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ABSTRACT

The COVID-19 pandemic drew new attention to the role of school boards in the U.S. In this paper, we examine school districts' choices of learning modality—whether and when to offer in-person, virtual, or hybrid instruction—over the course of the 2020-21 pandemic school year. The analysis takes advantage of granular weekly data on learning mode and COVID-19 cases for Ohio school districts. We show that districts respond on the margin to health risks: all else equal, a marginal increase in new cases reduces the probability that a district offers in-person instruction the next week. Moreover, this negative response is magnified when the district was in-person the prior week and attenuates in magnitude over the school year. These findings are consistent with districts learning from experience about the effect of in-person learning on disease transmission in schools. We also find evidence that districts are influenced by the decisions of their peers.

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1 Introduction

The COVID-19 pandemic was a life-changing event for adults and children alike. School closures in spring 2020 recast millions of homes as provisional classrooms and millions of parents and other guardians as part-time teachers. The 2020-21 school year confronted school leaders with extraordinary challenges in determining how to provide safe and effective learning for students amidst the uncertainties created by the on-going pandemic. How school districts responded frequently ignited controversy. Attendance at school board meetings soared as angry parents, students, and teachers sought to express their views. Parent frustrations related to schooling during 2020-21 were a factor in electoral upsets in Virginia and New Jersey and voter dissatisfaction even led to recall elections in San Francisco (Kamenetz, 2021; Fuller, 2022).

This paper contributes new evidence to the on-going debate concerning school learning mode decisions during the 2020-21 COVID-19 pandemic school year. This debate has centered on the question of whether school districts acted in the interest of social welfare, focusing principally on the role of partisanship: Were school districts that never returned to fully in-person learning—typically, "blue" urban districts—unconscionably cautious, especially given emerging evidence of learning losses (Jack et al., 2022).[1] Did districts that quickly returned to and remained in-person—generally "red" districts—contribute to community disease transmission and excess mortality?[2] A growing recent literature examines the correlates of school district re-opening plans, including partisanship and local COVID-19 severity (Singer, 2022).

There are at least two challenges to drawing normative assessments of school district learning mode decisions, however. As explained more below, a district's choice of learning mode will depend on both (a) the value it places on student learning relative to the cost of disease (i.e., the district's "preferences") and (b) its beliefs regarding the health consequences of in-person instruction. The first challenge is that different preferences (as defined above) can generate behavioral

[1]National (U.S. Department of Education, 2022) and state-level (Kogan & Lavertu, 2021) student test score data indicate score declines relative to prior cohorts. At the same time, Bacher-Hicks et al. (2022) present evidence that the disruption of in-person learning reduced school bullying. Jack et al. (2022) connect pass rates with learning mode differences across districts, providing evidence that outcomes in fully in-person districts declined relatively less.

[2]The evidence on the relationship between school re-openings during 2020-21 and disease spread is mixed. Courtemanche et al. (2021) found that school re-openings in Texas led to 12% more cases and 17% more fatalities. Goldhaber et al. (2022) only found a lasting relationship between in-person instruction and case rates in areas with moderate to high levels of preexisting COVID cases. Isphording et al. (2021) and Harris et al. (2021) found no conclusive relationship between school re-openings, case rates, and hospitalizations.

differences even if districts agree on how in-person learning contributes to disease spread. From this perspective, assessing district decisions involves the difficult problem of adjudicating whose preferences maximize the social welfare. The second challenge is that, in the cross-section, it is not possible to separate beliefs from preferences—either could rationalize the same pattern of behavior. It may be, for example, that "bluer" districts believed that re-opening would lead to catastrophic disease transmission and not that they undervalued in-person learning (and the inverse for "redder" districts).

Faced with these challenges, our contribution lies in developing and evaluating new criteria for assessing the quality of school district learning mode decisions. These criteria correspond to predictions of a model of how school districts choose learning mode as a function of their beliefs. In fall 2020, districts faced a great deal of uncertainty regarding both the future path of the pandemic and the risks associated with in-person learning. District leaders were forced to weigh the perceived costs and benefits of in-person instruction on a continual basis in light of changing health conditions. In our model, a district chooses learning mode each period based on its expectation for the health consequences of doing so. This expectation is formed using knowledge of the current state of the pandemic and the district's current belief about how in-person learning affects disease transmission. This belief is updated using new information generated by experience—the district's own as well as, in principle, those of peer districts—opening schools and observing subsequent disease spread.

The model yields several testable predictions regarding the *longitudinal* relationship between COVID-19 disease transmission and a district's subsequent choice of learning mode. The predictions are robust to heterogeneity across districts in preferences for in-person learning and aim to identify the role of beliefs about the disease risks of re-opening on district decision-making. We evaluate these predictions using a panel dataset of Ohio school districts' learning modes (i.e., in-person, hybrid or virtual) that we link, at the weekly level, with data on reported cases among students and staff. Because Ohio allowed districts tremendous flexibility in designing back-to-school plans for 2020-21, this setting is well-suited to analyzing the determinants of districts' decisions. Additionally, the diversity of Ohio's 600+ districts—spanning from small and rural to large and urban—presents wide variation in choice patterns and is representative of the country as a whole.

We begin our empirical analysis by examining variation across Ohio school districts in learning mode patterns over the school year. Specifically, we group school districts into six clusters based on similarity in their sequences of learning mode choices. One group of 127 districts were always in-person (five days a week); a second group of 79 districts never returned to in-person instruction during 2020-21. We then use data reduction techniques to identify four additional clusters that are broadly characterized by when during 2020-21 they permanently switched to in-person instruction (ranging from the end of August to early May). Notably, over 30% of the districts in these four clusters changed their learning mode 4 or more times during the school year.

We then examine what characteristics of school districts predict their learning mode patterns. We do this by estimating models of cluster membership as a function of district characteristics, such as size, urbanicity, and partisanship. We find that rural and Republican-leaning districts were more likely to be always in-person, whereas urban, more Democratic districts were more likely to spend much of the year virtual or hybrid. This finding is consistent with prior research on school re-openings (Hartney and Finger, 2020; DeAngelis and Makridis, 2021). Overall, though, district demographics and politics explain only a modest share of the overall variation. For the two-thirds of districts neither always nor never in-person, the only significant predictor of the week they returned is whether the district was coded "red" (the second highest level) or above on the Ohio COVID-19 alert scale in August 2020. This suggests that, rather than demographic and political characteristics, the course of the pandemic may have driven district choices over time.

To understand the sources of variation in district choices over time, we estimate a series of panel data models. For this analysis, we restrict the sample to the majority of Ohio districts that were neither always or never in-person. We find that the data support several predictions of the model. First, we find that a district's own experience with COVID-19 affects their subsequent learning mode decisions. Specifically, we find that an increase in 1 case per 1,000 students and staff while the district is in-person is associated with a 2.3 percentage point (3.5%) reduction in the likelihood that the district offers in-person instruction during the following week. Consistent with a key model prediction, this negative response is greater than the response to an equivalent increase in lagged cases arising from when the district was not in-person. While lagged county-level case rates (or other measures of COVID-19 risk) are negatively associated with in-person instruction as well, the data indicate that districts are responding to school-related cases in the district specifically.

Second, we find that the negative relationship between pandemic severity and the likelihood of in-person instruction attenuates as the school year progresses. For example, at the very beginning of the school year each additional COVID-19 case is associated with a 6.4 percentage point lower probability of returning in-person in the following week. By late March, an additional case is associated with a non-significant 2 percentage point increase in the likelihood of returning in-person. This pattern, which is consistent with districts learning from their own experience about the disease risks of in-person learning, is only partially explained by the rise in vaccination rates, which themselves are positively associated with in-person instruction.

Finally, we find that districts respond to the learning mode decisions of peer school districts. For example, as the fraction of peer districts within 20 miles increases by 10 percentage points—roughly equivalent to 2 additional peers—the likelihood of teaching in-person increases by 0.7 percentage points. This effect is consistent with peer choices exerting "pressure" on school districts via parents, state policymakers, media, and other stakeholders. At the same time, we do not find that districts respond to an increase in the lagged number of cases in in-person peer districts.

Together, these results suggest that these districts' learning mode choices are consistent in several respects with rational decision-making under uncertainty. The first is that school districts respond on the margin to changes in the risk of disease transmission in schools. Moreover, under the assumption that increases in cases correspond to higher perceptions of risk, districts lowering the likelihood of offering in-person instruction implies concern with student and staff health. The second respect concerns the use of new information about the risks of in-person learning. Our results show that the elicitation of this information modifies districts' subsequent learning choices and in the direction that is implied by a Bayesian learning process. These findings suggest that uncertainty is an important driver for district decisions and that understanding the sources of information frictions is key to developing policy recommendations.

Our paper makes an important contribution to the recent literature on school re-opening decisions during the COVID-19 pandemic. Prior work has mainly focused on districts' *initial choice* of learning mode. This research generally indicates that political partisanship is the strongest predictor of school district choices (with a secondary role played by teachers' unions), whereas health-related factors play a small role (DeAngelis and Makridis, 2021; Grossmann et al., 2021; Hartney and Finger, 2020; Marianno et al., 2022).[3] Our paper is distinct in examining the *within-*

[3] Houston and Steinberg (2022), however, find that county case counts measured in October 2020 are negatively associated with the percent of students learning in-person.

year changes in learning mode. Using this variation, we find that COVID-related health risks are indeed important for explaining learning mode choices.[4] Our results are also the first to quantify the importance of heterogeneity over time in the relationship between district learning mode and pandemic severity. Lastly, we find that peer districts' decisions matter, similar to Acton et al. (2022) who present evidence that the re-opening decisions of colleges were influenced by the behavior of peers.

Beyond the COVID-19 pandemic, our study makes a valuable contribution to the understanding of school boards more generally. Composed of nearly 90,000 lay members elected in non-partisan contests, school boards oversee the education of 50 million children and have broad responsibilities for district governance that include the allocation of $600 billion in expenditures. Boards not only lead the development and implementation of district policies, but also hire district superintendents and play a role in supervising the six million public school employees (Dervarics and O'Brien, 2019; Hess and Meeks, 2010). Despite their outsized role in the education process, we lack rigorous, quantitative evidence assessing school board quality because little, if any, data exist that track school board policy decisions. Faced with this limitation, several recent studies establish that school boards matter in education production by *inferring* the impact of district choices from marginal changes in school board composition (e.g., Macartney and Singleton, 2018; Shi and Singleton, 2022). For example, Fischer (2022) shows that the allocation of spending towards Hispanic-majority schools rises following the quasi-random election of an Hispanic board member.[5] Research outside of economics relies on survey or qualitative data to document correlations between measures of school board practices, e.g. emphasizing goal setting, embracing and monitoring data, and student outcomes (e.g. Togneri and Anderson, 2003; Duvall, 2005; Ford and Ihrke, 2016; Grissom, 2014).

In contrast, learning mode choices during 2020-21 represent high-profile and consequential school district policy decisions that can be directly analyzed. Given the relatively strong evidence of COVID-19 learning losses (Jack et al., 2022), the concerns about student mental health (Bacher-Hicks et al., 2022) and the mixed evidence on school re-openings and COVID spread (Courtemanche

[4]Some other studies have not necessarily used panel data, but have looked beyond the initial learning mode decision; for example, Houston and Steinberg (2022) examine the percent of students in-person over the entire 2020-21 school year and Parolin and Lee (2021) proxy for monthly school visits using SafeGraph cellphone data.

[5]The strategy of inferring decisions from the effect of marginal changes in organizational composition applies equally to other entities. For example, Beach et al. (2018) find that the quasi-random election of nonwhite city council members in California is associated with reductions in house price gaps between white and nonwhite neighborhoods.

et al., 2021, Goldhaber et al., 2022, Isphording et al., 2021, Harris et al., 2021), it is critical to better understand the quality of school district decision-making. Indeed, the focus on district decision-making is important given longstanding concerns that school boards are populated by non-experts, unaccountable to voters, beholden to teachers' unions, and becoming more partisan (e.g. Hartney, 2022; Hess and Leal, 2005; Holbein, 2016).[6]

2 Background and Data

Our analysis focuses on school districts in Ohio during the 2020-21 school year. Ohio provides a great context to study district decision-making because the state granted districts tremendous flexibility to develop and tailor re-opening plans to their local context. Districts were expected to be nimble and responsive to changing local health conditions and schools were expected to operate in various modes (in-person, hybrid, or virtual) at different times with little advance notice (Ohio Department of Education, 2020). Districts were required to follow state and local public health guidelines, which included a statewide mask mandate and recommendations for social distancing (maintaining a six foot distance from others) that were applicable for the general public and schools.

Ohio also presents an information rich environment. The state provided education leaders and the general public up-to-date information on the pandemic to help inform schooling decisions, which allows us to study how districts learn and react to new information. In early July, the state established the Ohio Public Health Advisory System (OPHAS) to provide local health departments and community leaders granular data on COVID-19 cases across the state, including the number of new cases and information on the local health care system, such as emergency room visits, hospital admissions, and intensive care unit bed occupancy (Ohio Governor's Office, 2020). When schools opened in the fall, staff and parents of students were required to notify schools if they or their child tested positive for COVID-19 (Himes, 2020). Schools had to report these cases to their local health department within 24 hours, which would then report them to the Ohio Department of Health. The Ohio Department of Education also collected the learning mode each district participated in each week. The state published all of these data publicly on an online dashboard on a weekly basis beginning on September 15th, 2020, and new cases among students and staff (by district) were

[6]Related, there is evidence of private returns from office: Billings et al. (2022) document that the election of a new board member causes the home values in their neighborhood to rise on average.

regularly reported by the local media. Thus, districts had a host of information in nearly real-time available on themselves and their peer districts to inform instructional mode decisions.

2.1 Data Sources

To examine district decisions regarding learning mode during the 2020-21 school year, we assembled data from various sources. We gather learning mode and COVID-19 student and staff case counts from the COVID-19 School Data Hub (CSDH)[7], county-level COVID-19 data from OPHAS, district characteristics from Common Core Data (CCD) and the Stanford Education Data Archive (Reardon et al., 2021), and partisanship data from Harvard's Dataverse (Voting and Election Science Team, 2018).

2.1.1 District Learning Mode and COVID-19 Case Counts

The CSDH consolidated publicly-available learning mode and COVID-19 case count data from the Ohio Department of Education and Ohio Department of Health. This includes information on whether a district was in-person, virtual, or hybrid, and COVID-19 cases reported to schools by parents/guardians and staff. A district is designated as in-person if all students had the option to attend class in-person 5 days of week and it is designated as virtual if all students only received remote education. Districts are labeled hybrid if they reported having a mix of in-person and remote education. This includes cases where a district had only particular grades in-person for the year (e.g., only grades K-3 are in-person and grades 4-5 are virtual) or if they had all grades in-person but asynchronously (e.g., grades K-3 are in-person Mondays and Tuesdays, grades 4-5 are in-person Thursdays and Fridays, and all grades are virtual on Wednesdays).

Districts self-reported learning mode and COVID-19 case count information on a weekly basis. However, the beginning and end dates of weekly reported cases did not perfectly align with those of the learning mode data. For example, learning mode data covered the week of 10/15-10/21, but the COVID-19 case count data covered 10/12-10/18 and 10/19-10/25. To align COVID-19 case counts and learning mode week, we first impute the week-level COVID-19 data to the daily level,

[7]The CSDH is an initiative spearheaded by Emily Oster to provide school leaders and researchers statewide data on school masking, learning modes, and COVID-19 case counts to inform education and public health decisions. We hand-collected 5 weeks of additional data from the beginning of the school year that were not originally present in the CSDH from the COVID-19 School Response Dashboard.

where each day contains the "expected value" number of cases that day. Then, we aggregate these daily case counts to the learning mode week level. This provides a standardized and consistently-measured dataset linking district learning mode with new cases by week.

2.1.2 County-level COVID-19 Risk Factors

We gather data on county-level factors that might influence districts' decisions from OPHAS. This includes seven indicators representing the COVID-19 risk level in the county based on the following: 1) number of new cases per capita, 2) sustained increase in new cases, 3) proportion of cases not in a congregate setting, 4) sustained increase in emergency department visits, 5) sustained increase in outpatient visits, 6) sustained increase in hospital admissions, and 7) intensive care unit bed occupancy. OPHAS coded counties meeting at most 1 indicator as yellow (lowest-risk), counties meeting 2-3 indicators as orange, counties meeting 4-5 indicators as red, and counties meeting 6-7 indicators as purple (highest-risk). In our analyses we include these color indicators, total score, and number of new cases per capita (over the prior two weeks). For some analyses we also include the number of first vaccination doses per 100 county residents, which we gather from the Ohio Department of Health.

2.1.3 District Characteristics

Our dataset also includes several time-invariant district characteristics that may play a role in district decisions. From the 2019-20 CCD we gather information on district urbanicity, student enrollment, student demographics (e.g., race and percent receiving free or reduced-price lunch), and total staff. We include measures of student achievement and district socioeconomic characteristics (e.g., family education level and poverty rate) from the 2018 SEDA data. We are also interested in how district decisions vary along political lines, so we include Republican vote share from the 2016 presidential election at the district level by aggregating precinct level data from Harvard's Dataverse, where votes are weighed by the geographic area each precinct covers in a district.[8]

[8] There were three districts that did not have precinct-level vote share from the 2016 election, so we use the 2020 vote share for these districts. The correlation between the 2016 and 2020 vote share is high ($\rho = 0.93$).

2.1.4 Sample

The high-frequency nature of this data allows us to create a balanced 37-week panel for 608 public school districts in Ohio over the course of the 2020-21 academic year.[9] We make a small number of data imputations and sample restrictions. 79 districts did not report learning mode data during the first two weeks of the school year, so we impute these weeks with the earliest learning mode available for these districts. We exclude from our data the week of Thanksgiving break and two weeks during winter holidays because districts did not report learning mode or COVID-19 case counts. We also exclude from our analysis ten districts without learning mode data and one district that had fewer than ten students.

In Figure 1, we show how learning modes and student and staff COVID-19 cases evolved in Ohio over the 2020-21 academic year. Roughly 60% of districts began the school year in-person in-person, with 25% and 15% starting hybrid and virtual, respectively. The number of student and staff cases rose in November, at which time we see that districts shifted to virtual instruction. Beginning in January 2021, cases started declining and districts started shifting to in-person instruction. By March 1, 2021, when districts were ordered to return to in-person or hybrid, nearly 70% of districts were back in-person.[10]

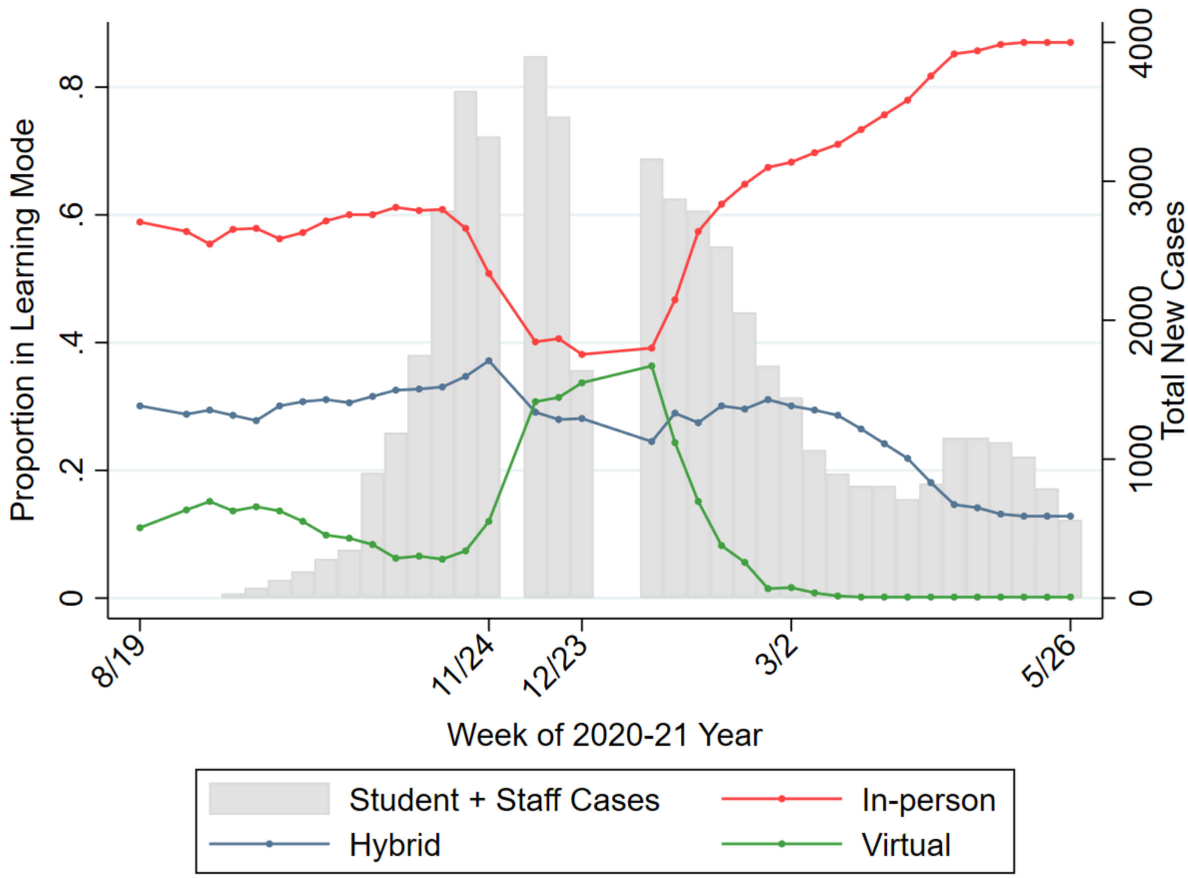
While cases and learning mode followed this general pattern statewide, there was substantial variation across districts. 127 districts (21%) were in-person the entire academic year and 79 districts (13%) almost never experienced full in-person instruction and ended the school year in virtual or hybrid learning. The remaining districts returned to in-person instruction at various points during the year, from late 2020 to the end of April 2021.

3 A Model of School District Learning Mode Choice

In this section, we sketch a model of how a school district decides whether to offer in-person instruction or not. This model provides a framework for the empirical analysis of Ohio school districts' week-by-week choices during the 2020-21 school year.

[9] Week refers to the week of the academic school year, excluding time off for holidays.

[10] Governor DeWine announced on Twitter that he would prioritize school personnel to receive vaccinations conditional on district superintendents signing a pledge that they agree to go back to full in-person or hybrid learning by March 1st. All superintendents signed this pledge, and all but 7 school districts returned to in-person or hybrid by the beginning of March. (DeWine, 2021).



Note: This figure shows the aggregate number of student and staff COVID-19 cases in Ohio during the 2020-21 school year (gray bars) along with the proportion of districts ($N = 608$) that were in-person, hybrid, or virtual.

Figure 1. Learning Mode and COVID-19 Cases by Week, 2020-21

3.1 Overview and Setup

In the model, a school district rationally weighs the benefits of in-person learning against expected costs incurred from negative health risks for staff and students each week of the school year. These risks are summarized by new reported cases, which the district aims to minimize. While the model in principle applies to community and school-related cases equally, our empirical analysis focuses on the role of cases among students and staff specifically because they are likely more salient to district leaders.

The model incorporates two sources of uncertainty facing the district. First, the district is not certain about how the pandemic will evolve and forecasts new cases for the upcoming week when it makes its choice of learning mode. Secular changes in the severity of the pandemic over the school year are thus one reason why a district's choice of learning mode may change over time. Second, we assume that the district is not certain about the *causal effect* of in-person learning on transmission of negative health outcomes. The district therefore uses information gleaned from its own experience—and, in principle, those of its peers—to update its belief about the relationship between in-person instruction and COVID-19 transmission, which it uses, in turn, to modify its subsequent choices.

Let district j's indirect utility from offering in-person instruction (relative to offering virtual or hybrid instruction) in period t be given by:

$$U_{jt} = \beta + \gamma \frac{1}{|\mathcal{P}_j|} \sum_{i \in \mathcal{P}_j} d_{it-1} - \alpha E[N_{jt} | N_{jt-1}, I_{jt}, d_{jt} = 1] \tag{1}$$

β represents a fixed effect for in-person instruction (relative to virtual or hybrid). We expect that β is positive, implying that the district would offer in-person in the absence of the pandemic (in recognition that in-person schooling produces higher quality instruction and more student learning).[11] $\alpha > 0$ represents the disutility of COVID cases. While not reflected in the notation, β and α are in principle heterogeneous across school districts due to varying voter and school board member preferences and/or institutional differences.[12]

At the time of its period t decision, the district forecasts new cases in the coming week assuming it offers in-person instruction ($d_{jt} = 1$), taking into account its case count in the prior week (N_{jt-1}), and its information set, I_{jt}, which underlies its current belief about the effect of in-person learning on new cases. On that basis, the district chooses in-person or not.[13] Changes *over time* in districts' choices in the model are thus driven by changes in N_{jt-1}, which captures secular change in pandemic severity, and changes in I_{jt}. The function also highlights two potential sources of differences in choices *across districts*: (1) heterogeneity in $\frac{\beta}{\alpha}$, the ratio of (perceived) benefits from in-person learning to the (perceived) cost per case; and (2) heterogeneity in I_{jt}, beliefs about the effect of in-person learning on new cases.

[11] Note that for parsimony our model treats hybrid and virtual learning as equivalent, whereas the former is both more likely to generate health risks and facilitate learning. In the empirical section, we relax this assumption, but continue to focus our analysis on predictions regarding in-person learning.

[12] For example, districts where teachers' unions are especially influential might place a higher cost on cases (and especially cases among staff).

[13] Later, we derive an expression for the probability that the district chooses in-person supposing that the district also privately observes a standard normal choice shock.

The model also allows the choices of peer districts to offer in-person instruction to exert pressure on district j to do likewise: $\frac{1}{|\mathcal{P}_j|}\sum_{i\in\mathcal{P}_j} d_{it-1}$ represents the share of j's peer districts offering in-person last week. $\mathcal{P}_j$ is a set containing other districts that are j's peer, e.g. they belong to the same county or are both large urban districts. The strength of this channel of peer influence is governed by γ.

New cases evolve in the following way in the model:

$$N_{jt} = \rho N_{jt-1} + \pi d_{jt} + \epsilon_{jt} \tag{2}$$

where $d_{jt} = 1$ if the district chooses in-person in period t (and 0 otherwise), and $\epsilon \sim N(0, \sigma_\epsilon)$. ρ is the marginal effect of previous cases on new cases, reflecting the transmissibility of the virus (independent of the district's choice of learning mode). Offering instruction in-person raises future cases by π, all else equal, though the district does not know this parameter with certainty. For simplicity, we assume that π is a constant that does not depend, for example, on lagged cases. ϵ_{jt} is a mean-zero error term that is realized *after* the district's period t decision. The board then observes N_{jt} and updates its belief about π. This updating process is detailed in the next subsection.

3.2 Learning and Learning Mode Choice

As highlighted earlier, the district is uncertain about the causal effect of in-person instruction on cases, π in equation 2. The district thus uses information from realized cases to update its belief, and modify its choices, over time. This information includes: a) its own new cases following in-person instruction; and b) its peers' realizations of new cases following in-person instruction.

We assume that the district updates in a Bayesian fashion, but that there may be a friction in the district's learning from peers. For ease of exposition, consider the district's choices in periods 1 and 2. The district's initial belief, which it uses to make its period 1 decision, is given by $\pi \sim N(\pi_0, \sigma_\pi)$. The district then updates its beliefs based on the cases observed at the end of period 1, $\mathbf{N}_1$.

$$\begin{aligned}\pi_1 &= E[\pi|\mathbf{N}_1] \\ &= \pi_0 + \lambda \underbrace{d_{j1}(N_{j1} - \rho N_{j0} - \pi_0)}_{\text{own experience}} \\ &\quad + \underbrace{\lambda\kappa \frac{1}{|\mathcal{P}_j|} \sum_{i \in \mathcal{P}_j} d_{i1}(N_{i1} - \rho N_{i0} - \pi_0)}_{\text{peers' experience}} \end{aligned} \tag{3}$$

where $\lambda = \frac{\sigma_\pi}{\sigma_\pi + \sigma_\epsilon}$.[14] Note that the district does not learn about π from its own lagged cases if its own instruction was not in-person. The district's posterior belief is thus a weighted average of its prior and new information extracted from the period 1 realizations, where the weight is proportional to the district's uncertainty about π. Intuitively, if a large fraction of the variance in expected new cases is determined by learning modality, then new cases convey substantial new information. On the other hand, if the variance of new cases is determined largely by random factors, then new cases will provide less information.

The parameter κ in equation 3, which is bounded between 0 and 1 (inclusive), captures a potential information friction to the district's learning from its peers. For $\kappa = 1$, the district will learn equally from its own and from peers' experiences. On the other hand, when $\kappa = 0$, the district does not respond to its peers' new cases. This could be because a peer district's cases are less salient to a district, or district leaders are hesitant to draw inferences from these cases without knowing about harder-to-observe COVID-19 protocols in other districts. Note that a district *may* respond to a peer district's decision to offer in-person instruction because it exerts some social or political pressure on the district leaders, even if the district is inattentive to its peers' COVID cases. The parameter γ reflects this social/political pressure channel. Thus, κ and γ summarize the two mechanisms via which peer districts can influence district j's choice of learning mode.

Given the above, we can express the probability that the district offers in-person instruction in period 2 as a function of a) its initial beliefs; and b) the cases realized following period 1 decisions.

[14] This expression follows from the fact that cases and the district's prior are normal.

We use this expression to develop several empirical predictions discussed in the next section.

$$P(d_{j2} = 1) = \Phi(\beta + \gamma \frac{1}{|\mathcal{P}_j|} \sum_{i \in \mathcal{P}_j} d_{i1} - \alpha E[N_{j2} | N_{j1}, I_{j2}, d_{j2} = 1])$$
$$= \Phi(\beta + \gamma \frac{1}{|\mathcal{P}_j|} \sum_{i \in \mathcal{P}_j} d_{i1} - \alpha \pi_1 - \alpha \rho N_{j1})$$
$$= \Phi(\beta + \gamma \frac{1}{|\mathcal{P}_j|} \sum_{i \in \mathcal{P}_j} d_{i1} - \alpha \pi_0 - \alpha \rho N_{j1}$$
$$- \alpha \lambda d_{j1}(N_{j1} - \rho N_{j0} - \pi_0) - \alpha \lambda \kappa \frac{1}{|\mathcal{P}_j|} \sum_{i \in \mathcal{P}_j} d_{i1}(N_{i1} - \rho N_{i0} - \pi_0)) \tag{4}$$

where we applied the assumption that a standard normal choice shock is added to the utility.

3.3 Predictions

Some comparative statics illustrate the predicted relationship between new cases in one period and the likelihood of offering in-person instruction in the next period. We develop these predictions to evaluate them in the data from the 2020-21 school year.

Prediction 1 *All else equal, the marginal effect of an increase in lagged new cases in the district on the probability of offering in-person instruction will be negative.*

This prediction says that, on the margin, a district will respond to a spike in its own new cases by being less likely to offer in-person in subsequent weeks. This follows from the assumption that cases provide disutility to the district and that the cost of cases, α, is not zero:

$$\frac{\partial P(d_2 = 1)}{\partial N_{j1}} = -\alpha(\lambda d_1 + \rho)\phi(\cdot) \leq 0 \tag{5}$$

where ϕ is the normal PDF. To see this, note that the model assumes $\alpha \geq 0$ and $\rho \geq 0$ and $0 \geq \lambda \geq 1$ and $\phi(\cdot) \geq 0$ by definition. Importantly, note that, $\alpha \geq 0$ provides that districts get disutility from cases. This prediction holds even if there is large heterogeneity across districts in the value placed on in-person instruction per case, $\frac{\beta}{\alpha}$.

Prediction 2 *The marginal effect of lagged cases generated while in-person on the probability of offering in-person instruction will, all else equal, be more negative than the effect of lagged cases generated while not in-person.*

Looking at the partial derivative shown in equation 5, one can consider the marginal effect depending on in-person instruction:

$$\frac{\partial P(d_{j2}=1)}{\partial N_{j1}}\Big|_{d_{j1}=1} = -\alpha(\rho+\lambda)\phi(\cdot) \leq -\alpha\rho\phi(\cdot) = \frac{\partial P(d_{j2}=1)}{\partial N_{j1}}\Big|_{d_{j1}=0}$$

This prediction highlights the learning from own experience channel.[15] The intuition is the following: all else being equal, a greater spike in lagged cases while in-person means the district is more likely to have underestimated its causal effect on cases, which will cause it to revise upward its belief about the health risks of opening schools. In other words, districts should, everything else held constant, respond more negatively to cases that its own learning mode choice contributed to generating than it would otherwise.

Prediction 3 *The marginal effect of lagged cases generated while in-person on the probability of offering in-person instruction will decline in magnitude over time as the district accumulates experience with offering in-person instruction. Moreover, it will converge with the marginal effect of lagged cases generated while not in-person.*

This prediction follows from the fact that uncertainty about π decreases (weakly) over time, as districts offer in-person instruction and observe realizations of new cases. As shown above, this marginal effect (in the two period case) depends on λ, the signal to noise ratio. Extending the model to many weeks, the Bayesian updating formula reveals that the information extracted from the marginal signal will be decreasing in the number of prior signals experienced. In particular, $\lambda_t = \frac{\sigma_\pi}{\sigma_\pi(1+N_t)+\sigma_\epsilon}$ where N_t is the total number of signals received up to t.[16]

The final two predictions summarize the respective mechanisms whereby peer districts' choices may influence the school district's choice of learning mode: pressure and information.

Prediction 4 *The marginal effect of an increase in the share of peer districts offering in-person instruction the prior week on the probability the district offers in-person will, holding constant the information extracted from lagged cases, be positive.*

[15] Note, however, that this prediction for the marginal effect may not hold if $\phi(d_{j1}=1)$ is sufficiently smaller than $\phi(d_{j1}=0)$.

[16] In the model above, each time the district opens for in-person generates one signal, whereas each time a peer district opens generates k signals.

Prediction 5 *The marginal effect of an increase in the aggregate number of lagged cases in peer districts that offered in-person instruction on the probability the district offers in-person will, holding constant the peer share offering in-person the prior week, be negative.*

The first prediction regarding the importance the peer pressure channel corresponds to the γ parameter in equation 4, which we assume is positive. The importance of the information channel corresponds to the friction parameter, κ. This can be seen from the partial derivative:

$$\frac{\partial P(d_2 = 1)}{\partial \frac{1}{|\mathcal{P}_j|} \sum_{i \in \mathcal{P}_j} d_{i1} N_{i1}} = -\alpha\lambda\kappa\phi(\cdot) \leq 0$$

Note that, in the case of no information friction (i.e. $\kappa = 1$), the district learns equally from its own experience as from its peer districts' experiences with offering in-person instruction.

4 Patterns of Learning Mode Decisions During 2020-21

To motivate our analyses on the determinants of learning mode choice, we begin by describing the variation in learning mode decisions over time. We first divide districts into three groups. We assign the 127 districts that provided in-person instruction the entire year to cluster 1 ("always in-person"), and the 79 that rarely offered full in-person instruction and ended the school in a virtual or hybrid mode into cluster 2 ("never returned to in-person").[17] The remaining 402 districts (roughly two-thirds of all districts in the state) changed learning mode multiple times across the year.

In Table 1 we show the frequency of learning mode changes separately by the mode in which the district started the year among the 402 districts. The modal district only changed learning mode two times over the year. One example of this scenario would be a district that switched from virtual to in-person in October and then switched back to virtual in November. Roughly 40% of districts changed mode 3 or 4 times over the year. Only a handful of districts changed more than 5 times.

Table 2 shows the transition matrix for weeks during which districts changed learning mode. Out of the 757 times a district changed mode (excluding the final switch to in-person), 62% involved

[17]There were two districts that were hybrid in the first week of the school year but were in-person for the rest of the year—these are grouped into cluster 1. Districts in cluster 2 were on average in-person 5% during the school year but ended the year either in virtual or hybrid instruction.

Table 1. Number of Learning Mode Changes by Initial Learning Mode

	1	2	3	4	5+	Total
In-Person	0	126	25	49	19	219
Hybrid	55	4	50	9	21	139
Virtual	3	6	4	25	6	44
Total	58	136	79	83	46	402

Note: $N = 402$. This table shows the total number of learning mode changes by initial learning mode. It excludes districts from the two clusters that were always in-person or mostly hybrid and virtual.

Table 2. Learning Mode Transiton Matrix, Excluding Final Transition to In-person

	Learning mode in t			
Learning mode in $t-1$	In-Person	Hybrid	Virtual	Total
In-Person	0	180	166	346
Hybrid	89	0	127	216
Virtual	38	157	0	195
Total	127	337	293	757

Note: Table excludes districts from the two clusters that were always in-person or mostly hybrid and virtual. It also excludes observations for when a district switched to in-person permanently.

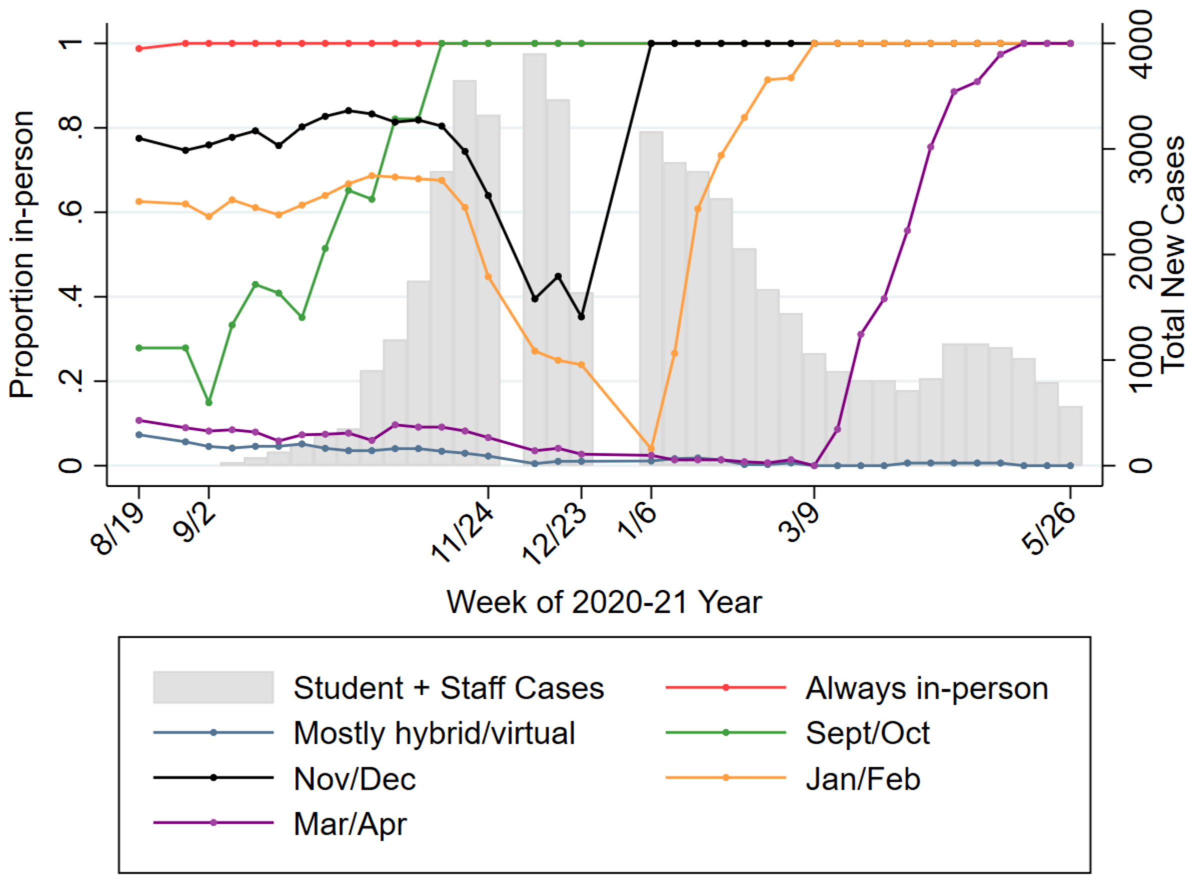
changes between in-person and another mode while 38% involved switches between hybrid and virtual. The four most common transitions are in-person to hybrid (180), in-person to virtual (166), virtual to hybrid (157), and hybrid to virtual (127).[18]

Next we conduct a K means cluster analysis, using three inputs: the percentage of weeks a districts was in-person during fall 2020, the last week that the district was **not** in-person, and the total number of times the district changed learning modes over the course of the year. To determine the number of clusters, we look to minimize the within sum of squares for all cluster solutions. This analysis resulted in four clusters in addition to the two earlier groups (i.e., always in-person and never returned to in-person).[19] Figure 2 shows how the fraction of students attending school in-person evolves over the year weighted by the number of students, separately by cluster. It is striking how the timing of the final in-person schooling decision differs sharply across clusters. A very small set of districts (n=24) start to return in the middle of the fall (green line) while the others start returning to in-person instruction in late December 2020 (black line, n=76), early January (orange

[18]The week that experienced the most changes was December 3, 2020, when 52 districts changed modes starting after students returned from Thanksgiving Break. In the first week of January, 121 districts changed learning mode (from what had been in place immediately before Winter Break). The third week of the school year (August 20, 2020) witnessed 24 districts changing learning mode. Immediately before making the final switch to in-person instruction, 260 districts were hybrid and 141 were virtual.

[19]The scree plot suggests the remaining districts can be subdivided into 4-5 clusters. See Appendix Figure 1.

line, n=197) and mid-March (purple line, n=105). To provide a frame of reference, Table 3 shows the largest five districts in each cluster.



Note: This figure shows the aggregate number of student and staff COVID-19 cases in Ohio during the 2020-21 school year (gray bars) along with the proportion of districts ($N = 608$) that were in-person separately by our six clusters.

Figure 2. Learning Mode and COVID-19 Cases in 2020-21, by Cluster

In Table 4 we present summary statistics separately by cluster, revealing several interesting patterns. Districts that were always in-person (column 1) feature rural and suburban areas with a very high share of Republican voters. However, the poverty rates and COVID-19 case rates at the start of the school year in these districts were similar to districts in several other clusters. The small set of districts that began to return to in-person instruction in early fall 2020 (column 2) included towns and suburbs in counties with relatively high COVID-19 risk levels according to the color warning system used by Ohio. On the other hand, the set of 79 districts that never returned to complete in-person instruction (column 6) are disproportionately larger, urban and suburban

Table 3. Largest 5 School Districts by Cluster

Always in-person		Never returned to in-person		Sep/Oct return	
Forest Hills Local	7,327	Columbus City	48,759	Hamilton City	9,938
Springboro Community City	6,325	Cleveland Municipal City	37,146	Springfield City	7,732
Kings Local	4,942	Toledo City	22,897	Newark City	6,481
Perry Local (Stark)	4,557	Akron City	21,298	Plain Local	6,245
Southwest Licking Local	4,423	Centerville City	8,413	Twinsburg City	4,191
127		79		24	
Nov/Dec return		Jan/Feb return		Mar/Apr return	
West Clermont Local	8,336	Lakota Local (Butler)	16,475	Cincinnati City	36,033
Sycamore Community City	5,567	Dayton City	12,446	South-Western City	22,727
Wadsworth City	4,715	Northwest Local (Hamilton)	8,904	Olentangy Local	22,124
Mayfield City	4,524	Canton City	8,045	Dublin City	16,624
Austintown Local	4,292	Beavercreek City	8,028	Hilliard City	16,519
76		197		105	

Note: This table presents the enrollment count of the five largest districts by our six clusters. We manually classified the first two clusters: districts that were always in-person and districts that never returned to in-person. For the rest of the districts, we conducted a K means cluster analysis, using three inputs: the percentage of weeks a districts was in person during fall 2020, the last week that the district was not in person, and the total number of times the district changed learning modes over the course of the year.

districts with higher shares of Black and low-income students, and the lowest Republican vote share. The clusters shown in columns 3, 4 and 5 have demographics that lie between the extremes. While it appears that some demographics are associated with cluster membership, we can also see that these demographics are not perfect predictors. The clusters in columns 1 and 3, for example, have virtually identical average values for many of these important characteristics, but made quite different learning mode decisions.

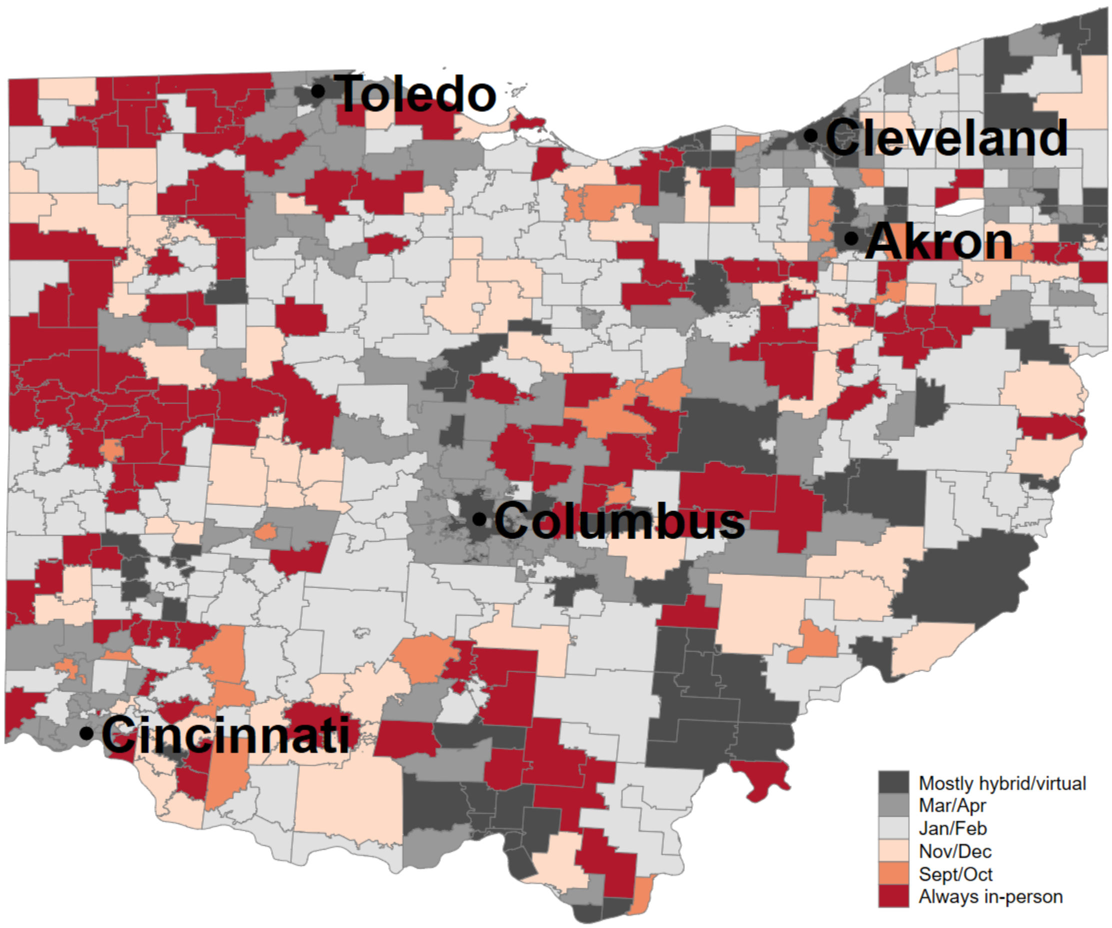
Figure 3 shows a map of school districts in Ohio, color coded to reflect the six clusters. The dark red districts were always in-person while the other districts are shades of gray, with darker shades reflecting the district started the final return to in-person instruction at a later date. Few clear patterns emerge. The dark red districts seem somewhat more common in the western and central parts of the state, but there are also notable red areas in the eastern section of the state, close to Akron. The urban areas around Cleveland and Columbus are notably dark gray, indicating later in-person dates. But there are some large dark areas in the southeast rural part of the state as well.

To more carefully explore what factors predict cluster membership, we estimated a series of regression models. We experimented with several LASSO and Random Forest models, but they did not provide substantially more insight than simple OLS models. These OLS estimates are shown in

Table 4. School District Characteristics by Cluster

	Always	Returned to in-person instruction...				
	in-person	Sep/Oct	Nov/Dec	Jan/Feb	Mar/Apr	Never
Prop in-person	1.00	0.85	0.86	0.73	0.26	0.05
Total changes	0.02	1.38	2.42	3.08	3.20	1.90
Last week not in-person	0.02	7.67	18.90	24.77	33.45	41.00
County cases/100K (baseline)	88.40	82.61	66.78	82.19	111.80	87.85
County Orange (baseline)	0.62	0.75	0.62	0.60	0.47	0.47
County Red+ (baseline)	0.15	0.17	0.11	0.21	0.44	0.37
Time invariant district characteristics						
Enrollment	1,653	3,024	1,579	2,151	4,263	3,851
Urban	0.03	0.04	0.00	0.02	0.04	0.08
Rural	0.57	0.29	0.66	0.46	0.31	0.33
Town	0.20	0.21	0.15	0.21	0.12	0.18
Suburb	0.21	0.46	0.20	0.31	0.53	0.42
Prop FRPL	0.44	0.46	0.44	0.45	0.48	0.64
Prop Black	0.03	0.06	0.02	0.04	0.11	0.20
Republican vote share	0.73	0.57	0.66	0.70	0.69	0.59
Poverty rate	0.11	0.11	0.11	0.11	0.12	0.17
Prop w/ BA+	0.20	0.25	0.19	0.24	0.28	0.22
Number of districts	127	24	76	197	105	79

Note: This table presents descriptive statistics for districts by our six clusters. We manually classified the first two clusters: districts that were always in-person and districts that never returned to in-person. For the rest of the districts, we conducted a K means cluster analysis, using three inputs: the percentage of weeks a districts was in person during fall 2020, the last week that the district was not in person, and the total number of times the district changed learning modes over the course of the year. Week corresponds to the start of the school year, where week 1 is the week ending in 8/19/2020. FRPL = Free or reduced price lunch. Republican vote share refers to the 2016 presidential vote share. Base county cases refers to the the number of positive COVID-19 cases in the county in the two weeks prior to the start of the school year. BA+ refers to the proportion of adults in the district with a bachelors degree or higher. The color indicators are risk ratings at the beginning of the school year that are based on a seven point scale developed by OPHAS: counties meeting at most 1 indicator are coded as yellow (lowest-risk), counties meeting 2-3 indicators as orange, counties meeting 4-5 indicators as red, and counties meeting 6-7 indicators as purple (highest-risk). These are measured the two weeks before the beginning of the 2020-21 academic year.



Note: This map classifies Ohio's 608 school districts by our six clusters. We manually classified the first two clusters: districts that were always in-person and districts that never returned to in-person (mostly hybrid/virtual). For the rest of the districts, we conducted a K means cluster analysis, using three inputs: the percentage of weeks a districts was in-person during fall 2020, the last week that the district was not in-person, and the total number of times the district changed learning modes over the course of the year.

Figure 3. Map of School Districts by Cluster

Table 5. In column 1, we regress an indicator for cluster 1 membership (always in-person) on a set of time-invariant district covariates. The sample excludes districts that were almost never in-person. Here we see that Republican vote share and district poverty rate are the strongest predictors. A 10 percentage point increase in the Republican vote share is associated with a 4.25 percentage point higher likelihood of being always in-person. Given the sample mean, this reflects a 22% increase. A 10 percentage point increase in the percent of non-white students is similar.

Table 5. OLS Estimates of Cluster Membership

	(1)	(2)	(3)
	Always in-person	Never Returned in-person	Week returned in-person
Log(enroll)	-0.094***	0.010	1.021*
	(0.029)	(0.024)	(0.522)
Urban	0.328**	-0.022	-3.462
	(0.138)	(0.142)	(3.639)
Town	0.020	-0.000	-1.008
	(0.054)	(0.047)	(0.954)
Suburb	0.035	-0.033	-1.148
	(0.057)	(0.049)	(1.046)
Prop FRPL	-0.200**	0.400***	2.421
	(0.096)	(0.090)	(2.017)
Prop non-white	0.236	0.032	1.822
	(0.153)	(0.182)	(3.040)
Log(repub. vote share)	0.425***	-0.274***	-4.190
	(0.105)	(0.098)	(2.547)
County orange (baseline)	-0.039	-0.016	0.798
	(0.053)	(0.044)	(0.817)
County red+ (baseline)	-0.029	0.005	2.861**
	(0.068)	(0.061)	(1.133)
Districts excluded	Never	Always	Never, Always
N	529	481	402
Mean outcome	0.24	0.16	25
R-squared	0.078	0.149	0.093

Note: Columns (1) and (2) report estimates of linear probability models for belonging to "Always" and "Never Returned" in-person clusters, respectively. Here, "Always" refers to districts that remained in-person throughout the entire school year and "Never Returned" refers to districts that never returned to in-person schooling by the end of the school year (though they may have been in-person earlier in the school year). Column (3) reports OLS estimates of week permanently returned to in-person instruction. The reference category for urbanicity is rural. FRPL = free or reduced-price lunch. Republican vote share refers to the 2016 presidential vote share. The color indicators are risk ratings in the two weeks prior to the start of the school year that are based on a seven point scale developed by OPHAS: counties meeting at most 1 indicator are coded as yellow (lowest-risk) and are the reference category, counties meeting 2-3 indicators are orange, counties meeting 4-7 indicators are coded red+ (highest-risk). These are measured the two weeks before the beginning of the 2020-21 academic year. *** p<0.01, ** p<0.05, * p<0.1.

These relationships run in the opposite direction when the outcome is never in-person and the always in-person cluster is excluded; a 10 percentage point increase in the Republican vote share is associated with a 2.74 percent point lower likelihood of being in the never return to in-person group. In column 3, we regress the number of the week in the academic year that the district returned to in-person instruction permanently. The sample includes districts that were not always in-person, but did return to in-person instruction at some point during the year. We see that district size is positively correlated with later returns, with a 10 percent increase in student enrollment associated with returning roughly 0.1 weeks earlier. Political partisanship is an important predictor as well—a 10 percent increase in Republican vote share is associated with returning roughly 0.4 weeks earlier. Finally, districts that started the school year in a heightened state of COVID-19 risk (red) returned 2.9 weeks later on average.

In this section, we show that Ohio school districts can be categorized into a modest number of groups based on their pattern of learning modes during the 2020-21 academic year. Perhaps most interesting, we find roughly two-thirds of districts changed learning modes multiple times during the year. Consistent with prior research (Hartney and Finger, 2020; DeAngelis and Makridis, 2021), we find that urbanicity, size and political partisanship predict whether a district was always or never in-person during the year. However, these variables explain only a modest share of the variation in district decisions. Moreover, roughly two-thirds of Ohio districts changed learning mode multiple times, and observable district characteristics do a poor job of predicting when a district returned permanently to in-person instruction.

5 Determinants of Changes in Learning Mode Decisions During 2020-21

The model above suggests that a district's choice of learning mode in week t will be a function of its own lagged COVID cases and learning mode as well as the lagged cases and learning mode of its peers. Using a district by week panel of public schools in Ohio during 2020-21, we estimate a series of regression models to explore these hypothesized relationships. We first focus on how districts respond to new cases among their own students and staff, and later examine potential peer effects. For this analysis, we restrict the sample to the two-thirds of Ohio districts that were neither always or never returned to in-person because they provide variation in learning modes over time.

5.1 Empirical Specification

We empirically model the likelihood of district j providing in-person instruction in week t as a function of the number of new COVID-19 cases in the *prior* week, the learning mode in the *prior* week and, consistent with the model predictions, interactions between these variables:

$$\begin{aligned}\text{in-person}_{jt} = {} & \beta^p \text{newcases}_{jt-1} + \beta^h(\text{newcases}_{jt-1} \times \text{hybrid}_{jt-1}) + \beta^v(\text{newcases}_{jt-1} \times \text{virtual}_{jt-1}) \\ & + \rho^h \text{hybrid}_{jt-1} + \rho^v \text{virtual}_{jt-1} + \theta X_j + \xi_t + \eta_{c(j)} + \epsilon_{jt} \end{aligned} \tag{6}$$

In our primary specifications, we define new cases as the total number of student and staff cases (per 1,000 students and staff) in the district, but we also present results that distinguish between student and staff infections. X_j includes time-invariant district and/or county covariates, including COVID-19 rates at the beginning of the school year. ξ_t reflects week fixed effects and $\eta_{c(j)}$ reflects county fixed effects. ρ^h and ρ^v capture auto-correlation in learning mode choice over time, including due to switching costs. In all cases, we estimate this model via OLS and present standard errors clustered by county.[20]

In this model, $\hat{\beta}^p$ reflects the marginal effect of an 1 unit increase in new cases last week while in-person on the probability of providing in-person instruction this week and is the principal object of interest. $\hat{\beta}^v$ and $\hat{\beta}^h$ reflect the *differential* marginal effect of COVID-19 cases reported while the district was operating virtually or hybrid relative to in-person. The first prediction from our model is that $\hat{\beta}^p$, $\hat{\beta}^p + \hat{\beta}^v$ (the marginal effect of lagged cases while virtual), and $\hat{\beta}^p + \hat{\beta}^h$ (the marginal effect of lagged cases while hybrid) are all negative. The second prediction from our model is that $\hat{\beta}^h > 0$ and $\hat{\beta}^v > 0$ (the marginal effect of lagged cases generated while in-person will be more negative than the effect of lagged cases generated while not in-person).

In order to test prediction 3 (that the marginal effect of lagged cases declines in magnitude over

[20] It is important to note that our learning mode and empirical specification are not completely identical. In equation 4 in our learning mode we model the district's learning choice as a probit model, but our main specification in equation 6 is a linear probability model (LPM). We use a LPM because we include a large number of fixed effects to account for variation across weeks and counties. Estimating many fixed effects with a probit model has two issues: 1) districts in county weeks that have no variation in their learning mode are "perfectly predicted" and dropped from the analysis and 2) estimators of fixed effects are inconsistent, which means our estimates of β will also be inconsistent as they are jointly estimated with maximum likelihood estimation (Heckman, 1981). Therefore, we decide to use a LPM as our main specification and run a sensitivity test in Table 7 with a probit model and confirm estimates are consistent across functional forms.

time), we will estimate a specification that includes interactions between lagged cases and week.

$$\begin{aligned}
\text{in-person}_{jt} = {} & \beta^p \text{newcases}_{jt-1} + \beta^h(\text{newcases}_{jt-1} \times \text{hybrid}_{jt-1}) + \beta^v(\text{newcases}_{jt-1} \times \text{virtual}_{jt-1}) \\
& + \delta^p(\text{newcases}_{jt-1} \times \text{week}_t) + \delta^h(\text{newcases}_{jt-1} \times \text{hybrid}_{jt-1} \times \text{week}_t) \\
& + \delta^v(\text{newcases}_{jt-1} \times \text{virtual}_{jt-1} \times \text{week}_t) \\
& + \rho^h \text{hybrid}_{jt-1} + \phi^h(\text{hybrid}_{jt-1} \times \text{week}_t) + \rho^v \text{virtual}_{jt-1} + \phi^v(\text{virtual}_{jt-1} \times \text{week}_t) \\
& + \theta X_j + \xi_t + \eta_{c(j)} + \epsilon_{jt} \qquad (7)
\end{aligned}$$

where week is a continuous measure of week in the academic year that ranges from 1 to 37. We predict that $\delta^p > 0$ (the marginal effect of lagged cases attenuates over time). This model also allows the costs of switching between learning modes to vary over time. In addition to interactions with week to test prediction 3, we also examine, motivated by the model, interactions with cumulative times in-person and cumulative cases while in person.

To test our predictions regarding peer effects, we will estimate the following specification:

$$\begin{aligned}
\text{in-person}_{jt} = {} & \beta^p \text{newcases}_{jt-1} + \beta^h(\text{newcases}_{jt-1} \times \text{hybrid}_{jt-1}) + \beta^v(\text{newcases}_{jt-1} \times \text{virtual}_{jt-1}) \\
& + \gamma(\text{peers-in-person}_{jt-1}) + \kappa^p(\text{peer-newcases-in-person}_{jt-1}) \\
& + \kappa^h(\text{peer-newcases-hybrid}_{jt-1}) + \kappa^v(\text{peer-newcases-virtual}_{jt-1}) \\
& + \rho^h \text{hybrid}_{jt-1} + \rho^v \text{virtual}_{jt-1} + \theta X_j + \xi_t + \epsilon_{jt} \qquad (8)
\end{aligned}$$

where peers-in-person is the fraction of peers that offered in-person instruction in a given week. The parameter κ^p captures the impact of the cases peer districts had while they were in-person in a given week. κ^h and κ^v represent the same information for peer districts that were hybrid or virtual, respectively. Prediction 4 suggests that $\gamma > 0$. As the fraction of peers offering in-person instruction in one week will, all else equal, increase the likelihood of a district offering in-person instruction in the following week. Prediction 5 suggests that $\kappa^p < 0$ if a district learns about the parameter π from the experience of peer districts. Note that the specification in equation 8 does not include county fixed effects because they are highly correlated with some of the peer measures.

5.1.1 Identification

A key empirical question is how to control for unobserved heterogeneity that may be associated with learning mode choices and new cases. In all models, we include week fixed effects to control for factors impacting all districts such as state or federal mandates. We also control for a set of time-invariant district characteristics including: binary indicates for urban, rural or town (suburban excluded), student enrollment, percent of students that are black and Hispanic, a composite measure of socioeconomic status[21], proportion of adults with a bachelor's degree or higher, the household poverty rate, average student achievement[22], and 2016 Republican vote share. We also include measures of baseline county-level COVID severity from the two weeks prior to the beginning of the school year (cases per 100,000 county residents, color indicators, and the total 1-7 score designated by OPHAS). To the extent that these factors are correlated with the evolution of COVID-19 cases in the district as well as the likelihood a district will choose to go back in-person in any given week, the inclusion of these covariates will mitigate bias.

Of course, it is possible to imagine the existence of permanent unobservable district factors that influence both COVID-19 cases and learning mode decisions throughout the school year. The most common solution to this concern would be to include district fixed effects. However, this approach is susceptible to lagged dependent variable bias in the presence of autocorrelation, which may generate misleading estimates (Angrist and Pischke, 2009). In practice, we find little difference between models with district fixed effects versus models with district covariates. For this reason, our preferred specification includes district covariates and county fixed effects.

5.2 Results

We report our main results in Table 6. Column 1 presents estimates from a model in which the only controls are week fixed effects. Consistent with prediction 1 from the model, the coefficient on lagged cases is -0.023, indicating that an increase of 1 case per 1,000 students and staff is associated, all else held equal, with a 2.3 percentage point reduction in the likelihood that the district was in-person during the following week. As a point of reference, 64% of district-week observations in the

[21]This is based on measures of median family income, adults with a bachelor's degree or higher, unemployment and poverty rates, SNAP recipients, and households headed by a single mother (Reardon et al., 2021).

[22]Four districts were missing district achievement in 2018, so we impute 0 for these observations and include a missing indicator variable.

sample were in-person and the standard deviation of COVID-19 cases was 1.3.

Table 6. The Effect of Lagged Cases on In-Person Instruction

	(1)	(2)	(3)	(4)	(5)
Lagged cases (per 1,000)	-0.023***	-0.023***	-0.023***	-0.023***	-0.022***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
* lagged virtual	0.045***	0.036***	0.040***	0.039***	0.034***
	(0.006)	(0.007)	(0.007)	(0.007)	(0.007)
* lagged hyrbrid	0.028***	0.029***	0.028***	0.028***	0.025***
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Week fixed effects	Yes	Yes	Yes	Yes	Yes
District fixed effects	No	Yes	No	No	No
District covariates	No	No	Yes	Yes	Yes
County fixed effects	No	No	No	Yes	Yes
County $\times$ week fixed effects	No	No	No	No	Yes
H_0: lagged cases while virtual = 0 (P-value)	0.000	0.048	0.006	0.010	0.067
H_0: lagged cases while hybrid = 0 (P-value)	0.089	0.076	0.100	0.110	0.341
R-squared	0.776	0.788	0.778	0.781	0.784

Note: Table reports OLS estimates of equation 6 for select variables (we include in our model but do not report estimates from lagged learning mode). N = 14,472 school district-week observations. Sample excludes clusters that are always in person or mainly hybrid or virtual. Sample average of in-person instruction is 0.64. Lagged cases include both student and staff cases. District covariates include log enrollment, indicators for urbanicity, district sociodemographic variables, controls for initial county COVID severity, and Republican vote share. Four districts were missing district achievement in 2018, so we impute 0 for these and include a missing indicator variable. Standard errors are clustered by county. *** p<0.01, ** p<0.05, * p<0.1.

The coefficients on the interaction between lagged cases and lagged learning mode (virtual or hybrid) are both positive, indicating that cases reported when instruction was virtual or hybrid are *less* negative predictors of future learning mode decisions. This is consistent with prediction 2, and suggests that new cases while hybrid or virtual provide less information regarding the true value of π, the impact of in-person instruction on case counts.

In columns 2 and 3, we report results for specifications that include district fixed effects and time-invariant district covariates, respectively. The results change very little relative to those in column 1. We know from the previous results that these district characteristics do predict in-person instruction, so these results indicate that the district factors are not systematically related to lagged cases and learning mode. We report our preferred specification in column 4, which includes week and county fixed effects and district controls. Again, point estimates do not change. In column 5, we add in county x week fixed effects to account for county time trends, and our estimates do not change. Our results remain consistent across a variety of specifications and provide strong evidence that districts that experience an increase in COVID cases are less likely to have in-person instruction the following week.

Robustness

Table 7. Predicting In-Person Instruction: Robustness

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Probit	Drop obs after perm. in-person	In-person/hybrid vs virtual	In-person/hybrid vs virtual	Student only	Staff only	Student & staff
Lagged cases (per 1,000)	-0.018***	-0.056***	-0.004**	-0.004**			
	(0.002)	(0.006)	(0.002)	(0.002)			
* lagged virtual	0.020***	0.061***	0.028***	0.035***			
	(0.003)	(0.008)	(0.007)	(0.007)			
* lagged hyrbrid	0.020***	0.065***					
	(0.002)	(0.006)					
Lagged student cases (per 1,000)					-0.021***		-0.014***
					(0.003)		(0.003)
* lagged virtual					0.040***		0.030***
					(0.008)		(0.009)
* lagged hyrbrid					0.028***		0.017***
					(0.004)		(0.005)
Lagged staff cases (per 1,000)						-0.010***	-0.007***
						(0.001)	(0.001)
* lagged virtual						0.014***	0.008**
						(0.003)	(0.003)
* lagged hyrbrid						0.011***	0.008***
						(0.002)	(0.002)
N	13,266	8,237	14,472	17,316	14,472	14,472	14,472

Note: Table reports estimates of variants of equation 6 for selected variables (we include in our model but do not report estimates from lagged learning mode). In column (1) we show our probit specification marginal effects. Column (2) drops observations after they went back to in-person permanently. Column (3) replaces the the main outcome, in-person vs hybrid/virtual, with in-person/hybrid vs virtual. Column (4) is the same as column (3) but includes districts that never returned to in-person learning. Columns (5) to (7) separate district cases out by students and staff. Sample excludes clusters that are always in person or mainly hybrid or virtual. Sample average of in-person instruction is 0.64. Lagged cases include both student and staff cases. District covariates include log enrollment, indicators for urbanicity, district sociodemographic variables, controls for initial county COVID severity, and Republican vote share. Four districts were missing district achievement in 2018, so we impute 0 for these and include a missing indicator variable. Standard errors are clustered by county. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 7 illustrates the robustness of our main findings to different model specifications. We first confirm our estimates remain consistent across functional form by estimating equation 6 with a probit model rather than a linear probability model. Column 1 shows the marginal effect of an increase in 1 case per 1,000 students and staff is associated with a 1.8 percentage point reduction in the likelihood the of being in-person the following week (vs a 2.3 percentage point decrease in our main LPM specification). Column 2 replicates our main results dropping all district x week observations after a district permanently returned to in-person learning. The results remain qualitatively the same.

Columns 3-4 explore whether our results are robust to the including hybrid learning with in-person instruction. As discussed above, districts may have had different definitions of hybrid learning, some of which could more closely mirror in-person instruction than others. For example, a district might have lower grades attending in-person half the week and upper grades attending the other half or a district might have lower grades fully in-person and upper grades fully virtual. In both cases, districts that had substantial in-person learning would be labeled as hybrid. To test the sensitivity of our results to this scenario, in columns 3 and 4 we replace our binary in-person learning outcome with a binary in-person or hybrid variable. The point estimate on lagged cases in column 3 shrinks by over 80%, but is still statistically significant and negative. In column 4, we expand our sample to include districts that were mainly hybrid or virtual during the school year to better test the sensitivity of our results to hybrid learning, and the point estimate on lagged case counts remains consistent and negatively signed, suggesting that our findings are robust to broad definitions of in-person learning.

In columns 5-7, we separate case counts by staff and students to test whether our results are driven exclusively by students or staff. We find that both types of case counts are negatively associated with the likelihood of learning in-person, but student case counts have a slightly stronger relationship. This is not surprising considering the district enrolls about 1.6 million students and 300,000 staff members, so a unit change in cases per 1,000 students is much larger in absolute terms than a unit change in cases per 1,000 staff members.

Finally, in Appendix Table A1, we present the results of several specifications that explore how districts respond to COVID cases even earlier in the school year. The learning model outlined above implies that, in addition to lagged new cases, COVID cases in all earlier weeks should influence the probability a district offers in-person instruction in the next week (see equation 4). To test whether these predictions are true, and how it influences the focus of our analysis (i.e., the coefficient on lagged new cases), in column 1 we show a model that includes twice-lagged new cases interacted with lagged learning mode and district demographics interacted with lagged learning mode. As expected, twice lagged cases are negatively associated with future in-person, and the inclusion of this control attenuates the absolute magnitude of the coefficient on once-lagged cases. However, the sum the coefficients on the lags is (roughly) equal to the coefficient in the single lag model. In column 2, we control for cumulative new case from week 1 through week $t-2$ (and the accompanying interactions). These estimates are negatively signed and consistent with our model. Finally, column 3 includes both twice lagged cases and cumulative new cases, and our estimates remain qualitatively similar. These findings lend further credibility to our learning model.

District Cases vs. County Cases

Thus far we have focused on COVID case counts among school district students and staff. However, we know that school and community leaders were acutely aware of the county-level COVID severity. County COVID-19 case rates, hospitalizations and deaths were widely reported in the media and used by federal, state and local officials to gauge the severity of the pandemic and guide policy decisions. Moreover, county rates were subject to less measurement error because they were reported for a larger population and over a two-week period. Finally, OPHAS reported a color-based risk level that may have been more salient to district officials than the small case numbers from their own districts. Because case rates in the county and the school district are highly correlated, it is possible that our estimates may simply reflect the responsiveness of district leaders to county COVID conditions.

In Table 8 we presents results from several models that include county COVID-19 information along with district measures. In all models, we control for week fixed effects, county fixed effects, and district characteristics. We interact our county measures by lagged learning mode but only present the main effects (refer to Appendix Table A2 for the estimates of interaction terms). Because county data was reported at the bi-weekly level, we use district cases during the prior two weeks instead of one week (this mechanically shrinks our sample size by 3%). In column 1, the lagged district case variable is the weekly average number of cases per 1,000 students and staff over the past two weeks. The coefficient of -0.036 is roughly 50% larger than the -0.023 reported in Table 6. This suggests that district officials were incorporating information from the prior two weeks to make learning decisions for the current week. The fact that this coefficient is not double the coefficient on the one-week measure indicates a diminishing marginal return of additional COVID-19 case information for district officials. In column 2, we show results from a model in which lagged district cases are expressed as the *total* reported student and staff cases over the past two weeks (rather than the average number of cases reported in each of the two weeks). We show this model in order to be able to compare to models with county case rates, which are reported as *total* cases over the two-week period. Mechanically, this simply shrinks each of the lagged case coefficients by half. Specifically, the -0.018 coefficient on lagged total cases in column 2 indicates that an increase in 1 reported case per 1,000 students and staff during the prior two-week period is associated with a 1.8 percentage point lower probability of being in-person in the next week.

Table 8. The Effect of Lagged Cases on In-Person Instruction: County-level COVID Severity

	(1)	(2)	(3)	(4)	(5)
Lagged cases (per 1,000)	-0.036***	-0.018***	-0.008***	-0.017***	-0.014***
	(0.005)	(0.002)	(0.003)	(0.003)	(0.003)
* lagged virtual	0.073***	0.036***	0.017**	0.035***	0.027***
	(0.012)	(0.006)	(0.008)	(0.008)	(0.008)
* lagged hyrbrid	0.051***	0.025***	0.008	0.022***	0.018***
	(0.007)	(0.003)	(0.005)	(0.004)	(0.005)
Lagged county cases (per 1,000)			-0.021**		
			(0.009)		
Lagged county indicator (1-7)				-0.023***	
				(0.007)	
Lagged county orange					-0.041**
					(0.019)
Lagged county red+					-0.063***
					(0.022)
Lagged district cases over past 2 weeks	Mean	Total	Total	Total	Total
H_0: lagged cases while virtual = 0 (P-value)	0.002	0.002	0.252	0.018	0.103
H_0: lagged cases while hybrid = 0 (P-value)	0.021	0.021	0.925	0.144	0.345
R-squared	0.640	0.640	0.647	0.642	0.643

Note: Table reports OLS estimates of equation 6 for selected variables with added controls for county-level COVID severity (we include in our model but do not report estimates from lagged learning mode). Specifically, we add lagged county controls (over the prior two weeks) and interact them with lagged learning mode. We only present the main effects in this table. $N = 14{,}070$ school district-week observations. Sample excludes districts that were always in-person or mainly hybrid or virtual. Sample average of in-person instruction is 0.64. Lagged cases include both student and staff cases. All columns include week fixed effects, district covariates, and county fixed effects. County orange = 2-3 COVID severity indicators met and County red+ = 4+ indicators met. Note that columns (3) through (5) control for (but do not report estimates on) interactions of county variables with lagged virtual and lagged hybrid. District covariates include log enrollment, indicators for urbanicity, district sociodemographic variables, controls for initial county COVID severity, and Republican vote share. Four districts were missing district achievement in 2018, so we impute 0 for these and include a missing indicator variable. Standard errors are clustered by county. *** p<0.01, ** p<0.05, * p<0.1.

We add total county cases over the past two weeks along with the interactions with learning mode in column 3.[23] The point estimate on county cases is negative and implies that an increase in 1 reported case per 1,000 people in the county in the previous two weeks is associated with a 2.1 percentage point lower probability of being in-person the following week. Adding in county case counts shrinks the coefficient on lagged district cases considerably, although it remains negative and significant. This suggests that district officials responded to case rates in the district, conditional on secular pandemic changes happening at the county.

In columns 4 and 5, we replace county case rates with alternative measures of county COVID-19

[23]In order to avoid a mechanical correlation between lagged district cases and lagged county cases, for each district x two-week period, we exclude district cases from the total county cases. Denote the total number of reported cases (per 1,000) in county j and district i as C_j and C_i respectively, and total number of people in the county as N_j and total number of students and staff in district i as N_i. We then calculate $\tilde{C_j} = (C_j * \frac{N_j}{1000} - C_i * \frac{N_i}{1000})/\frac{N_j}{1000}$.

severity—a discrete risk rating on a scale from 1-7 (column 4) and binary indicators for moderate (orange) and extreme (red+) COVID-19 risk (column 5). We see similar patterns; namely, the coefficient on district cases is smaller when we account for county COVID-19 severity and districts appear to respond to both county and district COVID-19 measures.

5.2.1 Heterogeneity Across Time

Our model implies that the marginal effect of new cases on in-person instruction will shrink (in absolute magnitude) over time, as districts experience more realizations of new cases and their uncertainty regarding the relationship between in-person instruction and future cases diminishes. We might also expect the cost of cases to fall over time as vaccines become more readily available and reduce the probability of hospitalization and death.[24] In Table 9 we report results from models that explore this type of heterogeneity. All models include week fixed effects, county fixed effects and district controls. We de-mean week so the main effect of lagged cases can be interpreted as the effect for a district with an average value of this variable.

The first three columns in Table 9 show that districts become less responsive to lagged district COVID-19 cases as the school year progresses. In column 1, we interact our continuous measure of week with lagged cases, lagged learning mode, and subsequent two-way interactions. The estimates in column 1 imply that at the very beginning of the school year (week 4), each additional COVID-19 case per 1,000 students and staff is associated with a 6.4 percentage point lower probability of returning in-person in the following week ($-.022 + (.003) * (-14) = -0.064$). In late March (week 32), an additional case is associated with a non-significant 2 percentage point increase in the likelihood of returning in-person ($-0.022 + .003 * 14) = 0.02$).

Even though we control for district characteristics and week fixed effects, it is possible that unobservable district-specific time-varying factors may be correlated with lagged cases and learning mode decisions. In order to control for such factors, in column 2 we add interactions of two highly predictive district characteristics—Republican vote share and baseline county-level COVID-19 severity—and lagged cases and lagged learning mode. This only slightly attenuates our estimates of lagged case counts and the interaction with week, suggesting that the diminishing importance of lagged cases over time is not merely a function of unobserved heterogeneity.

[24] Vaccines became available to healthcare workers and the public starting in December 2020.

Table 9. The Effect of Lagged Cases on In-Person Instruction: Heterogeneity Over Time

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Lagged cases (per 1,000)	-0.022***	-0.019***	-0.020***	-0.034***	-0.032***	-0.025***	-0.030***	-0.025***	-0.030***
	(0.003)	(0.005)	(0.004)	(0.006)	(0.008)	(0.008)	(0.004)	(0.007)	(0.006)
* week	0.003***	0.002***	0.004***						
	(0.000)	(0.000)	(0.001)						
* twice lagged cum. in-person				0.001***	0.001***	0.000			
				(0.000)	(0.000)	(0.000)			
* twice lagged cum. cases in-person							0.001***	0.001***	0.001**
							(0.000)	(0.000)	(0.000)
* lagged cum. first vax dose			-0.001*			0.001***			0.001***
			(0.001)			(0.000)			(0.000)
* log(repub. vote share) (normalized)		-0.014	-0.012		-0.028	-0.018		-0.029	-0.018
		(0.015)	(0.015)		(0.018)	(0.015)		(0.019)	(0.015)
* county orange (baseline)		-0.002	-0.001		-0.002	-0.003		-0.004	-0.004
		(0.005)	(0.005)		(0.005)	(0.005)		(0.005)	(0.005)
* county red+ (baseline)		-0.006	-0.005		-0.001	-0.009		-0.003	-0.009
		(0.009)	(0.009)		(0.010)	(0.010)		(0.010)	(0.009)
Twice lagged cum. in-person				-0.004***	-0.004***	-0.002***			
				(0.000)	(0.000)	(0.000)			
Twice lagged cum. cases in-person							-0.002***	-0.002***	-0.001***
							(0.000)	(0.000)	(0.000)
Lagged cum. first vax dose			0.003***			-0.001			-0.000
			(0.001)			(0.001)			(0.001)

Note: Table reports OLS estimates of equation 6 for selected variables with additional interaction terms (we include in our model but do not report estimates from lagged learning mode). N = 14,070 school district-week observations. Sample excludes districts that were always in-person or mainly hybrid or virtual. Sample average of in-person instruction is 0.64. Lagged cases include both student and staff cases. All columns include week fixed effects, district covariates, and county fixed effects. County orange = 2-3 COVID severity indicators met and County red+ = 4+ indicators met. Models under columns 2, 3, 5, 6, 8, and 9 include two-way interactions with lagged district cases and lagged learning mode with republican vote share and COVID color severity at baseline. Models under columns 3, 6, and 9 also include lagged cumulative first vaccine doses per 100 county residents in those interactions. District covariates include log enrollment, indicators for urbanicity, district sociodemographic variables, controls for initial county COVID severity, and Republican vote share (we include in our model but do not report estimates from lagged learning mode). Four districts were missing district achievement in 2018, so we impute 0 for these and include a missing indicator variable. Standard errors are clustered by school district. *** p<0.01, ** p<0.05, * p<0.1.

In column 3, we include the a main effect for the lagged cumulative first doses of vaccines and an interaction between lagged cumulative vaccines and lagged cases. The purpose of this specification is to explore the extent to which the changes we observe over time are due to the increasing availability of vaccines. The estimated main effect of lagged vaccine doses is a positive 0.003, suggesting that district-week observations with higher vaccination levels are more likely to provide in-person instruction. However, the increasing prevalence of vaccines does *not* appear to explain the pattern of diminishing importance of lagged cases over time. The coefficient on lagged cases x week actually increases slightly, from .002 to .004.

The advantage of using week to proxy for district experience is that it does not depend on any district decisions. In the Bayesian learning model described above, however, the information provided by new cases will be decreasing in the number of prior *signals*, which in our case translates to the number of past weeks the district has offered in-person instruction. For this reason, columns 4-6 use the twice lagged cumulative count of the weeks a district was in-person as the proxy for district learning. Columns 7-9 use the cumulative *cases* a district experienced while in-person as a third potential proxy of experience. Because a district's prior decisions regarding in-person instruction are clearly endogenous, it is particularly important to control for other time-varying, district-specific confounds when using these alternative proxies for district experience.

The estimates for these specifications (columns 4-9) reveal qualitatively similar patterns as those discussed above. Namely, the marginal effect of lagged cases diminishes as district experience with COVID-19 increases. Appendix Table A3 reports results for specifications that include these interactions together. We find that the interaction between lagged cases and week remains positive and significant even though the interactions between lagged cases and the other experience proxies (cumulative in-person and cumulative cases while in-person) are not significant. In Appendix Table A4, we redo the exercise above, but also include lagged county cases (main effects and interactions with the various experience proxies). Including county cases reduces the main effect of lagged district cases estimate by almost one-half, but the point estimate on the interaction between lagged district cases and week remains positive and significant. These estimates remain robust even when we add in measures to capture district heterogeneity in column 2 (Republican vote share and baseline county-level color indicators of COVID-19 severity) and cumulative vaccination counts (column 3). The negative sign on lagged county cases and the positive sign on the interaction between lagged county cases and week suggests that districts also become less responsive to county cases over time.

5.2.2 Peer Effects

We next investigate whether a district's behavior is influenced by its peers. Recall that in our model a district's learning mode choices can be influenced by its peers through two channels: either through information—learning about how another district's decision to offer in-person instruction affected their case rates—or through peer pressure. In this section, we examine both channels.

We define peers in three ways: 1) districts within 20 miles of a district's centroid, 2) districts that share the same urbanicity (rural, town, suburban, or urban) across the state, and 3) districts that share the same urbanicity within a commuting zone.[25] We choose 20 miles because the average person commutes just over 15 miles for work (McGuckin, 2021) and because this was the minimum distance at which each district had at least one peer. Our urbanicity-based definitions were motivated by discussions with district officials who mentioned being in regular contact with demographically similar districts during the school year. For this definition, we define a district as a peer if they share the same urbanicity. For example, Akron City and Cincinnati City are two large urban districts that we label as peers even though they are over 200 miles away. We use a stricter definition of peers where we restrict peers to districts sharing the same urbanicity within a commuting zone. In this case, Akron City and Cincinnati City would not be considered peers, but Akron City and Cleveland City, which are just 40 miles apart, would be considered peers.

We estimate equation 8, which includes additional predictors that measure the lagged new cases in peer districts as well as the lagged learning mode choices of peer districts. We present these results in Table 10. Across all three definitions of peers we consistently find that peer case counts do *not* predict a district's future learning mode, but that lagged peer learning mode choices *are* associated with a district's own future choice.

Focusing first on column 1, the coefficient of 0.072 on fraction of peer districts that were in-person last week implies that as the fraction of districts within 20 miles teaching in-person increases

[25] Commuting zones proxy for regional labor markets where people live, work, and commute. Researchers have used them to describe geographic labor market and economic patterns (e.g., Chetty et al., 2014; Chetty and Hendren, 2018; Autor and Dorn, 2013). Ohio is comprised of 20 commuting zones, and the average commuting zone contains about 30 districts.

Table 10. Predicting In-Person Instruction With Peer effects

	(1)	(2)	(3)
	Within 20 mi	Urbanicity	Urbanicity Within Commuting Zone
Lagged cases (per 1,000)	-0.023***	-0.023***	-0.023***
	(0.003)	(0.003)	(0.003)
* lagged virtual	0.039***	0.039***	0.041***
	(0.008)	(0.008)	(0.008)
* lagged hyrbrid	0.027***	0.028***	0.029***
	(0.005)	(0.005)	(0.005)
Lagged peers' cases in-person (per 1,000)	0.003	-0.017	0.008
	(0.005)	(0.018)	(0.005)
Lagged peers' cases virtual (per 1,000)	0.001	-0.007	-0.002
	(0.004)	(0.010)	(0.004)
Lagged peers' cases hybrid (per 1,000)	0.001	0.022	-0.007
	(0.003)	(0.022)	(0.004)
Lagged fraction of peers in-person	0.072***	0.140**	0.023
	(0.018)	(0.069)	(0.016)
N	14,472	14,472	14,328
R-squared	0.779	0.778	0.779

Note: Table reports OLS estimates of equation 8 for selected variables and includes week fixed effects but not county fixed effects (we include in our model but do not report estimates from lagged learning mode). Sample excludes clusters that are always in person or mainly hybrid or virtual. Sample average of in-person instruction is 0.64. Lagged cases include both student and staff cases. District covariates include log enrollment, indicators for urbanicity, district sociodemographic variables, controls for initial county COVID severity, and Republican vote share. Standard errors are clustered by county. Columns examining urbanicity within commuting zones have a smaller sample size because of the lack of variation of learning modes within commuting zones during certain weeks. *** p<0.01, ** p<0.05, * p<0.1.

by 10 percentage points—roughly equivalent to 2 additional peers—the likelihood of teaching in-person increases by 0.7 percentage points. This effect is highly significant, but given the sample mean of the outcome is 0.64, it is relatively small. In contrast, the coefficients on the lagged peer case variables are all small and statistically indistinguishable from zero.

When we define peers as sharing urbanicity, we find even stronger results for the fraction of peers in-person in the prior week, but still find small and non-significant effects for peer case counts in the prior week. As the fraction of districts sharing the same urbanicity across the state increases by 10 percentage points—roughly equivalent to 22 additional peers—the likelihood of teaching in-person increases by 1.4 percentage points. When we define peers based on urbanicity within a commuting zone, neither fraction peers in-person nor peer case rates are significant predictors of a district's learning mode decision. While the results are somewhat sensitive to how one defines a district's peers, these findings seem to suggest that while districts do not learn from their peers' experience with in-person instruction, they do feel pressure by their peers' learning mode decisions.

6 Conclusion

This paper examines school district reopening decisions during the COVID-19 pandemic. We assemble a data set that combines weekly information on school district learning mode with weekly COVID-19 health measures, along with a variety of district characteristics and other covariates. Using these data, we study districts' choice of whether and when to offer in-person, virtual, or hybrid instruction during the 2020-21 school year.

To begin, we document substantial variation in learning mode decisions across districts and over time. Consistent with prior research on school re-openings (DeAngelis and Makridis, 2021; Hartney and Finger, 2020), we find that Republican-leaning districts were more likely to be in-person for the school year, whereas urban and more Democratic districts were more likely to spend much of the year virtual or hybrid. However, we demonstrate that district characteristics only explain a modest share of the observed variation. While some districts were always in-person (21%) or never returned to in-person learning (13%), almost one-third of the remaining districts changed their learning mode 4 or more times during the school year.

We then develop a model of district decision-making that incorporates uncertainty about how in-person schooling influences pandemic disease transmission and allows for district learning over time. The predictions from this model provide us with a novel way to assess the quality of school district decision-making. Consistent with our model predictions, our results suggest districts respond to health risks. We find that districts are 2.3 percentage points (3.5%) less likely to be in-person the following week after an increase in 1 case per 1,000 students and staff while the district was in-person. This remains consistent across a range of model specifications, even when we control for county-level COVID severity. Furthermore, we find that the effect of lagged cases wanes over time. This is consistent with our learning model, which implies that as districts accumulate experience offering in-person instruction, their uncertainty about the relationship between in-person instruction and subsequent cases falls. Finally, we show that districts are influenced by the decisions of their peers via a "pressure" channel, but do not appear to be "learning" from their peers in the way they learn from their own experiences. Together, these results suggest that, on average, school districts acted as boundedly rationale decision-makers during the COVID-19 pandemic.

Our research makes important contributions along two dimensions. First, we extend the existing evidence on school re-opening decisions during COVID-19. Prior work has mainly focused on districts' *initial choice* of learning mode, finding that demographics and politics drive district decisions. Our paper is distinct in examining the *within-year* changes in learning mode. Using this variation, we find that COVID-related health risks are indeed important for explaining learning mode choices. Second, our study makes a valuable contribution to the understanding of school boards more generally. Despite the large role they play in K-12 schooling, there is little compelling evidence on how they make decisions or how their decisions influence critical outcomes. Our study takes advantage of the data on a set of high-profile and consequential district decisions to evaluate the decision-making process, illustrating how districts use new information to guide important choices.

Our findings suggest several possibilities for future research. Given the evidence we find on districts *on average*, it is natural in subsequent studies to focus on the heterogeneity of decision-making across districts. How much do districts vary in the quality of their decisions, and what characteristics are associated with this variation? To gain more insight into the *process* of decision-making, researchers might use natural language processing techniques to study written documents such as school board meeting notes or board reports. As school boards engage in a wider range of cultural, political and educational issues, knowledge of how they operate will become even more valuable.

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Appendix Tables

Table A1. Predicting In-Person Instruction: Additional Lags and Cummulative Cases

	(1)	(2)	(3)
	Twice lagged cases	Cumulative in-person cases	Twice lagged & cum. cases
Lagged cases (per 1,000)	-0.014***	-0.022***	-0.014***
	(0.003)	(0.003)	(0.003)
* lagged virtual	0.016*	0.037***	0.015
	(0.009)	(0.007)	(0.010)
* lagged hyrbrid	0.007	0.026***	0.010*
	(0.005)	(0.004)	(0.006)
Twice lagged cases (per 1,000)	-0.012***		-0.008**
	(0.004)		(0.004)
* lagged virtual	0.020**		0.011
	(0.010)		(0.011)
* lagged hybrid	0.029***		0.016**
	(0.006)		(0.007)
Twice lagged cum. cases in-person (per 1,000)		-0.001***	-0.001***
		(0.000)	(0.000)
* lagged virtual			0.007***
			(0.003)
* lagged hybrid			0.010***
			(0.002)
N	14,070	14,070	14,070

Note: Table reports estimates of variants of equation 6 for selected variables (we include in our model but do not report estimates from lagged learning mode). Column (1) includes twice lagged case counts and interactions of case counts and district characteristics with lagged learning mode, column (2) includes the cumulative number of cases in-person (twice lagged), and column (3) combines columns (1) and (2) and interacts the cumulative number of cases in-person and twice lagged cases in-person with lagged learning mode. Sample excludes clusters that are always in person or mainly hybrid or virtual. Sample average of in-person instruction is 0.64. Lagged cases include both student and staff cases. District covariates include log enrollment, indicators for urbanicity, district sociodemographic variables, controls for initial county COVID severity, and Republican vote share. Four districts were missing district achievement in 2018, so we impute 0 for these and include a missing indicator variable. Standard errors are clustered by county. *** p<0.01, ** p<0.05, * p<0.1.

Table A2. The Effect of Lagged Cases on In-Person Instruction: County-level COVID Severity

	(1)	(2)	(3)	(4)	(5)
Lagged cases (per 1,000)	-0.036***	-0.018***	-0.008***	-0.017***	-0.014***
	(0.005)	(0.002)	(0.003)	(0.003)	(0.003)
* lagged virtual	0.073***	0.036***	0.017**	0.035***	0.027***
	(0.012)	(0.006)	(0.008)	(0.008)	(0.008)
* lagged hyrbrid	0.051***	0.025***	0.008	0.022***	0.018***
	(0.007)	(0.003)	(0.005)	(0.004)	(0.005)
Lagged county cases (per 1,000)			-0.021**		
			(0.009)		
* lagged virtual			0.051***		
			(0.009)		
* lagged hybrid			0.043***		
			(0.007)		
Lagged county indicator (1-7)				-0.023***	
				(0.007)	
* lagged virtual				-0.020	
				(0.016)	
* lagged hybrid				0.042***	
				(0.011)	
Lagged county orange					-0.041**
					(0.019)
* lagged virtual					0.048
					(0.088)
* lagged hybrid					0.052
					(0.034)
Lagged county red+					-0.063***
					(0.022)
* lagged virtual					0.205**
					(0.096)
* lagged hybrid					0.160***
					(0.033)
Lagged cases over past 2 weeks	Mean	Total	Total	Total	Total
H_0: lagged cases while virtual = 0 (P-value)	0.002	0.002	0.252	0.018	0.103
H_0: lagged cases while hybrid = 0 (P-value)	0.021	0.021	0.925	0.144	0.345
R-squared	0.640	0.640	0.647	0.642	0.643

Note: Table reports OLS estimates of equation 6 for selected variables with added controls for county-level COVID severity (over the prior two weeks). We include in our models but do not report estimates from lagged learning mode. N = 14,070 school district-week observations. Sample excludes districts that were always in-person or mainly hybrid or virtual. Sample average of in-person instruction is 0.64. Lagged cases include both student and staff cases. All columns include week fixed effects, district covariates, and county fixed effects. County orange = 2-3 COVID severity indicators met and County red+ = 4+ indicators met. District covariates include log enrollment, indicators for urbanicity, district sociodemographic variables, controls for initial county COVID severity, and Republican vote share. Four districts were missing district achievement in 2018, so we impute 0 for these and include a missing indicator variable. Standard errors are clustered by county. *** p<0.01, ** p<0.05, * p<0.1.

Table A3. The Effect of Lagged Cases on In-Person Instruction: Heterogeneity Over Time, Full Interactions

	(1)	(2)
	Cum. in-person, week, & vax	Cum. cases in-person, week, & vax
Lagged cases (per 1,000)	-0.010	-0.016***
	(0.007)	(0.006)
* week	0.005***	0.005***
	(0.001)	(0.001)
* twice lagged cum. in-person	-0.000	
	(0.000)	
* twice lagged cum. cases in-person		0.000
		(0.000)
* lagged cum. first vax dose	-0.001***	-0.001**
	(0.001)	(0.001)
Twice lagged cum. in-person	-0.002***	
	(0.000)	
Twice lagged cum. cases in-person		-0.001***
		(0.000)
Lagged cum. first vax dose	0.000	0.001
	(0.001)	(0.001)

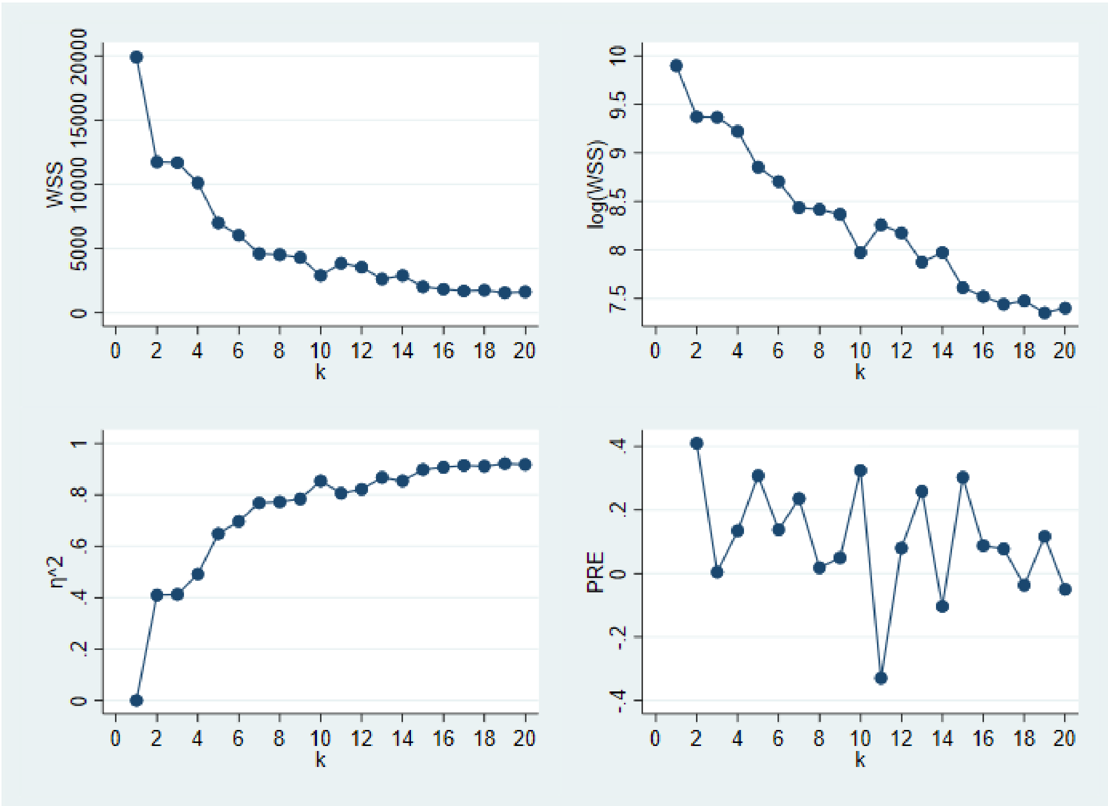
Note: Table reports OLS estimates of equation 6 for selected variables with additional interaction terms (we include in our model but do not report estimates from lagged learning mode). N = 14,070 school district-week observations. Sample excludes districts that were always in-person or mainly hybrid or virtual. Sample average of in-person instruction is 0.64. Lagged cases include both student and staff cases. All columns include week fixed effects, district covariates, and county fixed effects. Column 1 includes two-way interactions with lagged district cases and lagged learning mode with republican vote share, COVID color severity at baseline, week, twice lagged cumulative times a district was in-person and lagged cumulative first vaccine doses. Column 2 includes two-way interactions with lagged district cases and lagged learning mode with republican vote share, COVID color severity at baseline, week, twice lagged cumulative cases in-person, and lagged cumulative first vaccine doses. District covariates include log enrollment, indicators for urbanicity, district sociodemographic variables, controls for initial county COVID severity, and Republican vote share (we include in our model but do not report estimates from lagged learning mode). Four districts were missing district achievement in 2018, so we impute 0 for these and include a missing indicator variable. Standard errors are clustered by school district. *** p<0.01, ** p<0.05, * p<0.1.

Table A4. The Effect of Lagged Cases on In-Person Instruction: Heterogeneity Over Time With County Cases

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Lagged cases (per 1,000)	-0.014***	-0.008	-0.006	-0.028***	-0.022***	-0.018**	-0.018***	-0.010	-0.013*
	(0.003)	(0.006)	(0.006)	(0.006)	(0.008)	(0.009)	(0.005)	(0.007)	(0.007)
* week	0.002***	0.002***	0.005***						
	(0.000)	(0.000)	(0.001)						
* twice lagged cum. in-person				0.001***	0.001***	0.001*			
				(0.000)	(0.000)	(0.000)			
* twice lagged cum. cases in-person							0.000*	0.001**	0.000
							(0.000)	(0.000)	(0.000)
* lagged cum. first vax dose			-0.002***			0.001***			0.001***
			(0.001)			(0.000)			(0.000)
* log(repub vote share) (normalized)		-0.018	-0.016		-0.032*	-0.026		-0.034*	-0.023
		(0.016)	(0.016)		(0.018)	(0.016)		(0.019)	(0.016)
* county orange (baseline)		-0.004	-0.000		-0.004	-0.004		-0.006	-0.005
		(0.006)	(0.006)		(0.006)	(0.006)		(0.007)	(0.006)
* county red+ (baseline)		-0.015	-0.011		-0.011	-0.016		-0.014	-0.017
		(0.010)	(0.010)		(0.011)	(0.011)		(0.011)	(0.011)
Lagged county cases (per 1,000)	-0.008**	-0.013**	-0.027***	0.001	-0.005	-0.007	-0.012***	-0.016***	-0.021***
	(0.004)	(0.005)	(0.006)	(0.005)	(0.006)	(0.006)	(0.004)	(0.005)	(0.006)
* week	0.002***	0.002***	0.001						
	(0.000)	(0.000)	(0.001)						
* twice lagged cum. in-person				-0.001***	-0.001**	-0.001***			
				(0.000)	(0.000)	(0.000)			
* twice lagged cum. cases in-person							0.000**	0.000**	0.000
							(0.000)	(0.000)	(0.000)
* lagged cum. first vax dose			0.001**			0.001***			0.001***
			(0.000)			(0.000)			(0.000)
* log(repub vote share) (normalized)		-0.000	0.002		0.005	0.007		0.003	0.001
		(0.010)	(0.010)		(0.012)	(0.012)		(0.011)	(0.011)
* county orange (baseline)		0.004	0.005		0.005	0.005		0.004	0.005
		(0.004)	(0.004)		(0.004)	(0.004)		(0.004)	(0.004)
* county red+ (baseline)		0.011*	0.012*		0.012*	0.011*		0.013**	0.013**
		(0.006)	(0.006)		(0.006)	(0.007)		(0.006)	(0.006)
Twice lagged cum. in-person				-0.002***	-0.003***	0.000			
				(0.000)	(0.000)	(0.001)			
Twice lagged cum. cases in-person							-0.002***	-0.002***	-0.001***
							(0.000)	(0.000)	(0.000)
Lagged cum. first vax does			-0.001			-0.003***			-0.002***
			(0.001)			(0.001)			(0.001)

Note: Table reports OLS estimates of equation 6 for selected variables with additional interaction terms (we include in our model but do not report estimates from lagged learning mode). $N = 14{,}070$ school district-week observations. Sample excludes districts that were always in-person or mainly hybrid or virtual. Sample average of in-person instruction is 0.64. Lagged cases include both student and staff cases and lagged county cases are measured over the prior two weeks. All columns include week fixed effects, district covariates, and county fixed effects. County orange = 2-3 COVID severity indicators met and County red+ = 4+ indicators met. Models under columns 2, 3, 5, 6, 8, and 9 include two-way interactions with lagged district cases and lagged learning mode with republican vote share and COVID color severity at baseline. Models under columns 3, 6, and 9 also include lagged cumulative first vaccine doses per 100 county residents in those interactions. District covariates include log enrollment, indicators for urbanicity, district sociodemographic variables, controls for initial county COVID severity, and Republican vote share (we include in our model but do not report estimates from lagged learning mode). Four districts were missing district achievement in 2018, so we impute 0 for these and include a missing indicator variable. Standard errors are clustered by school district. *** p<0.01, ** p<0.05, * p<0.1.

Appendix Figures



Note: The following is a scree plot from conducting a k-mean cluster matching algorthim based on the percentage of weeks a districts was in-person during fall 2020, the last week that the district was **not** in-person, and the total number of times the district changed learning modes over the course of the year. The top two panels show the within sum of squares (WSS) for k clusters. The panel on the bottom left shows η^2, which measures the proportional reduction of the WSS for each cluster k, and the panel on the bottom right shows the proportional reduction of error coefficient by cluster. We adapt Stata code that was generously made available from Makles (2012).

Appendix Figure. 1. KNN Clusters Scree Plot