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

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Abstract

We study the distributional effects of remote learning using a novel approach combining preference data from a conjoint survey with administrative records. Experimentally derived preferences account for selection into remote learning and treatment effect heterogeneity. We validate the approach using random variation from school choice lotteries. On average, remote learning reduced reading and math achievement, but children whose parents showed strongest demand experienced positive effects. Parental concerns about bullying strongly predict demand, and remote learning consistently reduced bullying, partly offsetting learning losses. These results suggest that students who sort into post-pandemic remote learning may benefit from its expansion.

JEL Classification: I21, I24.
Keywords: Remote learning, COVID-19, school match effects, self-selection, school choice, virtual schooling

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1 Introduction

Classic theories of education markets predict that school choice can improve how students are allocated 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). Even substantial changes to the choice environment can fail to produce meaningful improvements in student-school match quality (Campos and Kearns, 2024). Imperfect information is a leading hypothesis for explaining 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 a unique setting to study allocative efficiency after families have more fully assessed 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 nationwide are planning to offer permanent, expanded remote options to satisfy ongoing parental demand (Musaddiq et al., 2022). Recent data from the National Center for Education Statistics (NCES) show that enrollment in exclusively virtual schools has increased by roughly 75 percent nationwide relative to enrollment just prior to the onset of the pandemic—accelerating growth by more than a decade ahead of pre-pandemic trends. Moreover, all of the 40 largest districts in the country currently offer a remote option or school.[1]

This paper studies the demand for remote learning in the post-pandemic environment and provides new evidence on which students are best suited for this schooling option. We focus on the second-largest school district in the United States, the Los Angeles Unified School District (LAUSD). At the onset of the pandemic, every student in the district had to participate in virtual learning and experienced a cycle of in-person and remote experiences in the following year. This unusual experience allowed families to assess their relative suitability for remote learning over an extended period and across a large spectrum of K-12 ages.[2]

It is worth highlighting that the sustained demand for remote learning among LAUSD students provides direct evidence on the continued importance of this mode of instruction in our setting. While the district returned in 2022 to in-person learning as the dominant mode of instruction, approximately 14,000 students chose to continue remote schooling. Why did so many families prefer the remote option? Evidence on this question is scarce. Bacher-Hicks et al. (2022) use nationwide data and find decreases in bullying during the remote era, implying demand for safety may play a role. In the context of higher education, 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

[1]Many school districts offer remote options for medically fragile students. Excluding these school districts, 31 of the 40 largest school districts offer a remote option or school.

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

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 while holding remaining school attributes fixed.[3]

We begin with a descriptive analysis that sheds light on family experiences and the 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.

The hypothetical choice data in the survey 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 tastes for academic quality and distaste for distance (Abdulkadiroğlu et al., 2020, Ainsworth et al., 2023, Allende, 2019, Beuermann et al., 2022, Burgess et al., 2015, Campos and Kearns, 2024, Neilson, 2021). 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. This suggests that the survey data accurately captures the underlying preferences that drive parental decision making.[4] Interestingly, baseline bullying outcomes also strongly predict increased demand for remote learning, suggesting that heavily bullied students require less compensation to switch to remote schooling—though this finding is imprecise.[5] Taken together, our survey analysis provides the first rigorous evidence of the diversity of preferences for remote learning in the post-pandemic landscape.

Next, we study the effects of remote learning on student outcomes using an approach where preferences identified via the choice experiments serve two empirical goals. First, we account for selection using a matching-style framework that relies on the preference results to estimate remote learning propensity scores. Concretely, this strategy relies on the notion that preference heterogeneity identified by the choice experiments drives selection into treatment. Under this assumption, we can draw from the existing literature on selection-on-observable approaches to recover unbiased estimates of the causal effect of remote learning on student outcomes (e.g., Dale and Krueger, 2002, Einav et al., 2022, Kline and Walters, 2016, Mountjoy and Hickman, 2020, Otero et al., 2021, Rosembaum and Rubin, 1983). An important testable implication of our approach is that the decision to enroll in the remote option should be orthogonal to baseline (pre-remote enrollment) characteristics among students with a similar propensity to enroll in the remote option. We verify this empirically: conditioning on the experimentally derived

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

[4]Empirically, we verify this by showing that the preferences uncovered by the survey imply choices that are forecast unbiased for the actual, real-world enrollment decisions subsequently made by parents.

[5]We measure bullying in a separate annual school experience survey linked to our conjoint data. See section 3 for more detail.

propensity score balances baseline measures of achievement, non-cognitive outcomes (i.e., grit and bullying), and a summary index of student characteristics. The data also show that the experimental nature of our approach is empirically important as propensity score estimates based on a purely observational approach do not allow us to achieve balance.

The second purpose of our survey-based preference estimates is to explore treatment effect heterogeneity. Specifically, the propensity scores based on experimental preference estimates provide an important measure of the demand for remote learning. Our main specification includes an interaction between the propensity score and remote enrollment to explore the nature of selection on levels versus gains in the spirit of Roy (1951). This specification allows us to test if families with stronger preferences for the remote option experience greater causal benefits.

For the average student, we find that remote learning has a large impact on both cognitive and non-cognitive outcomes. For example, remote-learning reduces reading and math scores by -0.13σ and -0.14σ, respectively. These estimates differ substantively from the results from simple models: regression adjustment with lagged achievement and standard covariates generates estimates ranging between -0.23σ and -0.26σ. This pattern of results further demonstrates that the experimentally derived preferences are necessary to account for otherwise unobserved sources of selection. For an index measure of bullying, we find that remote learning substantially improves outcomes: the average treatment effect is 0.17σ.

Our results provide the first comprehensive evidence regarding the causal impacts of remote learning on both cognitive and non-cognitive domains in the post-pandemic landscape. The results for mean impacts suggest that non-cognitive benefits may serve as a compensating differential for negative learning effects. This pattern provides new evidence to explain why parental demand for remote instruction remains high despite mounting evidence that it causes test scores to decline for the average student.

The evidence also shows that the mean impacts mask important heterogeneity that varies with a family's demand for remote learning. We find negative selection on achievement levels, indicating that students with high demand for remote learning perform poorly regardless of the school in which they enroll. In addition, there is positive selection on achievement gains, suggesting families choose remote learning, at least partly, using factors that correlate with their child's suitability for remote instruction. This has policy implications for understanding the efficiency of ongoing efforts to expand remote offerings. Taking our estimates 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 $0.04 - 0.07\sigma$.

The heterogeneity results also provide further evidence on the importance of compensating differentials. For bullying, we find negative selection on levels and modest selection on gains. The results point to an across-the-board improvement in bullying outcomes for students who select into remote learning. These improvements exist for both in-person and online bullying, revealing that schooling environment affects students' well-being both at school and at home.

In sum, the heterogeneity analysis underscores the tradeoff families face when choosing between remote and in-person modalities. Some families appear willing to forego short-run achievement gains in exchange for guaranteed improvements in bullying-related outcomes. Families with the largest tastes for remote learning experience improvements along both margins. These findings suggest 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. These findings also show that bullying-related considerations are an important, and previously unexplored, factor that governs family decision making.

We conclude our analysis by providing additional evidence that supports a causal interpretation of our results. The ability to balance baseline characteristics using our propensity-score based approach constitutes a key validity check on the credibility of our main results. As a supplement, we provide further validation by studying a sample of students who apply to over-subscribed school choice programs with remote schooling as a fallback option. Our test relies on lottery-based admission offers as an instrument to examine whether outcomes predicted from our propensity-score model of achievement accurately forecast observed variation in test scores. We fail to reject that the estimates derived from the choice experiment are forecast unbiased for the causal effects of remote learning implied by the lottery variation. This is reassuring evidence that the choice experiments adequately characterize selection into learning modality. Additional exercises demonstrate the robustness of our empirical methods to alternative modeling choices.

The rest of this paper is organized as follows. Section 2 discusses our contributions to the literature. In Section 3, we provide evidence highlighting the policy relevance of remote learning, review institutional details, and describe our administrative data. Section 4 contains details on the survey data we collected along with a presentation of experimental evidence on the demand for remote learning in the post-pandemic landscape. Sections 5 and 6 discuss our empirical strategy and provide details of estimation, respectively. Section 7 presents the main results and Section 8 discusses several additional robustness exercises.

2 Contribution and Related Literature

This paper contributes to four literatures. First, we contribute to a nascent but growing literature on the impact of remote or virtual learning by exploring selection and treatment effects in a post-pandemic environment. 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.

Second, we contribute to the literature on match effects in the education system by documenting evidence of selection on achievement gains with respect to remote offerings. 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, 2024). Other papers focus on match effects after accounting for preferences derived from observational choices and tend to find weak evidence of match quality (Abdulkadiroğlu et al., 2020, Mountjoy and Hickman, 2020). We complement this literature by using an experimentally derived, survey-based measure of preferences. This allows us to directly link parental demand to down-stream treatment effects and thereby asses the empirical relevance of match quality in this setting.

Third, we add to the literature on multi-dimensional learning by being the first to explore improvements in social-emotional well-being as a compensating differential (Beuermann et al., 2022, Campos and Kearns, 2024, Jackson, 2018, Jackson et al., 2020, Rose et al., 2022). Papers in the literature on multi-dimensional learning tend to find that schools and teachers impact both cognitive and non-cognitive domains. We find that learning modality, implicitly a schooling choice, affects achievement, bullying, and measures of student-level grit. Our holistic approach to evaluating the effects of remote learning demonstrates that students sort along these various dimensions in anticipation of gains, evidence consistent with multivariate Roy-style selection.

Fourth, this paper relates to the literature linking choice models to treatment effect estimation by exploring the potential for experimentally derived preferences to account for selection. Classic work in this space links choices to outcomes via an observed decision (Heckman, 1979, Heckman et al., 2006), while more recent advances leverage information on rank-ordered lists to account for selection bias (Abdulkadiroğlu et al., 2020, Einav et al., 2022, Otero et al., 2021). Our approach is similar but uses preferences derived from choice experiments, instead of observed choices or rank ordered lists, to characterize selection into program participation. This extends to 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 create an avenue for future work by bridging these two seemingly disconnected literatures and providing a validated empirical tool for general program evaluation.

3 Background and Data

The pandemic compelled all families to at least temporarily adopt remote schooling. Existing studies have shown that the disruption in learning modality has produced long-lasting changes to enrollment trends with increases in private and homeschooling (Bacher-Hicks et al., 2023, Musaddiq et al., 2022). A simultaneous and less-documented change in enrollment relates to remote schooling. In this section, we discuss national remote schooling trends, then zoom in on Los Angeles, and conclude with a discussion of the administrative data that we use.

3.1 Remote Schooling from a National Perspective

The pandemic allows us to separate remote schooling into three distinct periods. The first corresponds to the pre-pandemic years, which have been the subject of numerous studies. These studies focus on the effectiveness of virtual schools, with nearly all finding negative selection into remote schooling and evidence of negative causal effects (Bueno, 2020, Cordes, 2023, Erickson and Scriber, 2023, Kingsbury et al., 2022, Paul and Wolf, 2020). The second period corresponds to the pandemic years when families were compelled to adopt virtual schooling over varying durations. The research on this period emphasizes the learning losses induced by extended periods of remote instruction (Goldhaber et al., 2022, Jack et al., 2022). Although this period led to substantial learning loss with dire implications, it also compelled families to learn about their relative suitability for a learning modality they may not have otherwise tried. This forced learning may partly explain the sharp rise in virtual schooling in the post-pandemic landscape that we now turn to.

Remote enrollment was on the rise in the years leading into the pandemic, increasing by roughly 51 percent between 2015 and 2019. The pandemic accelerated this growth substantially. Figure 1 shows that the number of students enrolled in exclusively virtual schools has more than doubled relative to 2015, with a clear trend break in 2020. As a result, the most recent enrollment numbers are nearly 50% larger than what would be expected based on the pre-pandemic trend, effectively accounting for the majority of *all* growth in this sector over the relevant time period.

The national trends show a sizable disruption but also mask substantial heterogeneity. Appendix Figure B.1 demonstrates that ten percent of states have remote enrollment shares greater than 3.5 percent in 2023. Oklahoma, Idaho, and Oregon are states with the largest remote schooling shares, with enrollment shares of 5, 5, and 3.5 percent, respectively. This is slightly below these states' overall private school shares of 5, 6, and 7 percent, respectively (National Center for Education Statistics, 2024). The recent enrollment numbers are a product of sizable shifts in enrollment patterns (Bacher-Hicks et al., 2023, Musaddiq et al., 2022). States such as Nebraska, Tennessee, Florida, Alabama, and North Carolina stand out as states with at least a 150% increase in remote enrollment shares (see Appendix Figure B.2). These magnitudes are much larger than recent documented increases in homeschooling rates around the United States, which appear to be less correlated with changes in remote schooling trends (see Appendix Figure B.3 and Appendix Figure B.4).

In California, the setting of our study, the data show that 2.1 percent of students in the public sector enrolled in remote schooling in 2023. In fact, this high rate of enrollment places California as the state with the 9th largest virtual schooling share. Families in Los Angeles have expressed and continue to express persistent demand for remote learning above state-level averages.

3.2 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. To buffer the shock, the district took swift action by 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, their provision ebbed and flowed with each COVID wave. LAUSD schools remained closed until the week of April 19, when the district commenced a staggered reopening 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. However, all 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.[6] 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).[7] 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.[8] To accommodate a sizable share of families who continued to prefer remote learning, the district did not 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. Since then, the school district has introduced six new virtual learning academies that are permanently part of students' choice options.

3.3 Data

Our analysis uses two sources of administrative LAUSD data linked to survey data that we collect. The first source of administrative data is standard, containing student-level demographics, test scores, and residential addresses. Our analysis uses 2018–2019 test scores as baseline measures of lagged achievement and relies on 2021–2022 scores as outcomes.[9] The second source of administrative data comes from the School Experience Survey (SES) that LAUSD has administered to all students in the district since 2010. These data contain rich information on students' non-cognitive and socio-emotional outcomes related to bullying and standard measures such as

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

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

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

[9]The district did not administer standardized tests during the 2019-2020 pandemic year or the subsequent year.

grit (Jackson et al., 2020). We use these data to create index outcomes for our analysis where the definition of the non-cognitive outcomes follows Jackson et al. (2020) and Campos (2023).[10]

Table 1 provides summary statistics for in-person and remote students in 2022 in Columns 1 and 2. Remote students performed significantly worse on standardized exams in 2019, ranging between $0.24 - 0.32\sigma$ lower baseline test scores (see Column 3). Remote students also have worse socio-emotional outcomes, including school connectedness, grit, and bullying. Bacher-Hicks et al. (2022) argue that changes in bullying during the pandemic partly explain the mixed evidence surrounding the pandemic's effect on students' mental health and well-being. In our setting, it seems that students who were bullied at higher rates pre-pandemic are more likely to stay in remote schooling post-pandemic. Remote students are also more likely to be female, under-represented minorities, and more likely to have a special education status. They are less likely to be classified as English learners, while their low-income status is similar to that of in-person students in the district.

Our key data innovation is a survey we administered to a sample of parents with LAUSD students enrolled in grades 3–8 and grade 11 in April 2022. Appendix Section A.1 reproduces the survey instrument. 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 made 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 being instructed to hold all other attributes fixed. Section 4 provides further details on the preference measures, and Section 5 discusses how we use the estimated preferences as an input for our empirical strategy. A sample of 3,539 parents completed the basic descriptive survey questions, and 1,171 parents completed the hypothetical choice component. Respondents consented to have their responses linked to administrative records.

4 Survey Evidence

4.1 Characteristics of Survey and Conjoint Respondents

Columns 4 and 5 of Table 1 report average student characteristics of all survey and conjoint respondents, respectively. Survey respondents noticeably differ from the typical student in LAUSD in several important dimensions.[11] Families who initiated the survey have students performing above district averages, roughly 17–19 percent of a standard deviation. Notably, the

[10]Our analysis focuses on index measures of bullying and grit. Using the Student Experience Survey data, the bullying and grit indices are based on 8 and 13 questions, respectively. We standardize each question associated with the respective indices and sum the normalized values. Higher values of the indices indicate that students are bullied less or have more grit. See Campos (2023) for additional details related to the index creation.

[11]These 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.

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, special education, or English Learner students.

4.1.1 External Validity and the Conjoint Sample

The discrepancy between our survey sample and the average LAUSD student documented in Table 1 suggests that the interpretation of any analysis based on our survey may lack a broad claim to external validity. For example, it could be that the students whose families were more likely to complete the survey were also the types of students who benefit from remote instruction. In that case, using empirical findings from the highly selected conjoint sample to make general claims about the nature of remote learning could be misleading.

We address this key issue in three ways. First, as we will discuss in Section 6, our preferred econometric model uses covariates to extrapolate preferences from the conjoint sample to a more representative sample of LAUSD students. This kind of extrapolation is common across a large body of work in the school choice literature (e.g., Fack et al., 2019, Otero et al., 2021). Thus, our preferred causal analysis is based on a much broader, more representative sample. Second, we show in Section 8.3 that the estimates from our preferred model accurately predict the real-world remote learning take-up behavior of the students not contained in our conjoint sample. This suggests that the estimates from our preferred model are externally valid predictors for treatment take-up. Third, Section 7.4 provides results based on school choice lotteries which verify that the predicted causal effects of remote learning from our preferred model are forecast unbiased for lottery-based causal effects on the larger, more representative sample of students who were not contained in the conjoint sample. This suggests that the estimates from our preferred model are also externally valid predictors of treatment effects on the non-conjoint sample. Overall, while the summary statistics discussed earlier suggest that external validity is a potential issue, the data provide strong evidence that this issue is not severe enough to undermine the key conclusions of our paper.

4.2 Descriptive Evidence

Our descriptive analysis focuses on responses to four statements on experiences and future demand for remote learning. Figure 2 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 suggests 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. Among the survey respondents whose children are currently in remote learning, many cited academic factors as their reasons for selecting this modality (see Appendix Figure A.2). These findings highlight the possibility that the remote learning experience may have improved families' knowledge of their match quality.

4.3 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 treat the schooling options in each hypothetical as 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 E 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 (idiosyncratic) 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, 2024, Hastings et al., 2005, Neilson, 2021), we let the observable component of indirect utility be given by:

$$V_{ij} = \omega_Q Q_j + \omega_R Remote_j + \omega_d d_{ij}, \tag{1}$$

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). A logit distributional assumption on ε_{ij} allows us to estimate the preference parameters using an exploded logit framework (Hastings et al., 2005).

Figure 3a reports estimated mean willingness to travel estimates inferred from the choice experiments (i.e., -ω_Q/ω_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. Although Equation 1 defines a common set of preference parameters, we also explore heterogeneity by reporting preferences after re-estimating the same model separately for different subsamples (e.g., by grade level or remote status). 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.

Next, Figure 3b extends our analysis by showing the estimated achievement compensation needed to be indifferent between in-person and remote schooling (i.e., -ω_R/ω_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.

Appendix Figure A.3 and Appendix Figure A.4 shed further light on preference heterogeneity. Focusing on Appendix Figure A.3, Panel (a) demonstrates that families with lower-achieving students are willing to travel twice as long as families with higher-achieving students for higher-quality schools. We also find some evidence that Black families have stronger preferences for academic quality. This mirrors findings in Jacob and Lefgren (2007) that lower-income families have stronger tastes for academic quality. Panel (b) demonstrates similar preference heterogeneity for remote learning modality. A family with a lower-achieving student or a Black family needs to be compensated with a 20 percentage point increase in academic proficiency to switch to remote, while the average family in the district needs a 42 percentage point compensation. White families require an approximately 60 percentage point compensation to switch. The preference heterogeneity motivates our parameterization of demand in subsequent sections.

Finally, Appendix Figure A.4 demonstrates that families with students whose baseline bullying outcomes are worse (i.e., in the bottom two quartiles of the index measure) have stronger tastes for remote learning. The compensation that they require to be indifferent between in-person and remote schooling is roughly two-thirds that of families with students in the top two quartiles of the baseline bullying distribution.

5 Conceptual Framework

Our goal is to estimate the heterogeneous impact of remote-learning on student outcomes, and study how these effects relate to selection patterns. The analytic framework is based on linking a discrete choice model of remote learning choices to a treatment effects model. We begin our discussion by specifying our model of treatment effects on student outcomes.

We index a population of students by i and use the binary indicator D_i to denote the "treatment" of enrollment in remote learning. Define potential outcomes as $Y_i(1)$ and $Y_i(0)$ associated with students enrolling in remote or in person schooling, respectively. The observed outcome is $Y_i = Y_i(0) + D_i(Y_i(1) - Y_i(0))$. To see the challenges associated with identifying remote-learning effects, project observed outcomes Y_i onto a vector of observable characteristics, X_i, and the remote indicator using the following specification:

$$Y_i = \alpha + X_i'\gamma + \beta D_i + e_i, \tag{2}$$

where e_i is an error term that captures family inputs and other unobserved determinants of achievement. A key concern is that observational estimates of β may be biased because remote-learning participation is correlated with unobservable factors (i.e., $E[e_i|D_i] \neq 0$). 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 following from the logic of Rosembaum and Rubin (1983). Formally, our approach is based on the selection model represented in Equation 1 and maps to our treatment effects analysis by assuming there are only two schooling options, in-person (i.e., $j = 0$ and $Remote_0 = 0$) or remote schooling ($j = 1$ and $Remote_1 = 1$). This implies that the indirect utility of remote learning relative to in-person schooling can be compactly represented as:

$$u_i = v_i + \varepsilon_i, \tag{3}$$

where $u_i = U_{i1} - U_{i0}$, $v_i = V_{i1} - V_{i0}$, and $\varepsilon_i = \varepsilon_{i1} - \varepsilon_{i0}$. With this framework, we can state our first key assumption:

Assumption 1. *Given that selection into treatment is governed by v_i, we assume that*

$$Y_i(1), Y_i(0) \perp D_i \mid v_i.$$

Assumption 1 states that, once we know v_i, information about what learning modality the student selects does not provide any additional information about their potential outcomes. This assumption allows for the treatment effect $Y_i(1) - Y_i(0)$ to depend on v_i. In general, estimation of v_i imposes challenges that are hard to overcome in practice.[12] As noted in Section 4.3, we follow the school choice literature (e.g., Agarwal and Somaini, 2020, Allende, 2019, Hastings et al., 2005, Neilson, 2021, Park and Hahm, 2023) and assume that v_i is a function of a student's distance to school and academic quality in addition to whether the schooling option is remote or in-person.

Intuitively, the choice experiments generate random variation that we use to learn about the preferences contained in v_i. Specifically, we have unbiased estimates of the coefficients on school characteristics in Equation 1. We rely on these estimates in our second key assumption:

Assumption 2. *Let s_i correspond to the vector of observed school quality (Q_i) and distance (d_i) for student i and let ω_i be a vector of their preferences (ω_{Qi}, ω_{Ri} and ω_{di}). Formally, we assume that*

$$Y_i(1), Y_i(0) \perp\ D_i \mid v_i = v(s_i, \omega_i).$$

Assumption 2 allows us to map observed school choice characteristics s_i to indirect utility v_i through the preference vector ω_i.[13] Importantly, the choice experiments allow us to avoid the concern that observational estimates of school preferences could be confounded. Combining biased estimates with s_i would be insufficient to characterize the indirect utility v_i as the biased estimates would imply incorrect marginal rates of substitution between attributes. In sum, the choice experiments allow us to learn about a key determinant of selection into treatment that would otherwise be unobservable. We discuss several exercises to shed light on the plausibility of Assumption 2 in Sections 6.4 and 7.4.

[12] The most flexible approach would be to randomly vary a numeraire (e.g., a price) across choice trials with a remote and in-person option. This would identify v_i flexibly and without additional assumptions. However, such an approach would require each student to complete dozens of choice trials and induce survey fatigue, so an alternative approach is to use theoretically guided decisions to characterize v_i.

[13] Note that Assumption 2 is not equivalent to simply conditioning on the vector of observables s_i. For instance, two individuals may be enrolled at schools that have the same distance and peer quality but they could have different preferences over these characteristics. As a result, holding s_i constant across these students would not be sufficient to characterize the indirect utility v_i. In addition, the preference parameter ω_{Ri} is a remote schooling shifter that has no interactions with observable data (further underscoring that our approach does not only amount to conditioning on s_i).

With experimentally identified ω_i, we can summarize an individual's proclivity to self-select into treatment with the implied propensity score:

$$P(v_i) = P(v(s_i, \omega_i)) = \frac{\exp(v(s_i, \omega_i))}{1 + \exp(v(s_i, \omega_i))}.$$

For our analysis of student achievement and non-cognitive outcomes, the propensity score summarizing parental preferences serves two purposes. First, as Assumption 2 states, conditioning on the systematic component of 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. A restriction and parameterization of effect heterogeneity that is consistent with Assumption 2 is the following linear model:

$$E[Y_i | X_i, D_i, P(v(s_i, \omega_i))] = \alpha + X_i'\gamma + \beta D_i + \theta P(v(s_i, \omega_i)) + \psi\Big(P(v(s_i, \omega_i)) \times D_i\Big). \quad (4)$$

Equation 4 assumes a linear relationship between the observable preference heterogeneity and potential outcomes, bearing similarity to selection patterns studied in other settings (Abdulkadiroğlu et al., 2020, Einav et al., 2022, Kline and Walters, 2016, Otero et al., 2021). For example, θ governs selection on outcome levels, and ψ governs selection on potential 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. Note that we also explore models that allow for non-linear heterogeneous impacts to assess the robustness of our results.[14]

6 Empirical Methods

Our goal is to use choice experiments to help characterize selection into the binary treatment of enrolling in remote schooling. Therefore, we must first estimate preferences derived from the hypothetical choice experiments and use these estimates to construct estimates of v_i and $P(v_i)$. Given the selected response rates we observe, we adopt an extrapolation procedure to ensure coverage for the entire sample. We conduct a series of validation exercises and robustness checks to provide reassuring evidence supporting our empirical approach.

[14] It is worth mentioning that the restrictions outlined in Equation 4 bear similarity to control function approaches that model selection into treatment. The restrictions in such an approach would result in a model of student outcomes that can be represented as:

$$E[Y_i | X_i, s_i, \omega_i, D_i = 1] = X_i'\beta + s_i'\delta + g\left(P(v(s_i, \omega_i))\right),$$

where parametric restrictions are usually imposed on the function $g(P)$ to allow researchers to study similar selection patterns as those that we discuss above. For example, the canonical Heckit approach results in $g(P)$ being the inverse Mills ratio. In general, these approaches require a slightly different set of assumptions and interpretation, namely exclusion restrictions on the remote shifter and allowing for a direct influence of s_i on Y_i. Reassuringly, estimates adopting these alternative assumptions produce qualitatively similar results. Our preferred approach relies on fewer assumptions and a more natural interpretation of the estimates identified in the choice experiments, so we mainly report these results.

6.1 Estimating Preferences

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 in our analysis. As highlighted by Table 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.[15]

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

$$U_{ij} = \underbrace{\omega_{Qc(X_i)} Q_j + \omega_{Rc(X_i)} Remote_j + \omega_{dc(X_i)} d_{ij}}_{V_{ij}} + \varepsilon_{ij}, \tag{5}$$

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, poverty status, URM status, and sub-district code.[16] This specification makes the treatment of preference heterogeneity explicit, building on the heterogeneity we observed in earlier subsample analyses. Our approach to modeling preference heterogeneity is similar to Abdulkadiroğlu et al. (2020).

Preference extrapolation is common in the empirical school choice literature. For example, estimation procedures that rely on stability properties of centralized matches (Fack et al., 2019) require an extrapolation of preferences of individuals with larger feasible choice sets to those with smaller choice sets (Agarwal and Somaini, 2020). Equipped with these extrapolated preferences, there is precedent in using these within-cell extrapolated preferences inferred from observed choices to construct control functions to characterize selection into treatment (Abdulkadiroğlu et al., 2020, Otero et al., 2021). Our approach is similar but instead uses choice experiments to help predict choices that subsequently characterize selection into treatment, bridging literature using choice experiments to better understand demand (Mas and Pallais, 2017, Wiswall and Zafar, 2018) and literature using observed choices to characterize selection (Abdulkadiroğlu et al., 2020, Heckman, 1979).

Our estimation procedure aggregates across many hypothetical choices for each decision maker, so we now establish some notation. For each respondent i, we observe ten rank-ordered lists (ROLs) with three options. We denote the ROLs as $R_{ik} = (R_{i1k}, R_{i2k}, R_{i3k})$ and collection of ROLs for individual i as $R_i = (R_{i1}, \cdots, R_{i10})$. Similarly, let the vectors of attributes associated with each option across individual i's choices be denoted as $\mathcal{Q}_i = (Q_{i1}, \cdots, Q_{i10})$, $\mathscr{D}_i = (d_{i1}, \cdots, d_{i10})$, and $Remote_i = (Remote_{i1}, \cdots, Remote_{i10})$, where $Q_{ik} = (Q_{i1k}, Q_{i2k}, Q_{i3k})$ corresponds to the random vector of quality attributes associated with each option that partic-

[15]The results based on only students who participated in the survey are qualitatively similar to our headline estimates but are not estimated precisely. Appendix Figure C.4 shows that our preferred estimates based on the extrapolation method lie within the confidence intervals of estimates using only the conjoint sample. Our preferred estimates are more conservative in magnitude and qualitatively similar across the propensity score distribution.

[16]The LAUSD is the second largest school district in the U.S., and, as in other large urban school districts, is divided into sub-districts. Differentiating by sub-district captures preference heterogeneity that varies across space in Los Angeles.

ipant i observed in hypothetical choice k; d_{ik} and $Remote_{ik}$ are defined similarly.

Given the maintained assumption that ε_{ij} is a Type I extreme value, independent across options, and independent across choice experiments, the likelihood function for a given individual i can be written as:

$$\mathcal{L}(R_i|\mathcal{Q}_i, Remote_i, \mathscr{D}_i, X_i) = \prod_{k=1}^{10} \frac{\exp(V_{iR_{i1k}})}{\sum_{m\in\{R_{i1k},R_{i2k},R_{i3k}\}} \exp(V_{im})} \frac{\exp(V_{iR_{i2k}})}{\sum_{m\in\{R_{i2k},R_{i3k}\}} \exp(V_{im})}.$$

We aggregate across individuals and estimate preference models separately for each covariate cell, $c(X_i)$, via maximum likelihood and obtain a vector of coefficients, $(\omega_{Qc(X_i)}, \omega_{Rc(X_i)}, \omega_{dc(X_i)})$ for each cell c. The estimated vector of coefficients is key to characterizing v_i and $P(v_i)$ that we discuss next.

6.2 Propensity Score Estimates

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 v_i. To do this, we assume the following choice model for each student who makes a decision between enrolling in their neighborhood school or enrolling in the remote option:

$$v_i = \omega_{Qc(X_i)}Q_i + \omega_{Rc(X_i)} + \omega_{dc(X_i)}d_i,$$

where Q_i is the observed achievement at the remote option relative to student i's neighborhood school and d_i is the travel time to student i's neighborhood school. Equipped with experimental estimates of the preference parameters and student-specific relative choice attributes, the implied propensity score is $P(\hat{v}_i)$. Thus, our approach effectively assumes that the experimentally identified taste parameters—$\omega_{Rc(X_i)}$, $\omega_{Qc(X_i)}$ and $\omega_{dc(X_i)}$—are sufficient to pin down the correct marginal rates of substitution *within cell* necessary to convert the within cell variation in Q_i and d_i into propensity scores. If the propensity scores are accurate, then assumption 2 will allow us to use them to account for selection into treatment in the spirit of Rosembaum and Rubin (1983). Appendix Table A.1 reports summary statistics for the preference estimates.

How accurate are the estimated propensity scores? Later in Section 8.3, we provide a number of robustness checks meant to probe their validity. Intuitively, if the propensity score estimates are correct, then they should replicate the observed average likelihood that individual students in our data, even those not contained in our conjoint sample, actually take up the remote option. Reassuringly, we find that the estimated propensity scores are, in fact, highly predictive of real-world choice behavior.

6.3 Empirical Specification

Our causal analysis focuses on the following empirical specification for a cognitive or non-cognitive outcome Y_i:

$$Y_i = \alpha_c + \gamma' X_i + \beta D_i + \theta P(\hat{v}_i) + \psi\left(P(\hat{v}_i) \times D_i\right) + \epsilon_i, \tag{6}$$

which augments Equation 4 by including covariate cell fixed effects α_c and a vector of remaining mean zero baseline characteristic controls X_i. The covariate cell fixed effects are necessary to ensure that the variation in the propensity score leveraged for identification is driven entirely by the way students trade-off academic achievement and travel time as implied by the preferences estimated in the conjoint, and *not* from differences in the estimated preferences across students with different characteristics. The latter source of variation will be mechanically related to the student-level characteristics used for extrapolation and hence could create an avenue for confounding, which we avoid by including cell-fixed effects. We report robust standard errors clustered at the school level to account for correlation within schools.

A key component of our analysis centers on β, the average causal effect of remote learning. To interpret estimates from Equation 6 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.

6.4 Balance Tests and Validity

This section provides an initial test of the validity of our empirical approach by assessing balance on baseline student characteristics using our propensity-score based method.[17] Specifically, we use measures of lagged academic achievement as dependent variables in specifications based on Equation 6. Panel (a) of Figure 4 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 first two black bars, which show that conditioning on a rich set of covariates commonly used in the teacher and school value-added literature (Koedel and Rockoff, 2015) does *not* balance baseline test scores, which is the average of lagged ELA and math achievement. We also construct a covariate index by projecting math scores onto a vector of student characteristics including lagged test scores and grade indicators. The differences are sizable and range between 20 and 28 percent of a standard deviation. We also consider two non-cognitive outcomes, bullying and grit, both summarized by the indices we constructed from student survey data. Baseline balance tests that do not condition on our experimental preference estimates also fail for the two non-cognitive outcomes, showing that students self-selecting into the remote sector tend to have worse bullying outcomes and lower measures of grit.

In contrast, the results in the 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. We also show that our experimental preference estimates balance bullying and grit. The ability to balance lagged achievement, a rich covariate index, and two unrelated non-cognitive outcomes is reassuring from a causal perspective (Rosembaum and Rubin, 1983).

Notably, the ability to achieve balance appears to be unique to the experimental estimates. In Appendix Figure C.3, we show results that rely on propensity scores estimated using an observational approach rather than our survey data. When using the observationally estimated

[17] We defer discussion of additional exercises to test the validity of our empirical approach to Section 7.4.

propensity scores, the resulting balance tests fail. This lack of balance using observational methods emphasizes the importance of the experimental preference estimates in our empirical strategy, formally outlined in Assumption 2. The economic intuition for these contrasting findings is that the experimental estimates allow us to effectively pin down the marginal rates of substitution that govern choice behavior and more effectively characterize or learn about v_i through the experimentally identified ω_i.

7 Main Results

We provide a comprehensive assessment of remote learning's impacts on cognitive and non-cognitive outcomes measured in 2022. We consider cognitive outcomes from standardized test scores measured in 2022 and non-cognitive outcomes from the School Experience Survey, also measured in 2022. We further leverage our experimental preference estimates to assess treatment effect heterogeneity with respect to preference intensity and to more adequately characterize selection into the learning modality in terms of cognitive and non-cognitive outcomes. The latter set of exercises allows us to answer important empirical questions related to match quality and general sorting patterns.

7.1 Mean Test Score Impacts

We begin by examining the average effects of remote learning on academic outcomes. Panel (b) of Figure 4 reports average effects for our primary outcome, 2021–2022 academic achievement. On the left, the typical value-added estimates that condition on student attributes and lagged achievement show negative remote-learning effects ranging from 23 to 26 percent of a standard deviation. These effects are consistent with other studies employing alternative quasi-experimental methods that also find negative selection into remote learning (Bueno, 2020). By way of contrast, our estimates based on Equation 6 are more modestly negative for the average student. ELA and math effects for the average student are -0.13σ and -0.14σ, respectively. 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). Column 1 of Panel A in Table 2 reports the mean effects displayed in Figure 4, along with other selection parameter estimates, which we will discuss shortly.

7.2 Mean Non-Cognitive Impacts

Impacts on academic outcomes shed light on one side of the story but potentially paint an incomplete picture. Bacher-Hicks et al. (2022) find that reductions in bullying during the pandemic-induced remote learning experience contributed to mixed evidence on students' mental health and well-being, suggesting that students who were bullied pre-pandemic may have benefited from the remote experience along non-academic margins. The existing evidence motivates our focus on bullying and other non-cognitive outcomes of interest, such as grit, which has also been shown to be affected by schools (Jackson et al., 2020).

The rightmost two bars in Panel (b) of Figure 4 show a substantial mean improvement, roughly 0.17σ, in the bullying index and a precise mean null impact on the grit index. The bullying index contains physical (in-person) and non-physical (online) bullying questions. The reduction in physical bullying is partly mechanical, as remote students have less frequent encounters with their peers. Non-physical bullying materializes online through social media channels, which has also been linked to recent increases in adolescent depression (Twenge, 2017, Twenge et al., 2022, 2020).

Column 1 of Panel B in Table 2 reports mean impacts separately for physical and non-physical bullying. We find improvements in both physical and online bullying, with online bullying mean impacts of roughly half the magnitude of physical bullying, amounting to 0.15σ and 0.31σ improvements, respectively. This suggests that the reduction in bullying operated in ways that potentially improved the overall mental well-being of students and that potentially have complementary roles for academic outcomes.

7.3 Selection and Treatment Effect Heterogeneity

Our measures of preference intensity allow us to characterize both selection on levels into remote learning and also heterogeneity in its causal effect. For models with academic achievement as an outcome, the parameter θ governs selection on levels. It serves as a summary measure of the academic preparedness of students with varying degrees of remote learning demand. For models with bullying as the outcome, θ is a summary measure of students' proclivity to be bullied independent of the school in which they enroll. These summary measures governing sorting patterns allow us to paint a more complete picture of the factors governing sorting into the remote sector in the post-pandemic landscape. We begin by characterizing students sorting into remote schooling based on their academic and non-cognitive potential.

Starting with academic potential, Column 2 of Table 2 demonstrates that students with larger estimated tastes for remote learning perform more poorly on standardized exams than students with lower estimated tastes regardless of the school in which they enroll. Similarly, the results show that students with larger estimated tastes for remote learning have worse bullying-related outcomes regardless of their enrolled school. The difference between a student at the 90th and 10th percentile of the remote taste distribution is roughly -0.11σ, a strong indication that students with worse bullying-related outcomes are substantially more likely to sort into virtual schooling. This is consistent with the evidence in Table 1 showing that remote-learning students have worse baseline test scores and bullying outcomes, but the results using the experimental-based measure of demand reveal more specific sorting patterns directly related to preferences for remote learning.

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 the intensity of family preferences. The parameter ψ summarizes match effects, where $\psi > 0$ is an indication of positive sorting on gains. Table 2 reports point estimates from our preferred specification, while Figure 5 summarizes these results by plotting the mean treatment effects (i.e., $\hat{\beta} + \hat{\psi}p$), calculated separately for 12 bins of the demeaned propensity score. The upward slope shown in the figure reflects that the interaction coefficient $\hat{\psi}$ is positive at around 0.073 and 0.082 for ELA and math, respectively. The match effects are sizable, generating positive remote effects for a small share of students with large estimated remote-learning tastes. Reassuringly, Appendix Table C.2 shows that our estimated parameters remain qualitatively similar when we relax the linearity assumption in our approach and estimate a specification that includes quadratic terms for the propensity score and its associated interactions.[18]

For bullying outcomes, Panel B of Table 2 reports more nuanced results depending on the type of bullying interaction. The omnibus bullying index, including both online and physical bullying and reported in the first row, reveals mild negative selection on gains relative to a sizable 0.165σ main effect of remote learning. In other words, the difference in the remote effect between a student at the 90th and 10th percentile of the taste distribution is roughly 0.03σ. The subsequent two rows differentiate between physical and online bullying. As reported earlier, the treatment effects for physical bullying are twice as large as the effects on online bullying, but the match effects differ in sign. We find negative match effects for physical bullying and positive match effects for online bullying, but both are modest in size relative to sizable positive mean improvements on both. In summary, the match effects on bullying outcomes are sufficiently small that the overall treatment effects on bullying outcomes remain positive for all students across the taste distribution.

Turning to another non-cognitive outcome, grit, we do not find meaningful mean impacts of remote learning. This mean impact masks heterogeneity. The difference in treatment effects between the 90th and 10th percentile students is 0.05σ. This evidence is consistent with prior evidence showing grit is malleable in schools (Alan et al., 2019, Jackson et al., 2020). Match effects are positive, pointing to another margin families are sorting on that likely complements learning gains.

How do we interpret this collection of results? Taking the estimates for math achievement in Figure 5 literally suggests that students in the top decile of the estimated propensity score distribution do no worse than they would in person, and that some even have positive treatment effects. The typical student at the 95th percentile who enrolls in remote learning experiences a 0.04σ increase in achievement in math. 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). More generally, there are across-the-board improvements in bullying-related outcomes, both physical and online, for students with both low and high estimated demand for remote learning. This suggests that many students are willing to forego some relative academic improvements at their in-person neighborhood school to obtain positive returns along other non-cognitive dimensions such as bullying. The students with the largest estimated tastes for remote schooling benefit along both margins.[19]

7.4 Lottery-Based Validation

While the balance analysis in Section 6.4 is reassuring, this section explores another way to assess the credibility of our main empirical approach. Specifically, we use lottery variation to experimentally validate the heterogeneous treatment effect estimates generated from our

[18] Specifically, we estimate the following model: $Y_i = \alpha_c + \gamma' X_i + \beta D_i + \theta_1 P(\hat{v}_i) + \theta_2 P(\hat{v}_i)^2 + \psi_1 \left(P(\hat{v}_i) \times D_i\right) + \psi_2 \left(P(\hat{v}_i)^2 \times D_i\right) + \epsilon_i$.

[19] Appendix Table C.4 reports qualitatively similar impacts on 2023 outcomes, showing that remote enrollment in 2022 has persistent effects.

conjoint-based model of achievement. As we detail below, we find that our main estimates are forecast unbiased and that the predictive validity of each lottery is uniformly good based on overidentification tests (Angrist et al., 2017, Deming, 2014).

LAUSD has a large portfolio of offerings beyond neighborhood schools such as magnet programs, affiliated charter schools, and schools with selective criteria. Oversubscribed schools use lotteries to determine offers, and we use this lottery variation to validate our empirical approach. Intuitively, some students may hold remote learning as their most preferred alternative to a given oversubscribed school. For these students, randomly failing to receive an offer at their most preferred school exogenously increases the likelihood they attend remotely. Thus, if we find that our conjoint-based estimates of the student level heterogeneous effects from our main analysis predict the lottery-based causal effect for students, then this constitutes strong evidence in favor of the validity of our main approach.

For the 2022 academic year, there were 32 oversubscribed programs where students had remote learning as a fallback option. The random variation induced by losing the lottery allows us to assess both the average predictive validity of our approach and whether each lottery generates test score gains that are proportional to the predictions implied by our preferred model. Let Y_i correspond to the observed outcome of individual i and let $\tilde{Y}_i$ be the *predicted* outcome based on the parameter estimate results from Equation 6, the observed decision to go remote, and the associated propensity score estimates. Formally, we estimate the following model via two-stage least-squares:

$$Y_i = \phi_0 + \phi_1' W_i + \phi_2 \tilde{Y}_i + \epsilon_{i2} \tag{7}$$

$$\tilde{Y}_i = \pi_0 + \pi_1' W_i + \sum_{\ell} \pi_\ell Z_{i\ell} + \epsilon_{i1}, \tag{8}$$

where W_i is a vector of school-by-grade lottery strata so that lottery offers (denoted by $Z_{i\ell}$ for each of the ℓ lotteries) are random conditional on W_i.

The parameter of interest from Equation 8 is ϕ_2. A coefficient estimate of unity ($\phi_2 = 1$) indicates that the estimated treatment effect heterogeneity from our preferred model is forecast unbiased for the actual causal effects implied by the lottery. To interpret this, consider the case of standardized math scores. The finding of $\hat{\phi}_2 = 1$ shows that variation in conjoint-based predicted math achievement based on the lottery variation from $Z_{i\ell}$ modeled in the first-stage accurately predicts the *observed* variation in test scores in the second-stage. Tests of this nature are common in the education literature (e.g., Angrist et al., 2017).

Appendix Table C.1 reports the results. For both math and ELA, the first stage is well-powered, with the F-stats in excess of 10. For math, the forecast coefficient is 1.03, which suggests that the estimated heterogeneous effects are near perfect predictors of the actual change in test scores implied by the lottery variation. For ELA, the forecast coefficient is 0.67; however, the 95% confidence interval for this estimate also includes 1. In fact, for both math and ELA, we fail to reject the hypothesis that the coefficients are forecast unbiased (individually and jointly) using a formal over-identification test. More generally, we interpret the validation results as reassuring evidence that our empirical approach can be successful in a variety of other settings outside the remote learning context. At a minimum, we do not find concerning evidence regarding our empirical approach in this remote learning context.

8 Additional Robustness and Validation Exercises

This section provides additional tests of the robustness and validity of our empirical approach. We show that our conclusions are robust to alternative approaches to inference and varying the parameterization of utilities. Finally, we show that our extrapolated preferences generate propensity scores that are forecast unbiased and predict real-world behavior—findings that provide reassuring evidence that it is reasonable to extrapolate from the conjoint sample to the full sample.

8.1 Accounting for Variability from "First-stage" Estimation Error

Our main estimates and inference do not account for estimation error introduced in the preference estimation stage. To account for this error in the first-stage, we sample from the asymptotic joint distribution of the preference estimates 250 times. Equipped with these estimates, we construct the implied propensity score for each iteration and estimate Equation 6. We then report the mean estimate across iterations along with the 95 percent confidence region. Appendix Table D.1 and Appendix Figures D.1 and D.2 show that accounting for estimation error in the propensity scores does not qualitatively affect our estimates or inference.

8.2 Robustness to Varying the Functional Form of Preferences

In our preferred model, we use a linear parameterization of preferences for travel time and academic quality and assume 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 (a)–(c) of Appendix Table C.3 report estimates that are remarkably similar to our preferred estimates from the more parsimonious model.

8.3 Propensity Score Validation Exercises

To address concerns regarding the fact that our preferred model uses preferences that are extrapolated from the conjoint sample to all of LAUSD, we perform three exercises. The first exercise addresses the concern that there may not be sufficient overlap between the distribution of covariates for the subset of students whose parents completed the choice experiment survey and the full sample of LAUSD students. Appendix Figure C.1 summarizes baseline characteristics for each student using an index measure and plots the distribution for the survey and the general LAUSD samples.[20] The figure shows substantial overlap, indicating there is ample support to estimate preferences and extrapolate to non-survey respondents.

A related concern is the possibility that the extrapolation procedure may not accurately characterize the preferences of students who were not included in the estimation sample. To

[20]The index is the predicted ELA 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).

address this issue, we employ an out-of-sample validation approach. Intuitively, the procedure "mimics" the extrapolation exercise *within* the sample of students where we can actually estimate preferences. This allows us to validate the extrapolated preferences against the *actual* estimated preferences of students who were not used in the act of extrapolation. In other words, it will allow us to directly compare extrapolated preferences to actual preference estimates for a subsample where we can observe both.[21] Appendix Figure C.2 plots the extrapolated propensity scores against the true propensity scores. The associated slope is 0.96 and the intercept is near zero—a pattern of findings that indicates the extrapolation is approximately forecast unbiased for the preferences of students not included in the estimation sample.[22]

A final concern is that the probabilities derived using the data from families that completed the conjoint survey may not accurately characterize the real-world behavior of the broader sample of LAUSD students. To address this possibility, we ask whether the extrapolated preferences predict subsequent, real-world remote enrollment decisions. To accomplish this, we estimate models of the following form: $D_i = \pi_c + \beta P(\hat{v}_i) + \zeta_i$, where D_i is the indicator for actual remote enrollment in 2022, π_c is a covariate cell fixed effect, and $P(\hat{v}_i)$ is the propensity score for student i. We find that the extrapolated probabilities from our model—which is estimated using only information from the conjoint survey respondents—are effectively forecast unbiased for real world remote-enrollment decisions ($\hat{\beta} = 1.11$, $SE(\hat{\beta}) = 0.10$) among the full sample of LAUSD students. Note that including cell fixed effects ensures that the residual variation exploited in this specification mimics our main empirical exercise, as it is driven by estimated preferences and *not* driven by systematic differences in instructional mode uptake across demographic groups of students. Thus, the students that our model predicts have a high likelihood of remote enrollment, based purely on the conjoint derived preferences, are indeed more likely to enroll in remote-learning.[23] This suggests that our method captures important determinants of behavior even among families that did not complete the conjoint survey.

9 Conclusion: Policy Implications and Future Research

The COVID-19 pandemic accelerated national growth in remote-learning enrollment substantially (see Figure 1). As of the 2021-2022 academic year, roughly 800,000 U.S. students were engaged in remote instruction, rivaling both the Catholic and charter school total enrollments of 1.7 and 3.7 million, respectively. School districts are currently planning to expand remote

[21] Formally, the algorithm works as follows. We begin by creating 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 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 compares the two propensity scores to assess extrapolation quality.

[22] The mean difference between the extrapolated and true propensity score is -0.007, and the distribution is centered around 0 with standard deviation 0.08.

[23] Perhaps more strikingly, we find that the propensities estimated using the survey data can explain nearly 10% of the real-world variation in remote enrollment decisions for our full sample of LAUSD students ($R^2 = 0.09$). Importantly, the R^2 reported here reflects the explanatory power of $P(\hat{v}_i)$ *within* covariate cells. Therefore, this estimate reflects the share of variation explained by our preference estimates ($\hat{v}_i$) and *not* the covariate cell fixed effects.

options to satisfy continued parental demand (Musaddiq et al., 2022).

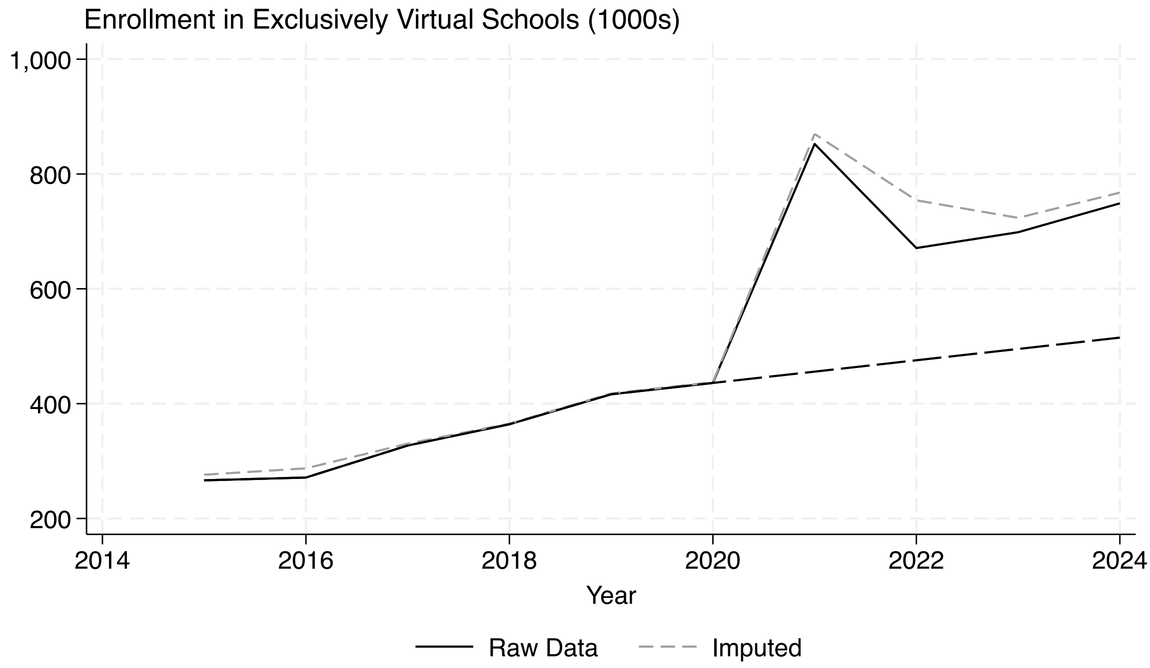
At first glance, it may seem puzzling why parents and students would willingly select into remote learning schooling options. Before the pandemic, a near consensus suggested that virtual schools negatively affect learning (Bueno, 2020, Fitzpatrick et al., 2020, Raymond et al., 2023). More recent studies on pandemic-era remote schooling similarly document learning losses (Goldhaber et al., 2022, Jack et al., 2022, Singh et al., 2022).

In this paper, we shed light on the drivers of growth in remote learning by studying selection and the heterogeneous causal impacts of this learning modality. Our evidence is based on original survey data that we combine with a novel empirical framework that uses choice experiment data to characterize selection into remote schooling. The survey was conducted after schools in Los Angeles returned to offering both traditional in-person and remote schooling options. 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. The results from our survey allow us to control for selection into remote learning while also allowing us to explore how remote learning effects vary with preferences. We validate the new approach using school choice lotteries and demonstrate robustness on several margins.

Our analysis provides important evidence of heterogeneous impacts of remote learning that suggest this form of learning can indeed be a preferred schooling modality for two main reasons. First, we demonstrate that, while remote-learning reduces achievement on average, there are positive match effects that are sufficiently strong to imply that the subset of students with the highest demand for remote-learning will experience gains in academic outcomes. Second, remote learning delivers an across-the-board improvement in bullying outcomes, including online bullying, relative to in-person learning. Importantly, this finding suggests that the substantial improvements in bullying outcomes could serve as a compensating differential for worse achievement for the students with weaker academic match effects.

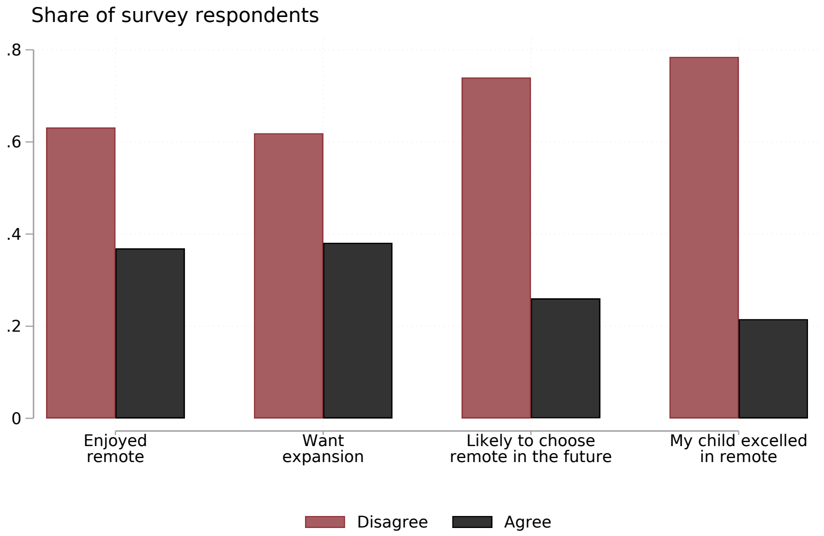
The combined findings underscore the nuanced nature of parental preferences. While much of the existing literature has focused on parental preferences for different dimensions of academic quality (Abdulkadiroğlu et al., 2020, Ainsworth et al., 2023, Campos, 2023, Hastings and Weinstein, 2008, Rothstein, 2006), an emerging consensus emphasizes the multi-dimensional nature of the education production function (Beuermann et al., 2022, Jackson, 2018, Jackson et al., 2020). In this paper, we draw a link to an understudied but increasingly important aspect of the broader schooling environment, bullying (Bacher-Hicks et al., 2022). Increases in adolescent depression are linked to social media and online harassment (Twenge, 2017, Twenge et al., 2022, 2020), which naturally connect to the in-person schooling environments. The remote schooling context allows us to demonstrate a previously undocumented tradeoff parents may face if their children experience substantial bullying. Our findings show that families are potentially willing to forego short-run human capital gains for this understudied but increasingly policy-relevant outcome. More work is needed to better understand how parents trade-off academic quality and bullying outcomes.

Figure 1: Remote Schooling Enrollment Trends, NCES 2015-2024



Notes: This figure reports enrollment trends in exclusively virtual schools as reported in the Common Core data provided by the National Center for Education Statistics (NCES). The figure reports three time-series plots. The first (solid black) corresponds to one derived by plotting the raw data in the Common Core data. Many school districts, however, under- or misreport their remote schooling numbers. The second (dashed gray) accounts for this measurement error by imputing additional enrollment for the fifteen largest school districts in the country. The third (dotted black) is a prediction-based series from a regression of enrollment on a linear time trend for the period 2015 to 2020.

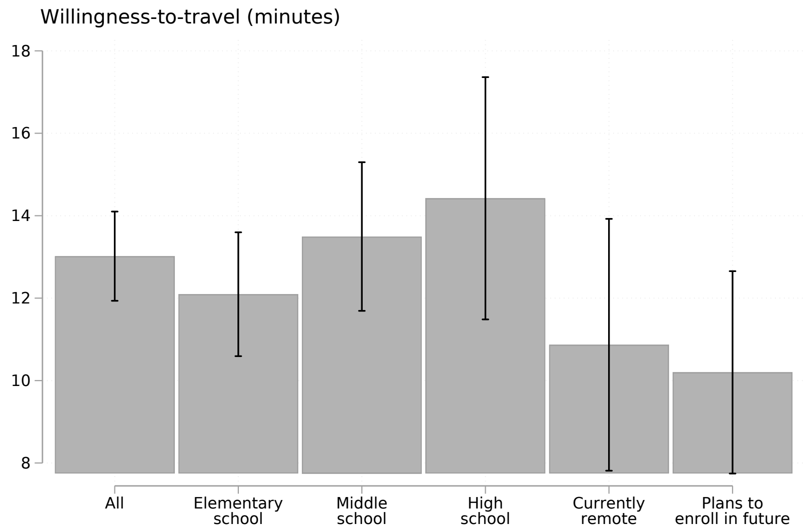
Figure 2: Experiences and Demand for Remote Learning



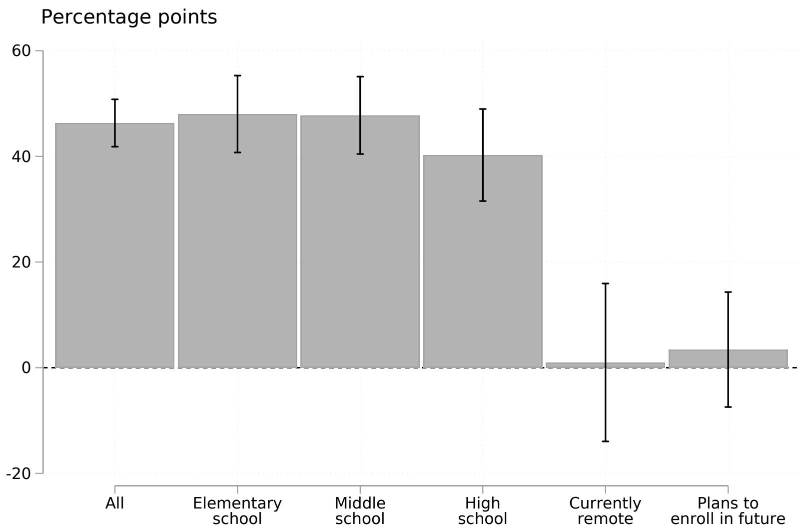
Notes: This figure reports survey results on the share of respondents ($N = 3,539$) who agree with four statements on their experiences and demand for remote learning. Individual responses are weighted to produce means that correspond to the average family in LAUSD. Specifically, we define the weight for each observation as $w_i \equiv P(Survey = 1)/p(Survey = 1|X_i)$, where $p(Survey = 1|X_i)$ is the estimated propensity to respond to the survey based on student characteristics X_i using the full sample of LAUSD students, and $P(Survey = 1)$ is the share of all LAUSD families with survey responses. Appendix Section A.1 reports the complete text for the survey questions (see question 5).

Figure 3: Experimental Preference Estimates

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



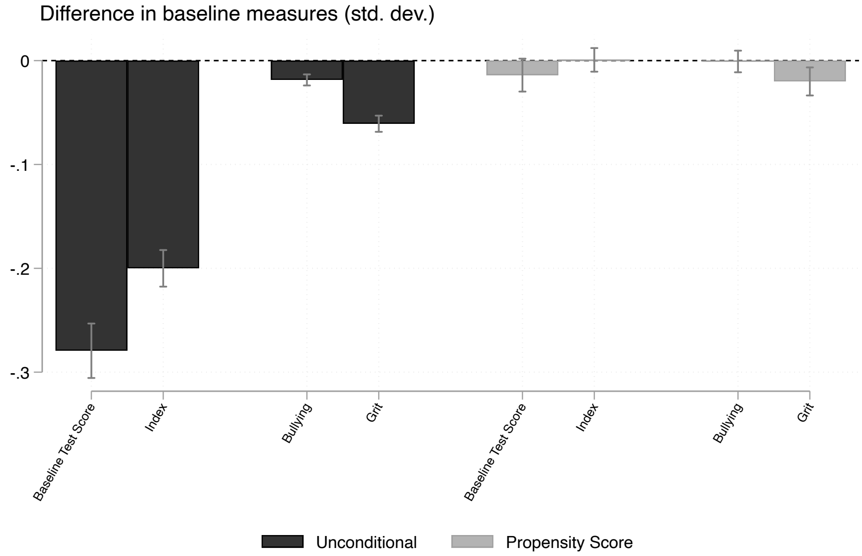
(b) Increase in achievement rate necessary to switch to remote



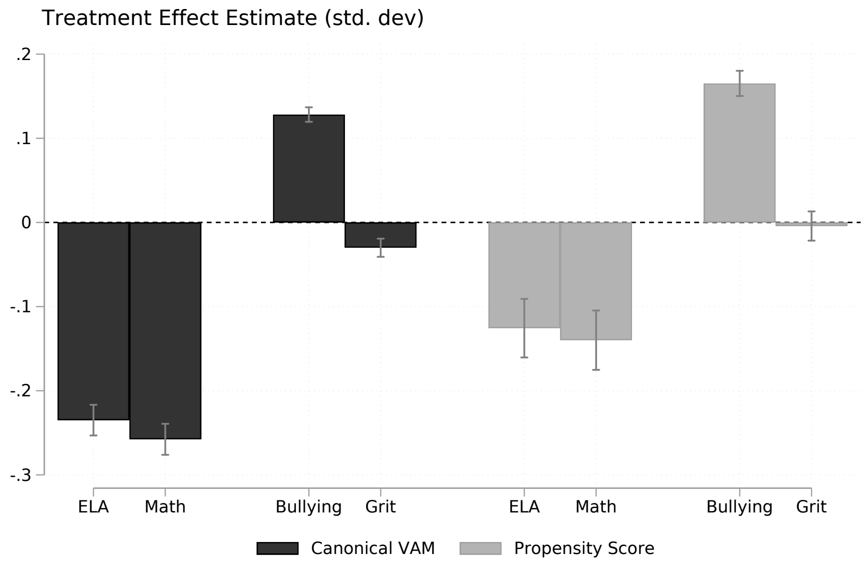
Notes: This figure reports willingness to travel estimates for achievement in Panel (a) and the estimated achievement necessary to make families indifferent between in-person and remote learning in Panel (b). Preference estimates are from a rank-ordered logit model relating indirect utilities of hypothetical 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 results from estimating Equation 1 using a different sample. By re-estimating the same model across subsamples, we allow for heterogeneity in preferences across the population. For example, the "All" bar in both panels corresponds 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. Sample sizes (total number of choice responses) for both panels: N = 22,338 (All), N = 9,780 (Elementary school), N = 7,947 (Middle School), N = 4,611 (High School), N = 2,088 (Currently remote), N = 3,012 (Plans to enroll in future).

Figure 4: Baseline Balance and the Average Effects of Remote Learning

(a) Student baseline characteristics



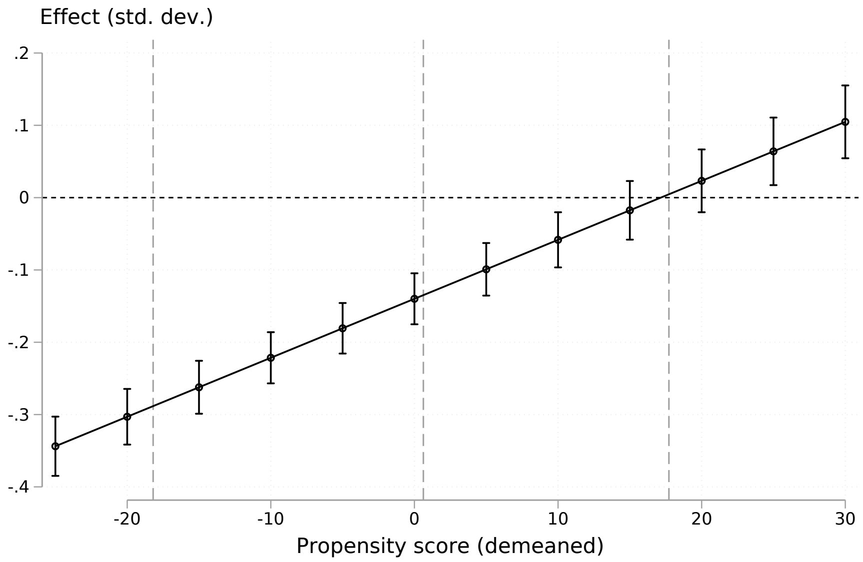
(b) Average effects on post-pandemic (2022) ELA and math achievement



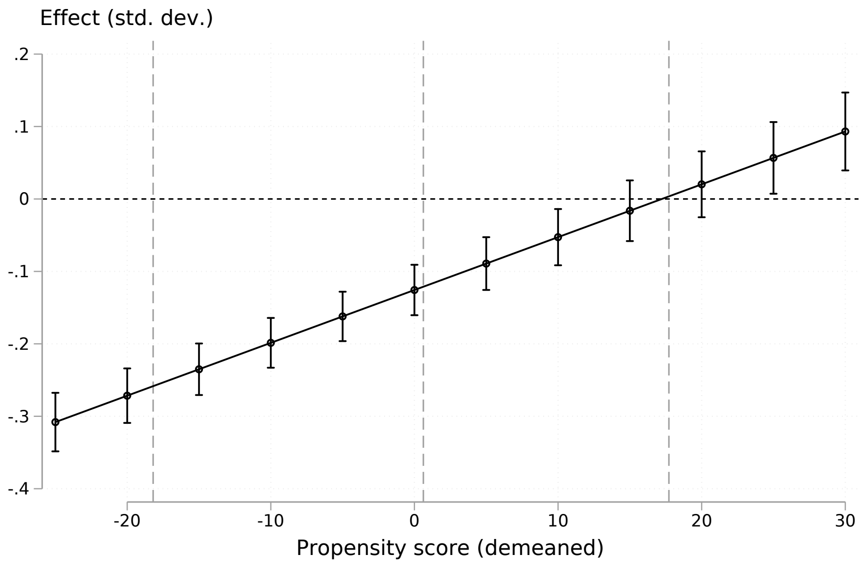
Notes: This figure reports estimates of the average effect of remote learning. Panel (a) reports balance test results where the dependent variable is set to the average of measures of lagged (2019) achievement scores in ELA and math, a summary index of baseline covariates, and index measures for bullying and grit (which are normalized so that larger values denote better outcomes).To construct the summary index, we regress 2022 math achievement on a vector of baseline covariates including race, sex, socioeconomic status, English learner status, and special education status as well as lagged (2019) achievement in ELA and math. The summary 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 correspond to results from models based on Equation 6, which controls for estimated propensity scores. Panel (b) reports corresponding results where the dependent variable is set to a measure of post-pandemic achievement in ELA and math as well as bullying and grit outcomes. Standard errors are robust and clustered at the school level. Gray bars are estimates of the 95 percent confidence intervals. Sample sizes for Panel A are as follows: $N = 146{,}908$ (Baseline Test Score), $N = 148{,}295$ (Index), $N = 142{,}195$ (Bullying, Grit). Sample sizes for Panel B are reported in Table 2.

Figure 5: Estimated Match Effects on Post-Pandemic Math Achievement

(a) Math



(b) ELA



Notes: This figure reports estimates of treatment effects on post-pandemic (2022) math and ELA test scores for 12 bins of estimated propensity scores. The points in black are means for each bin based on Equation 6 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., $\hat{\beta} + \hat{\psi}p$). 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 clustered at the school level. Bars surrounding the mean estimate for each bin are estimates of the 95 percent confidence intervals. Sample sizes for both panels are reported in Table 2.

Table 1: Summary Statistics for LAUSD Students

	(1) In-Person in 2022	(2) Remote in 2022	(3) Mean Diff.	(4) Survey Respondents	(5) Conjoint Respondents
Baseline ELA Scores	0.008	-0.234	-0.243***	0.191	0.471
	(0.992)	(0.955)	(0.028)	(1.045)	(1.045)
Baseline Math Scores	0.012	-0.311	-0.323***	0.170	0.445
	(0.992)	(0.923)	(0.028)	(1.011)	(1.011)
Baseline Bullying Index	0.007	-0.013	-0.020***	0.011	-0.006
	(0.647)	(0.677)	(0.003)	(0.608)	(0.667)
Baseline Connectedness Index	0.014	-0.071	-0.085***	-0.016	-0.044
	(0.568)	(0.596)	(0.004)	(0.576)	(0.580)
Baseline Grit Index	0.009	-0.054	-0.062***	0.047	0.089
	(0.664)	(0.679)	(0.006)	(0.667)	(0.649)
Female	0.484	0.505	0.021***	0.494	0.508
	(0.500)	(0.500)	(0.002)	(0.500)	(0.500)
Special Education	0.139	0.153	0.014***	0.108	0.101
	(0.346)	(0.360)	(0.002)	(0.311)	(0.301)
URM	0.817	0.842	0.025**	0.756	0.651
	(0.386)	(0.365)	(0.011)	(0.429)	(0.477)
English Learner	0.381	0.324	-0.057***	0.315	0.167
	(0.486)	(0.468)	(0.009)	(0.465)	(0.373)
Poverty	0.828	0.812	-0.016	0.740	0.598
	(0.377)	(0.390)	(0.010)	(0.439)	(0.491)
Students	276,553	12,326		3,539	1,171

Notes: This table provides summary statistics for LAUSD students as observed in the LAUSD student micro-data. Index variables (bullying, connectedness, and grit) are normalized such that larger values denote better outcomes. Columns 1 and 2 report averages for in-person and remote students, respectively. Column 3 reports the corresponding difference in average characteristics. We recruited a sample of survey respondents by randomly contacting 100,000 families through the LAUSD's internal communication system in April 2022. Column 4 reports averages for every family who completed at least one question on our survey. Column 5 reports averages for every student who completed the hypothetical choice experiment questions within the survey. Baseline test scores are measured in the 2018–2019 school year, and baseline non-cognitive outcomes are measured in the 2020-2021 school year. In Columns 1, 2, 4, and 5, standard deviations for each measure are reported in parentheses. In Column 3, standard errors clustered at the school level from a regression of each measure on a remote indicator are reported in parentheses.

Table 2: Causal Effects of Remote Learning on Key Outcomes

	(1) Main Effect (β)	(2) Selection on Levels (θ)	(3) Selection on Gains (ψ)
Panel A: Cognitive Outcomes			
ELA ($N = 210,306$)	-0.126	-0.194	0.073
	(0.018)	(0.020)	(0.006)
Math ($N = 211,324$)	-0.140	-0.201	0.082
	(0.018)	(0.021)	(0.005)
Panel B: Non-Cognitive Outcomes			
No Bullying Index ($N = 181,723$)	0.165	-0.030	-0.009
	(0.008)	(0.007)	(0.002)
No Physical Bullying ($N = 181,632$)	0.308	-0.032	-0.031
	(0.008)	(0.008)	(0.003)
No Online Bullying ($N = 177,249$)	0.149	-0.020	0.013
	(0.008)	(0.007)	(0.003)
Grit Index ($N = 184,041$)	-0.004	-0.024	0.014
	(0.009)	(0.009)	(0.003)

Notes: This table reports estimates of the effect of remote learning on cognitive and non-cognitive outcomes. All outcomes are measured in the post-pandemic school year 2022. Treatment effect estimates are based on the model specified in Equation 6. Panel (a) provides results on ELA and math achievement, and Panel (b) provides results on bullying-related outcomes and a grit index. Index variables are normalized such that larger values denote more beneficial outcomes. 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.

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Online Appendix

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

Jesse Bruhn   Christopher Campos   Eric Chyn   Anh Tran

September 11, 2025

Table of Contents

A Data Appendix

A.1 Remote-Learning Survey

A.1.1 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? *

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	○	○	○

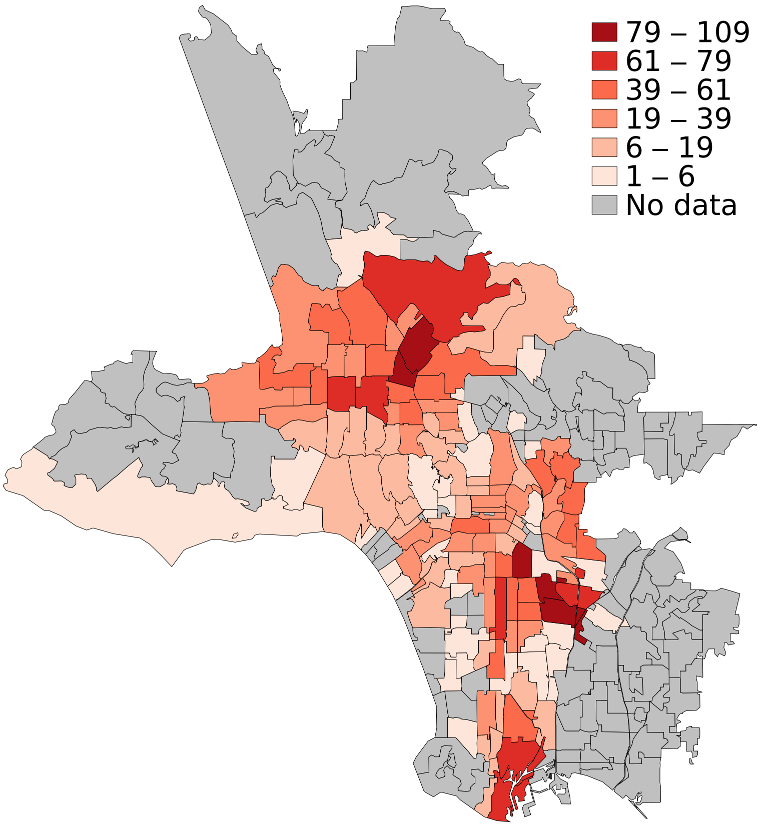
(untitled)

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

- Yes
- No

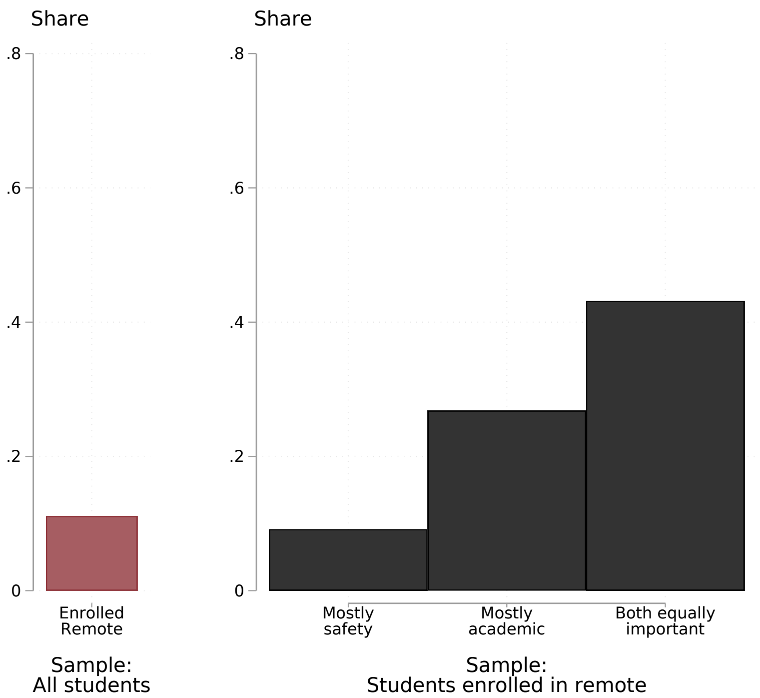
A.1.2 Descriptive Statistics

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 zip code and is shaded according to the number of remote-learning respondents residing in the zip code. Most of the gray areas in the figure are outside the purview of LAUSD. The cuts correspond to the 25th, 50th, 75th, 90th, and 95th percentiles of the zip code-level distribution

Figure A.2: Reasons for Enrolling in Remote Learning



Notes: This figure reports various statistics relating to respondents' current remote learning status and their reasons for enrollment. The first red bar reports the share of respondents with students currently enrolled in remote learning. The next three bars report shares of respondents' reason for selecting remote learning, conditional on current remote status. Observations are weighted to produce means that correspond to the average family in LAUSD. In particular, we predict whether we observe a survey response and obtain a propensity score $p_i = p(X_i)$. We weight each observation by $w_i = \frac{P(Survey=1)}{p_i}$, where $P(Survey = 1)$ corresponds to the share of families with survey responses. Appendix Section A.1 reports the actual survey questions, all part of Question 5.

A.2 Demand for Remote Learning

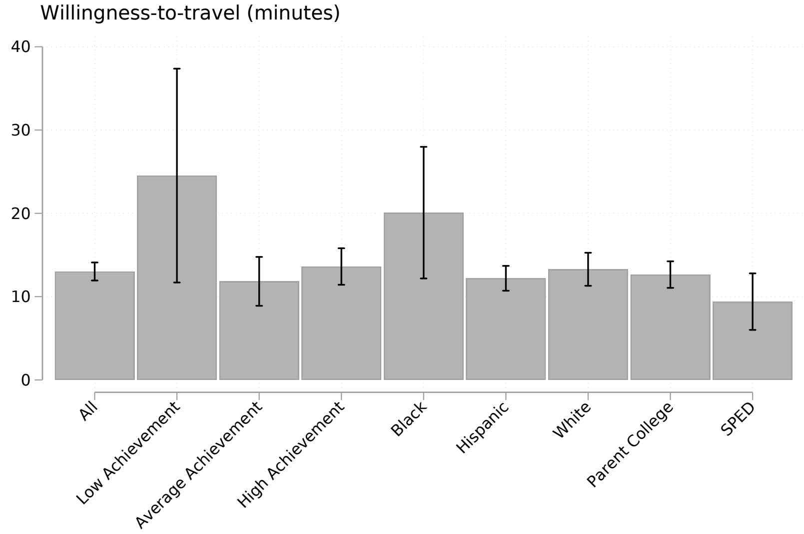
Table A.1: Summary Statistics for Preference Estimates

	(1) Mean	(2) SD	(3) P5	(4) P95
Academic Quality (ω_Q)	0.04	0.01	0.02	0.08
Remote (ω_R)	-2.08	0.64	-3.74	-0.36
Travel Time (ω_d)	-0.03	0.01	-0.06	-0.01
($-\omega_Q/\omega_d$)	1.41	0.55	0.65	2.75
(ω_r/ω_Q)	49.14	19.34	17.14	84.62
Number of Cells		32		

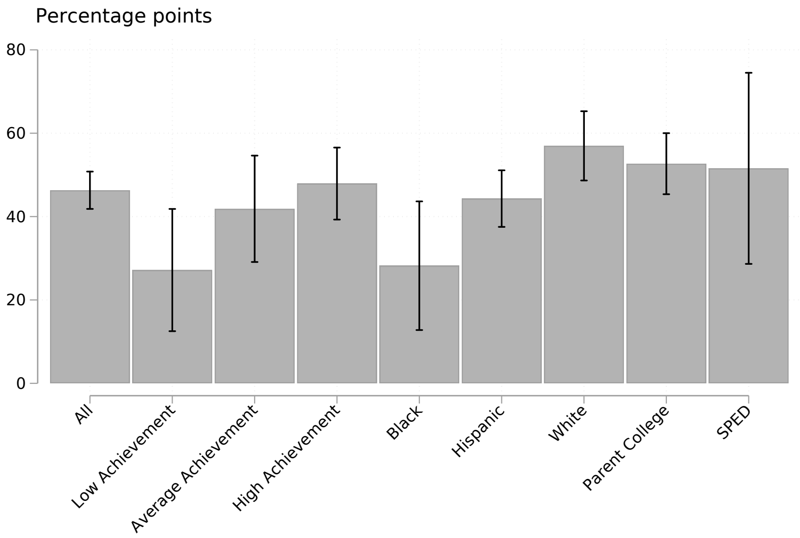
Notes: This table reports summary statistics for preference parameters that were estimated separately for each covariate cell. Columns 1–4 report the mean, standard deviation, and the 5th percentile and 95th percentiles of the respective row variable, respectively. The last two rows report the willingness 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.

Figure A.3: Experimental Preference Estimates

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



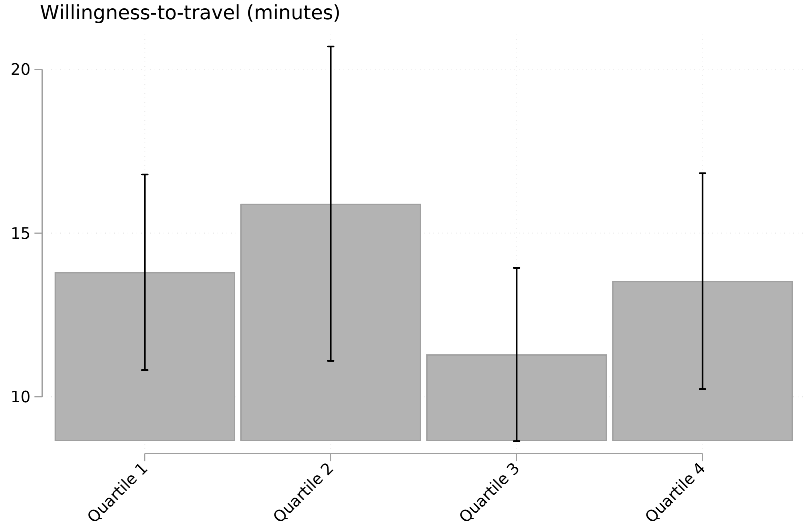
(b) Increase in achievement rate necessary to switch to remote



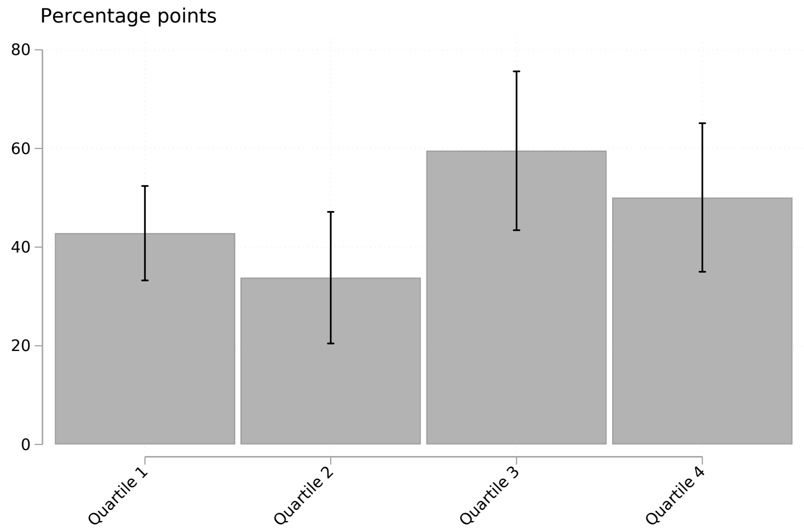
Notes: This figure reports willingness to travel estimates for achievement in Panel (a) and the estimated achievement necessary to make families indifferent between in-person and remote learning in Panel (b). Preference estimates are from a rank-ordered logit model relating indirect utilities of hypothetical 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 corresponds to estimates for the complete sample with hypothetical choice responses. The next three bars estimate preferences separately for students with different achievement levels based on their ELA and Math scores. The last five bars correspond to estimates for students with different demographic characteristics. Standard errors are robust and clustered at the respondent level.

Figure A.4: Experimental Preference Heterogeneity by Baseline Bullying

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



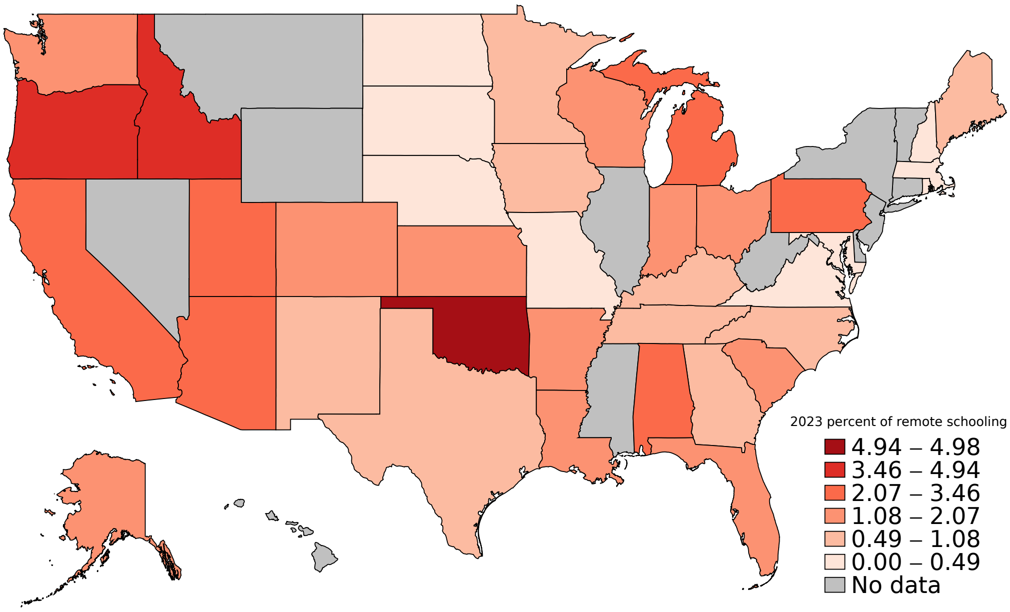
(b) Increase in achievement rate necessary to switch to remote



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 of students based on their baseline bullying quartile. For example, the “Quartile 1” bar in both panels corresponds to estimates for the subset of students in the bottom quartile of the bullying index defined in Campos (2023). A positive value of the index indicates better bullying-related outcomes. Standard errors are robust and clustered at the respondent level.

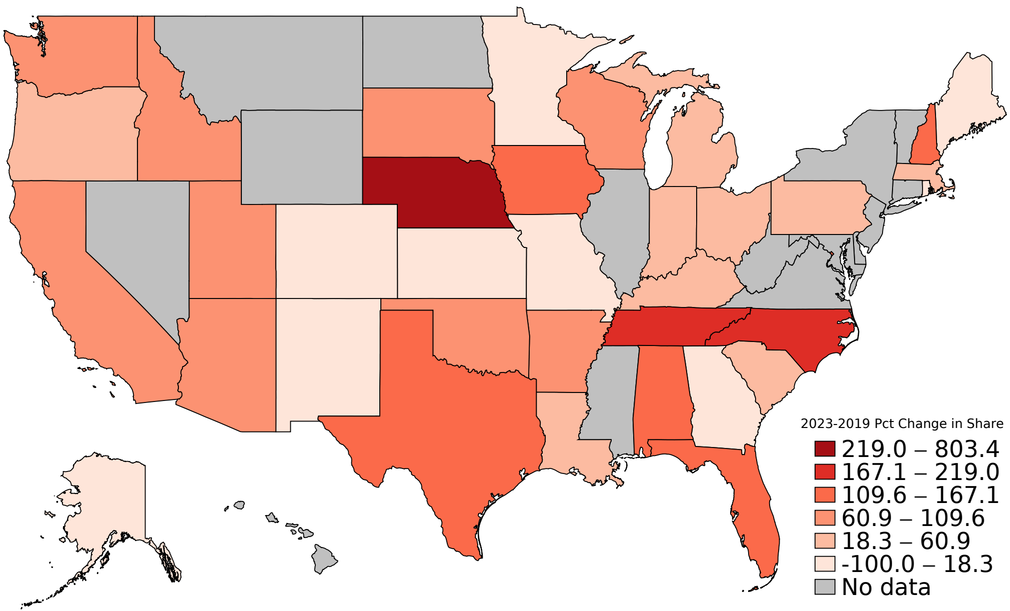
B Remote-Learning National Trends

Figure B.1: Remote Schooling Enrollment Shares by State, NCES 2023



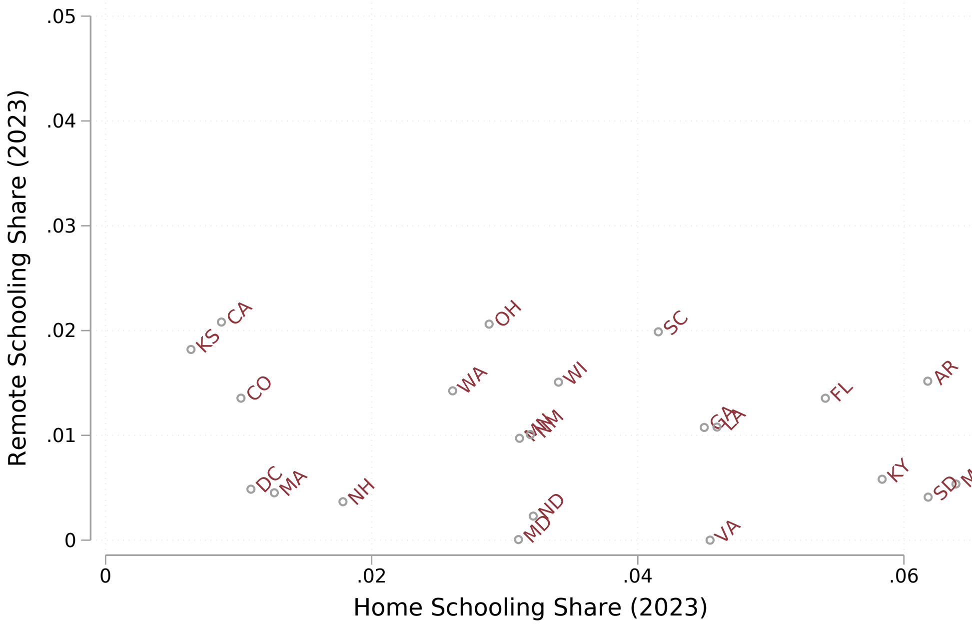
Notes: This figure reports exclusively virtual enrollment shares by state reported in the Common Core data provided by the National Center for Education Statistics (NCES). The cuts correspond to the 25th, 50th, 75th, 90th, and 95th percentiles of the state-level distribution.

Figure B.2: Remote Schooling Enrollment Percentage Change by State, NCES 2019-2023



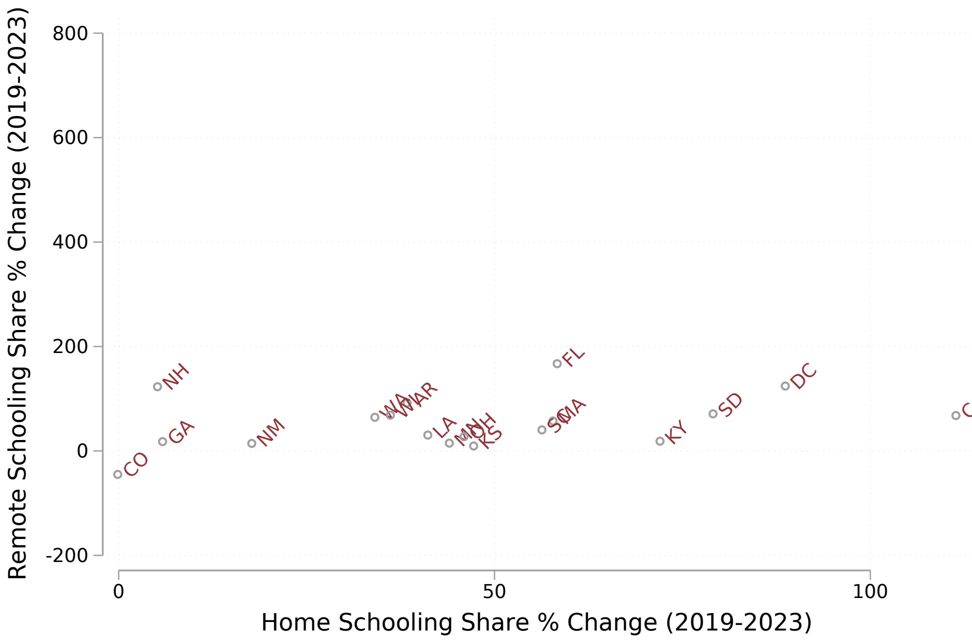
Notes: This figure reports 2019-2023 percent changes in exclusively virtual enrollment shares by state reported in the Common Core data provided by the National Center for Education Statistics (NCES). The cuts correspond to the 25th, 50th, 75th, 90th, and 95th percentiles of the state-level distribution.

Figure B.3: Remote and Homeschooling Shares by State, NCES 2023



Notes: This figure reports the state-level bivariate relationship between remote and homeschooling shares. The remote enrollment share is reported in the Common Core data provided by the National Center for Education Statistics (NCES) and the homeschooling share is reported by the Washington Post. Observations are labeled with their state identifier.

Figure B.4: Remote and Homeschooling Percentage Change by State, NCES 2019-2023



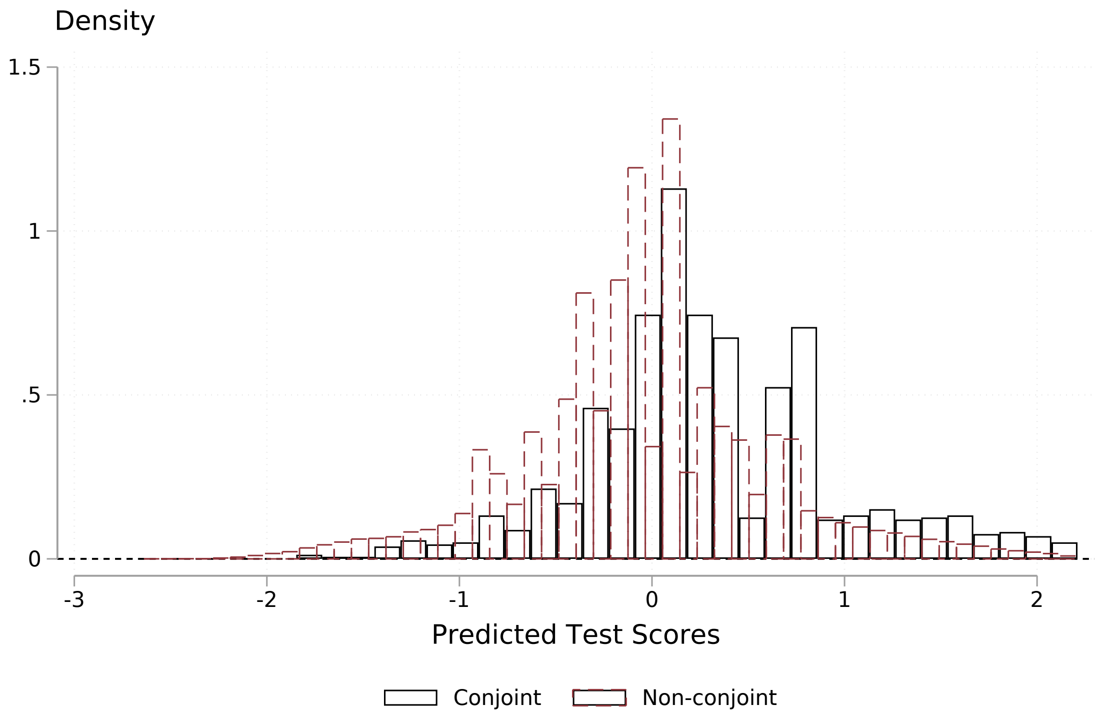
Notes: This figure reports the state-level bivariate relationship between 2019-2023 percent changes in remote and homeschooling shares. The remote enrollment share is reported in the Common Core data provided by the National Center for Education Statistics (NCES) and the homeschooling share is reported by the Washington Post. Observations are labeled with their state identifier.

C Validation Exercises and Robustness Checks

In this appendix section, we discuss two validation exercises. The first relates to the extrapolation procedure that is implicit in our main empirical results. The second validates our empirical estimates using lottery variation that is available for the various choice programs offered by LAUSD.

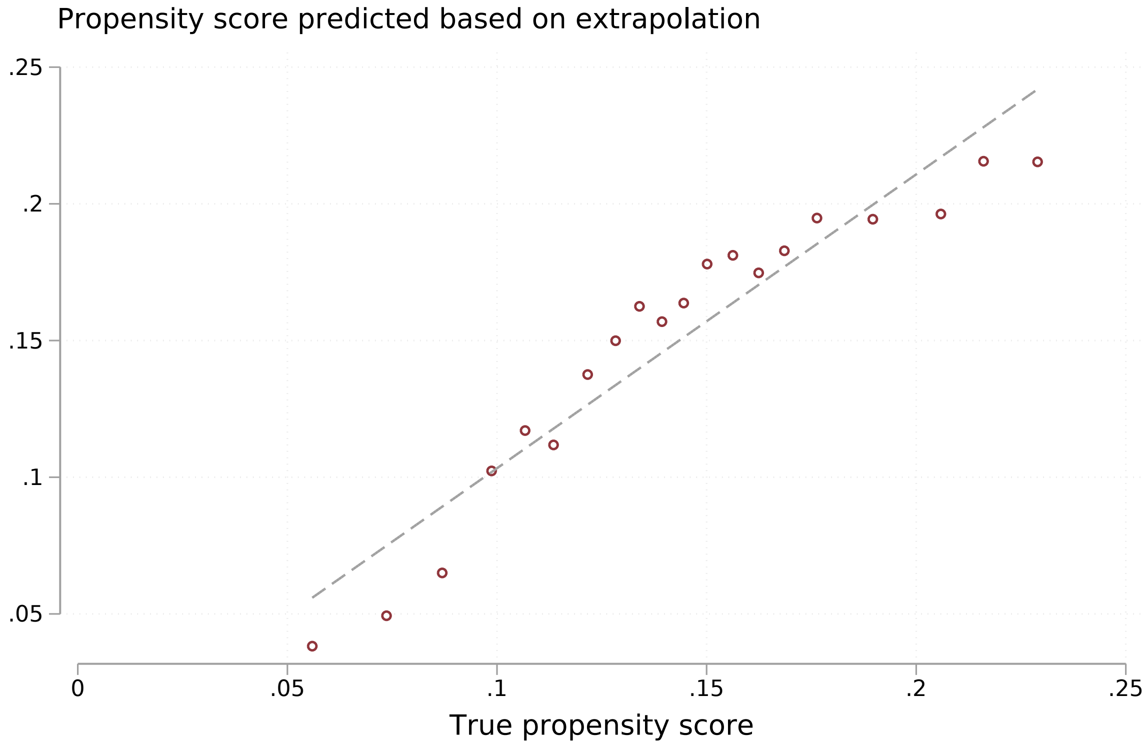
C.1 Overlap and Extrapolation

Figure C.1: 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 ELA 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 C.2: 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.

C.2 Lottery-Based Validation

Table C.1: Lottery-Based Tests for Bias in Remote Learning Estimates

	(1) Math	(2) ELA
Forecast Coefficient	1.03	0.67
	(0.20)	(0.22)
	[0.89]	[0.13]
First Stage F-statistic	16.60	24.67
Overidentification p-value	0.29	0.13
Observations	1,246	1,247

Notes: This table reports estimates of the lottery-based tests for bias in remote learning estimates discussed in Section 7.4. Specifically, it reports the parameter of interest ϕ_2 from Equation 8 for both Math and ELA 2022 achievement. A coefficient estimate of unity indicates that the estimated treatment effect heterogeneity from our preferred model is forecast unbiased. The table also reports F-stats from the first stage and p-values from a formal overidentification test. For both math and ELA, we fail to reject the hypothesis that the coefficients are forecast unbiased

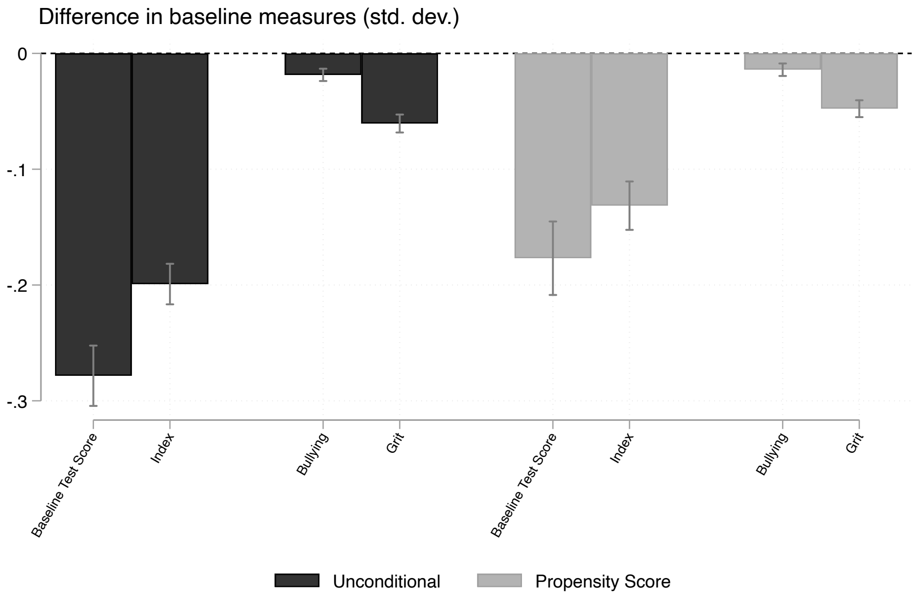
C.3 Robustness Checks

Table C.2: Estimated Parameters of the Linear and Quadratic Models

	Math		ELA	
Parameters	Linear (1)	Quadratic (2)	Linear (3)	Quadratic (4)
β	-.14	-.118	-.126	-.086
	(.018)	(.022)	(.018)	(.021)
θ_1	-.201	-.216	-.194	-.21
	(.021)	(.023)	(.02)	(.021)
ψ_1	.082	.027	.073	.041
	(.005)	(.009)	(.006)	(.009)
θ_2		.032		.035
		(.0073)		(.0076)
ψ_2		.0075		-.0116
		(.0034)		(.0037)
Obs. (N)	211,324	211,324	210,306	210,306

Notes: This table reports estimated parameters from our baseline linear specification according to Equation 6 (Column 1 and 3) and in an alternative quadratic specification of the form $Y_i = \alpha_c + \gamma' X_i + \beta D_i + \theta_1 P(\hat{v}_i) + \theta_2 P(\hat{v}_i)^2 + \psi_1 \left(P(\hat{v}_i) \times D_i\right) + \psi_2 \left(P(\hat{v}_i)^2 \times D_i\right) + \epsilon_i$ (Column 2 and 4). Our dependent variables are 2022 math achievement (Column 1 and 2) and 2022 ELA achievement (Column 3 and 4). Rows 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_1), and the selection on gains coefficient (ψ_1), respectively. Rows 4 and 5 report the quadratic term (θ_2) and its associated interactions (ψ_2), respectively. Propensity scores are in units equal to 10 percent for interpretation reasons. Standard errors are robust and clustered at the school level.

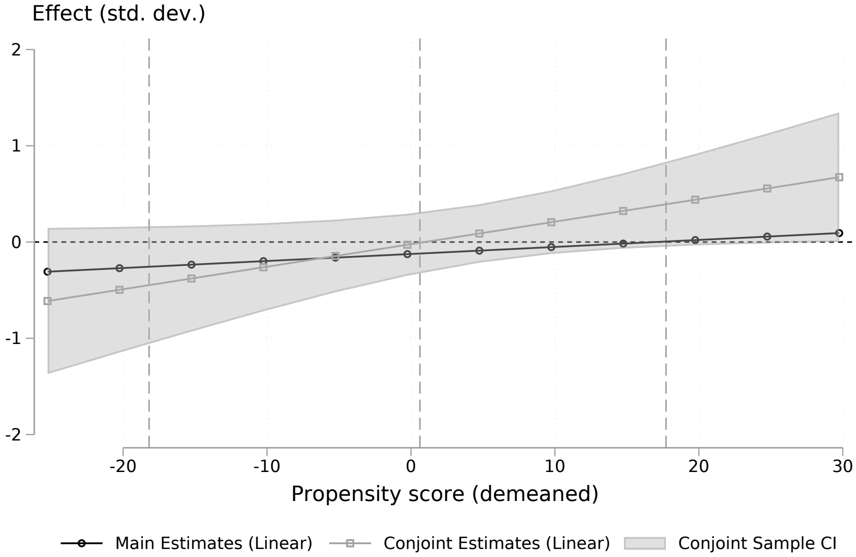
Figure C.3: Balance Results Using Observational Logit Model



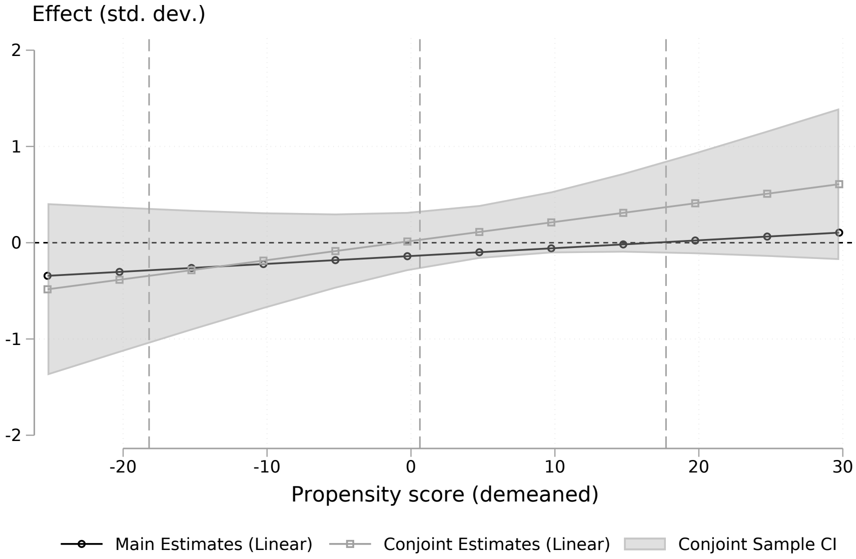
Notes: This figure reports the baseline balance of 2019 achievement (average of math and ELA) for both a conventional covariate-controlled and a propensity-controlled model derived from preferences estimated using observational data. The covariate-controlled model estimates correspond to regressions 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 C.4: Robustness to Estimates Using Only the Conjoint Sample

(a) ELA



(b) Math



Notes: This figure reports separate estimates of treatment effects for 12 bins of estimated propensity scores using our main sample (which extrapolates preferences from the conjoint survey) and the conjoint sample. The confidence interval (shaded region) is for the estimates using only the conjoint sample. Panel (a) reports treatment effects on 2022 ELA and Panel (b) reports treatment effects on 2022 Math. Standard errors are robust and clustered at the school level.

Table C.3: Effects of Remote Learning on Cognitive and Non-Cognitive Outcomes

	(1) Main Effect (β)	(2) Selection on Levels (θ)	(3) Selection on Gains (ψ)
Panel A: Non-linear Quality Preferences			
ELA	-0.131	-0.142	0.051
	(0.017)	(0.015)	(0.005)
Math	-0.145	-0.15	0.059
	(0.017)	(0.016)	(0.005)
No Bullying Index 2022	0.186	-0.024	-0.021
	(0.007)	(0.005)	(0.002)
Grit Index 2022	-0.036	-0.019	0.033
	(0.009)	(0.006)	(0.002)
Panel B: Non-linear Distance Costs			
ELA	-0.109	-0.188	0.056
	(0.019)	(0.019)	(0.005)
Math	-0.127	-0.195	0.066
	(0.019)	(0.02)	(0.004)
No Bullying Index 2022	0.158	-0.029	-0.002
	(0.008)	(0.006)	(0.002)
Grit Index 2022	0.015	-0.025	-0.001
	(0.009)	(0.008)	(0.002)
Panel C: Non-linear Quality Preferences and Distance Costs			
ELA	-0.118	-0.141	0.04
	(0.018)	(0.015)	(0.005)
Math	-0.136	-0.148	0.052
	(0.018)	(0.016)	(0.004)
No Bullying Index 2022	0.174	-0.023	-0.012
	(0.007)	(0.005)	(0.002)
Grit Index 2022	-0.01	-0.02	0.014
	(0.009)	(0.006)	(0.002)

Notes: This table reports estimates of the effects on cognitive and noncognitive outcomes based on versions of the model specified in Equation 6. Each panel reports estimates from models that differ in the underlying model of preferences used to construct propensity scores. Panel (a) provides results from a model that allows for interaction between preferences for academic quality and remote learning. Panel (b) provides results from a model with non-linear (quadratic) distance costs, and Panel (c) provides results from a model that allows for both non-linear preferences for distance costs and interaction between preferences for academic quality and remote learning. 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.

C.4 Additional Results

Table C.4: Effects of Remote Learning on 2023 Outcomes

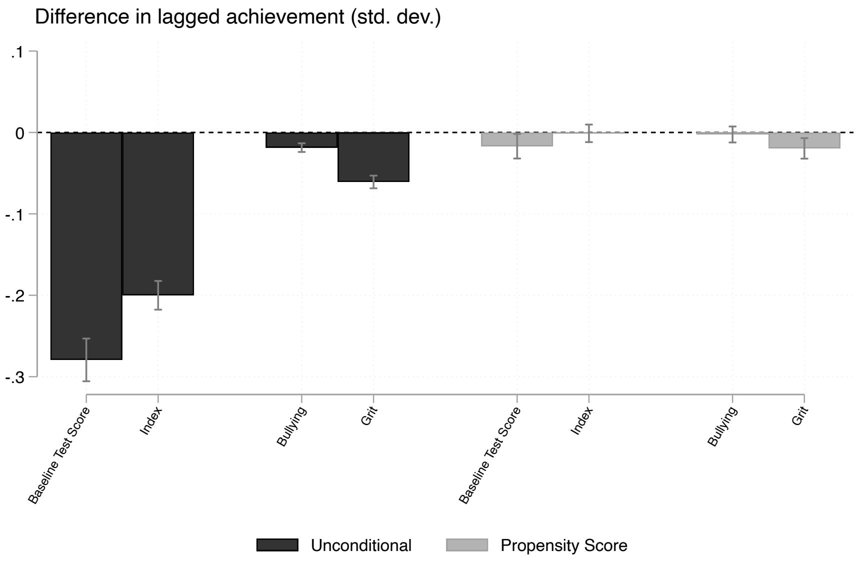
	(1) Main Effect (β)	(2) Selection on Levels (θ)	(3) Selection on Gains (ψ)
Panel A: Cognitive Outcomes			
ELA 2023 ($N = 164,041$)	-0.026	-0.203	0.063
	(0.017)	(0.019)	(0.006)
Math 2023 ($N = 163,883$)	-0.119	-0.204	0.074
	(0.017)	(0.021)	(0.005)
Panel B: Non-Cognitive Outcomes			
No Bullying Index 2023 ($N = 223,213$)	0.055	-0.017	0.008
	(0.005)	(0.006)	(0.002)
No Physical Bullying 2023 ($N = 223,146$)	0.104	-0.019	0.007
	(0.006)	(0.007)	(0.002)
No Online Bullying 2023 ($N = 217,893$)	0.023	-0.017	0.020
	(0.006)	(0.006)	(0.003)
Grit Index 2023 ($N = 225,383$)	-0.029	-0.021	0.021
	(0.005)	(0.005)	(0.002)

Notes: This table reports estimates of the effects on cognitive and noncognitive outcomes as observed in the school year 2023. Estimates are based on versions of the model specified in Equation 6. The remote enrollment treatment indicator is based on 2022 enrollment, as elsewhere in the paper. Panel (a) provides results on ELA and math achievement, and Panel (b) provides results on bullying-related outcomes and a grit index measured in 2023. Index variables are normalized such that larger values denote better outcomes. 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.

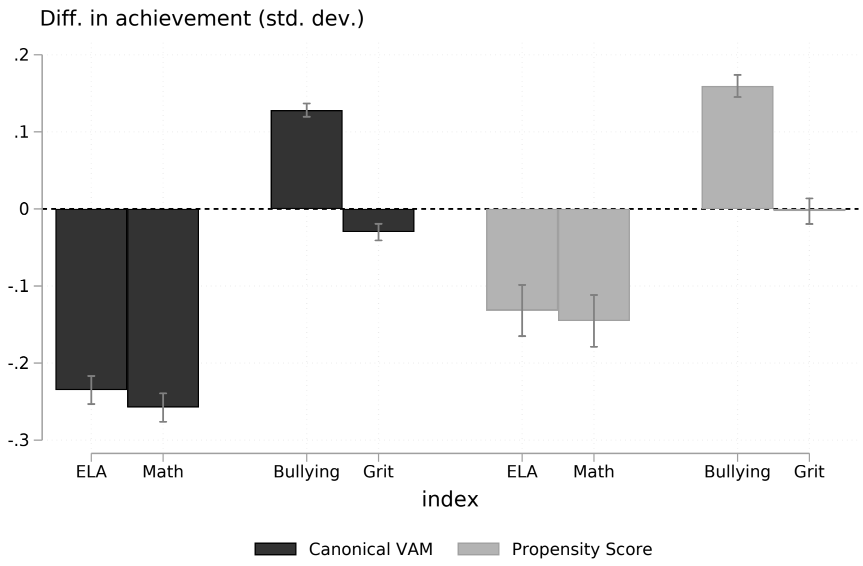
D Bootstrapped Estimates

Figure D.1: Baseline Balance and the Average Effects of Remote Learning (Bootstrap Version)

(a) Student baseline characteristics

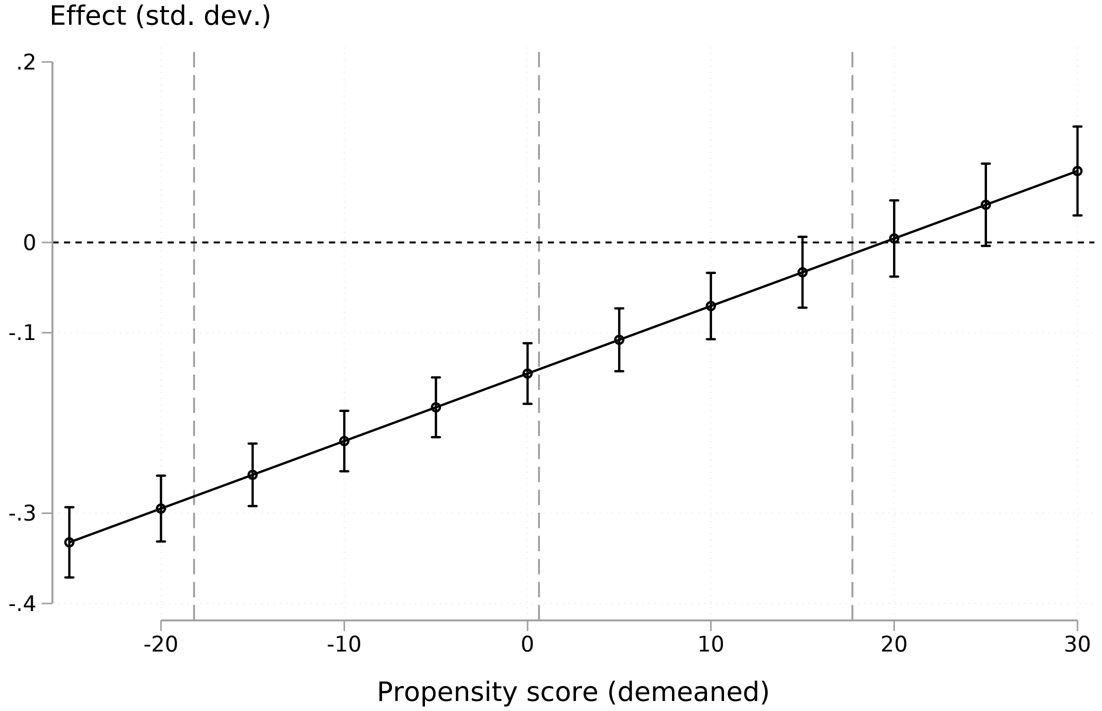


(b) Average effects on post-pandemic (2022) ELA and math achievement



Notes: This figure reports estimates similar to those in Figure 4 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 of utility weight estimates for each covariate cell 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 D.2: Estimated Match Effects on Post-Pandemic Math Achievement (Bootstrap Version)



Notes: This figure reports estimates similar to those in Figure 5 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 of utility weight estimates for each covariate cell 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.

Table D.1: Effects of Remote Learning (Bootstrap Version)

	(1) Main Effect (β)	(2) Selection on Levels (θ)	(3) Selection on Gains (ψ)
Panel A: Cognitive Outcomes			
ELA ($N = 210,306$)	-0.132	-0.182	0.068
	(0.017)	(0.019)	(0.006)
Math($N = 211,324$)	-0.145	-0.189	0.075
	(0.017)	(0.020)	(0.005)
Panel B: Non-Cognitive Outcomes			
No Bullying Index ($N = 181,723$)	0.159	-0.028	-0.005
	(0.007)	(0.006)	(0.002)
No Physical Bullying ($N = 181,632$)	0.298	-0.030	-0.023
	(0.008)	(0.007)	(0.002)
No Online Bullying ($N = 177,249$)	0.147	-0.019	0.014
	(0.008)	(0.007)	(0.003)
Grit Index ($N = 184,041$)	-0.003	-0.023	0.012
	(0.008)	(0.008)	(0.003)

Notes: This table reports estimates similar to those in Table 2 but instead provides estimates and standard errors obtained through a bootstrapping procedure. All outcomes are measured in the post-pandemic school year 2022. To account for estimation error in the propensity score estimation, we use a parametric bootstrap. We draw 250 sets of utility weight estimates for each covariate cell 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.

E 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.[24] In addition, we correlate the zip-code-level preferences with measures of local area case counts and deaths due to COVID-19.[25]

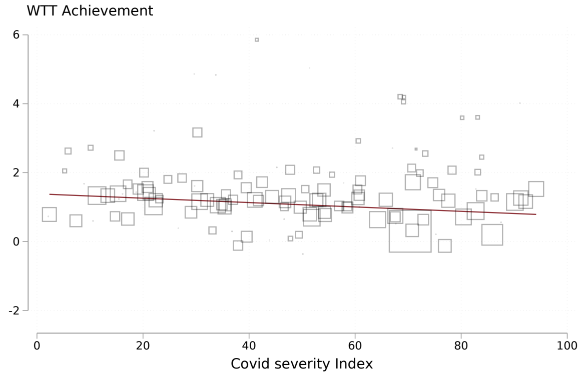
Appendix Figure E.1, Panels (a), (b), and (c) provide scatterplots of each zip code's estimated willingness to travel for academic quality and the three COVID-19 index measures. Each point's size is proportional to the number of respondents used to estimate preference parameters. To supplement these results, Panels (a) and (b) of Appendix Figure E.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 E.3 and Figure E.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.

[24]These 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.

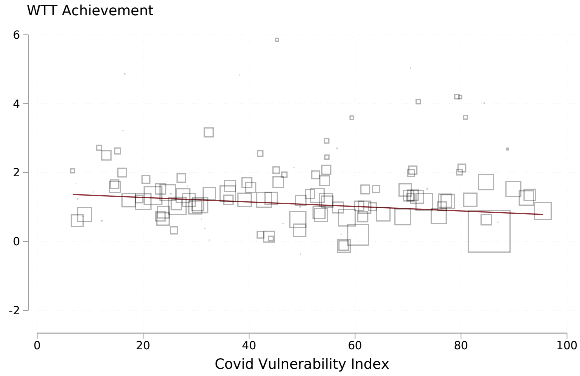
[25]The data used for these analyses were downloaded from http://publichealth.lacounty.gov/media/coronavirus/data.

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

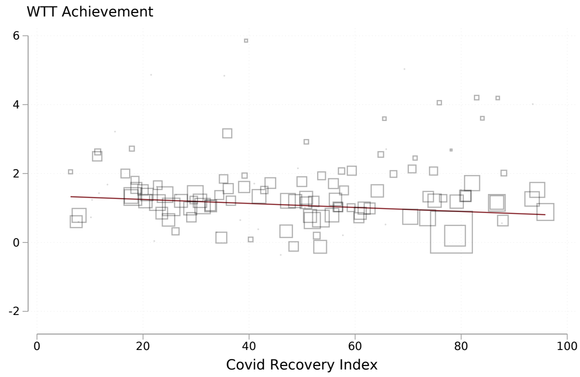
(a) Covid risk index



(b) Covid vulnerability index



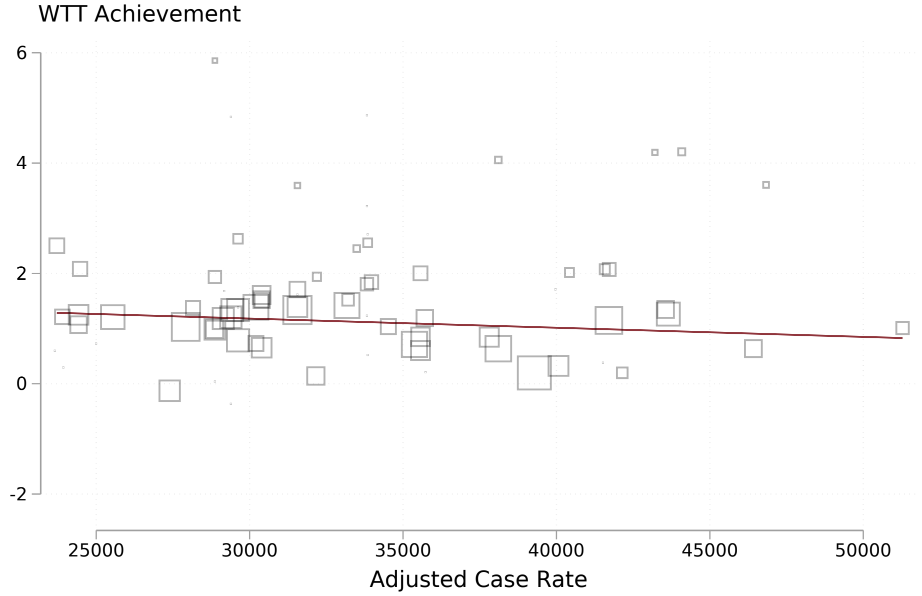
(c) Covid recovery need index



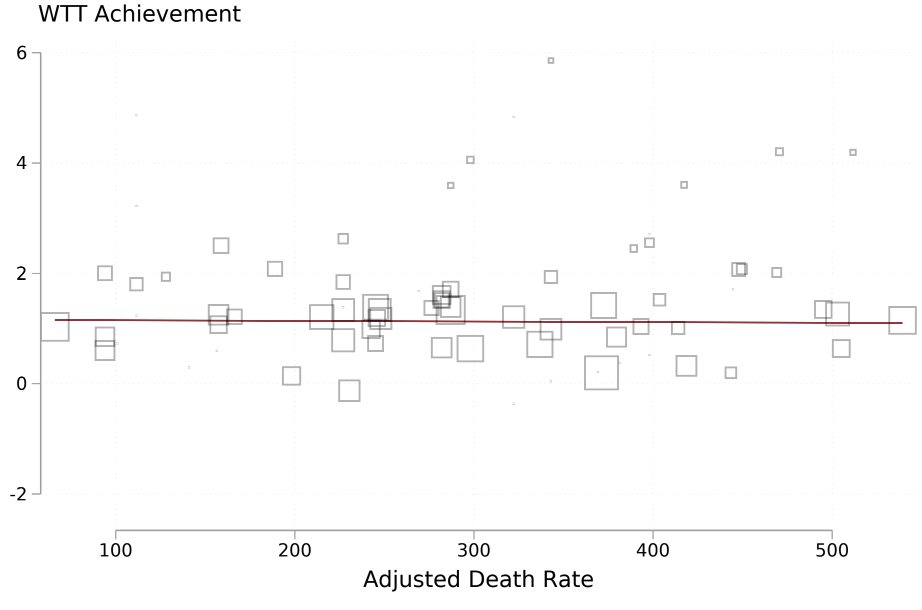
Notes: This figure presents scatterplots of zip-code-level mean willingness to travel for academic achievement (y-axis) and three measures from the COVID-19 Vulnerability and Recovery Index produced by Los Angeles County (x-axis). Panels (a), (b), and (c) present indices for the risk, severity, and recovery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

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

(a) Covid cases



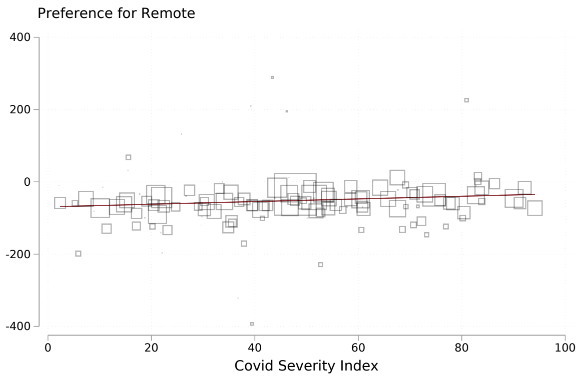
(b) Covid deaths



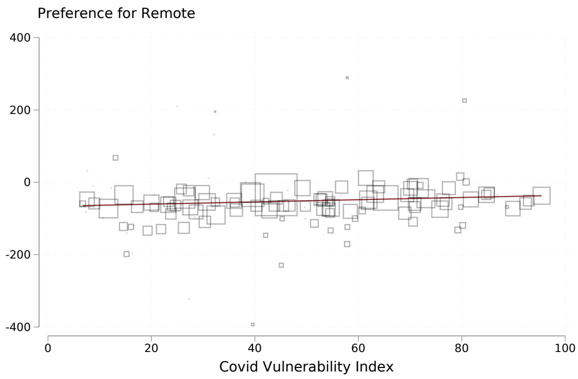
Notes: This figure presents scatterplots of zip-code-level mean willingness to travel for academic achievement (y-axis) and two measures of the severity of the COVID-19 pandemic on health outcomes in an area (x-axis). Panels (a) and (b) measure Covid health impact severity using case count and death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

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

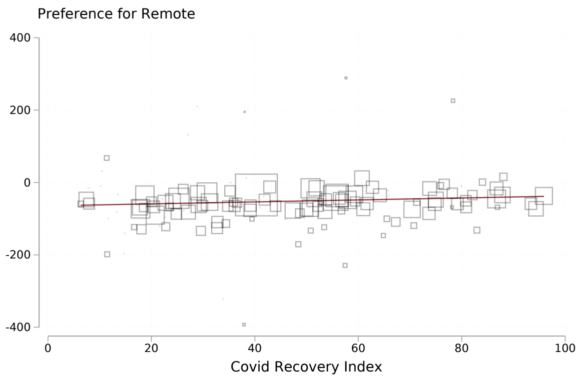
(a) Covid risk index



(b) Covid vulnerability index

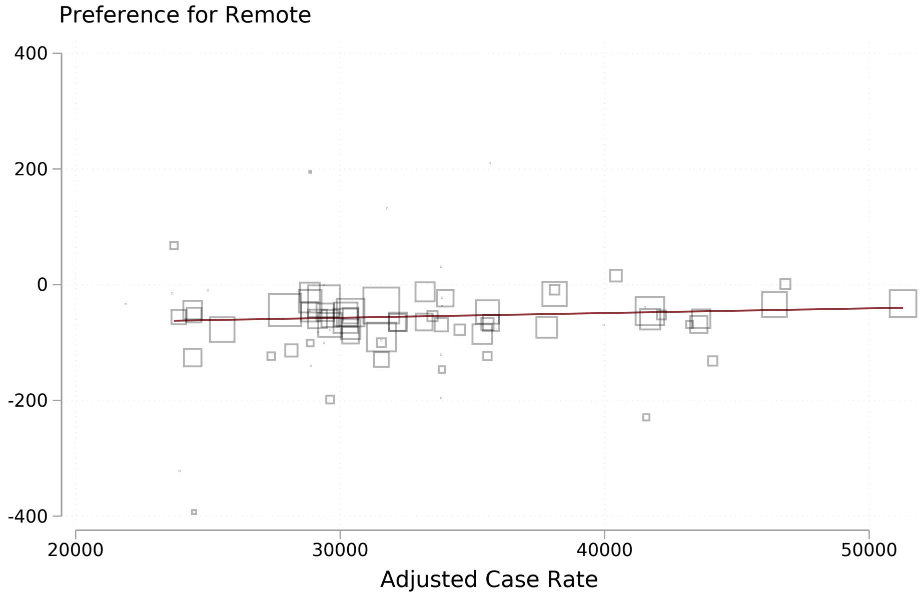


(c) Covid recovery need index

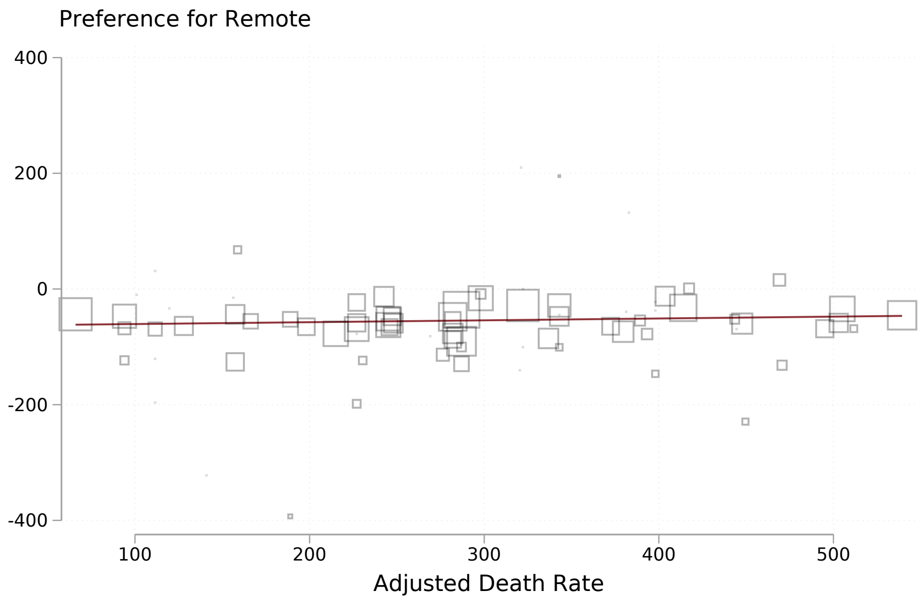


Notes: This figure presents scatterplots of zip-code-level measures of mean preferences for remote learning (y-axis) and three measures from the COVID-19 Vulnerability and Recovery Index produced by Los Angeles County (x-axis). Panels (a), (b), and (c) present indices for the risk, severity, and recovery 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 E.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.