NBER WORKING PAPER SERIES

THE IMPACT OF UNCONDITIONAL CASH TRANSFERS ON PARENTING AND CHILDREN

Patrick K. Krause
ⓡ
Elizabeth Rhodes
ⓡ
Sarah Miller
ⓡ
Alexander W. Bartik
ⓡ
David E. Broockman
ⓡ
Eva Vivalt

Working Paper 34040
http://www.nber.org/papers/w34040

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
July 2025, Revised February 2026

Authorship order is certified random (AEA confirmation code: PXhtgWg-tb7Q). Many people were instrumental in the success of this project. The program we study and the associated research were supported by generous private funding sources, and we thank the nonprofit organizations that implemented the program. We are especially grateful to Ethan Sansom, Kevin Didi, Marc-Andrea Fiorina, Sabrina Liu, and Joshua Lin for their research assistance, which was instrumental to the analysis in this paper. We also thank Isaac Ahuvia, Francisco Brady, Jack Bunge, Jake Cosgrove, Leo Dai, Rashad Dixon, Anthony McCanny, Oliver Scott Pankratz, Sophia Scaglioni, and Angela Wang-Lin for their excellent research assistance on the ORUS project overall. Tess Cotter, Karina Dotson, Aristia Kinis, Alex Nawar, Sam Manning, and Elizabeth Proehl were invaluable contributors through their work at OpenResearch. Carmelo Barbaro, Janelle Blackwood, Katie Buitrago, Melinda Croes, Crystal Godina, Kelly Hallberg, Kirsten Jacobson, Timi Koyejo, Misuzu Schexnider, and the staff of the Inclusive Economy Lab at the University of Chicago more broadly have provided key support throughout all stages of the project. We are grateful to Luke Hyde for sharing his expertise on how to measure parenting quality and child development. We received helpful feedback throughout the project from numerous researchers and from our advisory board, as well as from seminar and conference participants. This study was approved by Advarra Institutional Review Board (IRB).We received funding for this paper from NIH grant R01-HD103699. The conclusions of this research do not necessarily reflect the opinions or official position of the Texas Education Agency, the Texas Higher Education Coordinating Board, TexasWorkforce Commission, or the State of Texas. Any views expressed are those of the authors and not those of the U.S. Census Bureau. The Census Bureau has reviewed this data product to ensure appropriate access, use, and disclosure avoidance protection of the confidential source data used to produce this product. This research was performed at a Federal Statistical Research Data Center under FSRDC Project Number 3011. (CBDRB-FY25-P3011-R12471). The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

NBER working papers are circulated for discussion and comment purposes. They have not been peer-reviewed or been subject to the review by the NBER Board of Directors that accompanies official NBER publications.

© 2025 by Patrick K. Krause ⓡ Elizabeth Rhodes ⓡ Sarah Miller ⓡ Alexander W. Bartik ⓡ David E. Broockman ⓡ Eva Vivalt. All rights reserved. Short sections of text, not to exceed two paragraphs, may be quoted without explicit permission provided that full credit, including © notice, is given to the source.

The Impact of Unconditional Cash Transfers on Parenting and Children
Patrick K. Krause ⓡ Elizabeth Rhodes ⓡ Sarah Miller ⓡ Alexander W. Bartik ⓡ David E. Broockman ⓡ Eva Vivalt
NBER Working Paper No. 34040
July 2025, Revised February 2026
JEL No. H0, I28, I3, J13

ABSTRACT

This paper examines the impact of a large, randomized cash transfer on parental behaviors, investment in children, children's social, behavioral, and educational outcomes, and pregnancy and childbearing. We find that parents who were randomly selected to receive a $1,000 per month unconditional cash transfer for three years spent more on their children each month and reported better parenting behaviors (such as supervising their children more closely) compared to those randomized to receive $50 per month over the same period. However, possibly due to this closer monitoring, parents in the treatment group also reported that their child was experiencing more developmental difficulties and stress. Parents with the lowest baseline incomes experienced the largest improvements in parenting; among these parents, the transfer also increased the use and quality of non-parental child care. The transfer did not have a meaningful effect on most educational outcomes measured in school administrative records, nor did it affect characteristics of the home environment, child food security, exposure to homelessness, or parental satisfaction. Although treated families were more likely to move, we did not detect changes in most measures of neighborhood quality, though proximity to child-focused amenities such as daycares appeared to increase in the treatment group relative to the control group. The transfer did not affect childbearing, pregnancy, or outcomes related to contraception. While the transfer reduced parents' stress and mental distress in the first year of the program, these effects were short-lived and dissipated by the second year of the transfer, analogous to what was documented previously in the full population of participants.

Patrick K. Krause
OpenResearch
patrick@openresearchlab.org

ⓡ

Elizabeth Rhodes
OpenResearch
elizabeth@openresearchlab.org

ⓡ

Sarah Miller
University of Michigan
Ross School of Business
and NBER
mille@umich.edu

ⓡ

Alexander W. Bartik
University of Illinois Urbana-Champaign
abartik@illinois.edu

ⓡ

David E. Broockman
University of California, Berkeley
dbroockman@berkeley.edu

ⓡ

Eva Vivalt
University of Toronto
Department of Economics
eva.vivalt@utoronto.ca

A randomized controlled trials registry entry is available at
https://www.socialscienceregistry.org/trials/6750

1 Introduction

Children born into lower income families face worse lifelong trajectories on a number of dimensions compared to their higher income peers, with disadvantages emerging in childhood and persisting through the transition to adulthood (Case et al., 2002; Chetty et al., 2017; Duncan and Magnuson, 2013; Duncan and Murnane, 2011, and others). These disparities may be driven in part by differences in time, monetary, and in-kind investments during childhood and adolescence, as lower income families may struggle to invest at the same level their higher income counterparts (e.g., Carneiro and Heckman, 2002; Guryan et al., 2008; Kornrich and Furstenberg, 2013; Coley et al., 2016). Reflecting this logic, much of the United States' federal and state poverty alleviation efforts—such as the Earned Income Tax Credit and the Temporary Aid to Needy Families program—deliver cash transfers to parents in need so they can provide for their children. Expanding or creating new programs that distribute cash to low-income parents represents an active area of policy debate at the federal level, and several additional unconditional cash transfer programs specifically targeting parents have been recently proposed or implemented at various other levels of government.[1] These transfers put cash directly in the hands of parents, who may be best positioned to assess their children's needs and invest where the payoffs are highest. In turn, this could lead to improved outcomes for children across a number of dimensions, such as increasing educational achievement and social and emotional development during childhood, and help set children up for long-term success.

At the same time, other factors may temper our optimism about the ability of a cash transfer to improve outcomes for children in families who receive them. Parents may not always prioritize investments associated with improvements in children's outcomes, or they may not know which investments will be most productive. Furthermore, the fungibility of an unconditional transfer asks parents to balance their desire to invest in their children with their household's other needs and priorities. As a result, cash transfers may end up being spread thin across many types of consumption, some of which likely have negligible impacts on children's outcomes. In this case, the impact of even a large cash transfer on children's outcomes may be modest. Empirical evidence is needed to disen-

[1] For example, the city of Flint, Michigan recently implemented an unconditional cash transfer program, RxKids, that provides $1,500 to pregnant women mid-pregnancy, with additional transfers of $500 per month for the first year of their child's life (Hanna et al., 2024). Similar programs are currently being rolled out to other localities.

tangle such potentially countervailing influences of cash on child outcomes and to help policymakers understand the causal effect of parental income on investments in children and children's well-being.

In this paper, we examine the impact of a large cash transfer on parenting and children's outcomes over a three year period. The OpenResearch Unconditional income Study (ORUS) provided 1,000 randomly-selected low-income adults between the ages of 21 and 40 with a monthly transfer of $1,000 per month for three years, with a control group of 2,000 participants receiving $50 over the same period. Many of these participants were parents, and the households that participated in the program included more than 4,000 children. This transfer was large relative to baseline income and sustained for three years, providing an ideal setting to better understand how income affects parents and children.

We rely on several data sources to investigate this relationship. First, we surveyed participants about themselves and their children using a variety of methods, including web-based and enumerated telephone surveys. These surveys generally had high response rates and low differential attrition, giving us confidence in the survey data quality. The surveys covered a large number of topics, including parenting behavior and quality, parents' reports of their children's development and stress, the use and quality of non-parental care such as daycare and babysitters, precise information on address (allowing us to link to measures of neighborhood quality), and characteristics of the home environment. Second, we obtained consent from most participants to link them and their children to administrative records. We match children to state administrative records on K-12 educational outcomes, such as attendance, enrollment, grades, disciplinary measures, and performance on statewide standardized tests. We also match older children to national data on college attendance and degree attainment from the National Student Clearinghouse, and we match adult participants to administrative records on their childbearing. These variables provide insight into a broad array of measures capturing many aspects of parental investment and child outcomes over this three year period.

Using these data, we first examine how the cash transfer affected participants' investments in their children. We find that the transfer resulted in a modest but statistically significant improvement in parenting quality of about 0.05 standard deviations, as measured by a well-validated battery of survey questions on this topic. These overall improvements in parenting quality were driven by parental reports that they more closely supervised their children (especially older children). Treated participants were also somewhat less likely to report the use of corporal punishment. These effects on parenting are driven by families with earnings below the poverty level at the time of enrollment,

with the lowest income parents reporting much larger improvements in parenting quality overall and improvements on sub-scales related to the use of corporal punishment, inconsistent discipline, and monitoring and supervision. To a lesser extent, single parents also appear to experience more substantial parenting improvements as a result of the transfer than parents with a partner at baseline. We also see that spending on items specifically related to children, such as clothing or baby items, increased by 11% in the treated group relative to the control group, and we again observe the largest effects among those with the lowest baseline incomes. Spending on items not specifically for children but that likely benefited all household members—such as food—increased as well. Some indicators of parental involvement also showed suggestive improvements. For example, parents were more likely to report attending a meeting at their child's school in the past year. For parents of young children, we do not find much evidence of a change in childcare quality or quantity in the full sample, although those with low incomes at baseline were more likely to report using childcare. We observe no detectable effect of the transfer on measures of how satisfied participants feel with their parenting experience and can rule out small effects on these outcomes. Parents' stress and mental health improves the first year of the transfer, but these effects fade out by year two, mirroring the results from the full population of participants reported in Miller et al. (2025). And, although the cash transfer appeared to induce parents to move to new neighborhoods and housing units (as also reported in Bartik et al., 2024), we find limited evidence that children of treated participants were exposed to higher quality home environments or neighborhoods along most measures. However, we do detect a modest increase in the presence of child-related amenities (schools and daycares) in the area, although this effect does not remain statistically significant after we adjust our inference to control for the false discovery rate (FDR).

We next examine whether the transfer affected children's outcomes. When asked directly about the stress, difficulties, and conduct or behavioral problems experienced by children in their household, participants in the treated group reported worse outcomes, especially for older children. It is possible that the worse outcomes reported by parents reflect increased monitoring among those who received the transfer, leading them to notice problems that they might otherwise have missed. More research is needed to establish whether these negative effects on parent-reported subjective measures of stress and behavioral problems translate into worse long-term outcomes, or whether they merely reflect increased awareness (which could plausibly result in better long-term outcomes for these children through earlier recognition and intervention).

Examining administrative records, we do not find consistent evidence of an effect of the transfer on measures related to K-12 education, including attendance, repeating a grade, disciplinary actions such as suspensions or expulsions, or measures of school quality, and can rule out small improvements (of about 0.02 standard deviations) when these measures are aggregated. Within the subsample of students located in Illinois, we find worse grades among children in the treated group; however, these negative effects are not statistically significant after adjusting our inference for multiple comparisons. The cash transfer also may have increased the probability that students in this group attended school in a "hybrid" format (i.e., neither fully in person nor fully virtual) in the first year of the study, which occurred during the COVID-19 pandemic.[2] It is possible the pandemic setting affected the impact of the transfer on educational outcomes during this particularly turbulent time period. Among participants' children of college-going age, we do not find a statistically significant effect of the transfer on the number of months of enrollment in college or on the probability the child completed a post-secondary degree. However, our confidence intervals include large increases and decreases in these measures, in part due to the relatively small number of children in the study who were old enough to attend college by the end of the program (about 600).

Finally, we examine the impact of the transfer on childbearing and desire to have children. We find no statistically significant effect of the transfer on the probability that the participant had a new child via birth or adoption during the treatment period, or on the number of new children. These survey-based estimates are confirmed by estimates derived from administrative birth records, which also show no change in children born to participants. Our confidence intervals can rule out moderately-sized (about 18%) increases in childbearing. The transfer also did not affect participants' reported desire to become pregnant (either themselves or, for male participants, with a partner) in the immediate future. We find no effect of the transfer on the efficacy of contraception used among heterosexual participants who were sexually active, reported not desiring a pregnancy, and reported using some form of contraception other than abstinence. However, the analytic sample is quite small after applying these exclusions. We also find no effect of the transfer on overall contraceptive use (including abstaining from sex that could result in a pregnancy) among participants who did not desire to become pregnant.

Our work relates to a large literature on the impact of parental resources on measures of children's well-being and educational achievement. Quasi-experimental evidence on the efficacy of cash

[2]The point estimate on attending school in a "hybrid" format is relatively large, but the result is not significant after adjusting for multiple hypothesis testing.

transfers in improving child outcomes is mixed, with some results showing such transfers improve outcomes while others find minimal impacts (e.g., Akee et al., 2018; Barr et al., 2022; Borra et al., 2022; Hawkins et al., 2025; Cesarini et al., 2016). Most closely related are recent randomized controlled trials (RCTs) of cash transfers set in the United States. Baby's First Years is a landmark study that randomized 400 low-income mothers with newborn babies to receive $333 per month over the course of their infant's early childhood, while 600 mothers were randomized to a control arm receiving $20 per month. Through age 4, the study found no significant differences between treatment and control groups in children's development or health outcomes (Noble et al., 2024). However, analyses through age 3 suggest that the transfer increased the amount of money and time spent on children, and mothers in the treatment group reported engaging in more enrichment activities with their children, although other measures relevant to parenting (such as quality of play or parental stress) did not change (Gennetian et al., 2024; Magnuson et al., 2024). Chelsea Eats was a cash assistance program that provided $400 per month for nine months to 2,213 randomly-selected low-income households, beginning in November 2020, as a response to the recession that accompanied the COVID-19 pandemic. Analysis of this intervention found that those who were selected to receive the cash transfer were less likely to report that their children did not have enough to eat, but did not find an effect on children's school attendance (Liebman et al., 2022). This study did, however, find that the transfer increased pregnancy rates.

We build on these existing results in several ways. First, we bring in administrative records on educational outcomes that allow us to see detailed information on the effect of the transfer on students' school performance in grade school and high school and their attendance and degree attainment at post-secondary institutions. Second, we study a monthly transfer that is much larger in size than these other studies (i.e., $1000 per month versus $333 in Baby's First Years or $400 per month in Chelsea Eats). Depending on how investments in children scale with transfer size, we might expect larger effects in the context of the ORUS study. Third, we provide a variety of new outcomes not included in previous studies, such as detailed information on neighborhood quality and amenities, that are plausibly relevant for understanding the impact of a cash transfer on children's outcomes. Finally, our sample includes over 4,000 children in participant households—substantially more than other samples—and spans all ages, allowing us to examine the impact of a cash transfer throughout childhood, rather than early childhood only. However, this wide range of ages also makes it difficult for us to do a deep dive on measures that are highly age-specific, such as language acquisition.

Overall, our findings demonstrate that cash transfers to parents result in improved parenting behaviors and increased investments in children, although we do not detect improvements in a wide range of children's outcomes as a result of these parenting and investment changes over the three year period we examine. Future work is needed to assess whether these investments generate benefits that emerge over a longer time horizon as children age into adulthood.

2 Background

Compared to their higher-income peers, children born into low-income families experience worse health and educational outcomes during childhood and worse social and economic outcomes later in life. Researchers across disciplines have hypothesized that this relationship may be causal, operating through a variety of potential channels. First, parental income may directly improve parents' ability to invest in their child—for example, by allowing parents to afford medical care, food, higher quality child care and activities, tutoring services, or safer environments in which to raise their children. This proposed causal pathway is sometimes referred to as the "family investment model" (see Conger and Donnellan, 2007; Mayer, 1997) and reflects the idea that investment in children is a normal good that rises as income increases. Alternatively, the "family stress model" (as in Conger et al., 1994) posits that the financial strain of poverty may lead a parent to experience higher levels of stress and mental health problems, which in turn may undermine the parent-child relationship (for example, if stress causes parents to be less warm, emotionally available, engaged, or patient with their children). This deterioration of the parent-child relationship could then hamper the children's social and emotional development. Notably, this model hypothesizes that low income could be detrimental to children by increasing parental stress, even if it does not alter how much time or money parents invest in their children.

The observed relationship between parental income and children's outcomes may also partly capture non-causal associations. Parents living in poverty differ from those with higher incomes on characteristics other than income itself, such as education level, health, or social norms, which could in turn simultaneously affect parental income and child outcomes. Disentangling the causal impact of parental income on children's development and well-being is therefore difficult in the absence of random variation in income.

Several papers have overcome this challenge by using quasi-experimental research designs to

evaluate the impact of policy changes or shocks that put money into the hands of parents.[3] For example, Barr et al. (2022) exploits the fact that an infant born before the end of the calendar year can be included on their parents' tax form, resulting in higher tax refund payments in the first year of the infant's life. Using this discontinuity, the authors find that infants whose families received higher payments have better educational and earnings outcomes later in life. Work by Akee et al. (2018, 2010) takes advantage of family income shocks among the Eastern Band of Cherokee Indians in North Carolina driven by casino dividend payments to show children receiving such payments exhibited improved emotional and behavioral outcomes during adolescence and improved educational outcomes later in life. de Gendre et al. (2021) find that a $3,000 baby bonus reduced hospitalization rates among infants, Bullinger et al. (2023) find that payments made through the Alaska Permanent Fund reduce incidence of referrals of the child to protective services and child mortality, Dahl and Lochner (2012) find that increases in the EITC improved childhood test scores, Bastian and Michelmore (2018) link EITC increases to improved educational and economic outcomes later in life for children in families that benefit, and Milligan and Stabile (2011) find improvements on multiple measures of child well-being and achievement associated with child tax benefits in Canada. At the same time, some studies using quasi-experimental variation in income fail to find beneficial effects. For example, Hawkins et al. (2025) find no effect on either childhood health or education outcomes or later-life economic outcomes associated with a large means-tested transfer to low birthweight children in low-income, low-asset households; Borra et al. (2022) find no impact of a Spanish €2500 baby bonus on school or health outcomes for children; and Cesarini et al. (2016) report mostly null effects of lottery winnings on children's health and academic outcomes.

As noted earlier, some existing studies evaluate settings where unconditional cash transfers are allocated via a randomized controlled trial. The groundbreaking Baby's First Years study randomly allocated a $333 per month transfer to 400 randomly-selected low-income mothers in four US cities who were recruited at the time of the birth of their child, with 600 similar mothers serving as a control group (Noble et al., 2021). Early evidence from this study detected changes in infant brain activity for children in the high cash group (Troller-Renfree et al., 2022), although analysis at later ages did not detect any effects of the transfer on a variety of outcomes related to health, language, or social/emotional development through age 4 (Noble et al., 2021). The study did find that parents in the high cash group spent more money on their children and engaged with their children in more activities compared to

[3] Here, we discuss a few recent examples that we believe are representative, but do not claim to cover this large literature comprehensively. See Page (2024) for a broader overview.

the low cash group, and reported negative (but not statistically significant) effects of the transfer on reports of spanking their child (Gennetian et al., 2024). The authors also documented that the high cash group reported increases in parenting-related stress, which were statistically significant for some measures and time periods. Another large study, Chelsea Eats, found that a randomized $400 per month transfer for nine months during the COVID-19 pandemic increased food spending and reduced food insecurity for children in treated families but had no impact on children's school attendance (Liebman et al., 2022). Analysis of a 15-month, $1000 per month guaranteed income intervention in Los Angeles found that parents who were randomized to receive the payment were more likely to have their children participate in after-school activities such as sports or lessons (Kim et al., 2024).[4]

A separate strand of research investigates the relationship between income or household resources and childbearing. A well-established literature has shown that births are pro-cyclical (see discussion in Kearney and Dettling, 2024). More recently, researchers have investigated the effect of income or wealth/resource shocks at the "micro" level by examining the impact of partner layoffs, local labor demand or price shocks, and policy changes (e.g., Black et al., 2013; Lindo, 2010; Kearney and Wilson, 2018; Cumming and Dettling, 2023). These papers confirm that additional income increases fertility. Results from recent cash transfer RCTs in the United States tend to confirm this result. Using data from the Chelsea Eats study, Liebman et al. (2022) find that the randomized monthly transfer was associated with a roughly 2 percentage point increase in the probability of pregnancy. Analysis of the Baby's First Years intervention showed no statistically significant impact of the monthly cash transfers on the likelihood that participants had another child through the first three years of the study, although the point estimates were consistent with a positive effect of the transfers (Costanzo et al., 2025). Cash could also affect fertility by making it easier to avoid unwanted pregnancies. In their evaluation of a randomized voucher program, Bailey et al. (2023) find that women are sensitive to the price of contraception. If cash transfers help women afford to purchase contraception (or to purchase more efficacious contraception), they may reduce childbearing.

[4]Other randomized guaranteed income interventions have analyzed effects of adult participants without providing results specific to children, or have studied randomized one-time cash gift interventions. For example, see Balakrishnan et al. (2024), Jacob et al. (2022), Pilkauskas et al. (2024), and Jaroszewicz et al. (2023). And, monthly unconditional cash transfers have been studied extensively in the context of developing countries, but detailed data on children's outcomes is often lacking in these contexts; see Crosta et al. (2025) for an overview.

3 The OpenResearch Unconditional income Study (ORUS)

In this section, we provide an abbreviated description of ORUS. Additional details may be found in Bartik et al. (2024); Miller et al. (2025); Vivalt et al. (2025); Broockman et al. (2024). This study was pre-registered, although over the course of the study we deviated from or amended our pre-analysis plan in ways that are described in detail in Appendix Section B.[5]

3.1 Eligibility and Recruitment

Appendix Figure A1 provides a high-level timeline of the study's recruitment and implementation.[6]

The cash transfer program was implemented by two non-profit organizations in Texas and Illinois. It was administered in counties including urban (Dallas and Chicago), suburban, and rural areas. Participants were eligible for the program if they lived in eligible counties, were age 21 to 40 (inclusive) at the time of recruitment, and had total (self-reported) household income in the prior calendar year not exceeding 300% of the Federal Poverty Level (FPL). We excluded participants receiving disability benefits, living in public housing or using a housing choice voucher, or living in a household with a Supplemental Security Income (SSI) recipient.[7]

We assisted the partner organizations in recruiting participants to the program with the goal of over-sampling lower-income participants and achieving adequate representation across those living in rural, suburban, and urban areas. Recruiting materials described the possibility of participating in a new program providing "$50 per month or more" for three years. Applicants were not informed at the outset that they might receive $1,000 per month, although we provided an optional debrief at the end of the study with this information. Most participants (about 87%) were recruited via direct mailers that contained a unique code for each applicant and instructions on how to fill out an eligibility questionnaire online.[8] In total, of the approximately 1.1 million mailers sent, 38,823 individuals responded and completed the eligibility survey, of whom 12,745 were program eligible. We recruited 12% of the sample via ads placed on FreshEBT, a free mobile application used by Supplemental Nutrition Assistance Program (SNAP) recipients to manage their benefits, and 1% of the sample via Facebook

[5] See AEA RCT registry AEARCTR-0006750.

[6] Previous analysis of this experiment is found in Miller et al. (2025), Vivalt et al. (2025) and Bartik et al. (2024). Appendix Figure A1 and Appendix Table A1 are reproduced from those papers. The text in this section is also similar to what is found in those papers.

[7] The exclusions for the means-tested government programs were due to the fact that the guaranteed income program may have made participants ineligible and it could be difficult for participants to re-enroll in these programs at the conclusion of the intervention.

[8] See Miller et al. (2025) for more information on address sampling, incentives, and follow-up.

ads. Although response rates to these recruitment methods were low, we demonstrated in earlier work that participants were nonetheless largely representative of the eligible population in terms of income, geographic distribution, presence of children, household size, age, race, and geographic distribution, though they were somewhat more likely to be a renter and to have a college degree (see, e.g., Miller et al., 2025), and likely differed on other unobservable dimensions such as trust or attentiveness given our recruiting methods. Across all recruitment methods, 43,385 applicants completed the online screening survey and 14,573 were determined to be eligible.

3.2 Randomization and Enrollment

After obtaining a pool of eligible applicants, we conducted two randomizations. First, we randomized applicants into being in the program, for which they could receive either $50 or $1,000 per month. We used our randomization strategy to obtain a representative gender mix, adequate representation of racial and ethnic minority participants, and to over-sample participants from the lowest income households. We achieved this by implementing minimum quotas on these characteristics. As a result, in the first randomization—from the broader pool of applicants to the program participants—the probability of being selected depended on participant characteristics.

Once program participants were selected, they were enrolled by the University of Michigan Survey Research Organization (SRO). Enrollment was conducted in person from October 2019 until March of 2020, at which point enrollment was switched to phone due to the COVID-19 pandemic. During enrollment, program participants completed a baseline intake survey and provided bank account information so program funds could be directly deposited. For participants with no bank account, a no fee/no minimum online bank account was opened for them. Participants could also consent to have their data and their children's data linked to administrative records. Most participants consented at this point (i.e., prior to randomization), but some participants decided to consent after randomization and we include the data from these participants throughout the paper to improve statistical power. In total, 87.5% consented to data linkages, and consent rates were reasonably balanced across treatment arms: 86.9% consented in the control group and 88.8% consented in the treatment group. Enrollment concluded in October 2020. To keep participants engaged and to collect additional baseline information, all enrolled participants received monthly surveys and an unconditional $50 per month payment for the duration of the enrollment period.

The second and focal randomization occurred once enrollment ended. We assigned participants to

either continue receiving $50 per month ("control group") or to receive $1,000 per month ("treatment group") for 3 years. Unlike the first randomization, this assignment did not depend on participant characteristics and all participants had a 1/3 probability of being assigned to treatment.[9] The comparison of outcomes across these two treatment arms is the focus of our analysis. Additional details on this randomization are found in Appendix Section A.

Of the 3,000 participants in the study, 1,729 reported having children or a child living in their household at baseline and these participants had an average of 2.45 children each. An additional 247 participants who were not parents at baseline had a child at some point after the baseline period. Table 1 displays descriptive statistics among participants who ever had a child during the study period by whether the participant was assigned to the treatment or control arm.[10] This table demonstrates that baseline characteristics are very similar along a number of dimensions, including demographic and economic characteristics. Among those who were parents at baseline, we also see a high degree of balance on characteristics related to parenting and parental investments, such as monthly spending on consumption related to children, use of non-parental childcare such as daycare, participation in school events, volunteer activities, and parent-teacher conferences. Table 2 shows similar balance at the level of the child for children in the household. On average, children living with treated participants are about 7 years old. Most—about 88 percent—are the biological child of the participant, although the sample also contains adopted, foster, and stepchildren, and some grandchildren. About 3 percent of household children are child siblings of participants. Throughout the manuscript, we will refer to participants with children in the household as "parents" for brevity, even though a small percent are grandparents or older siblings. Our results are essentially unchanged if we restrict the analysis to biological children of the participant, as we discuss in Section 6.6. In Table 2, we report baseline measures of stress for children ages 5 to 17 based on the PROMIS pediatric stress survey battery (Bevans et al., 2013) and age-specific measures of developmental and behavioral characteristics from the Strengths and Difficulties Questionnaire (SDQ) for those age 2 to 17 (Goodman, 1997), as well as measures of trouble at school.[11] Parental reports of their children's characteristics are similar across

[9] We identified a small number of participants in the baseline period who knew each other; in these cases, we clustered participants together in the randomization so that all participants within the cluster had the same treatment assignment.

[10] Similar statistics for the full sample, including those without children, are reported in Miller et al. (2025) and other companion ORUS papers. The full sample is highly balanced across treatment arms. In this paper, we use the full sample to examine outcomes related to childbearing, but focus on parents and children for other outcomes.

[11] In our baseline survey, parents provided SDQ and PROMIS measures for each of their children; however, these responses were not linked to children's identities in a way that can be followed forward. As a result, we assign each child the average value of their parents' response for their relevant age group. For example, if the participant had 2 children in the 2 to 4 year old age group at baseline, and thus complete the age 2 to 4 SDQ twice at baseline, we assign both children the

treatment arms.

3.3 Intervention

Randomization occurred in October of 2020. The higher ($1,000) transfer payments to the treatment group began in November 2020 and continued until October 2023. The control group received the $50 per month transfer over the same period, and the transfers were not conditional on participation in any of the research activities. Since the transfers were provided as an unconditional gift from a non-profit organization, they were not subject to income tax. Furthermore, the non-profit organizations worked with state benefit offices to ensure the transfer did not affect eligibility for public benefits whenever possible. This effort was facilitated by the passage of state-level legislation in the state of Illinois (SB 1735) that specifically excluded cash transfers made as part of research studies, such as the ORUS payments, from the calculation of eligibility for several state programs. Appendix Table A1 contains detailed information on how government benefits were affected by the transfer. In practice, we did not find that participants assigned to the treatment group reported receiving significantly fewer government benefits than those assigned to the control group (see Vivalt et al., 2025).

4 Data

4.1 Survey data

To learn about the impact of the transfer on parents and children, we collected a few types of data. First, we collected survey data at the parent or household level; for example, we asked parents about their stress, the home environment, and household spending on items for children. Second, we asked about outcomes for parents' specific children in a series of questions that would repeat for each reported child in the relevant age range. For outcomes related to social development and behavioral problems measured with the Strengths and Difficulties questionnaire (Goodman, 1997), we limited data collection to children present in the household at baseline and to a maximum of two children per household. In households with more than two children at baseline, we randomly selected two to reduce respondent burden for participants with large families because this survey battery is long. In contrast, all other child-level questions—such as those on stress or school enrollment—were asked for each child in the relevant age range living in the household at the time of the survey. We also estimate effects using only data on children present in the household at baseline as a robustness check.

average of these two reports. These baseline values are not available for children who were born or joined the household during the treatment period.

These survey data were collected via two modes.[12] First, participants received short monthly surveys administered online through Qualtrics, with a $10 incentive provided upon completion. Regular surveys allowed us to keep up to date with contact information and gave researchers multiple opportunities to get questions answered in each year. For example, if a respondent missed a question about parenting behavior in March, they may have another chance to provide that information on the June survey of the same year. For the purpose of analysis, we treat responses to the same questions provided within the same year as capturing similar information. We collapse outcomes to the respondent by survey year level for analysis, taking the average within respondent/year for continuous outcomes and the maximum for binary outcomes if multiple responses to the same questions were provided.

Second, we conducted two longer phone surveys with trained interviewers in the middle and near the end of the study—a "midline" and an "endline" survey. Respondents received a $50 incentive payment, which was escalated to $100 for nonrespondents during the final phase of the endline field period. To keep the length of the phone interviews reasonable, we asked participants to complete additional midline and endline questions in three follow-up online surveys. We offered a $15 incentive per completed survey to encourage participation in the follow-up surveys; this amount was also raised to $30 during the final phase of endline data collection. In our analysis of survey data by study year, we group the "midline" data with year 2 of the study and "endline" data with year 3.

Response rates for all types of surveys were high. Figure 1 shows response rates for parents in the control group (red) and treatment group (blue). We do not see a significant difference in the probability of responding across treatment arms to any Qualtrics surveys in years 1 and 2 and to our enumerated midline survey. However, we do detect small but statistically significant differences in the probability of responding to any Qualtrics survey in the third year of the study and to our enumerated endline survey, though these differences are small. Furthermore, we do not see evidence that responders had different characteristics across treatment arms as measured at baseline, further enhancing our confidence in the survey responses. The differences in baseline characteristics across respondents and non-respondents by treatment arm are reported in Appendix Tables A2 and A3.

4.2 Administrative data

To measure outcomes related to school attendance and educational performance, we obtained statewide administrative records from public schools for school-aged children from the Illinois State Board of

[12] Additional details on the surveys are documented in Miller et al. (2025), Vivalt et al. (2025), Bartik et al. (2024), and Broockman et al. (2024).

Education (ISBE) and the Texas Education Agency (TEA). These data cover the 2015/2016 through 2023/2024 school years in Illinois, and the 2016/2017 through 2022/2023 school years in Texas. We sent first and last name, date of birth, address, and gender to ISBE for the purpose of matching. To TEA, we provided full name, date of birth, and gender. We included all children whose parent consented to data linkages in the matching process, including those who joined the household after the baseline. We also included those who were too young to be in school at the time, in hopes of conducting long-term follow up in the future. Of the 3,630 children whose parents consented to share information, 2,523 (or 69.5%) were matched to school records for at least one year of school, with match rates higher in Texas (74.1%) than in Illinois (63.8%). If we restrict attention to children who were at least 2 years old at enrollment (i.e., those who would plausibly be old enough for kindergarten attendance by the end of the intervention), match rates are higher, with 88.7% matched in Texas and 72.5% matched in Illinois. Match rates were similar across treatment arms: 78.9% of the treatment group children and 82.5% of the control group children who were at least 2 years old at baseline matched to a school database in at least one year. Unmatched children come from similar households as matched children and appear similar on many characteristics, but were more likely to be older on average (see Appendix Table A4). We also show in Appendix Table A5 that, within the group of children matched to at least one school year's record, baseline measures of our outcomes were highly balanced across treatment arms.

These data include information at the pupil by school year level. Across both states, we have variables related to enrollment in public school (whether the student was enrolled, if the child repeated a grade, and whether the child was old for their grade), percentage of days in attendance, participation in a gifted and talented program, statewide standardized test outcomes, and information on whether the student changed schools or school districts. In Texas, we additionally observe outcomes related to disciplinary measures (such as suspensions and expulsions, and the reason for these disciplinary measures), while in Illinois, we have information on students' grades. Although the data are held in separate computing environments, we combine treatment effect estimates across states using a fixed-effect meta-analysis model (see Deeks et al., 2001). Due to statistical power considerations, we use the outcomes that we can observe in both states (and combine into a single estimate) as our primary outcomes. We separately report state-specific outcomes as exploratory. Although both states administer state-wide standardized tests, the content differs by state and by grade level. Despite these differences, we combine analysis on indicators that the student met or exceeded expectations on these

standardized tests in the areas of math and English/language arts in order to pool information across states. Appendix Section C has further details on the variables.

In addition to examining student performance measures, we consider characteristics of the school or the school district itself. School quality or characteristics might change if the transfer enables parents to move to different neighborhoods or to enroll their children in different schools. Specifically, we examine the following school characteristics: average class size; the percentages of students who are economically disadvantaged, English learners, and chronically absent; average attendance rate; graduation rate (for high schools); the percentage of students proficient in English/language arts and math based on statewide standardized tests; and year-to-year growth in the math and English percentile scores from statewide standardized tests. We also construct value-added estimates for English/language arts and math test scores by regressing the annual change in test score percentile on school and year fixed effects, with controls for student characteristics (e.g., student demographic characteristics, percent of students economically disadvantaged, etc.), and using the coefficient on the school fixed effect as the measure of the value added. This calculation is done separately for primary and secondary schools and is also conducted separately by state.[13] Finally, we also consider two quality measures at the school district level: spending per pupil and average teacher salaries.

Because we only observe these outcomes for children attending public schools, our results may suffer from selection bias if the cash transfer induced parents to move their children to private schools or to homeschool them, or to move out of state. We examine these school changes directly as an outcome and do not find much evidence that students whose parents received the larger cash transfer were disproportionately likely to not be enrolled in public schools in our sample. We also estimate difference-in-differences models as an alternative specification to address such selection. Because we have several years of pre-treatment data in our administrative records, we are able to estimate "event study" versions of the model that look at year to year changes in school outcomes for several years before and after the intervention begins. Such an approach is not possible for our survey outcomes as we do not observe multiple pre-treatment periods.

We also linked older children in our sample to data from the National Student Clearinghouse (NSC) to measure post-secondary educational attainment. We examined this outcome for the 621 children of participants who were at least 16 years old at any point in the study. The NSC holds

[13] We did not have 2018 school-level data in Illinois and 2021 school-level data in Texas. For these years, we used value-added estimates from adjacent years. Also, due to a very high rate of missingness in Illinois secondary schools, we generated value-added estimates using percent proficient rather than test score change for these schools.

administrative records for approximately 97% of post-secondary institutions in the U.S..[14] These data include information on degree attainment and enrollment in post-secondary schools. We use these data to examine whether children of participants completed any college degree and the number of months they were enrolled in such a program. For the latter measure, we convert part-time enrollment into its full-time equivalent (e.g., we code 1 month of half-time enrollment as 0.5 months of full-time enrollment).

Finally, we use administrative data on births recorded in the Census Household Composition Key (CHCK) to measure the impact of the transfer on childbearing. Census creates this file by probablistically linking children to their parents based on names listed in the Social Security Administration's Numident file and address information from several other government administrative records held by the U.S. Census Bureau. These data track total births in the U.S. very closely (Genadek et al., 2021), although the probabilistic nature of the match between parents and children may result in some undercounting (Bernard et al., 2024).

4.3 External data

To measure neighborhood characteristics and quality, we link participants' addresses to a number of external datasets. Appendix Section D provides additional details on how each measure was produced. We measure tract-level characteristics, such as the share of households with children, using the 5-year American Community Survey (ACS) extracts provided by IPUMS (Ruggles et al., 2025). To measure the presence of amenities—like daycares and libraries—we used SafeGraph Places of Interest (POI) data. We used tract-level information on economic mobility and incarceration rate for those growing up in low-income households downloaded from the Opportunity Insights data portal (`opportunityinsights.org/data/`), as well as indices capturing area deprivation and child opportunity at the block-group and tract level (respectively) from Kind and Buckingham (2018) and University of Wisconsin School of Medicine and Public Health (2024) and from Noekle et al. (2024). Finally, we measured air pollution with modeled predictions of fine particulate matter at the Census tract level using data from the Centers for Disease Control and Prevention's National Environmental Public Health Tracking Network, and exposure to other environmental toxins using the EPA's Risk-Screening Environmental Indicators (RSEI).

[14] See https://nscresearchcenter.org/workingwithourdata/.

5 Empirical Approach

We estimate the impact of the treatment on each outcome using the following regression:

$$Y_i = \beta_0 + \beta_1 Treat_i + \beta_2 X_i + \epsilon_i. \tag{1}$$

Here, i indexes either the participant or their child, depending on the outcome. To improve precision, we include baseline characteristics X_i. We select which characteristics to include using the LASSO (Bloniarz et al., 2016). We estimate robust standard errors clustered at the level of treatment assignment. Most adults are in a cluster of one, except in a small number of cases where we determined participants knew each other in the pre-treatment period. Children of the same participant (or in the same participant's household) are clustered together.

The outcomes Y_i may be observed at multiple time periods, and we use these repeated observations to estimate time period specific effects (i.e., in the first, second, and third year of the study). We also pool time periods together to estimate a single "effect" of the intervention. Consistent with our pre-analysis plan, we place greater weight on observations toward the end of the study period.[15] If we have no measures of an item within a particular time period (e.g., year 2, at midline, etc.) for an individual but we do have measures of that item at other time periods, we average over the non-missing time periods and redistribute weights accordingly.[16]

An income transfer can affect a large number of outcomes, and in some cases it may be interesting to know whether we can reject the null hypothesis that certain groups of outcomes collectively were affected by the transfer. To facilitate this type of analysis, we group items at two levels. First, we group closely related items into groups that we refer to as "components."[17] For example, we group items measuring the frequency of reading to your child, helping with homework, eating dinner together, attending parent-teacher conferences or school events, and similar activities into a component called "Parental Interaction." Second, we aggregate related components into broader families. For example, we group the component "Parental Interaction" with components on "Parental Quality," "Expenditures on Children," "Parental Satisfaction," and "Parental Stress and Distress" into a fam-

[15] For estimates collected in all study years, we place 50% of the weight on surveys conducting in the final year, 30% of the weight on surveys conducted in the second year, and 20% of the weight on surveys conducted in the first year.

[16] For example, for those outcomes only collected in the enumerated surveys (most child-level outcomes), we place 62.5% of the weight on year 3 (endline) and 37.5% of the weight on year 2 (midline). This follows our standard approach for cases in which an individual is missing data for an entire year, whereby we weight the non-missing years proportionally following the 20%/30%/50% rule.

[17] The non-aggregated outcomes we refer to as "items" for clarity.

ily called "Parental Behaviors and Investments" which measures whether, collectively, these different parenting-related outcomes were affected. To construct these components and families, we start with the item-level regression estimates $\hat{\beta}_1$. Then, we standardize these estimates by dividing by the control group standard deviation and aggregate them using seemingly unrelated regression (SUR) into components and, subsequently, families, by orienting estimates so that higher values equate to better outcomes and then averaging the re-oriented, standardized effects.

We account for the fact that we are conducting many statistical tests by using a false discovery rate (FDR) adjustment. We use Benjamini and Hochberg (1995)'s false discovery rate adjustment to compute q-values within families of outcomes. Furthermore, we follow Guess et al. (2023) by placing the family-, component-, and item-level estimates into tiers for the purpose of this adjustment, corresponding with our prioritization of the estimates. Placing tests into tiers allows us to conduct exploratory or ad hoc analyses without compromising the statistical power of our primary outcomes of interest. These tiers were pre-specified. We consider family-level estimates for each age group (and all ages pooled) in the top tier and pool them across the paper in a single tier. We place component-level estimates overall and for each age group in the next tier and pool these tests with the other components within the family. Then we place all of our primary outcomes in the next tier; the q-values for these items are computed by pooling the family-level estimates and all component-level and other outcome estimates within the family. The last tier is comprised of estimates we consider to be more exploratory in nature: estimates by each time period, other subgroup analyses, and outcomes pre-specified as secondary. As a result of this tiering, these exploratory estimates must be highly statistically significant in order for the significance to survive the multiple comparisons adjustment. We do not conduct an FDR adjustment for robustness checks.

6 Results

We first summarize our results in Figure 2, which presents family-level treatment effects with 95% confidence intervals for the eight broad families of outcomes we consider. We find small but statistically significant improvements related to outcomes in the parenting and investments family, as well as small increases in the mobility and neighborhood environment family. We see a worsening in outcomes measured at the child-level related to parent-reported measures of child stress and social development. There are no detectable changes in the home environment, non-parental care, or education-related families. Figure 3 shows the same family-level estimates separately by child age

groups.[18] Improvements in parenting and investments are similar across age groups, and the worsening measures of stress appear largest for those age 5-10. In the next two sections, we discuss the individual outcome measures that comprise these family-level estimates in more detail.

6.1 Parent-level outcomes

We begin with the set of outcomes measured at the parent or household level. Because the transfer may affect parents differently depending on their children's ages, we show results both pooled across all parents and separately by whether a parent has a child in the household under age 5, age 5-10, or age 11-17 at the time of the survey.[19] Note that the same parent might contribute to multiple subgroups; for example, if they have one child under age 5 and a second child age 5-10, they would contribute to the estimates of both groups of parents. Furthermore, some types of investments are only relevant for certain ages—for example, parents might attend a parent-teacher conference, but such an action would only be relevant for children old enough to attend school. In such cases, parents are only surveyed on these items if they have a child in the relevant age group. At the bottom of the table we report the average number of observations associated with the estimates in each column, but note that this N may vary slightly from outcome to outcome for this reason, or due to missingness which may vary slightly from question to question.

Tables 3 and 4 show outcomes related to parenting behavior and investments in children. Taken together, we find that the transfer had a statistically significant positive effect on these outcomes of 0.036 standard deviations. While significant with traditional inference, the family-level estimate narrowly misses significance once we adjust for the fact that we conduct multiple tests ($q = 0.11$ after FDR adjustment). Next, we examine measures of parenting quality using the Alabama Parenting Questionnaire, a survey battery measuring parenting behavior across five domains: use of corporal punishment, inconsistent discipline, parental involvement, poor monitoring/supervision, and positive parenting (Essau et al., 2006). Here, we see a statistically significant improvement in overall parenting quality associated with the cash transfer of about 0.05 standard deviations that remains significant ($q = .051$) after FDR adjustment. Examining the subcomponents of this index, we see that all subcomponents improve, but that the most significant decrease in the subcomponent related to "poor monitoring or supervision." Across age groups, the improvement in parenting appears to be

[18] We exclude effects for non-parental care and higher education from this figure, as they are only measured for one of the three age groups.

[19] The column labeled "all ages" includes all parents who have a child, regardless of whether the child lives in the household. The subsequent columns include only those with children of the corresponding ages living in the household.

especially large among parents of older children and teenagers.

Next, we examine how the transfer affected expenditures on children.[20] To construct this measure, we ask parents about monthly spending on childcare, baby or personal care items (such as diapers, baby food, medicine, etc.), and children's clothing, education, extracurricular activities, and entertainment. We find that the transfer significantly increased spending on children by about $31 per month, an approximately 11% increase relative to the control group. In percent terms, this increase in spending was fairly stable across the age groups.[21] Breaking this expenditures category down further reveals significant increases in spending on children's clothing and baby items, with positive treatment effects for all subcategories (see Appendix Table A6).

We did not ask about food purchased specifically for children, since children often eat the same groceries and meals as their parents and it may be difficult for respondents to identify how much of the grocery expenses were for children versus for themselves or other adults in the household. We therefore estimate the effect of the transfer on food and beverage consumption at the household level—both overall and separately for food consumed at home and away from home. Notably, qualitative analysis based on semi-structured interviews with participants found that they often described meals out as a way to connect with their children, since they were able to talk and eat together without needing to cook and without the distractions of home (Dotson et al., 2025). Similarly, we did not ask participants to separately report health care expenditures for their children versus for themselves or others in the household. However, it is likely that some of the medical care expenses reported by participants are related to health care received by their children. Additionally, in qualitative interviews, some parents described how increased spending on their own medical care—particularly for mental health, chronic conditions, and prescriptions—enhanced their parenting capacity and ability to be present with their children (Dotson et al., 2025). We see increases in both food consumption and medical care consumption for parents who received the cash transfer (see Appendix Table A6).

Next, Table 3 reports the effects of the transfer on parents' engagement in different types of activities with their children (such as reading to them, eating dinner together, etc.). We do not find much

[20] These results are also found in Bartik et al. (2024) and Vivalt et al. (2025), along with more details on the effect of the transfer on different types of consumption. However, the analyses in this paper restrict attention to the set of participants who were parents. Estimates are correspondingly somewhat larger, as one might expect parents to allocate more of the transfers to children than non-parents. In this section, we do not distinguish between parents of children who live inside the household and parents of children who live outside the household.

[21] These increases in child-related spending are notably smaller than those observed in the Baby's First Years experiment. As reported in Gennetian et al. (2024), treated participants in Baby's First Years reported spending $67.8 more per month on the focal child (compared to a relative transfer of $313). These differences may in part be due to differences in the framing of the transfer—in Baby's First Years the money was labeled as for your baby, whereas the ORUS intervention provided a neutral framing—and the fact that our expenditure questions were worded differently.

change in parents' reports of engaging in these interactions, although the transfer is associated with a suggestive increase in reports of attending a meeting at the child's school in the past year, which increases by about 9.6%. Using time-use measures derived from our surveys, we find no effect of the transfer on time spent on childcare or family activities among parents of children of all ages. Similar null results are reported in Vivalt et al. (2025) for all participants (i.e., not just parents). There is some evidence of an increase in time spent with family for those with children in the 5- to 10-year-old age group in the household that is significant with traditional, but not FDR adjusted, inference. However, this may not necessarily reflect time spent parenting. It is also important to note that time spent on parenting is notoriously difficult to accurately collect, as parenting is often performed simultaneously with other activities (e.g., chatting with a child while making dinner) and some important parts of parenting may be passive (e.g., providing supervision and being available if needed even while not directly interacting with the child). Monna and Gauthier (2008) provides an overview of these measurement issues in the context of time diaries; see also the discussion in LaBriola and Schneider (2021). The null effects we document on time spent on childcare could be due to these measurement challenges, and/or could reflect parents not viewing time spent with older children as "child care" per se.

We continue our reporting of results on this family in Table 4. The next result shows the impact of the transfer on parental satisfaction. We do not find any change in these outcomes, and can rule out improvements in these variables of about 0.05 standard deviations. Similarly, we do not find that parents report being less stressed or experiencing less mental distress.

Table 5 considers whether the transfer affected aspects of the child's home environment. In models that include parents of children of all ages, we find no effect of the transfer on children's food insecurity, as measured by the USDA's 0 to 6 food insecurity scale for children, where higher scores indicate greater insecurity. If anything, food insecurity appears to have worsened for children of younger ages, driven by increases in the last year of the program (see Section 6.5), although these effects are not statistically significant once we account for the fact that we are conducting multiple hypothesis tests. Next, we examine the impact of the transfer on a shortened version of the Confusion, Hubbub, and Order Scale (CHAOS scale, Matheny et al. (1995)), which includes twelve statements related to order or chaos in the home environment—for example, "No matter how hard we try, we always seem to be running late," "At home we can talk to each other without being interrupted," and "Our home is a good place to relax" and asks participants to rate the statement from "very much like my home"

to "not at all like my home." Responses are first re-oriented so that higher values of the scale indicate greater chaos or disorder in the home environment and then aggregated into a single measure that ranges from 0 to 36. We find no impact of the transfer on the CHAOS scale and can rule out improvements larger than about 2.5% of the control group mean. Lastly, we construct a measure for each survey year indicating whether parents were ever unhoused (living in a shelter or in irregular housing such as a car or abandoned building) during that period. Although we did not ask specifically about whether this period of homelessness affected children in the household, parental spells of homelessness are likely a sign of housing instability for their children as well. We do not find any impact of the transfer on this outcome and can rule out moderately sized reductions of about 17% or greater. We also considered secondary outcomes related to being unhoused, such as the number of months unhoused, lived with others, or stayed in a shelter at least one night. These secondary measures also did not change significantly as a result of the transfer. See Appendix Table A7 for details.

In previous work, Bartik et al. (2024) documented that treated ORUS participants were significantly more likely to move than control group participants. These moves may have exposed participants' children to more favorable neighborhood environments. In Table 6, we show the impact of the transfer on the probability of a parent moving (either to a different unit or to a different neighborhood) and on multiple measures of neighborhood quality.[22] Although we see that the transfer generated significantly more moves in the treatment group relative to the control group, we do not find evidence that neighborhood quality changed significantly across the domains of family friendliness, exposure to pollution, potential for economic mobility, and quality of nearby public schools. However, we do find some suggestive evidence that parents in the treatment group may have moved to areas with more child-focused amenities (daycares and schools) compared to those in the control group. This appeared to be particularly true for parents of younger children (under age 5). However, these effects are only statistically significant using traditional inference and do not remain significant after adjusting inference to control the FDR.

Finally, we consider parents' reported use of non-parental care, such as daycares or babysitters, for those with children under the age of five. As reported in Table 7, we do not find any evidence that the treatment increased the use of childcare or changed the quality or stability of care used in the full sample. However, as we discuss in Section 6.4, estimates from the full sample mask substantial heterogeneity based on participants' pre-treatment household income. Appendix Table A8 reports effects

[22] Parts of this table also appear in Bartik et al. (2024).

of the transfer on secondary items capturing whether a participant moved to a childcare provider that was better in some respect (e.g., quality, cost, location) and if the participant missed work due to an inability to find childcare. We do not find any effects of the treatment on these secondary outcomes.

6.2 Child-level outcomes

We next examine outcomes measured at the child level. First, we assess how the transfer affected parents' reports of children's stress and social development in Table 8. We used the Strengths and Difficulties questionnaire, which asks a different set of questions about children age 2 to 4 and age 5 to 17, to measure parents' reports of the social and emotional development of their children. Parents in the treatment group reported that their children had significantly more difficulties, especially around hyperactivity, compared to parental reports of children's strengths and difficulties in the control group. These effects were largest for children ages 5 to 10; for children in this age range, the treatment led to significantly worse reports in the overall index as well as the subscales related to peer problems and hyperactivity. Treated participants were also significantly more likely to report that their child scored higher on the PROMIS (Patient Reported Outcome Measurement Information System) Pediatric Psychological and Physical Stress score, which is composed of questions asking participants about how often their child was overwhelmed, had problems that were piling up, was stressed, and was unable to manage things.

Table 9 presents results for parents' reports of children's educational outcomes. We do not find statistically significant effects on any of these measures, and our point estimates are small. Tables 10 and 11 show measures of educational outcomes as captured in administrative records. Public school enrollment, attendance, rates of repeating a grade, participation in gifted and talented programs, and most standardized test score measures do not appear to be affected by the transfer.[23] We find a decrease in the probability that a treated student meets expectations in statewide standardized tests for math, but the estimate is not statistically significant after adjusting our inference to control the FDR. We do not find similar changes for the probability that a student earns a mastery grade in math, nor on either of our English/language arts standardized test measures. Additional results reported in Table 11 do not show any significant changes across a range of school quality measures. Taken together, these results suggest that the transfer had minimal effects on school-related outcomes.

[23] We also examined alternative measures of attendance—days absent from school and an indicator that the student was "chronically absent" (more than 10% of school days absent in the year). And, we examined whether the transfer changed the probability that a student was enrolled in public school, home schooled, dropped out, or (among those in their senior year of high school) graduated. We do not find significant effects on any of these secondary items; see Appendix Table A9.

We also draw on a set of K-12 public education administrative measures that are available exclusively in either the Texas data or the Illinois data. We consider these items to be secondary or exploratory since they are only available for half of the sample. Appendix Table A10 shows the impact of the transfer on disciplinary measures such as suspensions and expulsions, which are only available in the Texas records. We find no effect of the transfer on disciplinary measures overall, nor on disciplinary action taken for a specific reasons (i.e., student committed a felony, student had a drug, alcohol, or tobacco violation, student committed physical violence). Appendix Table A11 shows how the transfer affected students' grades, which are only available in the Illinois data. We translate a variety of grading systems (e.g., letter grades, grades of meets or exceeds expectations, etc.) into an indicator that the student passed, met expectations, or mastered the material in each course. Different grading scales are used at different grade levels but most are able to be mapped into these indicators (see Appendix Table A39 for more details). We then weight the grades by the credit hours of each course to arrive at a school year by student level measure of these three indicators. We also translate the grading system into a number grade ranging from 62.5 (fail) to 99 (A+) and again collapse this imputed score to the student by school year level, weighting by credit hours. We find that children whose parents were treated had worse grades both in all courses and specifically in the "core" courses of math, science, and English/language arts. However, these estimated treatment effects do not remain statistically significant after adjusting our inference to account for the fact that we are conducting many tests. These negative results may be related to challenges posed by the COVID-19 pandemic, as well as the substantial variation in teaching and attendance modes across schools and students in Illinois during the 2020/2021 school year (COVID-19 School Data Hub, 2023). We find that treated students in secondary school attended schools that had about 10 percentage points more months with hybrid (vs. fully virtual or fully in person) instruction as the dominant school-level instruction mode during the 2020/2021 school year, though this does not survive the FDR adjustment.[24] Additional research is needed to confirm these results and to understand whether they reflect a COVID-specific response or an impact of cash transfers more generally.

Finally, Table 12 shows the higher education effects of the transfer on participants' children who

[24]The potential difference in instruction mode for these students raises the possibility that grades, attendance, or other outcomes may be recorded differentially across secondary schools attended by the treated and control children, or that school policies around these outcomes could differ. We do not observe detectable differences in a variety of school characteristics as measured in administrative records and parents' reports of school quality. However, if hybrid instruction was indeed more likely in the secondary schools of treated participants' children, it is possible these schools vary in other unobservable ways. Finally, it is important to note that this measures the dominant mode of instruction at the school level. These data show there was significant heterogeneity month-to-month in the percent of children enrolled in each learning mode within a given school, and we cannot observe this individual variation within our sample.

were at least 16 at any point during the transfer period. We do not find that the transfer had statistically significant effects on the probability a child obtained any post-secondary degree or on the months of full time equivalent enrollment in a post-secondary degree program over the period of the program. However, our standard errors are large enough that it is difficult to rule out large effects in either direction.

6.3 Childbearing, Pregnancy Intent, and Contraception

Finally, we examine whether the transfer affected childbearing or the desire to have another child using the full sample of participants (including non-parents). Table 13 shows estimated treatment effects for the impact of the transfer on outcomes related to childbearing, pregnancy, and contraception. We do not produce family-level estimates for this group of outcomes since there is not a natural "positive" or "negative" orientation for most of the items (e.g., desire for a pregnancy is not naturally "good" or "bad"). And, because the relevant subsample for questions varies substantially across outcomes, we report the number of observations for every outcome separately in the leftmost column. Outcomes pre-specified as secondary or exploratory are marked with ^{s}. We estimate these effects on the full sample of program participants, including non-parents. We find no impact of the transfer on the probability the participant had any new child (born or adopted) or on the number of children born to or adopted by participants during the transfer period, as reported by participants in surveys. In the second row, we show that administrative records of births from the Census CHCK also indicate no statistically significant impact of the transfer on births. Our point estimates are very small and fairly precise, allowing us to rule out increases of more than 0.03 additional children due to the transfer based on survey measures or 0.02 additional children based on administrative measures. We also do not find any evidence that the transfer affected participants' intention or desire to become pregnant in the next year on a 1 to 10 point scale, with higher values indicating stronger desire to become pregnant (for male participants, we asked about desire to become pregnant "with a partner").[25] We find no effect of the transfer on the the efficacy of contraception used among heterosexual participants who were sexually active, reported not desiring a pregnancy, and reported using some form of contraception other than abstinence. However, it is notable that after making these sample exclusions, the remaining sample is quite small. We also do not find that the transfer affected the overall use of contraception (including abstaining from sex that could result in a pregnancy) among participants

[25] We find similarly null effects on alternative versions of this scale, such as the probability of any reported desire to become pregnant or the probability of reporting moderate or strong pregnancy intention (i.e., above 5), see Appendix Table A12.

who did not desire a pregnancy at the extensive margin. For this last question, we coded participants who were not sexually active as not using contraception, but included them in the analysis.[26] However, we do find some evidence that the transfer modestly increased the probability of having a positive pregnancy test in the last 12 months by about 1.9 percentage points, even though it did not change participants' desire to become pregnant. Among those who reported a positive pregnancy test or whose partner had a positive pregnancy test over this time period, participants who received the cash transfer were more likely to report obtaining (or planning to obtain, if pregnant at the time of the survey) an abortion, but this effect is not statistically significant.

6.4 Heterogeneity

We examine heterogeneity in our estimated effects across pre-specified subgroups of baseline income, child age, and child gender. We also added a subgroup analysis of parents who were single vs. married or unmarried but living with a partner at baseline, and, for outcomes related to birth and childbearing, analysis by state subgroup. See Appendix Section B for more information on these changes from the pre-analysis plan.

Figure 4 presents the estimates for each broad family (in standard deviations) for outcomes measured at the participant or parent level, both in the full sample of parents and in each subgroup we considered. Results for each component and item within the family are reported in Appendix Tables A13-A29. Given the smaller sample available for outcomes related to post-secondary education, we do not conduct subgroup analysis for these outcomes.

Figure 4 panel (a) shows heterogeneity in effects related to parenting and investments. Much of the increase in parental investments we observe in the overall sample appears to be driven by those with baseline incomes under 100 percent of the FPL, with negligible effects observed among those whose incomes were initially higher. Appendix Table A13 shows that this higher family-level estimate is driven by much larger improvements in parenting quality for lower-income parents (0.091 SD improvement compared to -0.005 SD for higher-income parents), with particularly large decreases in reports of corporal punishment and inconsistent discipline. The effects of the transfer on child-related expenditures are also more than three times as large among the lowest-income parents.

We also see heterogeneity when comparing single parents to those who had a partner at baseline

[26] The treatment did not appear to change the fraction of participants who were sexually active, see Appendix Table A12. We defined contraception efficacy as one minus the CDC's published failure rate with typical use of each method. We obtained these rates from https://www.cdc.gov/contraception/about/index.html, accessed 12/15/2024. We also find no evidence of changes in contraceptive efficacy if we do not condition on pregnancy intent, see Appendix Table A12.

(Appendix Table A14). The transfer has the largest positive effect on parenting quality among single parents. However, single parents also report that the transfer significantly *increased* their stress and mental distress. This appears to be driven by higher stress in the last year of the transfer, possibly reflecting the fact that the transfer will soon end. These quantitative patterns are supported by a separate qualitative analysis based on semi-structured interviews. In these interviews, researchers found that many single parents used the transfer to reduce or change employment so that they could be more present for their children, which may have in turn resulted in greater increases in stress around the time the transfer ended (Dotson et al., 2025). On the other hand, parents who had partners at baseline experienced smaller improvements in parenting quality but also did not experience worsening stress and mental distress. Consequently, the overall family index shows more of an improvement for partnered parents.

The next panel of Figure 4 hows how the transfer affected the use of non-parental care for parents of children under age 5, such as day care or babysitters. We see that the null effect overall masks a positive effect among those with low incomes at baseline and a negative but not significant effect among higher income parents. We do not see an effect when splitting by whether or not the parent was single at baseline. We see worse effects of the transfer on the home environment among single parents, but no other heterogeneity for these outcomes (panel c). We see similar effects across groups when examining changes in the neighborhood environment (panel d), which are mostly driven by the propensity to move and not measures of neighborhood quality. Constituents measures of these indices are reported in Appendix Tables A15-A18.

Figure 5 shows heterogeneity for child-level outcomes at the family level. Here, in addition to considering the family's baseline income and the parent's single or partnered status at baseline, we also consider the child's gender, as was pre-specified. We see that the effect of the transfer on reports of their children's stress and social development is somewhat more negative among female versus male children. Single parents also tend to report worse stress and development outcomes for their children as a result of the transfer as compared to partnered parents, which is also consistent with observations in the qualitative analysis that single parents were more emotionally present and engaged with their children (Dotson et al., 2025).

The effects of the transfer on educational outcomes as measured in survey data are null across groups, although the point estimate indicates that the transfer may have somewhat improved these measures for households that were the lowest income at baseline and worsened them for higher in-

come households. Analyses of subgroups by baseline income and child sex also shows negative effects of the transfer for female children in survey reported, but not administrative, measures of educational outcomes (Appendix Tables A27 and A28).[27] There are a small number of exceptions: children in the higher-income households experience a negative effect of the transfer on the probability of meeting expectations on math standardized tests, with an insignificant positive effect for children from lower-income households; children in lower-income households are less likely to be enrolled in public K-12 schools during our sample years, and they appear to attend schools with higher average attendance rates; and female children appear to attend somewhat higher quality schools as the result of the transfer. However, none of these effects remain statistically significant after adjusting inference to control the FDR.

Finally, we show heterogeneity in the effects of the transfer on childbearing, pregnancy, and family planning outcomes in Appendix Tables A29 and A30. We do not include these in the figures because we do not produce family-level estimates for this group of outcomes since, as mentioned previously, there is not a natural "positive" or "negative" orientation of most items. Appendix Table A29 shows how these outcomes varied across participants in Texas and Illinois. The effects of the treatment on childbearing and fertility appear slightly positive in Texas and slightly negative in Illinois, although effects are not statistically significant in either state. Treated participants in Texas appear more likely to receive a positive pregnancy test as a result of the treatment, although this effect does not remain significant after adjusting to control the FDR. Effects on these outcomes do not vary much by income (see Appendix Table A30. Note that Census disclosure rules prevent us from analyzing subgroups using the administrative births data.

6.5 Treatment Effects over Time

In addition to examining the pooled effect of the transfer on outcomes, we estimate its effects separately for each of the three years of the program. To facilitate comparison across outcomes and over time, we standardize treatment effects by dividing each estimate by the control group standard deviation. Not every question was asked in all three years, so some estimates are missing for particular years. We present these results pooled across all ages for which the outcomes are available.

Appendix Figure A2 presents results for outcomes related to parental behavior and investments.[28]

[27] We were not able to examine the difference in the effects across parents who were single versus partnered at baseline in the administrative records because the Texas system only allowed a small number of baseline covariates to be brought in to the research environment, and we did not include the single vs partnered variable used throughout the paper in that list.

[28] We do not present results by year for outcomes presented in Table 13 related to childbearing and pregnancy as we only

Outcomes related to parenting quality appear better in year 3 than year 2 (these questions were not asked in year 1), suggesting there may have been an increasing benefit of the cash over time. The effect of the transfer on expenditures on children are similar across the three years, as are the effects on feelings of parental satisfaction. Due to the large number of outcomes, we only show the year by year effects on the aggregated index capturing the quantity of parental interactions; the effect on this component index is null in all three years. Finally, we find significant improvements in stress among parents in the first year of the transfer that dissipate by year 2. By year 3, mental health outcomes are worse among treated parents than non-treated parents, potentially reflecting increased anxiety about the program's end, as many year 3 observations were collected in the final months of the study. These results are very similar to those found in the full sample (see Miller et al. (2025)).

Next, we examine results related to the home environment in Appendix Figure A3. We find no significant effect of the transfer on the home environment overall in any year, nor on the CHAOS score or the probability a child is unhoused. It does appear that child food insecurity is significantly worse for transfer recipients in the third year of the transfer and close to zero in the first two years. Among adult participants, food security improves significantly as a result of the transfer in the first year but worsens over time (estimates reported in Miller et al., 2025). However, this third year effect on child food security does not remain significant after adjusting inference to control the FDR. Appendix Figure A4 shows similar results for the neighborhood environment. The propensity to move appears to grow from year 1 to year 3, with the change in child-related amenities following the same pattern. Other measures of neighborhood quality do not appear to be affected by the transfer, nor does the use or quality of non-parental care, as reported in Appendix Figure A5.

Appendix Figure A6 shows the effects of the transfer on stress and social development in years 2 and 3 (these measures were not collected in year 1). These effects appear similar across the two years, as do effects of the transfer on survey-reported school outcomes (Appendix Figure A7) and school outcomes from administrative records (Appendix Figure A8 and A9). Administrative measures of school outcomes available in only Texas (Appendix Figure A10) or Illinois (Appendix Figure A11) similarly do not show any clear pattern of time dynamics.

asked about these outcomes in the final year of the study, with the exception of pregnancy intention, which was asked both in year 2 and year 3. The treatment effects on intention to become pregnant in years 2 and 3 are both close to zero.

6.6 Robustness

We conduct a number of robustness checks to assess the sensitivity of our results to alternative sample definitions and methods for correcting for differential attrition for outcomes derived from our surveys. These results are reported in Appendix Tables A31-A37. We only report these results for family-level and component estimates.

First, we assess whether our results are robust to changes in model specification or sample. First, we re-estimate equation (1) but exclude all covariates X_i. The results from this exercise are reported in the second column, with baseline results reported in the first column to facilitate comparison. The point estimates from the models with versus without selected baseline controls are very similar, reflecting the high level of balance across treatment arms prior to randomization. However, the standard errors in the model without controls tend to be somewhat larger. Second, we restrict our sample to only the 89% of children who are the biological child of the participant; i.e., excluding foster and stepchildren, adopted children, grandchildren, and child siblings who reside with the participant. We again see very similar results as those generated using the full sample of household children. In the next column, we restrict the sample to only children who were present in the household at baseline, to account for the fact that the treatment may have affected how long children reside with the treated participant and whether new children enter the household.[29] Estimating effects on this sample, we again find very similar results as those reported in our full sample.

Next, we conduct tests related to differential response to surveys across treatment arms. Overall, our response rates were high and the differential response rate across treatment arms was quite modest, although not zero. Respondents were also highly balanced across treatment arms (see Appendix Tables A2 and A3), suggesting there was not systematic differential non-response. Nevertheless, it is possible that differential response to the surveys across treatment arms may be related to unobserved characteristics, which could in turn generate bias. We conduct two analyses to assess the risk of such bias. First, we estimate a "difference in differences" model that compares changes within the same participant before and after the treatment period for participants randomly assigned to the treatment or control groups. This model requires that participants in the two groups be on parallel trends in order to recover unbiased treatment effects of the transfer, but does not require that the treated and untreated group are observably and unobservably similar (i.e., have the same potential outcomes). We

[29] Outcomes related to stress and social development are only asked for children who were present at baseline, so we do not provide this check for that family.

report the results of this difference in differences model in Appendix Tables A37-A36 for outcomes for which we have baseline measures.[30] Our results are very similar using this model for outcomes for which we have baseline data. In addition, we use Lee (2009) bounds to bound the possible treatment effect under the assumption that the treatment affects response rates monotonically (i.e., that either the "best off" or "worst off" of the sample is moved into non-response by treatment). These bounds tend to be fairly wide for most outcomes, although we find qualitatively similar results across the upper or lower Lee bounds for parenting quality, child-related spending, and moving neighborhoods, three outcomes for which we find significant effects, and for measures of the home environment and childbearing, two outcomes for which we consistently find null effects.

6.7 Event Study Estimates for Administrative K-12 Outcomes

The longer pre-period available in our administrative records of K-12 outcomes allows us to conduct an "event study" style analysis of these outcomes that is not possible with the survey data. These analyses allow us to see how outcomes evolved before and after the intervention among kids observed in the treatment and control groups both before and after the transfer program. Although we do not find evidence that the transfer significantly affected selection into our administrative records, this approach should account for any time invariant selection that may be undetected (e.g., within our confidence intervals). And, since we have data for one state (Illinois) for the 2023/2024 school year—mostly after the cash transfers ended—we use this setup to examine whether any persistent effects emerged or dissipated after the end of the program.

To conduct this analysis, we construct a panel of children observed in both the pre- and post-intervention period. Then, we estimate a regression model that includes year, individual, and year $\times$ treatment fixed effects. We also estimate the associated difference-in-differences model that replaces the time indicators with a single indicator that the observation is in the post-treatment period. We plot the coefficients on the year $\times$ treatment indicators in Appendix Figure A12, with associated difference-in-differences estimates reported in A38. Note that, in the event study figures, the coefficients for the 2015/2016 school year and for the 2023/2024 school year come entirely from Illinois, as Texas data are not available in those years.

We see that students across treatment arms trended similarly in most outcomes during the pre-treatment period, and this mostly continued after the program began. One exception is that our mea-

[30] We are unable to directly link children longitudinal to their baseline strengths and difficulties questionnaire responses for those with more than two children in the household within the same questionnaire age group; for these cases, we take as the baseline value for each child the household by age group average response.

sure that the student met expectations on statewide standardized math tests does appear to worsen in the treatment group relative to the control group after the start of treatment; this is consistent with the negative effect on this outcome we report in Table 10 and is also apparent in the associated difference-in-differences estimate reported in Appendix Table A38.

We also conduct this analysis for outcomes observed only in Texas or only in Illinois in Appendix Figures A13 and A14 respectively. We do not find much evidence of dynamic treatment effects on disciplinary measures observed in Texas.[31] Using the Illinois data, we find some evidence that the negative effect observed on grades across all courses does not persist for some outcomes. For example, by the 2023/2024 school year, the percent of courses where the student meets expectations or masters the material is higher in the treatment group than in the control group, although this difference is not statistically significant. It's therefore possible the negative effects observed early in the study period were driven by the unique context of the COVID-19 pandemic.

6.8 Comparison to Expert Predictions

Prior to the collection of our endline data, we asked National Bureau of Economic Research (NBER) members to predict what effects we would find for a small number of outcomes related to children via the Social Science Prediction Platform. Twenty-three NBER members completed the prediction survey on outcomes related to this paper. We asked about the impact of the transfer on the number of new children in the household, whether the child had been suspended or expelled from school, and whether a staff member at the child's school asked someone to come in and talk about problems with the child's schoolwork or behavior in the past 2 years. We report histograms of these forecasts, along with our estimated year 3 effect, in Appendix Figure A15. Average and median predictions were very close to the actual treatment effect on the number of new children, although predictions were fairly diffuse (and all positive), suggesting that experts were not in agreement as to the effect of the transfer on new children *ex ante*. In contrast, experts were more optimistic about the transfer's ability to reduce suspensions and expulsions and requests from school staff to talk about children's schoolwork or behavior than we actually found. Experts generally predicted improvements in these outcomes, while our results showed a non-significant but worsening effect.

[31] We do not conduct this analysis on measures with very low baseline rates, such as expulsions or suspensions for specific offenses.

7 Discussion

This study provides new evidence on the effects of a large-scale, sustained unconditional cash transfer on parenting behavior, household investments in children, and child outcomes. We find that a $1,000 monthly transfer over three years led to clear improvements in parenting quality—especially in the form of better supervision and monitoring—and modest increases in spending on children. These effects were most pronounced among families with the lowest baseline incomes and among single parents, groups that may face the most constraints on time and resources.

However, these improvements in parenting and spending did not translate into observable gains in children's educational performance or broader measures of social and emotional well-being over the 3-year period studied. In fact, parents in the treatment group reported more behavioral difficulties and stress among their children. These findings may reflect heightened parental awareness resulting from increased supervision rather than deterioration in actual child well-being. This theory is consistent with observations made by qualitative researchers, who found in a series of semi-structured interviews with participants that parents perceived more behavioral issues through heightened parental presence and greater ability to engage with schools, clinicians, and support systems (Dotson et al., 2025). More research is needed to confirm this interpretation.

The transfer did not significantly affect childbearing, desire for pregnancy, or contraceptive use, nor did it meaningfully alter key aspects of the home or neighborhood environment. While there were short-term reductions in parental stress, these gains dissipated over time, suggesting adaptation effects or rising stress in anticipation of a transition as the program's end approached.

Taken together, our findings highlight the complexity of how increased financial resources translate into outcomes for children. While cash transfers can enable better parenting practices and increased investment in children, these inputs do not automatically yield measurable improvements in child development in the short run. It may be the case that the behavioral and investment changes that we document here, or other changes that were unmeasured, will lead to improved outcomes over a longer horizon as children age; for example, if the parenting effects of the transfer resulted in lasting improvements in the parent-child relationship. Such delayed beneficial effects for treated children have been observed in other experimental interventions that initially yielded small or null short-term benefits (e.g., Chetty et al., 2011, 2016). Alternatively, it may be the case that the lack of positive effects we observe over the three year window of the intervention will persist. We hope to assess these

long-term treatment effect dynamics in future work.

References

University of Wisconsin School of Medicine and Public Health (2024). Area deprivation index.

Akee, R., W. Copeland, E. J. Costello, and E. Simeonova (2018, March). How Does Household Income Affect Child Personality Traits and Behaviors? *American Economic Review 108*(3), 775–827.

Akee, R. K. Q., W. E. Copeland, G. Keeler, A. Angold, and E. J. Costello (2010, January). Parents' Incomes and Children's Outcomes: A Quasi-Experiment Using Transfer Payments from Casino Profits. *American Economic Journal: Applied Economics* 2(1), 86–115.

Bailey, M. J., V. W. Lang, A. Prettyman, I. Vrioni, L. J. Bart, D. Eisenberg, P. Fomby, J. Barber, and V. Dalton (2023). How costs limit contraceptive use among low-income women in the U.S.: A randomized control trial. NBER Working Paper.

Balakrishnan, S., S. Chan, S. Constantino, J. Haushofer, and J. Morduch (2024). Household Responses to Guaranteed Income: Experimental Evidence from Compton, California. NBER Working Paper.

Barr, A., J. Eggleston, and A. A. Smith (2022, 04). Investing in infants: the lasting effects of cash transfers to new families. *The Quarterly Journal of Economics 137*(4), 2539–2583.

Bartik, A., E. Rhodes, A. Bartik, D. Broockman, S. Miller, and E. Vivalt (2024, August). The impact of unconditional cash transfers on consumption and household balance sheets: Experimental evidence from two us states. Working Paper 32784, National Bureau of Economic Research.

Bastian, J. and K. Michelmore (2018). The long-term impact of the earned income tax credit on children's education and employment outcomes. *Journal of Labor Economics 36*(4), 1127–1163.

Benjamini, Y. and Y. Hochberg (1995). Controlling the false discovery rate: a practical and powerful approach to multiple testing. *Journal of the Royal statistical society: series B (Methodological) 57*(1), 289–300.

Bernard, J., K. Drotning, and K. R. Genadek (2024). Where are your parents? Exploring potential bias in administrative records on children. CES Working Paper 24-18.

Bevans, K., W. Gardner, K. Pajer, A. Riley, and C. Forrest (2013). Qualitative development of the promis® pediatric 8 stress response item banks. *Journal of Pediatric Psychology 38*(2), 173–191.

Black, D. A., N. Kolesnikova, S. G. Sanders, and L. J. Taylor (2013, 03). Are children "normal"? *The Review of Economics and Statistics 95*(1), 21–33.

Bloniarz, A., H. Liu, C.-H. Zhang, J. S. Sekhon, and B. Yu (2016). Lasso adjustments of treatment effect estimates in randomized experiments. *Proceedings of the National Academy of Sciences 113*(27), 7383–7390.

Borra, C., A. Costa-Ramón, L. González, and A. Sevilla (2022). The Causal Effect of an Income Shock on Children's Human Capital. *Journal of Labor Economics*.

Broockman, D. E., E. Rhodes, A. W. Bartik, K. Dotson, S. Miller, P. K. Krause, and E. Vivalt (2024). The causal effects of income on political attitudes and behavior: A randomized field experiment. NBER Working Paper.

Bullinger, L., A. Packham, and K. Raissian (2023, September). Effects of Universal and Unconditional Cash Transfers on Child Abuse and Neglect. Technical Report w31733, National Bureau of Economic Research, Cambridge, MA.

Carneiro, P. and J. J. Heckman (2002). The evidence on credit constraints in post-secondary schooling. *The Economic Journal 112*(482), 705–734.

Case, A., D. Lubotsky, and C. Paxson (2002, December). Economic status and health in childhood: The origins of the gradient. *American Economic Review 92*(5), 1308–1334.

Cesarini, D., E. Lindqvist, R. Östling, and B. Wallace (2016, May). Wealth, Health, and Child Development: Evidence from Administrative Data on Swedish Lottery Players. *The Quarterly Journal of Economics 131*(2), 687–738.

Chetty, R., J. N. Friedman, N. Hilger, E. Saez, D. W. Schanzenbach, and D. Yagan (2011, 11). How Does Your Kindergarten Classroom Affect Your Earnings? Evidence from Project Star. *The Quarterly Journal of Economics 126*(4), 1593–1660.

Chetty, R., D. Grusky, M. Hell, N. Hendren, R. Manduca, and J. Narang (2017). The Fading American Dream: Trends in Absolute Income Mobility Since 1940. *Science 6336*, 398–406.

Chetty, R., N. Hendren, and L. F. Katz (2016, April). The effects of exposure to better neighborhoods on children: New evidence from the moving to opportunity experiment. *American Economic Review 106*(4), 855–902.

Coley, R. L., J. Sims, and E. V. Drzal (2016, November). Family expenditures supporting children across income and urbanicity strata. *Children and Youth Services Review 70*(C), 129–142.

Conger, R. and M. Donnellan (2007). An interactionist perspective on the socioeconomic context of human development. *Annual Review of Psychology*.

Conger, R., X. Ge, G. Elder, F. Lorenz, and R. Simons (1994). Economic stress, coercive family process, and developmental problems of adolescents. *Child Development* (2).

Costanzo, M. A., K. A. Magnuson, G. J. Duncan, N. Fox, L. A. Gennetian, S. Halpern-Meekin, K. G. Noble, and H. Yoshikawa (2025, April). A research note on unconditional cash transfers and fertility in the united states: New causal evidence. *Demography 62*(2), 405–417.

COVID-19 School Data Hub (2023). All school learning model data. *Data Resources* (Version 3/8/23). Accessed June 1, 2025. Available at https://www.covidschooldatahub.com/data-resources.

Crosta, T., D. Karlan, F. Ong, J. Rüschenpöhler, and C. R. Udry (2025). Unconditional cash transfers: A bayesian meta-analysis of randomized evaluations in low and middle income countries. *NBER Working Paper 32779*.

Cumming, F. and L. Dettling (2023, 03). Monetary policy and birth rates: The effect of mortgage rate pass-through on fertility. *The Review of Economic Studies 91*(1), 229–258.

Dahl, G. B. and L. Lochner (2012, August). The Impact of Family Income on Child Achievement: Evidence from the Earned Income Tax Credit. *American Economic Review 102*(5), 1927–1956.

de Gendre, A., J. Lynch, A. Meunier, R. Pilkington, and S. Schurer (2021, August). Child Health and Parental Responses to an Unconditional Cash Transfer at Birth. IZA Discussion Paper Series No. 14693.

Deeks, J. J., D. G. Altman, and M. J. Bradburn (2001). Statistical methods for examining heterogeneity and combining results from several studies in meta-analysis. In M. Egger, G. Davey Smith, and D. G. Altman (Eds.), *Systematic Reviews in Health Care: Meta-analysis in Context* (2nd ed.)., pp. 285–312. London: BMJ Books.

Dotson, K., Y. Vargas, E. Rhodes, and J. Wiederspan (2025). Time well spent: Rethinking parental investments through the lens of presence and cash transfers. Working Paper.

Duncan, G. J. and K. Magnuson (2013). *The Long Reach of Early Childhood Poverty*, pp. 57–70. Dordrecht: Springer Netherlands.

Duncan, G. J. and R. J. Murnane (Eds.) (2011). *Whither Opportunity?: Rising Inequality, Schools, and Children's Life Chances*. Russell Sage Foundation.

Essau, C. A., S. Sasagawa, and P. J. Frick (2006). Psychometric properties of the alabama parenting questionnaire. *Journal of Child and Family Studies 15*, 597–616.

Genadek, K., J. Sanders, and A. Stevenson (2021, July). Measuring US fertility using administrative data from the Census Bureau. Technical Report 2021-02.

Gennetian, L., G. Duncan, N. Fox, S. Halpern-Meekin, K. Magnuson, K. G. Noble, and H. Yoshikawa (2024). Effects of a monthly unconditional cash transfer starting at birth on family investments among US families with low income. *Nature Human Behavior 8*, 1514–1529.

Goodman, R. (1997). The strengths and difficulties questionnaire: a research note. *Journal of Child Psychology and Psychiatry 38*(5), 581–586.

Guess, A. M., N. Malhotra, J. Pan, P. Barberá, H. Allcott, T. Brown, A. Crespo-Tenorio, D. Dimmery, D. Freelon, M. Gentzkow, S. González-Bailón, E. Kennedy, Y. M. Kim, D. Lazer, D. Moehler, B. Nyhan, C. V. Rivera, J. Settle, D. R. Thomas, E. Thorson, R. Tromble, A. Wilkins, M. Wojcieszak, B. Xiong, C. K. de Jonge, A. Franco, W. Mason, N. J. Stroud, and J. A. Tucker (2023). Reshares on social media amplify political news but do not detectably affect beliefs or opinions. *Science 381*(6656), 404–408.

Guryan, J., E. Hurst, and M. Kearney (2008, September). Parental education and parental time with children. *Journal of Economic Perspectives 22*(3), 23–46.

Hanna, M., H. Luke Shaefer, H. Fogle, J. Khaldun, W. McWeeny, O. Richardson, T. Thomas, and A. Pipa (2024). Scaling up prenatal and infant cash prescriptions to eradicate deep infant poverty in the united states. Technical report, Brookings Institution.

Hawkins, A., C. Hollrah, S. Miller, L. R. Wherry, G. Aldana, and M. Wong (2025, September). The long-term effects of income for at-risk infants: Evidence from supplemental security income. *American Economic Review 115*(9), 3081–3129.

Heroy, S., I. Loaiza, A. Pentland, and N. O'Clery (2023). Are neighbourhood amenities associated with more walking and less driving? Yes, but predominantly for the wealthy. *Environment and Planning B: Urban Analytics and City Science 50*(4), 958–982.

Jacob, B., N. Pilkauskas, E. Rhodes, K. Richard, and H. L. Shaefer (2022). The COVID-19 Cash Transfer Study II: The Hardship and Mental Health Impacts of an Unconditional Cash Transfer to Low-Income Individuals. *National Tax Journal 75*(3), 597–625.

Jaroszewicz, A., O. P. Hauser, J. M. Jachimowicz, and J. Jamison (2023). Cash can make its absence felt: Randomizing unconditional cash transfer amounts in the us. Working paper.

Kearney, M. and L. Dettling (2024). The cyclicality of births and babies' health, revisited: Evidence from unemployment insurance. NBER Working Paper 30937.

Kearney, M. S. and R. Wilson (2018, 10). Male earnings, marriageable men, and nonmarital fertility: Evidence from the fracking boom. *The Review of Economics and Statistics 100*(4), 678–690.

Kim, B. K. E., A. Castro, S. West, N. Tandon, L. Ho, V. T. Nguyen, and K. Sharif (2024). The american guaranteed income studies: City of los angeles big:leap. Report, University of Pennsylvania, Center for Guaranteed Income Research.

Kind, A. and W. Buckingham (2018). Making neighborhood disadvantage metrics accessible: The neighborhood atlas. *New England Journal of Medicine 378*.

Kornrich, S. and F. Furstenberg (2013, September). Investing in children: Changes in parental spending on children, 1972–2007. *Demography 50*(1), 1–23.

LaBriola, J. and D. Schneider (2021, December). Class inequality in parental childcare time: Evidence from synthetic couples in the atus. *Social Forces 100*(2), 680–705. Epub 2021 Jan 28.

Lee, D. (2009). Training, wages, and sample selection: Estimating sharp bounds on treatment effects. *The Review of Economic Studies 76*(3), 1071–1102.

Liebman, J., K. Carlson, E. Novick, and P. Portocarrero (2022). The Chelsea Eats Program: Experimental Impacts. Working paper.

Lindo, J. M. (2010). Are children really inferior goods? *Journal of Human Resources 45*(2), 301–327.

Magnuson, K. A., G. J. Duncan, H. Yoshikawa, P. Y. Yoo, S. Han, L. A. Gennetian, S. Halpern-Meekin, N. A. Fox, and K. G. Noble (2024). Can Cash Transfers Improve Maternal Well-being and Family Processes among Families with Young Children? An Experimental Analysis. Technical report.

Matheny, A. P., T. D. Wachs, J. L. Ludwig, and K. Phillips (1995). Bringing order out of chaos: Psychometric characteristics of the confusion, hubbub, and order scale. *Journal of Applied Developmental Psychology 16*(3), 429–444.

Mayer, S. (1997). *What money can't buy: Family income and children's life chances*. Harvard University Press.

Miller, S., E. Rhodes, A. Bartik, D. Broockman, P. Krause, and E. Vivalt (2025). Does Income Affect Health? Evidence from a Randomized Controlled Trial of a Guaranteed Income. NBER Working Paper.

Milligan, K. and M. Stabile (2011, August). Do Child Tax Benefits Affect the Well-being of Children? Evidence from Canadian Child Benefit Expansions. *American Economic Journal: Economic Policy 3*(3), 175–205.

Monna, B. and A. H. Gauthier (2008). A review of the literature on the social and economic determinants of parental time. *Child Development* (29), 634—653.

Noble, K., K. Magnuson, G. Duncan, L. Gennetian, H. Yoshikawa, N. Fox, S. Halpern-Meekin, S. Troller-Renfree, S. Han, S. Egan-Dailey, T. Nelson, J. Nelson, S. Black, M. Georgieff, and D. Karhson (2024). The effect of a monthly unconditional cash transfer on children's development at four years of age: A randomized controlled trial in the u.s. Technical report.

Noble, K., K. Magnuson, L. Gennetian, G. J. Duncan, H. Yoshikawa, N. A. Fox, and S. Halpern-Meekin (2021). Baby's first years: Design of a randomized controlled trial of poverty reduction in the united states. *Pediatrics 148*(4).

Noekle, C., N. McArdle, B. DeVoe, M. Leonardos, Y. Lu, R. Ressler, and D. Acevedo-Garcia (2024). Child opportunity index 3.0 technical documentation. Technical report, diversitydatakids.org, Brandeis University.

Page, M. (2024). New advances on an old question: Does money matter for children's outcomes? *Journal of Economic Literature 62*(3), 891–947.

Pilkauskas, N., K. Michelmore, N. Kovski, and L. Shaefer (2024). The expanded child tax credit and economic wellbeing of low-income families. *Journal of Population Economics 37*.

Ruggles, S., S. Flood, M. Sobek, D. Backman, G. Cooper, J. A. R. Drew, S. Richards, R. Rodgers, J. Schroeder, and K. C. Williams. (2025). Ipums usa: Version 16.0 [dataset]. Minneapolis, MN: IPUMS.

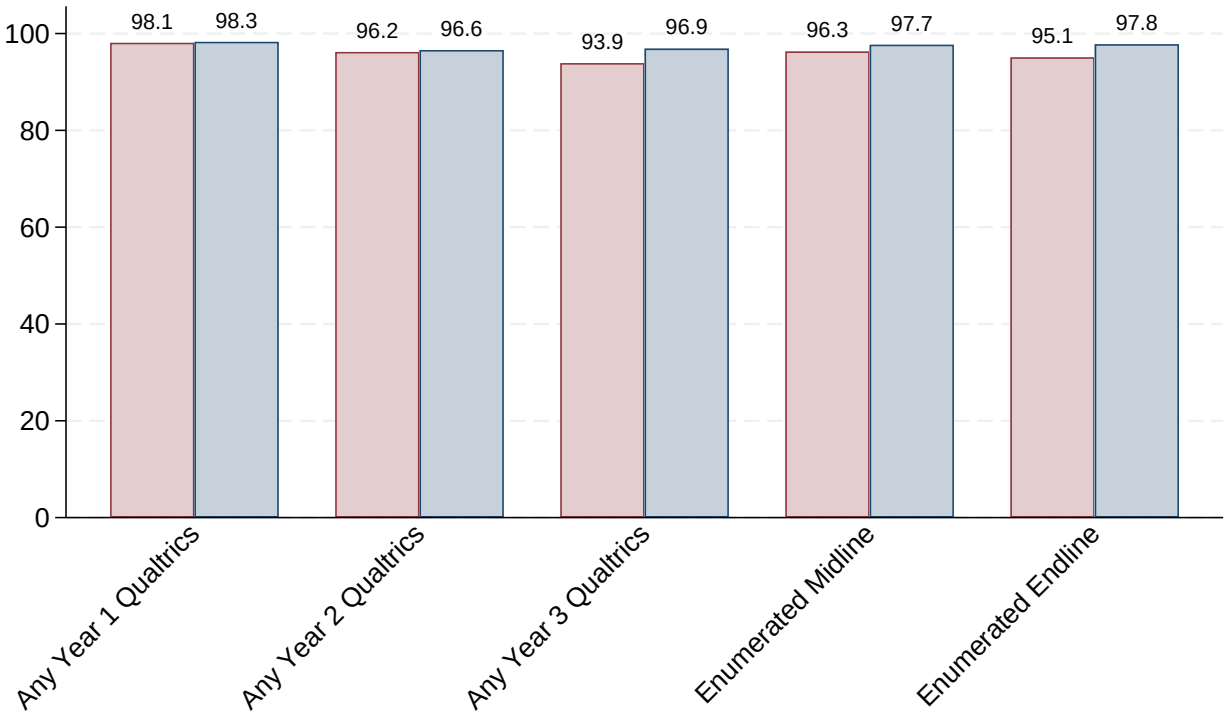
SafeGraph (2022). Global places (poi) & geometry. [Dataset].

Troller-Renfree, S. V., M. A. Costanzo, G. J. Duncan, K. Magnuson, L. A. Gennetian, H. Yoshikawa, S. Halpern-Meekin, N. A. Fox, and K. G. Noble (2022, February). The impact of a poverty reduction intervention on infant brain activity. *Proceedings of the National Academy of Sciences 119*(5).

Vivalt, E., E. Rhodes, A. Bartik, D. Broockman, and S. Miller (2025). The Employment Effects of a Guaranteed Income: Experimental Evidence from Two U.S. States. Working Paper 32719, National Bureau of Economic Research.

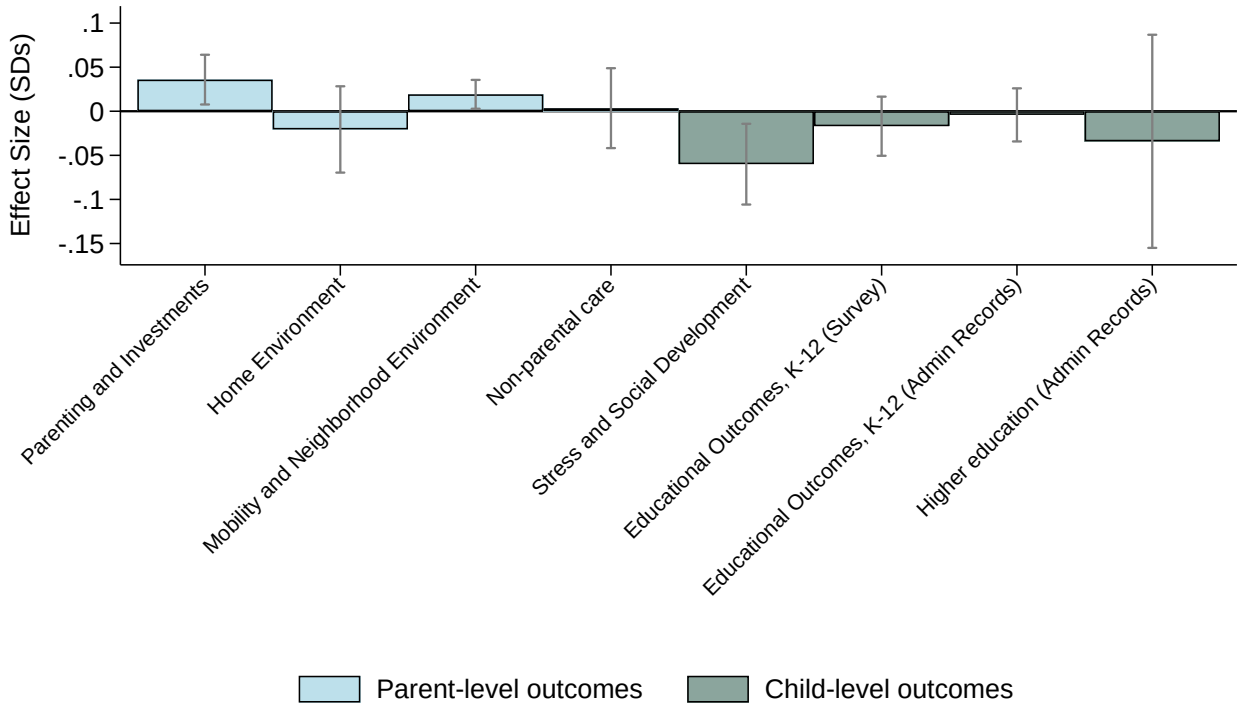
Zhao, A. and P. Ding (2024). No star is good news: A unified look at rerandomization based on p-values from covariate balance tests. *Journal of Econometrics 241*(1), 105724.

Figure 1: Response Rates by Treatment Arm (Sample of Parents)



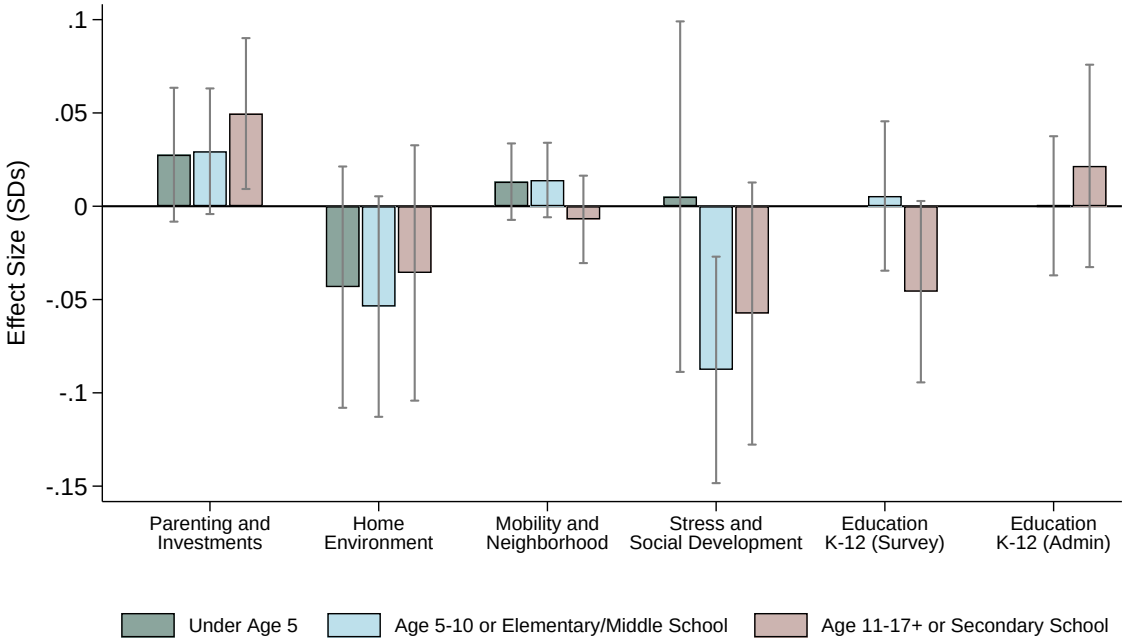
Note: Figure plots response rates by treatment arm to different surveys. For each time period, the first bar (in red) denotes control group response rates. The second bar (in blue) denotes treatment group response rates.

Figure 2: Summary of Results - Family Level Estimates Pooling All Age Children



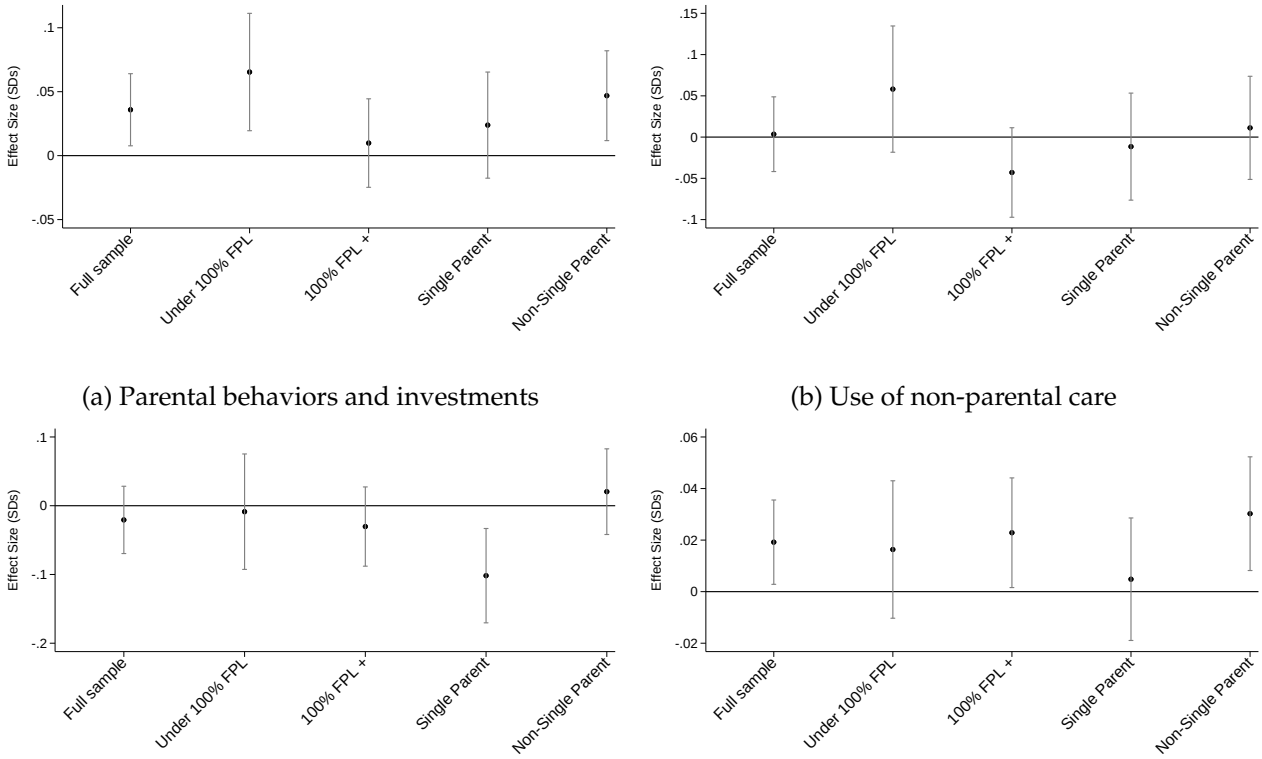
Note: Figure plots estimates of family-level treatment effect, with vertical lines indicating 95% confidence intervals.

Figure 3: Summary of Results - Family Level Estimates by Age Group



Note: Figure plots estimates of family-level treatment effect by age group, with vertical lines indicating 95% confidence intervals.

Figure 4: Family-Level Effects of Guaranteed Income by Subgroup - Outcomes of Parents



(a) Parental behaviors and investments

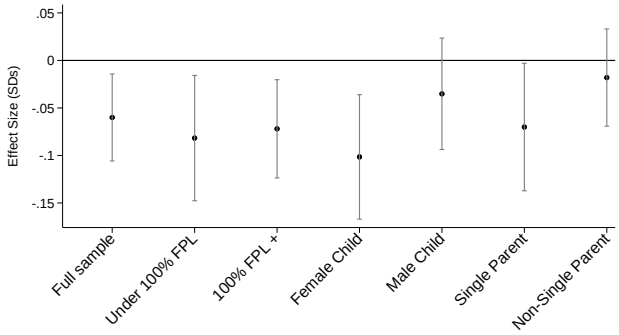
(b) Use of non-parental care

(c) Home environment

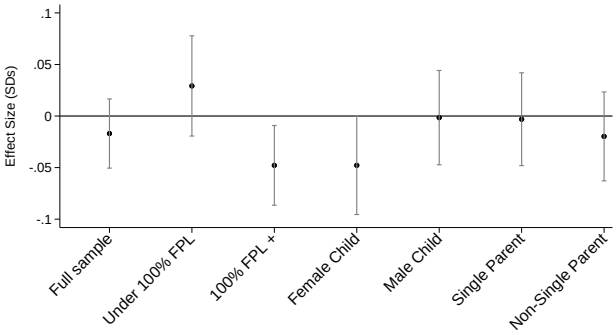
(d) Mobility and neighborhood

Note: Figure plots estimates of family-level treatment effect for full sample (left-most estimate) and by subgroup, with vertical lines indicating 95% confidence intervals.

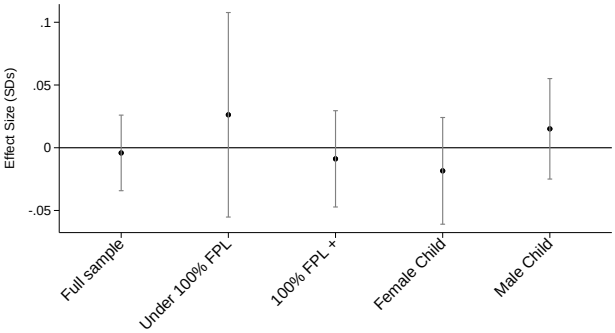
Figure 5: Family-Level Effects of Guaranteed Income by Subgroup - Outcomes of Children (All ages pooled)



(a) Stress and social development



(b) K-12 Educational outcomes (survey)



(c) K-12 Educational outcomes (administrative data)

Note: Figure plots estimates of family-level treatment effect for full sample (left-most estimate) and by subgroup, with vertical lines indicating 95% confidence intervals.

Table 1: Baseline characteristics by treatment arm - participants with child in household

	Treatment	Control	p-value
Demographic			
Age	31.357	31.255	0.698
Male	0.233	0.239	0.760
Female	0.766	0.761	0.819
Non-binary/other	0.002	0.000	0.317
Non-Hispanic Black	0.330	0.335	0.821
Non-Hispanic Asian	0.023	0.024	0.897
Non-Hispanic White	0.434	0.426	0.737
Non-Hispanic Native American	0.025	0.024	0.937
Hispanic	0.242	0.234	0.701
Household Size	3.706	3.706	0.996
Intent for future pregnancy	2.104	2.203	0.418
Employed	0.558	0.569	0.638
Economic			
Personal Income ($1000s)	22.701	22.462	0.820
Household Income ($1000s)	33.195	33.108	0.929
Under FPL	0.341	0.356	0.486
HS Degree/GED or higher	0.943	0.920	0.050
Parental satisfaction (1-5)	4.143	4.153	0.808
Parenting behavior and circumstances			
Child(ren) food insecure	0.353	0.338	0.471
Household CHAOS score	12.815	12.641	0.498
Monthly $ on personal items for child(ren)	39.843	37.156	0.434
Monthly child educational expenses	37.631	35.131	0.619
Monthly childcare expenses	60.967	48.038	0.117
Used childcare at baseline	0.323	0.313	0.527
Ever used childcare	0.526	0.504	0.188
# of hours of non-parental childcare	9.492	9.300	0.739
Attended parent-teacher conference last year	0.736	0.722	0.433
# days ate dinner with child last week	5.569	5.677	0.232
Attended school event last year	0.613	0.618	0.796
Frequency of working on homework with child(ren)	14.933	15.233	0.711
Volunteered at child's school in last year	0.331	0.313	0.349
Frequency of playing with child(ren)	4.650	4.657	0.802

Notes: This table shows baseline characteristics of ORUS participants who had a child in the household across treatment arms. The p-value is associated with the test of equality of means across the two treatment arms.

Table 2: Baseline characteristics of children in household by treatment arm

	Treatment	Control	p-value
Demographic			
Age at baseline	7.143	7.133	0.956
Non-Hispanic Black	0.361	0.343	0.499
Non-Hispanic Asian	0.021	0.027	0.387
Non-Hispanic White	0.365	0.370	0.836
Non-Hispanic Native American	0.019	0.027	0.315
Hispanic	0.334	0.316	0.475
Male	0.510	0.506	0.813
Female	0.490	0.494	0.813
Relationship to Participant			
Biological child of participant	0.892	0.879	0.340
Adopted child of participant	0.010	0.008	0.738
Foster child of participant	0.003	0.001	0.233
Step child of participant	0.036	0.044	0.357
Child sibling of participant	0.027	0.032	0.459
Grandchild of participant	0.007	0.006	0.882
Stress and social development			
PROMIS stress score (4-20)	7.623	7.635	0.848
SDQ conduct score (0-10)	2.053	1.994	0.201
SDQ hyperactivity score (0-10)	4.012	4.008	0.942
SDQ peer problems score (0-10)	2.205	2.140	0.119
SDQ prosociality score (0-10)	7.929	7.990	0.216
SDQ total difficulties (0-40)	10.013	9.884	0.408
School outcomes			
Currently enrolled in school	0.965	0.962	0.579
Days absent from school last school year	5.003	5.355	0.059
Ever suspended or expelled last 2 years	0.072	0.078	0.380
Ever repeated a grade	0.062	0.074	0.055

Notes: This table shows baseline characteristics of children in the households of ORUS participants across treatment arms. The p-value is associated with the test of equality of means across the two treatment arms.

Table 3: Impact of Guaranteed Income on Parental Behaviors and Investments

	All Ages		Under Age 5		Age 5-10		Age 11-17	
	Control Mean	Effect	Control Mean	Effect	Control Mean	Effect	Control Mean	Effect
Parental Behavior and Investment		**0.036 (0.014)****		**0.028 (0.018)**		**0.029 (0.017)***		**0.050 (0.021)****
		[0.112]		**[0.356]**		**[0.275]**		**[0.112]**
Parenting Quality		0.050 (0.020)**†		-0.013 (0.029)		0.024 (0.027)		0.065 (0.029)**†
		[0.051]		[0.780]		[0.491]		[0.059]
Corporal Punishment Subscale (0-15)	4.37	-0.134 (0.071)*	4.46	0.023 (0.101)	4.45	-0.038 (0.090)	4.40	-0.136 (0.111)
		[0.708]		[1.000]		[1.000]		[1.000]
Inconsistent Discipline Subscale (0-30)	13.23	-0.126 (0.163)	13.57	0.076 (0.252)	13.83	-0.010 (0.204)	13.40	-0.071 (0.247)
		[1.000]		[1.000]		[1.000]		[1.000]
Parental Involvement Subscale (0-50)	37.12	-0.018 (0.280)	37.65	-0.015 (0.395)	39.47	0.082 (0.278)	38.92	0.157 (0.315)
		[1.000]		[1.000]		[1.000]		[1.000]
Poor Supervision / Monitoring Subscale (0-50)	14.55	-0.483 (0.183)***	13.60	-0.124 (0.210)	13.67	-0.150 (0.190)	15.17	-0.494 (0.245)**
		[0.204]		[1.000]		[1.000]		[0.533]
Positive Parenting Subscale (0-30)	25.69	0.172 (0.162)	26.66	-0.174 (0.171)	26.56	0.118 (0.145)	25.93	0.327 (0.175)*
		[1.000]		[1.000]		[1.000]		[0.708]
Monthly expenditures on children	274	31 (10)***††	350	45 (15)***††	307	41 (12)***††	272	36 (14)**†
		[0.024]		[0.028]		[0.024]		[0.051]
Parental Interaction		0.019 (0.017)		0.028 (0.026)		0.019 (0.022)		0.021 (0.027)
		[0.402]		[0.402]		[0.491]		[0.508]
Frequency of reading to child(ren) under 5 (1=Never, . . . , 5=Daily)	4.51	0.009 (0.044)	4.51	0.012 (0.043)				
		[1.000]		[1.000]				
Frequency of outings with child(ren) under 5 (1=Never,. . . ,5=Daily)	3.78	-0.044 (0.056)	3.78	-0.032 (0.054)				
		[1.000]		[1.000]				
Frequency of playing with their child(ren) under 5 (1=Never, . . . , 5=Daily)	4.62	0.037 (0.042)	4.62	0.054 (0.041)				
		[1.000]		[1.000]				
# of days in the last week put their child to bed	4.99	-0.058 (0.081)	5.98	0.082 (0.080)	5.77	-0.008 (0.086)	4.74	-0.025 (0.124)
		[1.000]		[1.000]		[1.000]		[1.000]
Hours reading to children, helping w homework, other activities	12.47	-0.064 (0.505)	16.04	0.405 (0.856)	13.22	0.258 (0.694)	11.14	-0.546 (0.665)
		[1.000]		[1.000]		[1.000]		[1.000]
Frequency of working on homework w/ child(ren) (1=Never,. . . ,5=Daily)	3.70	-0.016 (0.053)			3.88	0.074 (0.060)	3.41	-0.105 (0.066)
		[1.000]				[1.000]		[0.971]
Attended a parent-teacher conference in the past year	0.53	0.034 (0.018)*			0.61	0.011 (0.024)	0.55	0.046 (0.026)*
		[0.708]				[1.000]		[0.761]
# of days in the last week the parent ate dinner with their child	4.97	0.051 (0.074)			5.46	0.092 (0.082)	4.94	-0.003 (0.103)
		[1.000]				[1.000]		[1.000]
Attended a school event in the past year	0.49	0.010 (0.019)			0.56	-0.019 (0.024)	0.56	0.024 (0.028)
		[1.000]				[1.000]		[1.000]
Worked with a group outside of school in the past year	0.21	-0.001 (0.016)			0.26	-0.028 (0.021)	0.21	0.023 (0.023)
		[1.000]				[1.000]		[1.000]
Attended a general meeting at the child's school in the past year	0.60	0.058 (0.018)***†			0.70	0.018 (0.022)	0.62	0.043 (0.027)
		[0.075]				[1.000]		[1.000]
Volunteered at child's school in the past year	0.22	0.011 (0.016)			0.27	0.007 (0.021)	0.22	0.020 (0.024)
		[1.000]				[1.000]		[1.000]
Hours spent last week on childcare	45.34	-0.383 (1.189)	60.30	0.258 (1.776)	52.97	-0.192 (1.492)		
		[1.000]		[1.000]		[1.000]		
Hours spent last week with family, in person	30.89	0.202 (0.808)	36.41	1.433 (1.291)	34.79	2.420 (1.148)**	32.37	-0.548 (1.221)
		[1.000]		[1.000]		[0.445]		[1.000]
N		1849		1097		1261		945

Notes: This table reports estimated treatment effects on outcomes listed in the rows. The family-level effect is reported in bold in the top row. Underlined outcomes represent components that contribute to the family-level estimate. If more than one outcome is related to the component topic, treatment effect estimates listed under the component are aggregated and the component is reported in standard deviation units. N is the average number of observations across the outcomes within an age group; N may differ based on (e.g.) survey non-response. ∗ and † denote traditional and FDR-adjusted significance levels respectively. In all cases, three symbols denote the 1% level, two denote the 5% level, and one denotes the 10% level of significance of the test.

Table 4: Impact of Guaranteed Income on Parental Behaviors and Investments (cont)

	All Ages		Under Age 5		Age 5-10		Age 11-17	
	Control Mean	Effect	Control Mean	Effect	Control Mean	Effect	Control Mean	Effect
Parental satisfaction		**0.003 (0.026)**		**-0.014 (0.034)**		**-0.053 (0.032)***		**-0.005 (0.040)**
Ranging from 1=Strongly Disagree to 5=Strongly Agree:		**[0.987]**		**[0.817]**		**[0.179]**		**[0.987]**
I feel alone when it comes to raising my child(ren)	2.84	-0.010 (0.047)	2.88	-0.034 (0.063)	2.81	0.104 (0.059)*	2.89	0.028 (0.074)
		[1.000]		[1.000]		[0.761]		[1.000]
I spend a great deal of time with my child(ren)	4.21	-0.001 (0.036)	4.38	0.001 (0.045)	4.35	-0.023 (0.040)	4.26	-0.009 (0.052)
		[1.000]		[1.000]		[1.000]		[1.000]
I get as much satisfaction from parenting as others do	3.97	0.001 (0.037)	4.06	-0.061 (0.050)	4.04	-0.040 (0.047)	3.95	0.018 (0.052)
		[1.000]		[1.000]		[1.000]		[1.000]
Parents' stress and distress		-0.021 (0.029)		-0.027 (0.036)		-0.009 (0.034)		0.010 (0.039)
		[0.508]		[0.508]		[0.895]		[0.895]
K6 score (0-24)	6.59	0.102 (0.145)	6.35	0.198 (0.197)	6.30	0.005 (0.179)	6.29	-0.153 (0.202)
		[1.000]		[1.000]		[1.000]		[1.000]
Composite stress score (0-40)	18.26	0.125 (0.218)	18.08	0.079 (0.288)	17.91	0.109 (0.274)	17.98	0.097 (0.319)
		[1.000]		[1.000]		[1.000]		[1.000]
N		1849		1097		1261		945

Notes: This table reports estimated treatment effects on outcomes listed in the rows. The family-level effect is reported in bold in the top row. Underlined outcomes represent components that contribute to the family-level estimate. If more than one outcome is related to the component topic, treatment effect estimates listed under the component are aggregated and the component is reported in standard deviation units. N is the average number of observations across the outcomes within an age group; N may differ based on (e.g.) survey non-response. $*$ and † denote traditional and FDR-adjusted significance levels respectively. In all cases, three symbols denote the 1% level, two denote the 5% level, and one denotes the 10% level of significance of the test.

Table 5: Impact of Guaranteed Income on the Home Environment

	All Ages		Under Age 5		Age 5-10		Age 11-17	
	Control Mean	Effect	Control Mean	Effect	Control Mean	Effect	Control Mean	Effect
Home environment		**-0.021 (0.025)**		**-0.043 (0.033)**		**-0.054 (0.030)***		**-0.036 (0.035)**
		[0.617]		**[0.369]**		**[0.271]**		**[0.489]**
Child Food Insecurity Score (0-6, higher is more insecure)	0.83	0.051 (0.039)	0.77	0.117 (0.056)**	0.81	0.103 (0.052)**	0.93	0.026 (0.065)
		[0.711]		[0.619]		[0.619]		[1.000]
Chaos Score (0-36, higher is more chaotic)	13.31	0.000 (0.206)	13.63	0.109 (0.294)	13.54	0.108 (0.268)	13.46	-0.072 (0.306)
		[1.000]		[1.000]		[1.000]		[1.000]
Parent was ever unhoused in survey year	0.10	0.003 (0.010)	0.08	-0.001 (0.013)	0.08	0.009 (0.011)	0.07	0.020 (0.013)
		[1.000]		[1.000]		[1.000]		[0.619]
N		1882		999		1198		914

Notes: This table reports estimated treatment effects on outcomes listed in the rows. The family-level effect is reported in bold in the top row. Underlined outcomes represent components that contribute to the family-level estimate. If more than one outcome is related to the component topic, treatment effect estimates listed under the component are aggregated and the component is reported in standard deviation units. N is the average number of observations across the outcomes within an age group; N may differ based on (e.g.) survey non-response. $*$ and † denote traditional and FDR-adjusted significance levels respectively. In all cases, three symbols denote the 1% level, two denote the 5% level, and one denotes the 10% level of significance of the test.

Table 6: Impact of Guaranteed Income on Mobility and the Neighborhood Environment

	All Ages		Under Age 5		Age 5-10		Age 11-17	
	Control Mean	Effect	Control Mean	Effect	Control Mean	Effect	Control Mean	Effect
Mobility and neighborhood environment		**0.019 (0.008)****		**0.013 (0.010)**		**0.014 (0.010)**		**-0.007 (0.012)**
		[0.112]		**[0.372]**		**[0.369]**		**[0.649]**
Moving behavior		0.105 (0.040)***		0.111 (0.055)**		0.084 (0.050)*		0.011 (0.058)
		[0.374]		[0.439]		[0.451]		[1.000]
Moved units	0.40	0.045 (0.018)**	0.38	0.042 (0.025)*	0.36	0.033 (0.024)	0.35	0.005 (0.027)
		[0.352]		[0.469]		[0.608]		[1.000]
Moved neighborhoods	0.36	0.044 (0.018)**	0.35	0.056 (0.026)**	0.32	0.041 (0.023)*	0.31	0.005 (0.027)
		[0.352]		[0.352]		[0.467]		[1.000]
Family friendliness		-0.008 (0.027)		-0.030 (0.032)		-0.046 (0.032)		-0.036 (0.036)
		[0.969]		[0.687]		[0.503]		[0.663]
Share of households with children	0.35	-0.000 (0.003)	0.35	-0.002 (0.003)	0.36	-0.003 (0.003)	0.36	-0.004 (0.003)
		[1.000]		[1.000]		[0.868]		[0.867]
Share of population that is children	0.25	-0.001 (0.002)	0.25	-0.003 (0.002)	0.25	-0.003 (0.002)*	0.25	-0.002 (0.002)
		[1.000]		[0.676]		[0.469]		[0.932]
Child-Focused Amenities		0.033 (0.017)*		0.037 (0.020)*		0.016 (0.020)		-0.039 (0.021)*
		[0.439]		[0.439]		[0.746]		[0.439]
Distance-decayed count of daycares within 1 mile	2.66	0.129 (0.065)**	2.68	0.177 (0.075)**	2.51	0.122 (0.074)*	2.64	-0.096 (0.079)
		[0.397]		[0.352]		[0.469]		[0.747]
Distance-decayed count of libraries within 1 mile	0.38	0.012 (0.014)	0.39	0.001 (0.018)	0.37	0.000 (0.017)	0.36	-0.028 (0.018)
		[0.932]		[1.000]		[1.000]		[0.542]
Distance-decayed count of parks within 1 mile	3.93	0.070 (0.079)	3.98	0.094 (0.091)	3.80	-0.014 (0.087)	3.72	-0.197 (0.089)**
		[0.924]		[0.867]		[1.000]		[0.352]
Distance-decayed count of schools within 1 mile	3.78	0.167 (0.074)**	3.81	0.219 (0.092)**	3.68	0.084 (0.083)	3.71	-0.066 (0.094)
		[0.352]		[0.352]		[0.867]		[1.000]
Economic Mobility		-0.011 (0.017)		0.012 (0.024)		-0.035 (0.023)		-0.024 (0.023)
		[0.746]		[0.759]		[0.489]		[0.663]
Income mobility measure	0.03	-0.000 (0.001)	0.03	0.001 (0.001)	0.03	-0.001 (0.001)	0.03	-0.001 (0.001)
		[1.000]		[0.868]		[1.000]		[0.859]
Incarceration rate for children w/ low-income parents	0.41	0.001 (0.002)	0.41	0.001 (0.002)	0.41	0.004 (0.002)**	0.41	0.001 (0.002)
		[1.000]		[1.000]		[0.397]		[1.000]
Census tract level pollution		0.001 (0.021)		-0.014 (0.020)		0.040 (0.024)*		0.016 (0.028)
		[1.000]		[0.746]		[0.451]		[0.746]
PM 2.5	8.95	0.005 (0.017)	8.95	0.019 (0.014)	8.95	-0.017 (0.018)	8.97	-0.014 (0.024)
		[1.000]		[0.676]		[0.868]		[1.000]
Risk-Screening Environmental Indicators	7.37	-0.022 (0.033)	7.42	-0.016 (0.038)	7.42	-0.078 (0.039)**	7.32	-0.008 (0.042)
		[1.000]		[1.000]		[0.397]		[1.000]
Quality indices		-0.005 (0.026)		-0.037 (0.037)		0.026 (0.033)		0.029 (0.036)
		[1.000]		[0.663]		[0.746]		[0.746]
Area Deprivation Index	81.04	0.648 (1.300)	81.34	2.107 (1.992)	79.67	-0.850 (1.851)	78.96	-3.166 (2.033)
		[1.000]		[0.867]		[1.000]		[0.542]
Childhood Opportunity Index	-0.27	0.007 (0.021)	-0.29	-0.018 (0.028)	-0.27	0.025 (0.026)	-0.25	-0.017 (0.027)
		[1.000]		[1.000]		[0.868]		[1.000]
N		1950		1148		1310		984

Notes: This table reports estimated treatment effects on outcomes listed in the rows. The family-level effect is reported in bold in the top row. Underlined outcomes represent components that contribute to the family-level estimate. If more than one outcome is related to the component topic, treatment effect estimates listed under the component are aggregated and the component is reported in standard deviation units. N is the average number of observations across the outcomes within an age group; N may differ based on (e.g.) survey non-response. $*$ and † denote traditional and FDR-adjusted significance levels respectively. In all cases, three symbols denote the 1% level, two denote the 5% level, and one denotes the 10% level of significance of the test.

Table 7: Impact of Guaranteed Income on Non-Parental Care (Kids under 5)

	Control Mean	Effect
Non Parental Care		**-0.003 (0.028)**
		[0.849]
Number of hours per week child(ren) are in care	11.66	-0.323 (0.789)
		[1.000]
Quality of childcare		0.016 (0.054)
		[1.000]
Changed to higher quality arrangement	0.04	0.007 (0.007)
		[1.000]
Changed to more reliable arrangement	0.05	0.002 (0.007)
		[1.000]
Participant is satisfied with current childcare	0.81	-0.011 (0.044)
		[1.000]
Stability of care		-0.023 (0.043)
		[1.000]
Count of childcare arrangement changes in period	0.54	0.110 (0.073)
		[1.000]
Number of childcare providers / arrangements	0.53	-0.073 (0.053)
		[1.000]
# times parent made arrangements bc child care fell through	1.43	-0.045 (0.162)
		[1.000]
Participant currently using childcare at time of survey	0.40	0.007 (0.022)
		[1.000]
N		811

Notes: This table reports estimated treatment effects on outcomes listed in the rows. The family-level effect is reported in bold in the top row. Underlined outcomes represent components that contribute to the family-level estimate. If more than one outcome is related to the component topic, treatment effect estimates listed under the component are aggregated and the component is reported in standard deviation units. N is the average number of observations across the outcomes within an age group; N may differ based on (e.g.) survey non-response. $*$ and † denote traditional and FDR-adjusted significance levels respectively. In all cases, three symbols denote the 1% level, two denote the 5% level, and one denotes the 10% level of significance of the test.

Table 8: Impact of Guaranteed Income on Child Stress and Social Development

	All Ages		Under Age 5		Age 5-10		Age 11-17	
	Control Mean	Effect	Control Mean	Effect	Control Mean	Effect	Control Mean	Effect
Stress and Social Development		**-0.060 (0.023)****		**0.005 (0.048)**		**-0.088 (0.031)*****		**-0.058 (0.036)**
		[0.112]		**[0.849]**		**[0.112]**		**[0.318]**
Strengths and Difficulties		-0.049 (0.026)*†		0.005 (0.048)		-0.080 (0.034)**†		-0.014 (0.037)
		[0.074]		[0.333]		[0.060]		[0.310]
Total Difficulties Score (0-40)	8.00	0.400 (0.202)**	8.87	0.018 (0.396)	7.66	0.684 (0.260)***†	7.93	0.135 (0.312)
		[0.116]		[1.000]		[0.077]		[0.970]
Conduct Problems Scale (0-10)	1.34	0.089 (0.061)	2.29	-0.012 (0.134)	1.18	0.071 (0.073)	1.04	0.172 (0.077)**†
		[0.268]		[1.000]		[0.636]		[0.094]
Emotional Problems Scale (0-10)	1.70	0.022 (0.063)	1.37	-0.086 (0.107)	1.70	0.057 (0.085)	1.86	0.060 (0.103)
		[1.000]		[0.737]		[0.782]		[0.872]
Hyperactivity Scale (0-10)	3.37	0.238 (0.091)***†	3.73	0.089 (0.178)	3.43	0.376 (0.128)***†	3.09	-0.016 (0.133)
		[0.077]		[0.970]		[0.077]		[1.000]
Peer Problems Scale (0-10)	1.59	0.064 (0.059)	1.48	0.081 (0.113)	1.36	0.182 (0.077)**†	1.95	-0.086 (0.095)
		[0.538]		[0.782]		[0.088]		[0.680]
Prosocial Scale (0-10)	8.54	-0.043 (0.063)	8.04	0.112 (0.135)	8.78	-0.021 (0.080)	8.54	0.047 (0.101)
		[0.782]		[0.737]		[1.000]		[0.970]
PROMIS mental health score (4-20)	6.61	0.208 (0.099)**†			6.18	0.263 (0.118)**†	7.13	0.317 (0.158)**†
		[0.067]				[0.062]		[0.067]
N		2648		636		1428		1053

Notes: This table reports estimated treatment effects on outcomes listed in the rows. The family-level effect is reported in bold in the top row. Underlined outcomes represent components that contribute to the family-level estimate. If more than one outcome is related to the component topic, treatment effect estimates listed under the component are aggregated and the component is reported in standard deviation units. N is the average number of observations across the outcomes within an age group; N may differ based on (e.g.) survey non-response. $*$ and † denote traditional and FDR-adjusted significance levels respectively. In all cases, three symbols denote the 1% level, two denote the 5% level, and one denotes the 10% level of significance of the test.

Table 9: Impact of Guaranteed Income on Survey-Reported Child Educational Outcomes (K-12)

	All Ages		Age 5-10		Age 11-17	
	Control Mean	Effect	Control Mean	Effect	Control Mean	Effect
Educational Outcomes (Survey, K-12)		**-0.017 (0.017)**		**0.005 (0.020)**		**-0.046 (0.025)***
		[0.489]		**[0.755]**		**[0.271]**
# days absent from school in the most recent school yr	7.17	-0.051 (0.355)	7.14	-0.605 (0.452)	7.08	0.449 (0.420)
		[1.000]		[1.000]		[1.000]
Disciplinary action, and help needed over past 2 years		-0.035 (0.024)		-0.035 (0.028)		-0.039 (0.034)
		[1.000]		[1.000]		[1.000]
Child got special help for behavioral/emotional problems	0.10	0.006 (0.010)	0.09	0.010 (0.011)	0.11	0.006 (0.016)
		[1.000]		[1.000]		[1.000]
Asked to meet about probs. w child's schoolwork or behavior	0.14	0.020 (0.012)*	0.13	0.022 (0.015)	0.16	0.021 (0.017)
		[1.000]		[1.000]		[1.000]
Child got special help at school for learning problems	0.18	0.014 (0.013)	0.19	0.009 (0.017)	0.18	0.013 (0.017)
		[1.000]		[1.000]		[1.000]
Child suspended/expelled	0.06	0.003 (0.007)	0.03	0.001 (0.007)	0.10	0.010 (0.014)
		[1.000]		[1.000]		[1.000]
School enrollment		-0.005 (0.028)		0.016 (0.035)		-0.049 (0.045)
		[1.000]		[1.000]		[1.000]
Child (5-17) is currently in school	0.91	0.009 (0.009)	0.92	0.013 (0.011)	0.97	-0.002 (0.009)
		[1.000]		[1.000]		[1.000]
Whether child has ever repeated a grade	0.02	0.005 (0.005)	0.02	0.002 (0.006)	0.02	0.010 (0.008)
		[1.000]		[1.000]		[1.000]
Report of child's grades (1=Mostly D's and F's, . . . , 5=All A's)	3.93	-0.019 (0.026)	4.08	-0.005 (0.035)	3.78	-0.039 (0.034)
		[1.000]		[1.000]		[1.000]
Perceived quality of child's education (1=Poor, . . . , 5=Excellent)	3.90	-0.022 (0.036)	4.04	-0.015 (0.043)	3.73	-0.033 (0.052)
		[1.000]		[1.000]		[1.000]
N		2906		1671		1360

Notes: This table reports estimated treatment effects on outcomes listed in the rows. The family-level effect is reported in bold in the top row. Underlined outcomes represent components that contribute to the family-level estimate. If more than one outcome is related to the component topic, treatment effect estimates listed under the component are aggregated and the component is reported in standard deviation units. N is the average number of observations across the outcomes within an age group; N may differ based on (e.g.) survey non-response. $*$ and † denote traditional and FDR-adjusted significance levels respectively. In all cases, three symbols denote the 1% level, two denote the 5% level, and one denotes the 10% level of significance of the test.

Table 10: Impact of Guaranteed Income on Child Educational Outcomes from Administrative Records (K-12)

	All Grades		Primary School		Secondary School	
	Control Mean	Effect	Control Mean	Effect	Control Mean	Effect
Education Outcomes from Admin Data		**-0.004 (0.015)**		**0.000 (0.019)**		**0.022 (0.028)**
		[0.755]		**[0.906]**		**[0.617]**
Enrollment Measures		-0.003 (0.027)		-0.006 (0.035)		0.044 (0.046)
		[1.000]		[1.000]		[1.000]
Enrolled in K-12 school	0.91	-0.022 (0.012)*	0.90	-0.009 (0.014)	0.84	-0.004 (0.024)
		[1.000]		[1.000]		[1.000]
Child repeated grade	0.04	-0.005 (0.007)	0.02	-0.003 (0.006)	0.11	-0.013 (0.021)
		[1.000]		[1.000]		[1.000]
Child's age above expectation for grade	0.03	-0.005 (0.005)	0.01	-0.000 (0.002)	0.09	-0.020 (0.015)
		[1.000]		[1.000]		[1.000]
Percentage of Days in attendance	0.90	-0.003 (0.004)	0.91	-0.007 (0.005)	0.87	-0.007 (0.010)
		[1.000]		[1.000]		[1.000]
Gifted and Talented Program	0.06	-0.002 (0.004)	0.05	0.002 (0.006)	0.05	0.001 (0.011)
		[1.000]		[1.000]		[1.000]
Standardized Test Performance		-0.026 (0.034)		-0.018 (0.038)		-0.022 (0.050)
		[1.000]		[1.000]		[1.000]
English / Language Arts Meets Expectations	0.37	-0.004 (0.021)	0.36	0.002 (0.024)	0.45	-0.043 (0.029)
		[1.000]		[1.000]		[1.000]
English / Language Arts Masters Expectations	0.11	0.002 (0.011)	0.12	0.009 (0.012)	0.07	0.003 (0.018)
		[1.000]		[1.000]		[1.000]
Math Meets Expectations	0.29	-0.042 (0.020)**	0.30	-0.042 (0.023)*	0.26	-0.019 (0.033)
		[1.000]		[1.000]		[1.000]
Math Masters Expectations	0.09	-0.002 (0.010)	0.09	-0.005 (0.012)	0.05	-0.004 (0.004)
		[1.000]		[1.000]		[1.000]
N		1737		1541		540

Notes: This table reports estimated treatment effects on outcomes listed in the rows. The family-level effect is reported in bold in the top row. Underlined outcomes represent components that contribute to the family-level estimate. If more than one outcome is related to the component topic, treatment effect estimates listed under the component are aggregated and the component is reported in standard deviation units. N is the average number of observations across the outcomes within an age group; N may differ based on (e.g.) survey non-response. $*$ and † denote traditional and FDR-adjusted significance levels respectively. In all cases, three symbols denote the 1% level, two denote the 5% level, and one denotes the 10% level of significance of the test.

Table 11: Impact of Guaranteed Income on Child Educational Outcomes from Administrative Records (K-12) (cont.)

	All Grades		Primary School		Secondary School	
	Control Mean	Effect	Control Mean	Effect	Control Mean	Effect
Education mobility		**0.006 (0.050)**		**0.029 (0.055)**		**0.056 (0.080)**
		[1.000]		**[1.000]**		**[1.000]**
Changed Schools	0.37	0.011 (0.020)	0.37	0.022 (0.022)	0.32	0.005 (0.030)
		[1.000]		[1.000]		[1.000]
Changed Districts	0.24	0.011 (0.022)	0.24	0.021 (0.024)	0.22	0.011 (0.032)
		[1.000]		[1.000]		[1.000]
School quality		0.028 (0.023)		0.031 (0.028)		0.023 (0.041)
		[1.000]		[1.000]		[1.000]
Class Size	19.61	-0.149 (0.186)	19.89	-0.234 (0.190)	18.29	0.413 (0.416)
		[1.000]		[1.000]		[1.000]
Average Teacher Salary	64535.54	-61.089 (338.342)	64023.00	-111.743 (314.097)	65594.21	653.169 (577.877)
		[1.000]		[1.000]		[1.000]
Peccent Students Chronic Absenteeism	0.25	-0.007 (0.005)	0.23	-0.002 (0.005)	0.28	-0.014 (0.009)
		[1.000]		[1.000]		[1.000]
Percent of Students with ESL	0.19	0.002 (0.007)	0.20	0.003 (0.008)	0.16	-0.003 (0.008)
		[1.000]		[1.000]		[1.000]
Percent of Students Economically Disadvantaged	0.64	0.015 (0.009)*	0.65	0.012 (0.010)	0.61	0.005 (0.015)
		[1.000]		[1.000]		[1.000]
Average Attendance Rate	0.92	0.002 (0.001)*	0.93	0.001 (0.001)	0.91	0.003 (0.003)
		[1.000]		[1.000]		[1.000]
Graduation Rate	0.87	0.008 (0.011)			0.87	0.014 (0.010)
		[1.000]				[1.000]
Percent Proficient in English Language Arts	0.37	-0.003 (0.007)	0.37	-0.001 (0.007)	0.40	-0.007 (0.012)
		[1.000]		[1.000]		[1.000]
Percent Proficient in Math	0.30	-0.007 (0.007)	0.31	-0.003 (0.008)	0.30	-0.000 (0.012)
		[1.000]		[1.000]		[1.000]
Value added Estimate English Scores	1.15	0.037 (0.038)	1.39	0.028 (0.042)	0.35	-0.009 (0.036)
		[1.000]		[1.000]		[1.000]
Value Added Estimate Math Scores	0.41	0.025 (0.034)	0.69	0.038 (0.038)	-0.67	-0.041 (0.038)
		[1.000]		[1.000]		[1.000]
N		1737		1541		540

Notes: This table reports estimated treatment effects on outcomes listed in the rows. The family-level effect is reported in bold in the top row. Underlined outcomes represent components that contribute to the family-level estimate. If more than one outcome is related to the component topic, treatment effect estimates listed under the component are aggregated and the component is reported in standard deviation units. N is the average number of observations across the outcomes within an age group; N may differ based on (e.g.) survey non-response. $*$ and † denote traditional and FDR-adjusted significance levels respectively. In all cases, three symbols denote the 1% level, two denote the 5% level, and one denotes the 10% level of significance of the test.

Table 12: Impact of Guaranteed Income on Child Higher Education Outcomes

	Control Mean	Effect
Higher Education		-0.034 (0.062)
		[0.649]
Any post-secondary program completed	0.04	-0.009 (0.014)
		[1.000]
Months of FTE enrollment in post-secondary program	0.71	-0.042 (0.169)
		[1.000]
N		603

Notes: This table reports estimated treatment effects on outcomes listed in the rows. The family-level effect is reported in bold in the top row. Underlined outcomes represent components that contribute to the family-level estimate. If more than one outcome is related to the component topic, treatment effect estimates listed under the component are aggregated and the component is reported in standard deviation units. N is the average number of observations across the outcomes within an age group; N may differ based on (e.g.) survey non-response. $*$ and † denote traditional and FDR-adjusted significance levels respectively. In all cases, three symbols denote the 1% level, two denote the 5% level, and one denotes the 10% level of significance of the test.

Table 13: Impact of Guaranteed Income on Pregnancy, Childbearing, and Contraception

	Control Mean	Effect	Number of Obs.
Number of new children (survey)	0.19	-0.000 (0.017)	2948
		[1.000]	
Number of births to participants (admin)^{s}	0.14	-0.011 (0.016)	2600
		[1.000]	
Participant reported any new child 2020^{s}	0.16	-0.004 (0.012)	2948
		[1.000]	
Participant had positive pregnancy test (past 6mos)^{s}	0.09	0.019 (0.009)**	2833
		[1.000]	
Extent to which participant and partner want to get pregnant (1=least desire, ... 10=most desire	2.20	-0.020 (0.078)	2941
		[1.000]	
Use of contraception		-0.020 (0.060)	2042
		[1.000]	
Efficacy of contraception used if no pregnancy desire	0.93	-0.011 (0.013)	382
		[1.000]	
Participant uses contraception if no pregnancy desire	0.32	0.025 (0.020)	2042
		[1.000]	
Participant or partner had or will have an abortion (conditional on positive pregnancy test)	0.07	0.026 (0.027)	425
		[1.000]	

Notes: This table reports estimated treatment effects on outcomes listed in the rows. If more than one outcome is related to the component topic (components are denoted by underlining), treatment effect estimates listed under the component are aggregated and the component is reported in standard deviation units. The number of observations differs across outcomes based on (e.g.) survey non-response and the population of interest. The symbol ^{S} denotes secondary/exploratory outcomes. $*$ and † denote traditional and FDR-adjusted significance levels respectively. In all cases, three symbols denote the 1% level, two denote the 5% level, and one denotes the 10% level of significance of the test.

The Impact of Unconditional Cash Transfers on Childbearing and Children

Appendix

Patrick Krause ⓡ Elizabeth Rhodes ⓡ Sarah Miller ⓡ Alexander Bartik ⓡ David Broockman ⓡ Eva Vivalt ⓡ

A Additional Details on Randomization

This section provides additional details on the randomization procedure used in ORUS. We wanted to avoid a situation where treatment and control groups varied meaningfully on baseline covariates simply due to chance. So, we used a blocked random assignment procedure to ensure balance. We also identified, over the course of the enrollment period, a small number of study participants who knew each other; we placed these individuals together in clusters so they would be assigned to either treatment or control together. We then formed blocks of clusters as follows. We formed strata based on race/ethnicity, income group (0-100% FPL, 101-200% FPL, 201-300% FPL), and state; any clusters with more than one individual within them were placed in their own strata. Within these strata, we grouped participants into blocks of three based on how similar they were across several dozen pre-treatment covariates, using Mahalanobis distance to measure similarity. When the number of clusters in a strata did not evenly divide into three, there were either one or two leftover clusters in a strata after the first round of blocking. We then conducted a second round of blocking for these leftover clusters, again forming blocks based on a set of pre-treatment covariates. Within each block of three, we selected one of three observations to be in the treatment group and placed the remaining two in the program control group. Given that the number of clusters did not evenly divide into three, within the final block we sampled from the vector $\{0, 0, 1\}$ without replacement to assign treatment.[2]

Finally, after randomizing, we further ensured balance by conducting a series of balance checks comparing the treatment and control group across pre-treatment covariates. We imposed a p-value floor, with covariates we deemed to be more important assigned a higher floor; these floors were determined ex ante. We rejected any randomization where the p-value on a t-test for difference across treatment arms was below the p-value floor for any of the selected variables and re-randomized, using

[2] We anticipated that some assigned to treatment may refuse the $1000 per month, so we created a randomized waitlist among the control group. However, this did not end up being relevant in practice, as only one participant out of 1,000 ended up not receiving the treatment as assigned.

a procedure similar to the one described in Zhao and Ding (2024). We also conducted an F-test for the joint significance of all of the same set of pre-treatment variables by outcome area and rejected a randomization if the p-value on the F-test was over 0.25.

If there were large outliers in the data, imposing balance in this way may generate a situation where some participants were more likely to be assigned to treatment than others. To examine this, we conducted 1,000 simulations and verified that this procedure resulted in all observations having a 1/3 probability of being assigned to the treatment group. We could not reject that the simulated distribution of treatment assignments was significantly different from what we would observe from a Bernouilli distribution with a one third probability of success. Furthermore, no baseline characteristics predicted the average probability over these 1,000 simulations that any participant received treatment. See the Appendix section of Miller et al. (2025) for more discussion and the results of this simulation.[3]

B Changes to the Pre-Analysis Plan

The activities of recruitment, intervention, and analysis took several years, and over the course of this period we made several small changes to our original pre-analysis plan. These amendments occurred as our understanding about how best to analyze, structure, and present our results evolved. In some cases, changes were made in response to feedback we received from external parties or because the data we planned to use ended up not being available. Note that some of the analysis in this paper is described in the PAP documents associated with mobility, rather than the PAP documents associated with children's outcomes. The following changes were made prior to the receipt of the enumerated midline data, and before any analysis of the data had occurred (note that much of this text also appears in the appendix to Miller et al. (2025)):

- We altered our proposed approach to multiple hypothesis testing. While we originally pre-specified we would calculate the family-wise error rate adjusted p-values, we updated the PAP to propose using tiered false discovery rate q-values.

- We added to the pre-analysis plan that expenditure variables (i.e. dollar denominated variables) would be winsorized due to the likely presence of outliers.

[3]Early in the study, one participant who was randomized into the treatment group was removed from the program by the non-profit partner, but continued to participate in the research activities. Another participant was assigned to treatment but initially declined, and was replaced from the waitlist. However, this participant changed their mind and ended up accepting the treatment. We use the original treatment assignment to calculate treatment effects, with 1001 participants assigned to treatment and 1000 actually receiving the cash transfers. Our estimates are therefore, technically, "intent to treat" (ITT) effects. However, given that the first stage effect of treatment assignment on program participation exceeds 0.999, the local average treatment effects are essentially indistinguishable from the ITT.

Additionally, we made additional changes following the midline survey, although most of these were implemented before we had computed treatment effects.

- Our pre-analysis plan specified that, in pooling items across time, we would impute any time periods for which an item was missing with the treatment group specific mean at that time period, and consider the pooled item as non-missing as long as the outcome was observed for at least one time period. In the current version, we do not perform such an imputation, and instead average over non-missing time periods.

- The pre-analysis plan specified that a robustness check using median regression would be reported for outcomes potentially susceptible to outliers (e.g., those based on expenditures). However, given that this concern affects only very few outcomes, and the fact that we are already reporting a large number of tables, we opted the skip this robustness check in the interest of space.

Additionally, we added the Alabama Parenting Questionnaire to our survey battery in response to the reviewer suggestions from NIH grant R01-HD103699. This was inadvertently left off of the post-midline pre-analysis plan updates, but it was part of the pre-planned analysis described prior to receiving these data in our NIH grant proposal. We also pre-specified that we would obtain the GreatSchools.org ratings for schools. However, we were not able to secure a data use agreement with GreatSchools, so we were unable to use these school quality measures. Additionally, to date, we have been unable to obtain birth certificate data for our sample, so are unable to examine pre-specified measures related to infant health. We pre-specified that we would examine fertility outcomes based on baseline desire to become pregnant, but decided not to do this due to small sample sizes and low rates of childbearing over the three years overall. While we originally planned to report estimates separately based on mobile phone surveys in each survey year *and* by midline and endline enumerated surveys, in order to streamline the presentation of results, we decided to group the midline surveys with survey year 2 and the endline surveys with survey year 3, so that we report results for 3 rather than 5 time periods. Along with this change, we also used the weighting proposed in cases when there are 3 survey years of data (versus when there are 3 survey years and 2 midline/endline periods); i.e., 50% on year 3, 30% for year 2, and 20% for year 1. Finally, we added two heterogeneity analyses that we did not originally pre-specify. First, for outcomes related to fertility, pregnancy, contraception and abortion, we examined outcomes by state, which was not pre-specified. We made this change in light

of the Dobbs Supreme Court decision, which generated large changes in access to certain types of family planning services in Texas vs Illinois. Second, in the early stages of the paper, we received feedback from other researchers and from the ORUS team of qualitative researchers conducting interviews with participants that examining treatment effects by whether the parent was single or married could be important in understanding the effect of the transfer. So, we added this heterogeneity analysis as well.

C Additional Details on Administrative School Record Variable Construction

This section provides additional details on how our primary outcomes were constructed from the TEA and ISBE administrative datasets.

- **Enrollment**. We used the state administrative measures to assess whether a student was enrolled in a K-12 public school in each study year, whether the student left the data to be home schooled or dropped out, whether the student repeated a grade, and whether the student was old for the grade in which they were enrolled.

- **Attendance**. Attendance rates were taken as the fraction of days in attendance divided by the total number of potential school days for a child observed in the data in a given school year. In a small number of cases, students had overlapping enrollment periods at multiple schools. If a student appeared to have overlapping enrollment in two schools within the same month, we re-scaled the number of days attended by the amount of enrollment in each school. For example, if a student was enrolled in School 1 for 15 days, and School 2 for 30 days, in a month, we would construct the number of days attended as (15/45)*[School 1 days] + (30/45)[School 2 days].

- **Standardized test scores**. Standardized tests differed across grades and across states. Because grading scales were different, and we were not able to calculate student percentiles in both states, we instead created indicators that students met or mastered the material in the subjects of English/language arts and math. We were not able to construct similar indicators for other subjects (like science) because they are only administered in select grades and in both states, leaving sample sizes very small. In Illinois, we observe the Illinois Assessment of Readiness (IAR) in grades 3-8 and the SAT (with scores broken down by Reading/Writing and Math) in grades 11 and 12. Note that Illinois requires grade 11 students to take the SAT. Some grade 12 students also must take the SAT if they did not take it previously or if they did not earn a qualifying score. From

these tests, we are able to observe whether the student meets expectations or masters the subject in math and language arts. Illinois students are also required to take a grade-specific version of the PSAT in grades 9 and 10. Although the PSAT does not report separate scores for english and math, we consider the student to "meet expectations" or "master the subject" for both English and math if their PSAT score falls within the meets expectation or masters subject range. Note that this means, for 9th and 10th grade Illinois students, "meets expectations" and "masters material" will be identical across the English and math variables. In Texas, the TEA administers the State of Texas Assessments of Academic Readiness (STAAR) grade level exams in grades 3-8, and end of course exams for high-school students after completing algebra 1 and english 1 . From these exams, we construct indicators of students meeting expectations in English/math and mastering the material in English/math. Note that the STAAR exam was redesigned for the 2022/2023 school year; however, we still are able to construct these indicators.

- **School quality data**. Data on different measures of school quality was obtained from multiple sources. In Illinois, we used data from the Illinois School Report Card (https://www.illinoisreportcard.com/). In Texas, we used data from the Texas Academic Performance Report (TAPR) provided by the Texas Education Agency (https://rptsvr1.tea.texas.gov/perfreport/tapr/2019/download.html)

- **Grades (IL only).** Classes for students in grades K-12 were assessed on primarily four grading scales: letter grades, above average/average/below average, and exceptional/ meets standard/ approaching standard/ below standard, and satisfactory (or pass)/unsatisfactory (fail). We converted these scales into indicators that the student passed the course, met expectations, or mastered the material. Appendix Table A39 describes how we assigned these indicators. In some cases, students were given a missing (indicated as "NA" in the table); for example, students were assigned a missing for their course grade if they withdrew from the course, audited the course and did not receive course credit, or if the course was noted as being recorded in error. In a small number of cases, the student has two grades reported for the same course (the most frequent case is when a student receives a grade but has also been marked as having withdrawn from the course). In these cases, we use the highest grade that generates a non-missing value for the course. To form a continuous measure of course performance, and to take advantage of differences in grades within the passing, meeting, and mastering categories, we also generate

a course score for each course ranging from 62.5 to 99. After generating passing, meeting, and mastering indicators, and imputed scores, for each course, we collapse the grades to the student by academic year level, weighting each course by the number of credit hours assigned to the course. Students graded using the satisfactory (pass) / unsatisfactory (fail) system do not receive scores for met expectations, mastered the material, or continuous score grades. The use of this grading system is correlated with grade level, with lower grade levels—particularly in elementary school—more likely to use it.

- **COVID-19 school data** Data on the learning modes by school was obtained from the COVID-19 School Data Hub. 2023. "All School Learning Model Data". *Data Resources* (Version 3/8/23). Accessed at https://www.covidschooldatahub.com/data-resourceson 6/1/2025.

D Additional Details on Neighborhood Quality Measures

This section provides additional details on how our primary neighborhood quality outcomes were created

Data Sources

- **Family Friendliness**. We use tract-level variables from the 2015–2019 American Community Survey (ACS) 5-Year Estimates, obtained via the National Historical Geographic Information System (NHGIS), a project of IPUMS at the University of Minnesota (https://www.nhgis.org/).

- **Child-Focused Amenities** To identify the locations and types of commercial establishments and public services in each area, we use SafeGraph Places of Interest (POI) data, licensed for academic research via Dewey Data SafeGraph (2022). SafeGraph aggregates geospatial information on millions of points of interest across the United States, including location name, North American Industry Classification System (NAICS) category, latitude, and longitude. The data used in our analysis is a snapshot downloaded from Dewey Data on January 15, 2024. We utilize NAICS codes to identify locations in our categories of interest: daycares, schools, parks, and libraries. To generate distance-decayed counts of nearby amenities, we first identify all places marked as open within a 1-mile radius (using the WGS84 ellipsoid) of each geocoded participant address. We then apply an exponential decay function following: $\sum e^{-d/\lambda}$ where d is the distance in miles and $\lambda = 1.5$ is the decay parameter following (Heroy et al., 2023) .

- **Economic Mobility** We use the *Household Income and Incarceration for Children from Low-Income Households* by Census Tract, obtained from the Opportunity Insights data portal (opportunityinsights.org/data/). These data are tract-level estimates of predicted mean household income (at ages 31–37) and incarceration rates for children born between 1978–1983 whose parents were at the 25th percentile of the national income distribution. Statistical noise is added to the estimates to preserve confidentiality. For our outcomes we use the overall estimates not disaggregated by race or gender.

- **Pollution** We utilize the Daily Census Tract–Level PM2.5 Concentrations (2016–2020) dataset from the Centers for Disease Control and Prevention's National Environmental Public Health Tracking Network. These data are modeled predictions of daily fine particulate matter (PM2.5) at the census tract level, based on EPA's downscaler framework .We aggregated the daily PM2.5 estimates by taking the median for each census tract across the period from January 1, 2016 to December 31, 2020. This produced one summary value per tract representing typical daily exposure. https://data.cdc.gov/Environmental-Health-Toxicology/Daily-Census-Tract-Level-PM2-5-Concentrations-2016/96sd-hxdt/about_data We also use census tract–level data from the Risk-Screening Environmental Indicators (RSEI) model, produced by the U.S. Environmental Protection Agency (EPA), covering the years 2019–2021. RSEI scores provide a measure of potential chronic human health risk associated with toxic chemical releases. These scores integrate data on estimated chemical doses, toxicity weights, exposure routes (e.g., inhalation or ingestion), and population size to produce risk-weighted values for each geographic area (https://www.epa.gov/rsei). For analysis, we first calculate the average RSEI score across the 2019–2021 period for each tract using 2010 census boundaries. To reduce the influence of outliers, we apply a $\log(1 + x)$ transformation.

- **Neighborhood Quality Indices** We utilize two composite indices examine more holistic measures of neighborhood quality for children: The Child Opportunity Index (COI) 3.0, developed by the Institute for Child, Youth and Family Policy at the Boston University School of Social Work and made available via (diversitydatakids.org), and the Area Deprivation Index (ADI), developed by the Center for Health Disparities Research at the University of Wisconsin School of Medicine and Public Health, and disseminated via the Neighborhood Atlas (https://www.neighborhoodatlas.medicine.wisc.edu/

COI(3.0) is a nationally normed index measuring neighborhood environments that promote healthy child development, available at the census tract level (using 2010 Census boundaries). It incorporates 29 indicators across three domains:Educational, heath and environment, and social and economic opportunity.Each indicator is standardized and combined to produce domain-specific z-scores and an overall composite z-score, with higher values indicating more favorable neighborhood conditions for children. In our analyses, we use the overall COI z-score.

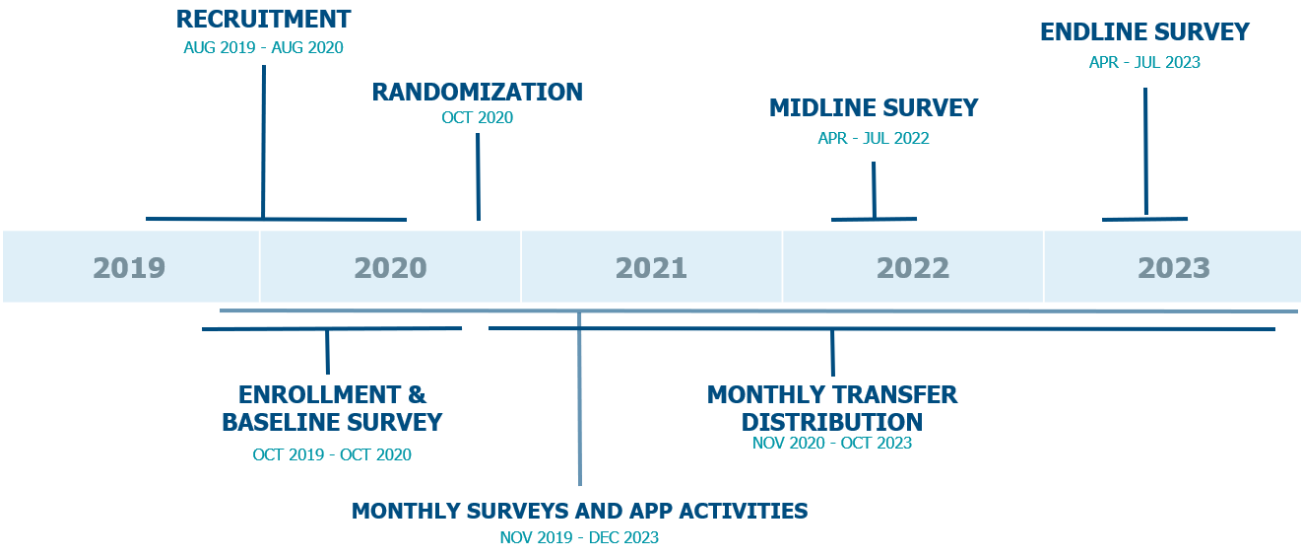
The ADI is a composite measure of neighborhood disadvantage based on 2018–2022 American Community Survey (ACS) 5-Year Estimates. It is provided at the 2020 Census Block Group level, which we use for linking to participants' geocoded addresses. The ADI combines 17 variables related to four domains of deprivation—income, education, employment, and housing quality—to produce a summary index that ranks neighborhoods by relative disadvantage. These variables include metrics such as percent of residents below the federal poverty line, median household income, percent with less than a high school education, unemployment rates, percent of households without a vehicle, and housing crowding. For our estimates we use the block group's national percentile ranking of disadvantage. Values range from 1 (least disadvantaged) to 100 (most disadvantaged), with higher values indicating greater relative deprivation.

Construction of Yearly Measures

For our analyses, we construct yearly measures of neighborhood "exposure" for all neighborhood quality outcomes described above. Each enumerated or online survey begins by asking participants to confirm their current contact information, including address. If a participant reports having moved, they are prompted to provide their new address. We also incorporate any address updates provided through our implementation partners or reported directly to the research team, allowing us to construct a longitudinal address panel for each participant over the study period

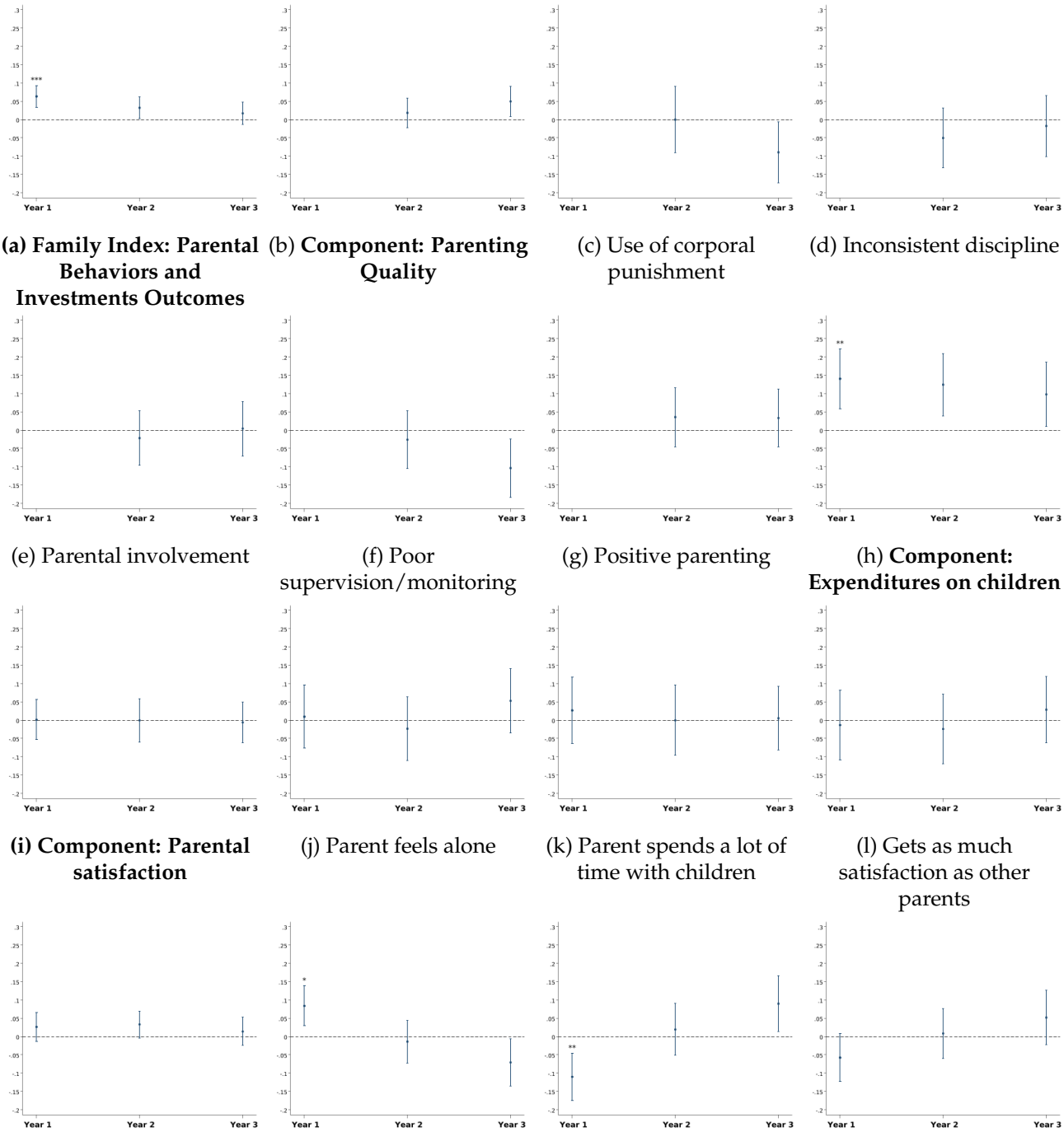
We assume that participants remain at a given address until they report a new one. We then match the external neighborhood quality measures to these geocoded addresses, using either tract or block group boundaries, depending on the outcome. To calculate annual exposure, we compute a weighted average of all oucomes for each participant, where weights are proportional to the number of days spent at each address during the study year.

Figure A1: Timeline of Recruitment, Enrollment, Treatment, and Research Activities



Note: Figure displays a timeline reporting the period of recruitment, enrollment of participants, cash disbursements, and research activities. See text for more details.

Figure A2: Standardized Effects on Parental Behaviors and Investments by Year



(a) Family Index: Parental Behaviors and Investments Outcomes

(b) **Component: Parenting Quality**

(c) Use of corporal punishment

(d) Inconsistent discipline

(e) Parental involvement

(f) Poor supervision/monitoring

(g) Positive parenting

(h) **Component: Expenditures on children**

(i) Component: Parental satisfaction

(j) Parent feels alone

(k) Parent spends a lot of time with children

(l) Gets as much satisfaction as other parents

(m) **Component: Parental interactions**

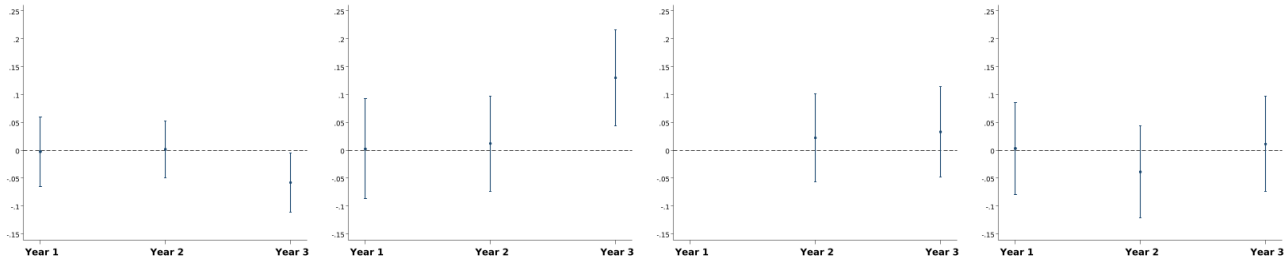
(n) Component: Parental distress and anxiety

(o) Perceived stress scale

(p) K6 (mental distress) scale

Notes: Figures show estimated treatment effects for each time period for which we have data. Treatment effects are standardized by the control group mean to facilitate comparison. 95% confidence intervals are included. The symbol $*$ indicates significance levels after adjusting p-values to control the false discovery rate. See text for more details.

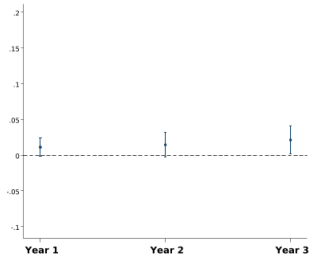
Figure A3: Standardized Effects on the Home Environment



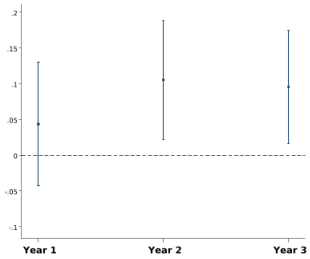
(a) Family Index: Home Environment (b) Child food insecurity scale (c) CHAOS score (d) Ever unhoused

Notes: Figures show estimated treatment effects for each time period for which we have data. Treatment effects are standardized by the control group mean to facilitate comparison. 95% confidence intervals are included. See text for more details.

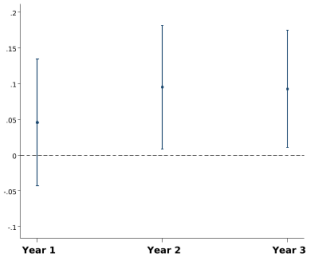
Figure A4: Standardized Effects on the Neighborhood Environment



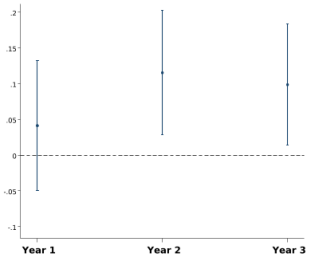
(a) Family Index: Neighborhood Environment



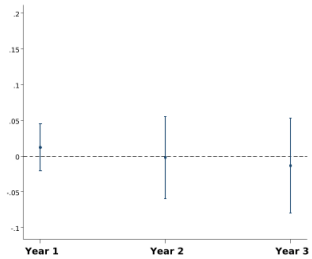
(b) **Component: Moving behavior**



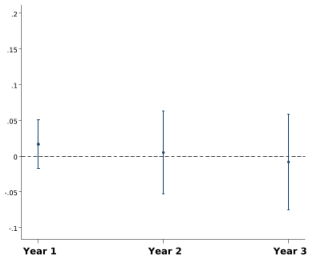
(c) Moved units



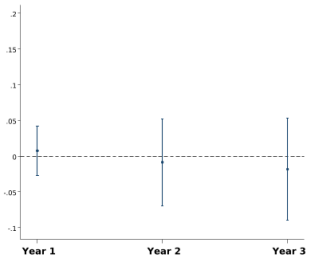
(d) Moved neighborhoods



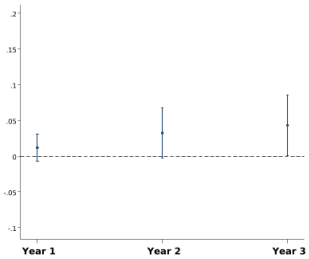
(e) **Component: Family friendliness**



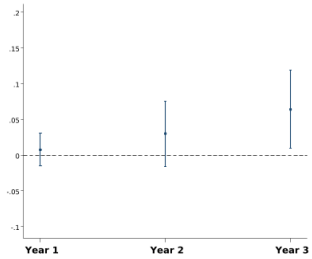
(f) Share of households with children



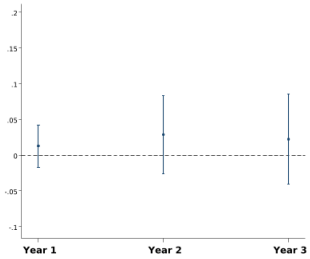
(g) Share of population who are children



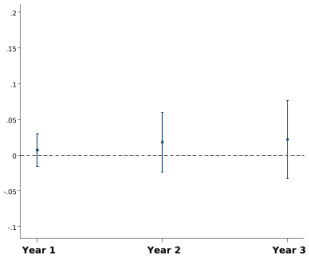
(h) **Component: child-focused amenities**



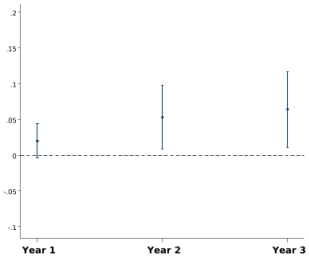
(i) Count of daycares



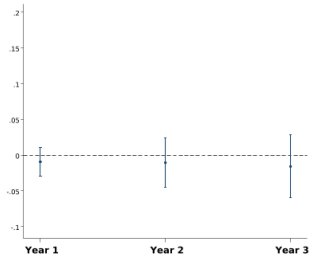
(j) Count of libraries



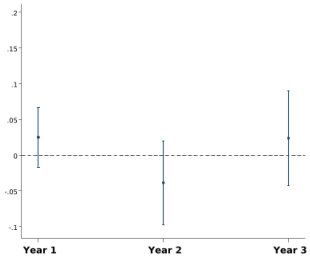
(k) Count of parks



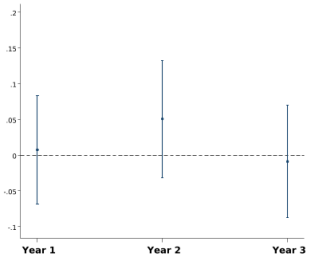
(l) Count of schools



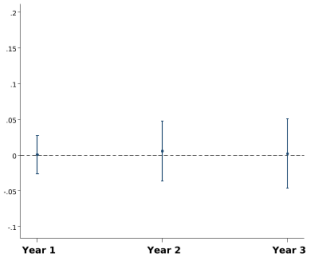
(m) **Component: Economic mobility**



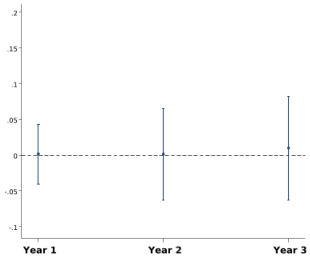
(n) Child opportunity index



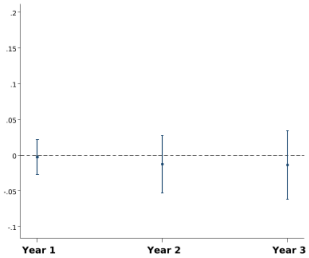
(o) Area deprivation index



(p) **Component: Pollution**



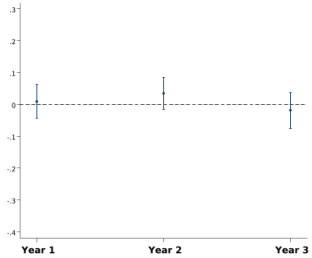
(q) Particulate matter 2.5



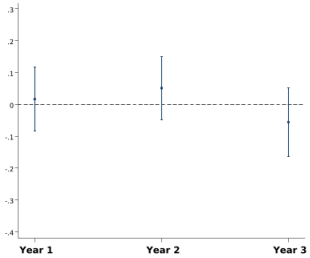
(r) Risk-screening Environmental Indicators

Notes: Figures show estimated treatment effects for each time period for which we have data. Treatment effects are standardized by the control group mean to facilitate comparison. 95% confidence intervals are included. See text for more details.

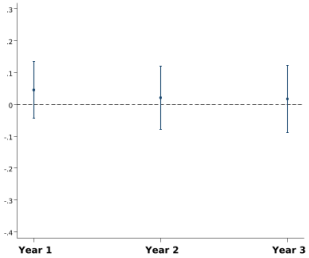
Figure A5: Standardized Effects on Non-Parental Care



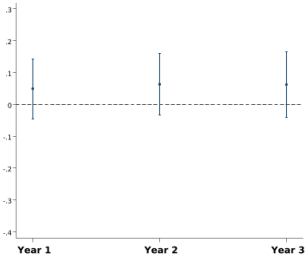
(a) Family Index: Non-Parental Care



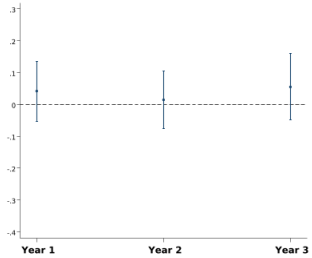
(b) **Component: Number of hours per week children are in care**



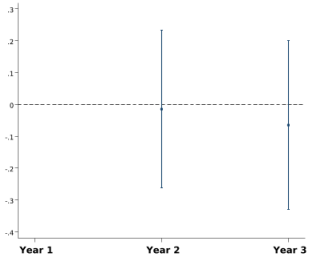
(c) **Component: Quality of childcare**



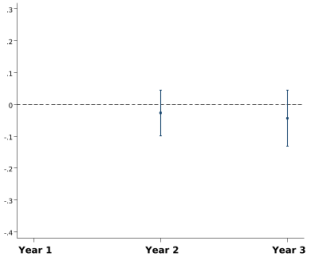
(d) Changed to higher quality childcare arrangement



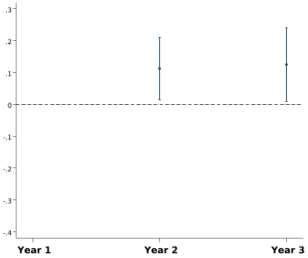
(e) Changed to more reliable childcare arrangement



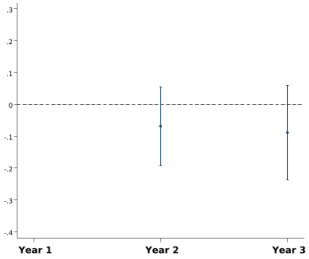
(f) Satisfied with current childcare situation



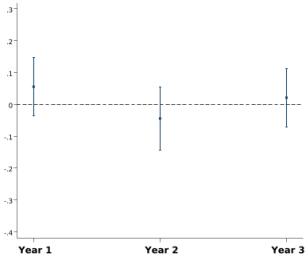
(g) **Component: Stability of childcare**



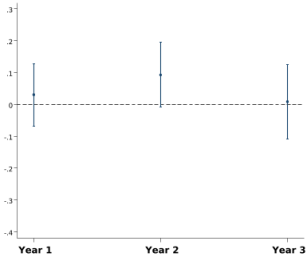
(h) Number of childcare arrangements changes



(i) # of childcare providers/arrangements



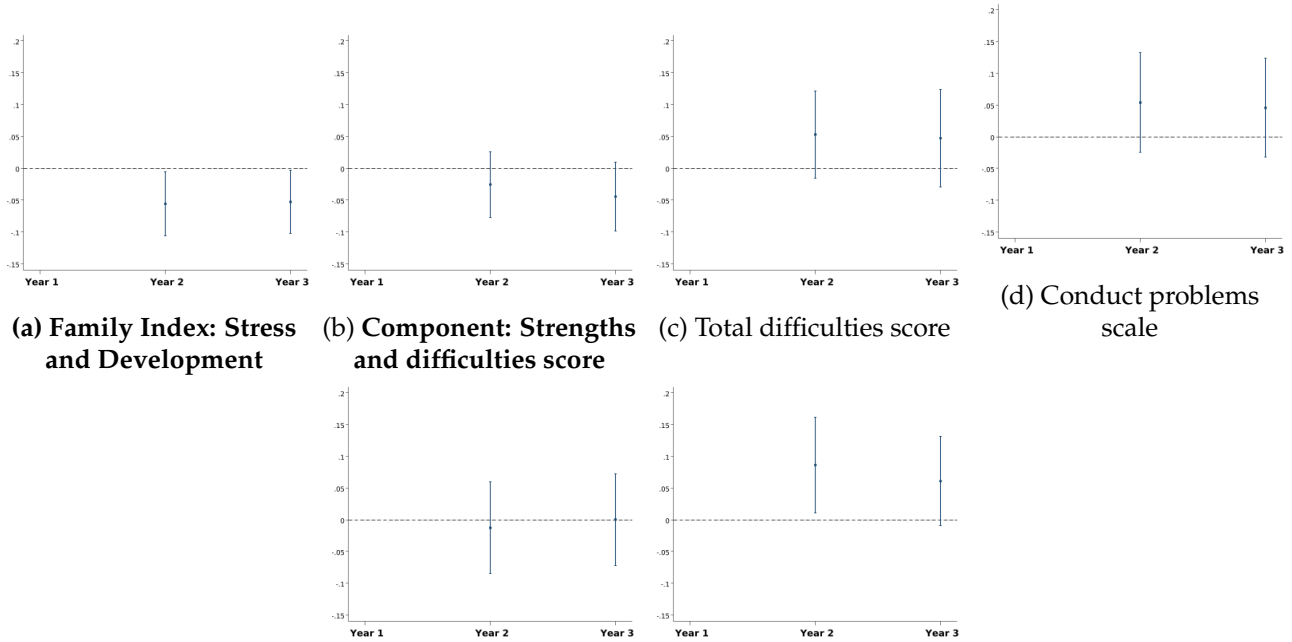
(j) # times had to make special arrangements because childcare fell through



(k) **Component: Participant currently uses childcare**

Notes: Figures show estimated treatment effects for each time period for which we have data. Treatment effects are standardized by the control group mean to facilitate comparison. 95% confidence intervals are included. See text for more details.

Figure A6: Standardized Effects on Child Stress and Development



(a) Family Index: Stress and Development

(b) **Component: Strengths and difficulties score**

(c) Total difficulties score

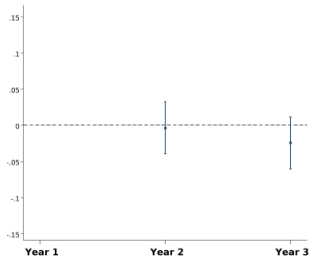
(d) Conduct problems scale

(e) Emotional problems scale

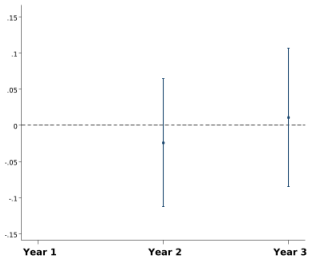
(f) **Component: PROMIS mental health score**

Notes: Figures show estimated treatment effects for each time period for which we have data. Treatment effects are standardized by the control group mean to facilitate comparison. 95% confidence intervals are included. See text for more details.

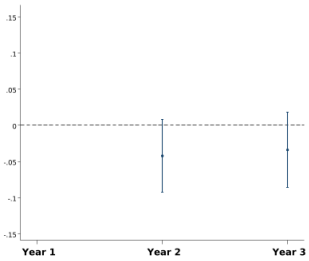
Figure A7: Standardized Effects on Child Educational Outcomes (Survey-Based)



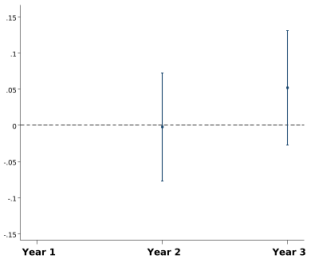
(a) Family Index: K-12 Education Outcomes



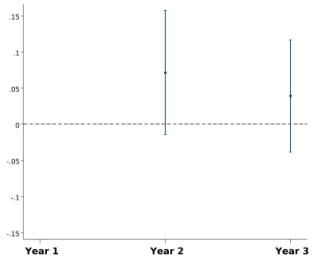
(b) **Component: Days absent**



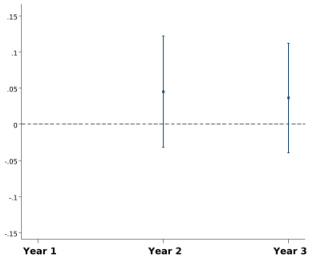
(c) **Component: Disciplinary action and help needed**



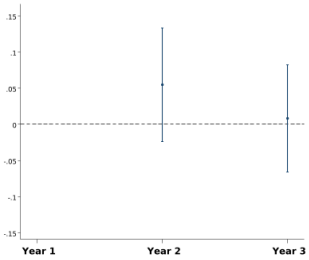
(d) Child got help for behavioral/emotional problems



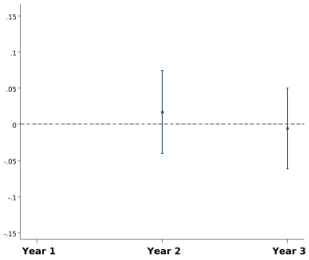
(e) Asked to come in to talk about child's schoolwork/behavioral problems



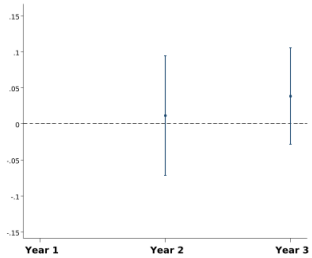
(f) Child got special help at school for learning problems



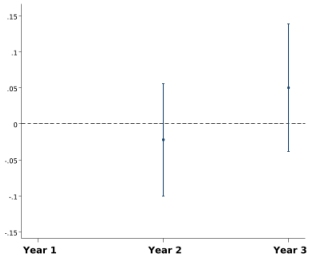
(g) Child suspending or expelled



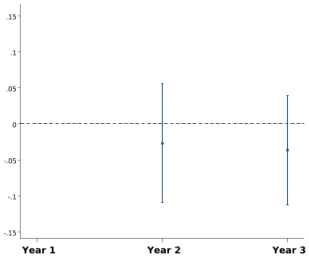
(h) **Component: School Enrollment**



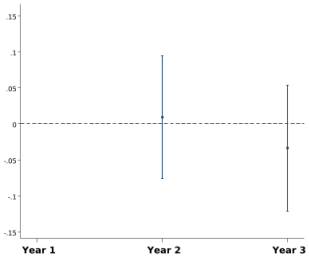
(i) Child currently enrolled



(j) Child ever repeated a grade



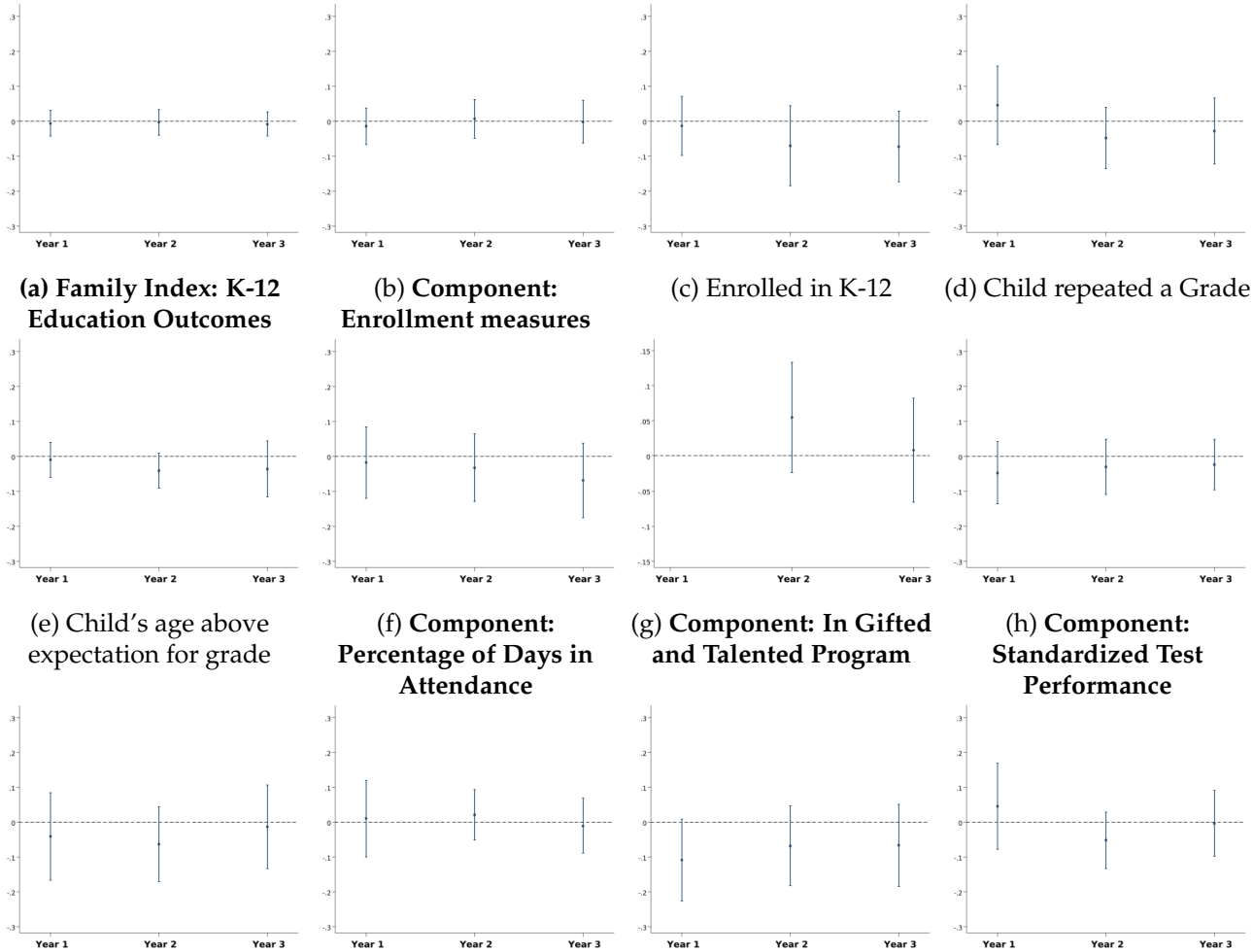
(k) Parents' report of child's grades



(l) Parent's perception of child's school's quality

Notes: Figures show estimated treatment effects for each time period for which we have data. Treatment effects are standardized by the control group mean to facilitate comparison. 95% confidence intervals are included. See text for more details.

Figure A8: Standardized Effects on Child Educational Outcomes (Administrative Records)



(a) Family Index: K-12 Education Outcomes

(b) **Component: Enrollment measures**

(c) Enrolled in K-12

(d) Child repeated a Grade

(e) Child's age above expectation for grade

(f) **Component: Percentage of Days in Attendance**

(g) **Component: In Gifted and Talented Program**

(h) **Component: Standardized Test Performance**

(i) English/Language Arts Meets Expectations

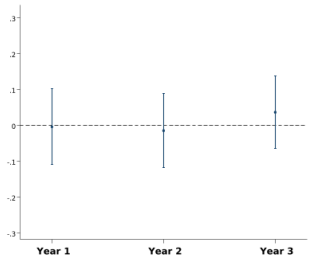
(j) English/Language Arts Masters Material

(k) Math Meets Expectations

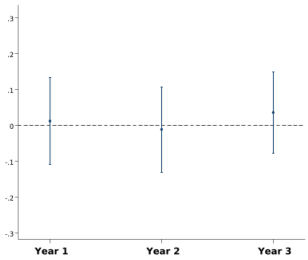
(l) Math Masters Material

Notes: Figures show estimated treatment effects for each time period for which we have data. Treatment effects are standardized by the control group mean to facilitate comparison. 95% confidence intervals are included. The symbol $*$ indicates significance levels after adjusting p-values to control the false discovery rate. See text for more details.

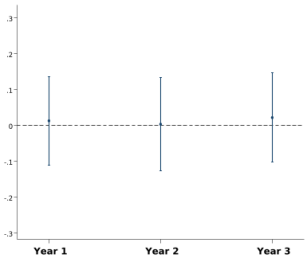
Figure A9: Standardized Effects on Child Educational Outcomes (Administrative Records) (cont)



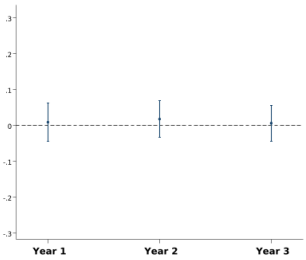
(m) Component: Educational mobility



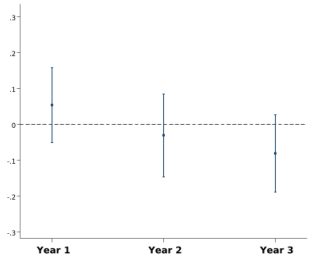
(n) Changed schools



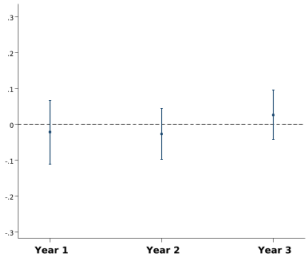
(o) Changed school district



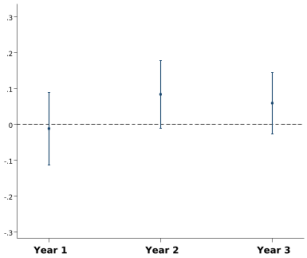
(p) **Component: School quality**



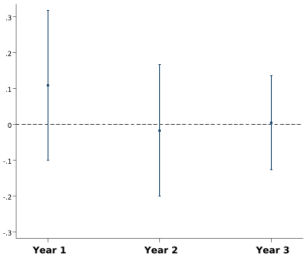
(q) Average class size



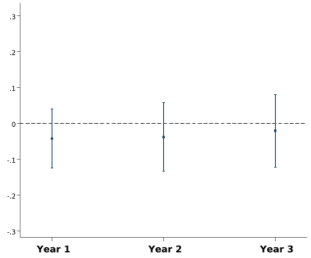
(r) Average teacher salary



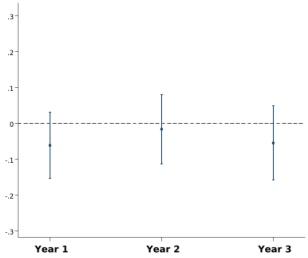
(s) Average attendance rate



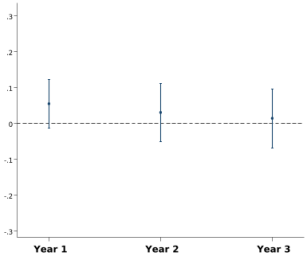
(t) Graduation rate



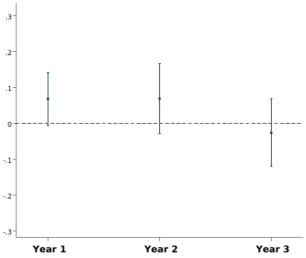
(u) Percent of students proficient in English/Language Arts



(v) Percent of students proficient in Math



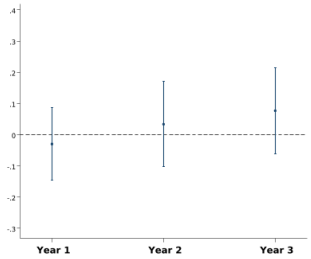
(w) Value-added estimate English test scores



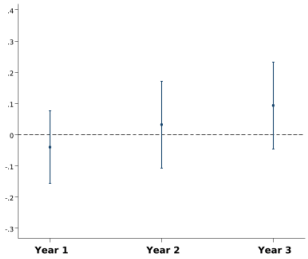
(x) Value-added estimate math test scores

Notes: Figures show estimated treatment effects for each time period for which we have data. Treatment effects are standardized by the control group mean to facilitate comparison. 95% confidence intervals are included. The symbol $*$ indicates significance levels after adjusting p-values to control the false discovery rate. See text for more details.

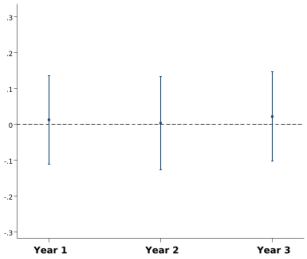
Figure A10: Standardized Effects on Child Educational Outcomes (Administrative Records) - Texas Only Outcomes



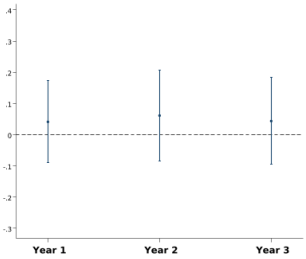
(a) Any Disciplinary Action



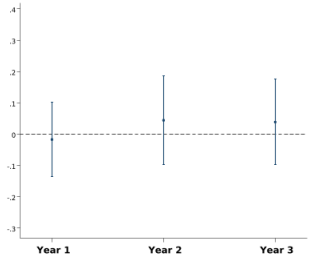
(b) Any Suspension



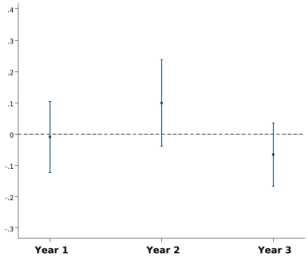
(c) Number of days of suspension served



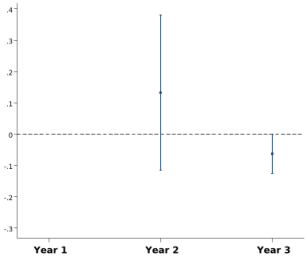
(d) Any in school suspension



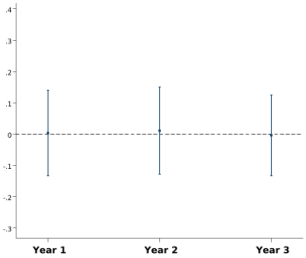
(e) Any out of school suspension



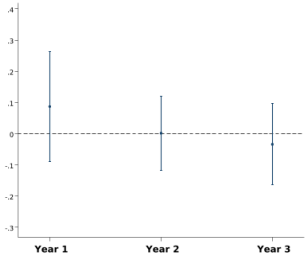
(f) Disciplinary alternative education program



(g) Suspended due to felony



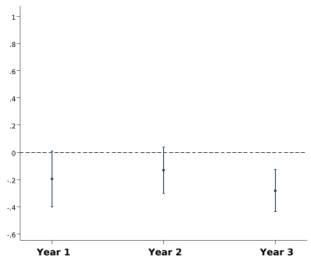
(h) Suspended for drug/alcohol/tobacco reasons



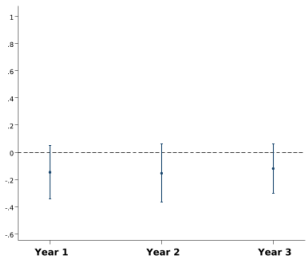
(i) Suspended because of physical violence

Notes: Figures show estimated treatment effects for each time period for which we have data. Treatment effects are standardized by the control group mean to facilitate comparison. 95% confidence intervals are included. See text for more details.

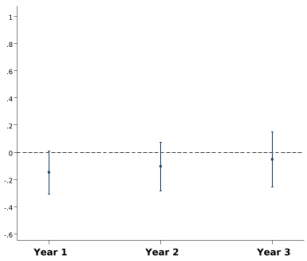
Figure A11: Standardized Effects on Child Educational Outcomes (Administrative Records) - Illinois Only Outcomes



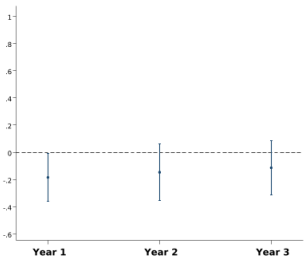
(a) Percent of courses passed



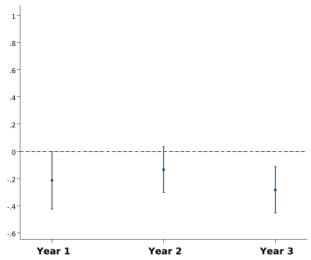
(b) Percent of courses - meets expectation



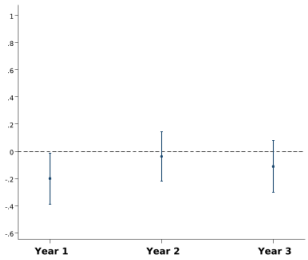
(c) Percent of courses - masters material



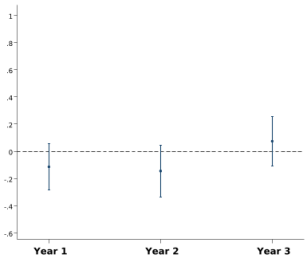
(d) Imputed average course score



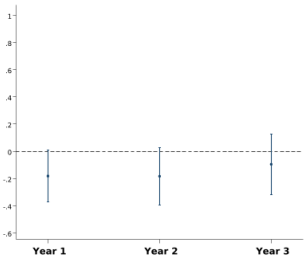
(e) Percent of core courses passed



(f) Percent of core courses - meets expectations



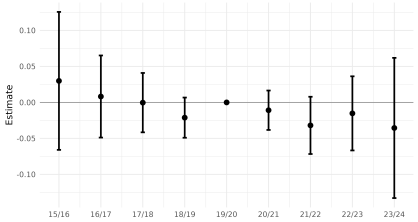
(g) Percent of core courses - masters material



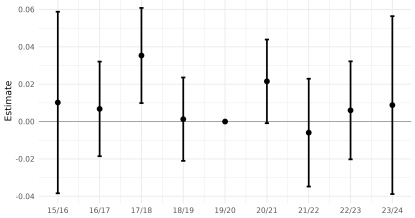
(h) Imputed average core course score

Notes: Figures show estimated treatment effects for each time period for which we have data. Treatment effects are standardized by the control group mean to facilitate comparison. 95% confidence intervals are included. See text for more details.

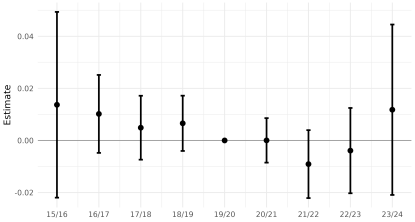
Figure A12: Event Study Estimates, K-12 Administrative Outcomes (Both States)



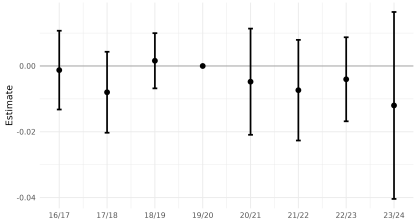
(a) Enrolled in a K-12 School



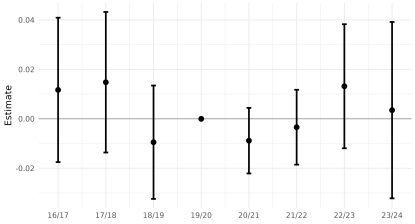
(b) Repeated a grade



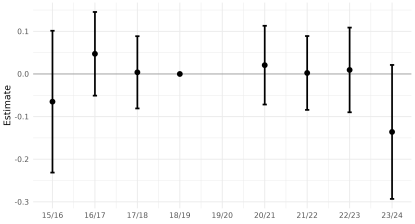
(c) Child older than expected for grade



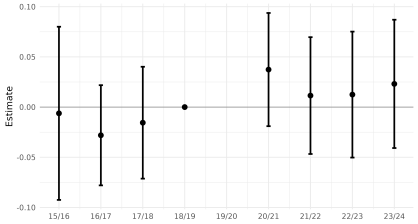
(d) Percentage of days in attendance



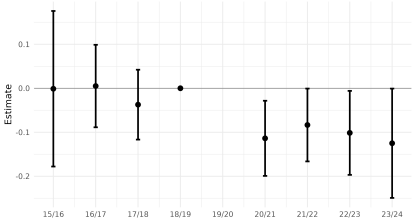
(e) Participating in gifted and talented program



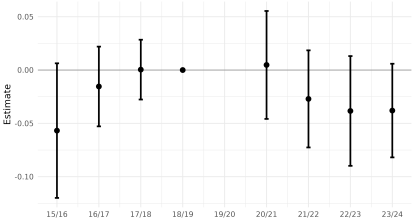
(f) English/language arts standardized test score meets expectations



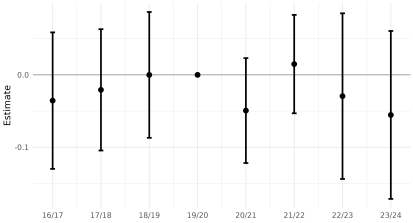
(g) English/language arts standardized test score shows subject mastery



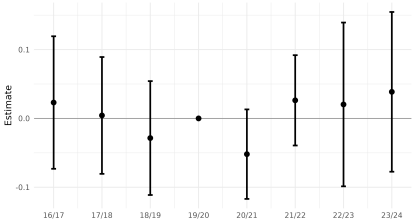
(h) Math standardized test score meets expectations



(i) Math standardized test score shows subject mastery



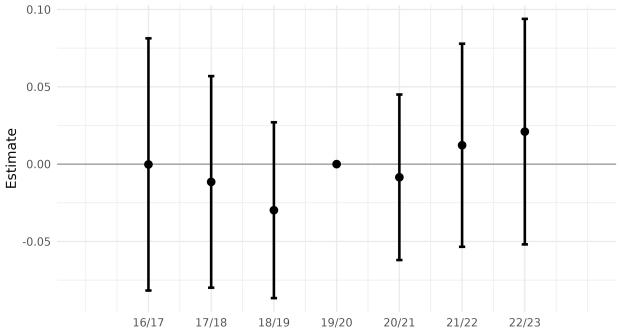
(j) Changed schools



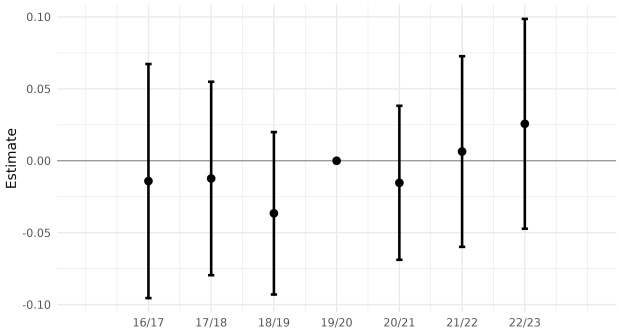
(k) Changed school districts

Notes: Figures show event study estimates for each time period for which we have data. Estimates are based on both TX and IL records for all years except 2016/2017 and 2023/2024 which use only IL data. 95% confidence intervals are included. See text for more details.

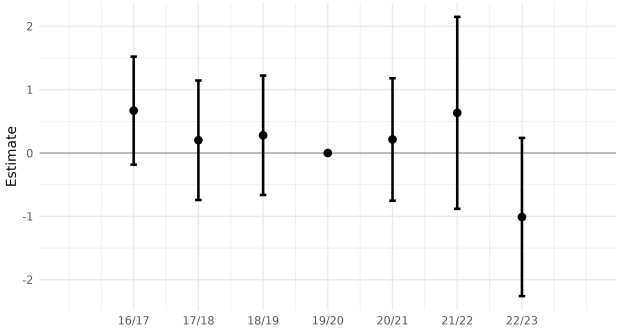
Figure A13: Event Study Estimates, K-12 Administrative Outcomes (TX Only Outcomes)



(a) Any Disciplinary Action



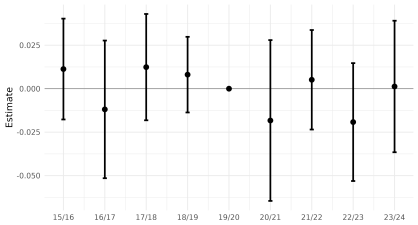
(b) Any Suspension



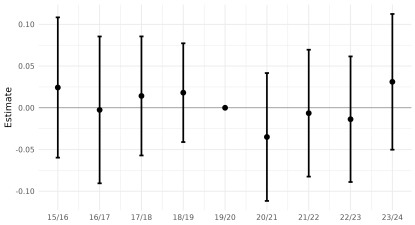
(c) Number of days suspended

Notes: Figures show event study estimates for each time period for which we have data. Estimates are based on both TX administrative records. 95% confidence intervals are included. See text for more details.

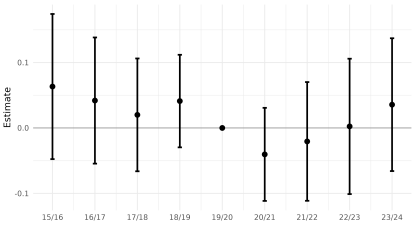
Figure A14: Event Study Estimates, K-12 Administrative Outcomes (IL Only Outcomes)



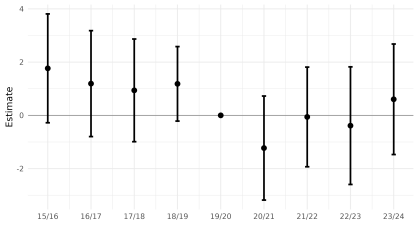
(a) Percent of courses passed



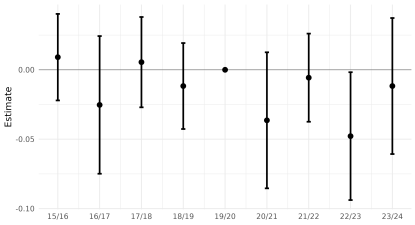
(b) Percent of courses meets expectations



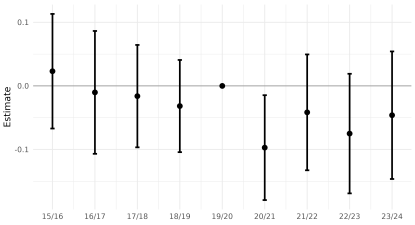
(c) Percent of courses with mastery



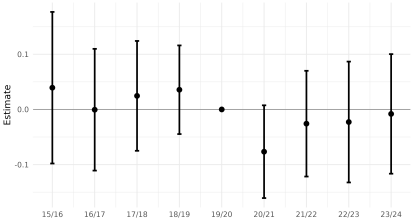
(d) Imputed average course score



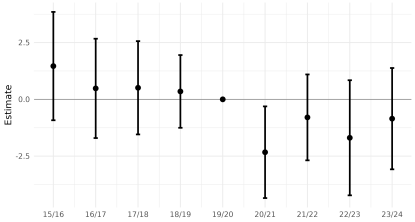
(e) Percent of core (English, math, science) courses passed



(f) Percent of core (English, math, science) courses meets expectations



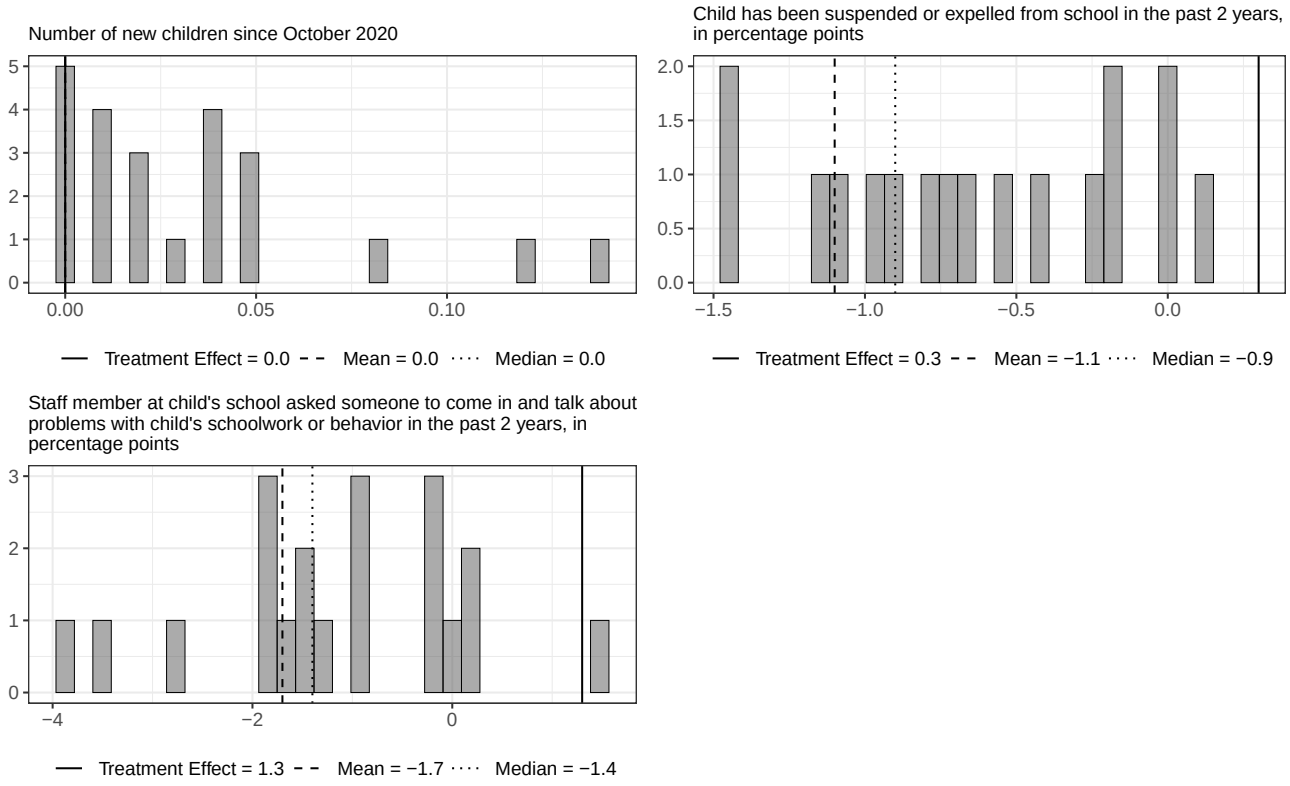
(g) Percent of core (English, math, science) courses mastery



(h) Imputed average core course score

Notes: Figures show event study estimates for each time period for which we have data. Estimates are based on both IL administrative records. 95% confidence intervals are included. See text for more details.

Figure A15: Expert predictions of of treatment effects



Notes: This figure shows histograms of effects as predicted by NBER members in a survey. The estimated year 3 treatment effect is plotted as a solid vertical line.

Table A1: Impact of ORUS program payments on public benefits

Benefit	Illinois	Texas
Medicaid	Eligibility was not affected	Eligibility was not affected
SNAP	Eligibility was not affected	First $300 per quarter did not affect SNAP, but the remaining amount of the transfer was considered unearned income for the purposes of determining eligibility and the amount of the benefit
TANF	Eligibility was not affected	First $300 per quarter did not affect TANF, but the remaining amount of the transfer was considered unearned income for the purposes of determining eligibility and the amount of the benefit
Housing Assistance	Did not affect eligibility for Chicago Housing Authority, eligibility was affected for other localities	Eligibility was affected by the cash transfer.
SSI	Not eligible to participate	Not eligible to participate

Notes: Table describes how ORUS program payments affected participants' eligibility for other public programs.

Table A2: Treatment control balance among parent respondents - monthly Qualtrics surveys

	Survey Year 1			Survey Year 2			Survey Year 3		
	Control	Treatment	p-value	Control	Treatment	p-value	Control	Treatment	p-value
Age	31.278	31.420	0.596	31.293	31.349	0.835	31.282	31.406	0.646
Any Children	0.860	0.879	0.235	0.862	0.879	0.300	0.863	0.880	0.293
Employed	0.568	0.552	0.508	0.568	0.550	0.467	0.567	0.556	0.653
Female	0.764	0.776	0.567	0.765	0.777	0.559	0.773	0.770	0.886
HS Degree/GED or higher	0.919	0.942	0.049	0.920	0.943	0.059	0.919	0.946	0.023
Hispanic	0.232	0.242	0.637	0.231	0.246	0.486	0.226	0.242	0.464
Household Income (1000*s*)	33.100	33.216	0.906	33.190	33.343	0.877	33.066	33.185	0.905
Household Size	3.704	3.718	0.853	3.722	3.718	0.961	3.725	3.714	0.885
Male	0.236	0.223	0.515	0.235	0.222	0.507	0.227	0.228	0.947
Non-Hispanic Asian	0.024	0.024	0.974	0.024	0.024	0.965	0.025	0.024	0.901
Non-Hispanic Black	0.336	0.331	0.847	0.333	0.331	0.907	0.338	0.331	0.763
Non-Hispanic Native American	0.025	0.024	0.890	0.025	0.022	0.711	0.024	0.022	0.813
Non-Hispanic White	0.428	0.433	0.815	0.430	0.431	0.945	0.432	0.434	0.929
Personal Income (1000*s*)	22.419	22.641	0.835	22.443	22.604	0.881	22.416	22.842	0.694
Under FPL	0.356	0.340	0.502	0.355	0.338	0.459	0.360	0.340	0.401
# Children	2.104	2.235	0.073	2.106	2.233	0.085	2.121	2.230	0.138

Notes: This table shows the baseline characteristics of participants who were parents and who responded to at least one Qualtrics survey in each study year, by treatment assignment status. The p-value is associated with the test of equality of means across the treatment and control arms within the sample of respondents.

Table A3: Treatment control balance among parent respondents - enumerated Midline/Endline surveys

	Enumerated Midline			Enumerated Endline		
	Control	Treatment	p-value	Control	Treatment	p-value
Age	31.288	31.345	0.831	31.209	31.342	0.622
Any Children	0.863	0.879	0.328	0.863	0.881	0.268
Employed	0.570	0.563	0.768	0.567	0.564	0.915
Female	0.765	0.767	0.959	0.773	0.767	0.786
HS Degree/GED or higher	0.919	0.943	0.043	0.920	0.943	0.052
Hispanic	0.231	0.242	0.594	0.227	0.243	0.458
Household Income (1000s)	33.123	33.553	0.662	33.085	33.468	0.698
Household Size	3.711	3.716	0.952	3.723	3.715	0.920
Male	0.235	0.232	0.898	0.227	0.231	0.845
Non-Hispanic Asian	0.024	0.024	0.936	0.025	0.024	0.914
Non-Hispanic Black	0.338	0.333	0.837	0.339	0.332	0.739
Non-Hispanic Native American	0.025	0.021	0.524	0.024	0.024	0.997
Non-Hispanic White	0.426	0.431	0.819	0.428	0.433	0.811
Personal Income (1000s)	22.507	22.979	0.660	22.470	22.937	0.664
Under FPL	0.355	0.333	0.347	0.359	0.334	0.299
# Children	2.112	2.219	0.145	2.122	2.209	0.235

Notes: This table shows the baseline characteristics of participants who were parents and who responded to the enumerated midline and endline survey, by treatment assignment status. The p-value is associated with the test of equality of means across the treatment and control arms within the sample of respondents.

Table A4: Baseline characteristics of participants whose child was vs was not successfully linked to the administrative school records databases, by state

	Matched Observations	Unmatched Observations	P-value
Illinois Sample			
Parent Employed	0.584	0.577	0.842
Parent Married at Baseline	0.315	0.368	0.151
Parent's Personal Income (in 1000s)	24.431	24.349	0.952
Number of Children in HH	2.758	2.550	0.141
Parent Non-Hispanic White	0.522	0.480	0.279
Parent Hispanic	0.234	0.215	0.546
Parent Non-Hispanic Black	0.341	0.371	0.414
Average Age of Children in HH at Baseline	7.839	7.449	0.157
Texas Sample			
Parent Employed	0.568	0.565	0.952
Parent Married at Baseline	0.399	0.494	0.130
Parent's Personal Income (in 1000s)	18.472	21.456	0.253
Number of Children in HH	2.977	3.229	0.293
Parent Non-Hispanic White	0.542	0.568	0.667
Parent Hispanic	0.326	0.284	0.418
Parent Non-Hispanic Black	0.412	0.355	0.320
Average Age of Children in HH at Baseline	8.627	6.718	0.000

Notes: This table shows baseline characteristics of ORUS participants whose child was versus was not matched to state school records. The p-value is associated with the test of equality of means across the two groups.

Table A5: Baseline characteristics of participants whose child was vs was not successfully linked to the administrative school records databases, by state

	Control Mean	Treatment Mean	P-value
Illinois Sample			
Number of days absent from school	11.150	11.308	0.827
Chronic Absenteeism	0.322	0.328	0.857
Enrolled in Public School	0.939	0.932	0.710
Enrolled in K-12 school	0.939	0.936	0.864
Child repeated grade	0.077	0.062	0.307
Child is home schooled	0.006	0.005	0.760
Child's age above expectation for grade	0.028	0.032	0.711
Percentage of Days in attendance	0.924	0.920	0.466
Gifted and Talented Program	0.008	0.011	0.748
English / Language Arts Meets Expectations	0.244	0.268	0.235
English / Language Arts Masters Expectations	0.026	0.028	0.802
Math Meets Expectations	0.201	0.243	0.029
Math Masters Expectations	0.027	0.032	0.443
School Characteristics			
Class Size	22.119	22.525	0.250
Average Teacher Salary	6.5e+04	6.4e+04	0.462
Peccent Students Chronic Absenteeism	0.186	0.194	0.366
Average Attendance Rate	0.937	0.938	0.848
Graduation Rate	0.830	0.833	0.175
Percent Proficient in English Language Arts	0.276	0.294	0.116
Percent Proficient in Math	0.232	0.252	0.079
Value Added Estimate Math Scores	1.806	1.865	0.314
Value added Estimate English Scores	2.881	2.867	0.883
IL-Specific Measures			
Percent of courses passed	0.976	0.966	0.204
Percent of courses meets expectations	0.855	0.855	0.998
Percent of courses with mastery	0.388	0.390	0.916
Percent of core courses passed	0.969	0.960	0.311
Percent of core courses meets expectations	0.797	0.812	0.395
Percent of core courses with mastery	0.288	0.300	0.532
Texas Sample			
Number of days absent from school	7.088	6.885	0.577
Chronic Absenteeism	0.261	0.297	0.244
Enrolled in K-12 school	0.945	0.953	0.641
Enrolled in Public School	0.945	0.953	0.641
Child is home schooled	0.006	0.017	0.117
Child repeated grade	0.070	0.053	0.243
Child's age above expectation for grade	0.012	0.010	0.712
Percentage of Days in attendance	0.949	0.950	0.783
Gifted and Talented Program	0.086	0.061	0.107
Enrolled in special education program	0.134	0.133	0.945
English / Language Arts Meets Expectations	0.389	0.367	0.217
English / Language Arts Masters Expectations	0.177	0.156	0.122
Math Meets Expectations	0.404	0.396	0.662
Math Masters Expectations	0.192	0.183	0.536
School Characteristics			
Class Size	18.448	18.606	0.465
Average Teacher Salary	5.3e+04	5.3e+04	0.782
Peccent Students Chronic Absenteeism	0.122	0.127	0.336
Average Attendance Rate	0.959	0.959	0.765
Graduation Rate	0.909	0.913	0.094
Percent Proficient in English Language Arts	0.447	0.436	0.253
Percent Proficient in Math	0.472	0.454	0.094
Value Added Estimate Math Scores	-0.455	-0.474	0.671
Value added Estimate English Scores	-0.101	-0.141	0.343
TX-Specific Measures			
Any Disciplinary Action	0.280	0.270	0.707
Any suspension	0.276	0.270	0.810
number of days served suspension	2.482	1.822	0.103
Number of days assigned to suspension	2.990	2.057	0.069
Expelled	0.006	0.004	0.579
In School Suspension	0.173	0.156	0.432
Out of school suspension	0.234	0.224	0.682
Disciplinary Alternative Education Program	0.039	0.029	0.278
Suspended because committed felony	0.000	0.003	0.324
Suspended for drug, alcohol, tobacco reasons	0.020	0.012	0.191
Suspended for physical violence	0.062	0.049	0.294

Notes: This table shows baseline characteristics measured in educational administrative data of matched children of ORUS participants assigned to the treatment vs control arms of the program. The p-value is associated with the test of equality of means across the two groups.

Table A6: Effect of Guaranteed Income on Secondary Spending Outcomes

	All Ages		Under Age 5		Age 5-10		Age 11-17	
	Control Mean	Effect	Control Mean	Effect	Control Mean	Effect	Control Mean	Effect
Total food and beverage consumption	1103.26	42.230 (21.667)*	1153.24	49.730 (33.132)	1182.94	53.819 (30.585)*	1235.22	31.707 (36.053)
		[0.321]		[0.543]		[0.396]		[1.000]
Health expenditures	280.21	27.564 (13.933)**	300.43	-4.469 (20.557)	290.12	33.397 (19.976)*	310.00	32.452 (26.570)
		[0.306]		[1.000]		[0.449]		[0.742]
Expenditures on items for baby/children	38.09	4.661 (1.864)**	61.82	5.243 (3.121)*	38.12	8.457 (2.460)***††	35.37	0.893 (2.667)
		[0.165]		[0.445]		[0.034]		[1.000]
Expenditures on children's clothing	83.26	14.036 (3.656)***††	94.24	14.484 (5.326)***	96.83	18.659 (5.106)***††	99.42	14.327 (5.803)**
		[0.013]		[0.129]		[0.022]		[0.170]
Expenditures on children's educational expenses	46.18	4.835 (3.917)	44.62	3.330 (5.151)	60.69	3.861 (5.943)	49.78	0.175 (5.233)
		[0.740]		[1.000]		[1.000]		[1.000]
Expenditures on children's entertainment	43.44	0.192 (1.740)	49.40	4.215 (2.688)	51.43	0.043 (2.487)	46.70	-0.555 (2.750)
		[1.000]		[0.503]		[1.000]		[1.000]
Expenditures on children's extracurricular activities	35.19	1.404 (2.521)	33.55	0.147 (3.340)	46.07	1.631 (3.625)	45.29	-0.275 (4.042)
		[1.000]		[1.000]		[1.000]		[1.000]
Expenditures on childcare	53.75	10.375 (5.841)*	100.23	12.827 (10.262)	50.96	7.189 (6.824)	29.57	11.111 (6.263)*
		[0.396]		[0.722]		[0.915]		[0.396]
Expenditures on food at home (groceries)	841.67	19.375 (17.278)	896.18	40.463 (27.212)	914.96	31.718 (24.989)	950.41	13.200 (28.971)
		[0.843]		[0.560]		[0.702]		[1.000]
Expenditures on food away from home	262.02	19.340 (7.487)***	258.25	11.594 (11.167)	268.14	16.462 (10.084)	285.22	12.030 (12.058)
		[0.152]		[0.925]		[0.475]		[0.969]

Notes: This table reports estimated treatment effects on outcomes listed in the rows. N is the average number of observations across the outcomes within an age group; N may differ based on (e.g.) survey non-response. $*$ and † denote traditional and FDR-adjusted significance levels respectively. In all cases, three symbols denote the 1% level, two denote the 5% level, and one denotes the 10% level of significance of the test.

Table A7: Effect of Guaranteed Income on Secondary Outcomes Related to the Home Environment

	All Ages		Under Age 5		Age 5-10		Age 11-17	
	Control Mean	Effect	Control Mean	Effect	Control Mean	Effect	Control Mean	Effect
Months of past 6 lived in non-regular housing at least one night	0.22	-0.012 (0.039) 0.13	0.044 (0.059)	0.15	-0.007 (0.044)	0.12	0.052 (0.056)	
		[1.000]	[1.000]		[1.000]		[1.000]	
Months of past 6 lived with others at least one night	1.61	-0.012 (0.112)	1.45	0.132 (0.156)	1.52	-0.097 (0.150)	1.24	-0.057 (0.158)
		[1.000]		[1.000]		[1.000]		[1.000]
Months of past 6 where lived in shelter at least one night	0.16	-0.011 (0.030)	0.14	-0.034 (0.035)	0.15	-0.032 (0.034)	0.08	0.030 (0.031)
		[1.000]		[1.000]		[1.000]		[1.000]
Months child was exposed to being unhoused in survey year	0.31	-0.015 (0.046)	0.21	0.052 (0.059)	0.23	0.003 (0.048)	0.16	0.062 (0.056)
		[1.000]		[1.000]		[1.000]		[1.000]

Notes: This table reports estimated treatment effects on outcomes listed in the rows. N is the average number of observations across the outcomes within an age group; N may differ based on (e.g.) survey non-response. $*$ and † denote traditional and FDR-adjusted significance levels respectively. In all cases, three symbols denote the 1% level, two denote the 5% level, and one denotes the 10% level of significance of the test.

Table A8: Effect of Guaranteed Income on Secondary Outcomes Related to Non-parental Care

	Control Mean	Effect
Relative satisfaction index of new arrangement (-1=Worse, 0=Same, 1=Better)	1.61	-0.231 (0.223)
		[1.000]
Satisfaction with quality of new arrangement (-1=Worse, 0=Same, 1=Better)	0.32	0.006 (0.062)
		[1.000]
Satisfaction with reliability of new arrangement (-1=Worse, 0=Same, 1=Better)	0.41	-0.016 (0.056)
		[1.000]
Satisfaction with location of new arrangement (-1=Worse, 0=Same, 1=Better)	0.37	0.028 (0.058)
		[1.000]
Satisfaction with hours of new arrangement (-1=Worse, 0=Same, 1=Better)	0.28	0.015 (0.056)
		[1.000]
Satisfaction with cost of new arrangement (-1=Worse, 0=Same, 1=Better)	0.25	-0.047 (0.066)
		[1.000]
Indicator if participant missed work due to inability to find childcare (last mo)	0.08	0.048 (0.022)**
		[0.681]
Participant used childcare at any point in period	0.54	0.027 (0.026)
		[1.000]
Count of childcare arrangement changes in period	0.54	0.081 (0.071)
		[1.000]

Notes: This table reports estimated treatment effects on outcomes listed in the rows. N is the average number of observations across the outcomes within an age group; N may differ based on (e.g.) survey non-response. $*$ and † denote traditional and FDR-adjusted significance levels respectively. In all cases, three symbols denote the 1% level, two denote the 5% level, and one denotes the 10% level of significance of the test.

Table A9: Effect of Guaranteed Income on Secondary Educational Outcomes from Administrative Data

	Control Mean	Effect
Number of days absent from school	15.08	0.168 (0.626)
		[1.000]
Chronic Absenteeism	0.33	0.016 (0.019)
		[1.000]
Enrolled in Public School	0.91	-0.021 (0.013)
		[1.000]
Child is home schooled	0.02	-0.001 (0.005)
		[1.000]
Child Dropped Out	0.01	-0.002 (0.003)
		[1.000]
Graduated High School	0.88	0.022 (0.046)
		[1.000]
Enrolled in special education program	0.14	0.007 (0.011)
		[1.000]

Notes: This table reports estimated treatment effects on outcomes listed in the rows. N is the average number of observations across the outcomes within an age group; N may differ based on (e.g.) survey non-response. $*$ and † denote traditional and FDR-adjusted significance levels respectively. In all cases, three symbols denote the 1% level, two denote the 5% level, and one denotes the 10% level of significance of the test.

Table A10: Effect of Guaranteed Income on Secondary Education Outcomes from Administrative Records - Texas Sample Only

	All Grades		Primary School		Secondary School	
	Control Mean	Effect	Control Mean	Effect	Control Mean	Effect
		[1.000]		**[1.000]**		**[1.000]**
Any Disciplinary Action	0.12	0.017 (0.017)	0.11	0.016 (0.019)	0.17	0.065 (0.043)
		[1.000]		[1.000]		[1.000]
Any suspension	0.12	0.019 (0.017)	0.11	0.015 (0.018)	0.16	0.077 (0.043)*
		[1.000]		[1.000]		[1.000]
number of days served suspension	1.43	-0.272 (0.308)	0.97	-0.029 (0.299)	3.14	-0.313 (1.072)
		[1.000]		[1.000]		[1.000]
Number of days assigned to suspension	1.88	-0.349 (0.404)	1.04	0.126 (0.397)	4.77	-1.046 (1.407)
		[1.000]		[1.000]		[1.000]
Expelled	0.00	0.001 (0.002)	0.00	0.002 (0.002)	0.00	-0.002 (0.002)
		[1.000]		[1.000]		[1.000]
In School Suspension	0.06	0.010 (0.012)	0.05	0.008 (0.013)	0.09	0.041 (0.033)
		[1.000]		[1.000]		[1.000]
Out of school suspension	0.09	0.014 (0.015)	0.09	0.009 (0.017)	0.13	0.058 (0.040)
		[1.000]		[1.000]		[1.000]
Disciplinary Alternative Education Program	0.02	-0.001 (0.006)	0.02	-0.003 (0.006)	0.05	0.008 (0.023)
		[1.000]		[1.000]		[1.000]
Suspended because committed felony	0.00	0.000 (0.002)	0.00	0.002 (0.002)	0.01	-0.004 (0.005)
		[1.000]		[1.000]		[1.000]
Suspended for drug, alcohol, tobacco reasons	0.01	0.001 (0.004)	0.00	-0.001 (0.003)	0.02	0.014 (0.018)
		[1.000]		[1.000]		[1.000]
Suspended for physical violence	0.02	-0.003 (0.006)	0.02	-0.005 (0.006)	0.03	0.004 (0.017)
		[1.000]		[1.000]		[1.000]
N		1041		892		336

Notes: This table reports estimated treatment effects on outcomes listed in the rows. N is the average number of observations across the outcomes within an age group; N may differ based on (e.g.) survey non-response. $*$ and † denote traditional and FDR-adjusted significance levels respectively. In all cases, three symbols denote the 1% level, two denote the 5% level, and one denotes the 10% level of significance of the test.

Table A11: Effect of Guaranteed Income on Secondary Education Outcomes from Administrative Records - Illinois Sample Only

	All Grades		Primary School		Secondary School	
	Control Mean	Effect	Control Mean	Effect	Control Mean	Effect
		[1.000]		**[1.000]**		**[1.000]**
Percent of courses passed	0.97	-0.026 (0.008)***	0.98	-0.032 (0.011)***	0.90	-0.064 (0.025)**
		[0.619]		[0.619]		[1.000]
Percent of courses meets expectations	0.83	-0.045 (0.021)**	0.85	-0.074 (0.030)**	0.77	-0.088 (0.036)**
		[1.000]		[1.000]		[1.000]
Percent of courses with mastery	0.38	-0.015 (0.024)	0.39	-0.002 (0.030)	0.33	-0.074 (0.027)***
		[1.000]		[1.000]		[1.000]
Imputed course score (60-100)	88.34	-1.410 (0.547)**	89.09	-1.611 (0.690)**	85.56	-2.488 (0.951)***
		[1.000]		[1.000]		[1.000]
Percent of core courses passed	0.96	-0.037 (0.010)***	0.98	-0.038 (0.012)***	0.88	-0.087 (0.033)***
		[0.321]		[0.593]		[1.000]
Percent of core courses meets expectations	0.79	-0.031 (0.026)	0.81	-0.038 (0.033)	0.73	-0.072 (0.045)
		[1.000]		[1.000]		[1.000]
Percent of core courses with mastery	0.29	-0.013 (0.028)	0.30	0.014 (0.032)	0.26	-0.068 (0.032)**
		[1.000]		[1.000]		[1.000]
Imputed core course score (60-100)	86.46	-1.338 (0.620)**	87.31	-1.185 (0.838)	83.80	-2.374 (1.094)**
		[1.000]		[1.000]		[1.000]
Percent of school's instruction in person (AY 2020-2021)	0.28	-0.016 (0.017)	0.29	-0.005 (0.020)	0.26	-0.031 (0.035)
		[1.000]		[1.000]		[1.000]
Percent of school's instruction hybrid (AY 2020-2021)	0.41	0.020 (0.023)	0.39	-0.001 (0.023)	0.45	0.103 (0.051)**
		[1.000]		[1.000]		[1.000]
Percent of school's instruction virtual (AY 2020-2021)	0.31	-0.016 (0.016)	0.32	-0.004 (0.017)	0.29	-0.015 (0.039)
		[1.000]		[1.000]		[1.000]
N		669		598		187

Notes: This table reports estimated treatment effects on outcomes listed in the rows. N is the average number of observations across the outcomes within an age group; N may differ based on (e.g.) survey non-response. $*$ and † denote traditional and FDR-adjusted significance levels respectively. In all cases, three symbols denote the 1% level, two denote the 5% level, and one denotes the 10% level of significance of the test.

Table A12: Effect of Guaranteed Income on Secondary Outcomes Related to Childbearing

	Control Mean	Effect
Whether participant and partner have any intention to get pregnant	0.23	0.000 (0.013)
		[1.000]
Participant and partner have moderate to strong intention to get pregnant	0.12	-0.005 (0.010)
		[1.000]
Participant or partner had a positive pregnancy test	0.09	0.019 (0.009)**
		[1.000]
Participant doesn't use contraception bc trying to conceive	0.08	0.008 (0.012)
		[1.000]
Got positive pregnancy test and wanted to be pregnant	0.41	0.028 (0.043)
		[1.000]
Efficacy of contraception used (regardless of preg. intention)	0.90	0.017 (0.017)
		[1.000]
Participant is sexually active	0.50	0.003 (0.020)
		[1.000]
Participant uses contraception (regardless of preg. intention)	0.30	0.033 (0.019)*
		[1.000]

Notes: This table reports estimated treatment effects on outcomes listed in the rows. $*$ and † denote traditional and FDR-adjusted significance levels respectively. In all cases, three symbols denote the 1% level, two denote the 5% level, and one denotes the 10% level of significance of the test.

Table A13: Impact of Guaranteed Income on Parental Behaviors and Investments by Poverty Status at Baseline

	Control Mean	Main Estimate	100% FPL+	Under 100% FPL
Parental Behavior and Investment		**0.036****	**0.010**	**0.065*****
		(0.014)	**(0.018)**	**(0.023)**
Parenting Quality		0.050**†	-0.005	0.091***
		(0.020)	(0.024)	(0.033)
Corporal Punishment Subscale (0-15)	4.37 (1.73)	-0.134*	0.000	-0.286**
		(0.071)	(0.083)	(0.124)
Inconsistent Discipline Subscale (0-30)	13.23 (4.05)	-0.126	0.217	-0.697**
		(0.163)	(0.193)	(0.285)
Parental Involvement Subscale (0-50)	37.12 (7.85)	-0.018	-0.169	-0.206
		(0.280)	(0.327)	(0.528)
Poor Supervision / Monitoring Subscale (0-50)	14.55 (4.87)	-0.483***	-0.178	-0.520*
		(0.183)	(0.219)	(0.308)
Positive Parenting Subscale (0-30)	25.69 (4.06)	0.172	0.062	0.250
		(0.162)	(0.186)	(0.298)
Monthly expenditures on children	274.09 (241.85)	31.099***††	15.045	51.353***††
		(10.027)	(13.002)	(14.878)
Parental Interaction		0.019	0.013	0.029
		(0.017)	(0.022)	(0.028)
Frequency of reading to child(ren) under 5 (1=Never, . . . , 5=Daily)	4.51 (0.78)	0.009	0.049	0.058
		(0.044)	(0.055)	(0.080)
Frequency of outings with child(ren) under 5 (1=Never,. . . ,5=Daily)	3.78 (0.90)	-0.044	-0.099	0.167*
		(0.056)	(0.068)	(0.092)
Frequency of playing with their child(ren) under 5 (1=Never, . . . , 5=Daily)	4.62 (0.69)	0.037	-0.029	0.104*
		(0.042)	(0.055)	(0.054)
# of days in the last week put their child to bed	4.99 (2.42)	-0.058	-0.040	-0.055
		(0.081)	(0.096)	(0.147)
Hours reading to children, helping w homework, other activities	12.47 (12.70)	-0.064	-0.039	-0.339
		(0.505)	(0.598)	(0.950)
Frequency of working on homework w/ child(ren) (1=Never,. . . ,5=Daily)	3.70 (1.19)	-0.016	-0.052	0.062
		(0.053)	(0.065)	(0.084)
Attended a parent-teacher conference in the past year	0.53 (0.41)	0.034*	0.068***	0.004
		(0.018)	(0.024)	(0.029)
# of days in the last week the parent ate dinner with their child	4.97 (2.08)	0.051	-0.073	0.028
		(0.074)	(0.086)	(0.132)
Attended a school event in the past year	0.49 (0.42)	0.010	-0.001	0.018
		(0.019)	(0.025)	(0.031)
Worked with a group outside of school in the past year	0.21 (0.35)	-0.001	0.004	-0.015
		(0.016)	(0.021)	(0.024)
Attended a general meeting at the child's school in the past year	0.60 (0.42)	0.058***†	0.051**	0.043
		(0.018)	(0.023)	(0.031)
Volunteered at child's school in the past year	0.22 (0.35)	0.011	0.015	-0.004
		(0.016)	(0.021)	(0.026)
Hours spent last week on childcare	45.34 (35.23)	-0.383	0.539	-2.951
		(1.189)	(1.425)	(2.040)
Hours spent last week with family, in person	30.89 (21.68)	0.202	0.557	-0.852
		(0.808)	(0.978)	(1.432)
Parental satisfaction		0.003	-0.004	-0.005
		(0.026)	(0.033)	(0.041)
I feel alone when it comes to raising my child(ren)	2.84 (1.21)	-0.010	-0.020	0.026
		(0.047)	(0.058)	(0.081)
I spend a great deal of time with my child(ren)	4.21 (0.88)	-0.001	-0.003	-0.028
		(0.036)	(0.044)	(0.064)
I get as much satisfaction from parenting as others do	3.97 (0.90)	0.001	-0.023	0.033
		(0.037)	(0.046)	(0.058)
Parents' stress and distress		-0.021	-0.013	-0.035
		(0.029)	(0.034)	(0.051)
K6 score (0-24)	6.59 (4.50)	0.102	0.063	0.204
		(0.145)	(0.176)	(0.267)
Composite stress score (0-40)	18.26 (6.61)	0.125	0.076	0.180
		(0.218)	(0.263)	(0.396)
N		1849	1149	650

Notes: This table reports estimated treatment effects of the guaranteed income payments on outcomes listed in the rows for participants who were in households earning above versus at or under the Federal Poverty Level, as labeled. $*$ and † denote traditional and FDR-adjusted significance levels respectively. In all cases, three symbols denotes significance at the 1% level; two denote significance at the 5% level; one denotes significance at the 10% level of the test.

Table A14: Impact of Guaranteed Income on Parental Behaviors and Investments by Single vs Partnered Parent at Baseline

	Control Mean	Main Estimate	Single parent	Partnered parent
Parental Behavior and Investment		**0.036****	**0.024**	**0.047*****
		(0.014)	**(0.021)**	**(0.018)**
Parenting Quality		0.050**†	0.092***	0.032
		(0.020)	(0.032)	(0.025)
Corporal Punishment Subscale (0-15)	4.37 (1.73)	-0.134*	-0.308***	-0.063
		(0.071)	(0.107)	(0.088)
Inconsistent Discipline Subscale (0-30)	13.23 (4.05)	-0.126	-0.246	-0.011
		(0.163)	(0.239)	(0.214)
Parental Involvement Subscale (0-50)	37.12 (7.85)	-0.018	0.078	0.041
		(0.280)	(0.325)	(0.409)
Poor Supervision / Monitoring Subscale (0-50)	14.55 (4.87)	-0.483***	-0.572**	-0.490*
		(0.183)	(0.251)	(0.255)
Positive Parenting Subscale (0-30)	25.69 (4.06)	0.172	0.217	0.105
		(0.162)	(0.171)	(0.246)
Monthly expenditures on children	274.09 (241.85)	31.099***††	41.474***	29.583**
		(10.027)	(14.413)	(12.850)
Parental Interaction		0.019	0.032	0.005
		(0.017)	(0.025)	(0.023)
Frequency of reading to child(ren) under 5 (1=Never, . . . , 5=Daily)	4.51 (0.78)	0.009	0.025	-0.015
		(0.044)	(0.069)	(0.055)
Frequency of outings with child(ren) under 5 (1=Never,. . . ,5=Daily)	3.78 (0.90)	-0.044	-0.046	-0.051
		(0.056)	(0.078)	(0.078)
Frequency of playing with their child(ren) under 5 (1=Never, . . . , 5=Daily)	4.62 (0.69)	0.037	-0.025	0.024
		(0.042)	(0.068)	(0.049)
# of days in the last week put their child to bed	4.99 (2.42)	-0.058	0.117	-0.146
		(0.081)	(0.114)	(0.113)
Hours reading to children, helping w homework, other activities	12.47 (12.70)	-0.064	-1.224*	0.712
		(0.505)	(0.686)	(0.683)
Frequency of working on homework w/ child(ren) (1=Never,. . . ,5=Daily)	3.70 (1.19)	-0.016	-0.015	-0.027
		(0.053)	(0.069)	(0.076)
Attended a parent-teacher conference in the past year	0.53 (0.41)	0.034*	0.062**	0.029
		(0.018)	(0.027)	(0.025)
# of days in the last week the parent ate dinner with their child	4.97 (2.08)	0.051	0.140	-0.011
		(0.074)	(0.097)	(0.101)
Attended a school event in the past year	0.49 (0.42)	0.010	0.022	-0.001
		(0.019)	(0.027)	(0.025)
Worked with a group outside of school in the past year	0.21 (0.35)	-0.001	-0.013	0.011
		(0.016)	(0.022)	(0.022)
Attended a general meeting at the child's school in the past year	0.60 (0.42)	0.058***†	0.065**	0.053**
		(0.018)	(0.026)	(0.025)
Volunteered at child's school in the past year	0.22 (0.35)	0.011	0.043*	-0.021
		(0.016)	(0.024)	(0.022)
Hours spent last week on childcare	45.34 (35.23)	-0.383	0.232	0.401
		(1.189)	(1.859)	(1.521)
Hours spent last week with family, in person	30.89 (21.68)	0.202	0.237	-0.473
		(0.808)	(1.167)	(1.093)
Parental satisfaction		0.003	-0.046	0.035
		(0.026)	(0.038)	(0.032)
I feel alone when it comes to raising my child(ren)	2.84 (1.21)	-0.010	0.119*	-0.065
		(0.047)	(0.071)	(0.061)
I spend a great deal of time with my child(ren)	4.21 (0.88)	-0.001	-0.028	0.052
		(0.036)	(0.047)	(0.048)
I get as much satisfaction from parenting as others do	3.97 (0.90)	0.001	-0.001	-0.007
		(0.037)	(0.054)	(0.047)
Parents' stress and distress		-0.021	-0.134***†	0.040
		(0.029)	(0.044)	(0.035)
K6 score (0-24)	6.59 (4.50)	0.102	0.714***†	-0.233
		(0.145)	(0.229)	(0.182)
Composite stress score (0-40)	18.26 (6.61)	0.125	0.717**	-0.192
		(0.218)	(0.347)	(0.269)
N		1849	784	1065

Notes: This table reports estimated treatment effects of the guaranteed income payments on outcomes listed in the rows for participants who were single versus partnered at baseline, as labeled. ∗ and † denote traditional and FDR-adjusted significance levels respectively. In all cases, three symbols denote the 1% level, two denote the 5% level, and one denotes the 10% level of significance of the test.

Table A15: Impact of Guaranteed Income on Home Environment by Poverty Status at Baseline

	Control Mean	Main Estimate	100% FPL+	Under 100% FPL
Home environment		**-0.021**	**-0.030**	**-0.009**
		(0.025)	**(0.029)**	**(0.043)**
Child Food Insecurity Score (0-6, higher is more insecure)	0.83 (1.01)	0.051	0.059	0.010
		(0.039)	(0.046)	(0.075)
Chaos Score (0-36, higher is more chaotic)	13.31 (5.42)	0.000	0.254	-0.165
		(0.206)	(0.248)	(0.377)
Parent was ever unhoused in survey year	0.10 (0.24)	0.003	-0.004	0.013
		(0.010)	(0.010)	(0.019)
N		1882	1174	658

Notes: This table reports estimated treatment effects of the guaranteed income payments on outcomes listed in the rows for participants who were in households earning above versus at or under the Federal Poverty Level, as labeled. $*$ and † denote traditional and FDR-adjusted significance levels respectively. In all cases, three symbols denote the 1% level, two denote the 5% level, and one denotes the 10% level of significance of the test.

Table A16: Impact of Guaranteed Income on Home Environment by Single vs Partnered Parent at Baseline

	Control Mean	Main Estimate	Single parent	Partnered parent
Home environment		**-0.021**	**-0.102*****	**0.020**
		(0.025)	**(0.035)**	**(0.032)**
Child Food Insecurity Score (0-6, higher is more insecure)	0.83 (1.01)	0.051	0.125**	0.029
		(0.039)	(0.060)	(0.048)
Chaos Score (0-36, higher is more chaotic)	13.31 (5.42)	0.000	0.542*	-0.208
		(0.206)	(0.291)	(0.279)
Parent was ever unhoused in survey year	0.10 (0.24)	0.003	0.021	-0.013
		(0.010)	(0.016)	(0.012)
N		1882	804	1078

Notes: This table reports estimated treatment effects of the guaranteed income payments on outcomes listed in the rows for participants who were single versus partnered at baseline, as labeled. $*$ and † denote traditional and FDR-adjusted significance levels respectively. In all cases, three symbols denote the 1% level, two denote the 5% level, and one denotes the 10% level of significance of the test.

Table A17: Impact of Guaranteed Income on Mobility and the Neighborhood Environment by Poverty Status at Baseline

	Control Mean	Main Estimate	100% FPL+	Under 100% FPL
Mobility and neighborhood environment		**0.019****	**0.023****	**0.016**
		(0.008)	**(0.011)**	**(0.014)**
Moving behavior		0.105***	0.115**	0.123*
		(0.040)	(0.053)	(0.069)
Moved units	0.40 (0.43)	0.045**	0.042*	0.059*
		(0.018)	(0.024)	(0.030)
Moved neighborhoods	0.36 (0.42)	0.044**	0.056**	0.046
		(0.018)	(0.024)	(0.031)
Family friendliness		-0.008	0.013	-0.035
		(0.027)	(0.034)	(0.047)
Share of households with children	0.35 (0.10)	-0.000	0.002	-0.003
		(0.003)	(0.004)	(0.005)
Share of population that is children	0.25 (0.06)	-0.001	0.000	-0.003
		(0.002)	(0.002)	(0.003)
Child-Focused Amenities		0.033*	0.037*	0.034
		(0.017)	(0.020)	(0.029)
Distance-decayed count of daycares within 1 mile	2.66 (2.95)	0.129**	0.151*	0.142
		(0.065)	(0.078)	(0.112)
Distance-decayed count of libraries within 1 mile	0.38 (0.55)	0.012	0.020	-0.004
		(0.014)	(0.017)	(0.024)
Distance-decayed count of parks within 1 mile	3.93 (3.79)	0.070	0.003	0.239*
		(0.079)	(0.094)	(0.140)
Distance-decayed count of schools within 1 mile	3.78 (3.40)	0.167**	0.188**	0.133
		(0.074)	(0.090)	(0.132)
Economic Mobility		-0.011	-0.011	-0.030
		(0.017)	(0.022)	(0.029)
Income mobility measure	0.03 (0.04)	-0.000	-0.001	-0.001
		(0.001)	(0.001)	(0.001)
Incarceration rate for children w/ low-income parents	0.41 (0.08)	0.001	0.001	0.004
		(0.002)	(0.002)	(0.003)
Census tract level pollution		0.001	0.025	-0.030
		(0.021)	(0.028)	(0.030)
PM 2.5	8.95 (0.51)	0.005	-0.015	0.031
		(0.017)	(0.021)	(0.024)
Risk-Screening Environmental Indicators	7.37 (1.69)	-0.022	-0.033	-0.000
		(0.033)	(0.045)	(0.049)
Quality indices		-0.005	-0.042	0.036
		(0.026)	(0.034)	(0.043)
Area Deprivation Index	81.04 (36.73)	0.648	2.524	-2.835
		(1.300)	(1.534)	(2.431)
Childhood Opportunity Index	-0.27 (0.80)	0.007	-0.008	0.001
		(0.021)	(0.027)	(0.034)
N		1950	1210	686

Notes: This table reports estimated treatment effects of the guaranteed income payments on outcomes listed in the rows for participants who were in households earning above versus at or under the Federal Poverty Level, as labeled. $*$ and † denote traditional and FDR-adjusted significance levels respectively. In all cases, three symbols denote the 1% level, two denote the 5% level, and one denotes the 10% level of significance of the test.

Table A18: Impact of Guaranteed Income on Mobility and the Neighborhood Environment by Single vs Partnered Parent at Baseline

	Control Mean	Main Estimate	Single parent	Partnered parent
Mobility and neighborhood environment		**0.019****	**0.005**	**0.030*****
		(0.008)	**(0.012)**	**(0.011)**
Moving behavior		0.105***	0.060	0.126**
		(0.040)	(0.058)	(0.056)
Moved units	0.40 (0.43)	0.045**	0.020	0.053**
		(0.018)	(0.028)	(0.024)
Moved neighborhoods	0.36 (0.42)	0.044**	0.032	0.053**
		(0.018)	(0.027)	(0.025)
Family friendliness		-0.008	-0.027	0.018
		(0.027)	(0.042)	(0.037)
Share of households with children	0.35 (0.10)	-0.000	-0.002	0.004
		(0.003)	(0.004)	(0.004)
Share of population that is children	0.25 (0.06)	-0.001	-0.002	0.000
		(0.002)	(0.003)	(0.002)
Child-Focused Amenities		0.033*	0.031	0.036
		(0.017)	(0.021)	(0.024)
Distance-decayed count of daycares within 1 mile	2.66 (2.95)	0.129**	0.109	0.128
		(0.065)	(0.091)	(0.089)
Distance-decayed count of libraries within 1 mile	0.38 (0.55)	0.012	0.015	0.010
		(0.014)	(0.021)	(0.019)
Distance-decayed count of parks within 1 mile	3.93 (3.79)	0.070	0.095	0.096
		(0.079)	(0.094)	(0.110)
Distance-decayed count of schools within 1 mile	3.78 (3.40)	0.167**	0.146	0.169*
		(0.074)	(0.106)	(0.098)
Economic Mobility		-0.011	-0.058**	0.019
		(0.017)	(0.026)	(0.023)
Income mobility measure	0.03 (0.04)	-0.000	-0.002	0.001
		(0.001)	(0.001)	(0.001)
Incarceration rate for children w/ low-income parents	0.41 (0.08)	0.001	0.005**	-0.000
		(0.002)	(0.002)	(0.002)
Census tract level pollution		0.001	0.002	0.004
		(0.021)	(0.027)	(0.027)
PM 2.5	8.95 (0.51)	0.005	0.023	-0.002
		(0.017)	(0.020)	(0.021)
Risk-Screening Environmental Indicators	7.37 (1.69)	-0.022	-0.080*	-0.010
		(0.033)	(0.044)	(0.045)
Quality indices		-0.005	0.021	-0.022
		(0.026)	(0.039)	(0.034)
Area Deprivation Index	81.04 (36.73)	0.648	-0.229	1.256
		(1.300)	(1.959)	(1.708)
Childhood Opportunity Index	-0.27 (0.80)	0.007	0.028	-0.008
		(0.021)	(0.031)	(0.028)
N		1950	812	1138

Notes: This table reports estimated treatment effects of the guaranteed income payments on outcomes listed in the rows for participants who were single versus partnered at baseline, as labeled. $*$ and † denote traditional and FDR-adjusted significance levels respectively. In all cases, three symbols denote the 1% level, two denote the 5% level, and one denotes the 10% level of significance of the test.

Table A19: Impact of Guaranteed Income on Use of Non-Parental Care by Poverty Status at Baseline

	Control Mean	Main Estimate	100% FPL+	Under 100% FPL
Non Parental Care		**0.004**	**-0.043**	**0.058**
		(0.023)	**(0.028)**	**(0.039)**
Number of hours per week child(ren) are in care	13.26 (16.70)	0.195	-2.136**	1.751
		(0.730)	(1.034)	(1.063)
Quality of childcare		0.009	-0.024	0.063
		(0.047)	(0.050)	(0.091)
Changed to higher quality arrangement	0.03 (0.08)	0.005	0.001	0.012*
		(0.004)	(0.004)	(0.006)
Changed to more reliable arrangement	0.03 (0.10)	0.000	-0.003	0.010*
		(0.004)	(0.005)	(0.005)
Participant is satisfied with current childcare	0.81 (0.36)	-0.011	-0.018	-0.051
		(0.044)	(0.048)	(0.084)
Stability of care		-0.044	0.027	-0.216**
		(0.037)	(0.044)	(0.090)
Count of childcare arrangement changes in period	0.34 (0.71)	0.096***	0.018	0.266***
		(0.036)	(0.041)	(0.088)
Number of childcare providers / arrangements	0.53 (0.86)	-0.084	-0.170**	0.053
		(0.054)	(0.072)	(0.085)
# times parent made arrangements bc child care fell through	1.17 (2.58)	0.084	-0.022	0.201
		(0.117)	(0.117)	(0.251)
Participant currently using childcare at time of survey	0.46 (0.43)	0.016	-0.023	0.106***†
		(0.021)	(0.026)	(0.031)
N		1228	767	428

Notes: This table reports estimated treatment effects of the guaranteed income payments on outcomes listed in the rows for participants who were in households earning above versus at or under the Federal Poverty Level, as labeled. $*$ and † denote traditional and FDR-adjusted significance levels respectively. In all cases, three symbols denote the 1% level, two denote the 5% level, and one denotes the 10% level of significance of the test.

Table A20: Impact of Guaranteed Income on Non-Parental Care by Single vs Partnered Parent at Baseline

	Control Mean	Main Estimate	Single parent	Partnered parent
Non Parental Care		**0.004**	**-0.012**	**0.011**
		(0.023)	**(0.033)**	**(0.032)**
Number of hours per week child(ren) are in care	13.26 (16.70)	0.195	0.918	-0.939
		(0.730)	(1.123)	(1.009)
Quality of childcare		0.009	0.006	-0.005
		(0.047)	(0.059)	(0.069)
Changed to higher quality arrangement	0.03 (0.08)	0.005	0.004	0.004
		(0.004)	(0.006)	(0.004)
Changed to more reliable arrangement	0.03 (0.10)	0.000	-0.004	0.005
		(0.004)	(0.007)	(0.005)
Participant is satisfied with current childcare	0.81 (0.36)	-0.011	0.004	-0.050
		(0.044)	(0.058)	(0.060)
Stability of care		-0.044	-0.119**	0.015
		(0.037)	(0.057)	(0.043)
Count of childcare arrangement changes in period	0.34 (0.71)	0.096***	0.107*	0.072**
		(0.036)	(0.057)	(0.036)
Number of childcare providers / arrangements	0.53 (0.86)	-0.084	0.016	-0.115*
		(0.054)	(0.095)	(0.064)
# times parent made arrangements bc child care fell through	1.17 (2.58)	0.084	0.378**	-0.116
		(0.117)	(0.189)	(0.119)
Participant currently using childcare at time of survey	0.46 (0.43)	0.016	0.005	0.039
		(0.021)	(0.030)	(0.028)
N		1228	550	677

Notes: This table reports estimated treatment effects of the guaranteed income payments on outcomes listed in the rows for participants who were single versus partnered at baseline, as labeled. $*$ and † denote traditional and FDR-adjusted significance levels respectively. In all cases, three symbols denote the 1% level, two denote the 5% level, and one denotes the 10% level of significance of the test.

Table A21: Impact of Guaranteed Income on Children's Stress and Development by Poverty Status at Baseline

	Control Mean	Main Estimate	100% FPL+	Under 100% FPL
Stress and Social Development		**-0.060****	**-0.072*****	**-0.082****
		(0.023)	**(0.026)**	**(0.034)**
Strengths and Difficulties		-0.049*†	-0.048	-0.050
		(0.026)	(0.029)	(0.036)
Total Difficulties Score (0-40)	8.00 (5.76)	0.400**	0.542**	0.354
		(0.202)	(0.236)	(0.280)
Conduct Problems Scale (0-10)	1.34 (1.61)	0.089	0.072	0.115
		(0.061)	(0.070)	(0.098)
Emotional Problems Scale (0-10)	1.70 (1.76)	0.022	-0.024	0.097
		(0.063)	(0.074)	(0.086)
Hyperactivity Scale (0-10)	3.37 (2.62)	0.238***†	0.232**	0.188
		(0.091)	(0.107)	(0.169)
Peer Problems Scale (0-10)	1.59 (1.50)	0.064	0.107	0.034
		(0.059)	(0.073)	(0.086)
Prosocial Scale (0-10)	8.54 (1.63)	-0.043	-0.003	-0.009
		(0.063)	(0.077)	(0.108)
PROMIS mental health score (4-20)	6.61 (2.95)	0.208**†	0.288**	0.322**
		(0.099)	(0.115)	(0.153)
N		2648	1631	959

Notes: This table reports estimated treatment effects of the guaranteed income payments on outcomes listed in the rows for children of participants who were in households earning above versus at or under the Federal Poverty Level, as labeled. $*$ and † denote traditional and FDR-adjusted significance levels respectively. In all cases, three symbols denote the 1% level, two denote the 5% level, and one denotes the 10% level of significance of the test.

Table A22: Impact of Guaranteed Income on Children's Stress and Development by Child Sex

	Control Mean	Main Estimate	Male child	Female child
Stress and Social Development		**-0.060****	**-0.035**	**-0.102*****
		(0.023)	**(0.030)**	**(0.033)**
Strengths and Difficulties		-0.049*†	-0.016	-0.080**
		(0.026)	(0.034)	(0.036)
Total Difficulties Score (0-40)	8.00 (5.76)	0.400**	0.158	0.503*
		(0.202)	(0.264)	(0.273)
Conduct Problems Scale (0-10)	1.34 (1.61)	0.089	-0.001	0.161**
		(0.061)	(0.084)	(0.081)
Emotional Problems Scale (0-10)	1.70 (1.76)	0.022	-0.057	0.035
		(0.063)	(0.083)	(0.089)
Hyperactivity Scale (0-10)	3.37 (2.62)	0.238***†	0.218*	0.210*
		(0.091)	(0.128)	(0.119)
Peer Problems Scale (0-10)	1.59 (1.50)	0.064	0.046	0.173**
		(0.059)	(0.084)	(0.079)
Prosocial Scale (0-10)	8.54 (1.63)	-0.043	0.013	-0.079
		(0.063)	(0.087)	(0.085)
PROMIS mental health score (4-20)	6.61 (2.95)	0.208**†	0.165	0.353***
		(0.099)	(0.129)	(0.136)
N		2648	1368	1281

Notes: This table reports estimated treatment effects of the guaranteed income payments on outcomes listed in the rows for children of participants by sex, as labeled. $*$ and † denote traditional and FDR-adjusted significance levels respectively. In all cases, three symbols denote the 1% level, two denote the 5% level, and one denotes the 10% level of significance of the test.

Table A23: Impact of Guaranteed Income on Children's Stress and Development by Single versus Partnered Parent

	Control Mean	Main Estimate	Single parent	Partnered parent
Stress and Social Development		**-0.060****	**-0.070****	**-0.018**
		(0.023)	**(0.034)**	**(0.026)**
Strengths and Difficulties		-0.049*†	-0.073**	-0.025
		(0.026)	(0.035)	(0.031)
Total Difficulties Score (0-40)	8.00 (5.76)	0.400**	0.697**	0.341
		(0.202)	(0.274)	(0.248)
Conduct Problems Scale (0-10)	1.34 (1.61)	0.089	0.061	0.027
		(0.061)	(0.086)	(0.080)
Emotional Problems Scale (0-10)	1.70 (1.76)	0.022	0.122	0.006
		(0.063)	(0.089)	(0.078)
Hyperactivity Scale (0-10)	3.37 (2.62)	0.238***†	0.255**	0.206
		(0.091)	(0.114)	(0.128)
Peer Problems Scale (0-10)	1.59 (1.50)	0.064	0.108	-0.006
		(0.059)	(0.082)	(0.076)
Prosocial Scale (0-10)	8.54 (1.63)	-0.043	-0.065	0.007
		(0.063)	(0.086)	(0.085)
PROMIS mental health score (4-20)	6.61 (2.95)	0.208**†	0.199	0.032
		(0.099)	(0.143)	(0.121)
N		2648	1259	1389

Notes: This table reports estimated treatment effects of the guaranteed income payments on outcomes listed in the rows for children of participants who were single versus partnered at baseline, as labeled. $*$ and † denote traditional and FDR-adjusted significance levels respectively. In all cases, three symbols denote the 1% level, two denote the 5% level, and one denotes the 10% level of significance of the test.

Table A24: Impact of Guaranteed Income on Children's K-12 Education Outcomes by Poverty Status at Baseline (Survey Measures)

	Control Mean	Main Estimate	100% FPL+	Under 100% FPL
Educational Outcomes (Survey, K-12)		**-0.017**	**-0.048****	**0.029**
		(0.017)	**(0.020)**	**(0.025)**
# days absent from school in the most recent school yr	7.17 (8.51)	-0.051	0.388	-0.648
		(0.355)	(0.415)	(0.500)
Disciplinary action, and help needed over past 2 years		-0.035	-0.035	-0.030
		(0.024)	(0.027)	(0.034)
Child got special help for behavioral/emotional problems	0.10 (0.27)	0.006	0.008	-0.005
		(0.010)	(0.011)	(0.013)
Asked to meet about probs. w child's schoolwork or behavior	0.14 (0.31)	0.020*	0.013	0.006
		(0.012)	(0.013)	(0.017)
Child got special help at school for learning problems	0.18 (0.36)	0.014	0.027*	0.012
		(0.013)	(0.016)	(0.019)
Child suspended/expelled	0.06 (0.22)	0.003	-0.001	0.016
		(0.007)	(0.010)	(0.012)
School enrollment		-0.005	-0.069*	0.076*
		(0.028)	(0.037)	(0.043)
Child (5-17) is currently in school	0.91 (0.26)	0.009	-0.014	0.023
		(0.009)	(0.010)	(0.018)
Whether child has ever repeated a grade	0.02 (0.12)	0.005	0.007	-0.010
		(0.005)	(0.006)	(0.008)
Report of child's grades (1=Mostly D's and F's, . . . , 5=All A's)	3.93 (0.72)	-0.019	-0.048	0.065
		(0.026)	(0.031)	(0.045)
Perceived quality of child's education (1=Poor, . . . , 5=Excellent)	3.90 (0.90)	-0.022	-0.021	-0.058
		(0.036)	(0.041)	(0.065)
N		2906	1783	1045

Notes: This table reports estimated treatment effects of the guaranteed income payments on outcomes listed in the rows for children of participants who were in households earning above versus at or under the Federal Poverty Level, as labeled. $*$ and † denote traditional and FDR-adjusted significance levels respectively. In all cases, three symbols denote the 1% level, two denote the 5% level, and one denotes the 10% level of significance of the test.

Table A25: Impact of Guaranteed Income on Children's K-12 Education Outcomes by Child Sex (Survey Measures)

	Control Mean	Main Estimate	Male child	Female child
Educational Outcomes (Survey, K-12)		**-0.017**	**-0.002**	**-0.048****
		(0.017)	**(0.023)**	**(0.024)**
# days absent from school in the most recent school yr	7.17 (8.51)	-0.051	-0.323	0.277
		(0.355)	(0.449)	(0.501)
Disciplinary action, and help needed over past 2 years		-0.035	-0.003	-0.078**
		(0.024)	(0.031)	(0.033)
Child got special help for behavioral/emotional problems	0.10 (0.27)	0.006	-0.010	0.015
		(0.010)	(0.015)	(0.011)
Asked to meet about probs. w child's schoolwork or behavior	0.14 (0.31)	0.020*	0.006	0.019
		(0.012)	(0.017)	(0.014)
Child got special help at school for learning problems	0.18 (0.36)	0.014	0.003	0.024
		(0.013)	(0.018)	(0.016)
Child suspended/expelled	0.06 (0.22)	0.003	0.005	0.017*
		(0.007)	(0.012)	(0.010)
School enrollment		-0.005	-0.025	0.024
		(0.028)	(0.038)	(0.035)
Child (5-17) is currently in school	0.91 (0.26)	0.009	-0.001	0.019
		(0.009)	(0.012)	(0.012)
Whether child has ever repeated a grade	0.02 (0.12)	0.005	0.006	0.003
		(0.005)	(0.007)	(0.006)
Report of child's grades (1=Mostly D's and F's, . . . , 5=All A's)	3.93 (0.72)	-0.019	0.008	-0.031
		(0.026)	(0.038)	(0.036)
Perceived quality of child's education (1=Poor, . . . , 5=Excellent)	3.90 (0.90)	-0.022	-0.025	-0.095*
		(0.036)	(0.047)	(0.049)
N		2906	1495	1411

Notes: This table reports estimated treatment effects of the guaranteed income payments on outcomes listed in the rows for children of participants by sex, as labeled. $*$ and † denote traditional and FDR-adjusted significance levels respectively. In all cases, three symbols denote the 1% level, two denote the 5% level, and one denotes the 10% level of significance of the test.

Table A26: Impact of Guaranteed Income on Children's K-12 Education Outcomes by Single versus Partnered Parent (Survey Measures)

	Control Mean	Main Estimate	Single parent	Partnered parent
Educational Outcomes (Survey, K-12)		**-0.017**	**-0.003**	**-0.020**
		(0.017)	**(0.023)**	**(0.022)**
# days absent from school in the most recent school yr	7.17 (8.51)	-0.051	-0.448	-0.192
		(0.355)	(0.516)	(0.451)
Disciplinary action, and help needed over past 2 years		-0.035	-0.033	-0.031
		(0.024)	(0.029)	(0.029)
Child got special help for behavioral/emotional problems	0.10 (0.27)	0.006	0.002	0.005
		(0.010)	(0.013)	(0.013)
Asked to meet about probs. w child's schoolwork or behavior	0.14 (0.31)	0.020*	0.011	0.016
		(0.012)	(0.016)	(0.013)
Child got special help at school for learning problems	0.18 (0.36)	0.014	0.029	0.002
		(0.013)	(0.019)	(0.016)
Child suspended/expelled	0.06 (0.22)	0.003	0.003	0.008
		(0.007)	(0.012)	(0.008)
School enrollment		-0.005	0.002	0.009
		(0.028)	(0.035)	(0.035)
Child (5-17) is currently in school	0.91 (0.26)	0.009	0.006	0.009
		(0.009)	(0.012)	(0.012)
Whether child has ever repeated a grade	0.02 (0.12)	0.005	0.002	0.002
		(0.005)	(0.006)	(0.007)
Report of child's grades (1=Mostly D's and F's, . . . , 5=All A's)	3.93 (0.72)	-0.019	0.021	-0.021
		(0.026)	(0.039)	(0.036)
Perceived quality of child's education (1=Poor, . . . , 5=Excellent)	3.90 (0.90)	-0.022	-0.056	-0.063
		(0.036)	(0.048)	(0.045)
N		2906	1342	1564

Notes: This table reports estimated treatment effects of the guaranteed income payments on outcomes listed in the rows for children of participants who were single versus partnered at baseline, as labeled. $*$ and † denote traditional and FDR-adjusted significance levels respectively. In all cases, three symbols denote the 1% level, two denote the 5% level, and one denotes the 10% level of significance of the test.

Table A27: Impact of Guaranteed Income on Children's K-12 Education Outcomes by Poverty Status at Baseline (Administrative Data Measures)

	Control Mean	Main Estimate	100% FPL+	Under 100% FPL
Education Outcomes from Admin Data		**-0.004**	**-0.009**	**0.026**
		(0.015)	**(0.020)**	**(0.042)**
Enrollment Measures		-0.003	0.010	-0.056
		(0.027)	(0.034)	(0.051)
Enrolled in K-12 school	0.91 (0.24)	-0.022*	-0.009	-0.067***
		(0.012)	(0.013)	(0.025)
Child repeated grade	0.04 (0.16)	-0.005	-0.001	-0.021*
		(0.007)	(0.007)	(0.012)
Child's age above expectation for grade	0.03 (0.15)	-0.005	-0.003	-0.012
		(0.005)	(0.005)	(0.011)
Percentage of Days in attendance	0.90 (0.10)	-0.003	-0.002	-0.001
		(0.004)	(0.005)	(0.010)
Gifted and Talented Program	0.06 (0.21)	-0.002	0.003	0.000
		(0.004)	(0.007)	(0.002)
Standardized Test Performance		-0.026	-0.041	-0.048
		(0.034)	(0.044)	(0.081)
English / Language Arts Meets Expectations	0.37 (0.42)	-0.004	-0.006	-0.031
		(0.021)	(0.025)	(0.047)
English / Language Arts Masters Expectations	0.11 (0.26)	0.002	0.000	-0.002
		(0.011)	(0.016)	(0.012)
Math Meets Expectations	0.29 (0.41)	-0.042**	-0.071**	0.035
		(0.020)	(0.029)	(0.052)
Math Masters Expectations	0.09 (0.25)	-0.002	0.001	-0.008
		(0.010)	(0.018)	(0.010)
Education mobility		0.006	-0.099*	0.262*
		(0.050)	(0.052)	(0.159)
Changed Schools	0.37 (0.36)	0.011	0.009	0.083**
		(0.020)	(0.026)	(0.041)
Changed Districts	0.24 (0.35)	0.011	-0.023	0.082
		(0.022)	(0.026)	(0.051)
School quality		0.028	0.018	-0.033
		(0.023)	(0.028)	(0.039)
Class Size	19.61 (3.71)	-0.149	0.014	-0.466
		(0.186)	(0.224)	(0.340)
Average Teacher Salary	64535.54 (9578.88)	-61.089	523.420	-1.1e+03
		(338.342)	(337.317)	(734.981)
Peccent Students Chronic Absenteeism	0.25 (0.11)	-0.007	0.008	-0.020**
		(0.005)	(0.005)	(0.009)
Average Attendance Rate	0.92 (0.03)	0.002*	-0.001	0.005**
		(0.001)	(0.001)	(0.002)
Graduation Rate	0.87 (0.14)	0.008	0.023*	-0.054**
		(0.011)	(0.014)	(0.027)
Percent Proficient in English Language Arts	0.37 (0.15)	-0.003	-0.009	-0.008
		(0.007)	(0.009)	(0.012)
Percent Proficient in Math	0.30 (0.15)	-0.007	-0.008	-0.007
		(0.007)	(0.010)	(0.010)
Value Added Estimate Math Scores	0.41 (0.84)	0.025	0.001	-0.005
		(0.034)	(0.040)	(0.071)
Value added Estimate English Scores	1.15 (1.08)	0.037	0.064	-0.100
		(0.038)	(0.045)	(0.077)

Notes: This table reports estimated treatment effects of the guaranteed income payments on outcomes listed in the rows for children of participants who were in households earning above versus at or under the Federal Poverty Level, as labeled. $*$ and † denote traditional and FDR-adjusted significance levels respectively. In all cases, three symbols denote the 1% level, two denote the 5% level, and one denotes the 10% level of significance of the test.

Table A28: Impact of Guaranteed Income on Children's K-12 Education Outcomes by Child Sex (Administrative Data Measures)

	Control Mean	Main Estimate	Male child	Female child
Education Outcomes from Admin Data		**-0.004**	**0.015**	**-0.018**
		(0.015)	**(0.020)**	**(0.022)**
Enrollment Measures		-0.003	0.021	-0.027
		(0.027)	(0.035)	(0.039)
Enrolled in K-12 school	0.91 (0.24)	-0.022*	-0.003	-0.027*
		(0.012)	(0.016)	(0.016)
Child repeated grade	0.04 (0.16)	-0.005	-0.003	-0.006
		(0.007)	(0.010)	(0.010)
Child's age above expectation for grade	0.03 (0.15)	-0.005	-0.009	0.000
		(0.005)	(0.006)	(0.007)
Percentage of Days in attendance	0.90 (0.10)	-0.003	0.007	-0.008
		(0.004)	(0.007)	(0.007)
Gifted and Talented Program	0.06 (0.21)	-0.002	0.002	-0.002
		(0.004)	(0.006)	(0.006)
Standardized Test Performance		-0.026	-0.003	-0.055
		(0.034)	(0.046)	(0.052)
English / Language Arts Meets Expectations	0.37 (0.42)	-0.004	0.000	-0.023
		(0.021)	(0.029)	(0.035)
English / Language Arts Masters Expectations	0.11 (0.26)	0.002	-0.019*	0.010
		(0.011)	(0.010)	(0.018)
Math Meets Expectations	0.29 (0.41)	-0.042**	-0.008	-0.052
		(0.020)	(0.034)	(0.034)
Math Masters Expectations	0.09 (0.25)	-0.002	0.015	0.007
		(0.010)	(0.020)	(0.009)
Education mobility		0.006	-0.023	0.008
		(0.050)	(0.059)	(0.063)
Changed Schools	0.37 (0.36)	0.011	0.023	0.020
		(0.020)	(0.030)	(0.028)
Changed Districts	0.24 (0.35)	0.011	0.006	0.034
		(0.022)	(0.031)	(0.034)
School quality		0.028	0.003	0.041
		(0.023)	(0.030)	(0.028)
Class Size	19.61 (3.71)	-0.149	0.049	-0.233
		(0.186)	(0.246)	(0.264)
Average Teacher Salary	64535.54 (9578.88)	-61.089	201.883	-11.809
		(338.342)	(359.915)	(467.903)
Peccent Students Chronic Absenteeism	0.25 (0.11)	-0.007	0.003	-0.008
		(0.005)	(0.006)	(0.006)
Average Attendance Rate	0.92 (0.03)	0.002*	-0.001	0.003*
		(0.001)	(0.002)	(0.002)
Graduation Rate	0.87 (0.14)	0.008	0.011	0.013
		(0.011)	(0.018)	(0.014)
Percent Proficient in English Language Arts	0.37 (0.15)	-0.003	-0.008	-0.000
		(0.007)	(0.008)	(0.010)
Percent Proficient in Math	0.30 (0.15)	-0.007	-0.014*	-0.002
		(0.007)	(0.008)	(0.009)
Value Added Estimate Math Scores	0.41 (0.84)	0.025	0.011	0.052
		(0.034)	(0.050)	(0.049)
Value added Estimate English Scores	1.15 (1.08)	0.037	0.006	0.042
		(0.038)	(0.054)	(0.056)

Notes: This table reports estimated treatment effects of the guaranteed income payments on outcomes listed in the rows for children of participants by sex, as labeled. $*$ and † denote traditional and FDR-adjusted significance levels respectively. In all cases, three symbols denote the 1% level, two denote the 5% level, and one denotes the 10% level of significance of the test.

Table A29: Impact of Guaranteed Income on Birth and Fertility Outcomes by State

	Control Mean	Main Estimate	Texas	Illinois
Number of new children (survey)	0.19 (0.47)	**-0.000**	**0.008**	**-0.016**
		(0.017)	**(0.022)**	**(0.023)**
Number of births to participants (admin)^{s}	0.14 (.)	-0.011		
		(0.016)		
Participant reported any new child 2020^{s}	0.16 (0.37)	-0.004	0.009	-0.018
		(0.012)	(0.017)	(0.017)
Participant had positive pregnancy test (past 6mos)^{s}	0.09 (0.24)	0.019**	0.036***	-0.000
		(0.009)	(0.014)	(0.013)
Extent to which participant and partner want to get pregnant	2.20 (2.36)	-0.020	0.032	-0.050
		(0.078)	(0.111)	(0.108)
Use of contraception		-0.020	-0.155	0.009
		(0.060)	(0.112)	(0.079)
Efficacy of contraception used if no pregnancy desire	0.93 (0.12)	-0.011	-0.021	-0.005
		(0.013)	(0.014)	(0.022)
Participant uses contraception if no pregnancy desire	0.32 (0.47)	0.025	0.011	0.025
		(0.020)	(0.028)	(0.030)
Participant or partner had or will have an abortion (conditional on positive pregnancy test)	0.07 (0.25)	0.026	-0.012	0.048
		(0.027)	(0.021)	(0.045)
N		1976	975	965

Notes: This table reports estimated treatment effects of the guaranteed income payments on outcomes listed in the rows for participants who were residing in Texas or Illinois at baseline, as labeled. $*$ and † denote traditional and FDR-adjusted significance levels respectively. In all cases, three symbols denote the 1% level, two denote the 5% level, and one denotes the 10% level of significance of the test.

Table A30: Impact of Guaranteed Income on Birth and Fertility Outcomes by Poverty Status at Baseline

	Control Mean	Main Estimate	100% FPL+	Under 100% FPL
Number of new children (survey)	0.19 (0.47)	**-0.000**	**-0.005**	**0.002**
		(0.017)	**(0.019)**	**(0.031)**
Number of births to participants (admin)^{s}	0.14 (.)	-0.011		
		(0.016)		
Participant reported any new child 2020^{s}	0.16 (0.37)	-0.004	-0.012	-0.006
		(0.012)	(0.015)	(0.021)
Participant had positive pregnancy test (past 6mos)^{s}	0.09 (0.24)	0.019**	0.003	0.032*
		(0.009)	(0.011)	(0.017)
Extent to which participant and partner want to get pregnant	2.20 (2.36)	-0.020	0.043	-0.101
		(0.078)	(0.098)	(0.141)
Use of contraception		-0.020	0.062	-0.201
		(0.060)	(0.063)	(0.140)
Efficacy of contraception used if no pregnancy desire	0.93 (0.12)	-0.011	0.001	-0.049
		(0.013)	(0.012)	(0.040)
Participant uses contraception if no pregnancy desire	0.32 (0.47)	0.025	0.054**	-0.030
		(0.020)	(0.026)	(0.033)
Participant or partner had or will have an abortion (conditional on positive pregnancy test)	0.07 (0.25)	0.026	0.013	0.053
		(0.027)	(0.031)	(0.047)
N		1976	1241	648

Notes: This table reports estimated treatment effects of the guaranteed income payments on outcomes listed in the rows for participants who were in households earning above versus at or under the Federal Poverty Level at baseline, as labeled. $*$ and † denote traditional and FDR-adjusted significance levels respectively. In all cases, three symbols denote the 1% level, two denote the 5% level, and one denotes the 10% level of significance of the test.

Table A31: Alternative Specifications and Samples: Parental Behaviors and Investments

	Main Estimate	No Covariates	Diff-in-Diff	Lower Lee Bound	Upper Lee Bound
Parental Behavior and Investment Index	**0.04****	**0.04****	**0.03***	**0.02**	**0.07*****
	(0.01)	**(0.02)**	**(0.02)**	**(0.01)**	**(0.01)**
Parenting Quality	0.05**	0.05**	N/A	0.03	0.10***
	(0.02)	(0.03)		(0.02)	(0.02)
Monthly expenditures on children	31.10***	40.57***	31.10***	26.46***	31.90***
	(10.03)	(11.92)	(10.03)	(9.66)	(10.05)
Parental Interaction	0.02	0.03	0.01	-0.01	0.06***
	(0.02)	(0.03)	(0.02)	(0.02)	(0.02)
Parental satisfaction	0.00	-0.00	0.01	-0.03	0.06**
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Parents' stress and distress	-0.02	-0.02	-0.02	-0.02	-0.02
	(0.03)	(0.05)	(0.03)	(0.03)	(0.03)

Notes: Table presents results for alternative specifications and robustness checks. See text for more details.

Table A32: Alternative Specifications and Samples: Home Environment

	Main Estimate	No Covariates	Diff-in-Diff	Lower Lee Bound	Upper Lee Bound
Home environment Index	**-0.02**	**-0.04**	**-0.03**	**-0.03**	**0.00**
	(0.02)	**(0.04)**	**(0.02)**	**(0.02)**	**(0.02)**
Child Food Insecurity Score (0-6, higher is more insecure)	0.05	0.08	0.08**	0.06	0.02
	(0.04)	(0.05)	(0.04)	(0.04)	(0.04)
Chaos Score (0-36, higher is more chaotic)	0.00	0.15	0.04	0.18	-0.20
	(0.21)	(0.26)	(0.22)	(0.21)	(0.20)
Parent was ever unhoused in survey year	0.00	0.01	0.00	0.00	0.00
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)

Notes: Table presents results for alternative specifications and robustness checks. See text for more details.

Table A33: Alternative Specifications and Samples: Mobility and the Neighborhood Environment

	Main Estimate	No Covariates	Diff-in-Diff	Lower Lee Bound	Upper Lee Bound
Mobility and neighborhood environment Index	**0.02****	**0.02**	**0.01**	**0.01**	**0.03*****
	(0.01)	**(0.01)**	**(0.01)**	**(0.01)**	**(0.01)**
Moving behavior	0.10***	0.08*	N/A	0.10**	0.11***
	(0.04)	(0.05)		(0.04)	(0.04)
Family friendliness	-0.01	0.01	-0.02	-0.01	0.00
	(0.03)	(0.05)	(0.04)	(0.03)	(0.03)
Child-Focused Amenities	0.03*	-0.00	0.07**	0.02	0.04**
	(0.02)	(0.04)	(0.03)	(0.02)	(0.02)
Economic Mobility	-0.01	0.03	-0.02	-0.02	-0.00
	(0.02)	(0.04)	(0.03)	(0.02)	(0.02)
Census tract level pollution	0.00	-0.00	0.03	-0.03*	0.02
	(0.02)	(0.05)	(0.04)	(0.02)	(0.02)
Quality indices	-0.00	-0.03	-0.01	-0.01	0.02
	(0.03)	(0.04)	(0.04)	(0.03)	(0.02)

Notes: Table presents results for alternative specifications and robustness checks. See text for more details.

Table A34: Alternative Specifications and Samples: Non-parental care

	Main Estimate	No Covariates	Diff-in-Diff	Lower Lee Bound	Upper Lee Bound
Non Parental Care Index	**0.00**	**-0.00**	**-0.02**	**-0.04***	**0.06****
	(0.02)	**(0.03)**	**(0.03)**	**(0.02)**	**(0.02)**
Number of hours per week child(ren) are in care	0.19	-0.10	0.21	-1.06	0.67
	(0.73)	(0.92)	(0.82)	(0.69)	(0.75)
Quality of childcare	0.01	0.01	-0.08	-0.04	0.05
	(0.05)	(0.05)	(0.07)	(0.05)	(0.05)
Stability of care	-0.04	-0.08	-0.01	-0.06	0.07**
	(0.04)	(0.05)	(0.03)	(0.04)	(0.03)
Participant currently using childcare at time of survey	0.02	0.03	-0.01	0.00	0.03
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)

Notes: Table presents results for alternative specifications and robustness checks. See text for more details.

Table A35: Alternative Specifications and Samples: Stress and Social Development

	Main Estimate	No Covariates	Biological Children	Diff-in-Diff	Lower Lee Bound	Upper Lee Bound
Stress and Social Development Index	**-0.06****	**-0.07***	**-0.07****	**-0.03**	**-0.09*****	**0.02**
	(0.02)	**(0.04)**	**(0.03)**	**(0.03)**	**(0.02)**	**(0.02)**
Strengths and Difficulties	-0.05*	-0.05	-0.05*	-0.03	-0.09***	0.06**
	(0.03)	(0.04)	(0.03)	(0.03)	(0.03)	(0.02)
PROMIS mental health score (4-20)	0.21**	0.29*	0.24**	0.06	0.25**	0.07
	(0.10)	(0.15)	(0.11)	(0.19)	(0.10)	(0.10)

Notes: Table presents results for alternative specifications and robustness checks. See text for more details.

Table A36: Alternative Specifications and Samples: Child Education Outcomes

	Main Estimate	No Covariates	Biological Children	Children in HH at BL	Diff-in-Diff	Lower Lee Bound	Upper Lee Bound
Educational Outcomes (Survey, K-12) Index	**-0.02**	**-0.02**	**-0.02**	**-0.02**	**-0.03**	**-0.03**	**0.04*** **
	(0.02)	**(0.03)**	**(0.02)**	**(0.02)**	**(0.02)**	**(0.02)**	**(0.02)**
# days absent from school in the most recent school yr	-0.05	0.07	0.08	-0.01	0.13	0.05	-0.83***
	(0.35)	(0.44)	(0.37)	(0.37)	(0.39)	(0.36)	(0.30)
Disciplinary action, and help needed over past 2 years	-0.04	-0.01	-0.04	-0.04*	-0.05*	-0.04*	0.02
	(0.02)	(0.03)	(0.02)	(0.02)	(0.03)	(0.02)	(0.02)
School enrollment	-0.00	0.00	-0.00	0.01	0.01	-0.01	0.09***
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.02)
Report of child's grades (1=Mostly D's and F's, . . . , 5=All A's)	-0.02	-0.03	-0.02	-0.02	-0.02	-0.03	-0.00
	(0.03)	(0.04)	(0.03)	(0.03)	(0.03)	(0.03)	(0.02)
Perceived quality of child's education (1=Poor, . . . , 5=Excellent)	-0.02	-0.05	-0.04	-0.03	-0.05	-0.04	-0.00
	(0.04)	(0.05)	(0.04)	(0.04)	(0.05)	(0.04)	(0.04)

Notes: Table presents results for alternative specifications and robustness checks. See text for more details.

Table A37: Alternative Specifications and Samples: Pregnancy, Childbearing, and Contraception

	Main Estimate	No Covariates	Diff-in-Diff	Lower Lee Bound	Upper Lee Bound
Birth Outcomes and Fertility Index	**0.02**	**0.02**	**-0.00**	**-0.12*** **	**0.08** **
	(0.03)	**(0.03)**	**(0.03)**	**(0.02)**	**(0.03)**
Number of new children (survey)	-0.00	0.00	N/A	-0.02	0.00
	(0.02)	(0.02)		(0.02)	(0.02)
Extent to which participant and partner want to get pregnant	-0.02	-0.02	-0.01	-0.09	-0.02
	(0.08)	(0.09)	(0.08)	(0.08)	(0.08)
Use of contraception	-0.02	-0.02	N/A	-0.13*	0.15***
	(0.06)	(0.06)		(0.07)	(0.04)
Participant or partner had or will have an abortion (conditional on positive pregnancy test)	0.03	0.03	N/A	-0.07***	0.04
	(0.03)	(0.03)		(0.02)	(0.03)

Notes: Table presents results for alternative specifications and robustness checks. See text for more details.

Table A38: Difference-in-differences estimates, Educational Outcomes from Administrative Records (K-12)

Outcome	DD Coefficient	Number of Observations
Enrolled in K-12 school	-0.016 (0.020)	10248
Child repeated grade	0.003 (0.010)	9143
Child's age above expectation for grade	-0.010 (0.006)*	9146
Percentage of Days in attendance	-0.004 (0.006)	8204
Gifted and Talented Program	0.002 (0.007)	7681
Test scores: English / Language Arts Meets Expectations	-0.005 (0.031)	3135
Test scores: English / Language Arts Masters Expectations	0.028 (0.021)	3135
Test scores: Math Meets Expectations	-0.100 (0.034)***	2941
Test scores: Math Masters Expectations	-0.019 (0.018)	2941
Changed Schools	-0.023 (0.026)	7962
Changed Districts	-0.013 (0.028)	7920
School quality: Class Size	-0.102 (0.273)	7964
School quality: Average Teacher Salary	59.125 (379.732)	8262
School quality: Peccent Students Chronic Absenteeism	-0.008 (0.007)	8274
School quality: Average Attendance Rate	0.002 (0.002)	8300
School quality: Graduation Rate	0.002 (0.016)	1398
School quality: Percent Proficient in English Language Arts	-0.003 (0.008)	8124
School quality: Percent Proficient in Math	-0.011 (0.009)	8121
School quality: Value Added Estimate Math Scores	-0.048 (0.050)	7695
School quality: Value added Estimate English Scores	-0.001 (0.052)	7718

Notes: Table presents results of difference-in-differences estimates for educational outcomes derived from administrative records. See text for more details.

Table A39: Grade outcome definitions (IL sample only)

Course Grade	Grades	Meeting	Mastering	Passing
A+, A, A-	K-12	1	1	1
Above Average, Exceptional	K-8	1	1	1
B+, B, B-, C+, C, C-	K-12	1	0	1
Average, Meets Standard	K-8	1	0	1
D+, D, D-	K-12	0	0	1
Below Average, Approaching Standard, Below Standard	K-8	0	0	1
S [Satisfactory or Pass. Student received course term credit.]	K-12	NA	NA	1
WP [Withdrew from course. Student did receive course term credit.]	K-12	NA	NA	1
P [Student was promoted at end of term. (Grades K-8 only)]	K-8	NA	NA	1
F	K-12	0	0	0
U [Unsatisfactory. Student did NOT receive course term credit.]	K-12	0	0	0
I [Incomplete. Student was enrolled on Course End Date but did not receive course credit.]	K-12	0	0	0
R [Student was retained at end of term]	K-8	0	0	0
W [Withdrew from course. Student did not receive course term credit.]	K-12	0	0	0
N [Student did not complete the term. (Grades K-8 only)]	K-8	0	0	0
X [Student waived from course requirement, did not receive course credit or final grade.]	K-12	NA	NA	NA
Audit [Student Audited the Course. Student did not receive course term credit.]	9-12	NA	NA	NA
Erroneous [Record entered in error. School district mistake.]	K-12	NA	NA	NA

Notes: Table shows how different grade codings were converted into grade-related outcome variables.

Table A40: Imputed grade conversions

Assigned Grade	Imputed Score
A+	99
A	97
A-	94
Above average/Exceptional	97
B+	91.5
B	89
B-	86
Meets Standard	89
Average	86
C+	83.5
C	81
C-	78
Approaching Standard	81
D+	75.5
D	73.5
D-	71
Below Average	75.5
Below Standard	73.5
F	62.5

Notes: Table shows how different grades were converted into an imputed grade score.