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FUTURE SCHOOLING SHOCKS

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Latent Human Capital: How Early Investments in Parents Buffer Against Future Schooling Shocks

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ABSTRACT

Do early investments in parents pay off even when treatment effects for children appear to fade out quickly? We answer this question by leveraging a field experiment in which households with 3-5 year-old children were randomized into a parenting program, a preschool, or a control group, combined with a natural experiment (Covid-19 school closures) that occurred ten years later. Prior to the closures, treatment effects on academic achievement had faded to zero across all groups. Yet, with Covid-19 school closures, effects of the parenting program re-emerge: test scores in control and preschool groups drop by roughly 0.25-0.30 SD, whereas scores in the parenting group drop by only 0.11 SD (difference: $p = 0.01$). Our findings suggest that parenting programs can generate latent human capital that only becomes visible when complementary school inputs are removed. This result is consonant with parental and school investments acting as substitutes in skill production. Because this latent parental capacity pays off precisely when school inputs fail, standard measurement tools for early childhood programs can miss crucial state-contingent value.

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1 Introduction

The global investment in early childhood programs is enormous and growing. Today, motivated by a body of evidence that early investments in children yield high returns (see, e.g., Heckman, 2006; Heckman et al., 2013; Yoshikawa et al., 2013; Elango et al., 2015), governments annually invest billions on early education programs. At the center of this investment is parents. Indeed, a growing literature recognizes that parents are a critical input in the education production function, particularly in the early years when the returns to child investment are thought to be greatest (Francesconi and Heckman, 2016; List et al., 2018; Almond et al., 2018). Programs that provide parenting education or give incentives to parents for investing in their children have resulted in short-term changes in children’s academic skills and executive functions (Fryer et al., 2015; Chuan et al., 2021; Cassidy et al., 2017; Lunkenheimer et al., 2008; Sheridan et al., 2011; Schaub et al., 2019; Özler et al., 2018; List et al., 2021; García and Heckman, 2022).

The literature’s most robust finding, however, is also its most troubling: the effects of many early childhood interventions are found to fade-out within a few years of program completion (Campbell et al., 2001; Barnett, 2011; Duncan and Magnuson, 2013; Almond et al., 2018).¹ Children who showed promising gains in preschool look indistinguishable from their peers by the time they reach elementary school. If the effects from early education programs depreciate considerably, then the case for large-scale public investment in early childhood weakens (or at minimum demands that programs be paired with sustained follow-up complementary interventions).

An important consideration, however, is what exactly fade-out means. Under the standard interpretation, fade-out means that the human capital built by treatment depreciates, and it is usually measured by student test scores. In that sense, with considerable fade-out, the case for large-scale public investment diminishes accordingly. But there is a second possibility. The program’s value might manifest where evaluators are not looking or in states of the world that they are not observing. For example, if a program builds human capital elsewhere in the household, and if that capacity substitutes for school inputs, then its payoff arrives precisely when necessary. This is a state of the world that few evaluations have visited or have even discussed. Under this interpretation, a decade of flat treatment effects reveals the properties of the situation in which outcomes were measured, not the durability of the underlying investment. Distinguishing these interpretations requires something evaluators cannot ordinarily engineer: the removal of a dominant input from every child’s production

¹For recent reviews see also Elango et al., 2015; Abenavoli, 2019; Bailey et al., 2020; Bruhn and Emick, 2023.

function at once. The Covid-19 pandemic did exactly that.

Our study begins by developing a theoretical framework that formalizes this intuition and generates conditions under which treatment effects appear, fade out, and reappear over the lifecycle. While our theory is general, we stylize it within early education, linking it closely to our own early education field experiment (the Chicago Heights Early Childhood Center (CHECC)), which we detail in this study. CHECC, which began in 2010, included two major treatment arms: a parent program and a preschool program for 3-5 year olds. The parent program consisted of bi-monthly classes for parents on how to teach to their children at home and provided incentives to parents for doing so.

A central tenet of the parent program was the concept of ‘parents as their child’s first teacher’ - that is, parents were encouraged to think about their investments in their child as central to the education production function. Importantly, within the parenting intervention families were further randomized into one of two groups. In one group, families received incentives as immediate cash payments. In the other (the *College* arm), they received cash deposits into a college savings account, accessible only upon postsecondary enrollment. This design choice was meant to create variation in the incentive to sustain parental investments that persisted beyond the program itself.

Critically for our purposes, about 10 years after the CHECC intervention, a natural experiment occurred: Covid-19 school shutdowns, which affected all of our CHECC students. Through the lens of our theoretical model, these shutdowns abruptly removed the dominant school input for children around the globe. Our model shows that by pairing our field experiment (which induced variation in parental investments) with a shock that stripped away the masking input, we are provided with a rare opportunity to learn about deep aspects of the education production function.

Our empirical results fall into two categories. In our first category we show short-term impacts of our program. As reported in prior work, the parenting program had short-term impacts on children’s executive functioning skills but not on their academic skills (Fryer et al., 2015). The preschool program had short-term impacts on children’s academic skills but not on their executive functions (Fryer et al., 2020). Consistent with the broader literature, the effects of both programs faded out over time (see Castillo et al., 2024; List and Uchida, 2024).² Indeed, by the beginning of the Covid-19 pandemic, the treatment and control groups were similar on standardized test scores and other measured dimensions.

²List and Uchida (2024) show that fade-out in this setting is partly an equilibrium phenomenon: CHECC gains persist among children who share classrooms with other treated children and erode when treated children are dispersed among untreated peers. We view our study and theirs as strong complements, as they both reveal that the trajectory of early gains depends critically on the post-treatment environment. In their study, through the composition of peers, in ours through the presence of school inputs.

In a second category, we provide evidence on whether CHECC generated lasting human capital that is latent rather than lost. If the parenting program built latent parental capital that was simply masked by school inputs, then the removal of those school inputs should reveal such capacity. If, instead, the treatment effects were truly zero, the parenting and control groups should experience similar learning losses. As the theory shows, part of the empirical difficulty is that once children enter school, the school system provides a large, common input to all children. If schools are doing their job, they may absorb enough of the variation in child outcomes that differences in parenting — even real, persistent differences induced by an intervention — become undetectable in standard measures of academic achievement.

We find that the effects of the parenting program re-emerge during the Covid-19 school closure. Specifically, parental investments induced by the parenting program seemingly shield students against the negative shock of Covid-19 school closures. While test scores in the control group drop by 0.30 standard deviations during the closures, test scores in the parenting group drop only by 0.11 standard deviations, a statistically significant difference ($p = 0.01$). The protective effect is driven by the college arm of the program, which incentivizes continued investments until the child graduates high school. Alternatively, the preschool program does not have a protective effect. Test scores among those assigned to the preschool program drop by 0.25 standard deviations, which is statistically indistinguishable from the control group ($p = 0.61$).

We view our work as contributing to several branches of the literature. First, our study joins a growing literature on the role of parents in learning losses children experienced during the Covid-19 pandemic. The school closures result in a widening of the skills gap between low and high socio-economic status (SES) students, due partly to differences in parents' ability to work from home (Agostinelli et al., 2022) and partly to the lower likelihood of low SES parents seeking outside tutoring resources (Bacher-Hicks et al., 2021). Closest to our work, Hassan et al. (2024) show that randomly assigning mothers in Bangladesh to a tele-mentoring intervention during the pandemic improved their children's skills, demonstrating that strengthening parental capacity in real time can offset pandemic-related learning losses. We show that a similar protective effect can emerge from investments in parental human capital made a full decade earlier, suggesting that the relevant skills are durable rather than requiring contemporaneous activation.

Second, we contribute to the literature on the long-run effects of early childhood interventions and the puzzle of fade-out. A well-documented pattern in this literature is that early gains from preschool and parenting programs dissipate within a few years of program completion (Almond et al., 2018; List and Uchida, 2024). Some influential studies have found

that treatment effects reappear later in life on outcomes such as employment, earnings, and criminal justice involvement (Heckman et al., 2013; García and Heckman, 2022), but the underlying mechanisms causing this disappearance and reappearance remain poorly understood. We produce both theory and empirical evidence for one such mechanism: treatment can build parental capacity whose payoff is state-contingent. It is invisible while school inputs are abundant, and revealed only when they are disrupted. In this manner, our results serve to reframe fade-out: the convergence of children’s test scores is real, yet, the inference that early investment therefore has no lasting value is incorrect. This is because such inference conflates the child’s measured skills with the household’s capacity to produce them. Standard evaluations, conducted only in states where schools function, effectively give zero value to the insurance.

Third, we contribute to the literature on the technology of skill formation (Cunha and Heckman, 2007; Heckman, 2007). A central question in this area concerns the substitutability of parent, school, and child inputs. The usual approach in the literature is to recover the parameters using cross-sectional variation in inputs or from structural estimation. We take an alternative approach, using a design that delivers exogenous variation in both arguments of the production function. Specifically, we controlled the assignment mechanism of parental investment, and nature controlled the complementary schooling input. The framework that motivates our empirical approach, formalized in Section 2, also yields a broader methodological insight: in any multi-input production function, the value of a minority input can go undetected while a dominant input is operating. This is true even if the treatment effects on the minority input are real and persistent. This implication travels well beyond education. Indeed, wherever interventions target one of several substitutable inputs, evaluations conducted in input-rich environments will understate their value.

Fourth, for policymakers, we view our findings as carrying two messages. The first message is an optimistic one: investments in parents during early childhood may be more valuable than the extant literature suggests. This is because such investments produce latent human capital: returns manifest in environments that most evaluation designs never observe. The second message takes a more cautionary tone: the CHECC program’s shielding effects were primarily focused in the treatment arm that maintained ongoing incentives for parental investment. This result suggests that one-time interventions without sustained incentive structures may be insufficient to keep parents engaged over the long run. The implication is that decision-making in this area is much more complex than: should we invest in parents? An affirmative response to this question must be paired with a first order design problem: what is the parental incentive structure of the program?

The remainder of our paper proceeds as follows. Section 2 provides a conceptual frame-

work, showing the difficulties facing standard evaluation. Section 3 provides our field experimental methods. Section 4 provides the results and Section 5 concludes.

2 Conceptual Framework

We develop a framework of child skill formation that generates predictions about when and why parental treatment effects appear, disappear, and reappear. The framework builds on the technology of skill formation in Heckman (2007), Almond et al. (2018), and List (2026) but we extend it to our environment. Specifically, while the model is general, we provide a tight link to the CHECC field experiment in our motivation. As discussed more fully below, CHECC includes two treatment arms (a parent program and a preschool program) and within the parent program, we vary the incentives to invest in their child in the long-run. This allows us to characterize how the *detectability* of parental treatment effects depends on the level and changing of complementary school inputs.

2.1 Production Function

Let child skills M_{it} be a vector of skills and economic preferences for individual i at time t . Following the literature, we model child skill development as a function of the child’s skills in the previous period, individual characteristics, and investments in the given period from the child, the child’s parents, and the child’s school:

$$M_{it} = f(M_{it-1}, Z_i, C_{it}, P_{it}, S_{it}) \quad (1)$$

where M_{it-1} is child i ’s skills in the prior period, Z_i are individual characteristics, and C_{it} , P_{it} , and S_{it} are inputs from the child, parent, and school, respectively.

We adopt a constant elasticity of substitution (CES) specification, which allows us to capture substitutability among inputs:

$$M_{it} = g \left(M_{it-1}, Z_i, A \left(\gamma_1 C_{it}^\phi + \gamma_2 P_{it}^\phi + \gamma_3 S_{it}^\phi \right)^{1/\phi} \right) \quad (2)$$

Here A represents total factor productivity, $\gamma_j > 0$ for $j \in \{1, 2, 3\}$ with $\gamma_1 + \gamma_2 + \gamma_3 = 1$ indicates the relative importance of each input, and $\phi \leq 1$ governs the elasticity of substitution $\sigma = 1/(1 - \phi)$. When $\phi > 0$ (i.e., $\sigma > 1$), inputs are gross substitutes: a reduction in one input can be offset by increases in another.

For the analysis that follows, to focus on the elements of greatest import, we suppress

M_{it-1} , Z_i , and A , and define the composite input:

$$Q = (\gamma_1 C^\phi + \gamma_2 P^\phi + \gamma_3 S^\phi)^{1/\phi} \quad (3)$$

so that child skills are monotonically increasing in Q .

2.2 Input Shares and the Detectability of Treatment Effects

Consider a parenting intervention that raises P by some amount ΔP . How does the detectability of this treatment effect depend on the level of school inputs S ? At first glance, the CES framework might suggest that detectability is increasing in S . The cross-partial derivative of Q with respect to parental and school inputs is positive for all $\phi < 1$:

$$\frac{\partial^2 Q}{\partial P \partial S} > 0 \quad (4)$$

Equation (4) shows that higher school inputs raise the marginal product of parental inputs, which might suggest that treatment effects from differences in P are most detectable when schools are operating normally. Whether this intuition holds, however, depends on what we mean by a “detectable effect”.

To see this, recall that treatment effects are measured in standard deviation units. That is, as a proportion of the variation in outcomes across individuals. As such, what matters for detection is not the marginal product of P in levels. It is the *share* of total output variation driven by differences in P . We therefore define the parental input share as:

$$s_P \equiv \frac{\gamma_2 P^\phi}{\gamma_1 C^\phi + \gamma_2 P^\phi + \gamma_3 S^\phi} \quad (5)$$

This share captures the proportion of total composite input driven by parental investments. Importantly, the detectable treatment effect scales with s_P , not with the marginal product of P .

Proposition 1. *Consider two groups: treated (T) and control (C). The two groups differ only in parental inputs: $P_T = P_0 + \Delta P$ and $P_C = P_0$, with isomorphic child and school inputs. The proportional difference in composite inputs is thus approximately:*

$$\frac{Q_T - Q_C}{Q_C} \approx s_P \cdot \frac{\Delta P}{P_0} \quad (6)$$

for small $\Delta P/P_0$. When inputs are substitutes ($\phi > 0$), s_P is decreasing in S :

$$\frac{\partial s_P}{\partial S} = \frac{-\gamma_2 \gamma_3 \phi P^\phi S^{\phi-1}}{(\gamma_1 C^\phi + \gamma_2 P^\phi + \gamma_3 S^\phi)^2} < 0 \quad (7)$$

This shows that higher school inputs compress the detectable treatment effect toward zero.

Proof. A first-order expansion of Q_T around P_0 gives $Q_T \approx Q_C + \gamma_2 P_0^{\phi-1} Q_C^{1-\phi} \cdot \Delta P$. Dividing by Q_C and simplifying yields $s_P \cdot (\Delta P/P_0)$. The sign of $\partial s_P / \partial S$ follows directly from differentiation. $\square$

Proposition 1 reveals that the standard marginal product intuition is misleading. A simple example will suffice. Consider a classroom of 20 students with a professional teacher spending six hours a day on instruction. In this case, the difference between a parent who reads with their child at night and one who does not is swamped by the large common school input. If we remove the classroom, that parental difference then becomes the primary driver of variation in learning. Of course, while the marginal product of P may be higher when S is large, the parental input constitutes a smaller share of total production. The treatment effect ΔP is embedded in Q , but when S dominates, that effect is a vanishingly small fraction of the total.

If we now consider the case of Covid-19 school closures, this causes S to drop to zero, which in turn causes s_P to increase substantially since:

$$\lim_{S \rightarrow 0} s_P = \frac{\gamma_2 P^\phi}{\gamma_1 C^\phi + \gamma_2 P^\phi} \quad (8)$$

and the treatment effect ΔP becomes the dominant signal in the outcome distribution. The parental treatment effect is *latent*—embedded in the production function all along—but is undetectable when school inputs are large.

2.3 Endogenous Parental Response

Our simple production function approach does not capture certain key elements of what potential investments occur alongside school closures. To make a crucial step in that direction, we must account for the possibility that treated parents *increase* their investments when school inputs drop. We model parental investment as a choice variable. Each period, parents choose P to maximize child skills net of the cost of parental time:

$$\max_P Q(C, P, S) - c(P) \quad (9)$$

where $c(P)$ is increasing and convex. This shape reflects the opportunity cost of parental time and effort. The first-order condition implicitly defines the optimal parental investment P^* :

$$\gamma_2 P^{\phi-1} Q^{1-\phi} = c'(P) \quad (10)$$

When S drops, the left-hand side increases for $\sigma > 1$, and parents optimally raise P^* . That is, $dP^*/dS < 0$ when inputs are gross substitutes. Parents respond to school closures by investing more.

We argue that programs, such as CHECC’s Parent Academy, affect this response via two channels. First, it operates through a skill (or constraint) channel. The program can reduce the cost $c(P)$ of augmenting their child’s human capital. This can occur by teaching parents effective strategies for investing in their child’s learning. For example, a treatment group parent who learns to be proficient in teaching letter recognition or counting faces a lower marginal cost of productive investment than a control group parent. Second, a belief channel: the program’s emphasis on parents as their child’s “first teacher” shifts parents’ perceived importance of their own input—effectively raising their subjective γ_2 . Parents who internalize this philosophy view themselves as central to the education production function, which steepens their investment response when school inputs are disrupted.

Let $P_T^*(S)$ and $P_C^*(S)$ denote the reaction functions of treated and control parents. The treatment effect on child skills at school input level S is:

$$\tau(S) = Q(C, P_T^*(S), S) - Q(C, P_C^*(S), S) \quad (11)$$

Proposition 2 (Reappearance of Treatment Effects). *Suppose (i) inputs are gross substitutes ($\sigma > 1$), and (ii) school inputs S are sufficiently large in normal times that s_P is small. Then:*

- (a) $\tau(S_{high})$ is small and may be statistically undetectable;
- (b) $\tau(S_{low})$ is large and detectable;
- (c) $\tau(S_{low}) > \tau(S_{high})$ even if $P_T^*(S_{high}) - P_C^*(S_{high}) = P_T^*(S_{low}) - P_C^*(S_{low})$;
- (d) If $|dP_T^*/dS| > |dP_C^*/dS|$, then the treated–control input gap widens as S falls:

$$P_T^*(S_{low}) - P_C^*(S_{low}) > P_T^*(S_{high}) - P_C^*(S_{high}),$$

and the reappearance in part (b) is amplified relative to the constant-gap benchmark in part (c).

Within this set of findings, the contrast between parts (c) and (d) is worth emphasizing. Part (c) reveals the power of the input share mechanism, showing that the treatment effect can reappear without any further change in parental inputs. Part (d) shows that the reappearance will be amplified if we assume that treated parents have a greater response to drops in S than control parents. The mechanism operates through the skill and belief channels, as treated parents face a lower cost $c(P)$ of productive investment and hold an elevated perceived γ_2 . Part (d) thus provides the theoretical basis for our hypothesis that treated parents differentially increase investment during closures.

One implication of Propositions (1) and (2) is a reinterpretation of how we interpret fade-out effects in the early childhood literature. What appears to be depreciation of treatment-induced human capital may partly be an equilibrium phenomenon: when schools provide a dominant common input, treatment-induced differences in parental behavior are inframarginal. In this case, the human capital is latent rather than lost. It becomes visible based on the properties of the situation. Whenever the institutional environment changes in a way that raises the parental input share, whether through school closures, summer breaks, or transitions between educational settings, the latent human capital shows itself.

3 Methods

3.1 Chicago Heights Early Childhood Center

We leverage the Chicago Heights Early Childhood Center (CHECC), a field experiment that was conducted in 2010-2014 (Fryer et al., 2015). CHECC was located in Chicago Heights, IL, a relatively low-income, high-minority south-side suburb of Chicago. The field experiment focused on evaluating the impact of various interventions with 3-5 year old children with the aim of reducing the academic achievement gap. Households who registered for CHECC in 2010 and 2011 were randomized to one of several programs, including a control group and a Parent Academy group aimed at teaching parents how to teach to their children.

Households participated in CHECC for 1-2 years, depending on the year they enrolled and the age of their child. The control group did not receive any educational interventions from CHECC, but did participate in periodic assessments and surveys. The Parent Academy group received educational workshops and incentives for investing in their child. In each year, there were 18 Parent Academy workshops and 17 homework assignments that involved teaching activities parents undertook with kids at home. Parent Academy lessons focused on teaching parents how to help their children with both academic skills (such as spelling and counting) as well as executive functioning skills (such as working memory and inhibitory

control). The lessons also emphasized the parent’s role as the “child’s first teacher.”

Parents received incentives for participating in the Parent Academy. They could earn up to \$6,900 per year, which amounted to over a quarter (27.6%) of median household income for the analysis sample. These earnings were based partly on attendance and partly on performance. All Parent Academy families received immediate cash incentives (\$100) for attending each workshop. In addition, families received performance incentives for completing homework assignments, evaluations, and for their child’s scores on assessments. Half of Parent Academy families were randomized to receive these incentives immediately in cash (Parent Academy-Cash), whereas the other half were randomized to receive these incentives in a savings account that would become accessible if and when the treatment child attended post-secondary education years later (Parent Academy-College).

Parents and children participated in an assessment and survey before, during and immediately after treatment. The assessment evaluated child academic skills (reading, writing and math) using the Peabody Picture Vocabulary-III test (PPVT-III, Dunn and Dunn (1965)) and the Woodcock-Johnson-III test (WJ-III, Woodcock, McGrew and Mather (2001)). The assessment also evaluated child executive functioning skills (working memory, inhibitory control and emotions) using tests developed by Blair and Willoughby (2006a, b) and the Preschool Self-Regulation Assessment (PSRA). The parent survey included questions about household socio-demographics, as well as questions about parent investments (time spent teaching to the child), parent self-efficacy (confidence in teaching) and beliefs about child abilities (see Appendix A).

Related papers have summarized the impact of Parent Academy on child short-term outcomes before Covid-19. Castillo et al. (2024) showed that Parent Academy affected executive functioning skills immediately after the program, but did not have medium-term effects on test scores, grades or disciplinary referrals in grades K-8³. Chuan et al. (2022) discussed the association of early childhood parental investments in reading and math on test scores in middle childhood. Different from these papers, we evaluate the impact of Covid-19 school closures on children from each of the treatment groups.

3.2 Negative Schooling Shock During Covid-19

In response to the Covid-19 pandemic, the state of Illinois moved public schools to fully remote instruction in March of 2020 through the end of the 2019-2020 academic school year. The remote model included synchronous Zoom instruction and asynchronous components. Some students received Chromebooks, iPads, or other electronic devices from the district

³In Appendix Table A6, we replicate these findings using our final analysis sample.

to participate in remote instruction. Available technology and school resources varied by district.

During the 2020-21 academic school year, instructional mode was decided at the district level, with substantial variation in remote vs. in-person learning across the state. Some districts began a hybrid model before returning to full in-person learning, which included students attending in-person for part of the week and learning asynchronously for part of the week. Attendance was taken as usual, but there was no consequence for extensive absences in most districts. Standardized tests were also canceled by most districts in spring 2021. In general, schools returned to in-person learning in fall 2021, although some districts employed a delayed start due to a new Covid-19 wave.

3.3 Standardized Test Scores

Following participation in CHECC, children matriculated into public or private schools in Chicago Heights, IL and surrounding areas. We obtained administrative data on Illinois Assessment of Readiness (IAR) scores on all children in the Illinois public schools from the state of Illinois. The public schools administer the IAR test each spring when the children are in grades 3-8. IAR is administered to all public school children that are present at the time of testing. Districts are not permitted to allow students to opt out of taking the test, although in practice some students do opt out, i.e., by not showing up on the testing date. The IAR test includes an assessment of skills in English and Language Arts (ELA) and Mathematics (MAT) and is based on the Common Core State Standards in Illinois. Each student's raw score is converted into a scale score which ranges from 650 to 850, by adjusting for slight differences in difficulty among the forms and administrations of the test. Students are also assigned a performance level, which is a categorical level defined by the scale score. Students with scale scores below 700 have not yet met expectations for their grade level, between 700 and 725 have partially met expectations, between 725 and 750 have approached expectations, and above 750 have either met or exceeded expectations.

Prior to shutdown, in spring 2018-2019, standardized tests were administered as normal. Testing was disrupted in the spring of 2020 when schools shut down. Due to schools providing education in a variety of methods during spring 2021, test administration did not return to normal. ISBE allowed districts two testing windows in either spring or fall 2021, but scores were categorized only as spring 2021. Districts had to decide the best way to execute testing and arrange multiple opportunities to test students. After connecting with multiple districts, it appeared that if students were learning remotely during the spring, they requested parents to make an appointment to test, often on Saturdays. This resulted in many students not

testing due to parents not showing for their assessment appointment or opting out of testing this year. One district shared families had to opt in to test during this time period. If students were hybrid learning, districts attempted to test when in school. A large number of students did not test in spring 2021 and these scores were reported as “NULL” or “absent from testing”. In 2022, all districts returned to typical testing procedures, as students were in attendance and remote learning was not an option. Therefore, we use the 2018-19 data as our measure of pre Covid-19 scores and the 2020-2021 (when available) and 2021-2022 data as our measure of post Covid-19 scores.

3.4 Analysis Sample

A total of 2,185 children were randomized in the CHECC field experiment. The focus in our study is the Parent Academy program and its relevant control group, which includes 863 children (316 in Parent Academy and 547 in Control) who were randomized to the program in 2010 and 2011. We limit our analysis to children who were in the 3rd-8th grades in both 2018-19 (pre Covid-19) and 2021-22 (post Covid-19), since IAR scores are only collected within this age range. This gives us a total of 676 children (240 in Parent Academy and 436 in Control) who are eligible for analysis. However, data on IAR scores in both the pre Covid-19 and post Covid-19 periods are only available for 338 (50.0%) of these children (136, 56.7% for Parent Academy and 202, 46.3% for control) either due to students exiting the public school system or due to imperfect administration of the standardized test by the district (especially in the postCovid-19 period).

Out of the original sample of 316 in Parent Academy and 547 in Control during 2010-2011 CHECC years, 43% of Parent Academy children remain in the sample, compared to 37% in the control group ($p = 0.08$). In Appendix Tables A1 and A2, we test for demographic differences between the final sample and the attrited group for both the control group and Parent Academy, respectively. Differences exist in part because districts experienced differential levels of success in administering the IAR test to all students. Due to these differences, we use inverse probability weights (IPW) to account for socio-demographic differences between our initial CHECC sample and the sample available for analysis.

Table 1 provides demographic summary statistics of our analysis sample by treatment. Similar to the surrounding community in Chicago Heights, our sample is lower-income and has a higher representation of racial minorities than the general US population. More than a third of the children in our sample come from families with an annual income of less than \$16,000, and less than a third have an annual family income greater than \$36,000. About 10% of our sample is white and 90% is Black or Hispanic (< 1% is another race).

We compare students in the Parent Academy treatment and the control group across various demographics to check that our sample is balanced. There are some small differences in characteristics across our treatment and control groups. For example, the control group is slightly more Black ($p = 0.12$) and less Hispanic ($p = 0.22$) than the Parent Academy group. However, none of the differences are statistically significant. The lack of significant differences by treatment suggests that the samples are sufficiently comparable for our analysis.

[INSERT TABLE 1 HERE]

3.5 Hypotheses

The theoretical framework in Section 2 generates several testable hypotheses that relate to our CHECC design.

Hypothesis 1. *Children randomized to the Parent Academy program will experience less learning loss during the Covid-19 school closures than children in the control group. Children randomized to the preschool program will not experience less learning loss than the control group.*

We denote this first hypothesis as the “Buffering Effect” of investments in parents. It follows directly from Propositions (1) and (2). The Parent Academy alters both $c(P)$ and perceived γ_2 for treated parents. When S dropped during Covid-19, the parental input share s_P increases. Now, differences in P between treated and control families become the key source of variation in child outcomes. In contrast, the preschool program affects S by providing school-like inputs directly to children. As such, it does not target parental investments. Because of this, its effects should not reappear when S drops, since the program did not alter $P_T^*(S)$.

Hypothesis 2. *The buffering effect will be stronger for children in the Parent Academy-College arm than in the Parent Academy-Cash arm.*

This hypothesis can be denoted as the “Incentive Effect” of parent investments. The College arm deposits performance incentives into a savings account accessible only upon postsecondary enrollment. Because the children in our sample are in middle school during Covid-19, the College incentives remains active. More specifically, parents and children in the College arm have an ongoing financial stake in academic performance. Through the lens of our model, this incentive effect in the College arm causes a steepness in the reaction function: the perceived γ_2 remains elevated and the returns to parental investment remain salient long

after the formal experimental treatment ends. Alternatively, for those in the Cash arm, performance incentives are distributed years earlier. The implication is that there is no ongoing mechanism to maintain the elevated reaction function in our model. We therefore expect $|dP_{College}^*/dS| > |dP_{Cash}^*/dS|$, yielding a larger protective effect in the College arm.

Hypothesis 3. *Within the Parent Academy treatment, the buffering effect will be increasing in program engagement intensity (workshop attendance, homework completion rates).*

We denote this hypothesis as the “Dose-Response” of inputs relationship. While there is, of course, endogeneity of parental engagement, this hypothesis is of the learning by doing nature. Parents who engage more intensively with the program receive a larger reduction in $c(P)$, and a larger shift in perceived γ_2 . If the protective effect operates through the skill and belief channels we propose (rather than simply through selection of inherently more motivated families into the treatment), then we should observe a dose-response relationship between engagement and the protective effect during Covid-19.

Beyond our specific exploration of human capital investment, the input share mechanism provides a universal lens for reinterpreting measurement approaches across the life cycle. Indeed, the latent capital idea is pervasive in fields ranging from corporate management to public health. As one example, consider workplaces that are highly structured, such as Walmart. If an employee has deep initiative (P), it might be the case that it is swamped by rigid operating procedures (S) that have been in place for fifty years. Yet, when a reorganization happens, the deep employee initiative can resurface as a primary driver of higher performance. Similar mechanisms can happen in personal healthcare. A patient may invest in health behaviors (P) that are undetectable under medical supervision (S). Yet, those investments are detectable when the medical supervision is withdrawn. The underlying thread that connects these examples is that whenever multiple inputs interact in a production function, some inputs can be undetected. In such cases, with a recognition that measured treatment effects are often latent rather than lost, researchers can identify a wide array of stress tests. Technological shifts, policy transitions, or economic shocks can all be leveraged to reveal the true value of earlier interventions that appeared to have had little impact.

4 Results

4.1 Overview and Short-Term Effects

We first provide a descriptive analysis of our sample. During the pre Covid-19 year (2018-19), the students in the control group are in grades 4-6 and have an average IAR score of

723.4 which is within the performance level “Approaching Expectations”. Figure 1 compares the performance of the CHECC control group to all students taking the IAR in Illinois. In Mathematics (MAT), 13% of CHECC control students have either met or exceeded expectations in 2019, compared to 31.8% of students in the state. In English and Language Arts (ELA), 27.6% of CHECC control students either meet or exceed expectations, compared to 37.8% of students in the state.

[INSERT FIGURE 1 HERE]

During the post Covid-19 year (2020-21), the students in the control group are in grades 6-8 and have an average IAR score of 714.7. This represents a 0.30 SD decrease in the score due to Covid-19 learning loss relative to pre Covid-19 test scores. By comparison, related work shows Covid-19 learning loss between 0.08 and 0.14 SD in the United States (Betthäuser et al., 2023) and 0.08 SD in the Netherlands (Engzell et al., 2021).

Covid-19 learning loss in our sample is substantially larger than the literature has found in other settings. This is likely due to the inequitable effects of Covid-19 on learning loss by race and socioeconomic characteristics of students (Goldhaber et al., 2022). Our sample is heavily Black, Hispanic, and low-income relative to the US population. This is consistent with the demographic makeup of Chicago Heights and surrounding areas where CHECC took place.

4.2 Determinants of Covid-19 Learning Loss

In Table 2, we regress Covid-19 learning loss on treatment assignment, performance pre Covid-19, and various demographic and socioeconomic characteristics. Within our sample, average learning loss is 6.7 points using the scale score with a standard deviation of 19 points. From this table, we first document several determinants of Covid-19 learning loss. Girls exhibit marginally significantly less learning loss compared to boys in our sample. This contrasts with evidence from Engzell et al. (2021) and Orlov et al. (2021) which found no gender differences in student learning in the Netherlands and among a sample of US college students, respectively. This also contrasts with evidence from Wu et al. (2022), which found that girls experienced greater learning loss during Covid-19 than boys in North America.

In our sample, we find that relative to Black students, Hispanic students experience lower levels of learning loss. This finding is consistent with Sass and Ali (2022) and Lewis and Kuhfeld (2021) in that Black students seem to experience the most Covid-19 learning loss. We do not replicate the finding that Black students experience less learning loss than White students, potentially due to our small sample of White students. Evidence from Goldhaber et al. (2022) suggests that within schools, there are no learning loss differences by race.

[INSERT TABLE 2 HERE]

Mother’s educational attainment is predictive of Covid-19 learning loss in our sample. Students with less educated mothers tend to experience greater learning loss than students with more educated mothers. Relative to students with mothers who do not have a high school degree, having a mother with a high school degree is associated with a 0.23 standard deviation reduction in Covid-19 learning loss. Having a mother with a Bachelor’s degree is associated with a reduction in learning loss of 0.36 standard deviations. After controlling for mother’s education, we see no differences in Covid-19 learning loss by parental income. Engzell et al. (2021) found that having a parent with low levels of educational attainment was associated with an increase of Covid-19 learning loss by 40% in the Netherlands. In comparison, our estimates suggest that having a mother with a high school degree is associated with a 53% less Covid-19 learning loss compared to having a mother without a high school degree.

Students who have higher standardized test scores pre Covid-19 experience greater learning loss during Covid-19 ($p = 0.005$). This pattern could arise through a few different pathways. Given our sample of students score on average lower than the general population in Illinois taking the IAR exam, we first consider the possibility that higher performing students experience a greater drop in test scores than lower-performing students because lower-performing students bottom-out. That is, perhaps lower-performing students’ scores can not decrease much further, whereas higher-performing students have more space for downward movement.

To evaluate this explanation, we graph separately the distributions of pre Covid-19 IAR scores and post Covid-19 IAR scores within our sample. Suppose lower-performing students in 2019 have less space for their scores to decrease. If this were the case, then we would expect the post Covid-19 distribution to be shifted to the left for higher scores, and less shifted for lower scores. In Appendix Figure A1, we see that this is not the case. IAR scores shift left across the distribution post Covid-19, suggesting that lower-performing students’ scores do not bottom-out prior to Covid-19.

Another explanation is that higher-performing students experience greater learning loss due to mean reversion in test scores. Student test scores are a noisy signal of true academic ability (Chay et al., 2005). That is, performance in any given period contains random errors such that some students’ scores are higher than their true ability, and other students’ scores are lower than their true ability. As a result of the randomness of these errors, students who experience a positive error in one year are more likely to experience a test score drop the following year, and students who experience a negative error are more likely to experience an increased test score the following year. This is referred to as mean reversion in test scores.

Mean reversion may explain the positive relationship between Covid-19 learning loss and pre Covid-19 test score. Following Chay et al. (2005), we construct a placebo test. Appendix Table A7 contains two models. The first is our main model of Covid-19 learning loss restricted to students with scores in 2019 and 2021. The second model is our placebo test, in which we calculate learning loss between 2017 and 2019. For both models, we regress learning loss on a “Normalized Pre Score” (students’ 2019 scores in the first model; students’ 2017 scores in the second model). The relationship between learning loss and pre-score is stronger in the placebo model than in our Covid-19 learning loss model ($p = 0.006$). We conclude that the greater learning loss among students who scored higher in 2019 may be a result of mean reversion, as opposed to a greater impact of Covid-19 for higher-performing students relative to lower-performing students.

4.3 Treatment Effects on Covid-19 Learning Loss

We now look at how treatment assignment in the CHECC program affects Covid-19 learning loss in our sample, mapping back to our hypotheses from Section 3.5. Figure 2 shows average Covid-19 learning loss (the difference between post Covid-19 and pre Covid-19 standardized test scores) by treatment group. On average, the control group loses 0.30 standard deviations post Covid-19 relative to their pre Covid-19 test scores. Students assigned to the Parent Academy experience a learning loss of 0.11 standard deviations, which is significantly smaller than the learning loss among the control group ($p = 0.01$). Students assigned to the Preschool program experience a Covid-19 learning loss of 0.25 standard deviations, which is not statistically significantly different from the learning loss of the control group ($p = 0.61$).

[INSERT FIGURE 2 HERE]

As Table 2 shows, Covid-19 learning loss is partially mitigated by prior assignment to Parent Academy. Assignment to Parent Academy decreases learning loss by 0.13 standard deviations after controlling for student and parent-level demographic information. This effect size is approximately half of the effect size of having a mother with a Bachelor’s degree compared to having a mother without a high school degree. Prior assignment to the preschool program, on the other hand, has no detectable impact on Covid-19 learning loss. To account for both hypotheses - Parent Academy vs. Control and Preschool vs. Control - we adjust for multiple hypothesis testing in Appendix Table A3. Significance of the Parent Academy coefficient remains using the family-wise error rate. In Appendix Table A8, we also use inverse probability weights (IPW) to account for socio-demographic and performance differences between our analysis sample and the sample available for analysis.

Result 1. *Assignment to the Parent Academy during the CHECC intervention period (2010-2011) reduces Covid-19 learning loss by 0.13 standard deviations ($p = 0.01$) relative to the control group.*

As discussed in Section 1, all Parent Academy families receive participation incentives for attending workshops and performance incentives for completing homework assignments and for their child’s scores on assessments. Half of Parent Academy families are randomized to receive performance incentives immediately in cash (Parent Academy-Cash), whereas the other half are randomized to receive these incentives in a savings account that will become accessible if and when the child attends post-secondary education years later (Parent Academy-College).

Returning to our hypotheses from Section 3.5, we test whether the impact of Parent Academy on Covid-19 learning loss differs based on incentive scheme by regressing learning loss on assignment to Parent Academy-Cash and Parent Academy-College separately. Because the students in our sample are in middle school, we may expect the college incentives to be more relevant than the cash incentives because the college incentives are only obtainable if the child attends post-secondary education, while the cash incentives were already distributed years prior. As a result, the college incentives give parents and children an added incentive to perform well academically, while cash incentives do not.

[INSERT TABLE 3 HERE]

As reported in Table 3, assignment to both Parent Academy-Cash and Parent Academy-College treatments results in less learning loss. After controlling for demographics and socioeconomic indicators, the effect of Parent Academy-Cash remains negative, but is no longer significant, whereas the effect of Parent Academy-College remains negative and highly significant. To account for both hypotheses - Parent Academy-Cash vs. Control and Parent Academy-College vs. Control - we adjust for multiple hypothesis testing in Appendix Table A4. The Parent Academy-College coefficient remains significant using the family-wise error rate⁴. This evidence suggests that the added incentives at stake in the Parent Academy-College treatment increase the effects of the Parent Academy treatment on learning loss.

Result 2. *Within Parent Academy, college incentives show a persistent buffering effect ($p < 0.01$), while cash incentives do not ($p = 0.32$), suggesting college incentives drive stronger buffering overall.*

⁴For robustness, we also report the FWER-adjusted p-values including all four hypotheses (Parent Academy, Preschool, Parent Academy-Cash, and Parent Academy-College) in Appendix Table A5. Results hold.

We test Hypothesis 3 using data from Parent Academy engagement measures collected during the CHECC years (2010-2012). We first consider Parent Academy session attendance as a measure of engagement. There are a total of 19 sessions throughout the Parent Academy program. Among our main sample of Parent Academy participants, parents attend an average of 76% of sessions. To test whether children whose parents experience a higher dosage of Parent Academy exhibit less Covid-19 learning loss, we regress learning loss on Parent Academy assignment and percentage of Parent Academy sessions attended.

[INSERT TABLE 4 HERE]

Table 4 reports the results. When we include attendance in the main regression, the effect of Parent Academy on Covid-19 learning loss is no longer significant, but the attendance variable is. That is, the effect of Parent Academy on Covid-19 learning loss is driven by those who attend more Parent Academy sessions, consistent with Hypothesis 3.

As another measure of Parent Academy dosage, we use parent payments. This measure aggregates parent session attendance data, homework scores, evaluation scores, and child assessment scores. Parents face total possible earnings of \$6900 from Parent Academy. On average, parents earn \$3889 (56%). We include this measure of dosage in the main regression, with results shown in Table 5. Similarly to Table 4, the Parent Academy indicator is no longer significant. The payment variable is significantly negative, indicating that the children whose parents are more engaged with Parent Academy experience a stronger buffering effect during Covid-19.

Result 3. *Children whose parents engage more with Parent Academy experience a greater buffering effect of Parent Academy on Covid-19 learning loss than children whose parents are less engaged with Parent Academy.*

[INSERT TABLE 5 HERE]

5 Epilogue

This paper asks whether early childhood parenting interventions generate lasting human capital that is latent rather than lost. We exploit a setting that combines two sources of exogenous variation to answer this question. The first is a randomized field experiment — CHECC — in which households with children ages 3-5 are assigned to a parenting program, a preschool program, or a control group beginning in 2010. The second is the Covid-19 school shutdowns, which abruptly remove the dominant school input for all children in our sample roughly a decade later. If the parenting program builds durable human capital in parents

that is simply masked by school inputs during normal times, then the removal of those school inputs should reveal treatment effects that have faded from measurability. If instead the treatment effects are truly gone, the parenting and control groups should experience similar learning losses.

Our main empirical results fall into two camps. First, the parenting program has short-term impacts on children’s executive functioning skills but not on their academic skills (Fryer et al., 2015). The preschool program has short-term impacts on children’s academic skills but not on their executive functions (Fryer et al., 2020). Equally as important, as is commonly observed in the literature, the effects of both programs fades out over time (see Castillo et al. (2024) and List and Uchida (2024)). Indeed, by the beginning of the Covid-19 pandemic, the treatment and control groups are similar on standardized test scores and other measured dimensions in aggregate.

Second, we find that the effects of the parenting program re-emerge during the Covid-19 school closure. Specifically, while test scores in the control group drop by 0.30 standard deviations during the closure, test scores in the parenting group drop only by 0.11 standard deviations, a statistically significant difference ($p = 0.01$). The observed effect is driven by Parent Academy-College, which incentivizes continued investments until the child graduates high school. In a practical sense, this result is consistent with the notion that parents compensate for the negative Covid shock by increasing childhood investments.

Our overarching empirical results have implications for our understanding of how education works and the long-standing question of why the benefits of early childhood programs seem to fade away over time. What is particularly interesting about our findings is that the benefits of the parenting program seem to disappear before the schools even close, following a familiar pattern in this area of research. However, when the schools suddenly shut down and the usual school inputs are no longer available, the benefits of the parenting program reappear.

We view this result as suggesting that the skills and knowledge parents gain through the program are still there, even if they are latent. In this manner, in the spirit of our theory, it seems that the schools provide a dominant influence that masks the differences in what parents do with their children. This has important implications for policy: it is possible that the fade-out of benefits from early childhood programs is not because the programs fail to work, but because we are not measuring their effects in the right way. That is, the properties of the situation are not correct to find such effects. If this is true more broadly, then the usual ways of evaluating programs may underestimate the long-term benefits of investing in parents and young children.

Our findings suggest that investments in parents may have positive long-term impacts

on children academically. Much of the literature on early childhood interventions finds immediate effects on child outcomes that then fade out (Almond et al., 2018). Some papers find evidence that the effects of interventions then reappear later in life (Heckman et al., 2013). We find that the effects of our intervention appear during middle school - a stage in life where early childhood interventions often disappear (Almond et al., 2018).

Our findings also provide evidence about the role of parents in education production function of children. Prior to the Covid-19 school closures, we did not detect differences in child academic performance from the Parent Academy treatment. When Covid-19 shut schools down, however, children whose parents are assigned to the parent training intervention experience less learning loss. Together, these findings suggest that the role of parents in the education production function is partially offset by schools. When schools are forced to shut down, parents have a larger impact in the education production function.

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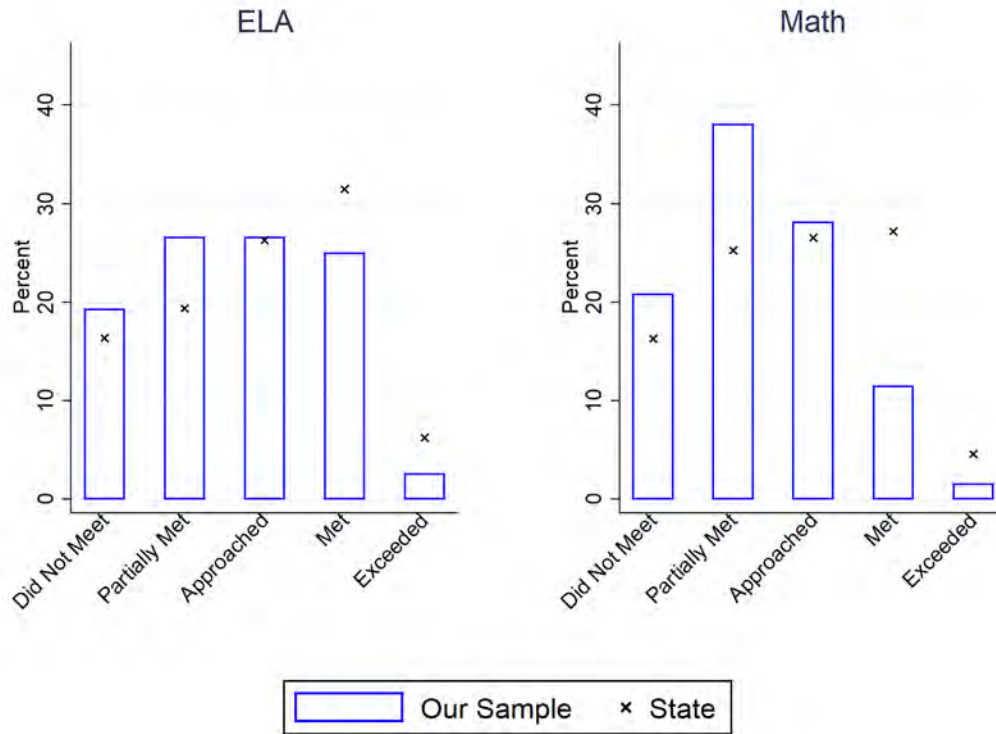
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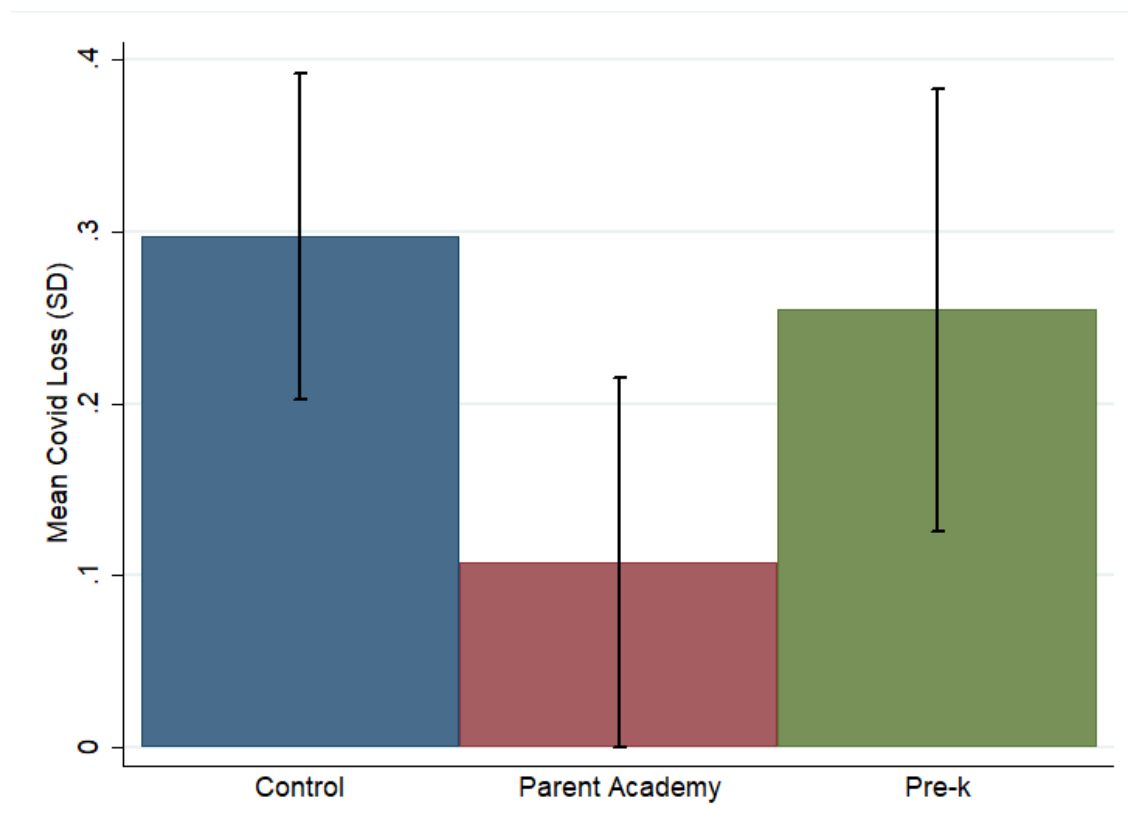
6 Figures

Figure 1: 2019 IAR Performance Levels: State vs. Sample



Notes. This figure shows the distribution of 2019 results on the Illinois Assessment of Readiness (IAR) among students in our sample (blue bars) and across Illinois (black Xs). Scale scores are categorized into 5 performance labels: “Did not Meet Expectations”, “Partially Met Expectations”, “Approached Expectations”, “Met Expectations”, and “Exceeded Expectations”. The left panel depicts results on the English and Language Arts (ELA) exam, and the right panel depicts results on the Math exam.

Figure 2: Treatment Effects using CHECC Years 2010 & 2011



Notes. This figure depicts average Covid-19 learning loss by treatment assignment in the Chicago Heights Early Childhood Center (CHECC). Covid-19 learning loss is calculated as the difference students' scores on the Illinois Assessment of Readiness (IAR) exam in 2021 (supplemented by 2022 scores if missing) and students' scores in 2019 (supplemented by 2018 scores if missing). Average learning loss is calculated in standard deviations of 2019 scores.

7 Tables

Table 1: Summary Statistics: Parent Academy vs. Control

	Control	Parent Academy	Pre-K	1v2 (p-val)
Female	0.48 (0.50)	0.52 (0.50)	0.50 (0.50)	0.45
Race: Black	0.39 (0.49)	0.31 (0.46)	0.40 (0.49)	0.12
Race: Hispanic	0.52 (0.50)	0.59 (0.49)	0.49 (0.50)	0.22
Race: White	0.09 (0.29)	0.10 (0.30)	0.11 (0.31)	0.84
Mother Edu: Less than HS	0.19 (0.40)	0.24 (0.43)	0.19 (0.40)	0.39
Mother Edu: HS or GED	0.26 (0.44)	0.22 (0.42)	0.30 (0.46)	0.51
Mother Edu: Some College/2-year Degree	0.37 (0.48)	0.29 (0.45)	0.34 (0.48)	0.15
Mother Edu: Bachelor's +	0.18 (0.39)	0.25 (0.44)	0.17 (0.38)	0.14
Income: \$0-\$16000	0.39 (0.49)	0.35 (0.48)	0.34 (0.48)	0.59
Income: \$16000-\$36000	0.32 (0.47)	0.38 (0.49)	0.37 (0.49)	0.38
Income: \$36000+	0.29 (0.46)	0.27 (0.45)	0.29 (0.46)	0.73
Observations	202	136	92	338

Notes. This table provides summary statistics of our analysis sample by CHECC treatment group: Control, Parent Academy, and Pre-K. Column 5 displays p values to identify whether the demographic differences between Control and Parent Academy treatments are statistically significant.

Table 2: Treatment Effects on Covid-19 Learning Loss

	(1) Covid-19 Loss (SD)	(2) Covid-19 Loss (SD)	(3) Covid-19 Loss (SD)
Parent Academy	-0.20*** (0.04)	-0.14*** (0.04)	-0.13*** (0.05)
Pre-k	-0.05 (0.07)	-0.02 (0.09)	-0.02 (0.10)
Score Pre-Covid	0.11*** (0.04)	0.15*** (0.04)	0.16*** (0.04)
Female		-0.09** (0.04)	-0.08* (0.05)
Race: White		-0.04 (0.08)	-0.07 (0.07)
Race: Hispanic		-0.18** (0.07)	-0.21** (0.09)
Age		0.12 (0.10)	0.11 (0.08)
Mother Edu: HS or GED			-0.16*** (0.04)
Mother Edu: Some College/2-year Degree			-0.10* (0.06)
Mother Edu: Bachelor's +			-0.25** (0.10)
Income: \$16000-36000			-0.04 (0.11)
Income: \$36000+			0.11 (0.10)
Constant	0.24 (0.16)	-1.24 (1.41)	-0.84 (1.24)
Observations	430	430	430
Control Mean	0.30	0.30	0.30

Notes. We regress students' Covid-19 learning loss on CHECC treatment assignment, pre-Covid IAR test score, various demographics, and socioeconomic variables. Covid-19 learning loss is calculated as students' post-Covid (2021) IAR score subtracted from their pre-Covid (2019) IAR score. Pre-Covid scores are supplemented by 2018 scores if 2019 scores are not available, and post-Covid scores are supplemented by 2022 scores if 2021 scores are not available. Learning loss is measured in standard deviations relative to 2019 scores. All models control for CHECC participation year and IAR exam year. Models 2 and 3 also control for baseline cognitive and executive function scores during CHECC participation. Standard errors clustered by district are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3: Cash vs College: Treatment Effects on Covid-19 Learning Loss

	(1) Covid-19 Loss (SD)	(2) Covid-19 Loss (SD)	(3) Covid-19 Loss (SD)
Cash	-0.16** (0.06)	-0.10 (0.07)	-0.07 (0.07)
College	-0.24*** (0.05)	-0.18*** (0.05)	-0.18*** (0.05)
Pre-k	-0.05 (0.07)	-0.02 (0.09)	-0.01 (0.10)
Normalized Score Pre-Covid	0.11*** (0.04)	0.15*** (0.04)	0.16*** (0.04)
Female		-0.09** (0.04)	-0.08* (0.04)
Race: White		-0.04 (0.08)	-0.08 (0.07)
Race: Hispanic		-0.18** (0.07)	-0.20** (0.09)
Age		0.13 (0.10)	0.12 (0.08)
Mother Edu: HS or GED			-0.16*** (0.04)
Mother Edu: Some College/2-year Degree			-0.10 (0.06)
Mother Edu: Bachelor's +			-0.25** (0.11)
Income: \$16000-\$36000			-0.04 (0.11)
Income: \$36000+			0.11 (0.10)
Constant	0.25 (0.15)	-1.39 (1.43)	-1.00 (1.25)
Observations	430	430	430

Notes. We regress Covid-19 learning loss on CHECC treatment assignment, pre-Covid IAR score, demographics, and socioeconomic variables. “Cash” and “College” refer to Parent Academy subtrements in which parents were incentivized either in cash or secondary education funding. Covid-19 learning loss is calculated as post-Covid (2021) IAR score subtracted from pre-Covid (2019) IAR score. Pre-Covid scores are supplemented by 2018 scores if 2019 scores are not available. Post-Covid scores are supplemented by 2022 scores if 2021 scores are not available. Learning loss is measured in 2019 standard deviations. All models control for CHECC participation year and IAR exam year. Models 2 and 3 control for baseline cognitive and executive functioning scores during CHECC. Standard errors clustered by district are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 4: The Buffering Effect of Parent Academy by Parent Session Attendance

	(1) Covid-19 Loss (SD)	(2) Covid-19 Loss (SD)	(3) Covid-19 Loss (SD)
Parent Academy	-0.01 (0.10)	0.05 (0.09)	0.07 (0.07)
PA attendance (perc)	-0.26** (0.11)	-0.26** (0.11)	-0.27*** (0.10)
Pre-k	-0.05 (0.07)	-0.02 (0.09)	-0.02 (0.10)
Normalized Score Pre-Covid	0.12*** (0.04)	0.16*** (0.04)	0.17*** (0.04)
Female		-0.09** (0.04)	-0.09* (0.05)
Race: White		-0.03 (0.08)	-0.07 (0.07)
Race: Hispanic		-0.18** (0.07)	-0.21** (0.09)
Age		0.11 (0.10)	0.09 (0.08)
Mother Edu: HS or GED			-0.16*** (0.04)
Mother Edu: Some College/2-year Degree			-0.12** (0.06)
Mother Edu: Bachelor's +			-0.25** (0.11)
Income: \$16000-\$36000			-0.04 (0.11)
Income: \$36000+			0.12 (0.10)
Constant	0.23 (0.15)	-1.08 (1.43)	-0.65 (1.26)
Observations	430	430	430
Control Mean	0.30	0.30	0.30

Notes. We regress Covid-19 learning loss on CHECC treatment assignment, pre-Covid IAR score, demographics, and socioeconomic variables. “PA attendance (perc)” is the percentage of Parent Academy sessions that the student’s parents attended during the CHECC years, out of 19 total sessions. Covid-19 learning loss is calculated as post-Covid (2021) IAR score subtracted from pre-Covid (2019) IAR score. Pre-Covid scores are supplemented by 2018 scores if 2019 scores are not available. Post-Covid scores are supplemented by 2022 scores if 2021 scores are not available. Learning loss is measured in 2019 standard deviations. All models control for CHECC participation year and IAR exam year. Models 2 and 3 control for baseline cognitive and executive functioning scores during CHECC. Standard errors clustered by district are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

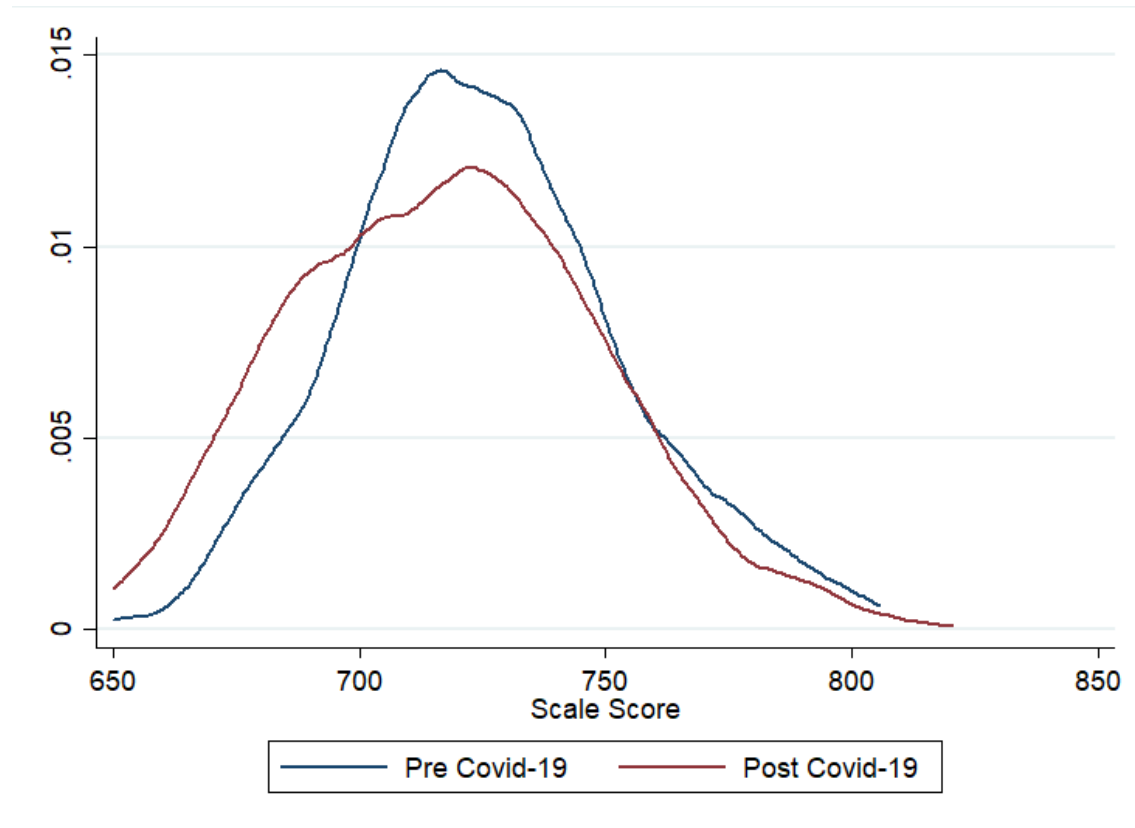
Table 5: The Buffering Effect of Parent Academy by Parent Payments

	(1) Covid-19 Loss (SD)	(2) Covid-19 Loss (SD)	(3) Covid-19 Loss (SD)
Parent Academy	-0.03 (0.08)	0.02 (0.07)	0.04 (0.06)
PA Payment (perc)	-0.31*** (0.11)	-0.30** (0.11)	-0.31*** (0.10)
Pre-k	-0.05 (0.07)	-0.02 (0.09)	-0.02 (0.10)
Normalized Score Pre-Covid	0.12*** (0.04)	0.16*** (0.04)	0.17*** (0.04)
Female		-0.09** (0.04)	-0.09* (0.04)
Race: White		-0.03 (0.08)	-0.06 (0.07)
Race: Hispanic		-0.18** (0.07)	-0.21** (0.09)
Age		0.11 (0.10)	0.10 (0.08)
Mother Edu: HS or GED			-0.16*** (0.04)
Mother Edu: Some College/2-year Degree			-0.12** (0.06)
Mother Edu: Bachelor's +			-0.25** (0.11)
Income: \$16000-\$36000			-0.03 (0.11)
Income: \$36000+			0.11 (0.10)
Constant	0.24 (0.15)	-1.08 (1.43)	-0.66 (1.26)
Observations	430	430	430
Control Mean	0.30	0.30	0.30

Notes. We regress Covid-19 learning loss on CHECC treatment assignment, pre-Covid IAR score, demographics, and socioeconomic variables. “PA Payment (perc)” is the percentage of total possible Parent Academy earnings that the student’s parent received out of a total possible \$6900. Earnings consist of payment for session attendance (\$100 per session for 19 total sessions), homework (\$100 per assignment for 17 assignments), evaluations (\$1800 total possible), assessments (\$1500 total possible). Covid-19 learning loss is calculated as post-Covid (2021) IAR score subtracted from pre-Covid (2019) IAR score. Pre-Covid scores are supplemented by 2018 scores if 2019 scores are not available. Post-Covid scores are supplemented by 2022 scores if 2021 scores are not available. Learning loss is measured in 2019 standard deviations. All models control for CHECC participation year and IAR exam year. Models 2 and 3 control for baseline cognitive and executive functioning scores during CHECC. Standard errors clustered by district are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

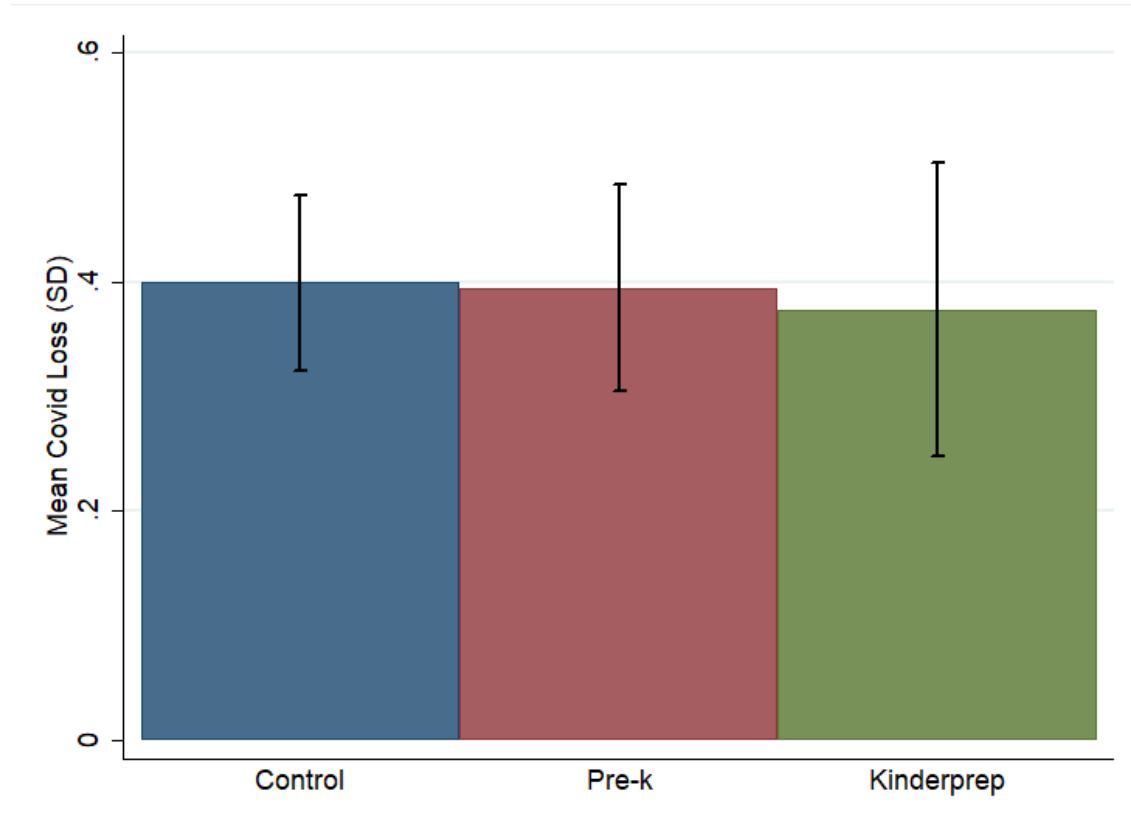
8 Appendix

Figure A1: Distribution of IAR Scores: Pre vs. Post Covid-19



Notes. This figure depicts the probability distribution function of students' scale scores on the Illinois Assessment of Readiness (IAR) exam pre Covid-19 in 2019 (blue line) and post Covid-19 in 2021 (red line) among students in our main sample.

Figure A2: CHECC Years 2012 & 2013



Notes. This figure depicts average Covid-19 learning loss by treatment assignment in the Chicago Heights Early Childhood Center (CHECC), restricting to students who participated in CHECC in 2012 and 2013. During this period, there was no Parent Academy. Covid-19 learning loss is calculated as the difference students' scores on the Illinois Assessment of Readiness (IAR) exam in 2021 (supplemented by 2022 scores if missing) and students' scores in 2019 (supplemented by 2018 scores if missing). Average learning loss is calculated in standard deviations of 2019 scores.

Table A1: Control Group: Demographics by Attrition

	Control: Main Sample	Control: Attrited	1v2 (p-val)
Female	0.48 (0.50)	0.46 (0.50)	0.66
Race: Black	0.39 (0.49)	0.44 (0.50)	0.29
Race: Hispanic	0.52 (0.50)	0.39 (0.49)	0.00
Race: White	0.09 (0.29)	0.13 (0.33)	0.17
Mother Edu: Less than HS	0.19 (0.40)	0.16 (0.37)	0.42
Mother Edu: HS or GED	0.26 (0.44)	0.21 (0.41)	0.28
Mother Edu: Some College/2-year Degree	0.37 (0.48)	0.39 (0.49)	0.63
Mother Edu: Bachelor's +	0.18 (0.39)	0.24 (0.43)	0.20
Income: <\$16000	0.39 (0.49)	0.35 (0.48)	0.55
Income: \$16000-\$36000	0.32 (0.47)	0.24 (0.43)	0.14
Income: \$36000+	0.29 (0.46)	0.41 (0.49)	0.05
Observations	202	345	547

Table A2: Parent Academy: Demographics by Attrition

	PA: Main Sample	PA: Attrited	1v2 (p-val)
Female	0.52 (0.50)	0.48 (0.50)	0.44
Race: Black	0.31 (0.46)	0.47 (0.50)	0.00
Race: Hispanic	0.59 (0.49)	0.36 (0.48)	0.00
Race: White	0.10 (0.30)	0.14 (0.35)	0.24
Mother Edu: Less than HS	0.24 (0.43)	0.19 (0.39)	0.34
Mother Edu: HS or GED	0.22 (0.42)	0.19 (0.39)	0.52
Mother Edu: Some College/2-year Degree	0.29 (0.45)	0.36 (0.48)	0.17
Mother Edu: Bachelor's +	0.25 (0.44)	0.26 (0.44)	0.98
Income: <\$16000	0.35 (0.48)	0.37 (0.48)	0.78
Income: \$16000-\$36000	0.38 (0.49)	0.24 (0.43)	0.02
Income: \$36000+	0.27 (0.45)	0.39 (0.49)	0.06
Observations	136	180	316

Table A3: Multiple Hypothesis Testing Adjustments: Parent Academy and Pre-K

Hypothesis	Coefficient	Std. Err.	p-value	p-value (FWER)
Parent Academy	-.1242178	.0382832	.0017326	.003
Pre-K	.0221756	.0384041	.5653138	.595

Notes: We adjust p-values using a family-wise error rate, in which the family includes both tests of Parent Academy and Pre-k treatment assignments in CHECC. FWER-adjusted p-values computed using Westfall-Young stepdown procedure with 1000 bootstrap replications, resampling at the district level.

Table A4: Multiple Hypothesis Testing Adjustments: Parent Academy-Cash and Parent Academy-College

Hypothesis	Coefficient	Std. Err.	p-value	p-value (FWER)
Cash	-.0822248	.0577734	.1586569	.141
College	-.1642462	.0482788	.0010578	.008

Notes: We adjust p-values using a family-wise error rate, in which the family includes both tests of Parent Academy-Cash and Parent Academy-College. FWER-adjusted p-values computed using Westfall-Young stepdown procedure with 1000 bootstrap replications, resampling at the district level.

Table A5: Multiple Hypothesis Testing Adjustments: Parent Academy, Pre-k, Cash, and College

Hypothesis	Coefficient	Std. Err.	p-value	p-value (FWER)
Parent Academy	-.1242178	.0382832	.0017326	.011
Pre-k	.0221756	.0384041	.5653138	.595
Cash	-.0822248	.0577734	.1586569	.296
College	-.1642462	.0482788	.0010578	.009

Notes: We adjust p-values using a family-wise error rate, in which the family includes all four tests of Parent Academy, Pre-k, Parent Academy-Cash, and Parent Academy-College. FWER-adjusted p-values computed using Westfall-Young stepdown procedure with 1000 bootstrap replications, resampling at the district level.

Table A6: Treatment Effect of Parent Academy on Pre-Covid-19 Outcomes

	(1) Cognitive Score	(2) Non-Cognitive Score	(3) Test Scores	(4) Ever Disciplined
Parent Academy	0.03 (0.11)	0.12 (0.14)	-0.13 (0.10)	-0.51* (0.31)
Pre-k	0.20* (0.11)	0.09 (0.15)	0.01 (0.10)	-0.20 (0.28)
Female	-0.00 (0.08)	-0.08 (0.11)	0.13* (0.08)	-0.61*** (0.23)
Race: White	0.11 (0.16)	0.33* (0.17)	0.34** (0.13)	-1.88*** (0.47)
Race: Hispanic	-0.10 (0.10)	0.47*** (0.15)	0.23** (0.09)	-0.38 (0.25)
Mother Edu: Bachelor's +	0.32* (0.17)	0.47* (0.27)	0.62*** (0.16)	0.48 (0.54)
Mother Edu: HS or GED	0.11 (0.11)	0.28* (0.17)	0.06 (0.12)	0.58 (0.36)
Mother Edu: Some College/2-year Degree	0.23* (0.13)	0.15 (0.20)	0.22* (0.12)	0.01 (0.40)
Income: \$16000-\$36000	0.11 (0.11)	-0.13 (0.15)	0.13 (0.11)	-0.37 (0.31)
Income: \$36000+	0.35** (0.14)	0.02 (0.22)	0.24* (0.14)	0.20 (0.37)
Constant	-0.62** (0.25)	-0.59* (0.34)	-1.35 (1.19)	-2.43 (4.70)
Observations	244	242	1473	259
Children	244	242	431	144

Notes. In this table, we regress various outcomes on CHECC treatment assignment, demographics, and socioeconomic variables. The outcome variables in Models 1 and 2 are cognitive score and non-cognitive score at the end of students' CHECC participation, respectively. The outcome variable in Model 3 is student IAR scale scores in grades 3-8 prior to 2020. The outcome variable in Model 4 is an indicator for each school year (grade 3-8) of whether the child received any disciplinary referral prior to 2020. All models restrict to our sample of students with Covid-19 learning loss scores. All models control for CHECC participation year, cognitive score at CHECC baseline, non-cognitive score at CHECC baseline, and age. Models 3 and 4 also control for exam year and grade level. Robust standard errors are in parentheses in Models 1 and 2. Standard errors clustered by student are in parentheses in Models 3 and 4. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A7: Test for Mean Reversion in Scores

	(1)	(2)
	Learning Loss (2019-2021)	Learning Loss (2017-2019)
Parent Academy	-0.168** (0.0692)	0.00335 (0.0374)
Pre-k	-0.00806 (0.115)	-0.0272 (0.0788)
Normalized Pre Score	0.145*** (0.0294)	0.372*** (0.0332)
Female	-0.101* (0.0527)	-0.152** (0.0615)
Race: White	-0.0207 (0.128)	-0.222* (0.119)
Race: Hispanic	-0.211** (0.102)	-0.289*** (0.0739)
Age	0.108 (0.114)	0.0561 (0.0894)
Mother Edu: HS or GED	-0.149*** (0.0511)	0.0128 (0.0472)
Mother Edu: Some College/2-year Degree	-0.103 (0.0955)	-0.0582 (0.0598)
Mother Edu: Bachelor's +	-0.308** (0.123)	-0.153 (0.0937)
Income: \$16000-\$36000	0.0402 (0.0916)	-0.0139 (0.0985)
Income: \$36000+	0.164 (0.109)	-0.112 (0.128)
Constant	-0.662 (1.646)	-0.267 (1.213)
Observations	338	338

Notes. In this table, we regress student learning loss on CHECC treatment assignment, pre-Covid IAR test score, various demographics, and socioeconomic variables. In Model 1, learning loss is calculated as students' 2021 (post-Covid) IAR score subtracted from their 2019 (pre-Covid) IAR score. In Model 2, learning loss is calculated as students' 2019 IAR score subtracted from their 2017 IAR score. Learning loss is measured in standard deviations relative to the first score. All models control for CHECC participation year, IAR exam year, baseline cognitive and executive function scores during CHECC participation. Standard errors clustered by district are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A8: Treatment Effects on Covid-19 Learning Loss with Inverse Probability Weighting

	(1) Covid-19 Loss (SD)
Parent Academy	-0.12** (0.06)
Score Pre-Covid	0.15*** (0.05)
Female	-0.15** (0.06)
Race: White	-0.14 (0.09)
Race: Hispanic	-0.19* (0.10)
Age	-0.01 (0.12)
Mother Edu: HS or GED	-0.28*** (0.09)
Mother Edu: Some College/2-year Degree	-0.24** (0.11)
Mother Edu: Bachelor's +	-0.34** (0.16)
Income: \$16000-\$36000	-0.05 (0.20)
Income: \$36000+	0.17 (0.15)
Constant	1.20 (1.80)
Observations	337

Notes. We use inverse probability weights (IPW) as a robustness test for our main finding to account for differences between our analysis sample and the sample of students that was eligible for analysis. We first predict how likely each student is to be included in our sample based on their IAR score prior to Covid-19, an indicator for whether this score is missing, CHECC year, and demographic characteristics. These prediction scores are calculated separately for those in the control group and the Parent Academy treatment. We then run our main specification, weighting observations by the inverse of this likelihood. Standard errors clustered by district in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$