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In July 2021, the Centers for Disease Control and Prevention (CDC, 2021) updated its guidance for COVID-19 Prevention in K-12 Schools. The updated guidance places a high priority on schools being fully open—with adequate safety protocols—for the 2021-22 school year. The priority on reopening is attributable, at least in part, to evidence that in-person learning is superior to online or hybrid learning. For instance, parents expressed greater satisfaction and less concern with in-person than online learning (Rapaport et al, 2020) and evidence from before and during the pandemic suggests students' learning was greater in in-person settings (e.g., Chetty et al, 2020; Domingue et al., 2021; Kaufman & Diliberti, 2021).

Yet opening schools will not actually result in all students retuning to in-person learning. A non-trivial proportion of families—15% according to nationally representative Understanding America Study (UAS) data collected in July, 2021—are "school hesitant," either planning to keep their child online in the fall or are unsure about online versus in-person learning (http://uasdata.usc.edu/education). Furthermore, school hesitant parents are disproportionately from already marginalized groups (Black and Hispanic families are 26% and 18% hesitant respectively).

Parents have a variety of reasons for preferring to keep their children home. Researchers and journalists have posed several narratives about why parents keep children home including fears about COVID-19, lack of trust in schools that had underserved their children pre-pandemic, the inadequacy of school physical infrastructure, and school closures (Camp & Zamarro, 2021; Kogan, 2021; Economist, 2021). Recent UAS data (Saavedra et al., 2021) suggests the primary reasons parents reported for keeping their children at home included those related to fit (e.g. child was happier and/or more academically successful at home) and safety. Among parents who did not send their child/ren to school in April/May 2021, 27% selected reasons related to fit, 28% to safety, and 33% to both fit and safety (the other 12% selected a variety of other reasons).

Since policymakers and practitioners want to encourage school-hesitant parents to send their children back to in-person school in the 2021-22 school year, we wondered whether well-targeted messages might address parents' concerns and increase the likelihood that parents would send their children back. We created a brief messaging intervention and tested it using a nationally representative sample of K-12 parents. Our results indicate that messaging targeted at the primary concerns of school-hesitant parents—those related to fit and safety—could substantially increase parents' likelihood to report they will send their children back in person in the fall. We found the messaging intervention worked well for parents who reported they were "unsure" at baseline, but did not work for parents reported at baseline they were not planning to send their children back in person.

Methods

Since 2014, the nationally representative, longitudinal UAS panel study has been surveying U.S. households about a range of issues including economics, health, and politics. At the onset of the pandemic, we began collecting data about families' K-12 education experiences related to COVID-19. Since spring 2020, we have collected over 20 waves of survey data on a range of education topics. For this analysis, we use data collected between June 9 and July 21, 2021 (UAS348). Details on the sample, including comparisons to national demographics and our data cleaning procedures, are available in the online appendix.

For our messaging intervention, we randomized parents into four groups and delivered respondents one of four messages (the full messaging texts are available in the online appendix):

1) A basic control message, simply welcoming parents and their children back to school and indicating the school would comply with CDC guidelines.
2) A specific "safety" message, that adds to (1) plans to describe specific safety protocols, vaccination rates, and COVID rates in the community.
3) A specific "fit" message, that adds to (1) an acknowledgment of the academic and nonacademic effects of the pandemic on children and encouragement to address those effects by sending their children back in person.
4) A specific "combined" message, that adds to (1) both the safety and fit messages.

After the message, we asked participants "On a scale from 1 to 10, how likely would you be to send [NAME] to school full-time, in-person after reading this message?"

To analyze our experimental data, we first assigned respondents to one of three "baseline" groups based on their response to an earlier question on the survey in the summer of 2021, "Are you planning to send [NAME] to school in-person at the beginning of the 2021-22 school year?" (with response options "yes", "no", and "unsure"). We then regressed a binary indicator of whether the respondent planned to send their child back to school in-person or not (as measured post-message)[1] on the two treatment indicators[2], parents' previous plans, and their interaction, with the control message as the reference category.[3] We also fit models that included additional covariates (e.g., parent race/ethnicity, parent educational attainment, household income, urbanicity), different ways of binning the 1-10 scale, and two model types. We report results from the most parsimonious model here, documenting additional analyses and their results in the online appendix. The primary results from the most parsimonious model were substantively unchanged in each of our sensitivity analyses.

Results

The results, shown in Figure 1 (with corresponding Table A1 in the online appendix), indicate the treatments had no effect on parents who said "yes" at baseline, negative but nonsignificant effects on parents who said "no" at baseline, and positive and statistically significant effects on parents who said "unsure" at baseline. The magnitude of the effects on "unsure" parents was substantial— reading a specialized message rather than the control message increased the probability of parents who reported being likely to send their child in person in the fall by about 26 percentage points, from 50% to 76% for unsure parents. Analyzing the outcome in different ways—for instance, treating it as a continuous variable, dichotomizing it to >5 versus 5 or less—produced the same pattern of results, though the effect was insignificant at the .05 level if the outcome was treated continuously.

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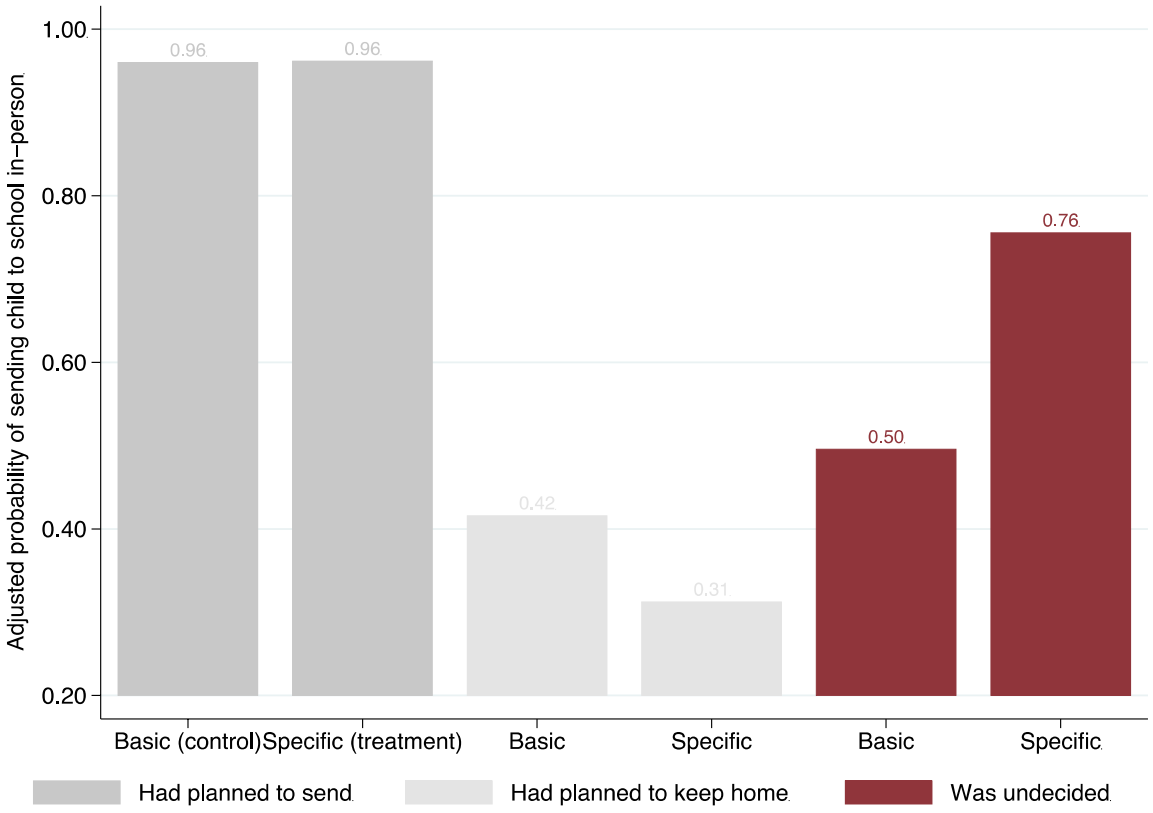
[1] We excluded the mid-point (5) from this outcome measure since it conveys uncertainty about parents' intention. Thus, the parents who selected a likelihood of sending their kids back in-person between 1-4 were coded as "0", and those who selected a likelihood of 6-10 were coded as "1". We present alternative scenarios in table A2. Including the mid-point in the reference category (models 4 and 5, table A2) yields results that are consistent with our primary method of operationalizing the response variable.

[2] For concision, our primary results reflect the treatment condition collapsed into two groups: those who received the control message, or "Basic" message, and those who received one of three treatment messages, or the "Specific" message. The differential effect for respondents who were undecided is insensitive to this choice; the findings are reported in Table A1, models 5 and 6.

[3] We report sample average treatment effects (SATEs) consistent with Miratrix et al. (2018), and do not apply sampling probability or post-stratification weights to generalize to the population of parents.

Figure 1: Predicted probability of parents reporting they would send their child to school in-person at the beginning of the 2021-22 school year after exposure to messaging, by previous plans



Note. Adjusted probabilities derived from a logistic regression predicting the log odds of a parent indicating that they will send their child back to school in-person in Fall 2021, conditional on the interaction between their previous plans and randomized exposure to a specific messaging manipulation. The specific messaging condition included messages that emphasized the COVID safety protocols schools have implemented, and the impact of the pandemic on students' academic and non-academic well-being. Additional covariates included in the statistical model included: household income (quintile), region of the county, parent gender, parent race/ethnicity, parent education, and parent age. Red bars indicate a statistically significant different ($p<.05$) between experimental conditions. Full model results are reported in Table A1 in the Appendix).

Discussion

Our results demonstrate that a brief messaging intervention targeted at hesitant (i.e. "unsure") parents' most serious concerns can affect their reported likelihood to send their children to in-person school. These findings have clear implications for districts seeking to boost in-person enrollment. Identifying parents' sources of concern and providing clear messaging to assuage those concerns is a relatively easy, low-cost intervention that could meaningfully boost student enrollment. Policymakers and practitioners seeking to message in these ways should align their messages with best practices from the messaging literature, for instance Oreopoulos (2020). Future work should continue to probe ways to support communication with parents during and after the COVID-19 pandemic.

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Supporting Materials

Figure A1: Covariate balance by experimental condition

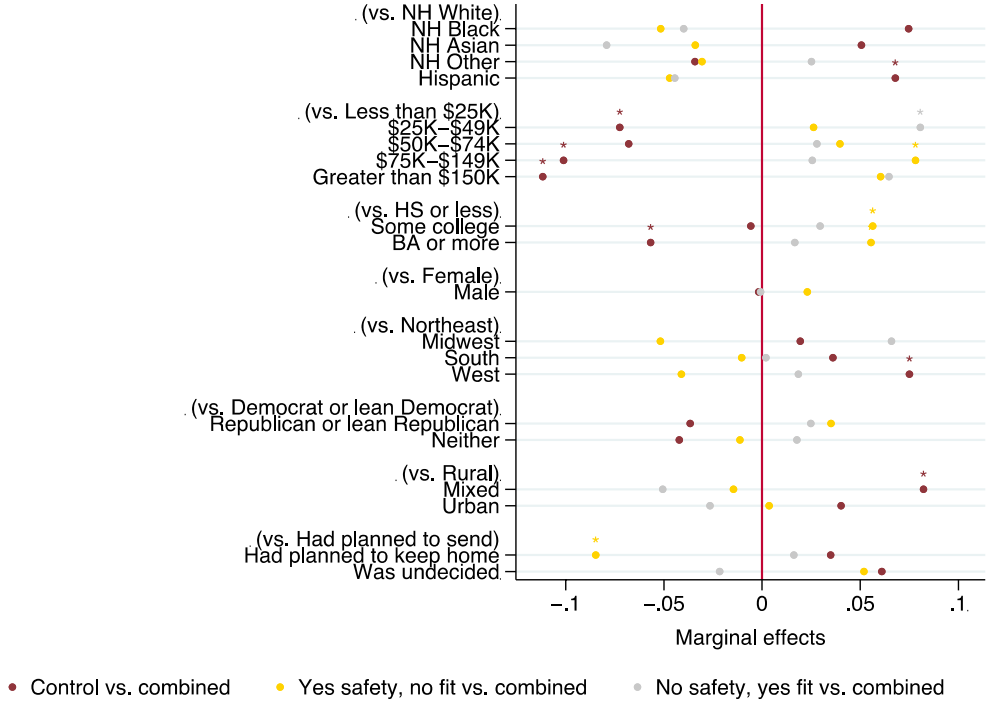

Table A1: Effect of UAS348 manipulation experiment on parents' likelihood of sending their children to school in person

	1	2	3	4	5	6	7	8
Specific msg								
Coefficient	-0.37	-0.45	0.03	-0.08				
Std. error	(0.50)	(0.54)	(0.17)	(0.18)				
Had planned to send								
Coefficient	3.63^	3.52^			3.63^	3.52^		
Std. error	(0.52)	(0.55)			(0.52)	(0.55)		
Was undecided								
Coefficient	0.51	0.32			0.51	0.31		
Std. error	(0.61)	(0.65)			(0.61)	(0.66)		
Specific msg x Had planned to send								
Coefficient	0.54	0.49						
Std. error	(0.62)	(0.65)						
Specific msg x Was undecided								
Coefficient	1.28#	1.60*						
Std. error	(0.73)	(0.78)						
NH Black								
Coefficient		-0.05		-0.13		-0.04		-0.14
Std. error		(0.39)		(0.26)		(0.39)		(0.26)
NH Asian								
Coefficient		0.38		0.03		0.38		0.02
Std. error		(0.59)		(0.42)		(0.59)		(0.42)
NH Other								
Coefficient		-0.32		-0.14		-0.29		-0.14
Std. error		(0.52)		(0.34)		(0.53)		(0.34)
Hispanic								
Coefficient		-0.57#		-0.51*		-0.57#		-0.52*
Std. error		(0.33)		(0.21)		(0.33)		(0.21)
$25K-$49K								
Coefficient		0.52		0.81^		0.55		0.82^
Std. error		(0.33)		(0.22)		(0.33)		(0.22)
$50K-$74K								
Coefficient		0.49		0.78^		0.53		0.78^
Std. error		(0.38)		(0.24)		(0.38)		(0.24)
$75K-$149K								
Coefficient		0.65#		1.00^		0.73*		1.00^
Std. error		(0.36)		(0.23)		(0.37)		(0.23)
Greater than $150K								
Coefficient		1.02*		1.30^		1.11*		1.31^
Std. error		(0.51)		(0.34)		(0.53)		(0.34)
Some college								
Coefficient		-0.27		-0.04		-0.29		-0.03
Std. error		(0.29)		(0.19)		(0.29)		(0.19)
BA or more								
Coefficient		0.16		0.52*		0.13		0.53*
Std. error		(0.36)		(0.23)		(0.37)		(0.23)
35-54								
Coefficient		0.14		0.18		0.13		0.18
Std. error		(0.29)		(0.19)		(0.29)		(0.20)
55-64								
Coefficient		-0.14		-0.43		-0.10		-0.42
Std. error		(0.43)		(0.27)		(0.43)		(0.27)
65+								
Coefficient		0.17		-0.13		0.21		-0.12
Std. error		(0.51)		(0.35)		(0.52)		(0.35)
Male								
Coefficient		0.00		-0.10		0.01		-0.10
Std. error		(0.26)		(0.17)		(0.26)		(0.17)
Midwest								

Coefficient		0.58		0.38		0.64		0.40
Std. error		(0.45)		(0.30)		(0.45)		(0.30)
South								
Coefficient		0.61		0.18		0.64		0.19
Std. error		(0.42)		(0.28)		(0.43)		(0.28)
West								
Coefficient		0.58		0.19		0.62		0.21
Std. error		(0.42)		(0.29)		(0.42)		(0.29)
Yes safety, no fit								
Coefficient					-1.10	-1.41	0.07	-0.07
Std. error					(0.88)	(0.93)	(0.21)	(0.22)
No safety, yes fit								
Coefficient					-0.24	-0.41	-0.06	-0.19
Std. error					(0.60)	(0.64)	(0.21)	(0.21)
Yes safety, yes fit								
Coefficient					-0.24	-0.14	0.08	0.01
Std. error					(0.58)	(0.63)	(0.21)	(0.22)
Yes safety, no fit x Had planned to send								
Coefficient					1.22	1.39		
Std. error					(0.99)	(1.03)		
Yes safety, no fit x Was undecided								
Coefficient					2.20*	2.80*		
Std. error					(1.08)	(1.14)		
No safety, yes fit x Had planned to send								
Coefficient					0.41	0.41		
Std. error					(0.75)	(0.78)		
No safety, yes fit x Was undecided								
Coefficient					0.94	1.34		
Std. error					(0.87)	(0.92)		
Yes safety, yes fit x Had planned to send								
Coefficient					0.45	0.26		
Std. error					(0.73)	(0.78)		
Yes safety, yes fit x Was undecided								
Coefficient					1.15	1.24		
Std. error					(0.94)	(1.00)		
Intercept								
Coefficient	-0.51	-1.21#	1.79^	0.92^	-0.51	-1.28#	1.79^	0.90*
Std. error	(0.42)	(0.66)	(0.15)	(0.35)	(0.42)	(0.66)	(0.15)	(0.35)
Observations	1,268	1,265	1,519	1,515	1,268	1,265	1,519	1,515

^p<.01; p<.05;# p<.1.*

Appendix 2: Effect of UAS348 manipulation experiment on parents' likelihood of sending their children to school in person, alternative methods of constructing the outcome measure

	(1:1-10)	(2:1-4;5;6-10)	(3:1-4;5-10)	(4:1-5;6-10)	(5:1-6;7-10)
Specific msg					
Coefficient	-0.42	-0.45	-0.35	-0.46	-0.74
Std. error	(0.47)	(0.49)	(0.51)	(0.52)	(0.55)
Had planned to send					
Coefficient	4.71^	3.10^	3.43^	2.74^	2.56^
Std. error	(0.42)	(0.46)	(0.53)	(0.48)	(0.49)
Was undecided					
Coefficient	1.06*	0.58	1.35*	-0.46	-0.67
Std. error	(0.52)	(0.50)	(0.57)	(0.58)	(0.61)
Specific msg x Had planned to send					
Coefficient	0.40	0.64	0.40	0.64	0.88
Std. error	(0.49)	(0.55)	(0.62)	(0.58)	(0.59)
Specific msg x Was undecided					
Coefficient	1.32*	1.07#	0.60	1.72*	1.85*
Std. error	(0.62)	(0.62)	(0.69)	(0.69)	(0.73)
NH Black					
Coefficient	-0.32	-0.32	-0.13	-0.26	-0.24
Std. error	(0.20)	(0.29)	(0.37)	(0.30)	(0.28)
NH Asian					
Coefficient	-0.42#	-0.29	0.34	-0.34	-0.70#
Std. error	(0.25)	(0.39)	(0.56)	(0.41)	(0.36)
NH Other					
Coefficient	-0.24	-0.21	-0.22	-0.20	-0.12
Std. error	(0.25)	(0.40)	(0.50)	(0.42)	(0.40)
Hispanic					
Coefficient	-0.50^	-0.46#	-0.36	-0.55*	-0.46#
Std. error	(0.16)	(0.25)	(0.32)	(0.26)	(0.25)
$25K-$49K					
Coefficient	0.46*	0.70^	0.41	0.76^	0.62*
Std. error	(0.18)	(0.25)	(0.32)	(0.26)	(0.24)
$50K-$74K					
Coefficient	0.72^	0.62*	0.34	0.75*	0.94^
Std. error	(0.19)	(0.28)	(0.36)	(0.29)	(0.28)
$75K-$149K					
Coefficient	0.88^	0.80^	0.46	0.92^	1.01^
Std. error	(0.18)	(0.27)	(0.34)	(0.28)	(0.26)
Greater than $150K					
Coefficient	1.07^	0.99^	0.64	1.29^	1.64^
Std. error	(0.22)	(0.39)	(0.49)	(0.41)	(0.39)
Some college					
Coefficient	-0.13	0.02	-0.13	-0.08	-0.04
Std. error	(0.15)	(0.21)	(0.27)	(0.23)	(0.21)
BA or more					
Coefficient	0.01	0.35	0.31	0.24	0.20
Std. error	(0.17)	(0.27)	(0.35)	(0.28)	(0.26)
35-54					
Coefficient	0.08	0.04	0.03	0.13	0.05
Std. error	(0.15)	(0.22)	(0.28)	(0.23)	(0.22)
55-64					
Coefficient	0.05	-0.15	-0.18	-0.08	0.12
Std. error	(0.23)	(0.33)	(0.41)	(0.35)	(0.34)
65+					
Coefficient	0.21	0.23	0.03	0.32	0.61
Std. error	(0.27)	(0.41)	(0.49)	(0.42)	(0.41)
Male					
Coefficient	-0.15	-0.06	0.09	-0.16	-0.12
Std. error	(0.12)	(0.19)	(0.25)	(0.20)	(0.19)

Midwest					
Coefficient	0.10	0.34	0.60	0.28	0.26
Std. error	(0.22)	(0.36)	(0.44)	(0.37)	(0.35)
South					
Coefficient	0.02	0.40	0.67	0.31	0.24
Std. error	(0.21)	(0.35)	(0.41)	(0.35)	(0.33)
West					
Coefficient	-0.07	0.36	0.62	0.28	0.22
Std. error	(0.21)	(0.34)	(0.41)	(0.35)	(0.33)
cut1					
Coefficient		0.76			
Std. error		(0.57)			
cut2					
Coefficient		1.58			
Std. error		(0.57)			
Intercept					
Coefficient	3.85^		-1.10#	-1.18*	-1.31*
Std. error	(0.47)		(0.64)	(0.59)	(0.58)
Observations	1,347	1,347	1,347	1,347	1,347
Estimation method	regress	ologit	logit	logit	logit

^p<.01; p<.05;# p<.1.*

Figure A2. Distribution of parents' reported likelihood of sending their child to school, by previous plan and experimental condition