Who Benefits from Remote Schooling? Self-selection and Match Effects*

Jesse Bruhn
Brown University

Christopher Campos
University of Chicago Booth School of Business

Eric Chyn
University of Texas-Austin and NBER

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Abstract

We study the distributional effects of remote learning. Our approach combines newly collected data on parental preferences with administrative data from Los Angeles. The preference data allow us to account for selection into remote learning and isolate plausibly exogenous variation. We find that the average effect of remote learning is negative (-0.11σ for reading; -0.13σ for math). Notably, we also find evidence of positive learning effects for children whose parents have strong demand for remote learning. Our results suggest that some students currently sorting into post-pandemic remote learning benefit from expanded choice.

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*__Bruhn:__ Brown University, Email: jesse_bruhn@brown.edu
__Campos:__ University of Chicago Booth School of Business, Email: Christopher.Campos@chicagobooth.edu.
__Chyn:__ University of Texas-Austin, Email: eric.chyn@austin.utexas.edu.
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Classic theories of education markets predict that school choice can improve the allocative efficiency of students to schools (Hoxby, 2003). Thus by improving match-quality, choice policies hold the potential to engineer better student outcomes at next to zero cost. Yet, existing research in the United States fails to find meaningful evidence that student-school match effects exist *at all* (Abdulkadiroğlu et al., 2020, Mountjoy and Hickman, 2020). And even substantial changes to the choice environment can fail to produce meaningful improvements in student-school match quality (Campos and Kearns, 2022).[1]

Imperfect information is a leading hypothesis that can rationalize this gap between theory and data. Families may not know their match quality at the time they choose schools and instead learn about it over time through a process of trial and error (Arcidiacono et al., 2016, Larroucau and Rios, 2020). The pandemic provides an unusual and unique setting where families were compelled to learn about their relative suitability for a particular schooling option: remote learning. While there is mounting evidence that remote learning contributed to sizable learning losses during the pandemic (Goldhaber et al., 2022, Jack et al., 2022, Singh et al., 2022), school districts across the country are now planning to offer permanent, expanded remote options to satisfy ongoing parental demand (Musaddiq et al., 2022).

In this paper, we study the allocative efficiency of students to instructional modes in the post-pandemic environment. We focus on the second-largest school district in the U.S., the Los Angeles Unified School District (LAUSD). At the onset of the pandemic, every student in the district was compelled to participate in virtual learning. The following year involved a cycle of in-person to remote periods. Put together, this unusual experience allowed families to learn about their students' relative suitability for remote learning over an extended period and across a large spectrum of K-12 ages.[2] In 2022, LAUSD returned to in-person learning as the dominant mode of instruction; however, the district continued to offer a remote learning option that was chosen by 14,000 students.[3] Why did so many families continue to prefer the remote option? Evidence on this question is scarce. Bacher-Hicks et al. (2022) find decreases in bullying during the remote era. Thus demand for safety may play a role. In a higher education context, Aucejo et al. (2020) find substantial heterogeneity in students' perceived remote learning experiences. This suggests that academic success may also be a factor.

Our analysis relies on a novel survey that we designed to learn about families' preferences for remote learning. Following previous research using conjoint experiments to learn about preferences for workplace characteristics and flexibility (Mas and Pallais, 2017, Wiswall and Zafar, 2018), we use a series of medium-stakes hypothetical choices to experimentally identify families' preferences for the remote option. The hypothetical choices provide rich information about how families trade-off academic quality, travel time, and remote offerings while holding all other school attributes fixed.[4]

[1] Bau (2022) is a notable exception, providing evidence of the importance of match quality in Pakistan. Bruhn (2019) finds evidence of substantial match effects in the context of between *district* sorting.

[2] Importantly, this cycle of remote to in-person learning in L.A. is remarkably similar to the experience of other school districts across the United States (Jack et al., 2022). Thus, our setting provides a natural context for studying ongoing selection into remote learning.

[3] Throughout, we adopt the convention of referring to the 2021-2022 school year as 2022.

[4] Prior work has found that preference estimates from similar experiments contain a high degree of external validity (Wiswall and Zafar, 2018).

The data on parental preferences serves a dual empirical purpose. First, our main empirical exercise leverages the preferences identified via the choice experiment to account for selection into remote learning. Intuitively, parents with similar preferences should have a similar propensity to choose the remote option. More concretely, this strategy relies on the notion that the dominant source of selection into remote learning emerges from preference heterogeneity. Under this assumption, we can borrow insights from the literature on selection corrections built from revealed preferences to recover unbiassed estimates of the causal effect of remote learning on student outcomes (e.g. Abdulkadiroğlu et al., 2020, Heckman, 1979, Mountjoy and Hickman, 2020, Rosembaum and Rubin, 1983). The main difference between our approach and this canonical work is that our preference estimates come from the experimental data rather than the observed choices. Second, the preference estimates also serve as an important measure of demand. This allows us to explore the nature of selection on levels versus gains in the spirit of Roy (1951) by asking, "Do families with more intense preferences for the remote option experience larger causal benefits?" (Einav et al., 2022, Kline and Walters, 2016, Otero et al., 2021).

Our analysis of the remote learning survey data begins with three descriptive facts. One-third of respondents want the district to expand remote offerings. One-fourth expect to enroll their children in remote offerings in the future. And twenty percent of respondents feel their children excelled in remote learning relative to traditional, in-person instruction. These findings suggest that there is substantial scope for permanent, post-pandemic remote offerings to generate improvements in allocative efficiency.

Conjoint analysis allows us to move beyond these descriptive facts and experimentally identify family-specific preference estimates. Consistent with previous literature spanning several countries, we find that families have a taste for academic quality and a distaste for distance (Abdulkadiroğlu et al., 2020, Ainsworth et al., 2020, Allende, 2019, Beuermann et al., 2022, Burgess et al., 2015, Campos and Kearns, 2022, Neilson, 2021). Unsurprisingly, we find that the average family has a strong distaste for remote learning. The average family would need to be compensated with a 40 percentage point increase in academic standards to be indifferent between remote and in-person offerings. In contrast, families are willing to travel an additional 10 minutes to obtain a 10 percentage point increase in academic standards. Reassuringly, we do not find a distaste for remote learning among families currently enrolled in remote offerings or those that indicated they anticipate doing so in the future. Overall, the survey provides the first rigorous evidence about families' varying tastes for remote learning in the post-pandemic landscape.

Next, we use our experimentally derived preference estimates to explore selection into remote learning. The conceptual framework we propose considers selection bias governed by students' preferences for remote learning, which we can estimate directly using our conjoint experiments. Our preferred approach is in the spirit of Rosembaum and Rubin (1983) and accounts for selection bias in the impact of remote learning by conditioning on propensity scores implied by the experimental preference estimates. This strategy balances baseline achievement, which we demonstrate is infeasible when conditioning on a rich set of covariates alone. We then turn to estimating causal remote-learning impacts. We find remote learning effects of -0.11σ on reading and -0.13σ on math, with the latter being distinguishable from statistical noise at

conventional levels. These estimates are noticeably different from models that simply adjust for lagged achievement and other baseline covariates: regression adjustment with standard covariates generates estimates that range between -0.25σ and -0.30σ.[5]

Our final analysis studies match effects in this setting. We find evidence of negative selection on levels, indicating that students with high proclivity to remote learning perform poorly regardless of the school they enroll in. In contrast, our analysis also suggests the presence of positive selection on gains. This suggests that families choose remote learning, at least in part, on factors that correlate with their child's suitability for remote instruction. This has important policy implications for understanding the allocative efficiency of ongoing, expanded remote offerings. Taking our linear extrapolation at face value implies that students at the ninetieth percentile of remote learning proclivity experienced a roughly 0.20σ improvement in math scores. This suggests that prior estimates of the impact of remote learning during the pandemic may not be predictive of the future effect that expanded remote offerings will have on the students who opt-in.

We address a variety of concerns with our empirical strategy. First, we replicate our analysis using observational preference estimates and are unable to balance lagged achievement. This emphasizes that the balancing nature of our approach is not a spurious consequence of linking any choice data with the reduced-form approach. Second, our balancing score relies on a crucial assumption that our utility specification captures all of the relevant choice margins influencing choices. To assuage concerns with the plausibility of this assumption, we construct control functions similar to Abdulkadiroğlu et al. (2020). This reorients our identifying assumption toward needing to account for selection bias driven by *unobserved* preference heterogeneity. Implementing this alternative strategy yields qualitivately similar results: balance of lagged achievement, negative selection on levels, positive selection on gains, and remarkably similar remote learning effects. Last, we note that our preferred approach relies on extrapolating preferences from our survey sample to the rest of the school district. While extrapolation of preferences are common in this literature (Agarwal and Somaini, 2020, Fack et al., 2019, Otero et al., 2021), we take steps to assess the internal validity of this extrapolation with a resampling exercise. We find that extrapolated preferences are forecast unbiassed for actual preferences. The combination of exercises reassures us of the internal validity of our findings.

This paper contributes to three broad literatures. A nascent but growing literature has focused on estimating the effects of remote or virtual learning. Bueno (2020) finds substantial negative effects of remote learning in the pre-pandemic era, although she documents negative trends preceding the switch to remote, a finding mirroring our negative selection on levels estimates. More recent evidence has pivoted to estimating remote-learning effects during the pandemic, with consensus that the pandemic caused sizable learning loss (Goldhaber et al., 2022, Jack et al., 2022, Singh et al., 2022). Jack et al. (2022) and Goldhaber et al. (2022) emphasize that remote learning offerings exacerbated learning loss relative to in-person schools and districts. Our paper looks ahead and considers the post-pandemic landscape and the implications of expanded remote offerings on the selected group of families opting into remote

[5]The estimates based on simple specifications that control for standard covariates are in line with previous quasi-experimental research (Bueno, 2020).

schooling and not mandated to do so. To that end, we provide evidence about how the expansion and persistence of remote learning can affect educational inequality and efficiency.

The second strand of literature we contribute to relates to studying match effects in the context of K-12 schooling and higher education. The notion of academic mismatch has received considerable attention in the related affirmative action literature, with some evidence pointing to potential efficiency losses (Arcidiacono et al., 2016, Dillon and Smith, 2020) and more recent evidence pointing to the opposite (Bleemer, 2021, 2022, Otero et al., 2021). Student-school match quality has been more elusive in the K-12 space, with some evidence pointing to the importance of match quality based on observables (Bau, 2022, Bruhn, 2019) and some pointing to the opposite (Campos and Kearns, 2022). Other papers focus on match effects after accounting for preferences and tend to find weak evidence of match quality (Abdulkadiroğlu et al., 2020, Mountjoy and Hickman, 2020). We complement this literature by collecting preference data and linking preference estimates to reduced-form approaches to assess the empirical relevance of match quality and add to the growing body of evidence.

Third, an extensive literature has linked choice models to treatment effect estimation (Heckman, 1979, Heckman et al., 2006) and more recent advances leverage information on rank-ordered lists to account for selection bias (Abdulkadiroğlu et al., 2020). Our approach is similar but infers preferences from hypothetical choices to construct measures of preference intensity or control functions that account for selection bias. This extends canonical work in economics using hypothetical choices to learn about preferences for workplace characteristics and flexibility (Mas and Pallais, 2017, Wiswall and Zafar, 2018). In that sense, we bridge these two seemingly disconnected literatures and create an avenue for future work.

1 Setting and Data

Remote Learning in Los Angeles

As in most school districts around the country at the onset of the pandemic, the Los Angeles Unified School District (LAUSD)—the second largest school district in the country—closed their schools and transitioned to remote learning on March 19, 2020. Swift actions were taken to buffer the shock, including the creation of online videos, coordinated meal distribution, laptop and tablet distribution, and private donors stepped up to provide broadband access and equipment for students. Students remained at home for the remainder of the academic year and recent evidence shows that this contributed to sizable learning loss (CITE).

The following academic year (2020-2021) commenced virtually, with a regular schedule that included daily interactions between teachers and students. While in-person tutoring services were offered, the provision of in-person tutoring ebbed and flowed with Covid waves. LAUSD schools remained closed until the week of April 19, where a staggered reopening commenced and students slowly returned to in-person schooling with some caveats. Elementary schools offered classes in three hour blocks and adult supervision when students were not in classes. Middle and high school students reported to campus on alternating days, with similar adult supervision provided. Families had the option to remain in remote learning.

For roughly one year, LAUSD students were required to remain home and receive instruction

virtually. Anecdotal evidence suggests most families disliked this experience and mounting evidence suggests this contributed negatively to student learning.[6] Evidence notwithstanding, there are still various reasons why some families may prefer remote learning. For example, bullied students may excel without the mental health costs they incur from in-person schooling (Bacher-Hicks et al., 2022). Other students may particularly benefit from learning at their own pace (Armstrong-Mensah et al., 2020).[7] This unusual experience provided families and students ample time to learn about their relative suitability for remote learning.

LAUSD returned to full in-person learning at the start of the 2021-2022 academic year.[8] To accommodate the preferences of a sizable share of families, the district did not make in-person learning mandatory and expanded its remote offerings. This resulted in the creation of a remote option called the City of Angels. This remote option offered students self-paced learning with consistent interactions with remote instructors and the opportunity to receive in-person tutoring. Remote students had the option to transition back to in-person learning at any time. We focus on the cohort 2021-2022 students who were allowed to self-select into remote offerings. These students had at least one year to adapt to remote instruction and learn about their relative suitability for remote learning.

Data

Our analysis uses administrative LAUSD data linked to survey data that we collect. The administrative data is standard, containing student-level demographics, test scores, and an array of other outcomes that vary by grade level. For our analysis we use 2018-2019 test scores as our measures of lagged achievement but otherwise use information from the 2021-2022 academic year.

The main data innovation is the survey we administered to a random sample of one hundred thousand parents in April 2022. The survey has two primary objectives. The first is quantifying parents' experiences and perceptions about remote learning through a series of simple questions. The second is to obtain measures of parents' preferences through a series of conjoint experiments similar to those used in other settings (Mas and Pallais, 2017, Moshary et al., 2022, Wiswall and Zafar, 2018). In the hypothetical choices, we make parents tradeoff preferences for academic quality, distance, and remote learning, while holding all other attributes fixed. The preference estimates are instrumental for our empirical strategy, which we discuss below.

Table 1 reports summary statistics of students in our sample. Column 1 reports mean characteristics for students selecting the remote option in 2021-2022, while Column 2 reports mean characteristics for students selecting in-person options. Column 3 reports the mean difference with associated standard errors below in parentheses. Students selecting remote offerings are noticeably different. Their 2018-2019 achievement was 14-18σ below district averages. Remote learning students are also more likely to be female, but similar in terms of special education status and underrepresented minority status. We address the baseline differences in our analysis

[6] https://ktla.com/news/nexstar-media-wire/nationworld/i-hate-it-families-frustrated-with-post-holiday-return-to-remote-learning/

[7] https://www.nytimes.com/2020/05/20/nyregion/coronavirus-students-schools.html

[8] All school districts in California were mandated to offer a remote option during this academic year as covid-related health concerns still loomed.

below.

2 Survey Evidence

In April of the 2021-2022 academic year, we distributed a survey to a random sample of families with LAUSD students enrolled in grades 3-8 and grade 11.[9] The survey was distributed through the school district's internal communications system and sent on behalf of the LAUSD. Because the messaging was on behalf of the school district, incentives were forbidden. Families were informed that their responses to the survey would affect future school offerings, and thus they had incentives to respond truthfully.

Table 2 reports respondent average characteristics. For comparison, Column 1 reports averages for all LAUSD students enrolled in the relevant grades. Survey respondents are noticeably different from the typical student in LAUSD. Families that initiated the survey have students performing above district averages, roughly 17-18 percent of a standard deviation. The differences get larger if we focus on the subset of families who we can estimate preferences for, roughly 44-46 percent of a standard deviation above district averages. Conjoint respondents are less likely to be classified as URM students and as special education students, and survey respondents to a lesser extent. We take these differences seriously in Section 4.

Appendix A reports various descriptive findings from the survey. Appendix Figure A.1 shows that roughly 35 percent of respondents enjoyed the remote experience, and similarly, roughly 35 percent want the district to expand remote offerings. Roughly 23 percent of respondents say they will opt for remote offerings in the future, and roughly 20 percent believed their children excelled in remote learning relative to in-person. The latter finding points to potential improvements in learning modality match quality. Appendix Figure A.2 shows that 10 percent of respondents have their children enrolled in remote offerings. Among those with children enrolled in remote learning, slightly less than 10 percent did so for mostly academic reasons, while roughly 25 percent did so for safety reasons, and 40 percent made the decision taking both into account. Lastly, Appendix Figure A.3 demonstrates that survey respondents represent all school district regions.

The last component of the survey allows us to recover parents' preferences using conjoint experiments. For each experiment, we present parents with three hypothetical choices that vary in terms of academic quality, travel time, and the type of instruction, remote or in-person. To gauge parental demand that is less affected by Covid-19 concerns, we ask them to make these decisions assuming pandemic-related safety issues are as they were in 2019 before the pandemic. Last, and common to other conjoint analyses, we emphasize that the schools are otherwise identical in terms of their academic quality and operation. In other words, these schools only differ in terms of the three attributes we randomly vary. Appendix C reports the survey items.

Figure 1a reports estimated willingness to travel estimates inferred from the conjoint experiments.[10] The average family is willing to travel an additional 13 minutes to enroll their children in a school with a 10 percentage point higher achievement rate. We find limited heterogeneity

[9] The exact survey instrument is detailed in appendix C

[10] Describe what we do.

concerning student age but do find that families currently in remote offerings or with plans to enroll in remote offerings have a lower willingness to travel for higher academic quality. Figure 1b shows estimates of the achievement compensation families need to be indifferent between in-person and remote schooling. The average family would need to be compensated with a 42 percentage point higher achievement rate to be indifferent between in-person and remote. This means that the average family has a distaste for remote learning. Reassuringly, we find that families currently in remote or those with plans to enroll in the future do not need such compensation. This evidence points to the survey evidence containing an informative signal.

3 Empirical Framework

Our focus is on estimating heterogeneous remote learning effects and studying how selection patterns map to them. This requires us to link an underlying choice model to a potential outcomes framework. We begin by introducing a potential outcome model.

We index a population of students by i, each of whom either attends school in-person, $D_i = 0$, or remote, $D_i = 1$. We consider a heterogeneous effects model that assumes that schooling achievement Y_i is a function of a vector of observable characteristics, X_i, and a remote indicator, D_i, can be expressed as:

$$Y_i = \alpha + X_i'\gamma + \beta D_i + u_i,$$

where u_i is an error term that captures family inputs and other unobserved determinants of achievement.. Notably, a key concern is that remote learning participation is correlated with unobservable factors (i.e., $E[u_i|D_i] \neq 0$). We now discuss an approach that allow us to move toward the causal parameters of interest and, as a consequence, study rich selection patterns into remote learning.

Our empirical strategies are analogous to adjusting for preferences that govern selection into remote learning. They both bear similarity to the self-revelation principle of Dale and Krueger (2002) although we rely on more parametric structure governing the underlying selection into treatment as in Abdulkadiroğlu et al. (2020). As detailed below, the two approaches that we utilize differ in that one relies on assuming selection on *observable* preference heterogeneity and another on *unobservable* preference heterogeneity.

Both of the empirical models that we ultimately estimate are based on a discrete choice model of schooling enrollment. Formally, student i's indirect utility of enrolling in schooling option j:

$$U_{ij} = V_{ij} + \varepsilon_{ij}$$

where $V_{ij} \equiv V(X_{j(i)})$ is the observable component of indirect utility which we assume is a function of a vector of relative school attributes, $X_{j(i)}$, and the term ε_{ij} captures any remaining unobserved preference heterogeneity. We assume that ε_{ij} is independent and identically distributed Extreme Value Type 1, a standard assumption in the discrete choice literature. For simplicity, we focus only two schooling options, in-person $j = 0$ or remote schooling $j = 1$, and normalize $V_{i0} = 0$. Indirect utility of remote learning relative to in-person schooling can be

more compactly be represented as:

$$u_i = v_i + \varepsilon_i,$$

where $u_i = U_{i1} - U_{i0}$, $v_i = V_{i1} - V_{i0}$, and $\varepsilon_i = \varepsilon_{i1} - \varepsilon_{i0}$.

Variation in v_i is driven by differences in a student's neighborhood schools, their proximity to schools, and their heterogeneous tastes for remote learning. We identify underlying preference parameters that govern v_i using conjoint experiments distributed in the survey. Specifically, we present respondents with hypothetical choices based on an underlying model of indirect utility that assumes:

$$V_{ij} = \omega_Q Q_j + \omega_R Remote_j + \omega_d d_{ij}$$

where Q_j is academic quality, $Remote_j$ is a remote schooling indicator, and d_{ij} is travel time to school j (set to 0 for remote learning). Given the randomization of characteristics presented in hypothetical choices in the conjoint survey, the parameters ω_Q, ω_R, and ω_d are identified. We combine our parameter estimates with students-specific neighborhood school Q_j and d_{ij}, to recover the implied v_i that is specific to students' realized choice between their neighborhood school and the remote option.

This choice model motivates our approach to identifying the effects of remote learning by comparing students who have similar preferences. In particular, we assume the following model of schooling achievement that allows for heterogeneous effects:

$$E[Y_i|X_i, D_i, v_i] = \alpha + X_i'\gamma + \beta D_i + \theta v_i + \psi v_i \times D_i. \tag{1}$$

Equation 3 assumes a linear relationship between the observable preference heterogeneity and potential outcomes that permits examination of selection patterns. For example, θ governs selection on levels and ψ governs selection on gains, where $\theta > 0$ indicates students with high tastes for remote learning do well regardless of the school they enroll in, while $\psi > 0$ indicates that students enrolling in remote options do better in remote rather than in-person. In practice, we consider models with interpretable functions of v_i, namely the implied the propensity score, where $P(v_i) = \frac{\exp(v_i)}{1+\exp(v_i)}$:

$$E[Y_i|X_i, D_i, v_i] = \alpha + X_i'\gamma + \beta D_i + \theta P(v_i) + \psi P(v_i) \times D_i. \tag{2}$$

This approach has a natural connection to the causal framework proposed by Rosembaum and Rubin (1983). If the observable determinants of indirect utility entirely capture selection into treatment, then causal estimates follow from conditioning on the implied propensity score.

Selection on observable preference heterogeneity may be too stringent, however, given our relatively parsimonious utility specification. We therefore also adopt an alternative approach that relies on a selection on *unobservable* preference heterogeneity assumption, similar to Abdulkadiroğlu et al. (2020). In particular, we assume

$$E[Y_i|X_i, D_i, v_i] = \alpha + X_i'\gamma + \beta D_i + \theta\lambda(X_i, X_{j(i)}) + \psi\lambda(X_i, X_{j(i)}) \times D_i. \tag{3}$$

where $\lambda(X_i, D_i, X_{j(i)}) = E[\varepsilon_i - \mu_\varepsilon|X_i, X_{j(i)}]$ are control functions implied by the choice model

(Abdulkadiroğlu et al., 2020, Dubin and McFadden, 1984). The selection parameters are identified by variation in students' relative choice sets, governed through $X_{j(i)}$, which includes relative distances.

Estimating Preferences

Our empirical strategy relies on preference estimates, $(\omega_Q, \omega_R, \omega_d)$ and v_i, to construct propensity scores or control functions. We use conjoint experiments to identify the parameters and quantities of interest. Each respondent is presented with $K = 10$ hypothetical choices, each with three options to choose from and the three attributes of interest randomized across hypothetical scenarios. We index respondents by i, hypothetical choices by k, and the choice set in each scenario as $\mathcal{J}_k$. In each scenario, we characterize respondent i's indirect utility as

$$U_{ikj} = \underbrace{\omega_{Qc(X_i)} Q_j + \omega_{Rc(X_i)} Remote_j + \omega_{dc(X_i)} d_{ij}}_{V_{ijk}} + \varepsilon_{ikj}$$

where $(\omega_{Qc(X_i)}, \omega_{Rc(X_i)}, \omega_{dc(X_i)})$ are allowed to vary flexibly by covariate cells defined by a combination of baseline achievement, gender, grade level, and URM status, similar to (Abdulkadiroğlu et al., 2020) but more limited in the cell structure. The covariate choice is governed by the survey results that provide perspective on preferences for remote learning. ε_{ikj} corresponds to uncertainty for option j presented in hypothetical k. As families are asked to make decisions between otherwise identical schools in the *future*, there is an element of uncertainty that is introduced in each hypothetical choice.We collect a rank-ordered list for each scenario, so the school ranked first in each scenario is

$$R_{1ik} = \underset{j \in \mathcal{J}_k}{\operatorname{argmax}} \, U_{ikj}$$

and the following options satisfy

$$R_{irk} = \underset{j \in \mathcal{J}_k \setminus \{R_{imk} : m < r\}}{\operatorname{argmax}} \qquad r > 1, k = 1, \cdots, 10.$$

The logit assumption implies that the likelihood function for a given individual i and hypothetical scenario k is

$$\mathcal{L}(R_{ik} | Q_j, Remote_j, d_{ij}, X_i) = \frac{\exp(V_{iR_{i1k}})}{\sum m \in \{R_{i1k}, R_{i2k}, R_{i3k}\} \exp(V_{im})} \frac{\exp(V_{iR_{i2k}})}{\sum m \in \{R_{i2k}, R_{i3k}\} \exp(V_{im})}$$

We estimate preference models separately for each covariate cell via maximum likelihood and obtain a vector of coefficients for each cell c.

With experimental estimates of $(\omega_{Qc(X_i)}, \omega_{Rc(X_i)}, \omega_{dc(X_i)})$ we can construct the implied student-specific observable component of indirect utility for remote schooling v_i. In particular,

$$v_i = \omega_{Rc(X_i)} + \omega_{Qc(X_i)} Q_{j(i)} - \omega_{dc(X_i)} d_{j(i)}$$

where $Q_{j(i)}$ is remote achievement relative to student i's neighborhood school and $d_{j(i)}$ is the

travel time to student i's neighborhood school. Appendix X reports the summary statistics of the preference estimates

With estimates of v_i, we estimate the following regression:

$$Y_i = \alpha_c + \gamma' X_i + \beta D_i + \theta P(v_i) + \psi P(V_i) \times D_i + e_i$$

where α_c are covariate cell fixed effects and X_i is a remaining vector of covariates not part of the covariate cells.[11] Standard errors are robust and double clustered at the school and covariate cell level to account for correlation in errors within cells across schools induced by the preference estimation.

4 Results

Figure 2 reports our main results. Panel (a) reports lagged achievement balance. The black bars show that conditioning on a rich set of covariates commonly used in the value-added literature (Koedel and Rockoff, 2015) unsuccessfully balance lagged achievement, with differences ranging between 15-20 percent of a standard deviation. In contrast, both of our empirical strategies mostly eliminate differences in baseline achievement. We successfully balance lagged math scores and the coefficients on ELA are reversed and marginally significant. The ability to mostly balance lagged achievement is reassuring from a causal perspective (Rosembaum and Rubin, 1983).

Panel (b) reports similar estimates but for 2021-2022 achievement. Canonical value-added estimates that condition on a rich array of student attributes and lagged achievement produce remote-learning negative effects ranging between 25-30 percent of a standard deviation. These estimates are in line with other research leveraging alternative quasi-experimental methods but also finding negative selection into remote learning (Bueno, 2020). In contrast to these rather large effects, our two empirical approaches produce more moderate negative remote-learning effects for the average student. Math effects for the average student are -0.13σ and -0.11σ for the propensity score and control function approach, respectively. Similarly, ELA effects are -0.11σ and -0.10σ for the two approaches. These results highlight and corroborate a growing body of evidence suggesting that remote learning tends to produce adverse outcomes for the average student (Bueno, 2020, Goldhaber et al., 2022, Jack et al., 2022, Singh et al., 2022).

The preference data we collect allows us to assess potentially rich sources of selection patterns into remote learning. We find evidence of negative selection on levels, with $\theta < 0$, and evidence of selection on gains, with $\psi > 0$. The finding of negative selection on levels mirrors findings in Bueno (2020) that students selecting into remote offerings pre-pandemic were trending negatively in terms of their achievement.In our setting, however, there are students who particularly benefit from remote learning modality relative to in-person. Figure 3 reports treatment effects across the (demeaned) propensity score distribution. While the average remote effect is negative, we find substantial heterogeneity in remote effects with respect to preference intensity. For example, students with a one standard deviation increase in remote learning effects have a 0.10σ larger remote learning effect. Figure 3 reports estimates based on the

[11] Due to power limitations we are unable to fully stratify on X and proceed in this manner instead.

implied linear extrapolation. The figure suggests that most students experience learning loss from remote instruction relative to in-person. There are a sizable share of students, however, that do experience positive remote learning effects. Taking our estimates at face value suggests that students in the top decile of the estimated propensity score distribution begin experiencing positive remote learning effects. These may be students for whom self-pace learning is more adequate (Armstrong-Mensah et al., 2020) or potentially benefit from less bullying (Bacher-Hicks et al., 2022) among other potential explanations. Future work can better understand why some students are particularly well-suited for remote offerings.

Robustness Checks

We now address a variety of concerns in our empirical approach. First, our results hinge on us adequately recovering preference estimates to then construct empirical objects that govern selection into remote learning. We emphasize the importance of the experimental estimates by reporting evidence from an exercise that constructs the empirical objects using observational approaches. In addition to not balancing lagged achievement (see Column 4 of Table 1), we find observational estimates produce willingness to travel estimates for academic quality that are four times as large as the experimental estimates. Further, the observational estimates yield vastly different selection patterns; we find evidence of negative selection on gains. The estimates are also not statistically distinguishable from the baseline value-added estimates and reported in Appendix Figure B.1. The lack of baseline balance with estimates that are observationally equivalent to other observational approaches emphasizes the importance of the experimental preference estimates.

Our preference estimates are estimated on a subsample of conjoint respondents, and the conjoint respondents have noticeably different attributes compared to the typical student in the district. Our first attempt to circumvent this issue is to estimate preferences by covariate cells that allow us to capture preference heterogeneity flexibly. Appendix Figure B.2 demonstrates that although our mean conjoint respondent is positively selected, there is substantial overlap across a summary index of covariates.[12] This indicates that we have ample support to estimate preferences and extrapolate within cells to non-survey respondents. Nonetheless, the extrapolation procedure can still fail given that our preference estimates are estimated from a highly selected sample.

We probe this issue and assess the internal consistency of the extrapolation using a bootstrapping procedure. First, we randomly sample from our conjoint respondents, stratifying to mirror the average student in the district. We then estimate propensity scores using the bootstrapped sample. Next, we estimate propensity scores using the residual sample and our covariate cell approach and extrapolate preference estimates from the residual sample to the representative bootstrap sample. We then compare the two propensity scores within the bootstrapped sample to assess the extrapolation quality. Appendix Figure B.3 reports a binscatter plot of the extrapolated propensity scores against the truth. We find a slope coefficient of 1.0005 and an intercept of 0.006, with each statistically indistinguishable from 1 or 0, respectively. Ap-

[12]We predict test scores using an array of student covariates, including lagged achievement and report the resulting predictions in Appendix Figure B.2.

pendix Figure B.4 reports a histogram of the difference, with the mean −0.006 and tightly around zero with standard deviation 0.06.[13] This evidence suggests that the extrapolation procedure is internally consistent and likely to produce reasonable estimates in our sample. We conclude by highlighting that these kinds of extrapolations are common in the literature. For example, preference estimation relying on stability assumptions (Fack et al., 2019) extrapolate preferences from larger choice sets to those with smaller choice sets (Agarwal and Somaini, 2020) with subsequent extrapolation to potential outcome models (Otero et al., 2021). In these settings, the extrapolation is more subtle while in our setting is front and center and we provide suggestive evidence that the extrapolation is not a first order concern.

5 Conclusion

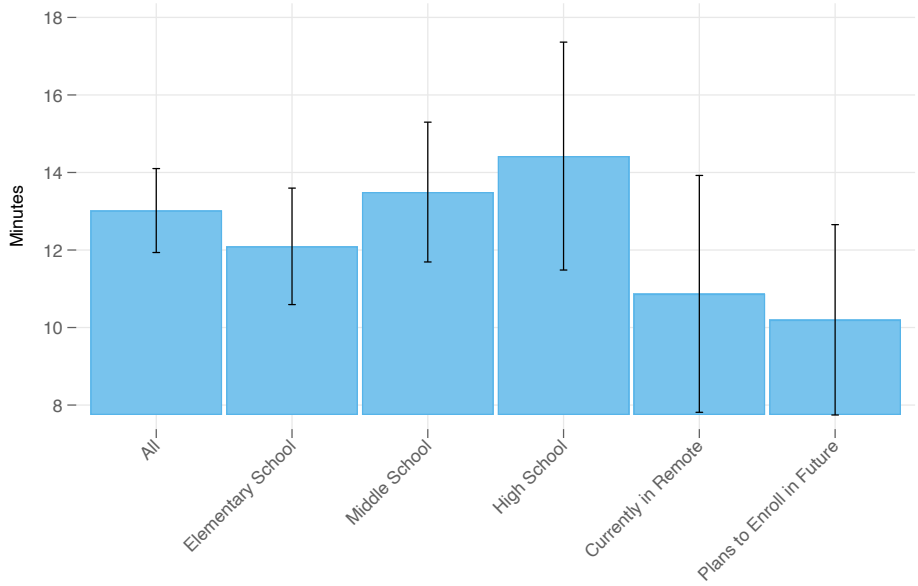
In this paper, we link novel survey data with administrative data to study both demand and effects of remote learning in a post-pandemic landscape. The pandemic provided an unusual setting for families to learn about their relative suitability for remote instruction, and perhaps not surprisingly, there is residual demand for remote offerings across districts and a more self-selected sample of families (Musaddiq et al., 2022). While a growing body of evidence demonstrates that the pandemic led to sizable learning loss and that remote learning played a sizable role (Goldhaber et al., 2022, Jack et al., 2022), these effects are potentially heterogeneous for those opting into remote learning post-pandemic.

We find that a significant share of families want districts to expand their remote offerings and that believe their children excelled in remote instruction relative to in-person. Consistent with perceived match effects, we find that families currently selecting into remote learning benefit in terms of achievement gains relative to in-person instruction. At the same time, we corroborate a growing body of evidence demonstrating remote-learning contributes to learning loss for the average student. This heterogeneity in learning gains points to potential allocative improvements in the K-12 sector as those who benefit from remote learning seem to be self-selecting to realize their match effect.

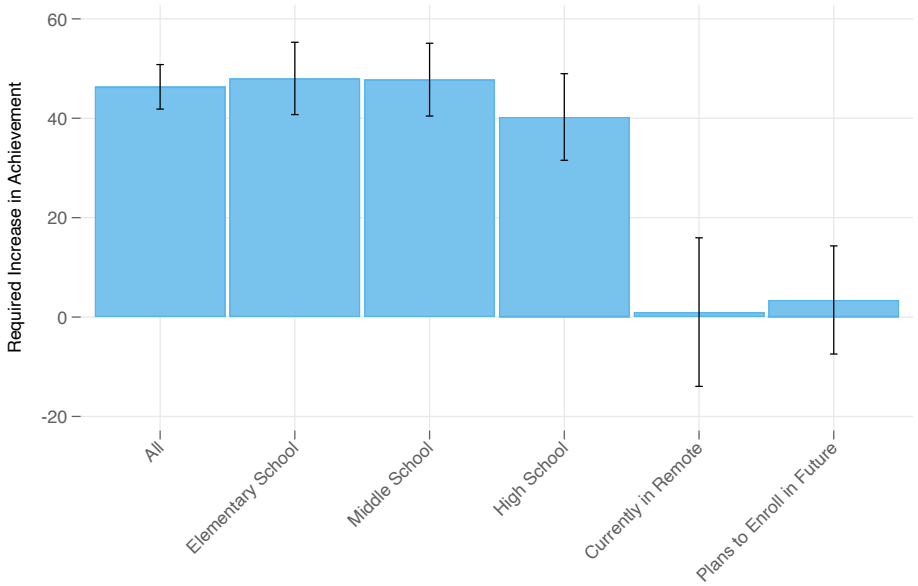
While our findings suggest that expanded remote offerings post-pandemic can potentially improve student outcomes for those selecting in and not forced in, our paper does not speak to why these students benefit and others do not. Our demand estimates are cross-sectional and do not capture potentialy dynamics at play. These are both important avenues for future work.

[13] The standard deviation estimate does not account for estimation error, so the true standard deviation is even smaller.

Figure 1: Experimental Preference Estimates



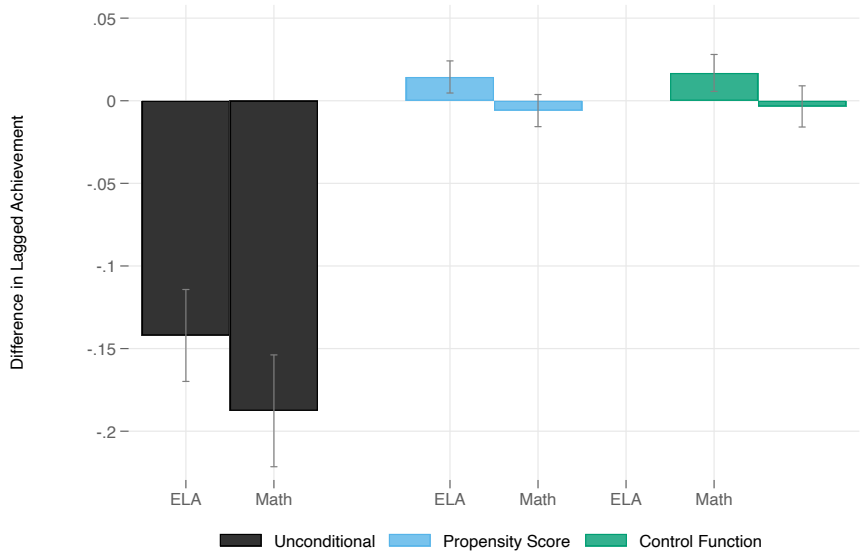
(a) Willingness to travel for achievement



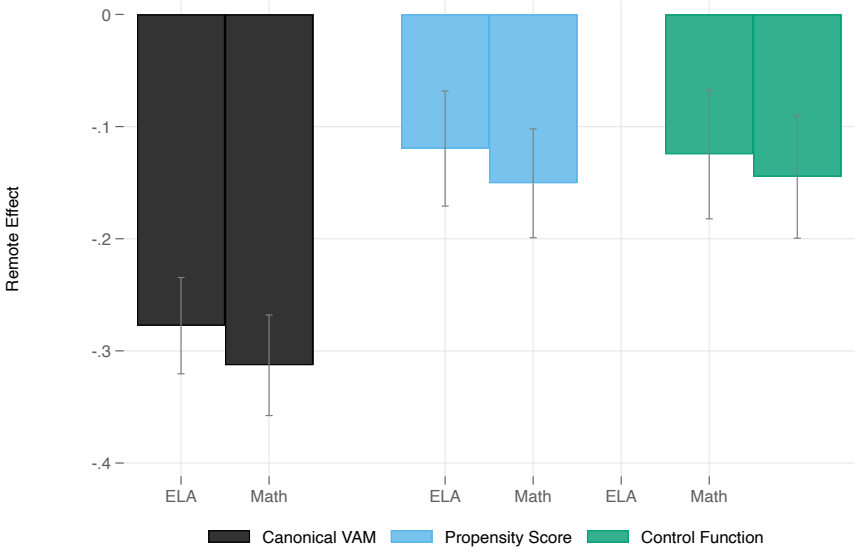
(b) Remote learning preferences

Notes: This figure reports willingness to travel estimates for achievement in Panel (a) and the estimated achievement necessary to make families indifferent between in-person and remote learning in Panel (b). Preference estimates are from a rank-ordered logit model relating indirect utilities of hypothetical choices to ranodmized school attributes, including academic quality, travel time, and remote status. Options that are designated as remote have travel time equal to zero. Each bar corresponds to estimates from a different sample. For example, the "All" bar in both Panel (a) and Panel (b) correspond to estimates for the complete conjoint sample. The next three bars estimate preferences separately for students in different grade levels. The "Currently in Remote" estimates are for the sample of families who have students enrolled in the remote option at the time of the survey. The "Plans to Enroll in Future" sample is the subsample of families that indicate they plan on enrolling their children in remote learning options in the future. Standard errors are robust and clustered at the respondent level.

Figure 2: Baseline Balance and Treatment Effects



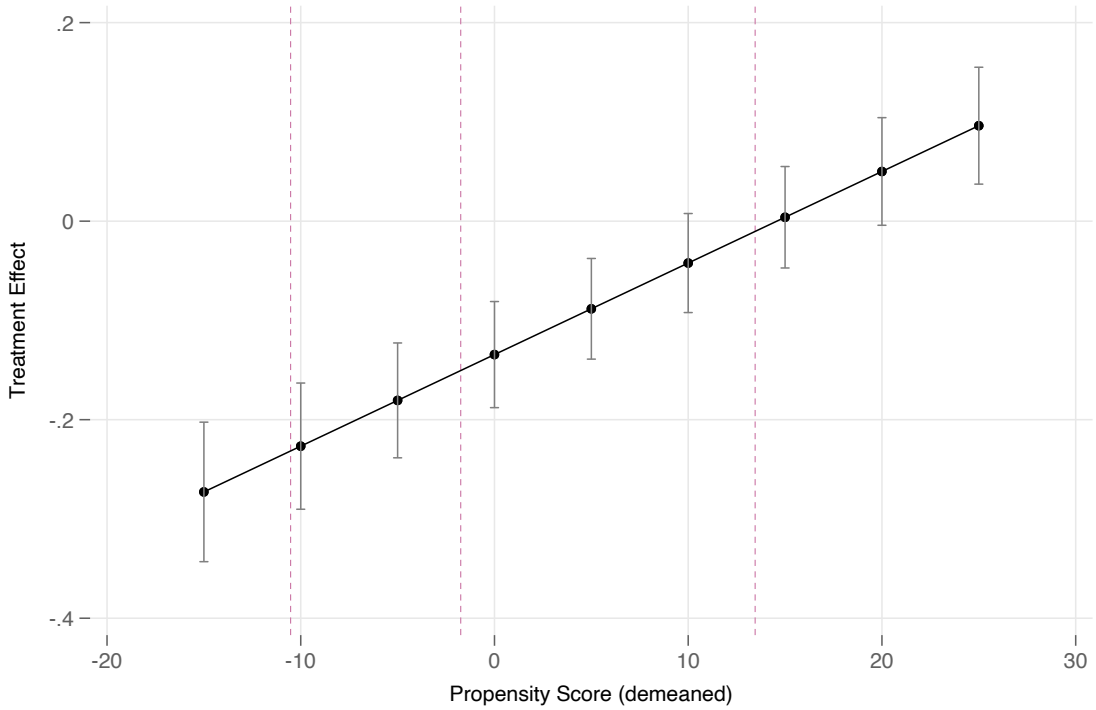
(a) Balance



(b) Treatment Effects

Notes: This figure reports estimates for six separate regression models, for 2022 achievement and 2019 achievement. Panel (a) reports balance on lagged achievement, 2019 test scores (Math and ELA). Black bars correspond to models regressing 2019 achievement on a remote indicator, grade-level indicators, and baseline student characteristics. Blue bars correspond to models augmenting the specification with estimated propensity scores, and green bars replace the propensity score with control function estimates. Panel (b) reports treatment effects on 2022 achievement. Black bars correspond to models regressing 2022 achievement (Math and ELA) on a remote indicator, grade-level indicators, baseline student characteristics, and lagged achievement measured in 2019. Blue bars correspond to models that regress 2022 achievement (Math and ELA) on a remote indicator, baseline covariates, estimation cell indicators, propensity score estimates, and propensity scores interacted with remote indicators. Green bars are similarly defined but use implied control function estimates instead of propensity scores. Propensity scores (control functions) are demeaned so that the coefficient on the remote indicator corresponds to the average treatment effect in the sample. Standard errors are robust and double clustered at the school and covariate cell level. 95 percent confidence intervals are reported in gray bars.

Figure 3: Estimated Match Effects



Notes: This figure reports estimated treatment effects across the estimated propensity score distribution. This amounts to summing the estimated average treatment effect with the product of the estimated match effect and propensity score (the sum of θ and ψ in the regression equation on page 10. The propensity score distribution is demeaned so that the estimate at zero corresponds to the average treatment effect for the average student. Three additional red dashed vertical lines are reported, corresponding to the 10th, 50th, and 90th percentile of the estimated propensity score distribution. Standard errors are robust and double clustered at the school and covariate cell level. 95 percent confidence intervals are reported by gray bars.

Table 1: Baseline Balance

	(1) Remote	(2) In Person	(3) Difference	(4) Observational	(5) Experimental
Lagged ELA	-.143	.001	-.144***	-.375***	.103*
			(.042)	(.019)	(.061)
Lagged Math	-.181	.007	-.189***	-.392***	.067
			(.051)	(.02)	(.061)
Female	.508	.483	.024***	.029**	-.029
			(.004)	(.014)	(.037)
Special Education	.151	.141	.009	.035***	-.014
			(.015)	(.009)	(.016)
URM	.844	.823	.021	.213***	.006
			(.015)	(.014)	(.01)
Students	12,726	255,310			

Notes: This table reports mean student characteristics in for remote and in-person students in Column 1 and Column 2. Column 3 to Column 5 reports a sequence of mean differences with corresponding standard errors in parentheses below. Column 3 reports mean differences only adjusting for grade-level fixed effects. Column 4 reports mean differences after adjusting for an observational propensity score. The observational propensity score is estimated in a probit model predicting remote enrollment based on remote relative achievement, remote relative travel time, and baseline student characteristics. Column 5 reports mean differences using our preferred model, which adjust for the experimental propensity score as derived from the conjoint experiments.

Table 2: Respondent Descriptive Statistics

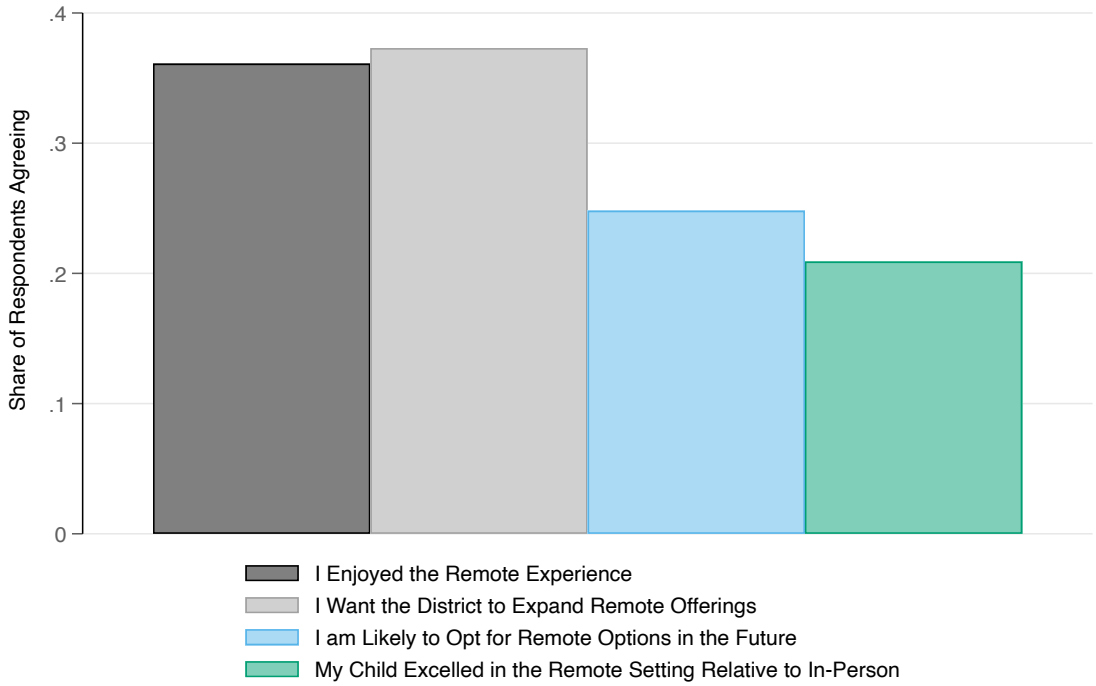
	All Students	Survey Respondents	Conjoint Respondents
Lagged ELA	0	.18	.46
	(1)	(1.05)	(1.05)
Lagged Math	.01	.17	.44
	(1)	(1.01)	(1.02)
Female	.48	.48	.49
	(.5)	(.5)	(.5)
Special Education	.12	.1	.09
	(.32)	(.3)	(.29)
URM	.8	.76	.65
	(.4)	(.43)	(.48)
N	225,696	3,447	1,148

Notes: The survey was sent to 100K families randomly sampled from students enrolled in grades 3-8 and grade 11. The column labeled "all students" presents averages for every student in the relevant grades. The column labeled "survey respondent" display averages for every family that completed at least one question on our survey. The column labeled "Conjoint respondents," displays averages for every student that completed the conjoint survey experiment within the survey.

ONLINE APPENDIX. Not for Publication

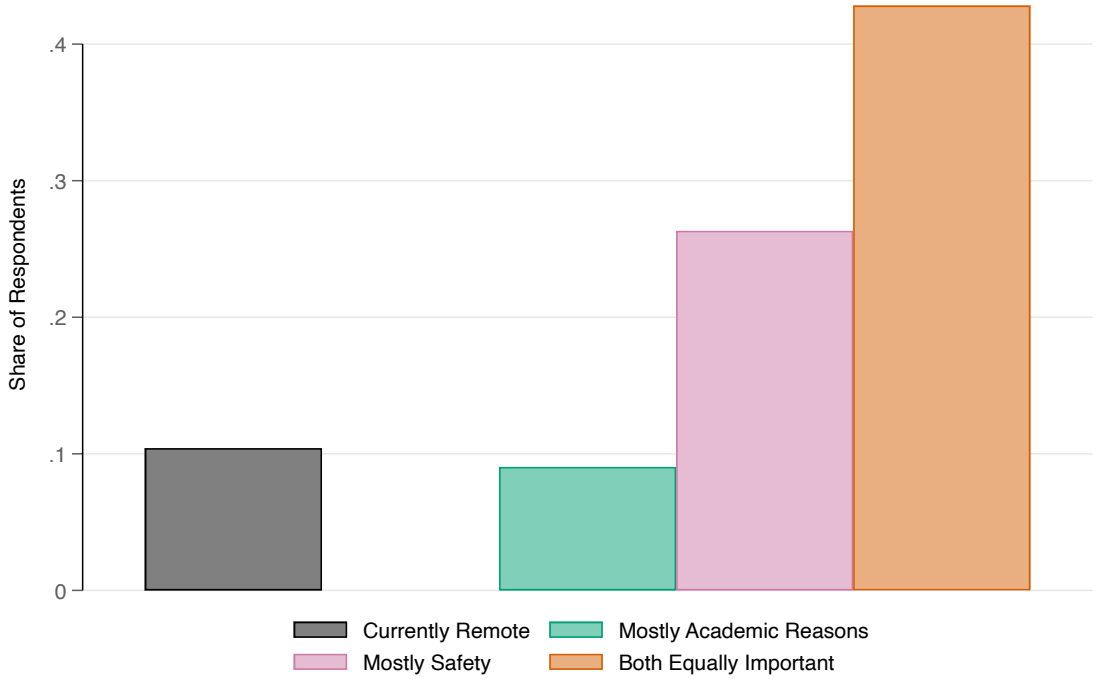
A Survey Details

Figure A.1: Survey Question Statistics



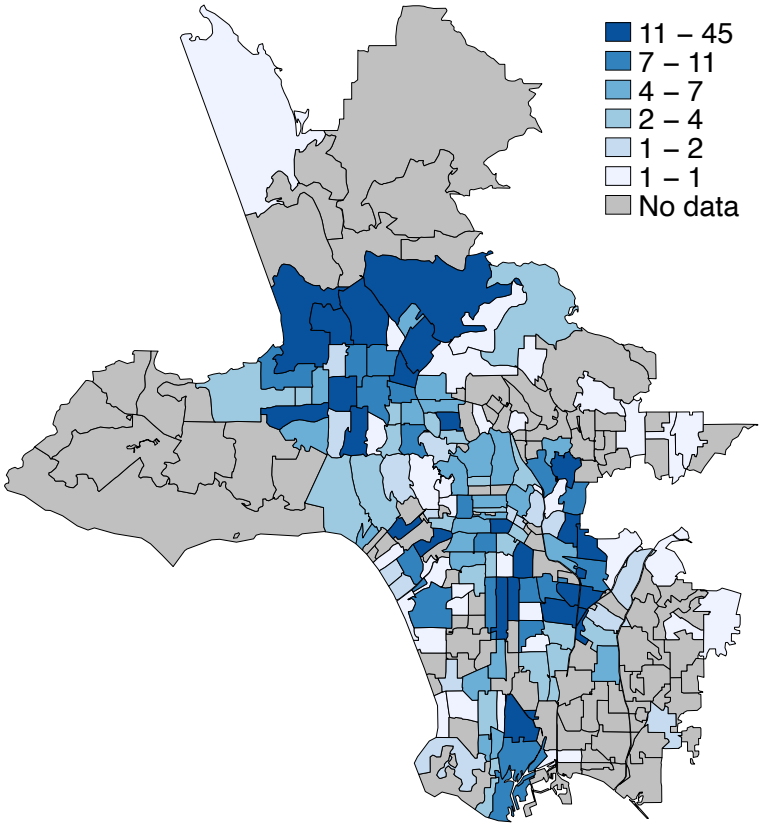
Notes: This figure reports the share of respondents agreeing with four statements on the survey. Appendix Section C reports the actual survey questions, all part of Question 5.

Figure A.2: Reasons for remote learning statistics



Notes: This figure reports various statistics relating to respondents' current remote learning status and reasons for that. The first dark gray bar reports the share of respondents with students currently enrolled in remote learning. The next three bars report shares of respondents' reason for remote learning, conditional on current remote status.

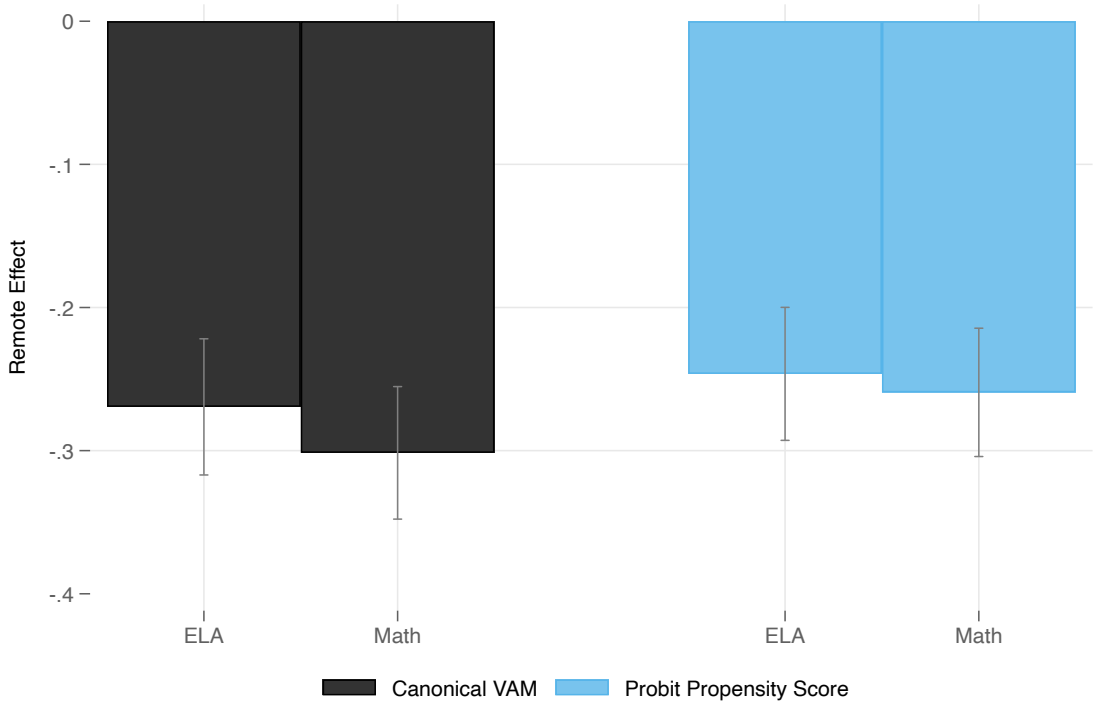
Figure A.3: Spatial Distribution of Remote Learning Students



Notes: This figure reports the spatial distribution of survey respondents. Each shaded polygon corresponds to a census tract and is shaded according to the number of remote learning respondents residing in the census tract. Most of the gray areas in the figure are outside the purview of LAUSD.

B Additional Results

Figure B.1: Treatment effects using observational probit model



Notes: This figue reports treatment effects on 2022 achievement (Math and ELA) for conventional value-added models and a propensity-controlled model derived from preferences estimated using observational data. The "Canonical VAM" estimates correspond to regressions of 2022 achievement on remote indicators, baseline covariates, grade indicators, and lagged achievement. The "Probit Propensity Score" estimates are derived from a model that augments the model with the implied propensity score from the observational data. Propensity scores are demeaned so that remote effects correspond to average effects.

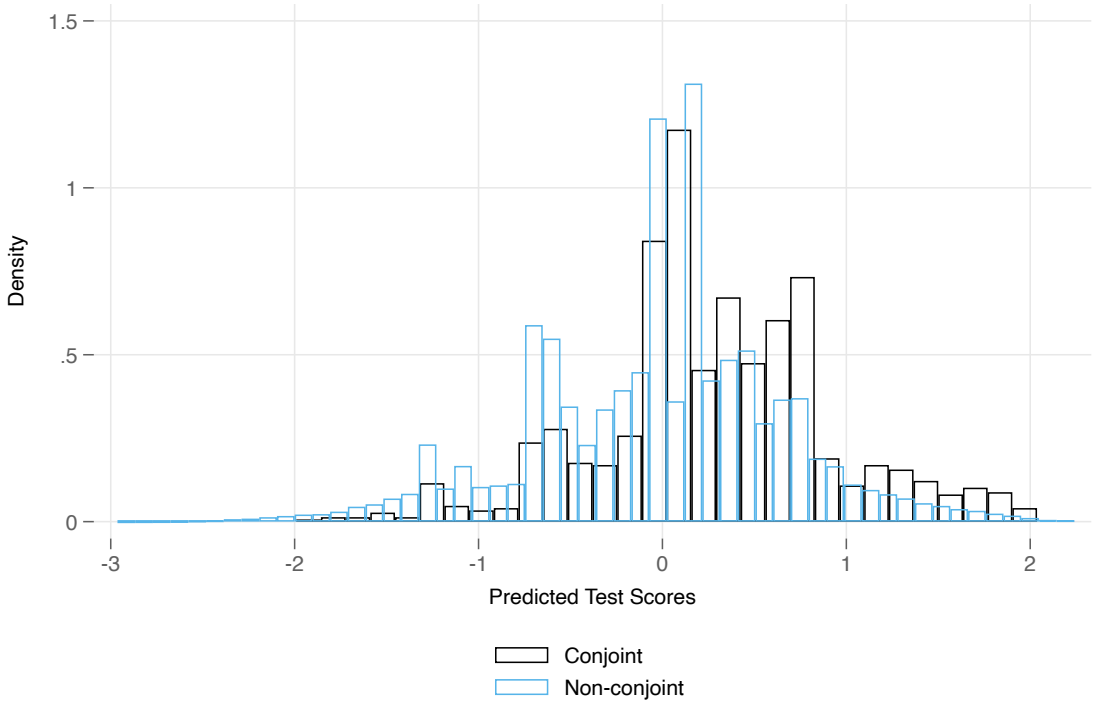
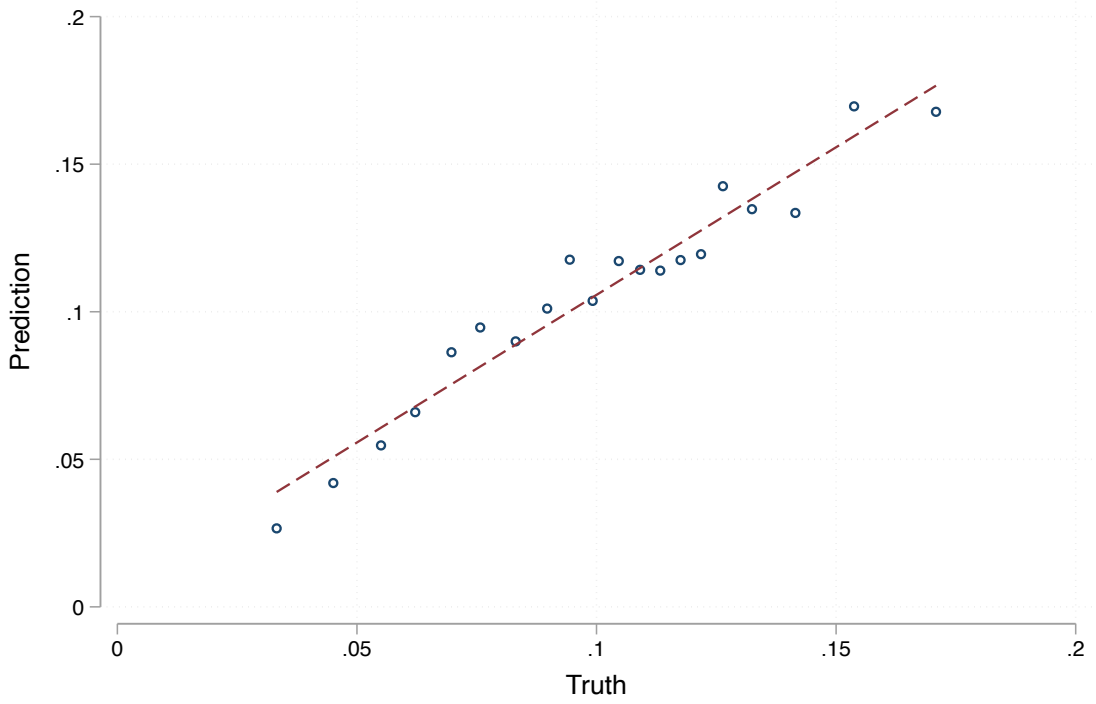


Figure B.2: Covariate Overlap

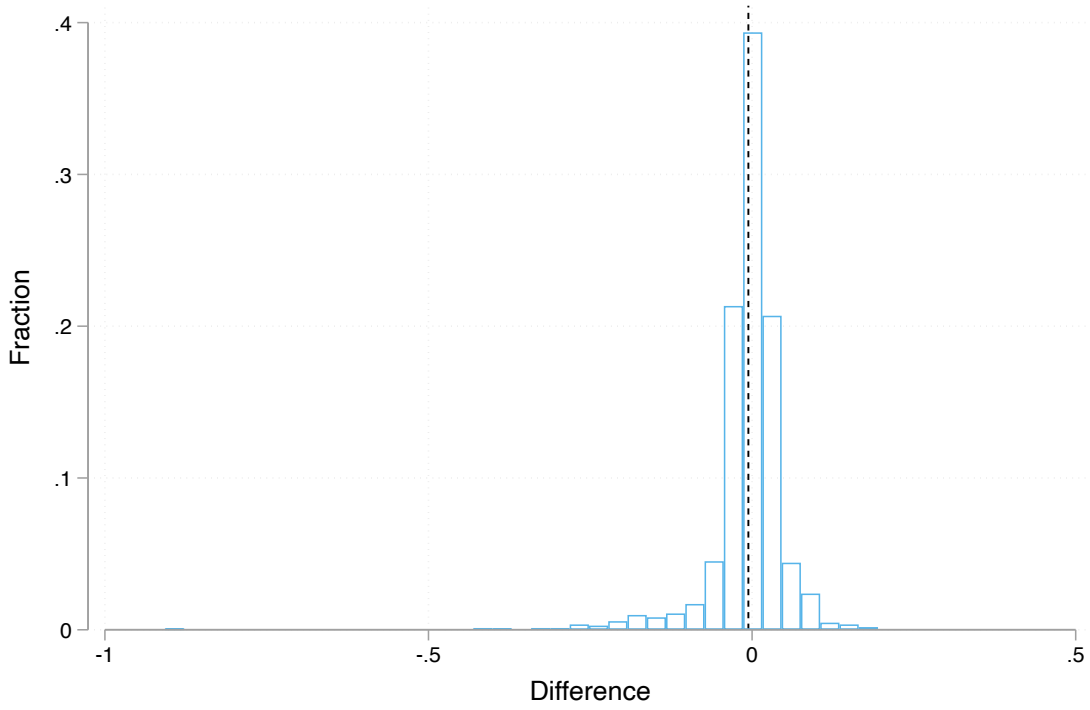
Notes: This figure reports the distribution a summary index of covariates for students in the conjoint sample (used to experimentally estimate preferences) and the non-conjoint sample. The summary index is constructed by regressing 2022 test scores on an array of student characteristics including lagged achievement. The summary index corresponds to the predicted values from this regression. The purpose of the histogram is to show that there is sufficient overlap between the conjoint and non-conjoint samples used in the empirical analysis.

Figure B.3: Bivariate Relationship between true propensity and extrapolated propensity scores



Notes: This figure reports the bivariate relationship betweeen propensity scores estimated in our bootstrapping procedure. First, we randomly sample from our conjoint respondents, stratifying to mirror the average student in the district. We then estimate propensity scores using the bootstrapped sample. Next, we estimate propensity scores using the residual sample and our covariate cell approach and extrapolate preference estimates from the residual sample to the representative bootstrap sample. We then compare the two propensity scores within the bootstrapped sample to assess the extrapolation quality. The "Prediction" propensity scores are those predicted propensity scores using our preferred approach and extrapolated to representative bootstrapped sample. We compare the "Prediction" propensity scores to "Truth" which correspond to propensity scores estimated within the bootstrapped sample and mirror propensity scores estimated using a representative sample of students.

Figure B.4: Histogram of difference between true propensity and extrapolated propensity scores



Notes: This figure reports a histogram of the difference between the extrapolated propensity score and the true propensity score estimated using a representative bootstrapped sample.

C Remote Learning Survey

LAUSD Remote Learning Survey

(untitled)

1. Are you a mother, father, or guardian of a K-12 student? *

- Mother
- Father
- Guardian

2. In what grade is your oldest child currently enrolled? *

Kindergarten
1
2
3
4
5
6
7
8
9
10
11
12

3. Is your oldest child currently enrolled in a virtual schooling option?

- Yes
- No

4. Did you choose a remote option mostly for academic or safety (COVID) reasons? *

- Mostly academic reasons
- Mostly safety reasons
- Academics and safety were equally important

(untitled)

5. For the following, please tell us if you agree or disagree.*

	Agree	Disagree
My child excelled academically with the virtual experience compared to in-person instruction.		
I would like the district to expand its virtual offerings in the future.		
I am likely to opt for virtual schooling in the future.		
I enjoyed the virtual schooling experience during the pandemic.		

(untitled)

6. You will now see a sequence of scenarios, each with three school options that the school district could offer you in Fall 2022. For each set of three, indicate the one you prefer the most (Best) and the one you prefer the least (Worst).

Recall that a fully remote option is entirely virtual (100% remote) and traditional in-person instruction is 0% remote.

Travel time corresponds to the commute time in minutes from your home to the school. For traditional in-person instruction, students make the trip to school every day.

Assume pandemic-related safety issues are as they were in 2019 before COVID.

Besides the characteristics shown, assume that these schools are otherwise identical in terms of their academic instruction and quality.

There are no right or wrong answers to these questions. We only want to know which of the options you would most prefer.

*

Type of Instruction	In Person	In Person	In Person
Percent of students meeting state academic standards	50	30	90
Travel time to school (minutes)	15	30	45
Best	○	○	○
Worst	○	○	○

(untitled)

7. Do you think your choices will be similar in Fall 2023?*

- Yes
- No

(untitled)

8. Thank you for taking the time to answer these questions! We now ask that you let your student in grade 8 through 11 answer the remaining questions, so we can learn more about their experience with remote learning.

Will your child be answering the remaining questions? *

- Yes
- No

(untitled)

9. For the following, please tell us if you agree or disagree.*

	Agree	Disagree
I am likely to opt for virtual schooling in the future.		
I excelled academically with the virtual experience compared to in-person instruction.		
I would like the district to expand its virtual offerings in the future.		

(untitled)

10. You will now see a sequence of scenarios, each with three school options that the school district could offer you in Fall 2022. For each set of three, indicate the one you prefer the most (Best) and the one you prefer the least (Worst).

Recall that a fully remote option is entirely virtual (100% remote) and traditional in-person instruction is 0% remote.

Travel time corresponds to the commute time in minutes from your home to the school. For traditional in-person instruction, students make the trip to school every day.

Assume pandemic-related safety issues are as they were in 2019 before COVID.

Besides the characteristics shown, assume that these schools are otherwise identical in terms of their academic instruction and quality.

There are no right or wrong answers to these questions. We only want to know which of the options you would most prefer.

*

Type of Instruction	In Person	In Person	In Person
Percent of students meeting state academic standards	90	60	30
Travel time to school (minutes)	75	30	15
Best	○	○	○
Worst	○	○	○

(untitled)

11. Do you think your choices will be similar in Fall 2023? *

- Yes
- No

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