# Education for All? A Nationwide Audit Study of School Choice[*]

Peter Bergman
Columbia University and NBER

Isaac McFarlin, Jr.
University of Florida and NBER

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**Abstract:** School choice may allow schools to impede access to students perceived as costlier to educate. To test this, we sent emails from fictitious parents to 6,452 charter schools and traditional public schools subject to school choice in 29 states and Washington, D.C. The fictitious parent asked whether any student is eligible to apply to the school and how to apply. Each email signaled a randomly assigned attribute of the child. We find that schools are less likely to respond to inquiries from students with poor behavior, low achievement, or a significant special need. Lower response rates to students with this special need are driven by charter schools. Otherwise, these results hold for traditional public schools, high value-added schools, including high-value added, urban charter schools.

**JEL Codes:** I20, I21, I24, I28.

## 1. Introduction

In the last 15 years, researchers have leveraged the randomization inherent in many school-choice admissions policies to demonstrate that certain schools have large impacts on test scores, college enrollment, risky health behaviors, and criminality (Hoxby and Rockoff, 2004; Deming, 2011; Angrist et al., 2013; Wong et al., 2014; Dobbie and Fryer, 2015). These studies, which have strong internal validity, examine the impacts on those who apply to a given set of schools. These estimates may change as schools expand, especially if they have been strategically altering the pool of applicants along hard-to-observe characteristics. We conduct a nationwide audit study to test whether schools attempt to manipulate their applicant pool by providing less application information to the parents of children perceived as more difficult or costlier to educate.

The concern that school choice enables schools to impede access for certain students is salient to policymakers (Cohodes and Dynarski, 2016).[1] To minimize this practice, regulators use lotteries and common applications to control admissions. While the educational costs of students with severe disabilities can exceed 10 times that of other students, the government provides financial offsets and requires that all districts serve these students—and procure their required services (Griffith, 2008). But frictions in the choice process may still allow schools to influence who applies. The most common concern families cite is a lack of information about schools' eligibility requirements (DeArmond et al., 2014), but families also lack information about school quality and admissions processes (Hastings and Weinstein, 2008; Kapor et al., 2019; Bergman et al., 2019).

While there are reports that schools take advantage of these frictions to hinder access for certain groups (Drame, 2011; Orfield et al., 2013; Welner, 2013), this behavior is difficult to detect. Differences in how families choose schools may reflect heterogeneous preferences rather than steering away applicants. Observational studies have focused on specific contexts using administrative data, which has limited information on student behaviors and needs (Lacireno-Paquet et al., 2002; Bifulco et al., 2009; Zimmer et al., 2009; Hoxby and Murarka, 2009; Zimmer and Guarino, 2013; Nichols-Barrer et al., 2015; Walters, 2018).[2] The findings are mixed.

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[1] There is also controversy in popular press on access to schools in areas with school choice. See "Are Charter Schools Cherry-Picking Students" in the *New York Times* (Dec. 10, 2014), which features a debate by policymakers on charter school access. In an article in the *Washington Post*, "The Masquerade of School Choice: A Parent's Story" (April 1, 2017), a parent describes her experience with racial discrimination and school choice.

[2] Several studies also look at voucher systems (Epple et al., 2017). Altonji et al. (2015) find little evidence that vouchers negatively impact students who remain in traditional public schools. Outside the United States, Hsieh and Urquiola (2006) show that the voucher system in Chile caused high-

We conduct a nationwide audit study to detect whether schools selectively provide application information to families.[3] Across two experiments, we sent emails from fictitious parents to 6,452 charter and traditional public schools subject to school choice. The parent asked whether any student is eligible to apply to the school and how to apply.[4] Each email signaled one of the following randomly-assigned attributes about the student: their disability status, poor behavior, high or low prior academic achievement, or no indication of these characteristics. We also randomly varied students' implied race, household structure, and gender. In the first experiment, we sent messages only to charter schools. Based on constructive feedback from researchers, we ran a second experiment, where we sent messages to both charter schools and traditional public schools (in areas practicing intra-district choice) to replicate our previous findings and to compare response rates between these two types of schools.

Our results were the same across both experiments: We find that schools respond less often to messages regarding students whom schools may perceive as more challenging to educate. The baseline response rate was 53 percent. Messages signaling that a student has a potentially severe special need were 5 percentage points less likely to receive a response than the baseline message. Messages signaling a behavior problem and messages indicating low prior achievement were 7 and 2 percentage points less likely to receive a response, respectively. A message indicating good grades and attendance, however, was neither more nor less likely to receive a response than the baseline message.

A key question is whether these results differ between traditional public schools subject to school choice and charter schools. Charter schools represent the fastest growing form of school choice in the country.[5] To answer this question, we matched charter schools to nearby traditional public schools subject to school choice. We find that, overall, traditional public schools' response rates are similar to the response rates from charter schools across treatment messages. However, there is a different response rate to messages that signal a child has a significant special need. Traditional public schools exhibit no differential response rate to these messages, but charter schools are 7 percentage points

less likely to respond to them than to the baseline message. This result is important because the differential cost of serving students with severe special needs cited above (Moore et al., 1988; Chambers, 1998; Collins and Zirkel, 1992, Griffith, 2008).[6]

These results hold for high-value-added schools, including urban, high-value added charter schools. Prior research has also shown that strict ("no-excuses") charter schools also tend to have high value added (Abdulkadiroglu et al., 2011; Angrist et al., 2013, 2016; Chabrier et al., 2016; Clark et al., 2015; Dobbie and Fryer, 2013; Dobbie and Fryer, 2015; Hoxby and Murarka, 2009). We identified 272 such schools in our data; these schools have a value-added one-half standard deviation above other charter schools. No-excuses charter schools are significantly less likely (10 percentage points) to respond to inquiries that signal a child's potentially significant disability than to the baseline message. We also explore heterogeneous effects by characterizing the "compliers" in our sample—those schools that discriminate in the sense they reply to an inquiry if sent a baseline message but do not reply if sent a particular treatment message.

We also present evidence that the monetary cost of serving students matters. States fund students with special needs in several different ways, including block grants designated for special education, cost-reimbursements for services rendered, and formulae that provide additional general funds to schools (Griffith, 2008; Millard and Aragon, 2015).[7] We find that charter schools in states, such as Wisconsin and Michigan, that reimburse districts for a large share of the realized cost of serving special-needs students exhibit no differential response rate to messages signaling a potentially high-cost special need.

Allowing charter schools to integrate with another Local Education Agency (LEA) could spread the risk of serving higher-cost students across multiple schools.[8] The legal obligation to provide services to students with special needs falls on the LEA. Integration between a charter school and a traditional-public school LEA could enable the schools to pool resources. We coded each charter school's LEA status based on states' LEA policies.[9] We find that LEA status does not moderate the differential response rate

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[6] Quality of response may matter as well. In a later section, we provide corroborating evidence that the messages traditional public schools send to parents of special needs students are viewed as more encouraging.

[7] Per-student education costs have steadily risen over time. Average expenditure per pupil for the 1990-1991 school year was $9,936. In 2016 dollars, this increased to $13,119 in the 2014-2015 school year (NCES Table 236.69). About 20 percent of the growth in new education spending is directed to special education services (Hanushek and Rivkin, 1997).

[8] A LEA is equivalent to a school district in most instances.

[9] LEA status varies within states because it can depend on what entity authorized the charter school (e.g. the state or a district).

to messages signaling a disability.

We find exploratory evidence of differences in response rates by the implied race of the family, but not by household structure. Schools may interpret these attributes as signals of families' socio-economic status. Overall, schools are 2 percentage points less likely to respond to emails signed by Hispanic-sounding names than to other messages. There is weaker evidence of a differential response rate for messages signed by Black-sounding names.

This paper makes several contributions to the literature on school choice. We provide the first experimental evidence that schools in areas with choice provide less application information to students whom they may perceive as harder to educate. Second, we incorporate signals of several student attributes—beyond race and gender—across a wide variety and a large number of schools. These features increase the external validity of our study and allow us to investigate an important dimension of heterogeneity comparing traditional public schools to charter schools.

Our results also have implications for interpreting studies that use evidence from schools' admission lotteries to examine school effectiveness. Our findings do not undermine the internal validity of lottery-based studies, but they underscore that lottery-based studies are conditional on the set of students who apply.[10] Hastings et al. (2006), Walters (2014) and Kline and Walters (2016) find that students who may benefit the most in terms of academic achievement are also the least likely to apply to high-performing schools or education programs. However, certain students—even those who may benefit most—may also impose high costs or negative behavioral spillovers (cf. Carrell and Hoekstra, 2010; Carrell et al., 2018) that reduce schools' demand to serve them. If the hardest-to-educate students were evenly distributed across schools, the impacts of highly-effective schools could decrease due to negative behavioral or fiscal spillovers.

Lastly, our research highlights the importance of providing transparent information to families to ensure all students have equal access in the choice process.[11] DeArmond et al. (2014) survey 4,000 parents across eight cities and find that the most common barriers in choosing a school is understanding eligibility requirements, followed by transportation issues and obtaining accurate information about

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[10] Fryer (2014) finds that the practices of high-performing charter schools, when installed in traditional public schools, produce comparable gains in achievement. Abdulkadiroğlu et al. (2016) find those with lower application costs to charter schools experience similar achievement gains to other students.

[11] For examples in the U.S. context, see Hoxby and Turner (2013) and Bergman (2015) in post-secondary and secondary education contexts.

school characteristics. Hastings and Weinstein (2008) show that providing schools' test score information to families increases their chances of selecting a higher-scoring school, which increases achievement. Corcoran et al. (2018) also show that providing information about nearby high schools' selectiveness and graduation rates improved the quality of middle-school students' high-school choices, particularly for non-English speaking households. Kapor et al. (2019) find that low-income families can misunderstand the school selection process and are less likely to be placed into their preferred school. Impeding access to information about how to apply could reduce opportunities for disadvantaged students even when there is, ostensibly, equal access ("open enrollment"). More broadly, we document the use of one mechanism to reduce access, but schools may use a number of ordeal mechanisms to screen applicants and the scope for choice, despite the intentions of policymakers.[12]

There is a normative question about what optimal policy *should* require of choice schools with respect to whom they enroll. Our paper does not aim to identify an optimal policy, which depends on a particular social welfare function. Instead, our results inform how a socially optimal policy might be achieved. For instance, if a particular social welfare function implies that all schools should have the capacity to serve any student, bolstering existing efforts to reimburse schools for realized costs may ensure this opportunity for a variety of students. Other policies could unify and simplify application processes to help families make informed decisions. Several education agencies have undertaken audits like ours to monitor whether schools are providing information equitably across families (Prothero, 2014).

The rest of our paper proceeds as follows. Section 2 provides background on school choice for our sample. Sections 3 and 4 describe our intervention, experimental design and empirical analysis. Section 5 presents our results and Section 6 concludes.

## 2. Background Information on School Choice

Our sample, which we describe in detail in the following section, covers traditional public schools in districts with various forms of school choice and charter schools. Charter schools are public, open-enrollment schools that have greater autonomy over their finances, staffing decisions, and curricula than traditional public schools, but they must admit students by lottery if more students apply to the

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[12] Welner (2013) documents that schools use a variety of mechanisms, such as releasing applications only for short periods of time early in the school year, demanding the presentation of social security cards and birth certificates, and documentation of disabilities.

school than can be accommodated.[13] Charter schools are the fastest growing form of school choice in the United States. Since 2010, more than 2,000 new charter schools have opened (NCES, 2015). They enroll nearly 3 million students at nearly 7,000 schools in 43 states and the District of Columbia. While their performance overall tends to be no better or worse than traditional public schools, charter schools in urban areas, which are often, no-excuses charter schools, have been shown to have large, positive impacts on student achievement (Abdulkadiroglu et al., 2011; Angrist et al., 2013, 2016; Cohodes, 2018; Chabrier et al., 2016; Clark et al., 2015; Dobbie and Fryer, 2013; Dobbie and Fryer, 2015; Hoxby and Murarka, 2009).

Among the traditional public schools in our sample, school choice operates in several ways. The rules are governed by both state and local laws.[14]  For example, some states like Arizona make school choice mandatory for all school districts in the state. In states where it is voluntary, it is often practiced mostly by large urban school districts. At the local level, school districts commonly establish attendance areas, and students' default to neighborhood-based assigned schools but allow applications to other schools within the district, subject to capacity constraints. Other school districts have no neighborhood assignment and require that families apply to schools as a requirement for enrollment. Schools may also offer priority enrollment to applicants based on residence, having a sibling within a school, or a safety concern. Among schools with more applicants than slots, schools may assign students based on a first-come, first-serve basis or a lottery.

Both traditional public schools and charter schools must comply with a number of anti-discrimination laws, several of which have overlapping protections. The Civil Rights Act of 1964 prohibits discrimination on the basis of race, religion, and country of origin.[15] The Rehabilitation Act of 1973, Individuals with Disabilities Education Act (IDEA) and Americans with Disabilities Act of 1990 require LEAs to provide all necessary services to students with physical or mental disabilities. IDEA mandates the creation of an Individualized Education Plan (IEP) for students with disabilities. The IEP, formulated with parents, school and other professional representatives, dictates what services the LEA

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[13] Specifically, lottery admission is required if they receive federal funding.

[14] National Center for Education Statistics Table 4.2 outlines states with mandatory open-enrollment policies for their traditional public schools and briefly describes the nature of each policy. For voluntary open-enrollment policies by state, we discovered inaccuracies within Table 4.2 and do not rely upon this particular information. For example, it is reported that the state of New York does not have any voluntary admittance of students from other schools when in fact New York City public schools have one of the largest open-enrollment programs in the country.

[15] The Civil Rights Act of 1964 further prohibits discrimination based on gender and sex, except for same-sex schools.

is legally obligated to provide a student to address their special needs.[16]

This last requirement may have greater consequences for charter schools that are authorized as their own LEA than for those authorized as part of an existing LEA or district (Heubert, 1997). The latter arrangement implies that charter schools may be able to draw on resources from the broader school district to help serve special-education students, while the former implies charter schools may have to address this requirement entirely on their own.[17] For this reason, charter schools that serve as their own LEA may respond to different incentives during the application process than those that are not their own LEA.

Traditional public and charter school funding comes from federal, state, and local governments. The degree of funding parity between charter schools and traditional public schools within the same state varies across states.[18] Supplementary funds for students with disabilities can also vary based on state and local policies. Special education funds overwhelmingly (90%) come from state and local sources (Cullen and Rivkin, 2003; Rhim et al., 2015). As a point of reference, the average cost of educating a child with special needs is roughly 2.3 to 2.5 times that of a child without special needs (Moore et al., 1988; Chambers, 1998; Collins and Zirkel, 1992).

A point of controversy is whether charter schools serve the most disadvantaged or costliest-to-educate students at similar rates to traditional public schools. Nationally, the Government Accountability Office found that charter schools enroll a smaller proportion of students classified with severe disabilities than traditional public schools (US Government Accountability Office, 2012). But evidence from specific locations is more nuanced. Setren (2015) finds that Boston-area charter schools *classify* fewer students as special needs (irrespective of whether they have a disability).[19] Hoxby and Murarka (2009) find that New York City charter schools enroll more low-income and minority students than traditional public schools as a

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[16] We summarize the above requirements as background information, but whether or not our findings constitute legal or illegal behavior on the part of a school is not germane to our first-order research question, which is to determine experimentally whether schools practice any form of differential treatment during the application process with respect to specific student characteristics.

[17] Akin to other social insurance programs such as Medicare and Food Stamps, the economic justification for special education services is multi-tiered. First, it provides a form of insurance to protect families who have children that are expensive to educate due to a disability; second, federal and state funding works as a form of insurance to protect local schools from the high cost of absorbing a disproportionate number of disabled students (Cullen and Rivkin, 2003). IDEA also permits the allocation of funds for a statewide "risk pool" to help LEAs serve students with high-cost disabilities (Rhim et al., 2015). States may also designate charter schools to be part of a larger LEA specifically for IDEA purposes. There is some evidence that individual schools existing as their own LEA may form consortia to pool resources, making it easier to establish economies of scale and provide appropriate services for all students (NCESCS, 2017).

[18] For example, the typical Oregon charter school receives only about 60 percent of the level of funding that a typical traditional public school receives while both charter and traditional public schools in Tennessee receive similar levels of funding (Batdorff et al., 2014; Epple et al., 2016).

[19] In part, this could be because the traditional public-school students were enrolled in prior to enrolling the charter school did not readily transfer their IEPs.

percentage of total enrollment. Using data from California and Texas, Booker et al. (2005) show that students who enroll in charter schools tend to have lower achievement than the students in the traditional public schools they left.

## 3. Experimental Design and Data

**Messages**

The field experiment consisted of email messages sent to charter schools and traditional public schools subject to school choice.[20] We framed each message as coming from a parent looking for a school. The parent contacts the school to ask about their child's eligibility and how to apply. We developed our messages in consultation with charter school and traditional-public school administrators who have received application inquiries via email. Our conversations with administrators at charter schools and traditional public schools found that parents do make eligibility inquiries and provided examples.[21] The baseline message indicated that the parent is looking for a school for their son or daughter and they would like to know whether anyone can apply to that school and how to apply. Each treatment message added a sentence to this baseline message to signal a child's potential cost to educate, disadvantage, or prior academic performance. This sentence indicated the child has one of the following: an IEP requiring they be taught in a classroom separate from mainstream students; poor behavior; bad grades; or good attendance and good grades. We show examples of the exact wording of these treatments in Figure 1 and Figure A1.

We chose these messages based on existing concerns about how schools may screen potential applicants. For instance, there is a national discrepancy in enrollment rates of students with severe disabilities between charter schools and traditional public schools. Students with severe disabilities may require significant support services, which may require teaching in a separate classroom from mainstream students. Note that this does *not* imply one-on-one instruction; instead, this is often instruction by a certified Special Education teacher in a separate or "self-contained" classroom with fewer students (e.g. a common set up is 12 students to one teacher with special-education certification, plus any other "para-

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[20] Email is a common way that parents make inquiries about schools. Schools post email addresses on their home page so parents can solicit information. The typical format for a school's email address is info@school.org.

[21] For instance, parents frequently asked whether the school admits students with special needs or low grades. Administrators reported other questions that parents ask as well, including whether parents have to volunteer, pay any fees, purchase school books or provide particular documentation about their child, whether there is a lottery or preferences for neighborhood families, and lastly, whether a charter school diploma is valid for college or university admission.

professionals" that may be specified in a student's IEP to provide further services).[22] The poor-behavior message ties to a contention that some schools push out or screen children with behavior issues (Zimmer and Guarino, 2013). The poor-grades message and the good grades and good attendance message reflects concerns that schools may seek out students or screen students based on their academic performance (Winters, Clayton, and Carpenter, 2017).

Lastly, demographic characteristics of the parent and student were varied at random across all messages. We randomized a signal of household structure by indicating that the parent and their spouse were making the inquiry (e.g. "My husband and I…") or that just one parent was making the inquiry. Following Bertrand and Mullainathan (2004), the name of the parent signaled the gender of the parent and whether they are Hispanic, Black, or white.[23] The choice to randomly vary demographic signals also reflects concerns about how schools may screen potential applicants. Minority background may signal socioeconomic disadvantage and a child's gender may signal disruptive behavior, as male students tend to be more disruptive in class than female students (Bertrand and Pan, 2013).

**Experimental Design and Sample Frames**

We conducted the first experiment in late 2014, in which we sent messages only to charter schools. Based on feedback we received in this first experiment, we replicated and extended the experiment in early 2018 with the goal of comparing the response rates of traditional public schools in areas with school choice to the response rates of nearby charter schools. The three-year gap between the two experiments provides a check on the consistency of the results across time.

We constructed the sample for each experiment using the Common Core Data from the National Center for Education Statistics, which has information on the universe of both charter and traditional public schools. In the first experiment, we chose the 17 states with the largest number of charter schools. These 3,131 schools were roughly half of the charter schools in the country at the time.

The initial sample for the second experiment was the 29 states (and the District of Columbia) with the largest number of charter schools and some form of intra-district choice.[24] We list these states in

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[22] Our study pertains to students with significant special needs. However, in a recent working paper, Ahmed et al. (2020) conduct an audit study in Sweden that focuses on and finds discrimination against preschool-aged children with common disabilities such as ADHD and diabetes.

[23] We chose names based on New York state data that associated names with race and gender. We then chose names that were overwhelmingly associated with one particular race and gender combination.

[24] In the second experiment, we restrict our sample to include charter schools with enrollments that exceed 200 students. As described in Appendix Table A3, our findings are robust to this imposed restriction.

Appendix Table A10. For every charter school in these states, we matched it to the nearest traditional public school with the same entry-grade level and within the same district boundaries. We then further restricted this sample to school districts in the top 40 in terms of enrollment because of the fixed costs of data collection.[25] The final sample consisted of 4,338 schools (1,016 of which were charter schools that were also in the first experiment) or 2,169 matched-pairs of traditional public and charter schools. Figure A2 shows states with charter schools colored in white, states in our sample colored in dark blue, and states with no charter schools colored in gray. The sample has broad geographic coverage across the United States. Appendix B discusses our sample construction in further detail.

In each experiment, we sent two emails to each school three-to-four weeks apart. The treatment messages were randomly assigned at the school level in the first experiment. For the second experiment, treatment messages were clustered at the pair level; identical messages from the same fictitious parent were sent to each charter school and its match-paired traditional public school. Within each experiment, no school received the same message treatment twice and a school was assigned a treatment without replacement. We also randomized the order in which schools were contacted.

Table 1 shows the results of regressing school characteristics on an indicator for each treatment. Each column restricts the sample to the baseline messages and the treatment message indicated in the column header. The results and joint test of covariates within each column show that randomization generated assignments uncorrelated with school characteristics.

Figure 1 shows the number of emails sent per treatment. More baseline and IEP messages were sent than behavior- and grades-related messages so that we would have additional precision with respect to those two treatments.[26] Across the two experiments, we sent the same baseline, IEP, poor behavior and low-grades messages.[27] The second experiment added the good-grades and attendance message to this list of treatments as well as the two-parent household signal.

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[25] Specifically, we selected the largest districts because of the fixed costs of investigating district-specific school choice policies.

[26] In our funding proposal, we pre-specified our primary treatments as the IEP, poor behavior, and poor grades in the first experiment, and these plus the good grades and attendance treatment in the second experiment. A key difference specified in the second experiment was to test for differences in response rates between the traditional public schools and charter schools. Sending one baseline message to every school would have drastically reduced our power to detect treatment effects for each of these message types relative to the baseline message. In our first experiment, we wanted added precision for the IEP message, so we sent out relatively more IEP messages. Our primary specification, compares response rates to each treatment message to the response rate of the baseline message. A power analysis showed that power in the second experiment was maximized by sending roughly twice as many baseline messages as treatment messages. See Figure 1 for the exact message counts per treatment in each experiment.

[27] Each message had randomly assigned wording variations as well. For instance, we randomly varied the subject line, the sign off ("thanks" or "thank you" or just signing the name) and the greeting.

**Data**

Data come from information on school websites, national databases of school demographic information, the census, a school-rating non-profit organization, and the responses to our emails. We hired research assistants to find and visit the website of every school in our sample frame. We then coded several variables including whether the school has a website, and, if so, whether the website includes a link to the school's contact information on its landing page, a webform to contact the school, or a requirement to add a phone number via the webform. We also used information on the school websites (and schools' handbooks on these websites) to identify the "no-excuses" charter schools in the sample. We based this determination on a template of characteristics common to such schools.

We supplemented these data with information on schools from two national databases: the Common Core of Data (CCD) and the Civil Rights Data Collection (CRDC). We used the CCD data to compute enrollment size, the share of students who are Black, Hispanic or white, and the share of students who receive free or reduced-price lunch at each school. These data also recorded the latitude and longitude of a school and whether it was located in an urban, suburban or rural area. We used the location data to link each school to census tract information on the share of individuals by race, education, income and disability in a tract. From the CRDC data, we used the number of students with a disability for each school, which the CRDC breaks down by the portion of the day students are *not* in a restricted environment (less than 40%, between 40% and 79%, and 80% or more). We translated these numbers into shares of enrollment. We also used the CRDC to calculate the share of students who are chronically absent (missing 10% of days or more), suspended, and have limited English proficiency.

Third, we used data provided by a nonprofit organization, *GreatSchools*, which collects proficiency rates based on test scores for traditional public schools and charter schools across the country. We average the proficiency rates across subjects (e.g. math and reading) and grade levels for each school. We use this measure to estimate each school's value added by measuring the difference between its observed average proficiency rate and the rate predicted based on the covariates specified above, state fixed effects, and an indicator for charter school or not. We then standardize this measure of value added according to the mean and standard deviation within the sample.

Fourth, we coded the responses of schools to our emails. We created an indicator for whether or not a school responds to a message, which is our outcome variable. Some schools provide automated responses (3% of emails receive an automated response) to our messages. Since each treatment is as

likely to receive an automated response as another, this practice only raises overall response rates and our results are robust to excluding these messages (available upon request).

Finally, a response does not necessarily indicate encouragement to apply. Schools may attempt to persuade a parent to either apply or not to apply, or it may contain other information that differs by message content. Thus, a reply is not necessarily encouragement to apply. For example, in conducting our first experiment, we received the following responses from two schools that appear to discourage children with significant special needs and poor grades, respectively, from applying:

> *Our school is high achieving and we don't teach students who need to be taught in a separate classroom on the basis of their IEP. Parents usually opt for larger schools for this, so they can be offered a range of services and staff. We encourage everyone to apply, but all our grades are at capacity and there is a lottery.*

> *Yes, anyone can apply, however, being accepted is another story. We do look at student's … academic performance, how they have performed on statewide assessments over time, and we look at whether they have incurred any disciplinary action. We are looking for motivated, high performing students that are capable of doing well in our mostly advanced curriculum. As the state has increased the rigor of academics, we have had to raise our admission criteria to insure the overall success of the school and our student body.*

We worked with a qualitative researcher to develop a codebook and train a team of research assistants to review each message and manually code school responses as either encouraging or not encouraging. Appendix C discusses in further detail how we created this measure.

In Appendix Table 1, we show the characteristics of our sample schools. On average, sample schools serve primarily students from low-income families; more than half of the students served in these schools receive free or reduced-priced lunch. Demographically, there are relatively similar shares of white, Black and Hispanic students across all sample schools. Over half the sample is located in an urban area. We also show that school-level correlates of disadvantage predict lower response rates, and that high-performing schools are less likely to respond overall (Table A2).

## 4. Empirical Strategy

Our main outcome is a binary variable for whether a school responds to our inquiry or not. We estimate a linear-probability model as follows:

$$R_i = \alpha + \text{Treatment}_i \beta + \text{Demographics}_i \gamma + X_i \delta + \text{Wave}_i \theta + \varepsilon_i$$

In this regression, $R_i$ is our binary outcome. $\text{Treatment}_i$ is a vector of treatment indicators for the mutually-exclusive treatments: the IEP treatment, the behavior treatment, the low-grades treatment,

and the good-grades and good-attendance treatment. Demographics$_i$ is a vector of indicators for the randomized implied race, gender of the child and parent, and whether the message indicates a two-parent household. X$_i$ is a vector of school covariates, including school demographics and whether or not the school is a traditional public school. Any missing covariates are imputed and an indicator for missing data is included in the regression. All regressions include indicators for the wave of emails.[28] Standard errors are clustered at the school or school-by-pair level.[29]

Outcomes include binary indicators for whether or not a school of choice responded to the email as well as indicators for the encouragement of the response. To ameliorate concerns about selection into response, all outcomes are unconditional on whether or not a school responds. For instance, if the outcome is an indicator for whether or not a response is "encouraging," then a one indicates that the school responded and wrote a message using an encouraging tone while a zero indicates either that the school's response was discouraging or that the school did not respond at all.

To test whether traditional public schools respond at different rates to each type of message than charter schools, we interact each treatment with an indicator for whether or not the school is a traditional public school. The significance of this interaction effect tests whether the traditional public school responded more or less frequently than the nearby charter school. To conduct other heterogeneity analyses, we either restrict the sample according to a certain variable or fully interact it with our treatment messages as specified in the text.

Our results replicate similarly across experiments, which may be unsurprising given that the treatment messages were the same (except for the addition of new treatments) and the policy environment was similar. For ease of presentation, we combine the two experiments in our analysis of the results. However, the appendix Table A3 shows results from each experiment separately.

Lastly, we show our results are robust to different specifications and multiple-testing corrections. We show whether controls for school characteristics from the CCD and CRDC, census tract characteristics, and pair fixed effects affect our results. Given random assignment, none of these additional covariates or fixed effects is required for identification. In the appendix, we adjust our main

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[28] There are four waves of emails: two in the first experiment and two in the second experiment.

[29] Specifically, clustering is at the school level for schools that are only in the first experiment, clustering is at the pair level for schools only in the second experiment, and clustering is at the school and pair level for schools that are in both the first and second experiment. In practice, the level of clustering does not alter the standard errors much and therefore the significance of the results; other versions of clustering are available upon request.

results—the effects of each treatment message and the differential effect between charter schools and traditional public schools—for multiple-hypothesis testing (Table A6). We use Holm's step-down procedure for groups of treatment effects specified in the text (Holm, 1979). This procedure controls for the family-wise error rate, which is the probability of at least one false rejection of the null hypothesis.

## 5. Results

Table 2 presents the effects of our primary treatments—messages indicating poor behavior, the IEP, poor grades, and good grades and attendance—on response rates. The control mean at the bottom of Column (1) shows that the baseline message received a response 53% of the time.

We find that schools are less likely to reply to messages signaling that the potential applicant has had bad grades, poor behavior, and an IEP (Column (1)) than the baseline message. The bad-grades treatment reduces response rates by 2.4 percentage points. The IEP message and poor behavior message reduce response rates by 5.2 percentage points and 7.0 percentage points, respectively. The signal of good grades and attendance has no discernable impact on response rates (the coefficient is 0.0 percentage points). One caveat of the latter result is that parents' describing their child's good grades may be considered "cheap talk" by the schools, and not taken seriously.

In interpreting these results, note that the IEP message signals a student who requires a restrictive environment. If the signal were for less restrictive or less costly services, the results may have differed. And while schools do not actively provide more information to higher-performing students, this signal could be viewed by schools as "cheap talk." Messages signaling these positive traits may be perceived as less truthful than the messages signaling other student traits.

Column (1) of Table 2 also shows the results of our other treatments signaling race, gender and household structure. Only the message that indicates a Hispanic-sounding name results in significantly lower—by 2.0 percentage points—response rates.[30] The coefficient on Black-sounding names is not significant. An F-test for whether we can reject the null hypothesis that these treatments of demographic characteristics are jointly equal to zero cannot be rejected (p=0.14 for the specification in column 1). One point of consideration for the Hispanic-sounding names is that the emails were sent in

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[30] These results are larger in magnitude for predominantly-white schools (results available upon request), though we do not know how common it is for these schools to receive inquiries from Black or Hispanic families.

English; the results could be a lower bound if schools are concerned about a student's English-language skills.

In the appendix (Table A4), we show that the results above are extremely similar across different specifications. The coefficients do not vary significantly with the addition of school-level covariates, census tract-level covariates, or pair fixed effects.

*Heterogeneity across Traditional Public and Charter Schools*

Given the growth rate of charter schools and concerns over whether they encourage all students to apply, we test whether our findings differ between charter schools and traditional public schools. Columns (2) and (3) of Table 2 show results separately for traditional public schools and charter schools, respectively. Column (4) shows the difference in response rates between traditional public schools and charter schools and its statistical significance.

Traditional public schools in areas of school choice generally respond at similar rates as charter schools for each treatment, with one exception. Traditional public schools are significantly—5.8 percentage points—more likely to respond to the IEP message than charter schools. This result remains statistically significant even after adjusting for multiple testing of these interaction terms (p=0.05, Table A6). A test of whether the interaction effects for the primary treatments are jointly different from zero is also statistically significant (p=0.03). Treatment effects for signals of demographic characteristics do not differ significantly between traditional public schools and charter schools.

Although we observe that traditional public schools respond at higher rates than charters for children with IEPs, it could be that traditional public schools are systematically less encouraging in their responses, which might discourage parents from applying. Based on qualitative data collected by trained coders, Table 3 reports findings on whether school responses are encouraging, with no response coded as a zero and an encouraging response coded as a one. (See Appendix C for a description of our qualitative data and reports on inter-rater reliability.) Responses to the IEP message are 4.2 percentage points more likely to be encouraging from traditional public schools than from charter schools. However, the evidence suggests otherwise for the bad grades and poor behavior treatments, where traditional publics provide significantly fewer encouraging responses.[31]

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[31] Table A9 also presents results on the likelihood that a typical person may apply to the school. Column (4) shows differences in impacts between traditional public schools and charter schools on the likelihood of applying. The findings are broadly supportive of earlier results: the typical

**Exploratory Analyses**

*Heterogeneous Effects and "No-Excuses" Charter Schools*

Access to high-performing schools is important if school choice is to reduce gaps in achievement across groups of students with different histories and attributes. We explore heterogeneity in two ways. First, in Table 4 we show treatment effect heterogeneity by cutting the sample into subgroups. Columns (1) and (2) show treatment effects for all high and all low-value-added schools, which we define as above- or below-average value added in the sample. The IEP, behavior, and grades treatment effects are similar for high- and low-value-added schools. Differences arise with the race implied by the messages, however: high value-added schools are less likely to respond to messages from families with Black or Hispanic-sounding names than low value-added schools. Results for messages from parents with Hispanic-sounding names remain significant at the 1% level after adjusting for multiple testing (p =0.01 and p =0.15 for Hispanic-sounding name and Black-sounding name, respectively). This result is driven by the high-value added charter schools in the sample and by schools serving low shares of minority students (results available upon request). All of these findings are exploratory, however.

Prior research has shown that urban, high-value added charter schools can significantly close racial gaps in student achievement (Abdulkadiroglu et al., 2011). Column (3) shows the treatment effects from the primary treatment messages do not qualitatively differ for this subgroup. Many "No-excuses" charter schools have demonstrated particular success in closing racial-achievement gaps in school districts. We identified a list of 272 no-excuses schools in our sample. These schools tend to have much higher test scores, value added, and serve larger shares of minority and low-income students relative to other charter schools. These schools also, however, serve fewer students with IEPs and have higher rates of suspensions than other charter schools (see Table A7). While the sample of "no-excuses" charter schools is small, the coefficient on the IEP treatment remains large (10 percentage points), negative, and significant.

A second way we explore heterogeneity is by characterizing the distribution of baseline attributes for schools that discriminate. In this instance, we define "discrimination" as a school that responds if it receives a baseline message and does not respond if it receives a treatment message. We implement

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person—or coder—is similarly likely to apply to traditional public and charter schools across the primary treatments, except for the IEP treatment. Specifically, we find that the typical person is 4.5 percentage points more likely to apply to a traditional public versus a charter school when sent the IEP message. Given that coders are in fact non-applicant parents, we generally view this evidence as suggestive.

this via an application of characterizing complier means (Abadie, 2003). "Compliers," in our study, are those who discriminate, as defined above.

Thus, our instrument, $Z_{it}$, is an indicator for a particular treatment message *t*. $D_i$ is an indicator for whether or not a school responds. As in any instrumental variable strategy, this application requires that monotonicity holds: schools are weakly less likely to respond to a treatment message than a baseline message (i.e. $D_i(1)<D_i(0)$).[32] Further, random assignment assures that our treatments are independent of covariates (i.e. $X_i \perp\!\!\!\perp Z_{it}$). Lastly, there must be a first stage: $E[D_i| Z_{it}=1]\neq E[D_i| Z_{it}=0]$), which in our case is the treatment effect of a message on response rates. Note that there is no first stage for the good-grades treatment, so we exclude it from this analysis. With these assumptions, we can identify characteristics of always takers (those who always respond to a message), never takers (those who never respond to a message), and compliers (as defined above).[33] Our strategy for identifying complier characteristics is simpler than Kline and Walters (2020), at the expense of not identifying *which* particular schools discriminate as their method could do.

Appendix Table A5 shows the results for our primary treatment messages. For each column we restrict the sample to schools who received either that particular treatment *t*, or a baseline message. The first column shows overall means (and standard deviations in parentheses). The remaining columns show how results differ by treatment message. Column (2) shows that compliers with respect to the bad-grades treatment are more likely to be traditional public schools with higher value added and higher shares of minority students—compared to the average school. For the IEP treatment, complier schools are more likely to be charters and have lower shares of IEP students. Lastly, compliers for the poor behavior treatment tend to be charter schools with more white students and much higher value added. Again, however, these findings are exploratory.

*Moderating Factors: Funding Strategies*

States typically provide funding for special education students in three ways. Most common is formula-based funding. The formula may be a funding multiplier based on student or staff counts.

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[32] This assumption may be violated if schools reply with a discouraging message, though our results do not change substantially if we use our qualitative coding of encouraging reply or not (available upon request).

[33] Though this analysis follows Abadie (2003), a short explanation follows. The mean for a particular characteristic X for always takers is $E[X_i| Z_{it}=1, D_i=1]$, and for never takers is $E[X_i| Z_{it}=0, D_i=0]$. Since we can also identify the overall mean of X and the shares of always takers, never takers, and compliers (the latter is the treatment effect of a given message), we can identify the complier mean as well.

Schools receive additional funds, but these are not necessarily ear-marked for special-education services. Categorical funds or block grants similarly provide funds for schools, but these are ear-marked for special-education services. Finally, states may provide partial or full reimbursement to districts for their realized special education expenditures. We generate a variable categorizing states by how they provide funding and we interact this variable with our IEP treatment indicator. We restrict the sample to charter schools, since traditional public schools are no less likely to respond to students with an IEP than to the baseline message.[34]

Table 5 shows heterogeneous effects by funding strategy. Charter schools in states that reimburse schools for (at least a portion of) their realized expenditures are 7 percentage points more likely to respond to the IEP treatment message than charter schools in other states. This result is statistically significant at conventional levels (p=0.05). Charter schools in states with categorical funding have slightly higher response rates as well, though the difference is not statistically significant. In states with formula-based funding, there are several weighting schemes; we do not find any significant heterogeneity with respect to these different schemes (results available upon request).

This suggestive pattern of results is consistent with an explanation that the costs of educating certain students are imperfectly compensated in most contexts. The latter could create an incentive for schools to provide less application encouragement to students with special needs. One caveat to a cost-reimbursement policy is that it can create incentives to classify students as requiring an IEP and associated services, irrespective of the student's actual needs.

*Diversifying Risk: Local Education Agency (LEA) Status*

Table 5 also shows heterogeneity by LEA status. LEAs, rather than schools, bear the legal obligation to provide the services specified in students' IEPs. Whether or not a charter school is its own LEA varies across and within states. Charter schools that are their own LEA may have difficulty pooling resources and risk across multiple schools. We assess whether the opportunity to pool resources in this way is associated with differential response rates to the IEP message. We restrict the sample to charter schools and interact an indicator for LEA status with our IEP treatment. While own-LEA charter schools are slightly less likely to respond to the IEP treatment, this result is not statistically significant.

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[34] Traditional public schools exhibit a similar pattern of heterogeneity with respect to funding as charter schools, but the estimates are much less precise.

*Household Structure and Demographics*

Lastly, we explore whether signals of household structure exacerbate or attenuate differential response rates across treatments. We randomized whether a message indicated that a husband and wife are or just one parent is looking for a school. We also randomized the race and gender of the child. Results restricting the sample to each of these groups are shown in Table A8. Messages signaling a two-parent household, which could signal socio-economic advantage, tend to increase the likelihood of response to children with poor behaviors and decrease the likelihood of response regarding children with good grades. Messages signaling a two-parent household also significantly increase the likelihood of response for Black families. Messages with a Hispanic-sounding name tend to have higher response rates to the good-grades treatment. Finally, treatment effects for the primary treatment messages do not differ significantly for messages signaling a male student compared to female students.

## 6. Conclusion

While school choice can, in theory, improve access and quality, competitive pressures may also induce schools to keep costs low and discourage students perceived as costly to educate from enrolling. These incentives may be present outside of school choice as well, but the application process presents a distinct opportunity for schools to treat costlier or students perceived as harder-to-educate students differently than with a neighborhood-based, school-assignment mechanism. Using an audit study approach, we provide the first experimental assessment of whether schools fail to provide application information to families with children who have a severe special need, behavior problem, or level of academic achievement, as well as a perceived race and gender. Our study is also much broader in scope than previous research; we sample schools across 29 states and the District of Columbia and approximately half of the charter schools in the United States.

We find that, on average, schools are significantly less likely to provide information to families with students who have low grades, behavior problems, or an IEP requiring they be taught in a separate classroom, than to families of students without these attributes. Charter schools are significantly less likely to reply to students to the IEP message than to the baseline message, while traditional public schools are not. There is also some evidence that schools are less likely to respond to families with Hispanic-sounding names.

One limitation of our analysis is that we chose one particular signal of disability, and one that may be perceived as particularly costly for schools to provide services. Our findings may not generalize to

other disabilities or students requiring other services such as students with limited English proficiency.[35] We did not incorporate a wider variety of treatments because of statistical power concerns. A second limitation is that we cannot estimate the impacts of these behaviors on the enrollment rates of different types of students into schools. We capture just one way schools can dissuade certain students from applying, but schools can use other ordeal mechanisms to screen students as well, such as moving up application deadlines, requiring parents fill out a paper applications, and attending school open houses.

The implications of our results for optimal policy requires specifying a social welfare function. Some might favor maximizing a weighted average of student achievement, but many families see inclusion as essential for improving pro-social outcomes, especially as they pertain to students with special needs or diversity.[36] Our results suggest that funding is a key constraint on schools' willingness to serve students with disabilities. We show suggestive evidence that cost-reimbursement funding mitigates charter schools' differential response rates for students with IEPs. All schools—including traditional public and charter—are less likely to respond to students with poor prior behavior and low grades. Conducting systematic audits could help deter this behavior. Some education agencies have already undertaken audits via phone calls asking eligibility questions to charter schools.

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[35] As a point in contrast, see Ahmed et al. (2020). Following our study, they conduct an audit study in Sweden and find that children with common disabilities such as ADHD are more likely to face discrimination.

[36] For instance, see work by Gautam Rao (2019) on how the integration of castes in India impacted social preferences around inequality, inclusion and altruism.

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# Figures



Sample | Message Type | Message Content

**N=17 States**
**n=3,131 schools**

**Control**
(n=2,082) — I am searching for schools for my daughter. Can anyone apply to your school? If so, can you tell me how to apply?

**IEP**
(n=2,091) — … Her IEP says she has to be taught in a separate room…

**Bad Behavior**
(n=1,026) — … She gets in trouble a lot for behaving badly in school…

**Poor Grades**
(n=1,011) — … She's been getting bad grades…

(a)First Experiment

Sample | Message Type | Message Content

**N=29 States**
**n=4,338 schools**

**Control**
(n=2,949) — I am searching for schools for my daughter. Can anyone apply to your school? If so, can you tell me how to apply?

**IEP**
(n=1,431) — … Her IEP says she has to be taught in a separate room…

**Bad Behavior**
(n=1,436) — … She gets in trouble a lot for behaving badly in school…

**Poor Grades**
(n=1,430) — … She's been getting bad grades…

**Good Grades**
(n=1,434) — … She has good grades and attendance…

(b)Second Experiment

**Figure 1** - *Experimental Design for (a) First experiment sample and (b) Second experiment sample (Note: This figure shows the study design and examples of the treatment phrasings). Race, gender and other email characteristics were assigned with equal probability across these treatment messages.*

# Tables

**Table 1 – Testing for baseline balance across treatment messages**

|  | Baseline | Bad Grades | IEP | Bad Behavior | Good Grades |
|---|---|---|---|---|---|
| TPS | 0.293 | 0.008 | 0.010 | -0.004 | -0.007 |
|  | (0.455) | (0.008) | (0.007) | (0.007) | (0.007) |
| % LEP students | 0.123 | 0.026 | 0.019 | -0.039 | -0.013 |
|  | (0.18) | (0.042) | (0.037) | (0.04) | (0.038) |
| % Chronically absent students | 0.159 | -0.031 | 0.041 | -0.012 | 0.019 |
|  | (0.187) | (0.038) | (0.033) | (0.039) | (0.034) |
| % Students with IEP | 0.075 | -0.034 | -0.049 | 0.032 | 0.029 |
|  | (0.084) | (0.069) | (0.066) | (0.068) | (0.06) |
| % Proficiency | 0.463 | 0.011 | 0.073* | 0.045 | 0.060 |
|  | (0.225) | (0.047) | (0.042) | (0.047) | (0.045) |
| % White students | 0.288 | 0.094* | 0.024 | -0.004 | -0.026 |
|  | (0.308) | (0.057) | (0.051) | (0.054) | (0.051) |
| % Black students | 0.254 | -0.082 | 0.022 | 0.022 | 0.010 |
|  | (0.321) | (0.054) | (0.05) | (0.051) | (0.05) |
| % Hispanic students | 0.321 | -0.036 | 0.046 | 0.060 | -0.019 |
|  | (0.31) | (0.054) | (0.049) | (0.051) | (0.049) |
| % FRPL students | 0.59 | -0.036 | 0.001 | -0.013 | 0.019 |
|  | (0.329) | (0.026) | (0.024) | (0.026) | (0.023) |
| Value added measure | 0.017 | -0.002 | -0.012 | -0.008 | -0.010 |
|  | (0.992) | (0.008) | (0.007) | (0.008) | (0.007) |
| % Urban | 0.606 | -0.043 | -0.026 | 0.014 | 0.000 |
|  | (0.489) | (0.032) | (0.027) | (0.033) | (0.027) |
| % Suburban | 0.269 | -0.046 | -0.015 | 0.005 | 0.003 |
|  | (0.443) | (0.033) | (0.028) | (0.034) | (0.028) |
| % Rural | 0.058 | -0.024 | 0.010 | 0.042 | 0.022 |
|  | (0.235) | (0.037) | (0.03) | (0.038) | (0.029) |
| P-value, joint test |  | 0.391 | 0.194 | 0.691 | 0.862 |
| Observations | 5,031 | 2,441 | 3,522 | 2,462 | 1,434 |

*Notes:* Column (1) shows means for the baseline message sample and the standard deviations in parenthesis. Each other column is as follows: For each treatment, the sample is restricted to baseline and treatment observation indicated in the column header, and the treatment indicator is regressed on the covariates shown in each row. School-level demographic data including absenteeism, Free-Reduced Priced Lunch (FRPL), Limited English Proficiency (LEP), and disability data are from the Civil Rights Data Collection 2013-14 public dataset. % Proficiency measures the rate of students at or above proficiency, as reported by Great Schools from 2016. See text for the construction of the Value-added measure. Two-way cluster-robust standard errors (by pair and school) in parentheses. The joint test is a test of whether the covariates are jointly different from zero.

\*\*\* p<0.01, \*\* p<0.05, \* p<0.1

**Table 2 – Effects of Message Treatments on School Response Rates**

| Sample | Full Sample | TPS | Charter | TPS - Charter diff. |
|---|---|---|---|---|
| Bad Grades | -0.024** | -0.044** | -0.015 | -0.030 |
| | (0.011) | (0.021) | (0.013) | (0.025) |
| IEP | -0.052*** | -0.002 | -0.065*** | 0.058** |
| | (0.010) | (0.021) | (0.011) | (0.023) |
| Bad Behavior | -0.070*** | -0.053*** | -0.076*** | 0.021 |
| | (0.011) | (0.021) | (0.013) | (0.024) |
| Good Grades | 0.001 | 0.013 | -0.005 | 0.015 |
| | (0.014) | (0.021) | (0.020) | (0.029) |
| Black | -0.012 | -0.004 | -0.015 | 0.010 |
| | (0.010) | (0.018) | (0.011) | (0.022) |
| Hispanic | -0.020** | -0.015 | -0.023** | 0.009 |
| | (0.010) | (0.018) | (0.011) | (0.021) |
| Father | 0.003 | -0.009 | 0.008 | -0.017 |
| | (0.008) | (0.015) | (0.009) | (0.017) |
| Son | -0.011 | -0.007 | -0.011 | 0.004 |
| | (0.007) | (0.011) | (0.008) | (0.014) |
| Two Parents | 0.010 | 0.007 | 0.014 | -0.009 |
| | (0.011) | (0.015) | (0.015) | (0.021) |
| TPS | 0.008 | | | |
| | (0.013) | | | |
| | | | | |
| Observations | 14,806 | 4,296 | 10,510 | 14,806 |
| Control Mean | 0.533 | 0.496 | 0.548 | 0.533 |

*Notes:* Table shows the results of a multivariate regression of an indicator for whether or not a school responded to the message on message-treatment indicators. Columns (1)-(3) show the results for different samples: (1) full sample, (2) only traditional public schools and (3) only charter schools. Column (4) interaction between primary and secondary treatments and TPS. TPS is an indicator variable (not a treatment) for whether a school is a traditional public school. All other variables included in the table are randomly assigned characteristics of the emails. Regressions include fixed effects by wave and state. Two-way cluster-robust standard errors (by pair and school) in parentheses.

*** p<0.01, ** p<0.05, * p<0.1

## Table 3 - Effects of Message Treatments on School Response Perceived as Encouraging

| Sample | Full Sample | TPS | Charter | TPS - Charter diff |
|---|---|---|---|---|
| Bad Grades | -0.027*** | -0.065*** | -0.012 | -0.052** |
| | (0.010) | (0.016) | (0.012) | (0.020) |
| IEP | -0.035*** | 0.001 | -0.044*** | 0.042** |
| | (0.009) | (0.017) | (0.010) | (0.020) |
| Bad Behavior | -0.061*** | -0.083*** | -0.051*** | -0.033* |
| | (0.010) | (0.015) | (0.012) | (0.019) |
| Good Grades | -0.003 | -0.009 | 0.001 | -0.013 |
| | (0.012) | (0.017) | (0.017) | (0.024) |
| Black | -0.018** | -0.013 | -0.019* | 0.006 |
| | (0.008) | (0.014) | (0.010) | (0.017) |
| Hispanic | -0.010 | 0.004 | -0.016 | 0.019 |
| | (0.008) | (0.014) | (0.010) | (0.017) |
| Father | -0.005 | -0.008 | -0.004 | -0.006 |
| | (0.007) | (0.011) | (0.008) | (0.014) |
| Son | -0.005 | -0.013 | -0.002 | -0.012 |
| | (0.006) | (0.010) | (0.008) | (0.013) |
| Two Parents | 0.012 | 0.022* | 0.003 | 0.017 |
| | (0.009) | (0.012) | (0.013) | (0.017) |
| TPS | -0.056*** | | | |
| | (0.009) | | | |
| | | | | |
| Observations | 14,806 | 4,296 | 10,510 | 14,806 |
| Control Mean | 0.256 | 0.195 | 0.281 | 0.256 |

Notes: Table shows the results of a multivariate regression of an indicator for whether or not a school responds in an "encouraging" manner to the message. Non-response is coded as not encouraging (or zero). See Appendix C for a description of our qualitative data and reports on inter-rater reliability. Columns (1)-(3) show the results for different samples: (1) full sample, (2) only traditional public schools and (3) only charter schools. Column (4) interaction between primary and secondary treatments and TPS. TPS is an indicator variable (not a treatment) for whether a school is a traditional public shcool. All other variables included in the table are randomly assigned characteristics of the emails. Regressions include fixed effects by wave and state. Two-way cluster-robust standard errors (by pair and school) in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

**Table 4 - Differential Effect of Messages by VAM Performance and Type of Charter**

| Sample | High VAM | Low VAM | High-VAM, Urban Charter | No Excuses Charter |
|---|---|---|---|---|
| Bad Grades | -0.022 | -0.025 | 0.010 | 0.016 |
| | (0.018) | (0.018) | (0.028) | (0.052) |
| IEP | -0.049*** | -0.056*** | -0.093*** | -0.104** |
| | (0.016) | (0.016) | (0.024) | (0.044) |
| Bad Behavior | -0.067*** | -0.066*** | -0.070** | 0.009 |
| | (0.018) | (0.018) | (0.028) | (0.054) |
| Good Grades | 0.035 | -0.034 | 0.054 | 0.033 |
| | (0.022) | (0.022) | (0.036) | (0.063) |
| Black | -0.032** | 0.007 | -0.049** | 0.036 |
| | (0.015) | (0.016) | (0.024) | (0.050) |
| Hispanic | -0.049*** | 0.019 | -0.062** | 0.029 |
| | (0.015) | (0.016) | (0.024) | (0.044) |
| Father | 0.012 | -0.006 | 0.017 | 0.085** |
| | (0.013) | (0.013) | (0.020) | (0.037) |
| Son | -0.006 | -0.012 | -0.007 | -0.069** |
| | (0.011) | (0.010) | (0.017) | (0.031) |
| Two Parents | 0.012 | 0.007 | 0.002 | 0.046 |
| | (0.017) | (0.018) | (0.029) | (0.047) |
| TPS | 0.016 | 0.003 | | |
| | (0.020) | (0.020) | | |
| | | | | |
| Observations | 8,835 | 2,516 | 3,457 | 689 |
| Control Mean | 0.549 | 0.538 | 0.538 | 0.559 |

*Notes:* Table shows the effect of different treatments on response rate for different samples. TPS is a covariate for traditional public school. All other variables included in the table are randomly assigned characteristics of the emails. Sample row shows the population for each regression: (1) Schools with high value-added measure (>0), (2) Schools with low high value-added measures (<0), (3) Urban charter schools with high VAM, and (4) "No Excuses" charter schools. Regressions include fixed effects by wave and state. Two-way cluster-robust standard errors (by pair and school) in parentheses.

    *** p<0.01, ** p<0.05, * p<0.1

**Table 5 - Effect of IEP Message by State Funding Category and LEA status for Charter Schools**

|  | (1) | (2) |
|---|---|---|
| IEP | -0.080*** | -0.064*** |
|  | (0.015) | (0.016) |
| IEP x Funding = Categorical | 0.022 |  |
|  | (0.025) |  |
| IEP x Funding = Reimbursement | 0.068** |  |
|  | (0.035) |  |
| IEP x Own LEA |  | -0.008 |
|  |  | (0.023) |
|  |  |  |
| Observations | 10,510 | 10,087 |
| Control Mean | 0.548 | 0.548 |

*Notes:* Table shows the effect of different treatments on response rate for charter schools in the sample depending on state funding and whether they are their own LEA (Local Education Agency). The categories for forms of funding are "Formula" (base), "Categorical," and "Reimbursement." All other treatment variables are randomly assigned characteristics of the emails. Regressions include all treatment indicators (only IEP is shown), wave and state fixed effects. Nested cluster-robust standard errors (by pair and school) in parentheses.

*** $p<0.01$, ** $p<0.05$, * $p<0.1$

**Appendix A.**

**Figures**

Subject: Application question

Hello,

I am looking for a school for my daughter. Does your school let anyone apply? Can you tell me how to apply?

Thanks,
Maria Gonzalez

(a)

Subject: Application question

Hello,

I am looking for a school for my daughter. **Her IEP says she has to be taught in a separate room.** Does your school let anyone apply? Can you tell me how to apply?

Thanks,
Maria Gonzalez

(b)

*Figure A1 - Message sent to parents based for (a) baseline and (b) IEP intervention*
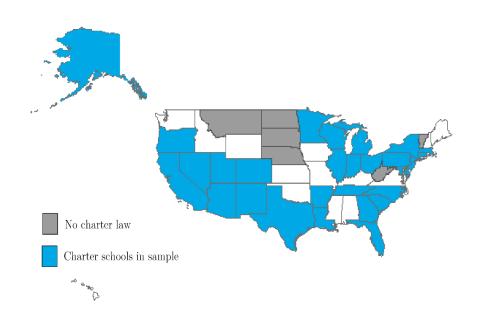*(Note: This figure shows an example of one phrasing for the Baseline and IEP message)*

***Figure A2*** *- Map of Sample Frame*
*(Note: The map shows the sample frame for the experiment)*

## Appendix Tables

### Table A1 – Total Population of Traditional Public Schools and Charter Schools vs. Sample

| | CRDC Population | | Sample | | | |
|---|---|---|---|---|---|---|
| | TPS | Charter | TPS | Charter (All) | Charter (Exp 1) | Charter (Exp 2) |
| % White students | 0.57 | 0.351 | 0.237 | 0.342 | 0.405 | 0.251 |
| | (.327) | (.325) | (.262) | (.323) | (.332) | (.285) |
| % Black students | 0.133 | 0.291 | 0.279 | 0.262 | 0.231 | 0.306 |
| | (.219) | (.338) | (.311) | (.324) | (.304) | (.347) |
| % Hispanic students | 0.205 | 0.275 | 0.399 | 0.314 | 0.278 | 0.367 |
| | (.258) | (.285) | (.307) | (.304) | (.285) | (.322) |
| % FRPL | 0.684 | 0.53 | 0.684 | 0.549 | 0.533 | 0.572 |
| | (.293) | (.329) | (.293) | (.33) | (.321) | (.34) |
| % Limited English Proficiency | 0.091 | 0.094 | 0.186 | 0.096 | 0.079 | 0.124 |
| | (0.148) | (0.162) | (0.2) | (0.167) | (0.154) | (0.182) |
| Absenteeism Rate | 0.112 | 0.162 | 0.188 | 0.17 | 0.18 | 0.152 |
| | (0.292) | (0.384) | (0.226) | (0.333) | (0.313) | (0.363) |
| % IEP Students | 0.108 | 0.086 | 0.117 | 0.088 | 0.093 | 0.08 |
| | (0.134) | (0.166) | (0.099) | (0.146) | (0.167) | (0.099) |
| % of IEP students in regular class <40% of time | 0.008 | 0.004 | 0.017 | 0.002 | 0.003 | 0.002 |
| | (0.023) | (0.03) | (0.034) | (0.024) | (0.03) | (0.008) |
| % Students Receiving In-School Suspension | 0.022 | 0.016 | 0.028 | 0.018 | 0.016 | 0.021 |
| | (0.296) | (0.304) | (0.109) | (0.227) | (0.207) | (0.257) |
| % Students Receiving 1 Out-of-school Suspension | 0.01 | 0.024 | 0.031 | 0.027 | 0.027 | 0.026 |
| | (0.263) | (0.299) | (0.077) | (0.223) | (0.203) | (0.253) |
| % Proficiency | 40.329 | 44.475 | 42.177 | 48.374 | 48.406 | 48.327 |
| | (15.725) | (20.445) | (22.273) | (22.515) | (21.998) | (23.282) |
| % Urban schools | 0.239 | 0.518 | 0.682 | 0.575 | 0.504 | 0.682 |
| | (0.427) | (0.500) | (0.466) | (0.494) | (0.500) | (0.466) |
| % Suburban schools | 0.318 | 0.291 | 0.285 | 0.269 | 0.263 | 0.278 |
| | (0.466) | (0.454) | (0.452) | (0.443) | (0.440) | (0.448) |
| % Rural schools | 0.299 | 0.117 | 0.017 | 0.098 | 0.148 | 0.023 |
| | (0.458) | (0.321) | (0.130) | (0.298) | (0.355) | (0.151) |
| Observations | 89,898 | 5,813 | 2,170 | 4,283 | 2,635 | 1,648 |

*Notes*: Means are reported for all schools that are available in CRDC 2013-14 dataset. The CRDC does not have data on all schools in the study sample. TPS schools are traditional public schools. IEP students refers to students that have an Individualized Education Program. School proficiency scores show the percentage of students scoring at or above proficiency on state assessments across grades and subject as reported by Great Schools. Charter (All) column represents all the charter schools in the experimental sample for both experiments. Charter (Exp 1) and Charter (Exp 2) columns show the characteristics of the charter schools included in the first and second experiment, respectively.

**Table A2 – The Relationship between Response and School Locations and Demographics**

|  | Responded |
|---|---|
| City | 0.008 |
|  | (0.027) |
| Suburb | -0.021 |
|  | (0.027) |
| Rural | 0.015 |
|  | (0.031) |
| % FRL | -0.070*** |
|  | (0.019) |
| % Female | 0.180*** |
|  | (0.067) |
| % Black | -0.321*** |
|  | (0.047) |
| % White | 0.006 |
|  | (0.051) |
| % Hispanic | -0.252*** |
|  | (0.047) |
| Enrollment | 0.000** |
|  | (0.000) |
| % Proficiency | -0.001** |
|  | (0.000) |
| Proficiency (missing) | -0.055*** |
|  | (0.018) |
| Constant | 0.662*** |
|  | (0.059) |
|  |  |
| Observations | 14,064 |
| R-squared | 0.049 |

*Notes:* Table shows regression coefficients and standard errors for a regression of school covariates on an indicator for response. School-level demographic data include enrollment, FRPL population, race and ethnicity breakdowns is retrieved from National Center for Education Statistics (NCES) Common Core of Data for the 2015-16 school year. School proficiency shows the % of proficient (or higher) students as reported by GreatSchools in their most recent dataset from 2016. Nested clustered standard errors in parentheses.

*** p<0.01, ** p<0.05, * p<0.1

**Table A3 - Effect of Message Treatments on Response Rates from Schools by Experiment**

| Sample | 1st Experiment | 2nd Experiment | TPS, 2nd Experiment | Charter, 2nd Experiment | Diff. TPS - Charter, 2nd Experiment |
|---|---|---|---|---|---|
| Bad Grades | 0.004 | -0.044*** | -0.044** | -0.043** | -0.005 |
| | (0.017) | (0.014) | (0.021) | (0.020) | (0.029) |
| IEP | -0.055*** | -0.042*** | -0.002 | -0.082*** | 0.073** |
| | (0.013) | (0.015) | (0.021) | (0.020) | (0.029) |
| Bad Behavior | -0.064*** | -0.072*** | -0.053*** | -0.091*** | 0.037 |
| | (0.017) | (0.015) | (0.021) | (0.020) | (0.029) |
| Good Grades | | -0.003 | 0.013 | -0.019 | 0.027 |
| | | (0.014) | (0.021) | (0.020) | (0.029) |
| Black | -0.024 | -0.003 | -0.004 | -0.002 | -0.003 |
| | (0.015) | (0.013) | (0.018) | (0.018) | (0.026) |
| Hispanic | -0.031** | -0.013 | -0.015 | -0.011 | -0.003 |
| | (0.015) | (0.013) | (0.018) | (0.018) | (0.025) |
| Father | 0.012 | -0.001 | -0.009 | 0.007 | -0.016 |
| | (0.012) | (0.011) | (0.015) | (0.015) | (0.021) |
| Son | -0.020* | -0.004 | -0.007 | -0.000 | -0.007 |
| | (0.011) | (0.008) | (0.011) | (0.010) | (0.015) |
| Two Parents | | 0.011 | 0.007 | 0.014 | -0.008 |
| | | (0.011) | (0.015) | (0.015) | (0.021) |
| TPS | | 0.008 | | | |
| | | (0.013) | | | |
| Observations | 6,210 | 8,596 | 4,296 | 4,300 | 8,596 |
| R-squared | 0.039 | 0.043 | 0.064 | 0.038 | 0.044 |
| Control Mean | 0.575 | 0.503 | 0.496 | 0.510 | 0.503 |

*Notes:* Table shows the effect of different treatments on response rate for the first (old) and second (new) experimental sample. TPS is a covariate for traditional public school. All other variables included in the table are randomly assigned characteristics of the emails. Regressions include fixed effects by wave and states. Sample row shows the population for each regression: (1) Old sample (full), which only included charter schools), (2) New sample (full), (3) only traditional public schools for new sample, (4) only charter schools for new sample, and (5) interaction between primary and secondary treatments and TPS for new sample. Nested cluster-robust standard errors (by pair and school for new sample (full and diff TPS-Charter) and only school for others) in parentheses.
    *** $p<0.01$, ** $p<0.05$, * $p<0.1$

**Table A4 - Effect of Message Treatments on Response Rates from Schools for Different Specifications**

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| Bad Grades | -0.024** | -0.027** | -0.027** | -0.022* | -0.022* |
| | (0.011) | (0.011) | (0.011) | (0.012) | (0.012) |
| IEP | -0.052*** | -0.051*** | -0.051*** | -0.056*** | -0.056*** |
| | (0.010) | (0.010) | (0.010) | (0.010) | (0.010) |
| Bad Behavior | -0.070*** | -0.069*** | -0.069*** | -0.070*** | -0.069*** |
| | (0.011) | (0.011) | (0.011) | (0.012) | (0.012) |
| Good Grades | 0.001 | 0.002 | 0.002 | -0.007 | -0.007 |
| | (0.014) | (0.014) | (0.014) | (0.015) | (0.015) |
| Black | -0.012 | -0.014 | -0.014 | -0.019* | -0.019* |
| | (0.010) | (0.010) | (0.010) | (0.011) | (0.011) |
| Hispanic | -0.020** | -0.021** | -0.021** | -0.018* | -0.017* |
| | (0.010) | (0.009) | (0.009) | (0.010) | (0.010) |
| Father | 0.003 | 0.000 | -0.000 | -0.001 | -0.000 |
| | (0.008) | (0.008) | (0.008) | (0.009) | (0.009) |
| Son | -0.011 | -0.010 | -0.010 | -0.009 | -0.009 |
| | (0.007) | (0.007) | (0.007) | (0.007) | (0.007) |
| Two Parents | 0.010 | 0.009 | 0.010 | 0.013 | 0.013 |
| | (0.011) | (0.011) | (0.011) | (0.012) | (0.012) |
| TPS | 0.008 | 0.027** | 0.030** | 0.033** | 0.031* |
| | (0.013) | (0.013) | (0.014) | (0.016) | (0.016) |
| | | | | | |
| Observations | 14,806 | 14,806 | 14,806 | 14,806 | 14,806 |
| R-squared | 0.039 | 0.068 | 0.070 | 0.267 | 0.269 |
| Control Mean | 0.533 | | | | |
| School-level Controls 1 | | X | X | X | X |
| School-level Controls 2 | | | X | X | X |
| Pair Fixed Effects | | | | X | X |
| Tract-level Controls | | | | | X |

*Notes:* Table shows the effect of different treatments on response rate. TPS is a covariate for traditional public school. All other variables included in the table are randomly assigned characteristics of the emails. School-level controls 1 include fraction of school population that is black, Hispanic, free-and-reduced price lunch eligible, female, the school's proficiency rating, and city, suburb, and rural status of the school's location. School-level controls 2 include fraction of the school population that has a disability, proportion of students with disabilities that are taken out of regular class <39%, 40-79%, and 80%+ of the time, fraction of school population that is chronically absent, and fraction of the population that has received an out-of-school suspension. Tract-level controls include disability shares, race/ethnicity shares, share of residents with a Bachelor's Degree, median earnings, poverty rates, and food stamp recipients for the school's associated census tract. Nested cluster-robust standard errors (by pair and school) in parentheses.

*** p<0.01, ** p<0.05, * p<0.1

**Table A5 – Complier Means of School Characteristics**

|  | Total Sample | Bad Grades | IEP | Bad Behavior |
|---|---|---|---|---|
| TPS | 0.291 | 0.581 | 0.025 | 0.205 |
|  | (0.454) | (0.427) | (0.141) | (0.099) |
| % Chronically absent | 0.129 | 0.192 | 0.059 | 0.090 |
|  | (0.178) | (0.161) | (0.054) | (0.041) |
| % Students with IEP | 0.070 | 0.124 | 0.047 | 0.070 |
|  | (0.081) | (0.080) | (0.024) | (0.018) |
| % Proficiency | 0.361 | 0.206 | 0.378 | 0.352 |
|  | (0.279) | (0.289) | (0.085) | (0.065) |
| % White | 0.288 | 0.264 | 0.340 | 0.357 |
|  | (0.307) | (0.297) | (0.093) | (0.072) |
| % Black | 0.251 | 0.389 | 0.177 | 0.189 |
|  | (0.318) | (0.289) | (0.089) | (0.070) |
| % Hispanic | 0.325 | 0.422 | 0.401 | 0.350 |
|  | (0.310) | (0.283) | (0.090) | (0.068) |
| % FRPL | 0.518 | 0.869 | 0.461 | 0.525 |
|  | (0.362) | (0.370) | (0.108) | (0.077) |
| Value added measure | 0.013 | 0.145 | 0.133 | 0.326 |
|  | (1.001) | (1.066) | (0.368) | (0.280) |
| No Excuses Charter | 0.601 | 0.855 | 0.693 | 0.691 |
|  | (0.490) | (0.488) | (0.152) | (0.114) |
| Observations | 14911 | 7472 | 8553 | 7493 |

*Notes:* This Table shows estimates of "complier" means for each treatment message shown in columns (2)-(4) (cf. Abadie (2003)). Complier, as we define it in this analysis, is a school whose response switches from reply if sent a baseline message and do not reply if sent a treatment message shown in the column header. Column (1) shows the overall sample mean and standard deviation in parentheses. Each cell in columns (2)-(4) shows the complier mean for a given characteristic. TPS is an indicator variable for whether a school is a traditional public school. Bootstrap standard errors, computed from 500 repetitions, shown in parentheses.

**Table A6 – Effects of Message Treatments on School Response Rates Adjusting by Multiple Hypothesis Testing**

| Sample | Full Sample | TPS | Charter | TPS - Charter diff. |
|---|---|---|---|---|
| Bad Grades | -0.024* | -0.044* | -0.015 | -0.030 |
| | [0.064] | [0.097] | [0.512] | [0.648] |
| IEP | -0.052*** | -0.002 | -0.065*** | 0.058* |
| | [<0.001] | [0.917] | [<0.001] | [0.053] |
| Bad Behavior | -0.070*** | -0.053** | -0.076*** | 0.021 |
| | [<0.001] | [0.038] | [<0.001] | [0.772] |
| Good Grades | 0.001 | 0.013 | -0.005 | 0.015 |
| | [0.941] | [1.000] | [0.817] | [0.595] |
| Black | -0.012 | -0.004 | -0.015 | 0.010 |
| | [0.631] | [0.818] | [0.578] | [1.000] |
| Hispanic | -0.02 | -0.015 | -0.023 | 0.009 |
| | [0.166] | [1.000] | [0.201] | [1.000] |
| Father | 0.003 | -0.009 | 0.008 | -0.017 |
| | [0.724] | [1.000] | [0.396] | [1.000] |
| Son | -0.011 | -0.007 | -0.011 | 0.004 |
| | [0.441] | [1.000] | [0.599] | [0.76] |
| Two Parents | 0.010 | 0.007 | 0.014 | -0.009 |
| | [0.719] | [1.000] | [0.708] | [1.000] |
| | | | | |
| Observations | 14,806 | 4,296 | 10,510 | 14,806 |
| Control Mean | 0.533 | 0.496 | 0.548 | 0.533 |

*Notes:* Table shows the results of regressing message-treatment indicators on an indicator variable for whether or not a school responded to the message. Columns (1)-(3) show the results for different samples: (1) full sample, (2) only traditional public schools and (3) only charter schools. Column (4) interaction between primary and secondary treatments and TPS. TPS is an indicator variable (not a treatment) for whether a school is a traditional public school. All other variables included in the table are randomly assigned characteristics of the emails. Regressions include fixed effects by wave and state. Adjusted p-values by multiple hypothesis testing in squared parentheses. Multiple hypothesis adjustment was done using Holm's method separately for primary and secondary treatments. *** p<0.01, ** p<0.05, * p<0.1

**Table A7 - Total Population of Charter Schools vs Sample Charter Schools and No Excuses Charter Schools**

| | CRDC | Sample | |
| --- | --- | --- | --- |
| | Charter | Charter | No Excuses Charter |
| % Limited English Proficiency | 0.094 | 0.096 | 0.157 |
| | (0.162) | (0.167) | (0.19) |
| Absenteeism Rate | 0.162 | 0.17 | 0.112 |
| | (0.384) | (0.333) | (0.157) |
| % IDEA Students | 0.086 | 0.088 | 0.069 |
| | (0.166) | (0.146) | (0.054) |
| % of IEP students in regular class <40% of time | 0.004 | 0.002 | 0.001 |
| | (0.03) | (0.024) | (0.006) |
| % of IEP students in regular class 40-79% of time | 0.005 | 0.005 | 0.004 |
| | (0.02) | (0.021) | (0.012) |
| % of IEP students in regular class >80% of time | 0.067 | 0.069 | 0.047 |
| | (0.088) | (0.096) | (0.051) |
| % Students Receiving In-School Suspension | 0.016 | 0.018 | 0.041 |
| | (0.304) | (0.227) | (0.064) |
| % Students Receiving 1 Out-of-school Suspension | 0.024 | 0.027 | 0.045 |
| | (0.299) | (0.223) | (0.042) |
| % Proficiency | 44.475 | 48.374 | 62.733 |
| | (20.445) | (22.515) | (21.945) |
| Value Added Measure | - | 0.000 | 0.775 |
| | | (1.092) | (0.976) |
| Observations | 5813 | 4283 | 272 |

*Notes*: Means are reported for all schools that are available in CRDC 2013-14 dataset. The CRDC does not have data on all schools in the study sample, which was from 2016-17. Charter schools are all schools that are classified by CRDC as charter schools. IDEA students refer to students in the Individuals with Disabilities Education Act. IEP students refers to students that have an Individualized Education Program covered by IDEA. Charter schools may be alternative, magnet, or special education schools. School proficiency scores show the percentage of students scoring at or above proficiency on state assessments across grades and subject as reported by Great Schools in their most recent dataset from 2016. Value-added measure (VAM) is constructed by calculating the normalized difference between the observed proficiency and the predicted proficiency from a regression including school-level and tract-level controls. VAM measures can only be estimated for the sample.

**Table A8 - Effect of Message Treatments on Response Rates by Different Randomized Characteristics**

| Sample | Two parent | One parent | Son | Daughter | Black | Hispanic |
|---|---|---|---|---|---|---|
| Bad Grades | -0.031 | -0.023* | -0.023 | -0.023 | -0.011 | -0.035* |
| | (0.028) | (0.012) | (0.017) | (0.017) | (0.020) | (0.021) |
| IEP | - 0.049* | -0.052*** | -0.051*** | -0.053*** | -0.053*** | -0.038** |
| | (0.026) | (0.011) | (0.016) | (0.015) | (0.018) | (0.018) |
| Bad Behavior | -0.025 | -0.080*** | -0.058*** | -0.084*** | -0.091*** | -0.061*** |
| | (0.027) | (0.013) | (0.017) | (0.017) | (0.020) | (0.021) |
| Good Grades | -0.047* | 0.029 | -0.012 | 0.015 | -0.027 | 0.058** |
| | (0.026) | (0.018) | (0.022) | (0.021) | (0.027) | (0.025) |
| Black | 0.022 | -0.021* | -0.023* | -0.001 | | 0.000 |
| | (0.022) | (0.011) | (0.014) | (0.014) | | (0.000) |
| Hispanic | -0.002 | -0.025** | -0.024* | -0.018 | 0.000 | |
| | (0.022) | (0.011) | (0.014) | (0.014) | (0.000) | |
| Father | 0.011 | 0.001 | 0.007 | -0.001 | 0.017 | 0.010 |
| | (0.018) | (0.009) | (0.011) | (0.011) | (0.013) | (0.014) |
| Son | 0.014 | -0.017** | | | -0.022* | -0.010 |
| | (0.017) | (0.008) | | | (0.013) | (0.014) |
| Two Parents | | | 0.025 | -0.004 | 0.033* | 0.005 |
| | | | (0.016) | (0.016) | (0.020) | (0.019) |
| TPS | 0.003 | 0.010 | 0.003 | 0.012 | 0.006 | 0.007 |
| | (0.019) | (0.015) | (0.015) | (0.015) | (0.020) | (0.019) |
| | | | | | | |
| Observations | 2,916 | 11,890 | 7,375 | 7,431 | 4,999 | 4,916 |
| R-squared | 0.052 | 0.041 | 0.039 | 0.042 | 0.044 | 0.044 |
| Control Mean | 0.522 | 0.535 | 0.532 | 0.551 | 0.546 | 0.523 |

*Notes:* Table shows the effect of different treatments on response rate for different samples. TPS is a covariate for traditional public school. All other variables included in the table are randomly assigned characteristics of the emails. Regressions include fixed effects by wave and states. Sample row shows the population for each regression identified in the e-mails: (1) two parents, (2) one parent, (3) student is a son, (4) students is a daughter, (5) black-sounding name, and (6) Hispanic-sounding name. Two-way cluster-robust standard errors (by pair and school) in parentheses.

*** p<0.01, ** p<0.05, * p<0.1

### Table A9  -  Effects of Message Treatments on the Likelihood of Applying to School

| Sample | Full Sample | TPS | Charter | TPS - Charter diff |
|---|---|---|---|---|
| Bad Grades | -0.045*** | -0.065*** | -0.038*** | -0.027 |
| | (0.009) | (0.015) | (0.011) | (0.019) |
| IEP | -0.054*** | -0.016 | -0.063*** | 0.045** |
| | (0.008) | (0.017) | (0.010) | (0.019) |
| Bad Behavior | -0.071*** | -0.081*** | -0.067*** | -0.016 |
| | (0.009) | (0.015) | (0.011) | (0.018) |
| Good Grades | -0.015 | 0.006 | -0.033** | 0.037 |
| | (0.011) | (0.017) | (0.016) | (0.023) |
| Black | -0.013* | -0.027** | -0.007 | -0.021 |
| | (0.008) | (0.014) | (0.009) | (0.016) |
| Hispanic | -0.003 | -0.002 | -0.003 | 0.001 |
| | (0.008) | (0.013) | (0.009) | (0.016) |
| Father | 0.002 | 0.011 | -0.001 | 0.009 |
| | (0.006) | (0.011) | (0.008) | (0.013) |
| Son | -0.003 | -0.012 | 0.002 | -0.014 |
| | (0.006) | (0.010) | (0.007) | (0.012) |
| Two Parents | 0.010 | 0.016 | 0.004 | 0.011 |
| | (0.008) | (0.011) | (0.012) | (0.016) |
| TPS | -0.020** | | | |
| | (0.009) | | | |
| Observations | 14,806 | 4,296 | 10,510 | 14,806 |
| Control Mean | 0.211 | 0.180 | 0.225 | 0.211 |

*Notes:* Table shows the results of a multivariate regression of a binary indicator for whether a school's response is likely to elicit an application based on trained coders review of school responses. See Appendix C for a description of our qualitative data and reports on inter-rater reliability. Non-response by the school is coded as not likely to apply to the school (or zero). Columns (1)-(3) show the results for different samples: (1) full sample, (2) only traditional public schools and (3) only charter schools. Column (4) interaction between primary and secondary treatments and TPS. TPS is an indicator variable (not a treatment) for whether a school is a traditional public school. All other variables included in the table are randomly assigned characteristics of the emails. Regressions include fixed effects by wave and state. Two-way cluster-robust standard errors (by pair and school) in parentheses.  *** p<0.01, ** p<0.05, * p<0.1

**Table A10 - States Included in Field Experiments**

| States | 1st Experiment | 2nd Experiment |
|---|:---:|:---:|
| Alaska | | ✓ |
| Arizona | ✓ | ✓ |
| Arkansas | | ✓ |
| California | ✓ | ✓ |
| Colorado | ✓ | ✓ |
| Connecticut | | ✓ |
| Delaware | | ✓ |
| DC | | ✓ |
| Florida | ✓ | ✓ |
| Georgia | ✓ | ✓ |
| Illinois | | ✓ |
| Indiana | | ✓ |
| Louisiana | ✓ | |
| Maryland | | ✓ |
| Massachusetts | | ✓ |
| Michigan | ✓ | ✓ |
| Minnesota | ✓ | ✓ |
| Nevada | | ✓ |
| New Jersey | ✓ | ✓ |
| New Mexico | | ✓ |
| New York | ✓ | ✓ |
| North Carolina | ✓ | ✓ |
| Ohio | ✓ | ✓ |
| Oregon | ✓ | ✓ |
| Pennsylvania | ✓ | ✓ |
| South Carolina | | ✓ |
| Tennessee | | ✓ |
| Texas | ✓ | ✓ |
| Utah | ✓ | ✓ |
| Wisconsin | ✓ | ✓ |

*Notes:* Table reports which states were included in the first and second field experiments, respectively.

## Appendix B.

**Data Collection and Implementation**

*First field experiment*

This subsection describes data collection for the first field experiment. Data collection of school contact information took place between August and October 2014. Email and web-form contact information was collected for charter schools from the 17 states with the most charter schools. Once these states were identified we used the U.S. Department of Education's Common Core of Data to determine which public schools identify as charter schools and limited our sample to these schools. The next step involved collecting contact information. To the extent possible, we visited charter school websites to obtain contact information. Sometimes websites were not easily located, so we used databases maintained by state Departments of Education. We emphasized the collection of contact information from school websites, because this is the likely contact information a parent would use if emailing a school. For both first and second field experiments, we prioritized the type of contact information collected. If there is an email address or webform that is used to field general inquiries, then we used this. Otherwise, we would identify a front office receptionist or office manager. If this was not available, then we would look for the contact information for one of the school principals.

To conduct the first experiment, we created several internet domains. For each internet domain, we created email addresses that signal ethnic- and gender-sounding names. In the first and second field experiments, we used Tor browsers to obscure the IP address of the sending location to make it appear messages were sent from local parents.

*Second field experiment*

This describes how we collected data on schools for the second field experiment for the school choice audit study. Data collection for the field experiment took place between Nov. 2017 and Jan 2018.

We worked with research assistants to identify school districts across states that practice some type of intra-district school choice and that also have charter schools within their respective catchment areas. In identifying these school districts, we first focused on whether intra-district choice is practiced. We used geographic information systems (GIS) data from U.S. Census Bureau to identify local school district catchment areas. We also used latitude and longitude data on each school from the U.S. Department of Education's Common Core of Data to identify whether charter schools are geographically situated in school districts that practice intra-district choice.

One challenge is that there is a variation in how intra-district choice is practiced across school districts within states. These practices helped guide the selection of catchment areas to include in our sample. *Open enrollment* represents one end of the spectrum. This type of intra-district school choice is well-studied. See, for example, the research conducted by David Deming on Charlotte-Mecklenburg schools. Bergman (2016) investigates another type of inter-district choice involving transfers in California schools. Typically, open enrollment and transfer policies are well known and formal application processes have been established. Less known are intra-district choice policies that require some form of administrative approval. For example, a superintendent may need to grant approval before parents can send a child to another school in a district, or, within a specific district, principals from sending and receiving schools may have to agree upon a child moving

from one school to another. These types of intra-district choice might be viewed as more restrictive than open-enrollment policies. For school districts that place strong restrictions on intra-district choice, it may not be viewed as unreasonable that teachers, receptionists, front desk and office managers may be neither aware of the policy nor its eligibility requirements.

We restricted our attention to states with the most charter schools such as California, Texas, Florida, Arizona, Ohio, Michigan, New York, Nevada, and the District of Columbia. For each of these states, we then identified the top 40 school districts in terms of student enrollments. Some states, such as Arizona, have mandatory uniform intra-district school choice policies. Uniformity in implementation of intra-district school choice laws simplified the process of selecting school district geographic areas to include in our sample because we did not have to check choice policies district by district. Other states, such as Tennessee, allow intra-district choice but it is only practiced among its largest school district located in Nashville. National Center for Education Statistics Table 4.2 identifies more than 25 different types of policies across states with open enrollment policies.[37]

For each state, our research assistants visited school districts' websites to learn about their intra-district enrollment policy. Once we determined that a school district within a state practices intra-district choice, we made the decision to include that school district's geographic area in our sampling frame (provided there are charter schools within the catchment area).

The NCES data used are for the 2014-2015 school year. Although these are not the latest files from the Common Core of Data, at the time we defined the sampling frame, they were the most recent files that are complete.

For each charter school, research assistants found the nearest traditional public school. We matched the charter school to the nearest traditional public based on the type of school (i.e., regular, special education, etc.) and the entry grade level.

We kept charter schools in our sample (and their respective matched pair) if the charter school enrollment was greater than 200 students. We also limited the number of schools we attempted to contact in a single school district to 80.

In order to implement the experiment, we created 24 domains and developed distinct websites for each domain. For each website, we created email addresses that signal ethnic-sounding names as well as whether a fictitious parent is male or female. When sending emails to schools, we used *Tor* browsers to obscure our IP address and make it appear that the email was being sent from a local parent. For our study, we did not observe any increase in website visits based on the implementation of the experiment.

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[37] See Table 4.2: "Numbers and types of open enrollment policies, by state: 2017."

# Appendix C.

**Content Analysis Methodology**

Content analyses turn unstructured textual content into quantitative data that can be analyzed as such. Specifically, features of text are converted into categorical, ordinal, or binary numbers. The current content analysis quantified the emailed responses from charter schools and traditional public schools subject to school choice to the inquiring "parents," as described above.

A data scientist who specializes in text analytics and the intersection of qualitative and quantitative research methods oversaw the content analysis utilized in this research. Specifically, the researcher created a codebook based on the research hypotheses and a grounded textual analysis of sample responses from the schools to the parents that creates quantitative variables from the text regarding the content of the email responses. The codebook continued to be adapted during preliminary coding.

The content analysis coded for two variables of interest. For whether the response was considered encouraging or discouraging, coders rated this on a four-point scale: very discouraging, somewhat discouraging, somewhat encouraging, and very encouraging. And for whether the response was considered to evoke the likelihood a coder would apply to the charter school or traditional public school, which also was coded on a four-point scale: very unlikely, somewhat unlikely, somewhat likely, and very likely.

The researcher also oversaw the training of four independent coders. Training included coding several emails as a group, working through any questions or differences of opinion on how to code. Then, the coders and researcher each independently coded the same 25 emails. The results were compared, with any discrepancies worked through as a group and resolved. All four coders then recoded the same 25 responses to ensure consistent coding. The codebook creates quantitative variables from the text regarding the content of the email. With consistent quality established, the group then proceeded to code a new set of 100 responses. Inter-coder reliability was calculated in several ways across each coded variable in order to ensure coders were consistent in their approach to coding.

Inter-coder reliability was calculated in four ways. First, average percent agreement is simply the percent of cases agreed upon across all coders. Scores above 80% are considered reliable for most studies, while scores above 70% are considered valid in exploratory studies. In this study, the mean average percent agreement across all coded variables is 96.8%. Second, Pairwise Cohen's Kappa is similar to average percent agreement but also factors in values coded by chance, with scores ranging from -1 to 1. Generally, .41 to .60 is considered moderate reliability, .61 to .80 substantial reliability, and .81 to 1.0 almost perfect reliability. The average across all variables in this study is .92. Fleiss Kapp, which also ranges from -1 to 1, is if the observed agreement is less than expected, while also penalizing 100% agreement. Above .75 is considered excellent. In this study, the average Fleiss' Kappa across coded variables is .92. Finally, Krippendorf's Alpha measured observed and expected agreement, and is considered the gold standard of inter-coder reliability. Scores above .80 are considered adequate. The average across all variables coded in this study is .92.