unintended effects on younger children may happen because pre-k centers crowd out four-year-old children from other facilities, who are the more profitable group for childcare providers. On the other hand, Bassok (2012) found that, as a response to state pre-k expansions, Head Start centers have been able to adapt and switch to serve younger children.[3]

In contrast to these studies, North Carolina's high-quality data system allows me to provide a complete picture of the childcare market across all ages and childcare settings, which can help illustrate the mechanisms behind these responses. For instance, Bassok et al. (2014) found that the universal pre-k programs in Georgia and Oklahoma increased the number of childcare centers and showed that there was crowd-out from private centers in Oklahoma, where pre-k was expanded only through public schools. However, they do not have information on the number of children in each childcare setting. High-quality administrative records also allow me to measure effects on enrollment counts as opposed to facilities' licensed capac-

[3]The federal Head Start program, which started in the 1960s, has been the primary preschool provider for low-income families in the US for a long time. However, in the last decade, funding for state-based pre-k programs has substantially increased and surpassed investments in Head Start.

ity, which may be a more relevant margin of response. The monitoring agency usually defines licensed capacity as the number of children the childcare facility is authorized to care for. It is computed based on the facility infrastructure (e.g., characteristics of the primary space, outdoor space, toilets, etc.), the age group composition, and staff-to-child ratios. Hence, it will be less sensitive to changes in childcare demand and might fail to capture responses in actual enrollment when facilities are not operating at total capacity.

An additional advantage of focusing on the NCPK setting is that, to my knowledge, all the existing evidence on pre-k crowd-out effects comes from universal pre-k programs. Despite its relevance, these effects might not generalize to targeted programs, which are most common nationwide (Friedman-Krauss et al., 2023). For instance, crowd-out concerns between providers should not be as relevant if pre-k is enrolling children who would not participate in any formal childcare arrangement in the absence of the program. In fact, crowd-out between private and public providers is more likely to happen among higher-income families (Cascio and Schanzenbach, 2013); however, low-income families are less likely to attend formal childcare centers (Herbst, 2022), and therefore, a pre-k program targeted at low-income families could potentially lead to different market dynamics. This study shows that in the context of a targeted program, adding pre-k slots did not come at the expense of infant and toddler slots. Additionally, I show that the program increased childcare access, which was larger in low-income areas due to a response in both pre-k and non-pre-k centers.

Second, I contribute to the literature on optimal design and effectiveness of state-funded pre-k programs. There is growing concern about why current early education programs yield smaller or more conflicting results than early preschool demonstration studies, such as the successful Perry preschool study (Whitaker et al., 2023). One likely explanation is that counterfactual conditions have changed over time: preschool programs now serve children who would otherwise be in a similar arrangement. My results support the hypothesis that, at least in higher-income areas, NCPK serves children who would otherwise attend formal childcare centers. The results are also in line with Kline and Walters (2016), who show that the Head Start program also crowds out children from other similar arrangements ("close substitutes"). They show that while Head Start did not have positive overall effects on child outcomes, it did generate positive effects for children who would otherwise not attend preschool. Consistently, Watts et al. (2023) found that NCPK had more significant effects on the academic achievement of low-income children. Combining the results with those in this study, I suggest that a likely explanation for this income heterogeneity is that higher-income children who attend the program but still under the income eligibility threshold, in the absence of NCPK, would have been served by closely substitute childcare providers.

Finally, I contribute to the literature on education systems and economic segregation. While school segregation in K-12 education has been widely studied (Owens et al., 2016), little is known about preschool economic segregation. Similarly to later grades, and given that proximity is a primary factor for childcare decisions, neighborhood segregation is expected to lead to high economic segregation in childcare centers. However, this study highlights an institutional factor, a targeted pre-k policy, that can unintentionally increase economic segregation in an educational context. This is driven by the fact that providers that joined the NCPK program shift their enrollment structures disproportionately towards low-income eligible children to the detriment of ineligible children. It is noteworthy that, given that lower-income children are being concentrated in a high-quality setting (because of the NCPK high-quality standards), this segregation does not necessarily lead to a difference in the quality of services received. However, there are other reasons why preschool segregation may still be problematic. Attending a more diverse education setting can increase the likelihood of having diversified social networks, alter perceptions around social differences, and affect social preferences (Londoño-Vélez, 2022), even as early as the preschool years (Cappelen et al., 2020).

The results in this paper are not only academically relevant but have strong policy implications. As many states are passing legislation and showing efforts to expand their prekindergarten programs (Friedman-Krauss et al., 2023), many have concerns about how these policies will be implemented and how childcare providers will respond. Given the fragmented nature of the early childhood education systems in the United States, the reliance on private centers, and the combination of private and public funding streams (Duer and Jenkins, 2023), this study suggests that when states decide to expand childcare and education for four-year-olds, they should also contemplate the changes that these programs may trigger in the whole childcare market and how certain spillover effects might be associated with the program design features. This study shows that concentrating subsidized pre-k through slots in existing facilities can lead to increased access to childcare and an enrollment reshuffle between centers that increases economic segregation.

In particular, this paper sheds light on two policy decisions in the context of targeted pre-k programs: (1) in which areas should subsidized slots be allocated, and (2) within an area, which centers should provide them. As to the first point, results support the hypothesis that in higher-income areas, pre-k slots are more likely to substitute other formal arrangements and increase economic segregation across childcare centers. As to the second point, even in low-income areas, concentrating many subsidized slots in the same providers while increasing access might also come at the expense of increased segregation. However, it is noteworthy that relying on centers that already participate in the program can facilitate its administration and make it easier to serve more children since it does not require finding more high-quality centers that meet the program requirements. Hence, when making slot allocation decisions, policymakers should consider these trade-offs.

The rest of the chapter is organized as follows. The conceptual framework is presented in Section 3.2; in Section 3.3, I describe the NCPK program's main characteristics; Sections 3.4 and 3.5 describe the data and analytical strategy, respectively; in Section 3.6, I present the main results, which are further analyzed in Section 3.7, and conclude in Section 3.8.

3.2 Conceptual Framework

The introduction of state-funded pre-k slots can affect both state- and non-state-funded pre-k centers. In this section, I discuss a conceptual framework based on the potential mechanisms that might operate in a setting where a targeted state-funded pre-k program for low-income children is introduced in a local pre-existing childcare market. The state-funded pre-k program offers free childcare slots to four-year-old children in childcare facilities that serve children aged 0 to 4 years before joining the program. This is how most pre-k programs operate in the United States (Friedman-Krauss et al., 2023). For simplicity and without loss of generality, in what follows, I will refer to centers that offer fully subsidized state-funded pre-k slots as NCPK and centers that do not offer these subsidized slots as non-NCPK.

3.2.1 Expected Transformations of NCPK Centers

One intuitive way to think of this type of policy is the introduction of a subsidy that reduces the price paid for daycare services by the eligible population. Participating NCPK centers will, by definition, offer free slots. If NCPK centers can accommodate as many children as they want, they are expected to increase the overall enrollment of the eligible four-year-old population.

Under optimization frictions (e.g., capacity constraints), NCPK centers might respond by changing the classroom composition. This restructure will depend on NCPKs' relative preferences for subsidized versus non-subsidized slots and/or the NCPK allocation system (i.e., how NCPK-eligible children are assigned to centers offering the program). For instance, centers offering NCPK slots might prefer enrolling NCPK-eligible children compared with non-NCPK children because it is easier to collect tuition and fees from the government than collecting them from each family individually. Alternatively, from a policy-administration perspective, NCPK officials might prefer to concentrate the NCPK slots in specific centers. In both cases, NCPK centers will tend to increase their share of NCPK-eligible children. In other words, to accommodate more NCPK-eligible population, **NCPK centers will reduce the enrollment of children who are not eligible either because of their age (i.e., younger children) or other individual characteristics (e.g., in targeted programs, higher-income 4-year-old-children).**

3.2.2 Expected Transformations of non-NCPK Centers in the Local Market

Non-NCPK centers might be indirectly affected by NCPK expansions. On the one hand, non-NCPK centers might face increased childcare demand from ineligible families who are displaced from centers that now offer the NCPK program. On the other hand, they could also lose 4-year-old enrollments from eligible families now switching to NCPK providers. Theoretically, the overall effect could go either direction, depending on which force predominates.

Scenario A: Increased demand from non-NCPK children $<$ Loss of NCPK children

This could be the case if non-NCPK providers used to enroll many four-year-old NCPK-eligible children who NCPK centers are now serving. In this scenario, the loss of NCPK-eligible children could be higher than the increased demand from displaced ineligible children coming from NCPK providers, leading to a **net decrease in the enrollment of four-year-old children in non-NCPK centers in the local market**.

However, in this scenario, **the expected effect on the enrollment of younger chil-**

dren is ambiguous. Because childcare provision is more costly when children are younger (Brown, 2018), childcare centers typically cross-subsidize care for infants and toddlers with the revenue obtained from their enrollment of older children. Therefore, when they lose enrollment of four-year-old children (the more profitable group), centers might be financially affected and need to either decrease the quality, increase the price for younger children, or run out of business. If providers run out of business, there would be a reduction of available seats for younger children in the area. However, if families are still willing to pay a higher price, the enrollment of younger children might be unaffected or increase. In fact, if NCPK centers decide to enroll NCPK-eligible children at the expense of younger children (i.e., changing the age structure of the population they serve), local non-NCPK providers might experience an increased demand for younger children seats from families who lost their slots in the newly established NCPK centers.

Scenario B: Increased demand from non-NCPK children > Loss of NCPK children

This could be the case if new NCPK centers used to enroll many ineligible NCPK children and shifted their focus strongly to serving NCPK children who were not enrolled in other childcare arrangements before the program. If this is the case, the loss of NCPK children faced by non-NCPK centers might be more than offset by the increased demand driven by children whom new NCPK centers are letting go after joining the program. Under this scenario, the introduction of a new NCPK center is expected to **increase the enrollment of 4-year-old children in non-NCPK providers**.[4]

As to the enrollment of younger children, as long as NCPK centers change the age structure of the population they serve, local non-NCPK providers will experience an increased demand

[4] This increased demand from ineligible NCPK children might also lead to an increase in the price charged by local private childcare centers. However, this is out of the scope of this paper.

for seats for younger children. As opposed to scenario A, since there is a positive net gain of 4-year-old enrolled children, **the predicted effect on the enrollment of 0- to 3-year-old children is positive**.

Scenario C: Increased demand = Loss of pre-k eligible children

Finally, the effects on age-4 enrollment in nearby non-NCPK facilities may be null when the increased demand from NCPK-ineligible children and the loss of NCPK-eligible children are similar. Regarding younger slots, and similar to Scenario B, if NCPK centers are not enrolling as many 0- to 3-year-old children as they would otherwise, non-NCPK centers face an increased demand for younger slots. Then, the expected outcome would be an increase in the enrollment of younger children.

Summary and additional considerations on socio-economic segregation

In the end, the effect of a new NCPK center on enrollment structures in other local non-NCPK centers is theoretically ambiguous and depends on several factors, such as how well NCPK providers can adapt to increased demand, what would have been the childcare arrangements of the NCPK population in the absence of NCPK slots, and the characteristics of the childcare market before the NCPK program. These materialize into two competing forces: the loss of NCPK-eligible children and the gain of non-NCPK-eligible children. For instance, age-4 enrollment in non-NCPK centers will increase if the increased demand driven by displaced ineligible NCPK children more than offsets the loss of NCPK-eligible children who are now shifting to NCPK centers. For younger-age enrollment, the effect will also be determined by how well childcare facilities might adapt their financial structures to the increase in operational costs associated with serving younger children.

An additional consideration on the potential effects of newly introduced NCPK centers regards the possible effects on socio-economic segregation across centers. Since NCPK eligibility relies on income, the reallocation of children into different childcare centers is expected to change socio-economic diversity within the provider. In particular, if NCPK centers concentrate on serving eligible children, they will serve more lower-income children. The opposite is true for non-NCPK centers, which will now receive more ineligible children. This represents an additional layer of economic segregation in education driven by institutional factors on top of the one caused by neighborhood segregation.

3.3 The North Carolina Pre-Kindergarten Program

States across the United States have followed different approaches to designing their pre-k programs. In this section, I provide more details about North Carolina's approach.

Program Overview. NCPK is a state-funded program that offers subsidized early education to four-year-old children from low-income families. The program's main goal is to better prepare children for their transition to kindergarten in terms of their overall well-being and academic readiness. Any licensed childcare center that meets a series of requirements described below can offer NCPK slots and get reimbursed for the eligible children they serve. Hence, NCPK slots are offered in private centers (a.k.a., community-based organizations, including non-profit, for-profit, Head Start centers, religious childcare centers, private schools, etc.) as well as public school buildings (as long as they have preschool classrooms for four-year-old children).

Program History. NCPK launched in 2001 as a pilot program in 100 classrooms under the name of More at Four and was gradually expanded until today (Appendix B.1 shows the number of centers joining and leaving the NCPK program between 2005 and 2018). In the first two decades, funding and enrollment in the program grew substantially during the first ten years and remained mostly stable during the 2010s. Despite the stability in the total number of NCPK centers in the post-2010 period, the expansion dynamics of NCPK still implied that many underserved areas faced NCPK openings for the first time. One figure that illustrates the importance of NCPK at the state level is that by the end of this study period, about 25% of all four-year-old children in North Carolina attended NCPK.

Existing Evaluations. Several studies have demonstrated that NCPK is effective not only in improving children's school readiness at the beginning of kindergarten (Peisner-Feinberg et al., 2019; Peisner-Feinberg and Schaaf, 2011) but also in boosting academic skills through the elementary school grades (Dodge et al., 2017; Ladd et al., 2014; Watts et al., 2023).

Child Eligibility. A key characteristic of the NCPK program is that it funds slots that should be filled with eligible children instead of funding entire classrooms or centers. NCPK targets four-year-old children from low-income families, i.e., with a household income below 75% of the state median income. In addition, up to 20% of slots could be filled with children who do not meet the income requirement but present developmental disabilities, have limited English proficiency, have educational needs, have chronic health conditions, or belong to military families. Roughly half of the four-year-old children in NC are eligible to participate in the program; however, only around 50% of them do it. This is still below the program's goal, which aims to serve at least 75% eligible children.[5]

NCPK Administration. NCPK is administered by "contractors" who receive funding from the state and oversee the program under their jurisdiction. Typically, there is one contractor per county, and each of them must have a committee representing relevant members of the early childhood education community (e.g., public schools, Head Start, private

[5]A possible explanation for the NCPK expansion slowdown is cost. NCPK contractors and providers willing to participate in the NCPK program usually need to find additional funding. In 2018, participating centers received approximately $5,500 for each NCPK slot. This amount represents about 60% of the actual cost; hence, contractors usually complement NCPK funds with other sources such as Smart Start, Head Start, Title I, or others (Barnett, 2018). The low share of the cost reimbursed by the state, the high standards required to participate, the shortage of qualified teachers, and the physical space needed to serve more children are some of the main difficulties in expanding the program.

childcare providers, and referral agencies). While there is flexibility and variation in how contractors implement the program, in most cases, program application and placement of children in centers is handled centrally at the contractor (county) level.

NCPK Providers. To receive NCPK funds, participating centers should meet a list of requirements that guarantee the high quality of the program. These include having a 4- or 5-star rating in the Quality Rating and Improvement System (QRIS), licensure requirements for administrators and teachers (e.g., Birth-through-Kindergarten or Preschool add-on standard licensures for lead teachers), providing at least two meals, following an approved preschool curriculum, implementing formative assessments, and meeting a 1:9 class ratio, among others. Currently, there are more than 1,000 centers that offer NCPK. 48% of them are public schools, 38% are private child care centers, and 14% are Head Start centers. These numbers vary considerably between counties (e.g., some contractors may choose to locate all their slots in public schools).

3.4 Data

To conduct this analysis, I use data at the childcare provider-year level. In short, I created and geo-coded a panel dataset of all licensed childcare providers in North Carolina. I located centers that joined the NCPK program between 2012 and 2018 and observed childcare centers around them. In this section, I describe the data sources, how the sample is defined and constructed, and an initial overview of the analytic sample.

3.4.1 Data sources

Enrollment data and provider characteristics. Information on all licensed childcare providers in North Carolina is collected by the North Carolina Department of Health and

Human Services (NCDHHS) for monitoring purposes. For this analysis, I use provider-level data from Statistical Reports publicly available on the NCDHHS website. These reports are available starting in 2005 and include license ID, facility name, facility type, and the number of enrolled children by age. While reports are generated and posted monthly, the data only changes when enrollment information is collected during a visit, and data is entered into the facility record. Following North Carolina's Child Care Rules, the Division of Child Development and Early Education makes at least one unannounced visit annually and additional unannounced visits when there is a complaint.[6] Even when no complaints are received, there is usually a second monitoring visit scheduled mid-way through the cycle. Hence, typically, visits are conducted a minimum of 1-2 times each year. Given this pattern of monitoring visits, I use data from one report per year.[7] These records are used to create the outcome variables used in the analyses.

NCPK centers. Participation in the NCPK program comes from NCPK program monitoring data (MAFREPS, which stands for More at Four Reporting System). These data were collected at the University of North Carolina at Chapel Hill and made available for this study from 2005-2006 to 2018-2019. I use these records to identify the treatment "shocks" used for the identification, i.e., when an NCPK center begins offering NCPK slots. We define the initial year as the year the center is observed in the MAFREPS data for the first time.

Childcare provider addresses. A key piece of information for a geographic analysis is mapping out the facilities. Addresses for all childcare providers were requested and provided by the NCDHHS for 2012 to 2022. The MAFREPS data also includes addresses for NCPK centers for all available years (2005-2018). After geocoding both sources, I merged them based on their spatial location.

Socio-economic environment. In addition to provider-level information, in some cases,

[6] For more information, see "Chapter 9- Child Care Rules" from the North Carolina Department of Health and Human Services: `https://ncchildcare.ncdhhs.gov/services/licensing/getting-a-license`

[7] Data correspond to the September report each year from 2012-2022, and from May reports in 2005-2011.

the empirical analysis relies on characteristics of the local area, which I obtained from the American Community Survey 5-year estimates at the Census Block level.[8]

3.4.2 Sample construction and Definition of Treatment and Control Areas

Data Cleaning and Geocoding. Childcare providers were geocoded using the US Census Batch Geocoding Tool and ArcGIS. Data from the two main sources (NCDHHS and MAFREPS) were separately geocoded and then merged based on their spatial location. Given the data availability described in subsection 3.4.1, the final universe of childcare providers includes all childcare providers that had a license to operate between 2012 and 2022. If they were operating before 2012, I could also retrieve their data from previous years. [9]

Reference Centers. Treatment in this study is defined by offering NCPK slots. Because the strategy is to analyze what happens when a center joins the program, I identify "reference centers". These are childcare providers that began offering NCPK between 2012 and 2018, as indicated in MAFREPS data. Each reference center has an associated reference year, which is the year in which they first started offering NCPK slots. Even though I have NCPK information for earlier years, I limit the definition of reference centers to those that opened after 2012. This decision is because I cannot fully observe the entire childcare market around them during the previous years.

Ring construction (treatment and control groups). After geocoding all childcare providers and identifying reference centers, I calculate the distance between each childcare

[8]The decision of using 5-year estimates at the block level is due to the existing trade-off between the geographic and time precision of the ACS estimates. I chose to prioritize variation at the geographic level.

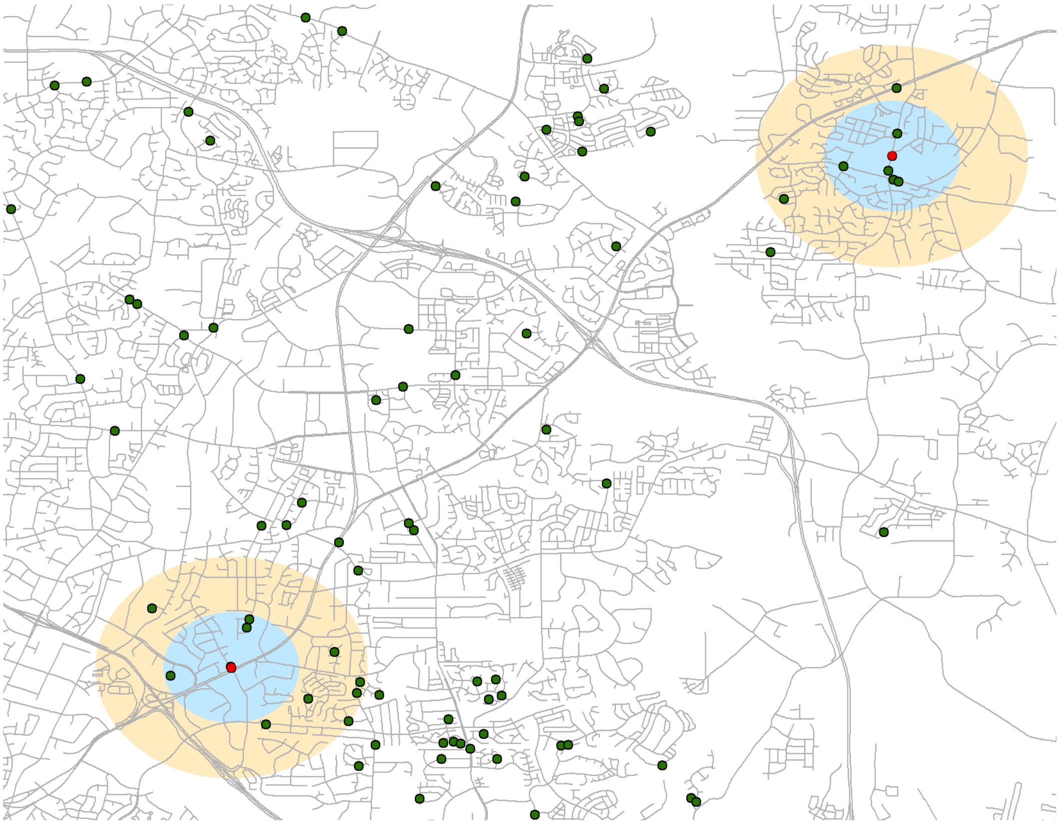
[9]There are some cases where the same childcare provider might have changed their license ID number before 2012. If that is the case, I cannot include their pre-2012 data.

provider and the closest reference center. After computing this distance, two groups are defined by drawing two concentric circles or rings around reference centers. On the one hand, centers within half a mile of a reference center are part of the "treatment" group, or *Ring 1*. On the other hand, centers that are between 0.5 and 1 mile away from a reference center are part of the "control" group, or *Ring 2*.[10] Despite being arbitrary, the choice of the ring size in this study is comparable to the distances used in recent related studies showing that childcare decisions happen at a very local level (e.g., Brown (2018)). Additionally, increasing the ring size can increase the likelihood of having overlapping rings, adding bias to the analysis. With the results, I show several sensitivity tests to ensure that the arbitrary ring-size choice does not drive the reported findings.

Example. Figure 3.1 shows an example of rings and group definition for one year. In this selected area, two centers began offering NCPK slots in 2016. Centers located in the blue region (within 0.5 miles) are treated, and centers in the yellow area (0.5-1 mile) are part of the control group. For all of them, 2016 will be their event zero, i.e., I will compare their outcomes before and after 2016. I implemented this approach for all NCPK centers that offered NCPK slots for the first time between 2012 and 2018 and their surrounding areas.

[10]See Currie and Walker (2011) for an implementation of the ring design, and Brown (2018) for a discussion of the method.

Figure 3.1: Example of centers that offered NCPK slots in 2016 for the first time, treatment and control group definition



Notes: This screenshot shows an area of North Carolina and all geocoded childcare providers (dots). In this area, in 2016, two centers started offering NCPK slots (red dots). Childcare providers within this year's study sample are those within 0.5 miles of the reference centers (in the blue area) and those between 0.5 and 1 mile (yellow area). The remaining childcare providers are not included in the 2016 reference year.

Sample overview. The main analytic sample is composed of 229 childcare centers that were within half a mile (treatment group) and 490 centers at a 0.5 to 1-mile distance of reference centers.[11] These facilities were either open when the reference center began offering NCPK (91%) or opened later (9%). After measuring the distance to the closest reference center, I apply the following sample restrictions. First, I remove providers that, during the years considered, were eventually in both treatment and control groups in different years (n=85). This allows for a "pure" treatment vs control comparison. Second, I removed providers who

[11] A count of childcare providers in the treatment and control group by year is included in Appendix B.1

were treated more than once (n=27), avoiding the post-treatment period receiving more shocks.

An additional consideration is that I do not consider providers with a license to operate as family childcare homes. Due to differences in the characteristics of the population served and licensing requirements (e.g., lower maximum capacity), they are likely less affected by NCPK. In any case, I include them as a robustness check.

Finally, due to the significant impact of the COVID-19 pandemic on the childcare market (Zhang et al., 2023), I removed the years 2020-2022 from the analysis. While this decision eliminates measurement error, it creates a sample imbalance, i.e., areas where NCPK opened later have fewer post-treatment years. I confirm the robustness of the results in the appendix.

3.5 Analytical Strategy

For this analysis, I exploited when a center begins to offer NCPK slots (defined as "reference centers"). Then, I created two groups of childcare providers based on their distance to the closest reference center.[12] I implemented a dynamic Difference-in-Difference design, comparing the changes in enrollment in childcare providers that are located within half a mile of a reference center before and after the corresponding reference center began offering NCPK slots, controlling for the changes in enrollment in childcare providers that are just half-a-mile further away from them. This was estimated with the following main two-way-fixed-effects (TWFE) specification:

$$Y_{pt} = \beta_0 + \beta_1 RingOne_{pt} + \beta_2 POST_{pt} + \beta_3 (POST * RingOne)_{pt} + \beta_4 Distance_{pt} + \theta_t + \gamma_{rc} + u_i \quad (3.1)$$

where Y_i is the outcome variable for provider p in event t, $RingOne_{pt}$ equals one if the provider is located in a first ring, $POST_{pt}$ equals one if the year is after the reference center

[12] Currie and Walker (2011) uses a similar approach to estimate the effects of traffic congestion on infant health. This and other geographic approaches are further discussed in Brown (2018).

started offering NCPK slots, $Distance_{pt}$ is the linear distance to the closest NCPK provider, and θ_t and γ_c are year and reference center fixed effects.

In addition, one could analyze it dynamically by decomposing the before and after periods in different year dummies interacted with the group assignment:

$$Y_{pt} = \beta_0 + \beta_1 RingOne_{pt} + \Sigma_{j=-7}^{j=2} \pi_j 1(\pi_{pt} = j) + \Sigma_{j=-7}^{j=2} \pi_j 1(\pi_{pt} = j) * RingOne_{pt} + \beta_4 Distance_{pt} + \theta_t + \gamma_{rc} + u_i \tag{3.2}$$

where $1(\pi_{pt} = j)$ are events relative to the year in which the reference center offered NCPK slots for the first time (year 0). The year before the switch is omitted, i.e., year -1.

A starting point and a fundamental assumption behind this analysis is that location plays a key role in childcare decisions. Following this, I conceptualize the proximity to an NCPK center as an intensity of treatment: the closer to an NCPK center a provider is, the more likely it is to be affected by the policy. For simplicity, by drawing rings, I am assuming that there is an actual geographic limit.

A simplified first analysis would be to define an area considered "close" and look at what happens to childcare providers' enrollment numbers in this area in a given year. In other words, this would only look at one "treatment" group (or one ring) affected in one year, before and after the event. However, NCPK funding allocations may be associated with other policies or conditions of the community that can simultaneously affect the enrollment of children in other childcare facilities. To address this, I take advantage of the continuous expansion of the NCPK program. By comparing changes before and after NCPK openings in different years, I can isolate the effect of the NCPK opening from other forces that could be operating in a specific year.

The described event study approach would possibly be enough to identify a causal effect of NCPK. However, the possibility of adding a second ring or a control group provides another layer of strength to the analysis. The assumption is that facilities just a bit further away from an NCPK center are similar to those in Ring 1, but they are not affected by the reference center's NCPK status. If we observed that the enrollment in Ring 2 facilities is flat, it would be more convincing that changes in enrollment in Ring 1 facilities are due to the policy.

It is worth noting that, in the real world, there is no natural line to distinguish both groups. In section 3.6.3, I discuss the validity of this analytical decision further and test the results with different ring size definitions.

As highlighted by the new developments in the difference-in-difference literature, if treatment effects vary over time, the estimates derived from equation 3.2 could be biased (Goodman-Bacon, 2021), and the coefficient for a specific period might be contaminated by the effects from other periods. Given that the treatment is staggered and absorbing (i.e., once a center is treated, it remains treated), this setting fits into the framework of Callaway and Sant'Anna (2021). However, an advantage of this study is that the control group remains always untreated. I present Sun and Abraham (2021)'s alternative estimator in 3.6.3.

Finally, non-linear models could provide a better fit in the context of limited dependent variables, such as enrollment counts, compared with linear models (Wooldridge, 2023). I report estimates based on Poisson regressions in Appendix B.4.

3.6 Main Results

3.6.1 First Stage: Changes in NCPK Centers *(Reference Centers)*

332 centers offered NCPK slots for the first time between 2012 and 2018, including private community-based centers (n=214) and classrooms in public school buildings (n=118). In

the typical case, these centers were already offering childcare services: licensed childcare providers that meet the requirements can join the program and offer NCPK slots, for whom they receive funding from their NCPK contractor. In some other cases, although the program's goal was not to create new centers, new facilities (or facilities without a previous childcare license) might offer NCPK.[13] Based on NCPK administrative data, NCPK centers enrolled an average of 14 NCPK-funded children when they first joined the program.

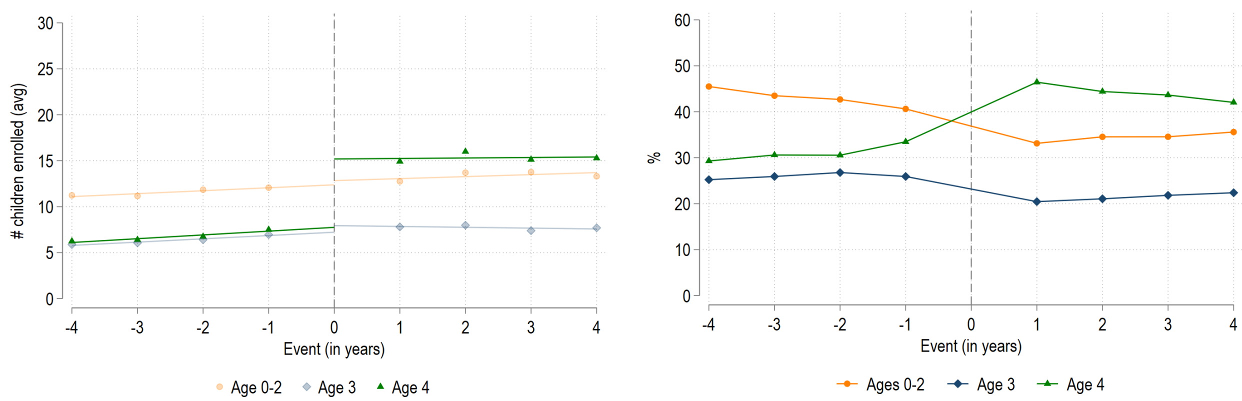
To understand what the treatment means for the surrounding childcare providers, it is important to understand the changes in enrollment in NCPK centers. While there is no clean comparison group for reference centers, one could take advantage of the variation in the year that they joined the program and see if trends change before and after this event. Taking all centers into account, NCPK centers duplicated four-year-old enrollments (Figure 3.2, Panel A). If we only consider centers that had been open for at least four years before joining NCPK, the enrollment grew from an average of fourteen to nineteen children (i.e., by 36%). Moreover, there was a change in the age composition of the centers: they increased the share of four-year-old children from about 30% to over 40%, while decreasing the share of 0- to 3-year-olds. In sum, NCPK centers increased the available seats in their promises to some extent but also changed the composition of the population served to receive more NCPK-eligible children.

[13] As shown in Figure B.2, the majority of private centers already existed when they joined the NCPK program (at event 0). Still, 15% of them were not operating in the previous year, and about 30% were not open four years before the event. This is different in public schools. While the schools are not new, the majority (76%) of public schools that joined NCPK did not have a childcare license before, suggesting that they did not have a preschool classroom. This distinction matters in understanding whether the treatment shock is only an increase in subsidized childcare or a combination of an increase in subsidized childcare and an expansion in childcare supply.

Figure 3.2: Changes in Reference Centers

A. Number of children enrolled in all reference centers

B. Composition of children enrolled in centers if they were open before joining NCPK



Notes: Panel A shows raw changes in enrollment in reference centers before and after joining the NCPK program. Panel B shows changes in the share of each age among all the 0- to 4-year-old children served in the center.

3.6.2 Effects on surrounding facilities

In this section, I present the results derived from estimating the two-way fixed-effects specification in providers that were located in areas where NCPK slots were introduced. First, Table 3.1 presents the results from Equation 3.1. Overall, when centers began offering NCPK slots, surrounding childcare providers experienced an increase in the enrollment of 3 and 4-year-old children of 17% and 25%, respectively. On average, centers located within half a mile of a reference center enrolled more than one additional four-year-old child ($\beta = 1.64, p < 0.01$) and one additional three-year-old child ($\beta = 0.93, p < 0.10$), compared with childcare centers that were located further away, but still within a mile distance. Moreover, four-year-olds' share among the enrolled children in these surrounding centers grew by about four percentage points ($p < 0.01$).

Table 3.1: Main effects of joining NCPK on nearby providers

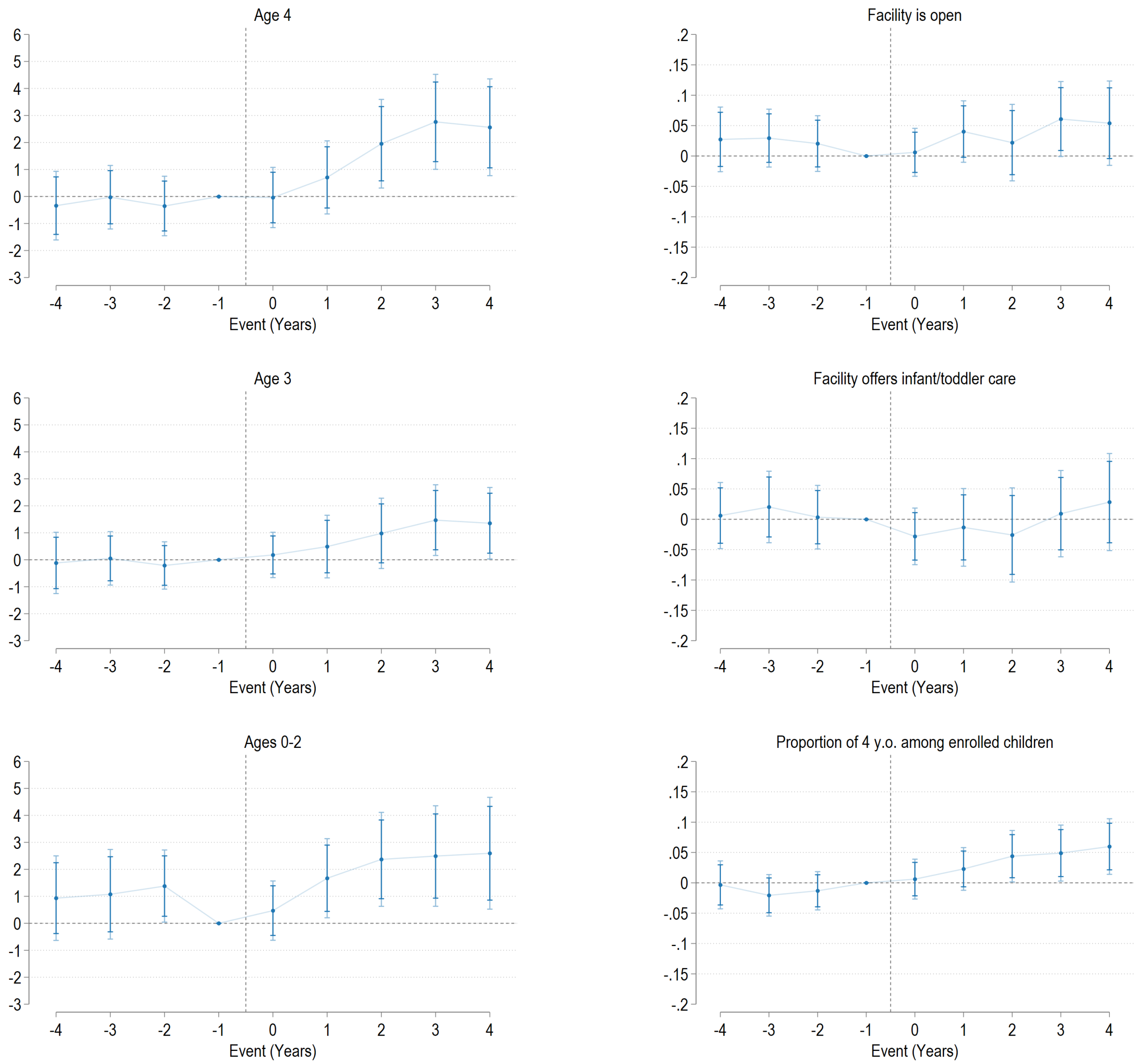
	# Children enrolled			Facility characteristics			
	Ages 0-2	Age 3	Age 4	Facility is open	Enrolls 0-3yo kids	Enrolls 4yo kids	Prop. of 4yo
Within 0.5 miles=1 × Post=1	1.03	0.93*	1.64***	0.02	-0.01	0.04	0.04***
	(0.79)	(0.47)	(0.52)	(0.02)	(0.03)	(0.03)	(0.01)
Post=1	0.56	0.34	-0.52	0.07***	0.06***	0.04*	-0.02**
	(0.47)	(0.33)	(0.36)	(0.01)	(0.02)	(0.02)	(0.01)
Within 0.5 miles=1	1.36	-0.62	-1.55	0.05	-0.00	0.03	0.02
	(2.08)	(1.15)	(1.64)	(0.04)	(0.05)	(0.05)	(0.04)
Observations	4435	5043	5355	5508	5508	5508	4696
Mean [post=0 & Ring1=0]	9.52	5.33	6.65	0.88	0.78	0.60	0.27

Notes: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. Standard errors in parentheses. This table presents the results from estimating Equation 3.1. Standard errors are clustered at the county level. The coefficient of interest is derived from interacting an indicator for being in Ring 1 and an indicator for the post-treatment years, i.e., after the corresponding Reference Center began offering NCPK slots. To facilitate the interpretation of the results, the last row shows the mean of the outcome variable in the pre-treatment years among providers in the control group.

Next, in Figure 3.3, I present the figures derived from estimating the dynamic specification (Equation 3.2). The corresponding table is included in Appendix Table B.2. The first thing to notice is that, in general, except for age 0-2 enrollments, which have more noise, all the pre-treatment event coefficients are not significantly different from zero. In other words, the parallel trend assumption holds.

The graphs also show that the effects on age-3 and age-4 enrollments are not immediate but grow slowly in the years after the event. This is not surprising. First, the data collection and registration process described in section 3.4.1 requires at least one year to be confident that all childcare providers have received a monitoring visit and updated their information. For instance, for providers for whom event zero is 2015, the event zero enrollment data corresponds to the Statistical Report of September 2015. This report includes the enrollment information based on the last monitoring visit, which could have happened some months earlier, e.g., in the spring of the academic year 2014-2015. Still, the shock is defined by the NCPK center that began offering NCPK in September 2015. For this reason, event zero should be interpreted cautiously and considered a lower bound. Secondly, even without measurement error, it is expected that some of the effects need some time to build up due to adjustment frictions in the childcare market.

Figure 3.3: Dynamic effects of offering NCPK slots on the enrollment in nearby providers



Notes: These figures are created by estimating Equation 3.2. The figures plot the coefficients of the interactions between each event (year) and "Ever Ring 1" (treatment group) indicator (Equation 3.2). The model also includes Reference Center and year fixed-effects, as well as non-interacted event (year) and "Ever Ring 1" indicators, and covariates. Standard errors are clustered at the county level.

3.6.3 Validation and Robustness Tests

One potential concern that arises given the two-way-fixed-effect setting with a staggered treatment is that there could be heterogeneity in the effects over time (de Chaisemartin and D'Haultfœuille, 2023; Roth et al., 2023). If this is true, the effects in other periods might contaminate the event coefficients. The figures in Appendix B.3 show that the results are, generally, heterogeneity-robust (Sun and Abraham, 2021).[14]

Another concern might be the use of linear models. Given that the main outcome variables are enrollment counts, which contain many zeros (e.g., if the provider is closed in a given year or did not enroll any child of the specified age), estimating a Poisson regression might be more appropriate. Following Wooldridge (2023)'s recommendation to compare the results from linear regressions to the Poisson regression, I found that the results are similar in both cases (see Appendix B.4).

On the other hand, the results might be sensitive to some of the decisions made around model specification, such as the use of covariates, the level of fixed effect, and sample restrictions. While some may decrease the effect sizes, the results presented in the previous section are not very sensitive to these changes (Appendix B.5).

Finally, the results could be sensitive to the definition of ring size. I explore this by changing the definition of ring size in two steps. First, I leave the treatment group fixed (at the baseline level, i.e., at a 0-0.5-mile distance) while changing the control group ring size (Appendix B.6). The risk of having a large second ring is that it becomes more likely to overlap with other treated areas. Still, the results remain robust when increasing the size of the control group area (Panel A). Second, I leave the control group size fixed while changing the treatment group size. As expected, the results are larger when the treatment group is defined within a

[14]The new estimation procedures increase the standard errors, and most of the point estimates lost significance. However, the trends shown for the TWFE specifications are still visible. The adjustments removed some of the pre-trends noise for enrollments of 0- to 2-year-old children.

shorter distance and closer to zero when the treatment area grows. This suggests that the effects are very local around the reference centers (within half a mile). In fact, measuring the effects in areas further away from the reference center can be interpreted as a placebo test.

3.7 Disentangling the results

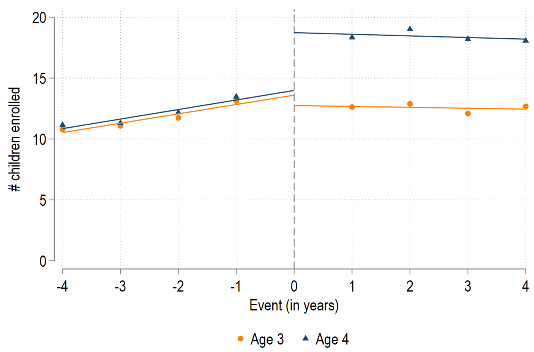
As presented in section 3.6, the opening of NCPK centers led to an increase in the enrollment of 3- and 4-year-old children in providers located within 0.5 miles. In this section, I present some supplementary analysis to understand the mechanisms.

These results are consistent with the hypothesis that NCPK led to a reallocation of children to centers. Based on this hypothesis, when childcare centers join the NCPK program and face capacity constraints, they "replace" ineligible children (either because of their age or income) with eligible four-year-old children. Nearby childcare might lose enrollment for low-income four-year-olds who switch to reference centers. Still, they appear to compensate for this loss by enrolling the children the reference centers are not serving anymore.

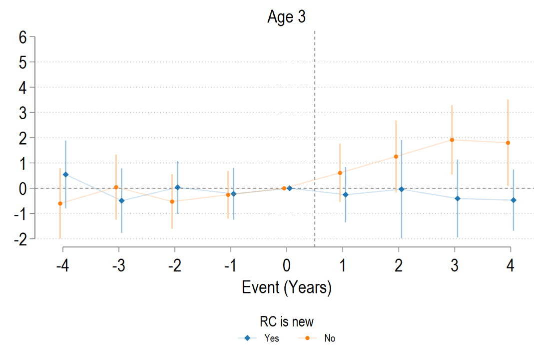
Supporting evidence for the reallocation of 3-year-old children comes from the finding that reference centers that already existed before joining the NCPK program decreased the number of slots they had allocated to three-year-olds (illustrated by the change in the enrollment trend, in Figure 3.4, Panel A). Moreover, the age-3 effect is driven by cases when the reference center was not new but existed before offering NCPK slots (Figure 3.4, Panel B).

Figure 3.4: Exploring mechanism behind age-3 results

Panel A. Changes in enrollment in Reference Centers that served three-year-old children



Panel B. Heterogeneity in age-3 effects by whether reference center is new

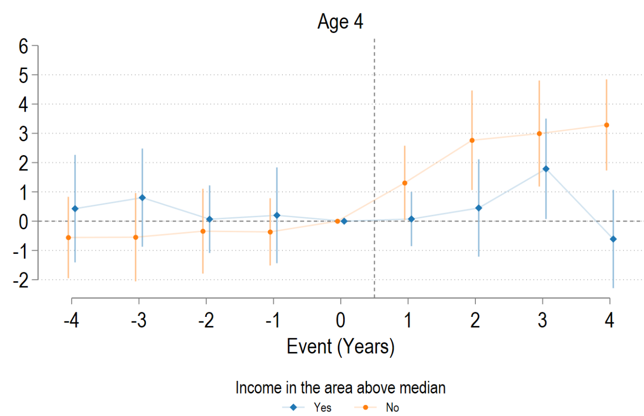


Notes: Similar to Figure 3.2, Panel A shows raw changes in enrollments in reference centers; however, in this figure, the sample is limited to centers that served at least one three-year-old children at event -1. Panel B plots the coefficients of the interaction between event and treatment group from Equation 3.2, estimated separately for providers that were near an NCPK provider that was already in place before joining the program and providers that were near an NCPK provider that did not have a childcare license before offering NCPK slots (i.e., as shown in Appendix Figure B.2, this is mostly public schools).

The mechanism behind the Age-4 effect is harder to test because we do not observe the characteristics of children enrolled in reference centers before joining the program (other than their age). Put differently, it is not possible to investigate whether, besides adding four-year-old seats, reference centers are serving fewer ineligible children. However, site-level data shows that even in the program's first year, most of the four-year-old children in NCPK classrooms are funded by NCPK (above 80%). This suggests that the number of slots available for non-eligible children must be reduced (or not growing as much as in the counterfactual condition).

As described in section 3.2, age-4 effects should be larger when NCPK centers enroll children who would not have participated in any formal childcare arrangement. Families from low-income backgrounds are less likely to participate in center-based care (U.S. Department of Education, National Center for Education Statistics, 2021; U.S. Department of the Treasury, 2021). Hence, we would expect that in lower-income areas, NCPK centers would enroll more children who would not have been in a childcare setting in the absence of NCPK. We observe that the age-4 results are driven by areas with household income below the median (Figure 3.5). This suggests that, in higher-income areas, NCPK is more likely to be substituting childcare from another provider than home-based or informal arrangements, which are more prevalent in low-income families.

Figure 3.5: Heterogeneity in age-4 results by area income



3.8 Conclusion

With growing awareness of the potential benefits of early childhood education, most states have been increasing their investments in preschool programs for four-year-old children. While universal pre-k is gaining attention, most states currently implement a targeted program for children from low-income families, usually delivered through existing childcare providers (Friedman-Krauss et al., 2023). Given the overlap and combination of different policies and funding streams directed to support the provision of childcare services (Duer and Jenkins, 2023), a natural question to analyze is how the childcare market responds to the addition of pre-k funded seats.

In this study, I found that offering subsidized NCPK slots for low-income families with children in North Carolina through existing public schools and private childcare centers increased access for the targeted age group. Each center that began offering NCPK slots between 2012 and 2018 served an average of 14 NCPK-eligible children. This led to an increase in the number of four-year-old children enrolled in NCPK centers by about 36%, and in surrounding non-NCPK centers (within 0.5 miles) by about 25%. This spillover can be explained by a market response to the high demand for the NCPK slots: to enroll more eligible children, ineligible children who would have attended those centers were crowded out to other nearby providers. To a lesser extent, three-year-old children were also reallocated to non-NCPK childcare centers. Notably, while increasing access to preschool, given the redistribution of children based on program eligibility, these results suggest that NCPK increased economic segregation across childcare centers. Future research may directly measure whether there was an increase in prices in nearby non-NCPK facilities, as well as the consequences of the increased segregation on child outcomes.

As mentioned, NCPK currently serves about 50% of the eligible 4-year-old population in North Carolina. What do these spillovers mean for future expansions of the NCPK program? NCPK centers seem to be operating at the limits of their capacity, meaning that, to increase program access, more centers should offer NCPK slots. Assuming that there is no treatment heterogeneity over time, a back-of-the-envelope calculation suggests that, for every 100 childcare centers that start offering the NCPK program, it will create 1200 new four-year-old seats in both NCPK and surrounding non-NCPK childcare providers. Additionally, about 90 three-year-old children are crowded out to non-NCPK childcare centers.

A key takeaway for policymakers is the importance of considering the different responses that the design of the pre-k programs might trigger. In contrast to Brown's study of Universal Pre-K in New York (Brown, 2018), I do not find evidence of crowd-out from younger children in the context of a targeted program. The main explanation is that non-pre-k centers still have a large demand from pre-k ineligible children they can serve. However, it leads to economic segregation through the reallocation of children to centers and the high concentration of eligible children in pre-k centers. A policy recommendation to mitigate this effect would be to cap the proportion of pre-k-funded slots that a pre-k center can offer. Because this will mechanically decrease the number of children that can be served, it must be accompanied by an expansion in the number of centers that provide the program, e.g., by allowing licensed family childcare homes to offer pre-k slots (Harmeyer et al., 2023).

Chapter 4

The Long-Term Effects of Pre-Kindergarten on Teen Births

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4.1 Introduction

While declining, teen birth rates in the United States are still very high. In 2021, there were 16 births for every 1,000 females between 15 and 19 years old, which is 8th highest among 38 OECD members[1](World Bank, 2024). Women who were teen mothers are substantially more disadvantaged than women who were not, and children born to teen mothers experience worse outcomes compared with children born to older mothers (Hoffman and Maynard, 2008; Kearney and Levine, 2012). However, in contrast to traditional views, there is no strong evidence to support a direct effect of teen childbearing on later outcomes. Instead, as argued by Kearney and Levine (2012), teen childbearing is a marker of a broader social problem, in which limited social and economic opportunities lead to worse economic trajectories *and* increase the likelihood of teen childbearing. Hence, human capital investments such as early childhood education, through increasing access to better opportunities, could be a way of reducing teen birth rates (Kearney and Levine, 2014). This paper addresses this question by analyzing the effects of the North Carolina state-funded pre-kindergarten program (NC pre-K) on teen childbearing.

Several studies have demonstrated that receiving public benefits during the early childhood years can have long-lasting effects that are still observed in adulthood (see reviews by Almond et al. 2018 and Hoynes and Schanzenbach 2018). Existing large-scale programs like the Supplemental Nutrition Assistance Program (Bailey et al., 2020), the Earned Income Tax Credit (Barr et al., 2022), and Medicaid (Brown et al., 2020) have been found to improve human capital accumulation and economic self-sufficiency, among other long-term outcomes. However, current early childhood *education* programs have more nuanced results.

The most expanded and studied program, Head Start, has shown positive long-term effects

[1] The OECD countries with a higher adolescent fertility rate are Colombia (59), Mexico (54), Costa Rica (37), Slovak Republic (26), Chile (24), Hungary (22), and Turkiye (17). North Carolina, the context of this study, had a teen birth rate of 16 in 2021, similar to the US average.

into adulthood (Deming, 2009; Garces et al., 2002; Bailey et al., 2021), even reaching the next generation (Barr and Gibbs, 2022). Still, the mechanisms are unclear, especially considering the convergence in academic performance during elementary school found in the Head Start Impact Study (Puma et al., 2010, 2012)[2]. In addition, most US states have expanded pre-kindergarten (pre-k) programs for four-year-old children. Long-term evaluations of these programs are still scarce. This is partially because, as opposed to Head Start, state-funded pre-k programs are much newer, and participants are still young today, limiting our ability to evaluate the effects on adulthood. However, long-term evaluations of pre-k programs are essential because pre-k is now the most extensive ECE policy in the country, with more than one-third of all four-year-old children attending a state-funded pre-k program (Friedman-Krauss et al., 2023). Moreover, the effects of Head Start might not necessarily be generalized to pre-kindergarten programs since, in contrast to Head Start, pre-K is usually designed to improve school readiness and academic success, with less of a focus on other components of the child's environment. One of the few evaluations of a pre-k program following individuals until their early adulthood found positive effects on high school and college outcomes and reduced juvenile incarceration (Gray-Lobe et al., 2023). However, the effects were concentrated on boys, while the paper did not include teen childbearing – which could be particularly relevant for girls.

North Carolina's pre-kindergarten program (NCPK) offers an ideal setting to fill this gap in the literature. First, NCPK is a high-quality, established pre-k program. It now serves almost one-fourth of all four-year-old children in the state (Friedman-Krauss et al., 2023). Second, long-term evaluations are very data-demanding, and the state of North Carolina offers high-quality longitudinal administrative records from multiple sources and agencies that can be matched at the individual level. This is key because these evaluations require

[2]Kline and Walters (2016) show that the overall fadeout can be explained by the fact that Head Start was substituting other forms of preschool arrangements. The program did improve academic performance for the most disadvantaged students and those who would not have attended any preschool program in the absence of Head Start.

information about early childhood program participation linked to later outcomes, which is usually hard to find. Finally, as NCPK launched in 2001, participants from the early cohorts are just reaching adulthood. Examining the NCPK program thus balances the need for an old-enough program to observe participants' long-term outcomes and a program model that is new enough to be informative for current pre-k implementations.

Finally, as discussed in the opening paragraph, teen childbearing can be an important indicator of future socio-economic trajectories. In their review, Almond et al. (2018) argue that there is a lack of information about the different stages and decisions that individuals make while growing up, describing the period between early childhood and adulthood as the "missing middle". This period might be particularly important for disentangling the puzzling findings of previous early childhood intervention studies. This paper offers new evidence in this area.

4.2 Pre-kindergarten, teen childbearing, and school engagement

The evidence on the effects of ECE interventions on teenage births is still scarce. Promising results are derived from the Perry Preschool and Abecedarian demonstration projects, which led to reductions in women's probability of having a child by age 19 (Anderson, 2008; Campbell et al., 2012; Schweinhart et al., 1985). However, these studies are limited by their small sample sizes (49 and 53 women respondents for Perry and Abecedarian, respectively) and the fact that these interventions happened many decades ago. Another study, Tennessee's Student/Teacher Achievement Ratio study (Project STAR), randomly assigned kindergarten to 3rd-grade students to different class sizes. Reduced class sizes were associated with a statistically significant 1.6 percentage point (or 33%) reduction in teen pregnancy for white female students (Schanzenbach, 2006).

To understand how a pre-kindergarten program might affect teen childbearing, it is important to think about the factors leading to teen births. A starting point is to distinguish unwanted pregnancies and those who wanted them or were more ambivalent about it. While the former might be more likely affected by policies and interventions around family planning and contraceptive access, these can only account for a portion of teen births Kearney and Levine (2014). On the other hand, Kearney and Levine (2012) argue that early childbearing decisions are more likely explained by limited economic and social advancement opportunities.

Participating in pre-kindergarten could then reduce teen pregnancy by improving children's academic trajectories, school engagement, and overall economic opportunities and labor market prospects. While we cannot empirically test a causal effect of school outcomes on teen births, in this paper, we shed light on this mechanism by considering whether NCPK affected academic outcomes through the end of high school.

Moreover, one could think of increased unsupervised time during the teenage years as an increased risk for adolescent pregnancy. We include measures of school engagement, such as absenteeism and suspensions during high school, to analyze whether an NCPK-driven reduction in out-of-school time might work as a mechanism to explain the effect of NCPK on teen births.

It is worth restating that the relationship between school outcomes and teen childbearing is hard to disentangle, especially when they overlap in time, such as high school outcomes. On the one hand, school outcomes could affect teen births if we consider education to increase later economic opportunities. On the other hand, it is also possible that, even at a lower degree, teen childbearing could affect high school engagement. For instance, Schulkind and Sandler (2019) found that teenage mothers who gave birth before the end of their senior year of high school are less likely to finish their high school education than those who gave birth some months later. Hence, we measure the effects of NCPK on measures of high school engagement and attainment as supplemental analyses but acknowledge that we would not be able to identify the direction linking these outcomes and teen births and, consequently, if these are mechanisms through which pre-k affects teen birth rates.

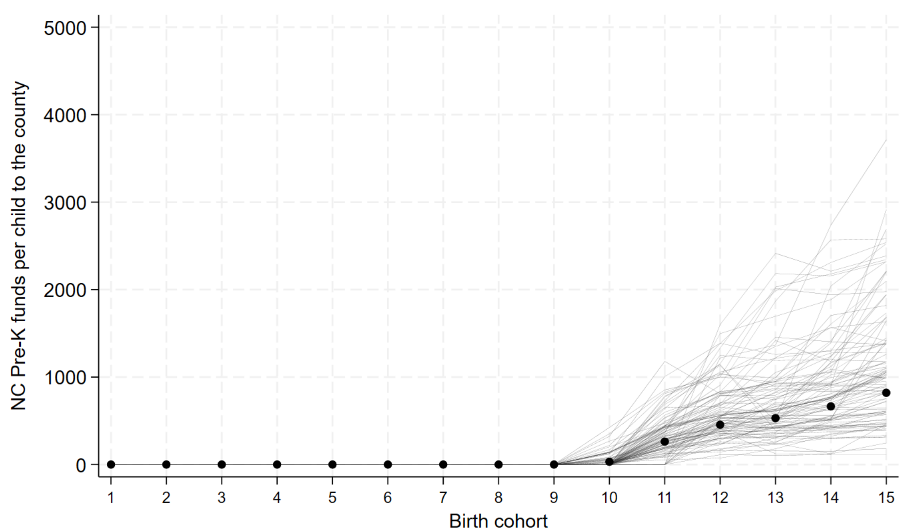
Finally, to some extent, pre-k participation could increase low-income families' connection with other social services and public benefits. Then, pre-kindergarten could reduce teen pregnancy by increasing participants' access to health care services through adolescence, such as family planning or subsidized contraceptives (e.g., see Kearney and Levine 2009 for the effects of family planning services on teen pregnancy). However, in this paper, due to data limitations, we cannot test whether NCPK affected health access, and thus, we do not offer insights into this path.

4.3 The North Carolina Pre-Kindergarten Program

The North Carolina Pre-Kindergarten program (NCPK) was launched in 2001 as a pilot program in 100 classrooms under the name of More at Four, and it has gradually extended to today. Figure 4.1 illustrates the program rollout across counties during the study period, i.e., up to the academic year 2006-2007. The main goal of NCPK is to prepare four-year-old children to enter kindergarten by improving their overall well-being and academic learning. To be NCPK-eligible, children must come from families with income below 75% of the state median income level. In addition, up to 20% of slots could be filled with children who do not meet the income requirement but present developmental disabilities, have limited English proficiency, have educational needs, have chronic health conditions, or have military families. Roughly half of the four-year-old children in NC are eligible to participate in the program; however, around 50% of them do it. This is still below the program's goal, which aims to serve at least 75% of eligible children.

NCPK is recognized as a high-quality state-funded pre-k program. While it does not create new classrooms, centers that want to offer pre-k-funded slots must meet a list of requirements that guarantee the high quality of the program. These centers could be public schools or community-based organizations. Requirements include having a 4- or 5-star rating in the Quality Rating and Improvement System (QRIS), licensure requirements for administrators and teachers (e.g., Birth-through-Kindergarten or Preschool add-on standard licensures for lead teachers), providing at least two meals, following an approved preschool curriculum as well as implementing formative assessments, meeting a 1:9 class ratio, among others. Several studies have demonstrated that NCPK is effective in improving children's school readiness at the beginning of kindergarten (Peisner-Feinberg et al., 2019; Peisner-Feinberg and Schaaf, 2011), and boosting academic skills through the elementary school grades (Dodge et al., 2017; Ladd et al., 2014; Watts et al., 2023).

Figure 4.1: Funds allocated to the NCPK program over time (per child)



Notes: NC Pre-K funds correspond to NC Pre-K program funding allocated to a child's county of birth in the program year when the child was four years old. Gray lines correspond to the average value within each of the 100 counties, and black dots represent the average across all individuals in the cohort.

4.4 Data

4.4.1 Study Sample

Sample definition

The study sample is initially defined as all females born in North Carolina between October 17th, 1987, and October 16th, 2002. Out of 767,215 individuals born between these dates, we found about 74% in North Carolina public school records (n=568,990)[3].

We restrict the sample to those observed in 8th grade in a North Carolina public or charter school (n=507,001). The goal is to reduce the bias from out-of-state migration before the years in which we observe the main outcomes (i.e., mainly before age 15)[4]. The last cohort – four years old in the AY 2006-2007—is the last possible cohort for whom we can observe a teen birth (they turned 19 in 2020-2021). Table 4.1 illustrates the timing of the study for three cohorts.

[3]Possible reasons for not being observed in public school records include out-of-state migration, private school attendance, and home-schooling.

[4]We assign a value of 1 when we observe a teen birth and 0 if an individual is not observed as having a teen birth on birth records. However, there is a possibility that an individual not observed in birth records had a birth outside North Carolina (i.e., not captured by the NC State of Health Statistics). By restricting the sample to individuals who were observed in 8th grade, we reduced the probability that individuals moved out of the state before the teenage years, and we are then more confident that the reason why those coded as "0" are not in birth records is that they did not have a baby. In Table C.1, we show that the likelihood of being observed in later grades was not associated with our treatment variable. Figure C.1 suggests that it is a good idea to implement the restriction in 8th grade at the latest. First, we do not have high school membership data for the earlier cohorts. Second, as dropping out of high school becomes a more relevant margin of response, it increases the likelihood that the NCPK program influences it. The proportion of individuals observed in grades 3-8 does not vary much. While we choose to restrict at grade 8, we conduct a robustness check where we restrict the sample to those observed in grade 3 at the latest.

Table 4.1: Timing of measurements: illustration of three cohorts

	First study cohort	First NCPK-exposed cohort	Last study cohort
Born between	10/17/1987-10/16/1988	10/17/1996-10/16/1997	10/17/2001-10/16/2002
Pre-K year	AY 1992-1993	AY 2000-2001	AY 2006-2007
High school graduation year	AY 2005-2006	AY 2014-2015	AY 2019-2020
Turned 19 y.o. between	10/17/2007-10/16/2008	10/17/2014-10/16/2015	10/17/2020-10/16/2021

Notes: Kindergarten entry date shifted to September 1st for those born in 2004, i.e., after the study period. High school graduation year is the academic year in which students would graduate if they were not retained in any grade. AY = Academic Year.

4.4.2 Data Sources

The main sources of data for this analysis are birth records from the NC State Center for Health Statistics, public school records provided by the NC Department of Public Instruction, NCPK funding provided by the NC Office of Early Learning, and NCPK attendance data provided by the University of North Carolina, Chapel Hill. The North Carolina Education Research Center (NCERDC) at Duke University received, de-identified, and linked individual records.

4.4.3 Measures

This subsection describes the main variables used in the analysis. Figure 4.2 illustrates the county-level variation for the analysis cohorts (additional figures in Appendix C.2, C.3, and C.4). Additional descriptive statistics are included in Appendix Table C.2.

NCPK funding. The primary treatment variable is the dollar amount allocated to the NCPK program for each academic year at the county level. The state allocates NCPK funds to contractors that mostly mimic the county structure in North Carolina (generally, one contractor per county). There are 100 counties. We merge the yearly NCPK amount to each individual's county of birth and to the year when they were four years old. This means that we assume that individuals were exposed to NCPK in their county of birth, or, in other words, that they stayed in their county of birth until they were at least four years old. We transform county-level total funding allocations to a measure of funding allocation per four-year-old[5]. Given that NCPK started in the academic year 2001-2002, the first nine cohorts of the study were not exposed to any NCPK funding. Thirty-four counties received NCPK funding for the first time when cohort 10 was age-eligible; 57 counties were first exposed the following year (i.e., cohort 11 was the first cohort exposed); and the remaining nine counties the following year (i.e., cohort 12 was the first cohort exposed).

NCPK attendance. For one of the empirical approaches described below, we also use an indicator of whether the individual participated in the NCPK program derived from NCPK administrative records. As expected, actual participation mimics the growth in funding. Almost 12% of the last birth cohort attended NCPK. More details on attendance are presented in the following sections, which describe the instrumental variable approach and results.

Teen births. For each individual, we create an indicator variable for whether she had a baby before turning 20 years old. It is worth noting that this is not a measure of teen pregnancy but teen births, i.e., we do not consider pregnancies that ended before the birth of the child. 12% of the analytic sample were teenage mothers. In line with national trends (U.S. Department of Health & Human Services, 2024), the percentage of teen mothers in our sample decreased from 18% for the first cohort, born in 1992-1993, to 6% for the last cohort, born in 2006-2007 (Appendix Table C.2).[6]

[5] We divide total funds allocated to the county by the four-year-old population in that year. This allows us to remove the variation in county size.

[6] To validate our data, it is useful to compare our teen birth rates with statistics reported by national or state offices. However, teen birth rates are usually measured as the number of births per 1,000 females ages 15-19 in a given year. This metric slightly differs from the percentage of women in a cohort who were teenage mothers between 15 and 19 years old. Creating a similar measure with our sample yields a lower rate than the one for the general population (e.g., 15 vs. 20 births per 1,000 females ages 15-19 in 2017) (Appendix Figure C.5). This could be explained by the fact that our sample only includes women born in North Carolina. If women born out of state are more likely to be teenage mothers, our sample's teen birth rate would be lower. This makes sense, for instance, considering the large increase in NC's Hispanic

School outcomes. For school outcomes, we are particularly interested in measures of high school attainment and engagement[7]. Specifically, we observe whether individuals in the sample graduated from high school and if they did it on time; their absence rates and whether they were above the 10% threshold for chronic absenteeism; and in-school and out-of-school suspensions during high school. High school graduation is defined as 1 if the individual was coded as a graduate in school exit records, and 0 otherwise, while absences and suspensions are conditional on ever being observed in a high school grade. We also look at academic achievement in 8th grade, the last year before high school, since the sample is restricted to those observed in 8th grade. We create an academic composite by averaging the reading and math standardized end-of-grade test scores.

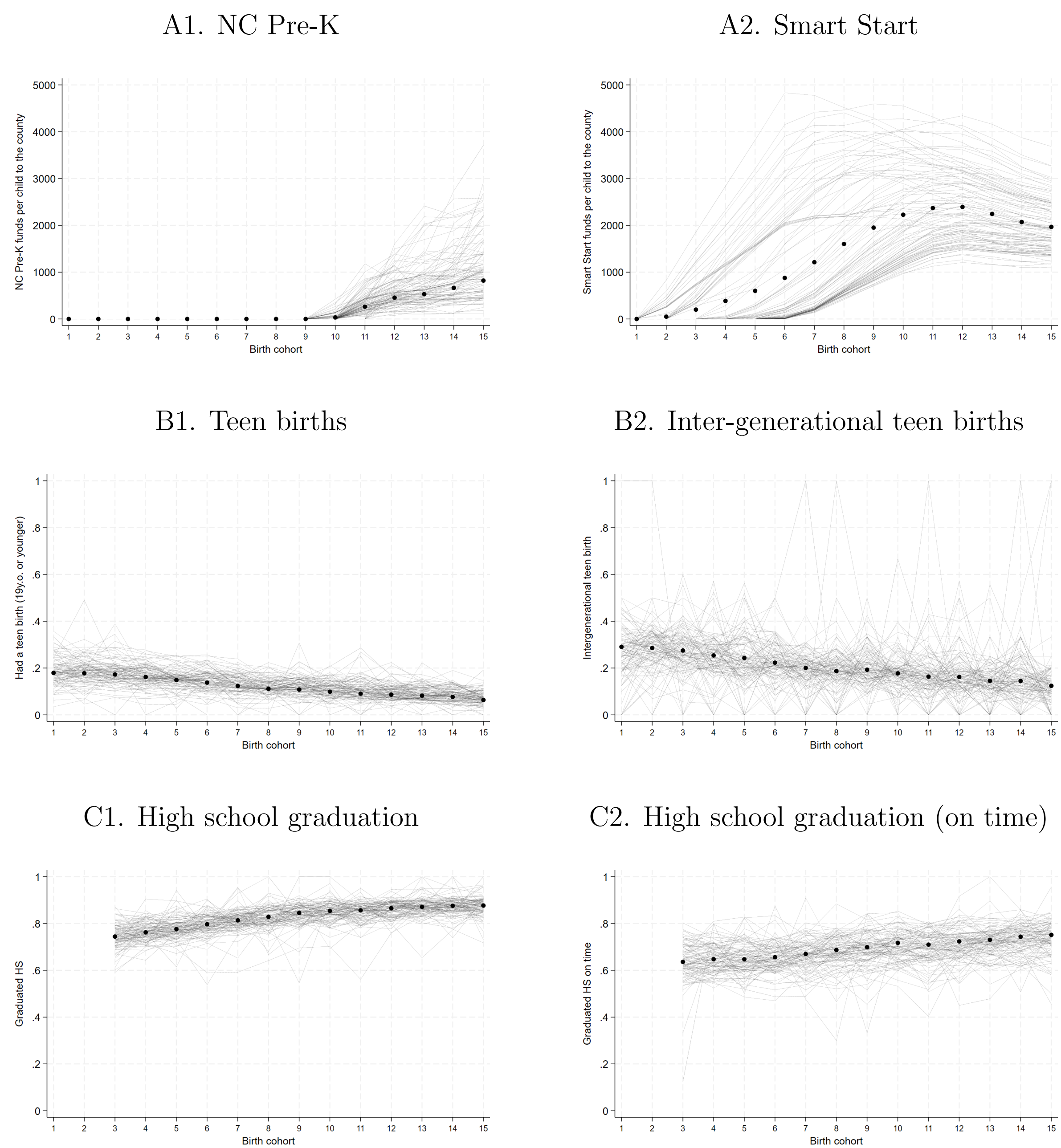
Covariates. Our models include a series of individual covariates and time-varying county covariates. On the former, we include: funding allocated to Smart Start[8]; demographic characteristics of the population born in that year including the percent of Black and Hispanic births, percent of births from low-educated mothers, number of births (in log), county total population (in log); and median family income (inflation-adjusted), percent of the population eligible for SNAP, and percent of the population that receives Medicaid. Individual-level covariates are derived from birth records and include birth weight, maternal education, maternal immigration status, maternal age at the time of birth, whether the child is the firstborn, and maternal racial-ethnic characteristics.

population and that Hispanic women are more likely to be teenage mothers than other ethnic groups. In 2021, only one-third of Hispanic North Carolinians were born in the state (NC OSBM, 2023). Additionally, our sample could be biased downwards if women moved out of state after 8th grade, i.e., they are not counted in the official statistics, and they still are in our sample, coded as teen birth = 0.

[7]See section 4.2 for a larger discussion on the relationship between schooling and teen births

[8]Smart Start offers funding to be spent on a variety of community-level initiatives to support the early childhood years.

Figure 4.2: County-level variation in selected variables throughout the study period



Notes: NC Pre-K funds correspond to NC Pre-K program funding allocated to a child's county of birth in the program year when the child was four years old. Smart Start funding aggregates the five years of funding allocated to their county of birth while the child was 0-5 years old. Gray lines correspond to each of the 100 counties, and black dots represent the average across all individuals in the cohort. For panels B and C, the sample is restricted to individuals who were observed in the public school system in grade 8.

4.5 Analytical Strategy

4.5.1 TWFE using county-level NCPK funding allocations

We first estimate the Intention-to-Treat effects of increasing NCPK exposure on the probability of having a teen birth. We exploit within-county variation in NCPK funding allocations. Using a two-way-fixed-effect (TWFE) specification, we measure associations between NCPK funding allocated to the county of birth when individuals were four years old and the probability of them having a teen birth, exploiting the differences in the timing and level of exposure to NCPK funding experienced by 100 counties:

$$TB_{ict} = \beta^{twfe} NCPKfunds_{ct} + X_{ct} \cdot \delta + Z_i \cdot \lambda + \theta_t + \gamma_c + \epsilon_{ict} \tag{4.1}$$

Where TB_{ict} indicates whether individual i, born in county c, who was four-year-old in year t, had a teen birth; $NCPKfunds_{ct}$ is the amount of NCPK funds per capita allocated to county c in year t (in thousand dollars); and θ_t and γ_c are cohort and county fixed effects. X_{ct} are time-varying characteristics of county c in year t, and Z_i is a vector of individual characteristics derived from birth records (described in the previous section). Standard errors are clustered at the county level. β^{twfe} provides an estimate of the causal effect of NCPK funding on teen births under the identifying assumption that the amount allocated to the NCPK program in a county and year is not correlated with other factors that could influence the probability of having a teen birth.

To test the validity of the identifying assumption, we estimate the following equation:

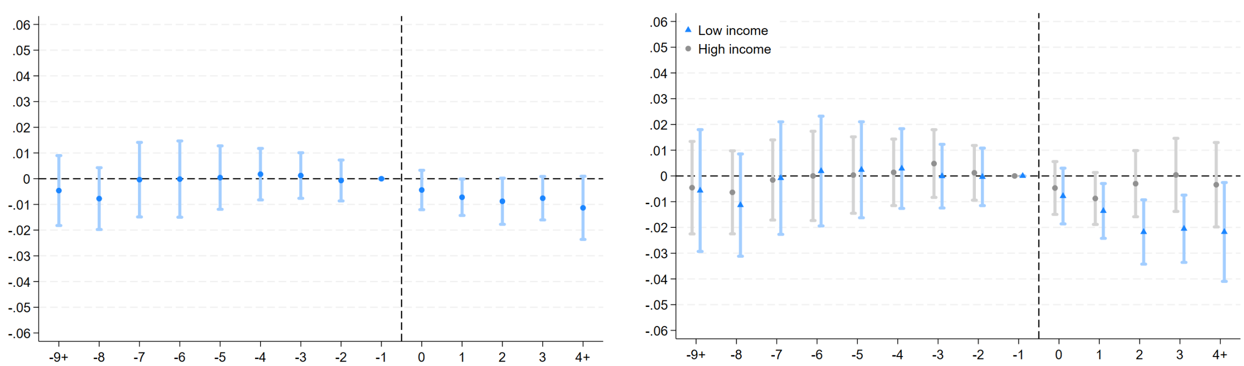
$$TB_{ict} = \beta_e \sum_{t=-9}^{5} I_t \cdot PostNCPKfunds_c + X_{ct} \cdot \delta + Z_i \cdot \lambda + \theta_t + \gamma_c + \epsilon_{ict} \tag{4.2}$$

where $PostNCPKfunds_c$ is the average amount of NCPK funds received by county c after they started receiving NCPK funds (in thousand dollars). Equation 4.2 removes the variation in the NCPK funding received within a county over time and exploits the variation in the NCPK funding allocations between counties. As shown in Figure 4.3, the average amount of NCPK funds per child received once the NCPK program started was not associated with pre-existing county-specific trends in teen births, providing robust evidence of the exogeneity of the funding allocation between counties.

Figure 4.3: Dynamic effects of NCPK funding on teen births

A. All counties

B. By county income



Notes: This figure plots the coefficients of the interactions derived from estimating Equation 4.2, with 95% confidence intervals. To balance sample size across events, we grouped events -9, -10, and -11, as well as 4 and 5. Regressions are estimated separately for counties with a median income below or above the state median in the year previous to the NCPK rollout (2001).

4.5.2 New developments in the differences-in-differences literature

While some variation comes from the *timing* of the initial rollout of the NCPK program, which varied between 2002 and 2004 (i.e., a staggered treatment), most of the relevant variation comes from the amount of funding allocated to each county, or funding *dosage* (i.e., a continuous treatment). Recent developments in the DID literature discuss some concerns around how to interpret this estimand (Callaway et al., 2024). In particular, concerns might arise if there are heterogeneous effects, or in other words, if counties with different exposure to NCPK funding experienced differential gains of an increase in the exposure to NCPK funding.

Equation 4.2 combines different comparisons, e.g., comparing counties that receive NCPK funds and counties that still do not receive any NCPK funds, as well as comparing counties that receive more with those receiving less NCPK funds. Moreover, 4.1 also incorporates variation from changes in the funding dosage over time.

Callaway et al. (2024) show that TWFE estimates an average causal response (ACR) when treatment is a continuous variable, such as NCPK funding (in dollars). This parameter combines *(i)* the effect of an increment in the amount of NCPK funding (i.e., the average causal response on the treated, ACRT(d/d)) and *(ii)* a selection bias that comes from the difference in the treatment effects for a given level of exposure across counties with different levels of exposure. In other words, to estimate an ACRT(d/d), we need to assume that, for instance, going from $100 to $200 NCPK funds has the same effect in all counties, regardless of the amount of NCPK funds they received. In future steps, we will incorporate new developments in the DID literature to address these concerns.

4.5.3 Instrumental variables approach

If, as opposed to county-level funding, we wanted to measure the direct effects of NCPK *enrollment* on individual outcomes, the main challenge would be that families self-select into the program. In other words, children who enroll in an NCPK center may have different characteristics from those who do not. To address this, we present an instrumental variables approach, in which we use variation in the exposure to NCPK programs given by the exogenous variation of county-level funding to instrument program participation. We estimate it using a two-stage least-square regression, using the *ivreghdfe* command in STATA, which provides a Local Average Treatment Effect (LATE) of NCPK.

To do so, first, it measures how NCPK funding predicts NCPK enrollment, i.e., how much the probability of enrolling in NCPK increases for each additional $1,000 NCPK funds allocated to the child's county when she is four:

$$NCPKenrollment_{ict} = \beta^{twfe} NCPKfunds_{ct} + X_{ct} \cdot \delta + Z_i \cdot \lambda + \theta_t + \gamma_c + \epsilon_{ict} \tag{4.3}$$

Second, it regresses the outcome on the predicted values for enrollment:

$$TB_{ict} = \beta^{twfe} NCPK\widehat{enroll}ment_{ict} + X_{ct} \cdot \delta + Z_i \cdot \lambda + \theta_t + \gamma_c + \epsilon_{ict} \tag{4.4}$$

For funding to be a good instrument, it must meet two main conditions (Angrist and Pischke, 2009). First, the inclusion restriction indicates that NCPK funding for the county of birth when the child is four years old must predict a child's NCPK enrollment. This is empirically tested in the "first stage". The second main assumption, the exclusion restriction, is that NCPK funding can only affect the outcome (e.g., the probability of having a teen birth) through NCPK enrollment. This assumption cannot be empirically tested. While it seems fairly reasonable, it is possible that NCPK funding might affect individual outcomes, for instance, if non-NCPK children benefited from sharing the classroom in later grades with former NCPK participants (i.e., if there were peer effects). This is a limitation of this approach and must, therefore, be interpreted with caution.

4.6 Main results

4.6.1 Effects of NCPK funding on teen births

Table 4.2 presents the effects of NCPK funding on the probability of having a teen birth, estimated using Equation 4.1. We found that, on average, a $1,000 increase in NCPK funding to an individual's county during pre-k age decreases the probability of having a teen birth by 0.6 percentage points (or by 5%). This result is driven by an even larger effect in lower-income counties, of 1.2 percentage points, which translates into an 8% decrease in the teen birth probability, and null effects in higher-income counties.

Table 4.2: Effects of NCPK funding on teen births

	(1) Main	(2) High Income	(3) Low income
NCPK funds (in 000s)	-0.006*	-0.003	-0.012**
	(0.004)	(0.005)	(0.005)
Mean	0.121	0.110	0.147
Observations	507001	362136	144865

Notes: * p<.10, ** p<.05, *** p<.01. Clustered standard errors in parentheses. Each column is a separate regression. The dependent variable is whether the individual had a teen birth. The regression includes birth cohort and county fixed effects, as well as individual and county-level time-varying covariates.

From a developmental perspective, and even considering the legal-age definition, it might be worth looking separately at the effects on teen births for females before turning 18 and those who were 18 or 19 years old (Hardy and Zabin, 1991). As shown in Appendix Table C.3, the reductions in teen births are driven by older teenagers.

Finally, we explored whether the effects differed based on population characteristics (Table 4.3). We found that Black women experienced a larger reduction in the probability of having a teen birth as a response to county-year NCPK funding when compared with non-Black women. Similarly, the effects are larger for Hispanic vs not-Hispanic and those whose mothers did not complete high school education. We also found that women born to teen mothers showed a more significant reduction in the probability of being teen mothers themselves; however, this coefficient is not significant when we include all the subgroups and their interactions in the same regression.

Table 4.3: Effects of NCPK funding on teen births, by subgroups

	(1)	(2)	(3)	(4)	(5)
			Had a teen birth		
NCPK funds (in 000s)	-0.000	-0.001	0.007**	-0.002	0.016***
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Black=1	0.031***				0.033***
	(0.005)				(0.005)
Black=1 × NCPK funds (in 000s)	-0.025***				-0.027***
	(0.004)				(0.004)
Hispanic=1		0.039***			0.039***
		(0.007)			(0.006)
Hispanic=1 × NCPK funds (in 000s)		-0.052***			-0.041***
		(0.009)			(0.010)
Maternal low education=1			-0.100		-0.100
			(0.168)		(0.167)
Mat. low ed=1 × NCPK funds (in 000s)			-0.047***		-0.040***
			(0.005)		(0.005)
Mother had a teen birth=1				0.003	-0.002
				(0.002)	(0.002)
Mat. TB=1 × NCPK funds (in 000s)				-0.026***	-0.004
				(0.005)	(0.004)
Observations	507001	507001	507001	507001	507001

Notes: * p<.10, ** p<.05, *** p<.01. Clustered standard errors in parentheses.Each column is a separate regression. The dependent variable is whether the individual had a teen birth. All regressions include birth cohort and county fixed effects, as well as individual and county-level time-varying covariates.

4.6.2 Effects of NCPK using an instrumental variable approach

This section presents the estimates obtained by using county-level funding as an instrument to measure the effects of individual enrollment in NCPK on teen birth probability.

4% of the females in the sample enrolled in NCPK when they were four years old. When we only consider the years after the program's launch, this share is 10%. Our first-stage estimates (Table 4.4) show that a $1,000 increase in NCPK funding to the county when an individual is four years old increases the probability of enrolling in NCPK by almost 16 percentage points. This number is pretty similar in both high and low-income countries.

Table 4.4: First-stage results: effects of NCPK funding on NCPK enrollment

	(1) All counties	(2) High Income	(3) Low Income
NCPK funds (in 000s)	0.157***	0.149***	0.159***
	(0.006)	(0.009)	(0.008)
Observations	507001	362136	144865
Enrollment Mean	0.039	0.034	0.052
Enrollment Mean if Funds>0	0.105	0.088	0.151
F-stat of instrument	721.412	289.412	382.283

Notes: * p<.10, ** p<.05, *** p<.01. Clustered standard errors in parentheses. Each column is a separate regression. The dependent variable is whether the individual enrolled in NCPK when she was four years old. The regression includes birth cohort and county fixed effects, as well as individual and county-level time-varying covariates. The "enrollment mean" is calculated for all individuals included in each regression, while the "enrollment mean if funds>0" is calculated for individuals who were four years old once there was a positive amount of NCPK funds allocated to her county.

Next, Table 4.5 shows the effects of NCPK enrollment on teen births. These can be interpreted as LATE, representing the effects of increases in NCPK enrollment that were sensitive to increases in county-year funding (i.e., effects that are local to "compliers"). We found that, across all counties, enrolling in NCPK reduced the probability of having a teen birth by 4.1 percentage points, which is considerably large given that the base teen birth rate across our study period is 12%. Similar to the NCPK-funding results, these are driven by low-income countries, where the probability of having a teen birth was reduced by 7.6 percentage points.

Table 4.5: Effects of NCPK enrollment on teen births (instrumental variable approach)

	(1) All counties	(2) High Income	(3) Low Income
NC Pre-K enrollment	-0.041*	-0.021	-0.076**
	(0.022)	(0.031)	(0.033)
Observations	507001	362136	144865

Notes: * p<.10, ** p<.05, *** p<.01. Clustered standard errors in parentheses. Each column is a separate regression. The IV was estimated using the *ivreghdfe* command in STATA. The regression includes birth cohort and county fixed effects, as well as individual and county-level time-varying covariates.

4.7 Supplemental Analysis: Effects on School Outcomes

As discussed in Section 4.2, our main hypothesis is that NCPK reduced teen births by increasing the social and economic expectations of its participants. While we cannot directly test it, we offer some supplemental analyses that can shed light on this mechanism.

Table 4.6 presents the effects of NCPK funding on female school outcomes. We found that NCPK increased 8th-grade test scores by 2.6 standard deviations, reduced the probability of being chronically absent (i.e., missing more than 10% of school days) by 1.4 percentage points, and the likelihood of receiving an out-of-school suspension by one percentage point. There were no significant effects on the probability of graduating high school.

While the test score effects are similar in magnitude in low- and high-income counties, the effects on absenteeism and suspensions were driven by low-income counties. Considering that the impact on teen births was concentrated in low-income counties as well, our results suggest that improvements in cognitive skills by themselves might not necessarily translate into teen birth reductions. Rather, NCPK must have affected other outcomes (e.g., non-cognitive skills, social skills) that led to higher presence in high school, lower probability of suspensions, and lower likelihood of having a teen birth.

Table 4.6: Effects of NCPK funding on female education outcomes

	(1) Had a teen birth	(2) G8 Academic composite (std)	(3) Ever chronic. absent (HS)	(4) Out-of-school suspension (HS)	(5) Graduated HS on time	(6) Graduated HS
			All counties			
NCPK funds (in 000s)	-0.007**	0.026*	-0.014**	-0.010*	-0.005	-0.006
	(0.003)	(0.014)	(0.007)	(0.005)	(0.006)	(0.004)
Mean	0.093	0.001	0.224	0.183	0.742	0.879
Observations	291452	291452	291452	291452	291452	291452
			High income counties			
NCPK funds (in 000s)	-0.003	0.018	-0.005	0.005	-0.008	-0.009
	(0.005)	(0.021)	(0.011)	(0.008)	(0.010)	(0.006)
Mean	0.084	0.065	0.216	0.169	0.756	0.883
Observations	210444	210444	210444	210444	210444	210444
			Low income counties			
NCPK funds (in 000s)	-0.009*	0.016	-0.014	-0.011*	-0.003	-0.007
	(0.005)	(0.018)	(0.009)	(0.006)	(0.008)	(0.006)
Mean	0.117	-0.166	0.243	0.220	0.707	0.867
Observations	81008	81008	81008	81008	81008	81008

Notes: * p<.10, ** p<.05, *** p<.01. Clustered standard errors in parentheses. The sample was restricted to individuals with non-missing observations in all outcomes. Each cell is a separate regression. The column title indicates the outcome. The regressions include birth cohort and county fixed effects, as well as individual and county-level time-varying covariates. Chronically absenteeism is defined as missing at least 10% of school days. HS=High School.

4.8 Sensitivity of the results

The results presented above are robust to different regression specifications, as shown in Table 4.7. First, we redefine the treatment as the average amount of NCPK funding in the county once the county started offering NCPK and interact this amount with an indicator of whether the child was four years old after her county of birth started offering NCPK. This mimics the treatment definition of Equation 4.2[9].

Then, we show that the results are consistent for different sample definitions. For instance, in our main specification, we restricted the sample to individuals observed in public schools

[9]Event-study figures for the education outcomes are included in Appendix Figure C.6. As opposed to Table 4.7, we plotted the coefficients from regressions run with a consistent sample size across outcomes, meaning that we limit the analysis to individuals born after October 17th, 1993, who attended at least one high school grade in a public school in North Carolina.

in 8th grade. We show that the results are not qualitatively different when we limit the sample to those observed in 3rd grade at the latest[10]. The results are also quite similar when we create smaller samples, either restricting to those born after October 1990 (i.e., those for whom we have absenteeism data) or after October 1993 (i.e., those for whom we have high school suspension data).

Table 4.7: Robustness of the results

	(1) Had a teen birth	(2) G8 academic composite	(3) Ever chronically absent (HS)	(4) Out-of-school suspension (HS)	(5) Graduated HS	(6) Graduated HS on time
			Main specification			
post=1 × NCPK funds (in 000s)	-0.006*	0.043***	-0.015**	-0.011**	-0.006	-0.004
	(0.004)	(0.016)	(0.007)	(0.005)	(0.005)	(0.006)
Mean	0.121	-0.000	0.231	0.186	0.828	0.694
Observations	507001	494223	399055	298658	440720	440720
			Using average treatment			
post=1 × Avg NCPK funds	-0.007*	0.036**	-0.015*	-0.006	-0.008	-0.004
	(0.004)	(0.017)	(0.008)	(0.006)	(0.005)	(0.007)
Mean	0.121	-0.000	0.231	0.186	0.828	0.694
Observations	507001	494223	399055	298658	440720	440720
			If ever in grade 3			
post=1 × NCPK funds (in 000s)	-0.006	0.038**	-0.016**	-0.010*	-0.005	-0.003
	(0.003)	(0.015)	(0.007)	(0.005)	(0.006)	(0.007)
Mean	0.118	0.000	0.230	0.186	0.773	0.648
Observations	508745	457109	385784	288835	450189	450189
			Birth cohorts 4+			
post=1 × NCPK funds (in 000s)	-0.008**	0.040***	-0.015**	-0.011**	-0.006	-0.004
	(0.003)	(0.015)	(0.007)	(0.005)	(0.005)	(0.006)
Mean	0.107	-0.000	0.231	0.186	0.835	0.699
Observations	406471	395503	399055	298658	406471	406471
			Birth cohorts 7+			
post=1 × NCPK funds (in 000s)	-0.007**	0.027*	-0.015**	-0.011**	-0.007	-0.005
	(0.003)	(0.015)	(0.007)	(0.005)	(0.005)	(0.006)
Mean	0.093	-0.000	0.227	0.186	0.854	0.715
Observations	304821	297092	298324	298658	304821	304821

Notes: * p<.10, ** p<.05, *** p<.01. Each cell is a separate regression. The column title indicates the outcome. The regressions include birth cohort and county fixed effects, as well as individual and county-level time-varying covariates. Chronically absenteeism is defined as missing at least 10% of school days. HS=High School.

[10]Note that the coefficient on teen births is similar in magnitude but less precise and not statistically significant in this case.

4.9 Conclusion

Teen childbearing is still prevalent in the United States, and the complexity of understanding its causes and effects limits the ability to provide precise policy prescriptions. This paper shows that investing in early childhood education programs should also be considered an important policy intervention to address this matter.

We found that a $1,000 increase in funding to the NCPK program when individuals were age-eligible reduced the probability of having a teen birth by 5%. This effect was higher for more disadvantaged populations, and it was driven by reductions in births by older teenagers (ages 18-19). Additionally, we found that while NCPK increased the academic performance of students in all counties, reductions in teen births were observed in low-income counties only. In these counties, NCPK also reduced high school absenteeism and out-of-school suspensions. We interpret these results as an overall positive effect of NCPK on outcomes that go beyond the skills that are captured in academic test scores.

While our study sheds some light on how early childhood investment might influence teen birth rates, further research is needed to understand the mechanisms behind these effects better. Our results suggest that improvements in non-cognitive skills might be key in explaining how a pre-kindergarten program can affect teen births. In future analyses, it would be important to include more behavioral and social measures at different points in childhood.

Additionally, while we focus on teen childbearing as an essential indicator by itself, it would also be interesting to keep observing fertility outcomes up to later ages and see whether NCPK decreased the probability of ever having a child and the total number of births, or alternatively, if the teen birth reduction is only a delay in the timing of births. This could have implications if we wanted to measure whether and how NCPK affected family formation more generally.

Chapter 5

Conclusions

5.1 Policy implications

In this dissertation, I evaluated two interventions that have implications for two important early childhood education policies in the United States: cash allowances for low-income families with young children and state-funded pre-kindergarten programs for four-year-old children.

5.1.1 Cash support to low-income mothers

Unlike its industrialized peer nations, the U.S. lacks a cohesive national policy that supports the transition to parenting among workers. Under certain conditions, federal parental leave policies ensure parents are guaranteed their job after the birth of a child, but the majority of low-income workers do not have access to paid leave. The challenge of balancing employment with caring for young children is heightened by the instability of the low-wage labor market and access to reliable and affordable child care for infants (Ananat and Gassman-Pines, 2021; Bassok and Galdo, 2016; Henly and Lambert, 2014). Financially subsidizing families during the early years of children's development has never received full policy attention despite the growing evidence about the importance of high-quality caregiving during children's earliest years, particularly among children who are born in low or unstable economic circumstances. Concerns about maternal employment disincentives have limited these initiatives, often fueled by public opinion and stigma, as well as ideological views about the role of welfare (Gilens, 2009; Mead, 1989). Indeed, the goal of welfare reforms in the 1990s was to increase the work efforts of low-income mothers by limiting their access to cash welfare and making it contingent on work.

Current policy interest in basic income has renewed attention to the question of whether low-income mothers with babies and young children will use these benefits to reduce their labor supply. The 2021 Child Tax Credit that is part of the U.S. National Recovery Plan is a departure from the U.S. tradition of low government spending in cash benefits, offering \$3,600 per child under age 6 for most low- and middle-income households, paid monthly. While this tax credit was temporary and linked to the COVID-19 pandemic, it brought back discussions about a permanent cash allowance. On the other hand, some analysts and policymakers fear that this kind of policy might create disincentives to work, which in the long run, some argue, would decrease intergenerational mobility for children who grow up in households where adults are not engaged in paid employment (Rachidi and Doar, 2019; Winship, 2021).

While the Baby's First Years study does not have high statistical power to precisely estimate how much low-income mothers would reduce employment as a response to a cash transfer, our study shows that these reductions, if any, should not be a matter of policy concern. We see some reduction in the hours worked during the pandemic among the mothers who received a higher cash gift. In my opinion, this short-lasting effect illustrates the flexibility of this type of support. This flexibility became important during an important life shock.

5.1.2 Pre-kindergarten programs

As states increase their efforts to provide early childhood education to four-year-old children through pre-kindergarten programs, many areas remain of concern and policy debate. While most states have implemented pre-k programs, there is a lot of variation in how they approach it, including the population served, the curricula, and where and who provides the service (Friedman-Krauss et al., 2023). This dissertation has implications for two areas of pre-k debates.

First, pre-k expansions have raised concerns about how they can affect infant and toddler care (Loewenberg, 2023; Paul, 2023). For instance, in New York City, where pre-k was made universal, centers switched to serve pre-k eligible children and reduced the availability of childcare for younger children (Brown, 2018). However, the findings in Chapter 3 add more nuance. In the North Carolina case, NCPK centers' shift to serving eligible children did not lead to a decrease in the availability of care for younger children. I explain this by a response from surrounding non-NCPK providers that received NCPK-ineligible children. Additionally, one could ask, what if the market is so tight that surrounding childcare centers could not serve pre-k ineligible children? It is possible that in these cases, there might have been an increase in childcare prices. This highlights the importance of having better information about childcare prices at the center level. And, to understand how these processes take place, it is important to put the different results in conversation, paying particular attention to differences in the pre-k designs. In any case, the results reinforce the need to consider the impacts on infant and toddler care when evaluating the advantages and disadvantages of each approach.

A second area of discussion around pre-kindergarten programs is whether they help children in the long run. Researchers have argued that modern pre-k programs might not yield the positive results that early demonstration programs have found (Whitaker et al., 2023). Even more concerning are the results from the randomized controlled trial of the Voluntary Pre-Kindergarten program in Tennessee. Despite having positive effects at the end of the pre-k year, these effects faded out and even became negative in elementary school (Lipsey et al., 2018; Durkin et al., 2022). However, the results that I provide from North Carolina pre-K program are more encouraging. Although limited to females, this study shows that pre-K can have positive effects that go beyond academic outcomes. As argued by Kearney and Levine (2012), teen births are likely a response to limited social and economic prospects and opportunities to advance. Our study motivates more research to come to understand how exactly is pre-k generating these encouraging effects.

5.2 Next steps

With the overarching goal of contributing to the evaluation of policies for reducing poverty and economic inequalities, I plan to continue my research on how long-term effects are generated and why some effects fade out over time. In particular, I would like to quantify further the role of socio-emotional development and social support during childhood. Additionally, I plan to continue analyzing the interaction of policy effects with different environments and how we can better prepare those environments to ensure that children can continue on a positive trajectory.

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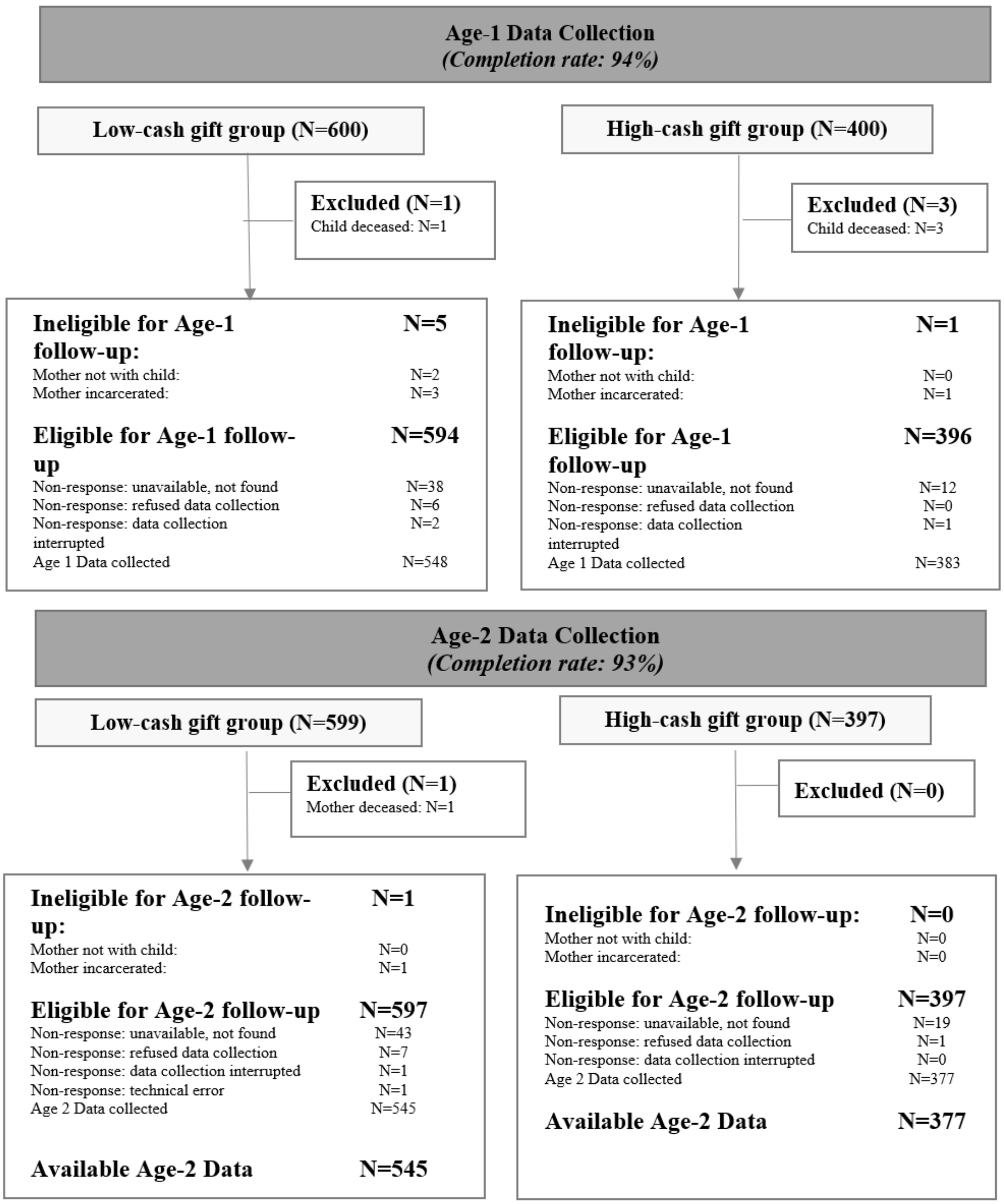
Appendix A

Appendix to Study 1

Additional Figures

Figure A.1: CONSORT diagram



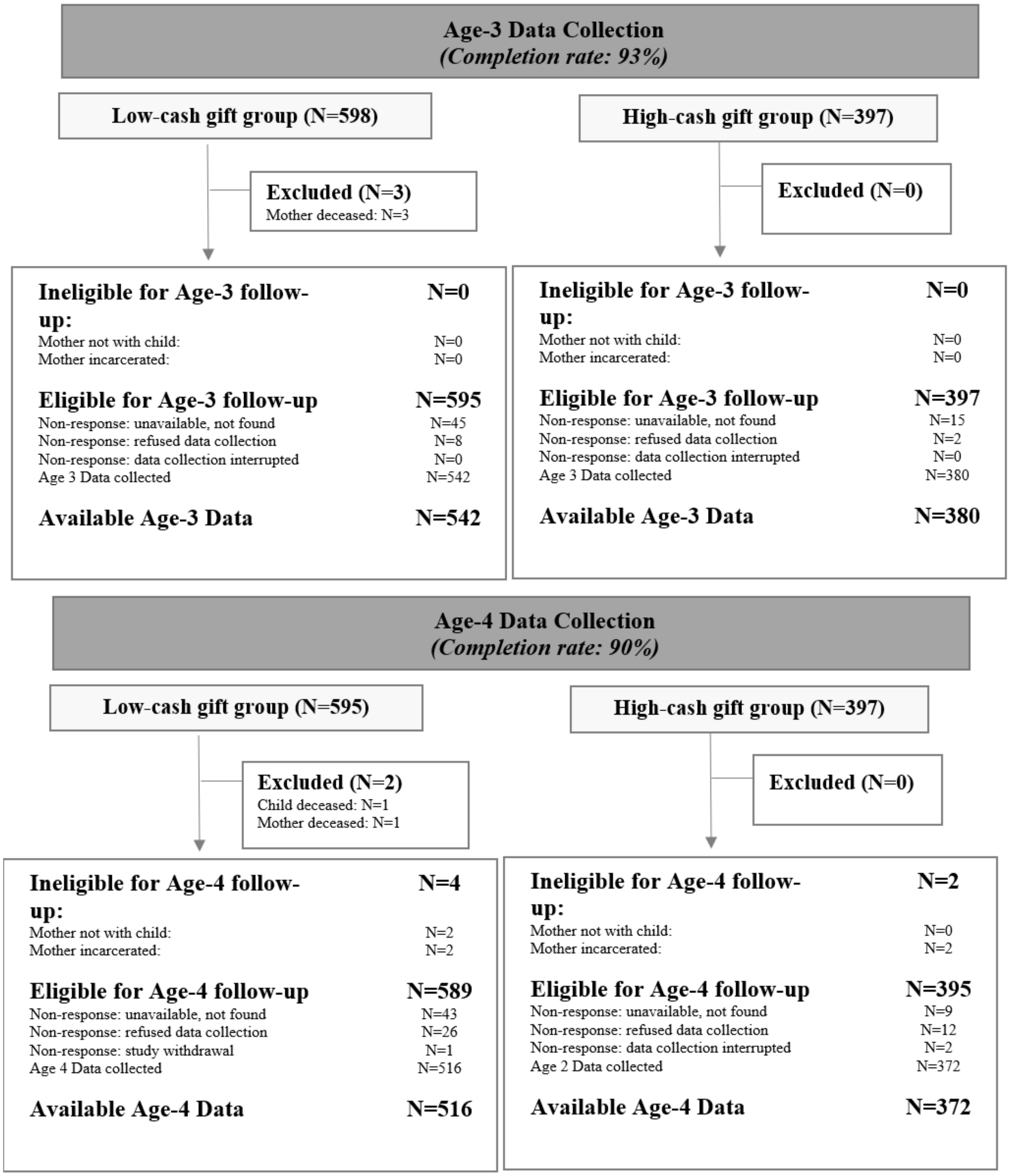
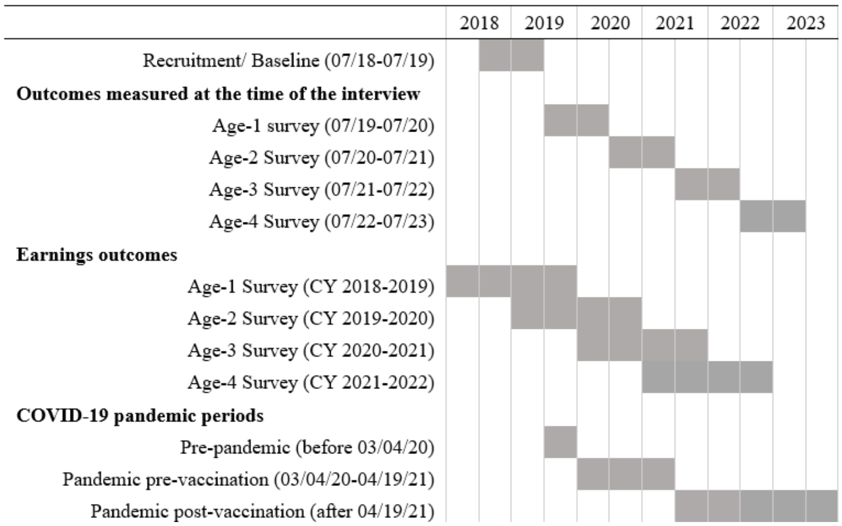
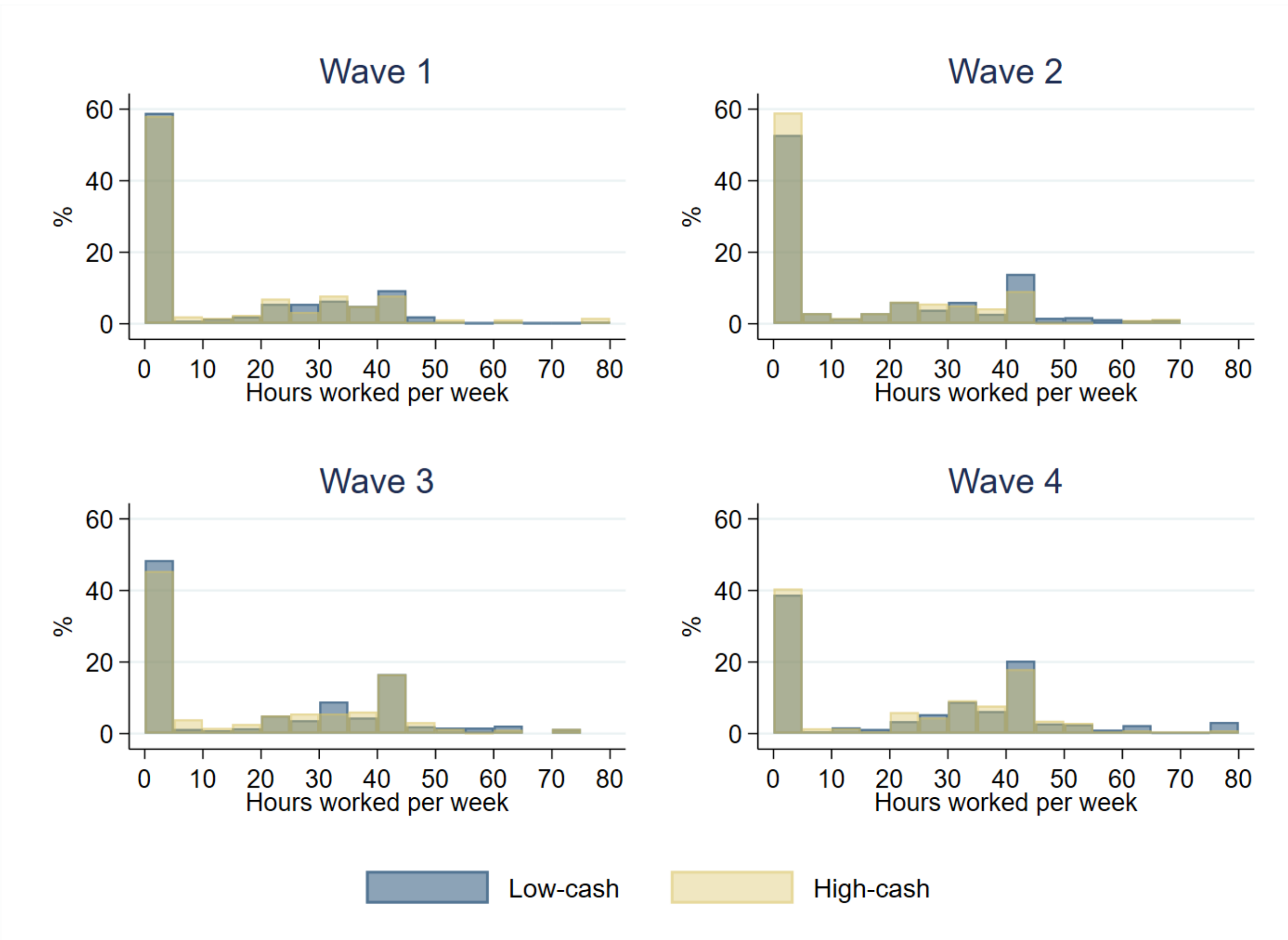

Age-3 Data Collection
(Completion rate: 93%)
Low-cash gift group (N=598)
Excluded (N=3)
Mother deceased: N=3
Ineligible for Age-3 follow-up: N=0
Mother not with child: N=0
Mother incarcerated: N=0
Eligible for Age-3 follow-up N=595
Non-response: unavailable, not found N=45
Non-response: refused data collection N=8
Non-response: data collection interrupted N=0
Age 3 Data collected N=542
Available Age-3 Data N=542
High-cash gift group (N=397)
Excluded (N=0)
Ineligible for Age-3 follow-up: N=0
Mother not with child: N=0
Mother incarcerated: N=0
Eligible for Age-3 follow-up N=397
Non-response: unavailable, not found N=15
Non-response: refused data collection N=2
Non-response: data collection interrupted N=0
Age 3 Data collected N=380
Available Age-3 Data N=380
Age-4 Data Collection
(Completion rate: 90%)
Low-cash gift group (N=595)
Excluded (N=2)
Child deceased: N=1
Mother deceased: N=1
Ineligible for Age-4 follow-up: N=4
Mother not with child: N=2
Mother incarcerated: N=2
Eligible for Age-4 follow-up N=589
Non-response: unavailable, not found N=43
Non-response: refused data collection N=26
Non-response: study withdrawal N=1
Age 4 Data collected N=516
Available Age-4 Data N=516
High-cash gift group (N=397)
Excluded (N=0)
Ineligible for Age-4 follow-up: N=2
Mother not with child: N=0
Mother incarcerated: N=2
Eligible for Age-4 follow-up N=395
Non-response: unavailable, not found N=9
Non-response: refused data collection N=12
Non-response: data collection interrupted N=2
Age 2 Data collected N=372
Available Age-4 Data N=372

Figure A.2: Timing of BFY data collection and measures



Notes: This table illustrates the overlap of the different measures as well as the COVID-19 pandemic periods. Given that participants were continuously recruited across the 12-month recruiting period, follow-up surveys were also continuously conducted. As opposed to labor market participation questions, earnings were reported for the previous calendar year. Because interviews within a wave fall over two calendar years, earnings collected in the same wave can also correspond to two different calendar years, depending on the time of the year when the participant was recruited.

Figure A.3: Histograms of hours worked per week



Notes: Hours are truncated at the 99th percentile (within wave).

Figure A.4: Monthly unemployment rate and sectoral employment over time, by site

Notes: Source: Bureau of Labor Statistics. Unemployment rates are not seasonally adjusted.

Additional Tables

Table A.1: Baseline balance between survey respondents and non-respondents

	Either wave		Wave 1		Wave 2		Wave 3		Wave 4	
	Diff	*p*-val	Diff	*p*-val	Diff	*p*-val	Diff	*p*-val	Diff	*p*-val
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Prior Employment and Plans										
Worked while pregnant	-0.15	0.138	-0.05	0.453	-0.01	0.799	-0.06	0.333	-0.12	0.013
Continued working until birth	-0.03	0.710	-0.04	0.388	-0.01	0.779	-0.01	0.779	-0.03	0.399
Planning to work	0.11	0.147	0.02	0.727	0.06	0.151	0.04	0.289	-0.02	0.551
Mother plans to work in X months	2.95	0.000	1.01	0.045	0.60	0.201	1.23	0.011	1.17	0.004
Baby's Characteristics										
Female	0.05	0.623	-0.03	0.627	-0.02	0.746	-0.01	0.929	0.02	0.704
Weight at birth (pounds)	-0.53	0.014	-0.19	0.144	-0.14	0.261	-0.17	0.169	-0.06	0.576
Gestational age (weeks)	-0.32	0.221	-0.00	0.992	-0.08	0.579	0.02	0.880	-0.01	0.947
Mother's Characteristics										
Age at child's birth (years)	-0.80	0.506	-1.41	0.054	-1.14	0.099	-0.66	0.336	-1.26	0.031
Years of education	-1.12	0.061	0.03	0.926	-0.82	0.016	-0.65	0.055	-0.16	0.579
White non-Hispanic	0.11	0.077	0.08	0.037	0.06	0.107	0.03	0.407	0.07	0.026
Black non-Hispanic	0.00	0.979	0.07	0.262	-0.05	0.431	0.07	0.260	-0.07	0.134
Multiple races non-Hispanic	-0.04	0.338	-0.02	0.321	0.00	0.903	-0.03	0.253	-0.01	0.579
Hispanic	-0.12	0.237	-0.19	0.002	-0.04	0.487	-0.10	0.098	-0.02	0.713
Never married	0.09	0.378	0.04	0.492	-0.00	0.937	-0.00	0.937	0.00	0.958
Single living with partner	-0.04	0.689	-0.06	0.274	0.06	0.266	0.03	0.574	0.07	0.113
Married	-0.05	0.590	-0.06	0.277	-0.09	0.062	-0.02	0.674	-0.10	0.018
Divorced or separated	0.04	0.290	0.03	0.172	0.03	0.284	0.03	0.284	0.01	0.477
Health is good to excellent	-0.19	0.003	-0.06	0.135	-0.11	0.003	-0.07	0.070	-0.05	0.095
Depressive symptoms (CESD)	0.34	0.000	0.16	0.005	0.23	0.000	0.13	0.016	0.02	0.687
Cigarettes/week in pregnancy	10.22	0.006	5.52	0.015	3.74	0.084	1.36	0.524	5.11	0.005
Alcohol drinks/week in pregnancy	0.18	0.487	0.16	0.317	0.13	0.393	-0.02	0.878	0.11	0.411
Household Characteristics										
Children born to mother	-0.29	0.309	-0.18	0.314	-0.03	0.844	-0.02	0.910	-0.09	0.530
Number of adults in the household	0.26	0.204	0.32	0.010	0.19	0.101	0.01	0.935	0.19	0.054
Biological father in the household	-0.09	0.373	-0.10	0.114	0.02	0.727	-0.01	0.891	0.02	0.749
Household combined income	-430.34	0.926	1231.68	0.630	3628.82	0.136	880.49	0.720	-327.20	0.874
Household net worth	-2780.84	0.615	1792.55	0.607	-1645.76	0.606	-6826.56	0.032	-5079.12	0.063
Joint Test:										
	$\chi^2(31)$= 87.22		$\chi^2(31)$= 62.62		$\chi^2(31)$= 45.71		$\chi^2(31)$= 37.38		$\chi^2(31)$= 52.59	
	p-value= 0.000		p-value= 0.001		p-value= 0.043		p-value= 0.199		p-value= 0.009	
	1000		1000		1000		1000		1000	

Notes:Differences in means between respondents and non-respondents were derived from a series of OLS bivariate regressions in which each respective baseline characteristic was regressed on an indicator of whether they were part of each final sample. *p*-values are reported for a test of equal means between both groups. The CEDS-D depressive symptoms measure is calculated as a within-person item average (range 0-3) among the 10 items from the short-scale. Joint tests of orthogonality were conducted using a probit model with robust standard errors and site-level fixed effects.

Table A.2: Robustness checks

	Working for pay				Working full-time				Annual earnings			
	Main	No Covariates	NRW	Bounds	Main	No Covariates	NRW	Bounds	Main	No Covariates	NRW	Bounds
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A. Pooled results												
High cash gift	-0.01	-0.02	-0.01		-0.03	-0.03	-0.03		-451.21	-715.26	-432.95	
	(0.02)	(0.02)	(0.02)		(0.02)	(0.02)	(0.02)		(532.78)	(604.84)	(536.40)	
lower				-0.05*				-0.07**				-2428.68**
				(0.02)				(0.02)				(470.36)
upper				0.01				-0.02				-91.87
				(0.02)				(0.02)				(438.45)
Observations	3661	3661	3661	4000	3283	3283	3283	4000	3582	3582	3582	4000
Panel B. Wave 1												
High cash gift	-0.04	-0.04	-0.04		-0.01	-0.02	-0.01		204.10	104.66	172.41	
	(0.03)	(0.03)	(0.03)		(0.03)	(0.03)	(0.03)		(508.07)	(570.46)	(509.87)	
lower				-0.07*				-0.07				-1327.72*
				(0.04)				(0.06)				(646.83)
upper				-0.02				-0.01				564.15
				(0.03)				(0.03)				(608.94)
Observations	931	931	931	1000	582	582	582	1000	922	924	922	1000
Panel C. Wave 2												
High cash gift	-0.02	-0.02	-0.02		-0.07*	-0.07**	-0.07*		-229.53	-317.48	-140.40	
	(0.03)	(0.03)	(0.03)		(0.03)	(0.03)	(0.03)		(676.47)	(738.61)	(681.04)	
lower				-0.04				-0.10**				-1450.03
				(0.04)				(0.03)				(905.81)
upper				-0.01				-0.07*				87.28
				(0.03)				(0.03)				(784.16)
Observations	921	921	921	1000	908	908	908	1000	907	907	907	1000
Panel D. Wave 3												
High cash gift	0.01	0.00	0.01		0.01	-0.00	0.00		-816.22	-1177.62	-820.54	
	(0.03)	(0.03)	(0.03)		(0.03)	(0.03)	(0.03)		(829.99)	(845.03)	(828.91)	
lower				-0.02				-0.03				-2773.62**
				(0.04)				(0.03)				(972.00)
upper				0.03				0.01				-700.38
				(0.04)				(0.03)				(893.56)
Observations	922	922	922	1000	910	910	910	1000	913	913	913	1000
Panel E. Wave 4												
High cash gift	-0.01	-0.01	-0.01		-0.04	-0.04	-0.03		-1021.38	-1558.82	-1001.47	
	(0.03)	(0.03)	(0.03)		(0.03)	(0.03)	(0.03)		(984.82)	(1004.74)	(982.78)	
lower				-0.05				-0.08*				-4042.80**
				(0.04)				(0.04)				(1180.11)
upper				0.04				-0.00				-345.06
				(0.04)				(0.04)				(1134.75)
Observations	887	887	887	1000	883	883	883	1000	840	840	840	1000

Notes: + p<.10, * p<.05, ** p<.01. Standard errors in parentheses. NRW=Non-Response Weights. These weights are estimated following a machine learning algorithm package, called TWANG (Toolkit for Weighting and Analysis of Nonequivalent Groups), using inverse probability weighting. Bounds are based on Lee (2009) and estimated using the *leebounds* STATA command by Tauchmann (2014). This method generates treatment-effect bounds in the context of random assignment and non-compliance, and it assumes monotonicity. Note that to use this command, it is not possible to include covariates in the model.

Table A.3: Balance of baseline characteristics across low- and high-cash gift group

	At least one wave			Wave 1		Wave 2		Wave 3		Wave 4	
	Low-cash	Diff	p-value	Diff	p-value	Diff	p-value	Diff	p-value	Diff	p-value
Prior Employment and Plans											
Worked while pregnant	0.571	0.010	0.860	0.000	0.933	0.020	0.549	0.020	0.649	0.010	0.749
	(0.02)										
Continued working until birth	0.156	0.010	0.759	0.010	0.820	0.010	0.759	0.010	0.563	0.010	0.734
	(0.01)										
Planning to work	0.849	-0.010	0.710	-0.010	0.668	0.000	0.888	-0.010	0.745	0.000	0.939
	(0.01)										
Plans to work in X months	2.981	0.120	0.600	0.210	0.371	0.100	0.674	0.170	0.453	0.190	0.418
	(0.15)										
Baby's Characteristics											
Female	0.499	0.020	0.523	0.030	0.407	0.030	0.418	0.020	0.494	0.020	0.565
	(0.02)										
Weight at birth (pounds)	7.139	0.040	0.588	0.030	0.721	0.030	0.649	0.050	0.481	0.050	0.502
	(0.04)										
Gestational age (weeks)	39.102	0.070	0.396	0.060	0.476	0.060	0.446	0.040	0.661	0.090	0.290
	(0.05)										
Mother's Characteristics											
Age at child's birth (years)	26.856	-0.490	0.195	-0.470	0.221	-0.550	0.160	-0.470	0.231	-0.440	0.269
	(0.24)										
Years of education	11.912	0.010	0.971	-0.050	0.802	0.040	0.818	0.110	0.576	0.040	0.848
	(0.12)										
White non-Hispanic	0.108	0.020	0.232	0.020	0.204	0.020	0.320	0.030	0.145	0.020	0.379
	(0.01)										
Black non-Hispanic	0.397	-0.040	0.179	-0.050	0.097	-0.040	0.245	-0.050	0.114	-0.050	0.173
	(0.02)										
Multiple races non-Hispanic	0.041	0.010	0.402	0.010	0.402	0.010	0.369	0.020	0.231	0.010	0.512
	(0.01)										
Hispanic	0.407	-0.010	0.695	0.000	0.965	-0.020	0.623	-0.020	0.613	-0.010	0.772
	(0.02)										
Never married	0.421	-0.080	0.020	-0.080	0.018	-0.070	0.030	-0.070	0.025	-0.080	0.023
	(0.02)										
Single living with partner	0.263	0.050	0.085	0.060	0.051	0.050	0.085	0.050	0.072	0.040	0.126
	(0.02)										
Married	0.210	0.000	0.864	0.000	0.964	0.000	0.976	-0.010	0.842	0.000	0.931
	(0.02)										
Divorced or separated	0.048	0.020	0.123	0.020	0.189	0.020	0.199	0.020	0.185	0.020	0.201
	(0.01)										
Health is good to excellent	0.880	-0.050	0.021	-0.040	0.027	-0.040	0.047	-0.040	0.038	-0.040	0.061
	(0.01)										
Depressive symptoms (CESD)	0.680	0.000	0.920	0.000	0.896	-0.010	0.743	0.000	0.968	0.010	0.854
	(0.02)										
Cigarettes/week in pregnancy	4.826	1.650	0.149	1.570	0.172	1.430	0.217	1.770	0.141	0.980	0.375
	(0.86)										
Alcohol/week in pregnancy	0.161	0.140	0.110	0.130	0.148	0.130	0.151	0.150	0.099	0.120	0.166
	(0.07)										
Household Characteristics											
Children born to mother	2.405	-0.140	0.134	-0.110	0.234	-0.130	0.175	-0.130	0.175	-0.120	0.195
	(0.06)										
N adults in the household	2.106	0.080	0.223	0.060	0.353	0.060	0.341	0.080	0.233	0.050	0.436
	(0.04)										
Bio father in the household	0.402	0.050	0.106	0.060	0.061	0.060	0.089	0.050	0.091	0.060	0.084
	(0.02)										
Household income	22472.371	1538.340	0.242	1328.490	0.318	1132.720	0.356	1707.960	0.210	1744.230	0.206
	(913.86)										
Household net worth	-1928.025	1287.860	0.472	1080.400	0.558	1396.190	0.458	450.440	0.794	340.720	0.846
	(1270.63)										
Observations		976		931		922		922		888	
Joint Test		$\chi^2(33)$=33.60		$\chi^2(33)$=30.05		$\chi^2(33)$=31.97		$\chi^2(33)$=35.53		$\chi^2(33)$=31.08	
		p=0.438		p=0.615		p=0.518		p=0.350		p=0.563	

Notes: Differences between groups were derived from a series of OLS bivariate regressions in which each respective baseline characteristic was regressed on the cash-gift group indicator. p-values are reported for a test of equal means between the high-and low-cash gift group. The CEDS-D depressive symptoms measure is calculated as a within-person item average (range:0-3) among the 10 items from the short-scale. Join tests of orthogonality were conducted using a probit model with robust standard errors and site-level fixed effects.

Table A.4: Sensitivity checks: effects of BFY cash gift on household earnings with and without outliers

	Maternal annual earnings		Non-maternal annual earnings		Total HH earnings	
	T	NT	T	NT	T	NT
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Pooled						
High cash gift	-451.21	-788.15	-167.63	131.25	-391.89	-271.12
	(532.78)	(624.29)	(853.44)	(977.68)	(1024.17)	(1163.82)
Observations	3582	3582	2654	2654	2638	2638
Low-cash gift group mean	10967.79	11554.61	12554.16	12845.72	22322.04	22861.47
Effect in %	-4.11%	-6.82%	-1.34%	1.02%	-1.76%	-1.19%
Panel B. Wave 1						
High cash gift	204.10	-17.90	-574.68	-769.79	-467.77	-916.61
	(508.07)	(563.98)	(1004.71)	(1074.86)	(1140.05)	(1281.18)
Observations	922	922	892	892	887	887
Low-cash gift group mean	7254.38	7561.78	12995.78	13410.04	20272.44	21025.84
Effect in %	2.81%	-0.24%	-4.42%	-5.74%	-2.31%	-4.36%
Panel C. Wave 2						
High cash gift	-229.53	-246.79	-618.94	70.71	-674.21	50.49
	(676.47)	(925.85)	(1199.28)	(1596.23)	(1513.98)	(1873.08)
Observations	907	907	879	879	873	873
Low-cash gift group mean	9868.70	10309.93	12777.73	13053.26	22906.13	23309.42
Effect in %	-2.33%	-2.39%	-4.84%	0.54%	-2.94%	0.22%
Panel D. Wave 3						
High cash gift	-816.22	-1158.43	549.88	962.64	-206.72	-122.83
	(829.99)	(924.31)	(1202.54)	(1343.49)	(1528.99)	(1639.36)
Observations	913	913	883	883	878	878
Low-cash gift group mean	11391.83	12021.94	11883.79	12067.87	23788.13	24250.11
Effect in %	-7.16%	-9.64%	4.63%	7.98%	-0.87%	-0.51%
Panel E. Wave 4						
High cash gift	-1021.38	-1760.07				
	(984.82)	(1303.62)				
Observations	840	840				
Low-cash gift group mean	15816.61	16824.81				
Effect in %	-6.46%	-10.46%				

Notes: + p<.10, * p<.05, ** p<.01. Standard errors in parentheses. T = Truncated at 99th percentile, NT= Not truncated.

Table A.5: Reported earnings calendar' year, household composition, and cash gift group assignment

	Calendar Year				Partner reported			Other adult reported		
	2019	2020	2021	2022	Wave 1	Wave 2	Wave 3	Wave 1	Wave 2	Wave 3
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
High cash gift	-0.01	0.00	0.05	-0.01	0.00	0.01	-0.01	-0.02	-0.05	-0.01
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Observations	931	922	922	888	931	922	922	931	922	922

Notes: + p<.10, * p<.05, ** p<.01. Standard errors in parentheses. Participants were asked to report earnings form the previous calendar year. Given that each wave was collected throughout two calendar years, the outcomes for the first four columns indicate whether the earnings reported in the survey refer to calendar year (CY) 2019 in Wave 1 (column 1), CY 2020 in Wave 2 (column 2), CY 2021 in Wave 3 (column 3), and CY 2022 in Wave 4 (column 4). Alternatively, participants reported earnings for CY 2018 in Wave 1, CY 2019 in Wave 2, CY 2020 in Wave 3, and CY 2021 in Wave 4. All regressions include site fixed effects and the following covariates: mother's age, maximum education level attained, race and ethnicity, marital status, general health, maternal depressive symptoms, cigarette and alcohol consumption during pregnancy, number of children born to mother, number of adults in the household, father living with the mother, household income, household net worth, baby's weight and gestational age at birth, mother worked while pregnant, continued working until birth, and plans to go back to work after birth. Regressions in columns 5-10 also include: calendar month indicator, and the time distance to the child birthday within wave, in months (i.e., how far away they were from the scheduled data collection).

Table A.6: Effects of BFY cash gift on maternal employment by subgroup

	Working for pay	Working full-time	Annual earnings
	(1)	(2)	(3)
High Cash Gift	-0.06	-0.09*	-2679.16**
	(0.04)	(0.04)	(1020.87)
High Cash Gift × Completed High School=1	-0.01	0.05	2983.61**
	(0.04)	(0.04)	(1079.25)
High Cash Gift × No partner=1	0.09+	0.08+	921.66
	(0.04)	(0.04)	(1098.37)
High Cash Gift × BFY child is first child=1	-0.00	-0.05	693.69
	(0.05)	(0.04)	(1153.41)
High Cash Gift × Worked while pregnant=1	0.02	0.02	58.00
	(0.04)	(0.04)	(1025.98)
Observations	3661	3283	3582

Notes: + p<.10, * p<.05, ** p<.01. Standard errors in parentheses. All subgroup variables were measured at baseline. "No partner" is defined as not living with a partner or not married at the time of the baby's birth. All regressions include site and wave fixed effects and the following covariates: mother's age, maximum education level attained, race and ethnicity, marital status, general health, maternal depressive symptoms, cigarette and alcohol consumption during pregnancy, number of children born to mother, number of adults in the household, father living with the mother, household income, household net worth, baby's weight and gestational age at birth, mother worked while pregnant, continued working until birth, plans to go back to work after birth, calendar month indicator, and the time distance to the child birthday within wave, in months (i.e., how far away they were from the scheduled data collection).

Table A.7: Descriptive statistics of employment outcomes by wave

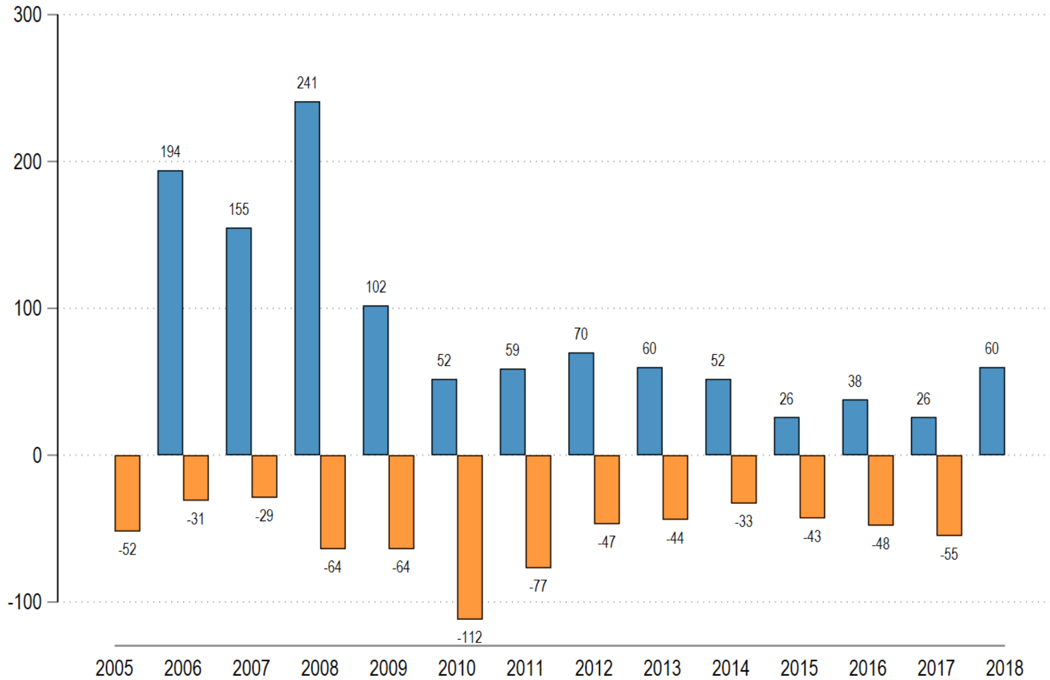
		Wave 1		Wave 2		Wave 3		Wave 4		Pooled	
		Low Cash	High Cash	Low Cash	High Cash	Low Cash	High Cash	Low Cash	High Cash	Low Cash	High Cash
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Working for pay	Mean	0.45	0.41	0.42	0.40	0.50	0.50	0.56	0.55	0.48	0.46
	SD	0.50	0.49	0.49	0.49	0.50	0.50	0.50	0.50	0.50	0.50
	N	548	383	544	377	542	380	515	372	2149	1512
Self-employed	Mean	0.11	0.13	0.13	0.13	0.13	0.13	0.21	0.18	0.15	0.14
	SD	0.31	0.34	0.34	0.33	0.34	0.34	0.41	0.38	0.35	0.35
	N	350	243	545	376	542	380	515	372	1952	1371
Working full-time	Mean	0.19	0.17	0.24	0.16	0.30	0.30	0.46	0.42	0.31	0.27
	SD	0.39	0.38	0.43	0.37	0.46	0.46	0.50	0.49	0.46	0.45
	N	341	241	537	371	536	374	514	369	1928	1355
Hours worked per week	Mean	13.41	13.29	15.41	12.22	18.46	17.82	23.49	21.24	17.91	16.28
	SD	17.94	18.31	18.94	17.10	20.18	19.13	22.02	20.07	20.24	19.01
	N	341	241	537	371	536	374	461	333	1875	1319
Maternal earnings	Mean	7254.38	7369.66	9868.70	9589.23	11391.83	10225.77	15816.61	14299.47	10967.79	10297.90
	SD	8228.35	8819.11	11378.61	10740.29	13457.24	12031.23	15778.59	13499.31	12794.27	11614.55
	N	540	382	536	371	538	375	488	352	2102	1480
Non-maternal earnings	Mean	12995.78	11627.40	12777.73	11599.40	11883.79	11700.49	.	.	12554.16	11642.68
	SD	16658.22	15535.83	19243.72	18699.39	18536.69	19405.24	.	.	18172.69	17927.07
	N	519	373	519	360	515	368	.	.	1553	1101
Total HH earnings	Mean	20272.44	18824.01	22906.13	21863.73	23788.13	22373.06	.	.	22322.04	20998.00
	SD	19310.88	18140.77	25586.12	23289.18	25473.48	23974.06	.	.	23672.58	21957.13
	N	515	372	517	356	514	364	.	.	1546	1092
HH has zero earnings	Mean	0.12	0.13	0.15	0.14	0.18	0.18	.	.	0.15	0.15
	SD	0.33	0.34	0.36	0.35	0.38	0.39	.	.	0.36	0.36
	N	515	372	517	356	514	364	.	.	1546	1092

Appendix B

Appendix to Study 2

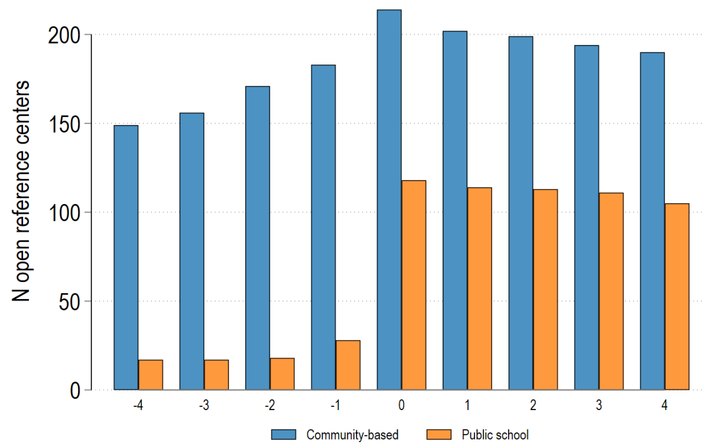
Additional Figures

Figure B.1: Number of childcare centers joining and leaving the NCPK program, 2005-2018



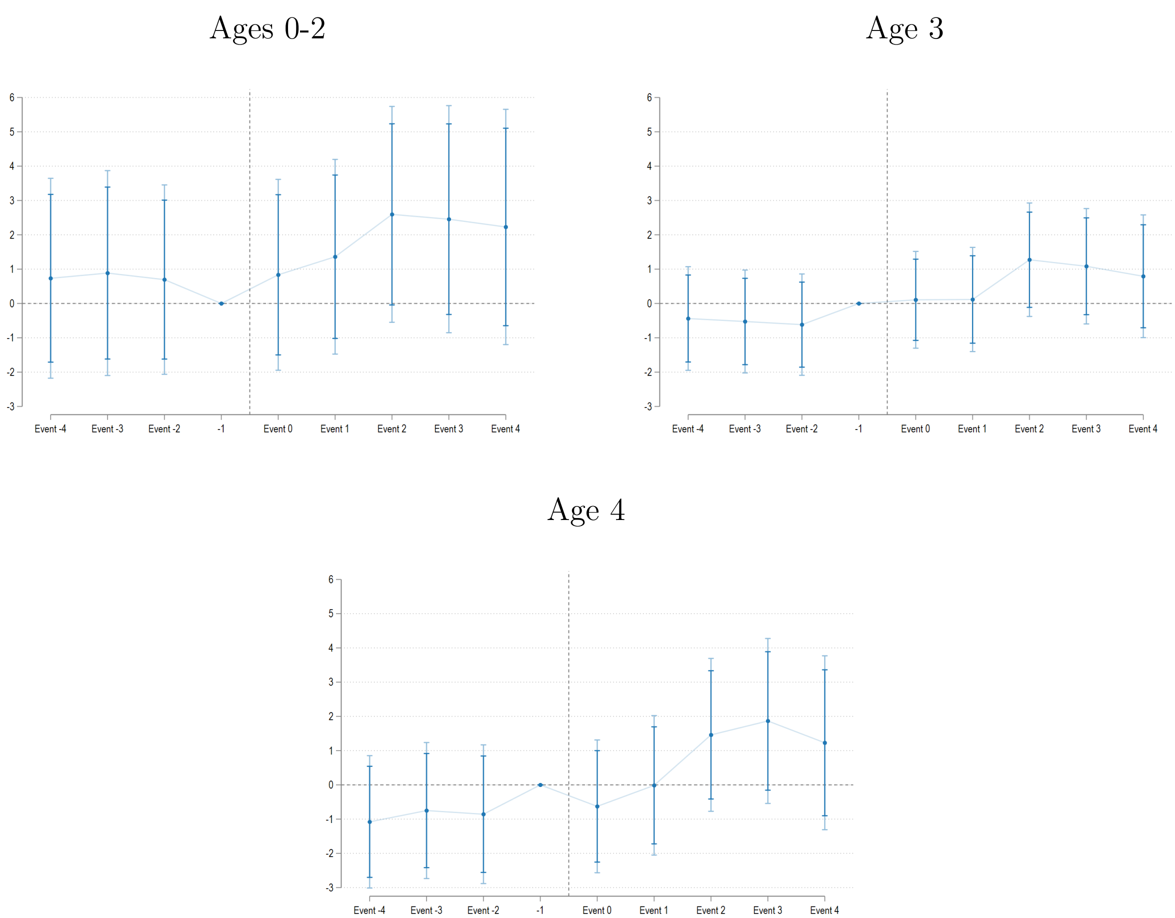
Notes: This figure plots the number of centers that joined (blue) and left (orange) the NCPK program by year. The program started in 2001 as a pilot program with 100 centers. However, I have access to the NCPK data starting in 2005. I cannot distinguish if a center joined the program in 2005 or earlier, therefore I omit 2005 for openings. Similarly, every center is "leaving" the dataset in 2018, then, I cannot identify closures in 2018. Given the accuracy of the data, I use the variation that comes from centers that joined the program between 2012 and 2018.

Figure B.2: Number of reference centers opened by event



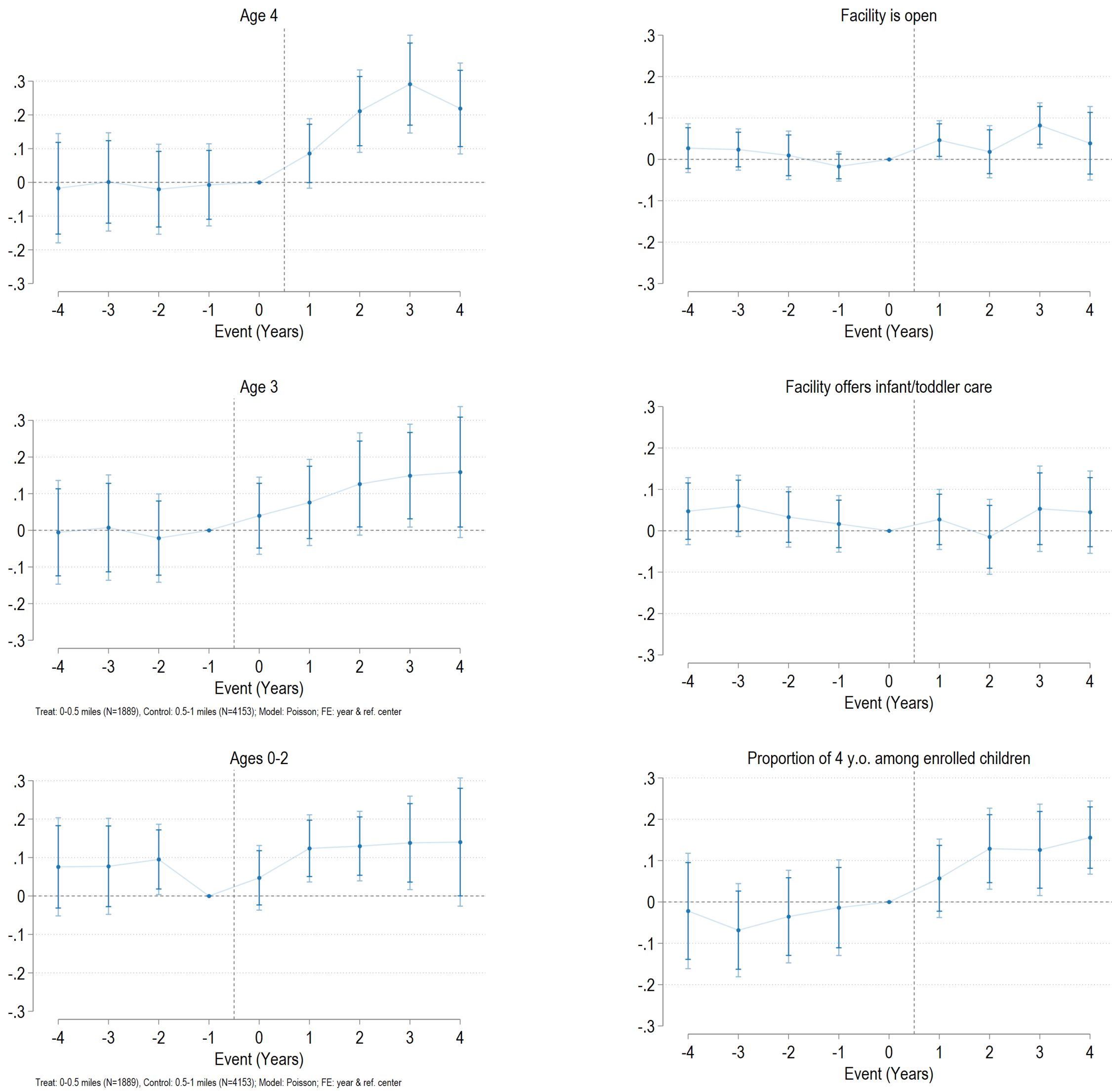
Notes: This figure illustrates the number of NCPK centers that are open by year, relative to the year when they joined the NCPK program (event 0). By construction, all are open at event 0. Hence, the graph shows for how long they had a childcare license before joining the NCPK program (negative events) and whether they stayed open after it (positive events).

Figure B.3: Heterogeneity-robust estimates



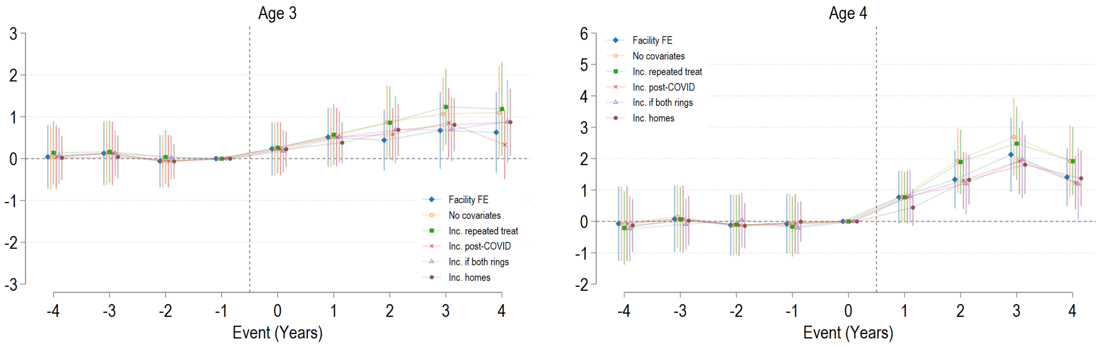
Notes: These figures show the results of the heterogeneity-robust alternative estimator created by Sun and Abraham (2021) to estimate dynamic effects. In short, this approach proposes a way to re-weight each cohort-specific estimate by considering its appropriate share. It was estimated using the *eventstudyinteract* command in STATA (Sun, 2021).

Figure B.4: Dynamic effects of offering NCPK slots on the enrollment in nearby providers, Poisson estimates



Notes: These figures are created by estimating Equation **??** with a Poisson regression (using STATA command *ppmlhdfe*). The figures plot the coefficients of the interactions between each event (year) and "Ever Ring 1" (treatment group) indicator (Equation **??**). The model also includes Reference Center and year fixed-effects, as well as non-interacted event (year) and "Ever Ring 1" indicators, and covariates. Standard errors are clustered at the county level.

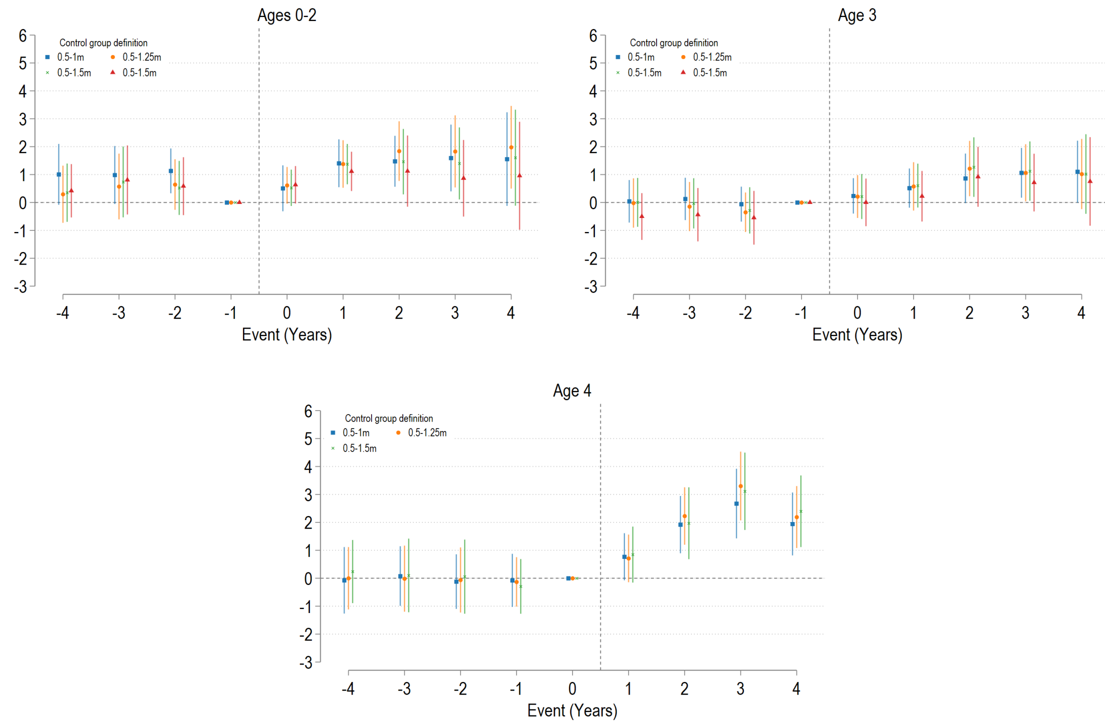
Figure B.5: Robustness of the age-3 and age-4 enrollment results



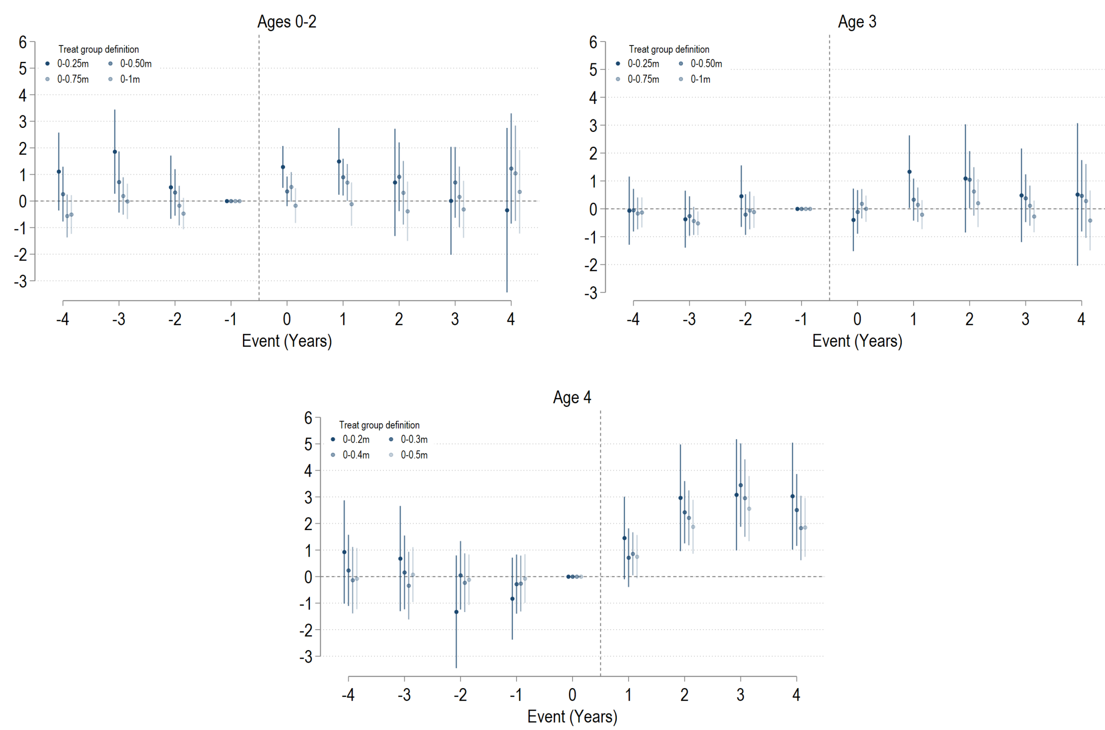
Notes: The figures plot the coefficients of the interactions between each event (year) and "Ever Ring 1" (treatment group) indicator (Equation **??**). The model also includes Reference Center and year fixed-effects, as well as non-interacted event (year) and "Ever Ring 1" indicators, and covariates. Standard errors are clustered at the county level. Each line is derived from a different regression with the following variations: (1) using facility fixed effects instead of reference center fixed effects, (2) removing covariates, (3) including centers that were "treated" in more than one year, (4) including post-COVID years, (5) Including providers that were in both rings at different times, (6) Including family childcare homes.

Figure B.6: Effects on enrollment when varying ring size

Panel A. Treatment group fixed at a 0 to half-a-mile distance, varying control group size

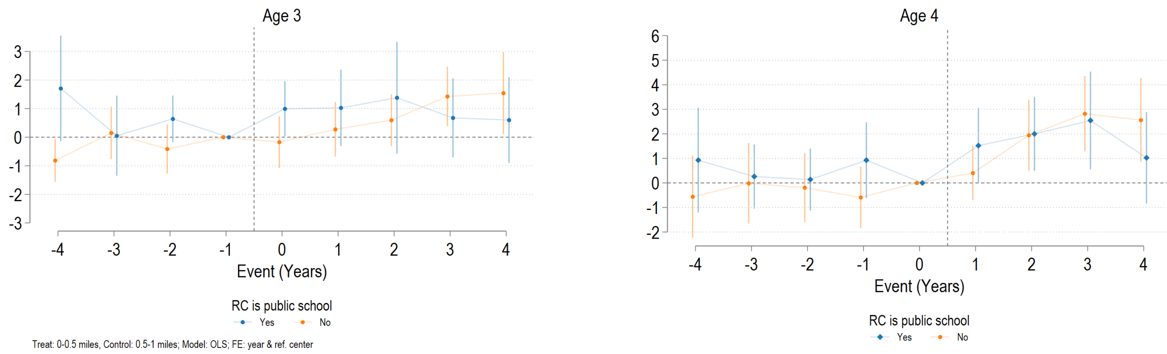


Panel B. Control group fixed at a 1 to 1.5-mile distance, varying treatment group size



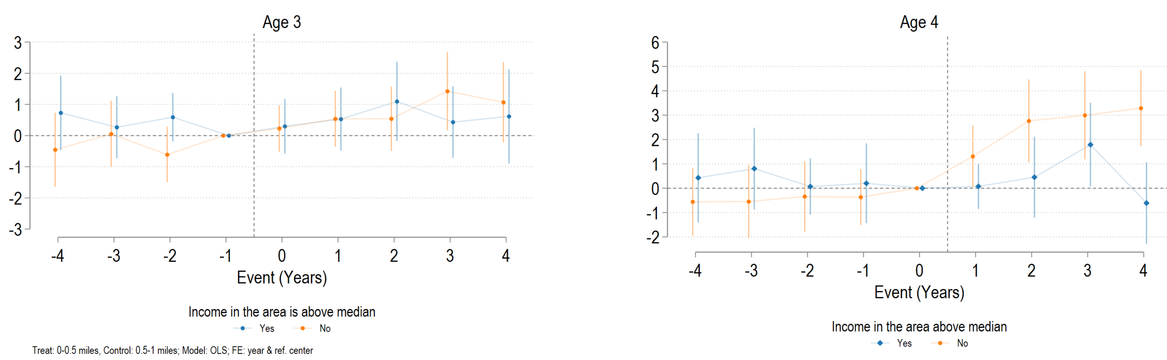
Notes:

Figure B.7: Dynamic effects of offering NCPK slots on the enrollment in nearby providers by NCPK type



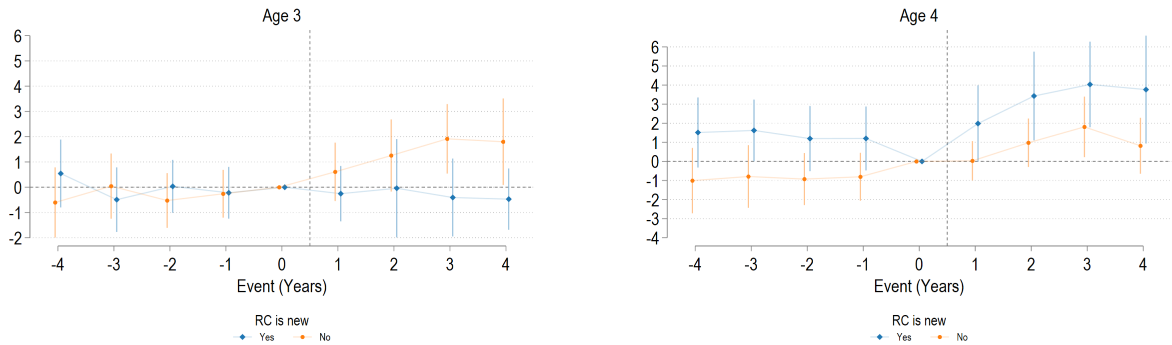
Notes: These figures are created by estimating Equation **??** separately for two subgroups. The figures plot the coefficients of the interactions between each event (year) and "Ever Ring 1" (treatment group) indicator. The model also includes Reference Center and year fixed-effects, as well as non-interacted event (year) and "Ever Ring 1" indicators, and covariates. Standard errors are clustered at the county level.

Figure B.8: Dynamic effects of offering NCPK slots on the enrollment in nearby providers by income in the area



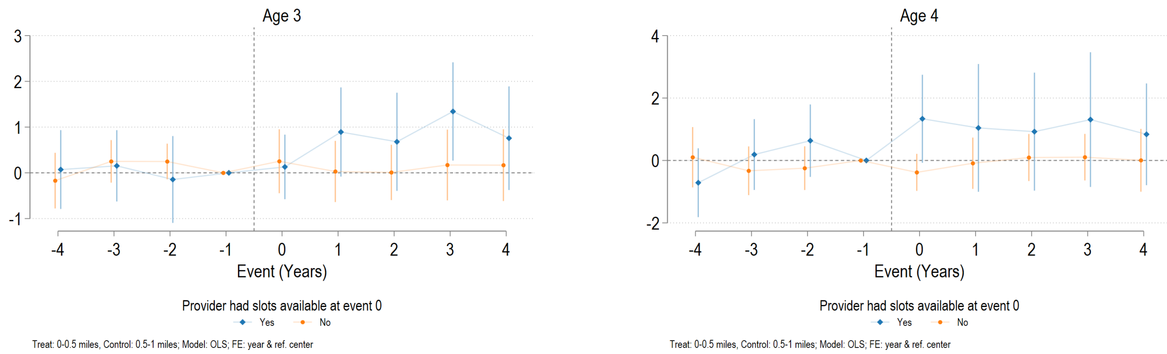
Notes: These figures are created by estimating Equation **??** separately for two subgroups. The figures plot the coefficients of the interactions between each event (year) and "Ever Ring 1" (treatment group) indicator. The model also includes reference center and year fixed-effects, as well as an "Ever Ring 1" indicator. Standard errors are clustered at the county level.

Figure B.9: Dynamic effects of offering NCPK slots on the enrollment in nearby providers by whether RC is new



Notes: These figures are created by estimating Equation **??** separately for two subgroups. The figures plot the coefficients of the interactions between each event (year) and "Ever Ring 1" (treatment group) indicator. The model also includes reference center and year fixed-effects, as well as an "Ever Ring 1" indicator. Standard errors are clustered at the county level.

Figure B.10: Dynamic effects of offering NCPK slots on the enrollment in nearby providers by whether provider had slots available at event 0



Notes: "Having slots available" is defined as having a difference between licensed capacity and actual enrollment of at least 5.

Additional Tables

Table B.1: Treatment and control counts by reference year

	Before exclusions		After exclusions	
	Treatment	Control	Treatment	Control
	(1)	(2)	(3)	(4)
2012	76	134	55	134
2013	52	107	38	107
2014	60	108	45	108
2015	19	23	13	23
2016	32	43	24	43
2017	18	11	14	11
2018	46	64	40	64
All	303	490	229	490

Notes: Treatment is defined as being within 0.5 miles of a reference center, while control is defined as being between 0.5 and 1 mile away from a reference center.

Table B.2: Dynamic effects of offering NCPK slots on the enrollment in nearby providers by age and the probability of being open

	# Children enrolled			Facility characteristics		
	Ages 0-2	Age 3	Age 4	Facility is open	Facility enrolls 0-3yo kids	Facility enrolls 4yo kids
Event -4=1 × treat=1	0.93	-0.12	-0.34	0.03	0.01	-0.00
	(0.79)	(0.58)	(0.64)	(0.03)	(0.03)	(0.02)
Event -3=1 × treat=1	1.08	0.05	-0.03	0.03	0.02	-0.02
	(0.84)	(0.50)	(0.60)	(0.02)	(0.03)	(0.02)
Event -2=1 × treat=1	1.38**	-0.21	-0.35	0.02	0.00	-0.01
	(0.68)	(0.44)	(0.56)	(0.02)	(0.03)	(0.02)
Reference year	0.00	0.00	0.00	0.00	0.00	0.00
	(.)	(.)	(.)	(.)	(.)	(.)
Event 0=1 × treat=1	0.47	0.18	-0.04	0.01	-0.03	0.01
	(0.56)	(0.43)	(0.57)	(0.02)	(0.02)	(0.02)
Event 1=1 × treat=1	1.67**	0.49	0.71	0.04	-0.01	0.02
	(0.74)	(0.59)	(0.69)	(0.03)	(0.03)	(0.02)
Event 2=1 × treat=1	2.37***	0.98	1.95**	0.02	-0.03	0.04**
	(0.88)	(0.66)	(0.83)	(0.03)	(0.04)	(0.02)
Event 3=1 × treat=1	2.49***	1.47**	2.76***	0.06*	0.01	0.05**
	(0.94)	(0.67)	(0.89)	(0.03)	(0.04)	(0.02)
Event 4=1 × treat=1	2.60**	1.35**	2.56***	0.05	0.03	0.06**
	(1.05)	(0.67)	(0.91)	(0.04)	(0.04)	(0.02)
treat	0.54	-0.54	-1.38	0.03	-0.01	0.03
	(2.04)	(1.14)	(1.64)	(0.04)	(0.05)	(0.04)
Observations	4435	5043	5355	5508	5508	4696

Notes: This table presents the results from estimating Equation ??. The model also includes Reference Center and year fixed-effects, as well as a treatment indicator. Standard errors are clustered at the county level.

Table B.3: Heterogeneous effects of joining NCPK on nearby providers

	Ages 0-3			Age 4		
	Base	NCPK Type	Income	Base	NCPK Type	Income
Within 0.5m=1 × Post=1	1.658	2.427	2.522*	1.640***	2.073***	2.537***
	(1.100)	(1.484)	(1.393)	(0.525)	(0.720)	(0.647)
Within 0.5m=1	-0.010	0.602	0.667	-1.547	-1.492	-0.711
	(3.077)	(3.627)	(3.378)	(1.645)	(1.773)	(2.057)
Post=1	0.835	0.832	0.319	-0.523	-0.534	-1.069**
	(0.650)	(0.806)	(0.936)	(0.360)	(0.436)	(0.461)
Within 0.5m=1 × RC is public school=1		-1.659			-0.170	
		(3.323)			(1.756)	
Post=1 × RC is public school=1		-0.128			-0.041	
		(1.156)			(0.630)	
Within 0.5m=1 × Post=1 × RC is public school=1		-1.999			-1.175	
		(2.150)			(0.983)	
Within 0.5m=1 × Income in the area above median=1			-1.582			-2.146
			(3.857)			(2.304)
Post=1 × Income in the area above median=1			1.156			1.234*
			(1.235)			(0.650)
Within 0.5m=1 × Post=1 × Income in the area above median=1			-2.021			-2.097**
			(1.976)			(1.052)
Observations	5083	5083	5083	5355	5355	5355

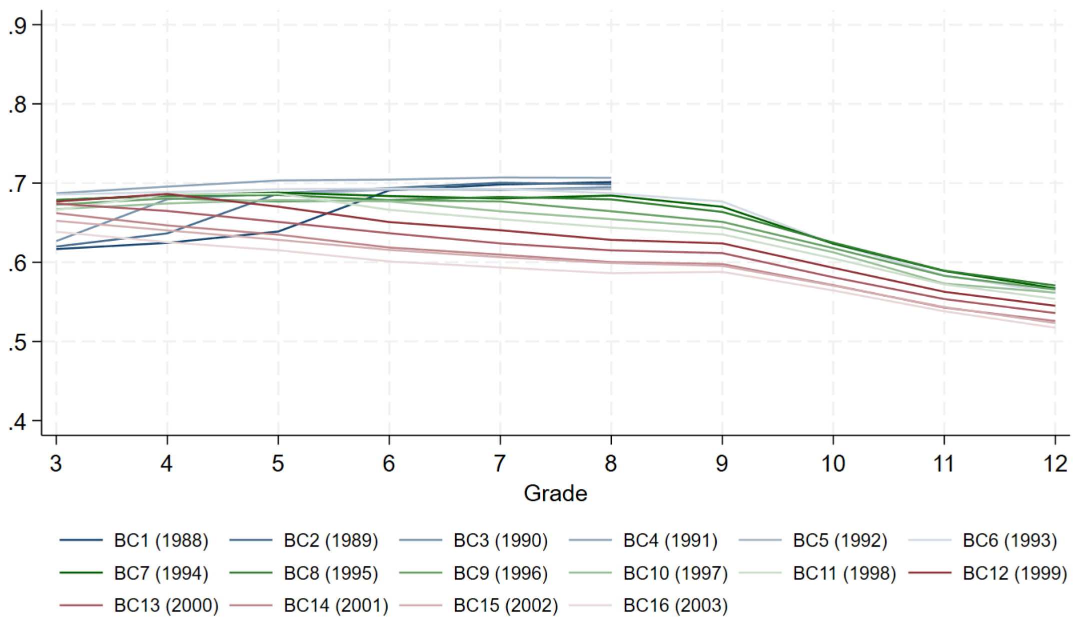
Notes:

Appendix C

Appendix to Study 3

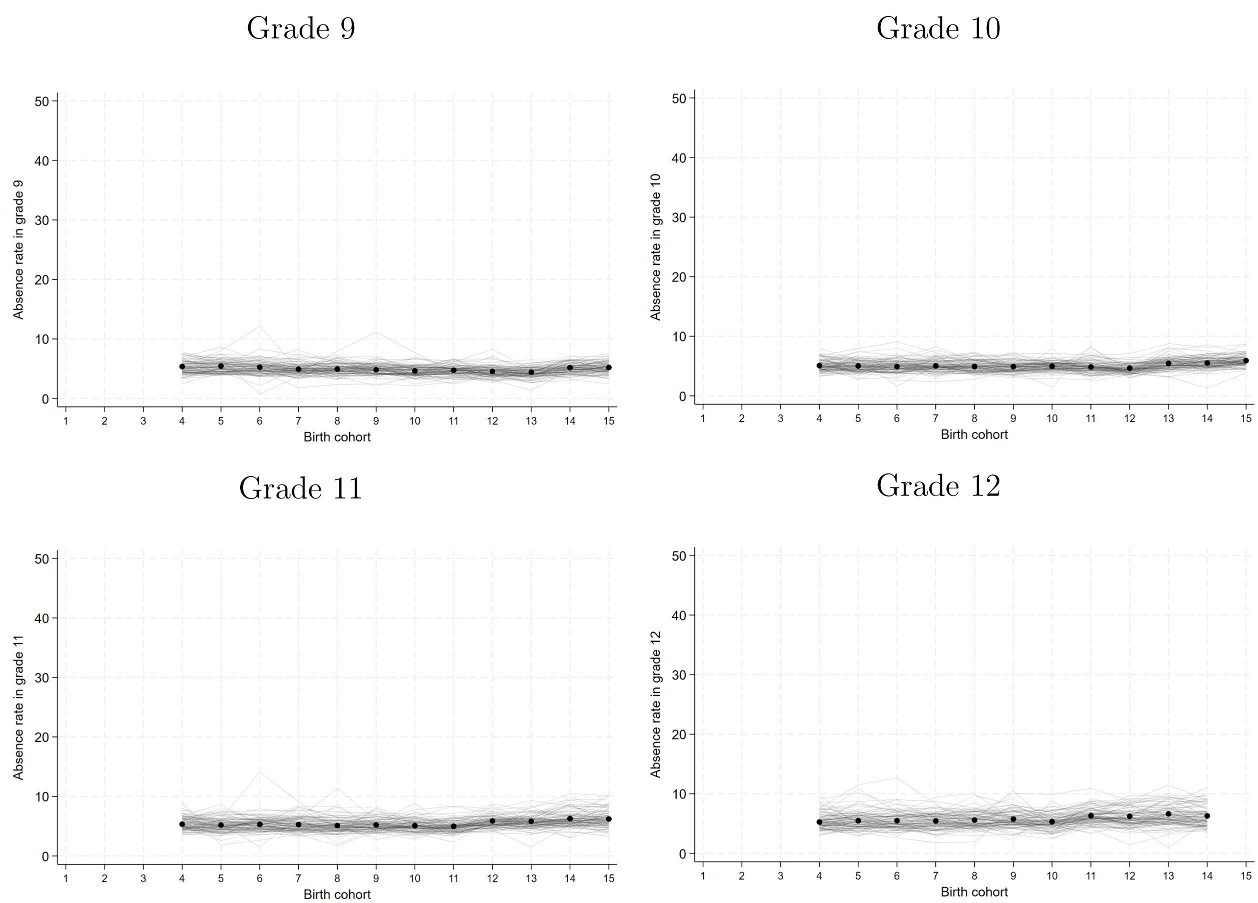
Additional Figures

Figure C.1: Proportion of cohort observed in each school grade



Notes: High school membership files were not available for the early cohorts.

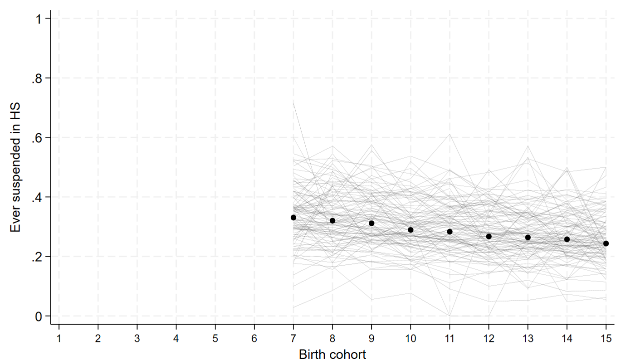
Figure C.2: High school absenteeism



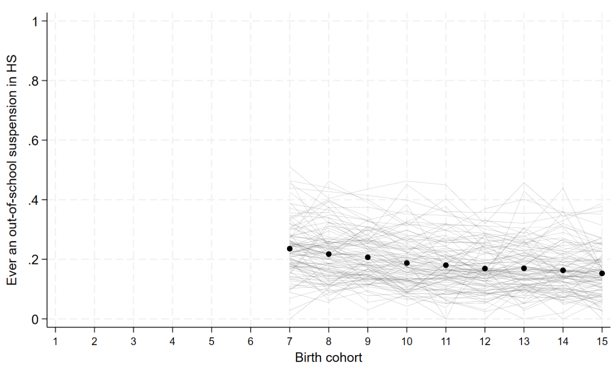
Notes: Gray lines correspond to each of the 100 counties and black dots represent the average across all individuals in the cohort. The sample is restricted to individuals who were observed in the public school system in grade 8.

Figure C.3: High School Suspensions

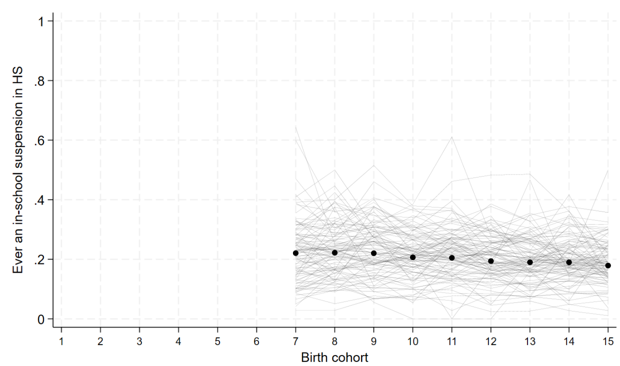
Ever suspended in high school



Ever suspended in HS: out-of-school

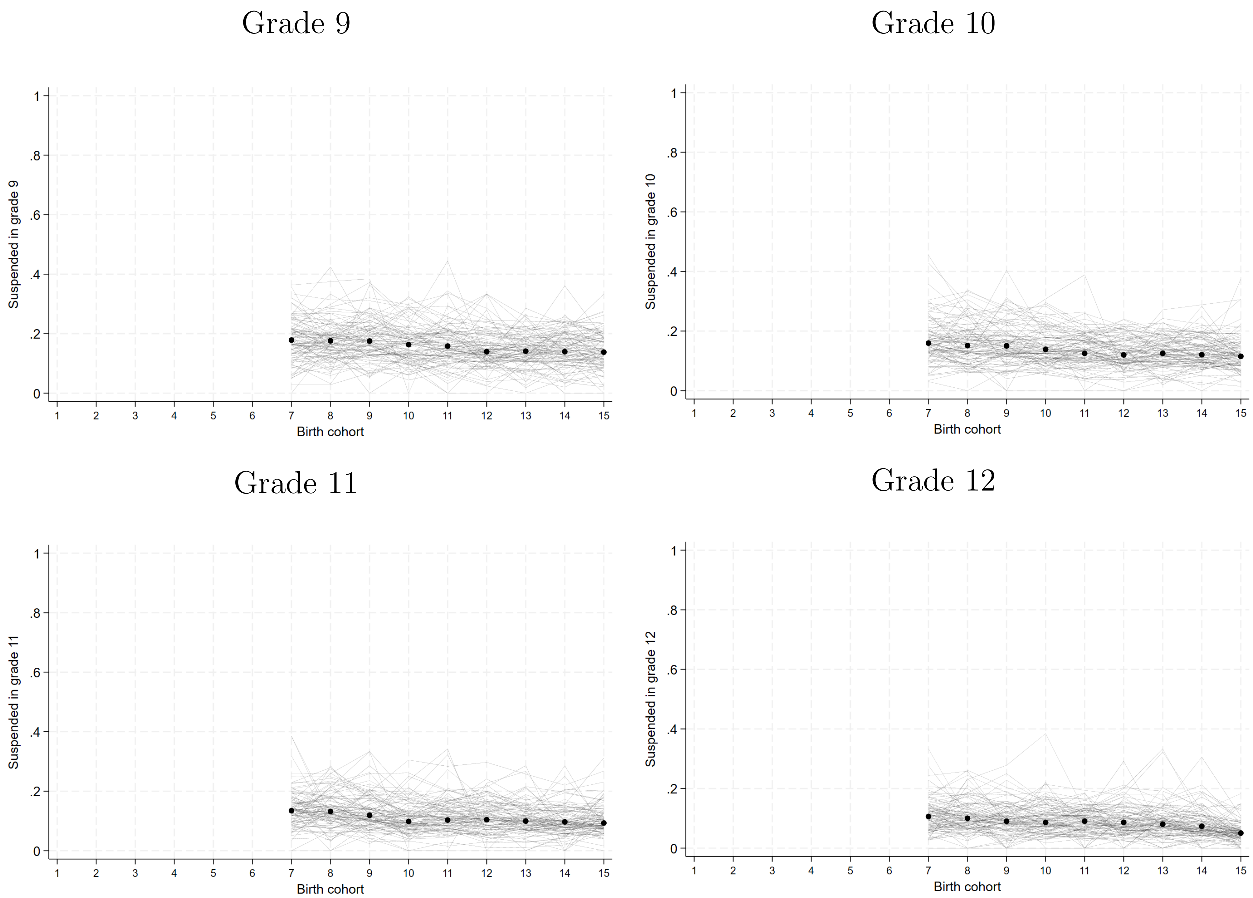


Ever suspended in high school: in-school



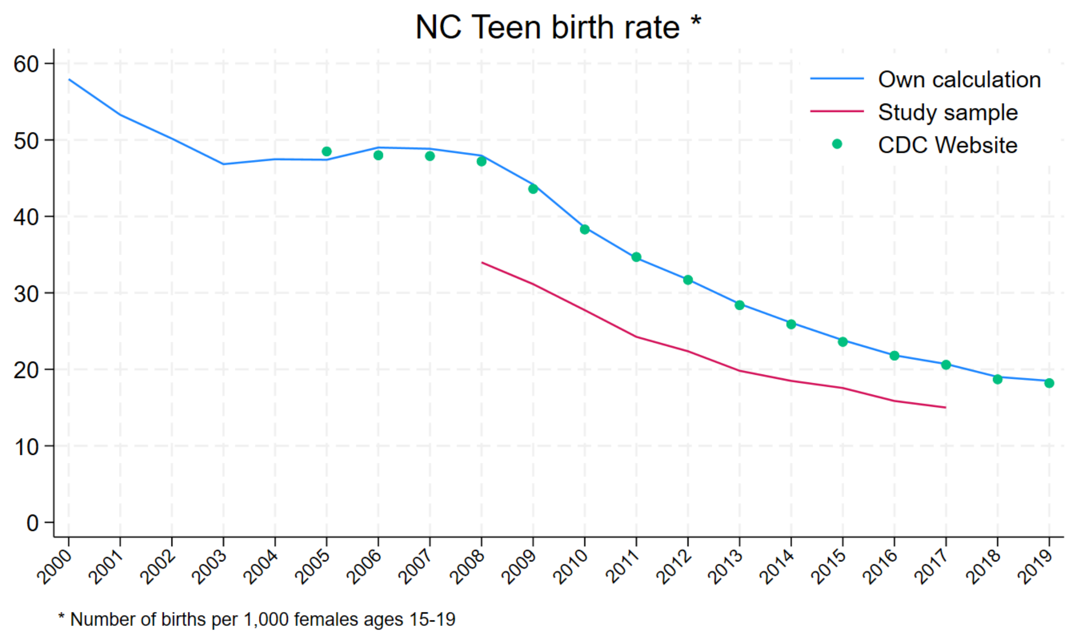
Notes: Gray lines correspond to each of the 100 counties and black dots represent the average across all individuals in the cohort. The sample is restricted to individuals who were observed in the public school system in grade 8.

Figure C.4: High School Suspensions by Grade



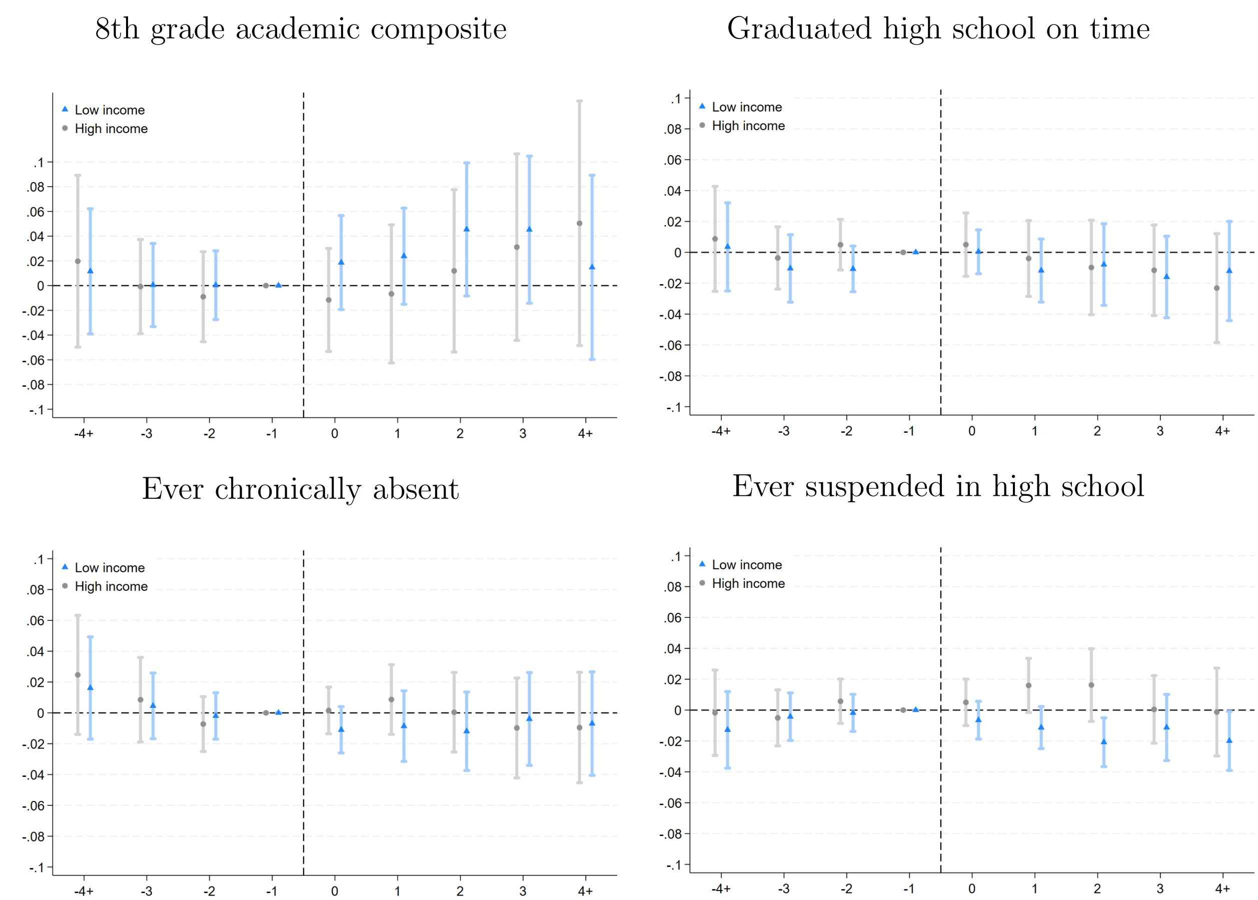
Notes: Gray lines correspond to each of the 100 counties and black dots represent the average across all individuals in the cohort. The sample is restricted to individuals who were observed in the public school system in grade 8.

Figure C.5: Comparison of teen birth rates in North Carolina



Notes: The blue line was calculated based on the number of births from teenage mothers in a given year (based on all the birth records datasets that we obtained for our analysis) and the estimated population between 15 and 19 years old in the same year. The red line was calculated based on the number of women included in our analytic sample who were 15-19 years old in a given year, and how many were observed as teen mothers in birth records in that year.

Figure C.6: Event-study figures of the effects on NCPK on education outcomes (small sample)



Notes: For these outcomes, due to data availability, we restrict the sample to individuals born after October 17th, 1993. Event -4+ is a combination of observations in events -4 and -5, and has a smaller sample size.

Additional Tables

Table C.1: Effects of NCPK on being observed in public schools

	All cohorts						Cohorts 6-16			
	G3	G4	G5	G6	G7	G8	G9	G10	G11	G12
NCPK funds (in 000s)	0.00	0.00	-0.00	-0.01*	-0.01*	-0.01	-0.01	-0.01	-0.01**	-0.01
	(0.01)	(0.01)	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Mean	0.66	0.67	0.67	0.67	0.66	0.66	0.63	0.60	0.57	0.55
Observations	816287	816287	816287	816287	816287	816287	574708	574708	574708	574708

Notes: * p<.10, ** p<.05, *** p<.01. Data on school enrollment in grades 3-8 is derived from end-of-year tests, while grades 9-12 are derived from course membership files (only available starting from birth cohort 6.

Table C.2: Descriptive statistics

		NCPK funds in 000s	Smart Start funds in 000s	Had a teen birth	Graduated HS	Graduated HS on time	Ever chronic absent	Ever suspended	Grade 8 academic comp.
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
All	mean	0.19	1.35	0.12	0.83	0.69	0.23	0.28	-0.00
	sd	0.35	1.17	0.33	0.38	0.46	0.42	0.45	1.00
	n	507001	507001	507001	440720	440720	399055	298658	494223
BC 1	mean	0.00	0.00	0.18					-0.00
	sd	0.00	0.00	0.38					1.00
	n	32385	32385	32385	0	0	0	0	31652
BC 2	mean	0.00	0.05	0.18					0.00
	sd	0.00	0.12	0.38					1.00
	n	33896	33896	33896	0	0	0	0	33412
BC 3	mean	0.00	0.20	0.17	0.74	0.64			0.00
	sd	0.00	0.34	0.38	0.44	0.48			1.00
	n	34249	34249	34249	34249	34249	0	0	33656
BC 4	mean	0.00	0.39	0.16	0.76	0.65	0.25		0.00
	sd	0.00	0.58	0.37	0.43	0.48	0.43		1.00
	n	34354	34354	34354	34354	34354	34354	0	33511
BC 5	mean	0.00	0.60	0.15	0.78	0.65	0.24		-0.00
	sd	0.00	0.82	0.36	0.42	0.48	0.42		1.00
	n	33918	33918	33918	33918	33918	33918	0	32738
BC 6	mean	0.00	0.88	0.14	0.80	0.66	0.24		-0.00
	sd	0.00	1.05	0.34	0.40	0.48	0.43		1.00
	n	33378	33378	33378	33378	33378	32459	0	32162
BC 7	mean	0.00	1.21	0.12	0.81	0.67	0.23	0.33	-0.00
	sd	0.00	1.14	0.33	0.39	0.47	0.42	0.47	1.00
	n	32999	32999	32999	32999	32999	32000	32046	31796
BC 8	mean	0.00	1.60	0.11	0.83	0.69	0.22	0.32	0.00
	sd	0.00	1.09	0.31	0.38	0.46	0.41	0.47	1.00
	n	32753	32753	32753	32753	32753	31712	31758	31715
BC 9	mean	0.00	1.95	0.11	0.85	0.70	0.22	0.31	-0.00
	sd	0.00	1.01	0.31	0.36	0.46	0.41	0.46	1.00
	n	33039	33039	33039	33039	33039	32148	32190	31932
BC 10	mean	0.03	2.23	0.10	0.85	0.72	0.22	0.29	0.00
	sd	0.06	0.92	0.30	0.35	0.45	0.41	0.45	1.00
	n	33187	33187	33187	33187	33187	32452	32490	32086
BC 11	mean	0.26	2.37	0.09	0.86	0.71	0.21	0.28	-0.00
	sd	0.18	0.80	0.29	0.35	0.45	0.41	0.45	1.00
	n	34459	34459	34459	34459	34459	33813	33835	33321
BC 12	mean	0.46	2.39	0.09	0.87	0.72	0.21	0.27	-0.00
	sd	0.25	0.73	0.28	0.34	0.45	0.41	0.44	1.00
	n	34343	34343	34343	34343	34343	33783	33810	33466
BC 13	mean	0.53	2.24	0.08	0.87	0.73	0.23	0.26	-0.00
	sd	0.30	0.67	0.27	0.34	0.44	0.42	0.44	1.00
	n	35333	35333	35333	35333	35333	34783	34818	34771
BC 14	mean	0.67	2.07	0.08	0.88	0.74	0.26	0.26	-0.00
	sd	0.38	0.59	0.27	0.33	0.44	0.44	0.44	1.00
	n	34631	34631	34631	34631	34631	34090	34130	34265
BC 15	mean	0.82	1.97	0.06	0.88	0.75	0.26	0.24	0.00
	sd	0.52	0.54	0.24	0.33	0.43	0.44	0.43	1.00
	n	34077	34077	34077	34077	34077	33543	33581	33740

Notes: BC= Birth Cohort. NCPK = North Carolina pre-Kindergarten program. The 8th grade academic composite was constructed by standardizing reading and math end-of-grade scores within the academic year, averaging the two, and re-standardizing it within birth cohort. Absences and suspensions are only observed for individuals who enrolled in a public school during at least one high school grade.

Table C.3: Effects of NCPK on having a teenage birth, by age

	(1) All counties	(2) High Income	(3) Low income
Gave birth before age 18			
NCPK funds (in 000s)	-0.002	-0.001	-0.001
	(0.002)	(0.003)	(0.003)
Mean	0.044	0.040	0.053
Observations	507001	362136	144865
Gave birth at ages 18 or 19			
NCPK funds (in 000s)	-0.004	0.000	-0.011**
	(0.003)	(0.004)	(0.004)
Mean	0.089	0.081	0.108
Observations	507001	362136	144865

Notes: * p<.10, ** p<.05, *** p<.01