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

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July 7, 2023

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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 while also studying selection patterns and treatment effect heterogeneity. We find a negative average effect of remote learning on reading (-0.14σ) and math (-0.17σ). Notably, we find evidence of positive learning effects for children whose parents have the strongest demand for remote learning. Our results suggest an important subset of students who currently sort into post-pandemic remote learning benefit from expanded choice.

JEL Classification: I21, I24.

Keywords: Remote learning, COVID-19, school match effects.

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1 Introduction

Classic theories of education markets predict that school choice can improve the allocative efficiency of sorting students to schools (Hoxby, 2003). By improving match quality, choice policies hold the potential to engineer improved student outcomes. Yet, existing research in the U.S. fails to find meaningful evidence that student-school match effects exist at all (Abdulkadiroğlu et al., 2020, Mountjoy and Hickman, 2020). Moreover, 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 the gap between theory and data. Families may not know their match quality when choosing schools and only learn gradually through trial and error (Arcidiacono et al., 2016, Larroucau and Rios, 2020). The coronavirus disease (COVID-19) pandemic provides an unusual and unique setting in which families were compelled to assess their relative suitability for a particular schooling option: remote learning. Although mounting evidence shows 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). The continued demand for remote learning underscores the need for a deeper understanding of which students are best suited for this schooling option.

This paper studies remote learning and 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 had to participate in virtual learning, with the following year involving a cycle of in-person and remote periods. This unusual experience allowed families to assess 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 but continued to offer a remote-learning option that was chosen by 14,000 students. 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, implying 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, suggesting academic success may also be a factor.

Our analysis relies on a novel survey that we designed to learn about family experiences and preferences for remote learning. Following previous research using choice experiments to understand 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

¹Bau (2022) is an exception, providing evidence from Pakistan on the importance of match quality. Bruhn (2019) finds substantial match effects in terms of sorting between districts.

²This cycle of remote to in-person learning in L.A. is remarkably similar to the experience of other school districts across the U.S. (Jack et al., 2022). Our setting provides a natural context for studying ongoing selection into remote learning.

while holding remaining school attributes fixed.³

The data on parental preferences serve 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 plausibly have a similar propensity to choose the remote option. More concretely, this strategy relies on the notion that preference heterogeneity is driving the selection into remote learning. Under this assumption, we can draw from existing literature on selection corrections built from revealed preferences to recover unbiased estimates of the causal effect of remote learning on student outcomes (e.g., Abdulkadiroğlu et al., 2020, Einav et al., 2022, Heckman, 1979, Kline and Walters, 2016, Mountjoy and Hickman, 2020, Rosembaum and Rubin, 1983). Our approach differs in that our preference estimates come from experimental data rather than 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 if families with stronger preferences for the remote option experience greater causal benefits.

Our analysis of the remote-learning survey data begins with a basic descriptive analysis of parental experiences and demand for remote learning. Although most respondents report having a negative experience with remote learning during the pandemic, one-third want expanded remote offerings, and a quarter expect to enroll their children in remote learning in the future. Moreover, 20 percent feel their children excelled in remote learning relative to traditional, in-person instruction. These findings suggest there is substantial scope for permanent, post-pandemic remote offerings to generate improvements in match quality.

Hypothetical choice data allow us to move beyond 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, the average family has a strong distaste for remote learning: they would need to be compensated with a 40 percentage point increase in academic standards to be indifferent between remote and in-person offerings. Reassuringly, we do not find a distaste for remote learning among families currently enrolled in remote offerings or among those who 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 that we propose considers selection bias governed by students’ preferences for remote learning, which we can estimate directly using our choice 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 both baseline achievement and a summary index of baseline student characteristics, which we show is infeasible when conditioning on a rich set of covariates alone. In terms of causal impacts, we estimate average

³Prior work finds that preference estimates from similar experiments contain a high degree of external validity (Wiswall and Zafar, 2018).

remote-learning effects of -0.14σ on reading and -0.17σ on math. These estimates differ substantively from simple models: regression adjustment with lagged achievement and standard covariates generates estimates ranging between -0.28σ and -0.31σ .

Our final analysis studies heterogeneous remote-learning effects. We find evidence of negative selection on levels, indicating that students with high demand for remote learning perform poorly regardless of the school they enroll in. In contrast, our analysis also finds evidence of positive selection on gains, suggesting families choose remote learning, at least partly, using factors that correlate with their child’s suitability for remote instruction. This has important policy implications for understanding the efficiency of ongoing efforts to expand remote offerings. Taking our extrapolation at face value implies that students above the 90th percentile of remote-learning proclivity fared no worse in remote instruction, while those at the 95th percentile and above experienced improvements of at least 0.04σ . This suggests that prior estimates of the impact of remote learning during the pandemic may not accurately predict the future effects that expanded remote offerings could have on the students who opt in.

We address several 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 connecting any choice data to the reduced-form approach and underscores the importance of our survey data. Second, we show that our estimates are robust to alternative parameterizations of the underlying choice model. Last, we adopt empirical approaches that account for selection on unobserved preference heterogeneity (Abdulkadiroğlu et al., 2020) and find qualitatively similar evidence, assuaging concerns about selection on unobserved dimensions.

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 but also documents negative trends before the switch to remote. More recent evidence estimates remote-learning effects during the pandemic, reaching a 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 freely opting into remote schooling. 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 studies 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 suggesting the importance of match quality based on observables (Bau, 2022, Bruhn, 2019) and some suggesting the contrary (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 on canonical work in economics that has used hypothetical choice surveys 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.

2 Background and Data

2.1 Remote Schooling in Los Angeles

As in most U.S. school districts at the onset of the pandemic, the LAUSD closed their schools and transitioned to remote learning on March 19, 2020. Swift actions were taken to buffer the shock, including creating online videos, coordinating meal distribution, distributing laptops and tablets, and using private donations to provide broadband access and equipment for students. Students remained at home for the rest of the academic year.

The following academic year (2020–2021) started virtually, with a schedule that included daily interactions between teachers and students. While in-person tutoring services were offered, its provision ebbed and flowed with each Covid wave. 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 continue with remote learning.

The LAUSD’s response to the pandemic meant that, for roughly one year, students in the district remained at home and received instruction virtually. Anecdotal evidence suggests most families disliked the online experience, and mounting evidence suggests this contributed negatively to student learning.⁴ However, there is also evidence that suggests *some* subset of families may have preferred remote learning. For example, bullied students may excel without the mental health costs incurred from in-person schooling (Bacher-Hicks et al., 2022), and others may benefit from learning at their own pace and reduced disruption (Armstrong-Mensah et al., 2020).⁵ This unusual experience provided families and students ample time to assess their relative suitability for remote learning.

LAUSD returned to full in-person learning for the 2021–2022 academic year.⁶ To accommodate a sizable share of families who continued to prefer remote learning, the district did not

⁴For example, Williams (2022) discusses student and parental frustration with remote schooling.

⁵Media accounts also testified to remote-learning benefits for some students (Harris, 2020).

⁶California mandated that all school districts had to offer a remote option during 2021–2022 due to COVID-19-related concerns.

make in-person learning mandatory and created a new online option called the City of Angels. This option offered self-paced learning with regular interactions with virtual instructors and the opportunity to receive in-person tutoring. Remote students could transition to in-person learning at any time. We focus on the cohort 2021–2022 students who could self-select into remote offerings. These students had at least one year to adapt to remote instruction and assess their own relative suitability for remote learning.

2.2 Data

Our analysis uses administrative LAUSD data linked to survey data that we collect. The administrative data are standard, containing student-level demographics and test scores. Our analysis uses 2018–2019 test scores as measures of lagged achievement⁷ and relies on 2021–2022 scores as outcomes.

Our key data innovation is a survey of a sample of parents with LAUSD students enrolled in grades 3–8 and grade 11 in April 2022.⁸ Invitations for the survey were distributed to a random sample of 100,000 families through LAUSD’s internal communications system. Because messaging was on behalf of the district, incentives were forbidden; however, families were informed that their responses could affect future policy decisions by the district.

The survey had two primary sections. The first section quantified experiences and perceptions about remote learning through basic descriptive questions. The second section measured preferences through a series of hypothetical choice experiments that were similar to those used in other settings (Mas and Pallais, 2017, Moshary et al., 2022, Wiswall and Zafar, 2018). In the hypothetical choices, parents trade off between preferences for academic quality, distance, and remote learning while holding all other attributes fixed. Section 3 provides further details on the preference measures, and Section 4 discusses how we use the estimated preferences as an input for our empirical strategy. A sample of 3,611 parents completed the basic descriptive survey questions, and 1,191 parents completed the hypothetical choice component. Respondents consented to have their responses linked to administrative records.

3 Survey Evidence

Appendix Table A.1 reports mean characteristics for our survey sample. For comparison, Column 1 reports averages for all LAUSD students enrolled in the relevant grades.⁹ Survey respondents noticeably differ from the typical student in LAUSD in terms of academic achievement.¹⁰ Families who initiated the survey have students performing above district averages, roughly 16–17 percent of a standard deviation. Notably, the academic differences are larger for the subset of families who completed the hypothetical choice questions. These respondents are also less likely to be classified as URM or special education students.

⁷The district did not administer standardized tests during the pandemic year.

⁸Appendix Section B reproduces the survey instrument.

⁹Although we sent the survey to 100,000 randomly sampled families, we report mean characteristics for the population.

¹⁰These differences do not appear to be driven by geographic differences in response rates. Appendix Figure A.1 shows that respondents represent all school district regions.

3.1 Descriptive Evidence

Our descriptive analysis focuses on responses to four statements on experiences and future demand for remote learning. Figure 1 illustrates the results by reporting the mean rates of disagreement (maroon) and agreement (black) for each statement. The results reveal two main findings. First, most respondents had negative experiences with remote learning during the 2021 academic year when LAUSD was fully remote. For example, 62 percent disagreed with the statement that their child enjoyed remote learning. These results are broadly consistent with other research that suggest students struggled with virtual schooling during the pandemic (Goldhaber et al., 2022, Jack et al., 2022, Loades et al., 2020). Second, a substantial fraction of respondents reported having positive experiences with remote learning. Most notably, 22 percent reported that their child excelled in remote learning. This latter finding highlights the possibility that the remote-learning experience may have improved families’ knowledge of their match quality.

3.2 Experimental Preference Estimates

We experimentally identify preferences using hypothetical choices. Each respondent is sequentially presented with $K = 10$ hypothetical choices, each involving three schooling options. Within each option, we randomized three school attributes: distance, peer achievement, and instruction mode (remote versus in person). As is standard with this approach, the survey stated that respondents should believe that the schooling options in each hypothetical were identical in terms of remaining (unspecified) schooling characteristics. The survey also attempted to shape respondent beliefs over safety by instructing them to make choices while assuming that pandemic-related safety conditions were at levels observed before the pandemic in 2019. Consistent with parents following this instruction, Appendix Section C shows that survey responses do not vary with local Covid-related conditions, outcomes, and predictors.

Our survey allows us to estimate a standard discrete choice model of schools using experimental data. Formally, our estimates are based on a model that assumes student i ’s indirect utility of enrolling in schooling option j is:

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

where V_{ij} is the observable component of indirect utility and the term ε_{ij} captures any remaining unobserved preference heterogeneity. Informed by a robust empirical school choice literature (Abdulkadiroğlu et al., 2020, Allende, 2019, Beuermann et al., 2022, Burgess et al., 2015, Campos and Kearns, 2022, Hastings et al., 2005, Neilson, 2021), we let the observable component of indirect utility be given by:

$$V_{ij} = \omega_Q Q_j + \omega_R \text{Remote}_j + \omega_d d_{ij}, \quad (1)$$

where Q_j is academic quality of school option j , Remote_j is a remote schooling indicator, d_{ij} is travel time (set to 0 for remote learning), and ε_{ij} are idiosyncratic preferences for schooling option j . A logit distributional assumption on ε_{ij} allows us to estimate the preference parameters

using an exploded logit framework (Hastings et al., 2005).

Figure 2a reports estimated mean willingness to travel estimates inferred from the choice experiments (i.e., $-\omega_Q/\omega_d$). 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 based on student grade level. Reassuringly, families currently in remote offerings or with plans to enroll in them have a lower willingness to travel for higher academic quality.

Figure 2b extends our analysis by showing the estimated achievement compensation needed to be indifferent between in-person and remote schooling (i.e., $-\omega_R/\omega_Q$). The average family would need to be compensated with a 42 percentage point higher achievement rate to be indifferent between in person and remote, implying that the average family has a strong distaste for remote learning. Importantly, we find that families currently in remote learning or those with plans to enroll in the future do *not* need such compensation, suggesting the survey responses contain an informative signal about preferences for remote instruction.

4 Empirical Strategy

4.1 Conceptual Framework

Our focus is estimating heterogeneous remote-learning effects and studying how selection patterns map to them. Our analytic framework is based on linking a discrete choice model to a potential outcomes model. Below we detail this framework by beginning with a potential outcome model.

We index a population of students by i , each of whom either attends school in person or remotely, which we denote using an indicator as $D_i = 0$ and $D_i = 1$, respectively. We assume that academic achievement Y_i is a function of a vector of observable characteristics, X_i , and a remote indicator that 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. Of course, 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 allows us to move toward the causal parameters of interest and to study patterns of selection into remote learning.

Our primary empirical strategy leverages rich preference information from the survey to account for selection into remote schooling. Intuitively, conditioning on the experimentally identified preferences allows us to compare two families who have a similar propensity to take up the remote option, with causal identification then following from the logic of Rosenbaum and Rubin (1983). Formally, our approach builds on Equation 1 and the associated distributional assumptions by assuming there are two schooling options, in-person $j = 0$ or remote schooling $j = 1$, and making the normalization $V_{i0} = 0$. Therefore, the indirect utility of remote learning

relative to in-person schooling can be compactly represented as:

$$u_i = v_i + \varepsilon_i,$$

where $u_i = U_{i1} - U_{i0}$, $v_i = V_{i1} - V_{i0}$, and $\varepsilon_i = \varepsilon_{i1} - \varepsilon_{i0}$. This model of school choice summarizes the information contained in the experimentally identified preferences by constructing the implied propensity score:

$$P(v_i) = \frac{\exp(v_i)}{1 + \exp(v_i)}.$$

For our analysis of student achievement, the propensity score summarizing parental preferences serves two purposes. First, as previewed earlier, conditioning on these preferences accounts for selection into remote schooling. Second, the propensity score serves as a measure of “preference intensity” that allows us to characterize how selection into remote learning governs treatment effect heterogeneity. Specifically, we assume the following model of achievement that allows for heterogeneous effects:

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

This approach has connections to the causal framework proposed by Rosembaum and Rubin (1983). If the experimentally identified preferences govern selection into treatment, then causal estimates follow from conditioning on the implied propensity score. Equation 2 assumes a linear relationship between the observable preference heterogeneity and potential outcomes, enabling an analysis of selection patterns in a similar spirit as Kline and Walters (2016), Abdulkadiroğlu et al. (2020), Otero et al. (2021), and Einav et al. (2022). For example, θ governs selection on levels, and ψ governs selection on gains, where $\theta > 0$ indicates that students with high tastes for remote learning do well regardless of the school they enroll in, while $\psi > 0$ indicates that those enrolling in remote options do better remotely rather than in person.

4.2 Propensity Score Estimates and Validation

As described above, we use choice experiments to obtain credible preference estimates for the subset of students with parents who completed our survey. To maximize statistical power, we use the full sample of LAUSD students.¹¹ As highlighted by Appendix Table A.1, one challenge with this approach is that our sample of LAUSD respondents differs from the general population of LAUSD students. To ensure that the preference estimates are representative, we use an extrapolation approach that assumes preferences vary flexibly with baseline student characteristics.

Formally, our extrapolation approach assumes that a student’s indirect utility over schooling choices takes the form:

$$U_{ijk} = \underbrace{\omega_{Qc(X_i)} Q_j + \omega_{Rc(X_i)} Remote_j + \omega_{dc(X_i)} d_{ij}}_{V_{ijk}} + \varepsilon_{ijk}, \quad (3)$$

¹¹The results based on only students who participated in the survey are qualitatively similar to our headline estimates but are less precisely estimated.

where the parameters $\omega_{Qc(X_i)}$, $\omega_{Rc(X_i)}$, and $\omega_{dc(X_i)}$ are allowed to vary flexibly by covariate cells, $c(X_i)$, defined by a combination of baseline achievement, gender, grade level, and URM status. This approach to modeling preference heterogeneity is similar to Abdulkadiroğlu et al. (2020) but is more limited due to the cell structure we assume.¹²

We estimate preference models separately for each covariate cell via maximum likelihood and obtain a vector of coefficients for each cell c . Given that the maintained assumption that ε_{ijk} is a Type I extreme value, the likelihood function for a given individual i and hypothetical scenario k can be written as:

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

To obtain propensity scores, we use estimates of $\omega_{Qc(X_i)}$, $\omega_{Rc(X_i)}$, and $\omega_{dc(X_i)}$ to compute an implied student-specific observable component of indirect utility for remote schooling v_i . Specifically, we construct:

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

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. The implied propensity score is $P(v_i)$. Appendix Table A.2 reports summary statistics for the preference estimates.

To address concerns regarding our extrapolation approach, we perform two exercises. One concern is that there is not sufficient overlap between the distribution of covariates of the subset students whose parents completed the choice experiment survey and all students. Appendix Figure A.2 summarizes baseline characteristics for each student using an index measure and plots the distribution for the survey and general LAUSD samples.¹³ The figure shows substantial overlap, indicating there is ample support to estimate preferences and extrapolate to non-survey respondents.

Another key concern is the potential possibility that the extrapolation procedure may generate spurious results given that the preference parameters are estimated from a selected sample. To address this issue, we employ a split sample procedure that “mimics” our extrapolation exercise, using only the sample of students for whom we experimentally identify preferences. This approach is useful as it allows us to diagnose the performance of our extrapolation by directly comparing extrapolated preferences to actual preference estimates for a subsample where we can observe both.

Our extrapolation test proceeds as follows. We create an estimation sample through stratified random sampling of one-third of the sample of choice respondents. Our stratification ensures the resulting estimation sample matches baseline characteristics of the average student in LAUSD as a whole. Using the estimation sample, we estimate preference parameters and

¹²Preference extrapolation is common in the literature. For example, estimation relying on stability (Fack et al., 2019) extrapolates preferences of individuals with larger feasible choice sets to those with smaller choice sets (Agarwal and Somaini (2020) Otero et al. (2021)).

¹³The index is the predicted math test score based on a model that includes student covariates such as URM status, sex, socioeconomic status, English-learner status, special education status, and lagged achievement in math and English language arts (ELA).

construct propensity scores. Next, we return to the original survey respondent sample and use the residual set of respondents who were not included in the estimation sample. In this residual sample, we use our covariate cell approach to create a second set of preference estimates that we extrapolate to the estimation sample. Our test is to compare the two propensity scores to assess extrapolation quality. Appendix Figure A.3 is a plot of the extrapolated propensity scores against the true propensity scores for the estimation sample. The associated slope is near 1 and the intercept is near 0, indicating the extrapolation is forecast unbiased.¹⁴

4.3 Empirical Specification

Our analysis focuses on the following empirical specification:

$$Y_i = \alpha_c + \gamma' X_i + \beta D_i + \theta \hat{P}(v_i) + \psi \hat{P}(v_i) \times D_i + e_i, \quad (4)$$

which is based on Equation 2 but includes covariate cell fixed effects α_c in addition to a vector of remaining baseline characteristic controls X_i . We report robust standard errors clustered at the school and covariate cell level to account for correlation within cells across schools induced by the preference estimation.

A key component of our analysis centers on β , the average causal effect of remote learning. To interpret estimates from Equation 4 as causal, identification relies on the idea that students who do and do not enroll in remote learning have similar unobservables after controlling for factors that drive selection into this learning mode using our propensity scores.

Our framework provides a test of identification based on assessing balance on baseline student characteristics. Specifically, we use measures of lagged academic achievement as dependent variables in specifications based on Equation 4. Panel (a) of Figure 3 reports estimates of the coefficient on a remote-learning indicator from these balance tests.

As a benchmark, our balance assessment begins with results on the left (black bars), which show that conditioning on a rich set of covariates commonly used in the value-added literature (Koedel and Rockoff, 2015) does *not* balance lagged ELA or math achievement. The differences are sizable and range between 18 and 22 percent of a standard deviation. In contrast, the results in the middle (gray bars) show that the propensity score strategy strongly eliminates differences in baseline achievement between students who do and do not enroll in remote learning. In addition to lagged achievement, tests for balance using the index discussed above are also strongly balanced using the propensity score approach. The ability to balance lagged achievement and the index is reassuring from a causal perspective (Rosembaum and Rubin, 1983).

Finally, Appendix Table A.3 tests for balance on additional baseline characteristics. Notably, the ability to achieve balance appears to be unique. In Appendix Table A.3 and Appendix Figure A.7, we also show results that rely on propensity scores estimated using an observational approach rather than our survey data. When using the observationally estimated propensity scores, the resulting remote coefficient estimates for baseline characteristics are large and statistically significant. This lack of balance using observational methods emphasizes the importance of the experimental preference estimates in our empirical strategy.

¹⁴Appendix Figure A.4 reports a histogram of the difference that shows the mean is 0.002, and the distribution is centered around 0 with standard deviation 0.06.

5 Main Results

We begin by examining the average effects of remote learning. Panel (b) of Figure 3 reports average effects for our primary outcome, 2021–2022 academic achievement. On the left, the canonical value-added estimates that condition on student attributes and lagged achievement show remote-learning negative effects ranging from 25 to 30 percent of a standard deviation, consistent with other studies employing alternative quasi-experimental methods that also find negative selection into remote learning (Bueno, 2020). In contrast to these large effects, our estimates based on Equation 4 are more moderately negative for the average student. ELA and math effects for the average student are -0.14σ and -0.17σ , respectively, using the propensity score approach. These results corroborate recent 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).

Next, we turn to our main analysis of the heterogeneous effects of remote learning. As motivated in our framework, the preference data allow us to assess how remote-learning selection patterns interact with treatment effect heterogeneity. Formally, we use Equation 4 to estimate effects across the distribution of remote-learning propensity scores for the sample of all LAUSD students. Panel (a) of Table 1 reports point estimates from our preferred specification, while Figure 4 summarizes these results by plotting the mean treatment effects (i.e., $\beta + \hat{\psi}\hat{p}$), calculated separately for 12 bins of the demeaned propensity score.

These results reveal negative selection on levels alongside evidence of positive selection on gains. Table 1 demonstrates that students with higher estimated propensity scores tend to perform worse than those with lower scores regardless of their instruction mode; that is, $\hat{\theta} < 0$. The upward slope shown in Figure 4 reflects that the interaction coefficient $\hat{\psi}$ is positive at around 0.056 and 0.078 for ELA and math, respectively.

How do we interpret these results? Figure 4 reveals important heterogeneity in remote effects with respect to preference intensity. For example, students with a one standard deviation increase in remote-learning demand have a 0.09σ larger remote-learning achievement effect for math. Moreover, the figure suggests that most students experience learning loss from remote instruction relative to in-person—consistent with prior evidence (Goldhaber et al., 2022, Jack et al., 2022). However, there is a small share who experience positive remote-learning effects. Taking the estimates literally suggests that students in the top decile of the estimated propensity score distribution do no worse than they would in person and some have positive treatment effects. These may be students for whom self-paced learning is more adequate (Armstrong-Mensah et al., 2020) or those who potentially benefit from reduced social pressure or bullying (Bacher-Hicks et al., 2022).

5.1 Robustness Checks

This section discusses three exercises that demonstrate the robustness of our results. First, our main estimates and inference do not account for estimation error introduced in the preference estimation stage. Appendix Table A.4 and Appendix Figures A.5 and A.6 show that accounting for estimation error in the propensity scores does not qualitatively affect our estimates or infer-

ence. Second, our preferred results are based on parameterizing preferences for travel time and academic quality linearly and assuming no interactions with preferences over remote learning. As robustness checks, we estimate alternative specifications that allow for non-linear travel costs and various interaction terms with the remote schooling indicator. Panels (b)–(d) of Table 1 reports estimates that are remarkably similar to our preferred estimates.

Third, despite the balance results in Section 4, one may remain concerned with the distributional assumptions necessary for our model to accurately map experimentally identified preferences to propensity scores. To provide further robustness to our findings, we use the experimentally identified preferences in a control function framework, in line with existing work leveraging revealed preferences to estimate causal effects (e.g., Abdulkadiroğlu et al., 2020). Specifically, we estimate

$$E[Y_i|X_i, D_i, v_i] = \alpha + X_i'\gamma + \beta D_i + \theta\lambda(X_i, X_{j(i)}) + \psi\lambda(X_i, X_{j(i)}) \times D_i. \quad (5)$$

where $\lambda(X_i, D_i, X_{j(i)}) = E[\varepsilon_i - \mu_\varepsilon|X_i, X_{j(i)}]$ is an estimate of the unobserved preference heterogeneity implied by our choice model (Abdulkadiroğlu et al., 2020, Dubin and McFadden, 1984) and μ_ε is Euler’s constant. While conceptually similar to our preferred model, Equation 5 imposes a linear relationship between potential outcomes and the unobserved preference heterogeneity implied by the choice model. Both models capture selection driven by preference heterogeneity. The estimates based on Equation 5 are similar to our preferred approach and are reported on the right (maroon bars) of Figure 3.

6 Conclusion

Demand for virtual and remote schooling options has grown substantially during the past decade. Appendix Figure A.8 shows that enrollment in exclusively virtual charter schools grew steadily and reached 714,000 total students in the year before the pandemic. After the pandemic peaked, this enrollment expanded to 1.1 million in 2022. As a benchmark, the 2022 virtual learning enrollment was only somewhat smaller than the estimated 1.7 million attendance at Catholic institutions.

The growth in the virtual schooling and experiences with remote learning during Covid has motivated an emerging literature on the consequences for students. Before the pandemic, a near consensus suggested that virtual schools negatively affect learning (Bueno, 2020, Fitzpatrick et al., 2020, Raymond et al., 2023). More recent studies on pandemic-era remote schooling similarly document learning losses (Goldhaber et al., 2022, Jack et al., 2022, Singh et al., 2022). Notwithstanding this evidence, school districts are currently planning to expand remote options to satisfy continued parental demand (Musaddiq et al., 2022).

This paper provides novel evidence on parental and student experiences and demand for remote-learning options in Los Angeles. Our evidence is based on a novel survey conducted after the peak of the COVID-19 pandemic. When responding to our survey, parents and students were uniquely able to draw on their past experience with remote learning and firsthand understanding of this mode of instruction.

Overall, we find that an important share of families want expanded remote offerings and

believe their children excel in remote instruction. Combining information on preferences with a model of achievement, we show that the subset of students with the highest demand for remote-learning experience achieve gains relative to in-person instruction. At the same time, our results corroborate growing evidence showing remote learning contributes to learning losses for the average student. The heterogeneity in achievement benefits suggests that continued and expanded remote schooling options can potentially improve match quality in the post-pandemic K-12 landscape. An outstanding question is why some students appear to benefit from remote learning while others do not. A deeper understanding of the mechanisms that may promote remote-learning gains remains an important avenue for future work.

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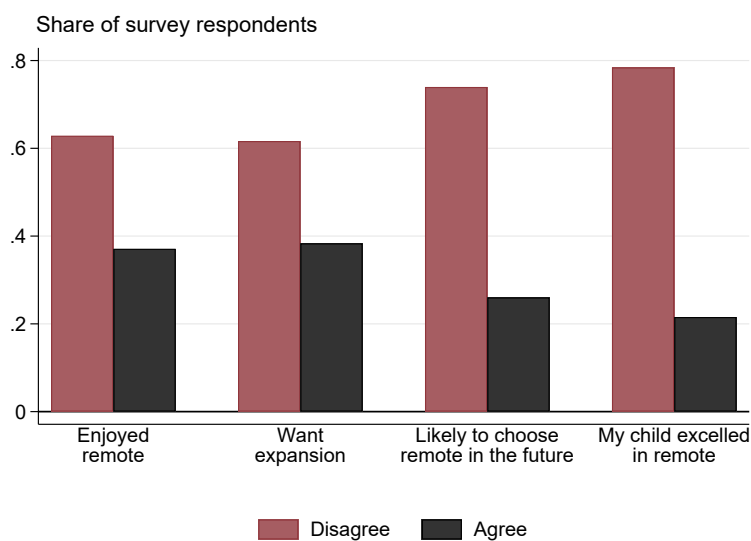
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7 Main Figures and Tables

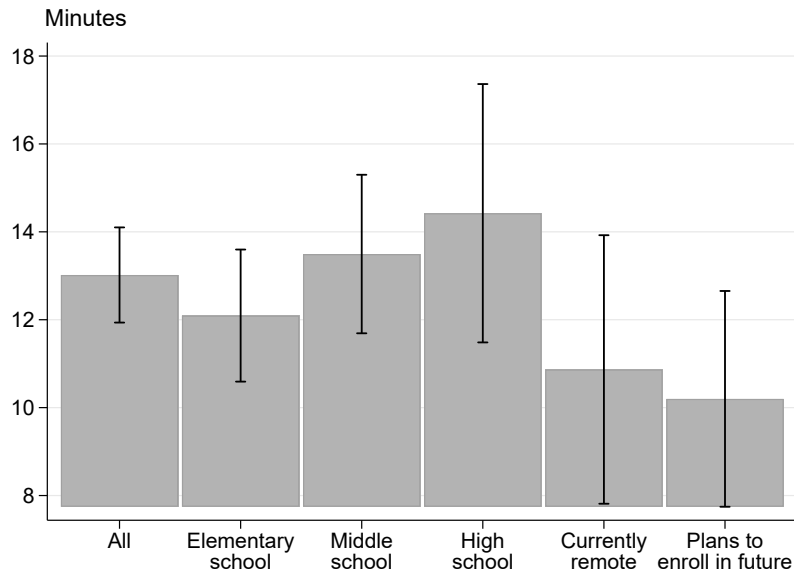
Figure 1: Experiences and Demand for Remote Learning



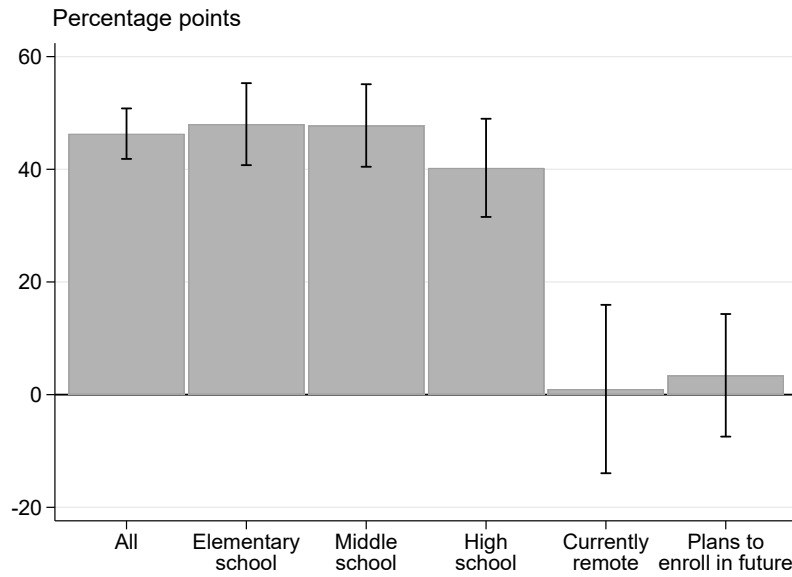
Notes: This figure reports survey results on the share of respondents ($N = 3,611$) who agree with four statements on their experiences and demand for remote learning. Individual responses are weighted to produce means that correspond to the average family in LAUSD. Specifically, we define the weight for each observation as $w_i \equiv P(\text{Survey} = 1)/p(\text{Survey} = 1|X_i)$, where $p(\text{Survey} = 1|X_i)$ is the estimated propensity to respond to the survey based on student characteristics X_i using the full sample of LAUSD students, and $P(\text{Survey} = 1)$ is the share of all LAUSD families with survey responses. Appendix Section B reports the complete text for the survey questions (see question 5).

Figure 2: Experimental Preference Estimates

(a) Minutes willing to travel for a 10 percentage point increase in achievement rate



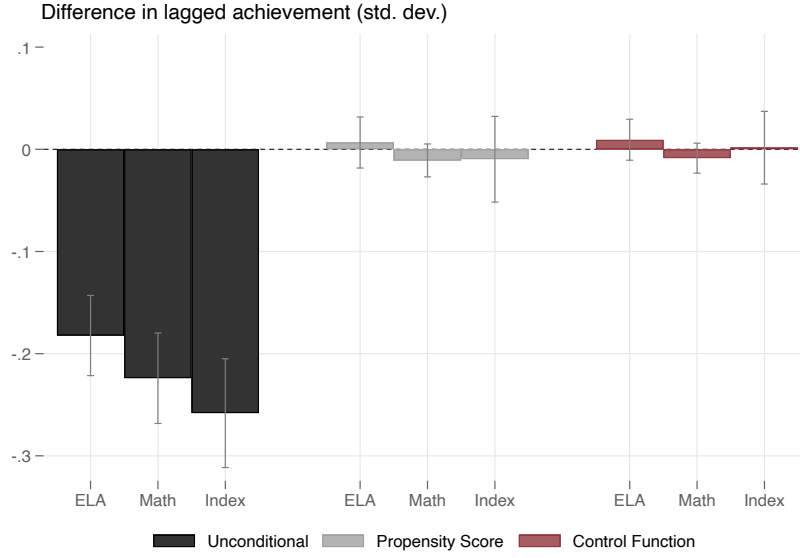
(b) Increase in achievement rate necessary to switch to remote



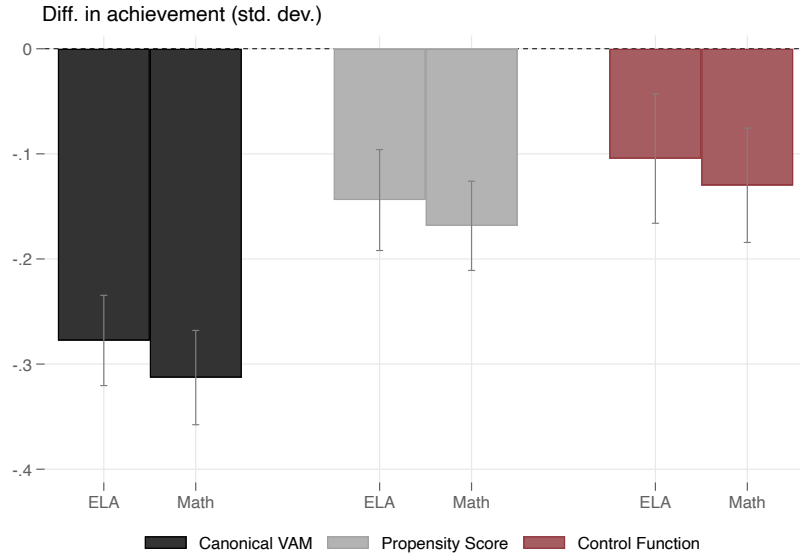
Notes: This figure reports willingness to travel estimates for achievement in Panel (a) and the estimated achievement necessary to make families indifferent between in-person and remote learning in Panel (b). Preference estimates are from a rank-ordered logit model relating indirect utilities of hypothetical choices to randomized 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 panels correspond to estimates for the complete sample with hypothetical choice responses. The next three bars estimate preferences separately for students in different grade levels. The “Currently 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 who 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 3: Baseline Balance and the Average Effects of Remote Learning

(a) Student baseline characteristics

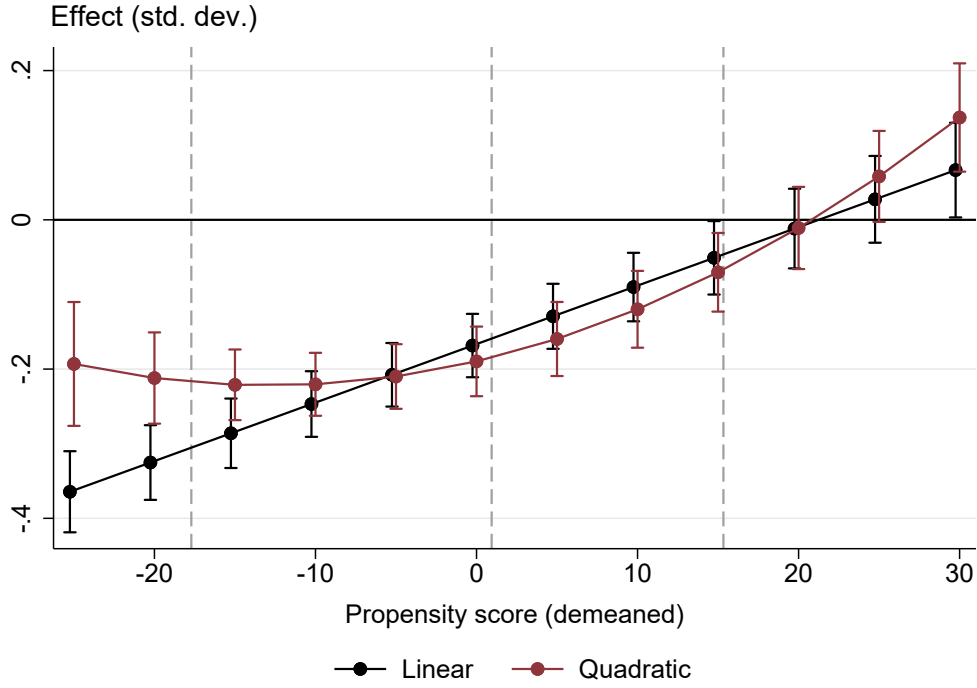


(b) Average effects on post-pandemic (2022) ELA and math achievement



Notes: This figure reports estimates of the average effect of remote learning. Panel (a) reports balance test results where the dependent variable is set to measures of lagged (2019) achievement scores in ELA and math as well as a summary index of baseline covariates. To construct the index, we regress 2022 math achievement on a vector of baseline covariates including URM status, sex, socioeconomic status, English learner status, and special education status as well as lagged (2019) achievement in ELA and math. The index is defined as the predicted values from this regression. The black bars on the left correspond to models where the independent variables are a remote indicator, grade-level indicators, and baseline student characteristics. The gray bars in the middle correspond to results from models based on Equation 4, which controls for estimated propensity scores. The maroon bars on the right correspond to results from models based on Equation 5, which include control function estimates. Panel (b) reports corresponding results where the dependent variable is set to a measure of post-pandemic (2022) achievement in ELA and math. Standard errors are robust and double clustered at the school and covariate cell level. Gray bars are estimates of the 95 percent confidence intervals.

Figure 4: Estimated Match Effects on Post-Pandemic Math Achievement



Notes: This figure reports estimates of treatment effects on post-pandemic (2022) math test scores for 12 bins of estimated propensity scores. The points in black are means for each bin based on Equation 4 and are constructed by summing the coefficient on the remote-learning indicator (representing the average effect) with the product of the estimated match effect and (demeaned) propensity score (i.e., $\theta + \psi * p$). The points in maroon are means for each bin constructed by summing the coefficient on the remote-learning indicator with non-linear (quadratic) match effects (i.e., $\theta_1 + \psi_1 * p + \psi_2 * p^2$). Note that the propensity score is demeaned so that the estimate at zero corresponds to the average treatment effect for the average student. The three dashed, gray vertical lines correspond to the 10th, 50th, and 90th percentiles of the propensity score distribution. Standard errors are robust and double clustered at the school and covariate cell level. Bars surrounding the mean estimate for each bin are estimates of the 95 percent confidence intervals.

Table 1: Effects of Remote Learning

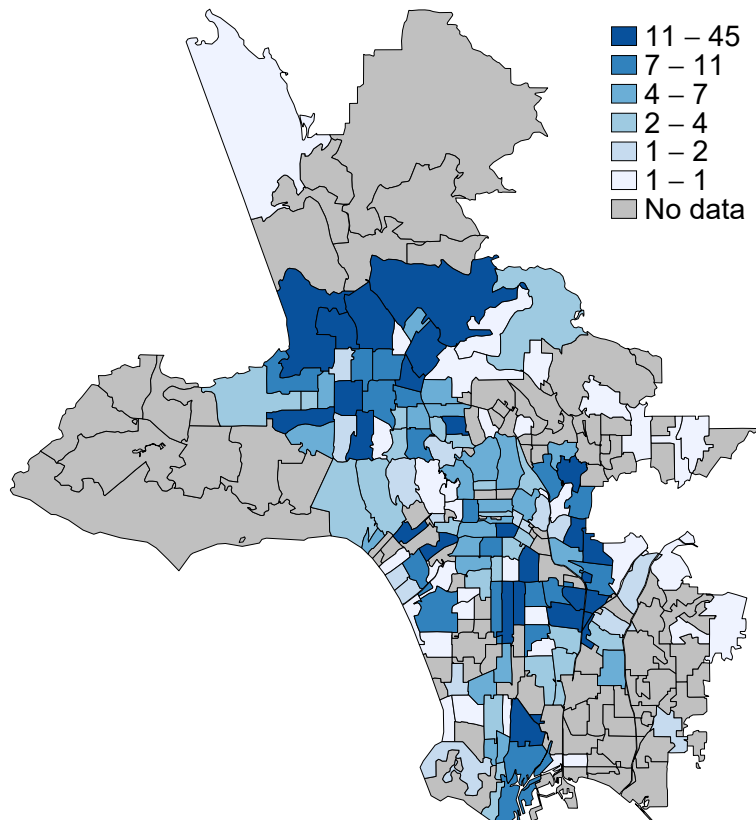
	(1)	(2)	(3)
	Main Effect (β)	Selection on Levels (θ)	Selection on Gains (ψ)
(a) Baseline			
ELA	-0.144 (0.024)	-0.176 (0.012)	0.056 (0.008)
Math	-0.168 (0.022)	-0.182 (0.013)	0.078 (0.008)
(b) Non-linear pref.			
ELA	-0.0138 (0.024)	-0.147 (0.01)	0.04 (0.007)
Math	-0.17 (0.021)	-0.151 (0.011)	0.067 (0.007)
(c) Non-linear dist.			
ELA	-0.154 (0.024)	-0.151 (0.011)	0.062 (0.008)
Math	-0.174 (0.022)	-0.157 (0.012)	0.073 (0.007)
(d) Non-linear pref. and dist.			
ELA	-0.136 (0.025)	-0.137 (0.01)	0.037 (0.009)
Math	-0.166 (0.022)	-0.141 (0.011)	0.057 (0.008)

Notes: This table reports estimates on the effects on ELA and math achievement based on versions of the model specified in Equation 4. Each panel reports estimates from models that differ in the underlying model of preferences used to construct propensity scores. Panel (a) provides results from our preferred (baseline) model with linear distance costs and preferences for academic quality, and Panel (b) provides results from a model that allows for non-linear (quadratic) preferences for academic quality. Panel (c) provides results from a model with non-linear (quadratic) distance costs, and Panel (d) provides results from a model that allows for both non-linear preferences for academic and distance costs. Columns 1, 2, and 3 report estimates of the main effect of remote learning (β), which represent the average effect, the selection on levels effect (θ), and the selection on gains coefficient (ψ), respectively. Propensity scores are in units equal to 10 percent for interpretation reasons. Standard errors are robust and clustered at the school level.

ONLINE APPENDIX

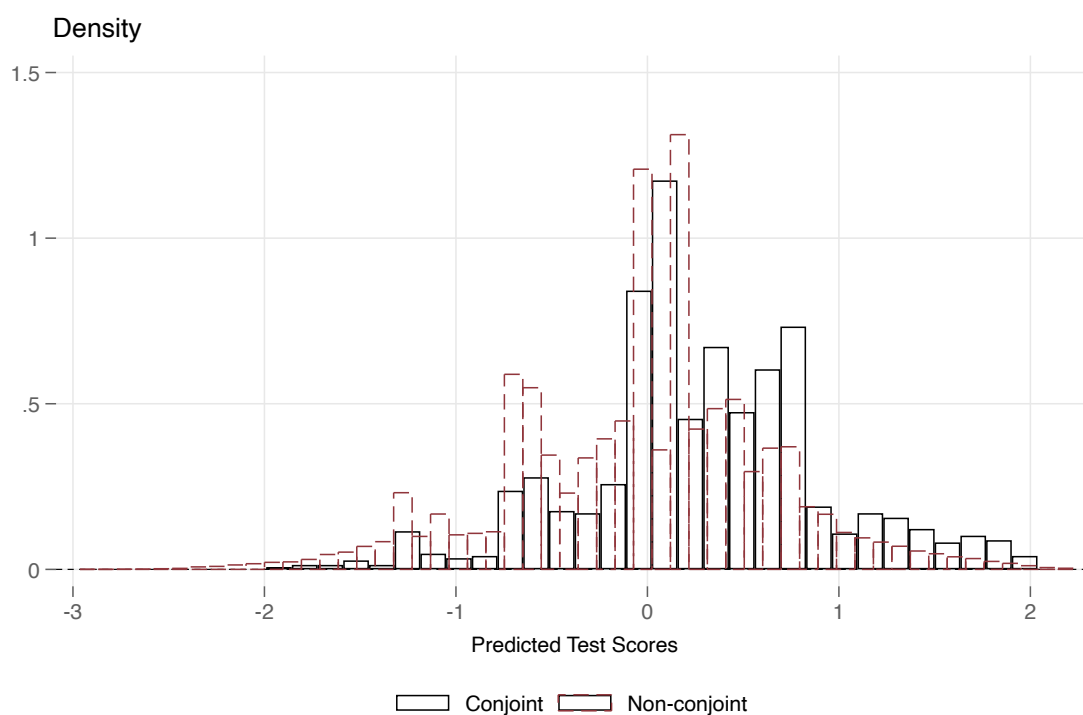
A Appendix Figures and Tables

Figure A.1: Spatial Distribution of Remote-Learning Survey Respondents



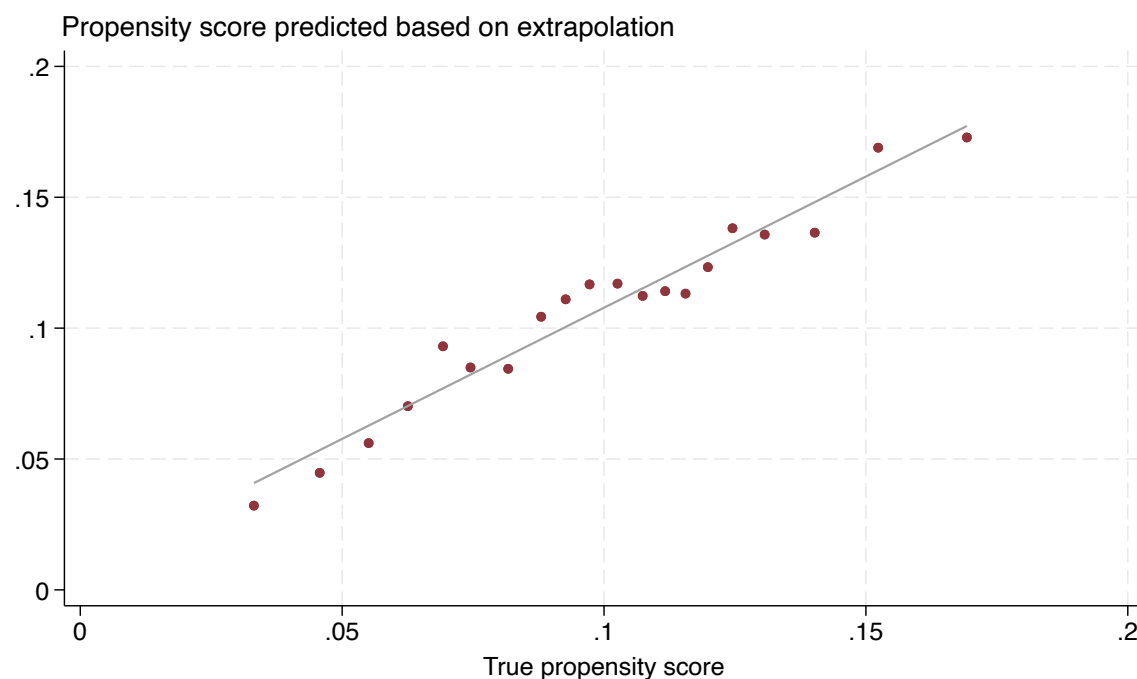
Notes: This figure is a map illustrating the spatial distribution of survey respondents. Each shaded polygon corresponds to a census tract and is shaded according to the number of remote-learning respondents residing in the census tract. Most of the gray areas in the figure are outside the purview of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

Figure A.2: Distributions of an Index of Baseline Characteristics



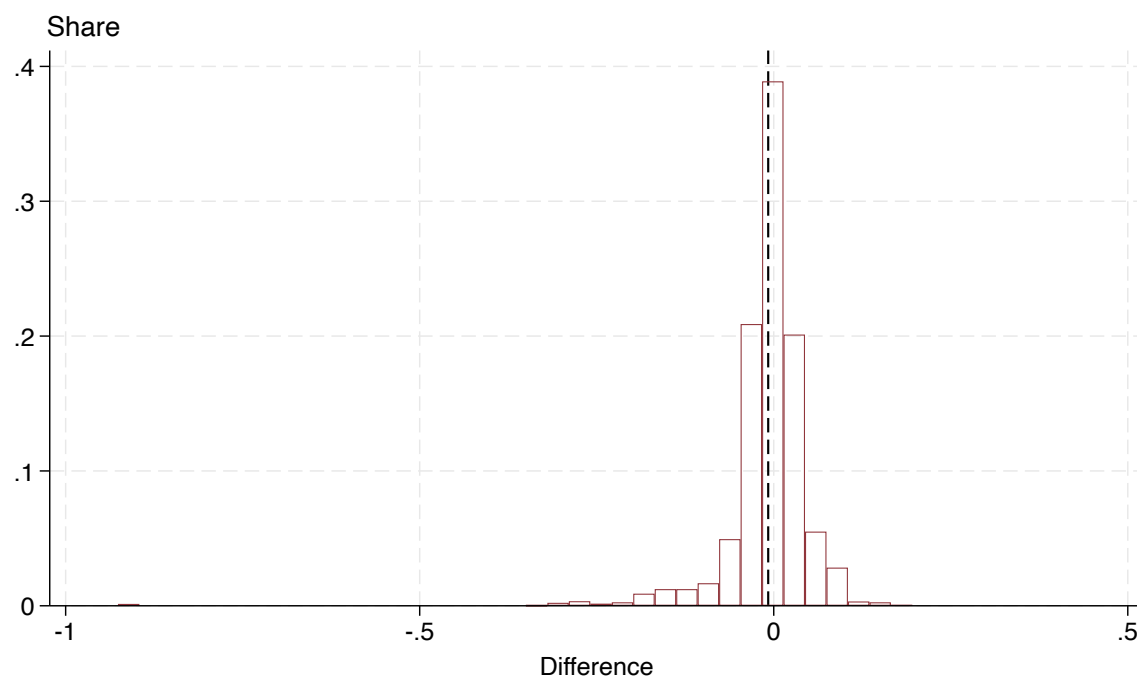
Notes: This figure reports the distribution of a summary index measure for the baseline covariates for students in the hypothetical choice and the general student samples. The summary index is constructed by regressing 2022 math test scores on an array of student characteristics including lagged (2019) achievement. The summary index corresponds to the predicted values from this regression. The histogram shows there is sufficient overlap between the hypothetical choice and the full LAUSD samples used in the empirical analysis.

Figure A.3: Correlation Between True Estimated Propensity and Extrapolated Propensity Scores



Notes: This figure compares two propensity scores that we construct to test the validity of our extrapolation approach. The two scores are estimated as follows. First, we create an estimation sample through stratified random sampling of one-third of the sample of hypothetical choice survey respondents. Our stratification ensures that the resulting estimation sample matches the average student's baseline characteristics. Using this estimation sample, we estimate preference parameters and construct propensity scores. Second, we return to the original survey hypothetical choice sample and use the residual set of respondents who were not included in the estimation sample. In this residual sample, we use our covariate cell approach to create a second set of preference estimates that we extrapolate to the estimation sample. The x -axis of the figure shows the "true" propensity scores that we estimate in the first step using the estimation sample. The y -axis of the figure shows the "predicted" propensity scores that we estimate for the estimation sample created by extrapolating the preference estimates from the residual sample.

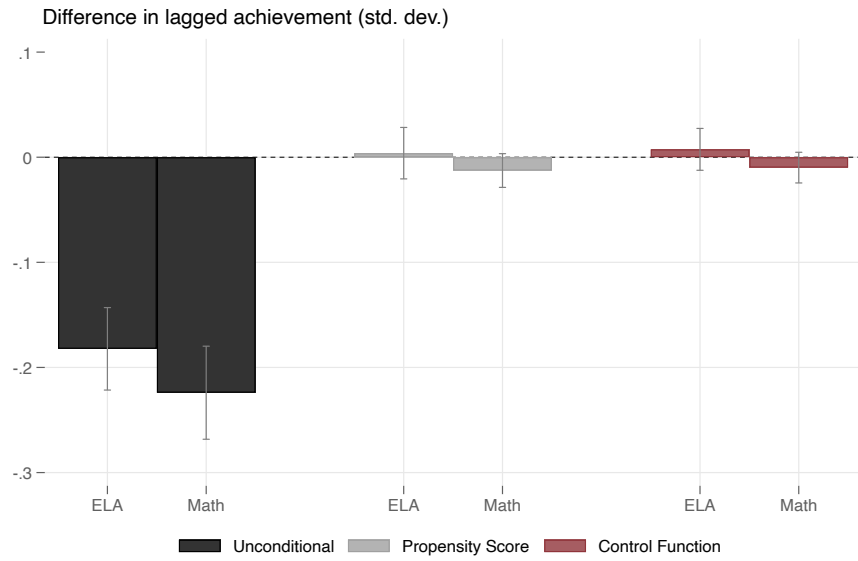
Figure A.4: Histogram of the Difference Between True Propensity and Extrapolated Propensity Scores



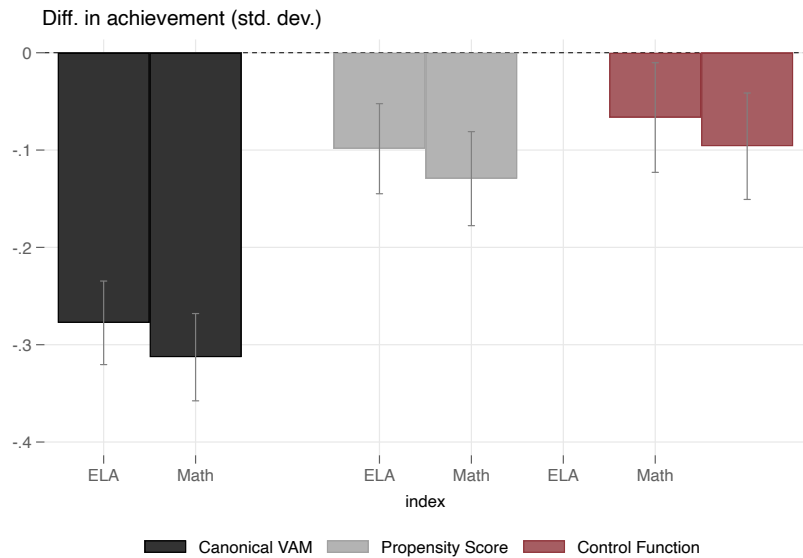
Notes: This figure reports a histogram of the difference between the extrapolated propensity score and the true propensity score for the estimation sample described in the notes to Appendix Figure [A.3](#).

Figure A.5: Baseline Balance and the Average Effects of Remote Learning (Bootstrap Version)

(a) Student baseline characteristics

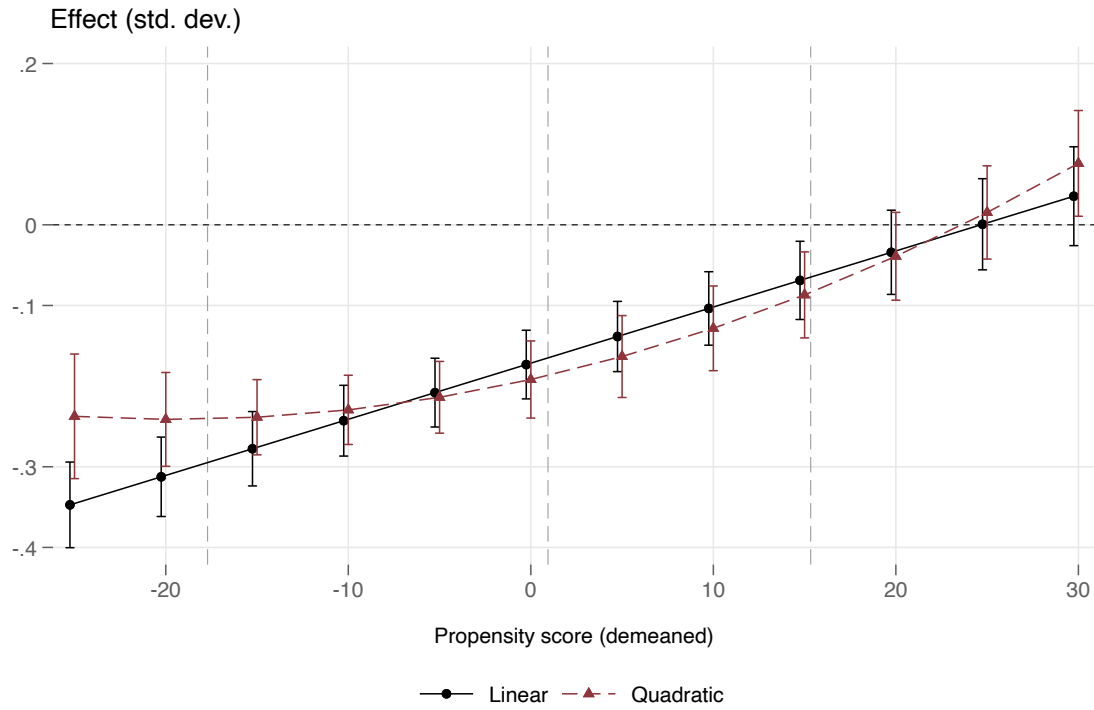


(b) Average effects on post-pandemic (2022) ELA and math achievement



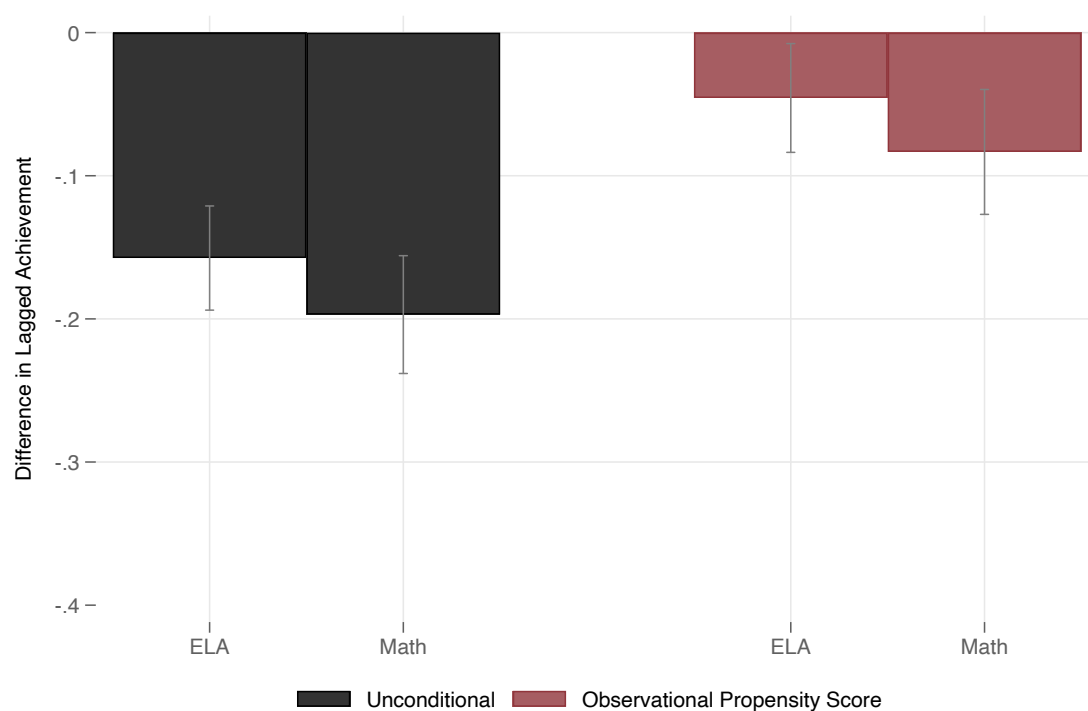
Notes: This figure reports estimates similar to those in Figure 3 but instead provides estimates and confidence intervals obtained through a bootstrapping procedure. To address estimation error in the propensity score estimation, we use a parametric bootstrap. We draw 250 sets utility weight estimates for each covariate from the joint normal distribution with the mean and variance-covariance matrix obtained in the initial estimation step. We then estimate the corresponding regressions 250 times. Finally, we report the mean parameter estimates and the 95 percent confidence region obtained in the bootstrapping procedure.

Figure A.6: Estimated Match Effects on Post-Pandemic Math Achievement (Bootstrap Version)



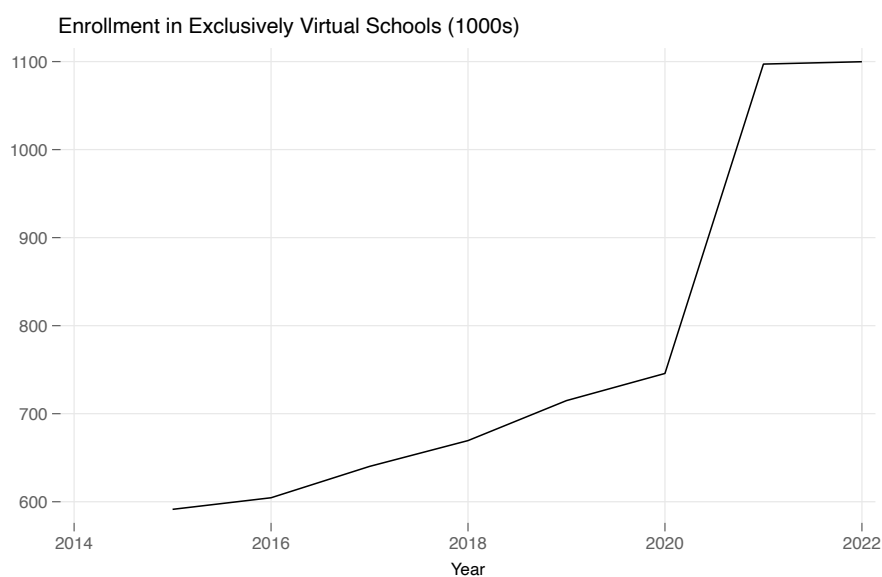
Notes: This figure reports estimates similar to those in Figure 4 but instead provides estimates and confidence intervals obtained through a bootstrapping procedure. To address estimation error in the propensity score estimation, we use the parametric bootstrap. We draw 250 sets utility weight estimates for each covariate from the joint normal distribution with the mean and variance-covariance matrix obtained in the initial estimation step. We then estimate the corresponding regressions and associated linear combination of the parameter estimates 250 times. Finally, we report the mean parameter estimates and the 95 percent confidence region obtained in the bootstrapping procedure.

Figure A.7: Balance Results Using Observational Logit Model



Notes: This figure reports the baseline balance of 2019 achievement (math and ELA) for both a conventional covariate-controlled and a propensity-controlled model derived from preferences estimated using observational data. The covariate-controlled model estimates correspond to regressions of 2019 achievement on remote indicators, baseline covariates, and grade indicators. The “Observational 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 coefficients correspond to average differences.

Figure A.8: Enrollment Trends for Exclusively Virtual Schools (NCES)



Notes: This figure presents statistics on annual enrollment in exclusively virtual schools from the Common Core of Data, provided by the National Center for Education Statistics (NCES).

Table A.1: Respondent Descriptive Statistics

	(1)	(2)	(3)
	All Students	Survey Respondents	Hypothetical Choice Respondents
Lagged ELA	0.01 (0.99)	0.17 (1.05)	0.45 (1.03)
Lagged Math	0.01 (0.99)	0.16 (1.01)	0.44 (1.00)
Female	0.49 (0.50)	0.51 (0.50)	0.49 (0.50)
Special Education	.13 (0.34)	.1 (0.3)	.07 (0.26)
URM	0.82 (0.38)	0.77 (0.42)	0.68 (0.47)
N	230,347	3,611	1,191

Notes: This table provides summary statistics for all LAUSD students and samples of survey respondents. Column 1 presents averages for every student in the relevant grades. We recruited a sample of survey respondents by randomly contacting 100,000 families through the LAUSD's internal communication system in April 2022. Column 2 reports averages for every family who completed at least one question on our survey. Column 3 reports averages for every student who completed the hypothetical choice experiment questions within the survey.

Table A.2: Summary Statistics for Preference Estimates

	(1) Mean	(2) SD	(3) P5	(4) P95
Academic Quality (ω_Q)	0.10	0.22	0.01	0.89
Remote (ω_R)	-5.06	13.03	-35.77	0.22
Travel Time (ω_d)	-0.07	0.17	-0.12	-0.01
$(-\omega_Q/\omega_d)$	1.53	1.20	0.48	3.81
(ω_r/ω_Q)	37.66	35.17	-6.91	74.23
Number of Cells	45			

Notes: This table reports summary statistics for preference parameters that were estimated separately for each covariate cell. Columns 1–4 report the mean, standard deviation, and the 5th percentile and 95th percentiles of the respective row variable, respectively. The last two rows report the willingness to travel for an extra percentage point in academic proficiency and the amount of compensation in achievement units necessary to make respondents choose the remote option. We omit two outlier observations in the statistics presented for the final row as they skew the mean and standard deviation.

Table A.3: Comparing Remote and In-Person Students

	(1)	(2)	(3)	(4)	(5)
	Remote in 2022	In Person in 2022	Mean Diff.	Mean Diff. Obs. <i>p</i> -score	Mean Diff. Exp. <i>p</i> -score
Lagged (2019) ELA Scores	-0.141	0.001	-0.142 (0.016)	-0.217 (0.017)	-0.078 (0.075)
Lagged (2019) Math Scores	-0.185	0.002	-0.187 (0.019)	-0.254 (0.02)	-0.106 (0.093)
Female	0.508	0.483	0.025 (0.017)	-0.039 (0.015)	0.001 (0.049)
Special Education	0.151	0.141	0.01 (0.016)	-0.012 (0.007)	0.009 (0.021)
URM	0.843	0.824	0.02 (0.011)	0.071 (0.012)	0.01 (0.018)
# Students	12,902	257,877			

Notes: This table reports an analysis of baseline (pre-pandemic) student characteristics for students who selected remote and in-person learning options in the 2021–2022 academic year. Columns 1 and 2 report averages for remote and in-person students, respectively. Column 3 reports the corresponding difference in average characteristics based on Columns 1 and 2. Column 4 reports mean differences based on a regression that controls for an estimated propensity score based on an observational model. Specifically, the observational propensity score is estimated in a logit model predicting remote enrollment based on remote relative achievement, remote relative travel time, and baseline student characteristics. Column 5 reports mean differences based on a regression that controls for an estimated propensity score based on the experimental survey data. The difference in the estimates in Column 5 with those reported in Figure 3a is that estimates in Figure 3a further condition on cell strata. Column 5 demonstrates that conditioning on the propensity score alone is sufficient to eliminate baseline differences in achievement. Standard errors are reported in parentheses.

Table A.4: Effects of Remote Learning (Bootstrap Version)

	(1)	(2)	(3)
	Main Effect (β)	Selection on Levels (θ)	Selection on Gains (ψ)
(a) Baseline			
ELA	-0.097 (0.024)	-0.236 (0.015)	0.101 (0.01)
Math	-0.128 (0.025)	-0.243 (0.016)	0.119 (0.009)
(b) Non-linear pref.			
ELA	-0.142 (0.024)	-0.142 (0.01)	0.038 (0.006)
Math	-0.163 (0.023)	-0.148 (0.01)	0.049 (0.006)
(c) Non-linear dist.			
ELA	-0.159 (0.025)	-0.156 (0.011)	0.052 (0.006)
Math	-0.177 (0.024)	-0.161 (0.012)	0.058 (0.006)
(d) Non-linear pref. and dist.			
ELA	-0.152 (0.025)	-0.131 (0.009)	0.041 (0.006)
Math	-0.17 (0.024)	-0.137 (0.01)	0.047 (0.006)

Notes: This table reports estimates similar to those in Table 1 but instead provides estimates and standard errors obtained through a bootstrapping procedure. To account for estimation error in the propensity score estimation, we use a parametric bootstrap. We draw 250 sets utility weight estimates for each covariate from the joint normal distribution with the mean and variance-covariance matrix obtained in the initial estimation step. We then estimate the corresponding regressions and associated linear combination of the parameter estimates 250 times. Last, we report the mean parameter estimates and the standard errors (in parentheses) obtained in the bootstrapping procedure.

B Remote-Learning Survey Instrument

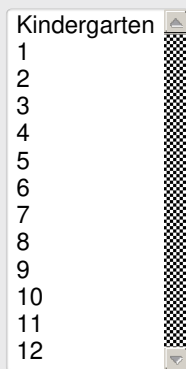
LAUSD Remote Learning Survey

(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? *



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.	<input type="radio"/>	<input type="radio"/>
I would like the district to expand its virtual offerings in the future.	<input type="radio"/>	<input type="radio"/>
I am likely to opt for virtual schooling in the future.	<input type="radio"/>	<input type="radio"/>
I enjoyed the virtual schooling experience during the pandemic.	<input type="radio"/>	<input type="radio"/>

(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	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
Worst	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>

(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.	<input type="radio"/>	<input type="radio"/>
I excelled academically with the virtual experience compared to in-person instruction.	<input type="radio"/>	<input type="radio"/>
I would like the district to expand its virtual offerings in the future.	<input type="radio"/>	<input type="radio"/>

(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	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
Worst	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>

(untitled)

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

☐ Yes

☐ No

C Survey Responses and Covid Experience Heterogeneity

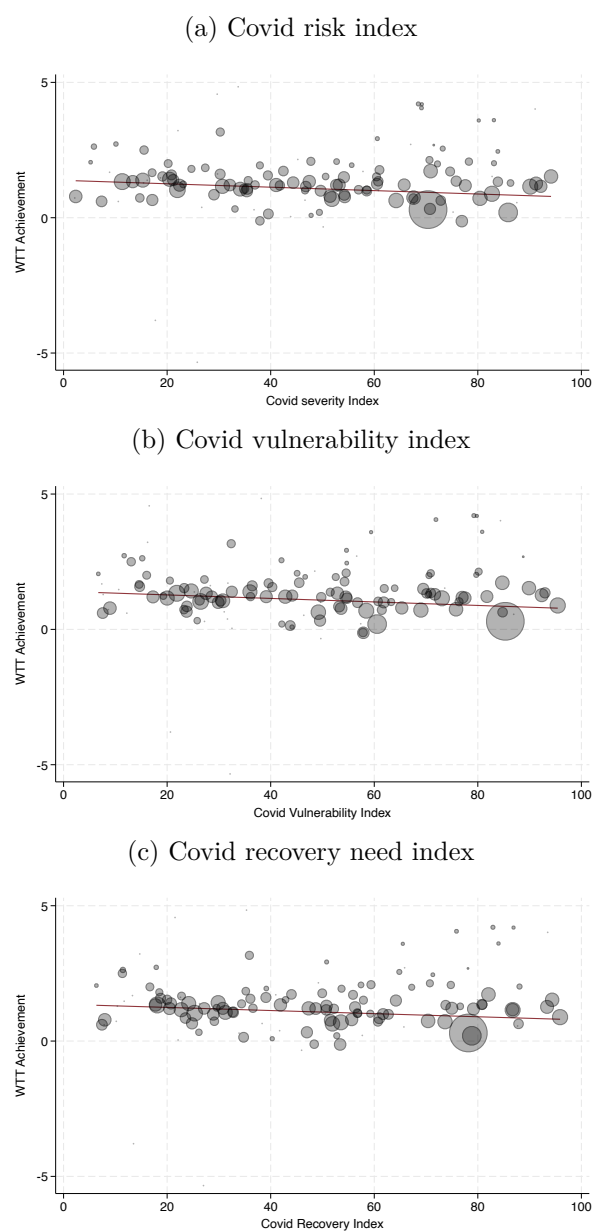
Although we asked survey respondents to remove the influence of Covid-related concerns from their stated choices, our preference estimates could still partly reflect residual COVID-19-related concerns. To assess this possibility, we generated new preference estimates by splitting the sample of choice survey respondents at the zip code level and generating geographic-specific estimates of willingness to pay measures. We correlate these zip-code-level preference estimates with measures from the COVID-19 Vulnerability and Recovery Index produced by Los Angeles County. For each area, the three index measures are intended to measure the risk, severity, and recovery need due to COVID-19.¹⁵ In addition, we correlate the zip-code-level preferences with measures of local area case counts and deaths due to COVID-19.¹⁶

Appendix Figure C.1, Panels (a), (b), and (c) provide scatterplots of each zip code’s estimated willingness to travel for academic quality and the three COVID-19 index measures. Each point’s size is proportional to the number of respondents used to estimate preference parameters. To supplement these results, Panels (a) and (b) of Appendix Figure C.2 report similar plots for willingness to travel and measures of cases and deaths due to Covid. We report analogous results for estimated measures of preferences for remote schooling (i.e., the amount by which achievement would need to change to make a respondent indifferent between the remote and in-person options) in Appendix Figures C.3 and Figure C.4. Overall, there is little visual evidence of a systematic relationship between preference parameters and either the Covid-related index measures or health outcomes at the zip code level. This provides reassuring evidence against the possibility that Covid-related concerns influence respondent choices in our survey.

¹⁵These measures were defined as follows. The risk measure is based on American Community Survey data from the U.S. Census Bureau on the share of individuals without U.S. citizenship, the share of the population below 200 percent of the federal poverty line, the share of overcrowded housing units, and the share of essential workers. The severity index is based on asthma hospitalization rates, the share of the population below 200 percent of the federal poverty line, the share of seniors aged 75 and over in poverty, the share of the population who is uninsured, heart disease hospitalization rates, and diabetes hospitalization rates. The recovery need index is based on the share of single-parent households, gun injury rates, the share of the population below 200 percent of the federal poverty line, the share of essential workers, the unemployment rate, and the share of the population who is uninsured. The data used for these analyses were downloaded from <https://geohub.lacity.org/datasets/lacounty::covid-19-vulnerability-and-recovery-index/about>.

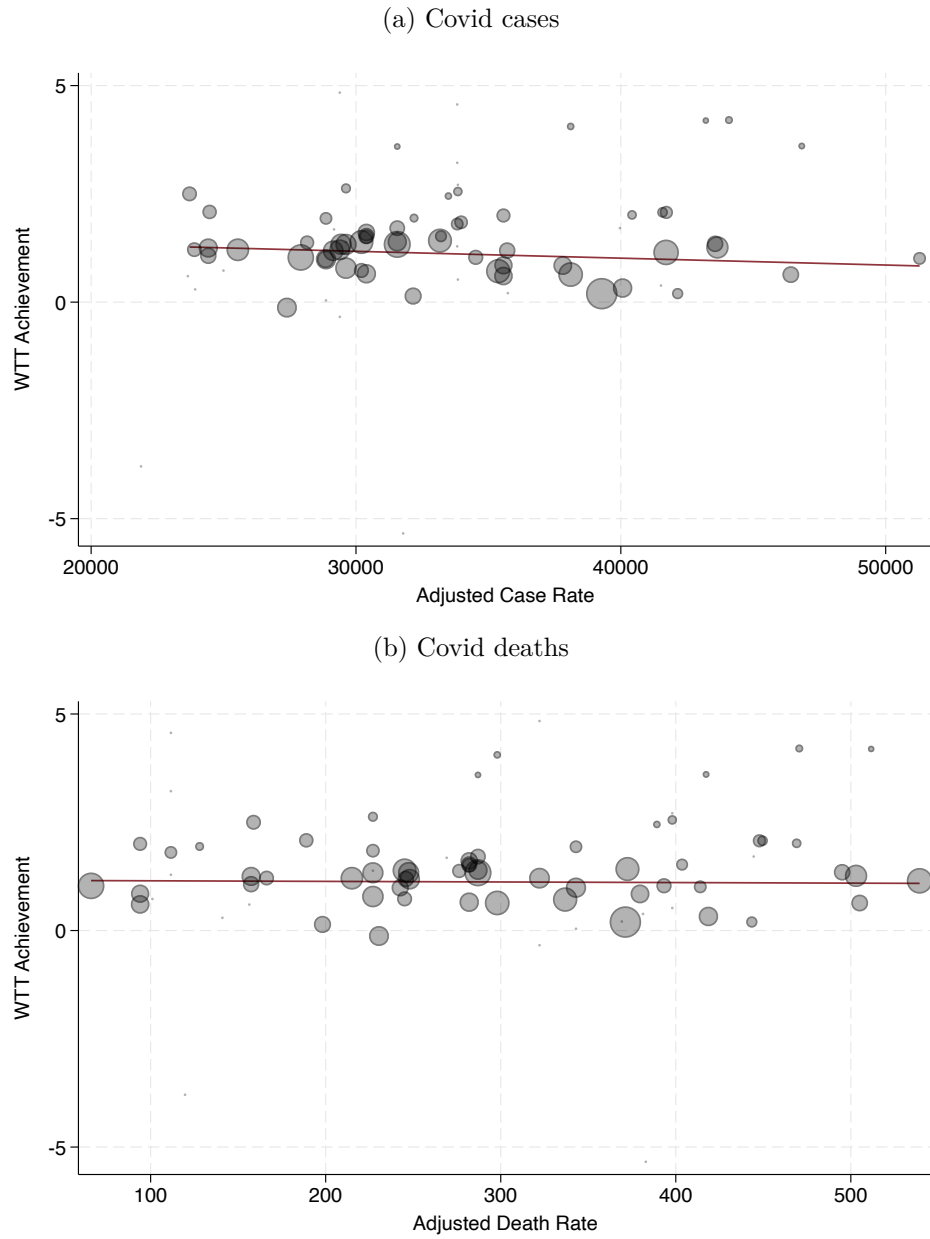
¹⁶The data used for these analyses were downloaded from <http://publichealth.lacounty.gov/media/coronavirus/data>.

Figure C.1: Preferences for Academic Quality and Covid Index Measures for Risk, Severity, and Recovery Need



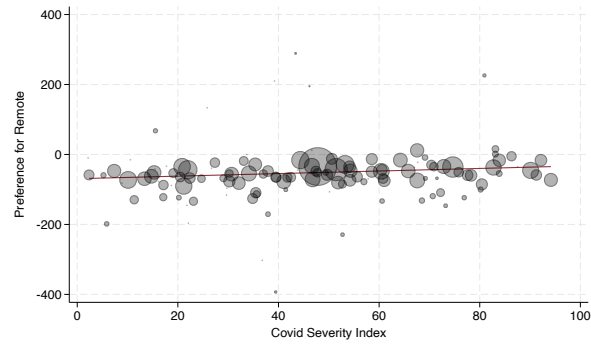
Notes: This figure presents scatterplots of zip-code-level mean willingness to travel for academic achievement (y -axis) and three measures from the COVID-19 Vulnerability and Recovery Index produced by Los Angeles County (x -axis). Panels (a), (b), and (c) present indices for the risk, severity, and recovery need due to COVID-19, respectively. Each point's size is proportional to the number of respondents used to estimate preference parameters.

Figure C.2: Preferences for Academic Quality and Covid-Related Health Outcomes



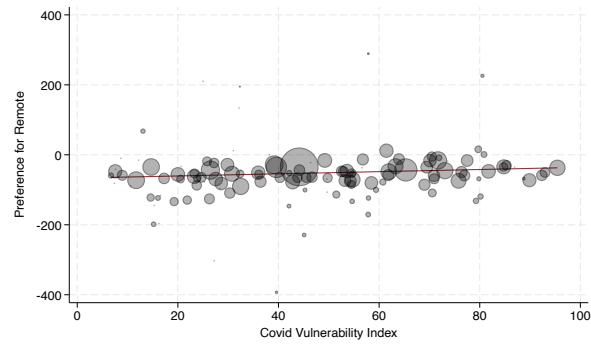
Notes: This figure presents scatterplots of zip-code-level mean willingness to travel for academic achievement (y -axis) and two measures of the severity of the COVID-19 pandemic on health outcomes in an area (x -axis). Panels (a) and (b) measure Covid health impact severity using case count and death measures, respectively. Each point's size is proportional to the number of respondents used to estimate preference parameters.

Figure C.3: Preferences for Remote Learning and Covid Index Measures for Risk, Severity, and Recovery Need

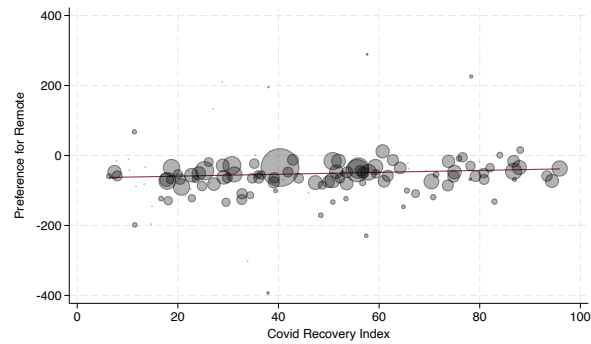


(a) Covid risk index

(b) Covid vulnerability index

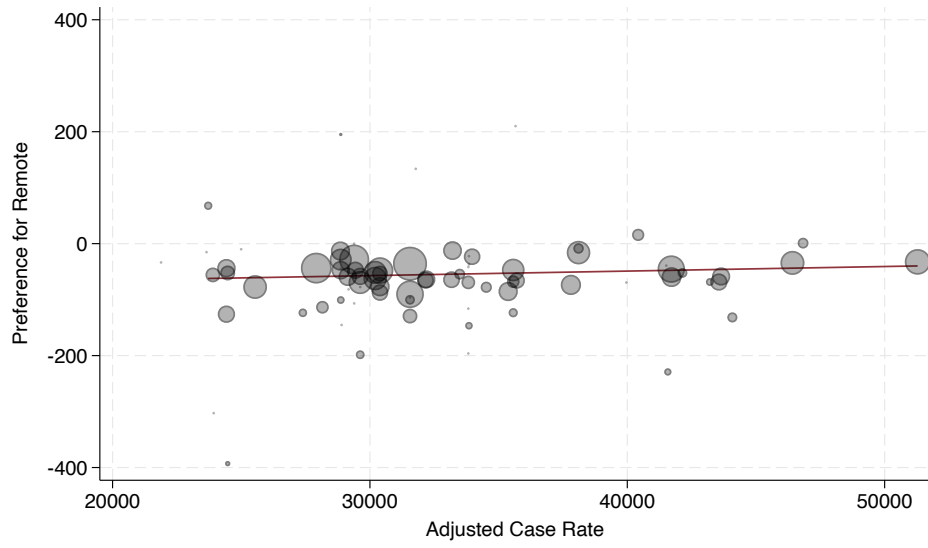


(c) Covid recovery need index

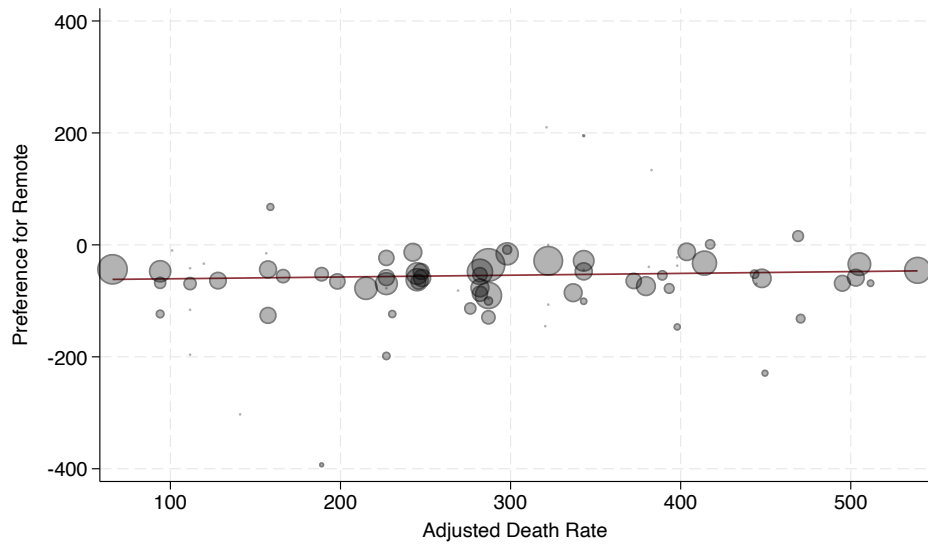


Notes: This figure presents scatterplots of zip-code-level measures of mean preferences for remote learning (y -axis) and three measures from the COVID-19 Vulnerability and Recovery Index produced by Los Angeles County (x -axis). Panels (a), (b), and (c) present indices for the risk, severity, and recovery need due to COVID-19, respectively. Preferences for remote learning are measured as the change in achievement needed to make a family indifferent between the remote and in-person schooling options. Each point's size is proportional to the number of respondents used to estimate preference parameters.

Figure C.4: Preferences for Remote and Covid-Related Health Outcomes



(a) Covid cases



(b) Covid deaths

Notes: This figure presents scatterplots of zip-code-level measures of mean preferences for remote learning (y -axis) and two measures of the severity of the COVID-19 pandemic on health outcomes in an area (x -axis). Preferences for remote learning are measured as the change in achievement needed to make a family indifferent between the remote and in-person schooling options. Panels (a) and (b) measure Covid health impact severity using case count and death measures, respectively. Each point's size is proportional to the number of respondents used to estimate preference parameters.