Can Light-Touch College-Going Interventions Make a Difference? Evidence from a Statewide Experiment in Michigan

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Abstract

I conduct a statewide experiment in Michigan with nearly 50,000 high-achieving high school seniors. Treated students are mailed a letter encouraging them to consider college and providing them with the web address of a college information website. I find that very high-achieving, low-income students, and very high-achieving, minority students are the most likely to navigate to the website. Small changes to letter content affect take-up. For example, highlighting college affordability induces 18 percent more students to the website than highlighting college choice, and 37 percent more than highlighting how to apply to college. I find a statistically precise zero impact on college enrollment among all students who were mailed the letter. However, low-income students experience a small increase in the probability that they enroll in college, driven by increases at four-year institutions. An examination of persistence through college, while imprecise, suggests that the students induced into college by the intervention persist at a lower rate than the inframarginal student. © *2019 by the Association for Public Policy Analysis and Management.*

INTRODUCTION

Due to information constraints and administrative hurdles in the college and financial aid application process, many high-achieving, low-income students either do not apply to college or apply to colleges that are less-selective, under-resourced, and at which they will have a low probability of success (Hoxby & Avery, 2013). Mentoring, in-person application assistance, and other "boots-on-the-ground" strategies to dismantle these hurdles have shown promising impacts, but are relatively expensive to implement (Bettinger & Evans, Forthcoming; Bettinger et al., 2012; Carrell & Sacerdote, 2017; Oreopoulos & Ford, 2019). Other recently evaluated interventions that combine these services with salient financial aid offers are even more effective, but are also costlier (Andrews, Imberman, & Lovenheim, 2017; Dynarski et al., 2018). Researchers evaluating a series of lighter-touch nudge and information interventions have found mixed results, with some studies finding null effects (Bergman, Denning, & Manoli, Forthcoming; Bettinger et al., 2012; Carrell & Sacerdote, 2018; Foote, Shulkind, & Shapiro, 2015; Phillips & Reber, 2018), and others finding

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sizable impacts (Barr & Turner, Forthcoming; Bird et al., 2017; Castleman & Page, 2015; Hoxby & Turner, 2013; Page & Gelbach, 2017).[1]

While the studies finding positive impacts of light-touch interventions provide reason for cautious optimism, they have several limitations. First, with the exception of Bird et al. (2017), the interventions in these studies still tend to cost several dollars per student, which can be a barrier to large-scale implementation for budget-constrained states and school districts. Second, most focus on students who have already taken concrete steps toward applying to college, for example students who have taken a college entrance exam (Hoxby & Turner, 2013) or signed up with the Common Application (Bird et al., 2017), thus missing the large fraction of high-achieving, low-income students who never make it to these points in the college application process (Hyman, 2017a). Finally, a key concern with light-touch policies is that they may reduce informational and administrative hurdles to the college application process, but not provide students with any lasting improvements in their skills or knowledge, thus potentially inducing marginal students to attend but not persist through college. Studies that only estimate effects on attendance in the first year of college (e.g., Bird et al., 2017; Castleman & Page, 2015; Oreopoulos & Ford, 2019; Page & Gelbach, 2017)[2] may overstate program benefits if marginal students induced into college drop out at a higher rate than the inframarginal student.

I conduct a statewide experiment operating at scale with nearly 50,000 high-achieving high school seniors in Michigan. Treated students were mailed a letter from the Michigan Department of Education encouraging them to consider applying to college and providing them with the web address of a website containing information about the college and financial aid application process. The intervention was inexpensive, costing only fifty cents per student. The experimental sample includes all Michigan 11th-grade students during 2013/2014 who scored at least the statewide median on the ACT college entrance exam. Because the ACT was mandatory for Michigan students at this time, no active steps toward college application were necessary for students to enter the sample. The letter contained an individual-specific password allowing me to track who navigates to the website and their browsing behavior on the site. I observe students through their junior year of college, facilitating an examination of up to three years of college persistence in response to the intervention.

Approximately 10 percent of treated students entered their password on the college information website, though this overall take-up rate masks substantial heterogeneity by student characteristic. Non-white students were three percentage points (24 percent) more likely to take-up than white students, economically disadvantaged students were 1.2 percentage points (13 percent) more likely than economically advantaged students, and students scoring higher on the ACT were 6.3 percentage points (85 percent) more likely than students with lower ACT scores. Economically disadvantaged, higher-scoring students and non-white, higher-scoring students had the highest take-up rates suggesting that these students are the most interested in gaining information about the college and financial aid application process.

I find that small changes to letter content affected take-up. For example, including the phrase "Learn how to make college affordable" produced a take-up rate 1.8 percentage points (18 percent) higher than including the phrase "Learn which college is right for you," and 3.2 percentage points (37 percent) higher than the

[1] I focus here on the literature examining light-touch interventions aimed at altering whether and where low-income students enroll in college. A growing, related literature examines the impacts of light-touch interventions targeting the borrowing behavior of existing college students (Barr, Bird, & Castleman, 2017; Marx & Turner, 2018).

[2] Bird et al. (2017) is a working paper, and the authors note that they plan to examine college persistence in a subsequent version of the paper.

phrase "Learn how to apply to college." These differences represent a revealed-preference approach to determining which barriers students perceive to be most salient when applying to college. The findings suggest that most students perceive college affordability to be a more important barrier than understanding which college to apply to or how to apply. Consistent with this result, students were more likely to navigate to website pages, or click on links to external sites, related to college affordability.

Finally, I match the sample to data from the National Student Clearinghouse to examine effects on postsecondary outcomes. I find a statistically precise zero impact on college enrollment among the entire sample; I can rule out effects larger than 0.7 percentage points. However, I also find a suggestive pattern of small, positive impacts for disadvantaged groups, such as economically disadvantaged students and racial minorities. For example, economically disadvantaged students were 1.4 percentage points, or nearly 2 percent, more likely to enroll in college, driven by increased enrollment at four-year colleges. Thus, while this extremely inexpensive and light-touch intervention produced no discernable impact on enrollment for the average student, the heterogeneity analysis suggests possible cost-effective increases for disadvantaged groups.

I next examine impacts on persistence through college. While low statistical precision precludes any firm conclusions, the marginal disadvantaged students induced into college due to the intervention appear to persist through college at a somewhat lower rate than the inframarginal disadvantaged student. I explore and rule out several possible mechanisms for this apparently lower rate of persistence, and conclude that any higher rate of drop-out among these marginal students was likely due to the same unobserved student and household factors that would have led them to not enroll in the absence of the intervention, likely related to a lack of information, support, and familiarity with college.

This paper makes important contributions to the literature evaluating light-touch policies to reduce the income gap in postsecondary attainment. First, the fact that minor changes to letter content affected the proportion of students who navigated to the website suggests that students are sensitive to the design of light-touch interventions, and that minor aspects of intervention design and content can be important to their success.

Second, the intervention that I evaluate, while having no detectable effect among the entire population, seemingly increased high-achieving, economically disadvantaged students' likelihood of enrolling in postsecondary education, and did so at appropriately selective colleges. Unlike prior work finding positive effects of light-touch interventions, this intervention was extremely inexpensive and operated at scale. Perhaps most importantly, because all students in Michigan take the ACT, the sample for this study does not suffer from the same type of selection as do other similar studies: the intervention was not targeted only to students who had already taken concrete steps toward applying to college. This is important, because it suggests that light-touch interventions can affect the college-going behavior of even the most vulnerable students who, in the absence of the intervention, would not have taken a college entrance exam, signed up for The Common Application, or otherwise been on track to apply to a four-year college.

Finally, the fact that the marginal students induced into college by the intervention may have dropped out at a higher rate than the inframarginal student highlights that light-touch interventions may help students enroll in college, but that these students may need additional support to stay enrolled. Researchers should increasingly evaluate programs that support marginal enrollees through college (e.g., Bettinger & Baker, 2014; Castleman & Page, 2016; Oreopoulos & Petronijevic, 2018), and should be sure to examine persistence, in addition to enrollment, when studying light-touch interventions designed to nudge students into college.

The remainder of this paper is organized as follows. In the next section, I first describe the intervention and data, and then report balance across the treatment and control groups. The third section presents results, focusing first on treatment group take-up and web-browsing behavior, and then turning to an evaluation of the effects of the intervention on postsecondary enrollment and persistence. The fourth section explores possible mechanisms, and the fifth section concludes.

THE EXPERIMENT

In this section, I describe the intervention and experimental sample. I then discuss the data, report summary statistics, and compare balance across the treatment and control groups.

The Intervention and Experimental Sample

The basic intervention is a single page letter on Michigan Department of Education letterhead mailed to students during fall 2014, when the students were in twelfth grade. As shown in Figure 1, the letter congratulates students on their ACT score and tells them that they are "receiving this message as part of a free service by the Michigan Department of Education to ensure that students who are qualified to succeed in college have the information necessary to successfully navigate the application process."[3] I randomly assigned four treatment arms to test the relative importance of different information barriers to college application. The second paragraph of the letter begins: "The following link contains information and resources to help you . . . " For students assigned to the first treatment arm, this sentence is completed with the bolded phrase: "learn how to make college affordable." The second arm replaces that phrase with: "learn how to apply to college." The third replaces it with: "learn which college is right for you." Finally, the fourth version of the letter includes all three bolded phrases.[4]

The letters were mailed in letter-size, white business envelopes imprinted with the MDE logo and return address and displaying the student's name and home address through a clear, plastic window (see Figure 2a). In order to test whether mailing timing impacts take-up or college enrollment, I randomly assigned the month the letter was sent (i.e., October, November, or December). I also randomly varied the day of the week the letter would be sent (Monday or Thursday). The rationale for the former was that students may be more apt to respond to the letter at different points in the college application process. The rationale for the latter was that most letters mailed on Monday would arrive during the week, on Tuesday and Wednesday, while most letters mailed on Thursday would arrive during the weekend, on Friday and Saturday, when students may be more or less likely to open their mail.[5]

The letter contains a web address for a "college information website," *micollegeinfo.org*, and an individual-specific password. The web address directs students to a gateway website that I created for this study (Figure 2b) where students are prompted to enter their password, and then are redirected to a publicly available college information website.

[3] Note that all students in both the treatment and controls groups were already mailed an official score report from ACT during the previous spring.
[4] Figure 1 shows the college affordability version of the letter. Appendix Figures A1, A2, and A3 show the other letter versions. All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's website and use the search engine to locate the article at http://onlinelibrary.wiley.com.
[5] A logistical reason to vary the timing of the mailings was that MDE staff recommended against sending out nearly 25,000 letters from the MDE mail services department on a single date, due to processing constraints.

STATE OF MICHIGAN
DEPARTMENT OF EDUCATION
LANSING

RICK SNYDER
GOVERNOR

MICHAEL P. FLANAGAN
STATE SUPERINTENDENT

[Student first and last name]
[Student address]
[Student city, state zip]

Dear [Student first name],

Congratulations on your score of [ACT Score] on the ACT, which you took as part of the Michigan Merit Exam in March. Your score suggests that you are ready to enroll and succeed in college. You are receiving this message as a free service from the Michigan Department of Education to ensure that students who are qualified to succeed in college have the information necessary to successfully navigate the application process.

The following link contains information and resources to help you ***learn how to make college affordable***, and more. After navigating to the link, you will need to enter your personal password, provided below.

College Information Website: **micollegeinfo.org**
Your Personal Password: [password]

By entering your password, you will automatically be entered into a drawing to ***receive a free iPad Mini!**** You can also scan below to navigate to the above link.

If you have any questions about this letter, please call (517)-258-0294 or email hymanj@michigan.gov.

Congratulations again on your ACT score and readiness to succeed in college. I am excited for the benefits that your college attendance and future successes can bring to you and to the State of Michigan. Good luck!

Sincerely,

Venessa A. Keesler, Ph.D.
Deputy Superintendent, Education Services
Michigan Department of Education

*NO PURCHASE NECESSARY. Complete rules at micollegeinfo.org/rules.pdf.

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Figure 1. Sample Letter (College Affordability Version).

I randomly assign half of the passwords to redirect students to *https://KnowHow2GOMichigan.org* (see Figure 3). This college information website was created and maintained by the Michigan College Access Network (MCAN), a Michigan-based non-profit organization.[6] I chose this website because through

[6] The website is no longer active.

(a)

(b)

Notes: Figure 2(a) shows the envelope used to mail the letters. Figure 2(b) shows the gateway website set up for this project, micollegeinfo.org.

Figure 2. Letter Envelope and Gateway Website.
[Color figure can be viewed at wileyonlinelibrary.com]

my collaboration with MCAN, I could track the web-browsing behavior of students who navigated to this site. The other half of the passwords redirected to *https://bigfuture.collegeboard.org/*, a college information website created by and

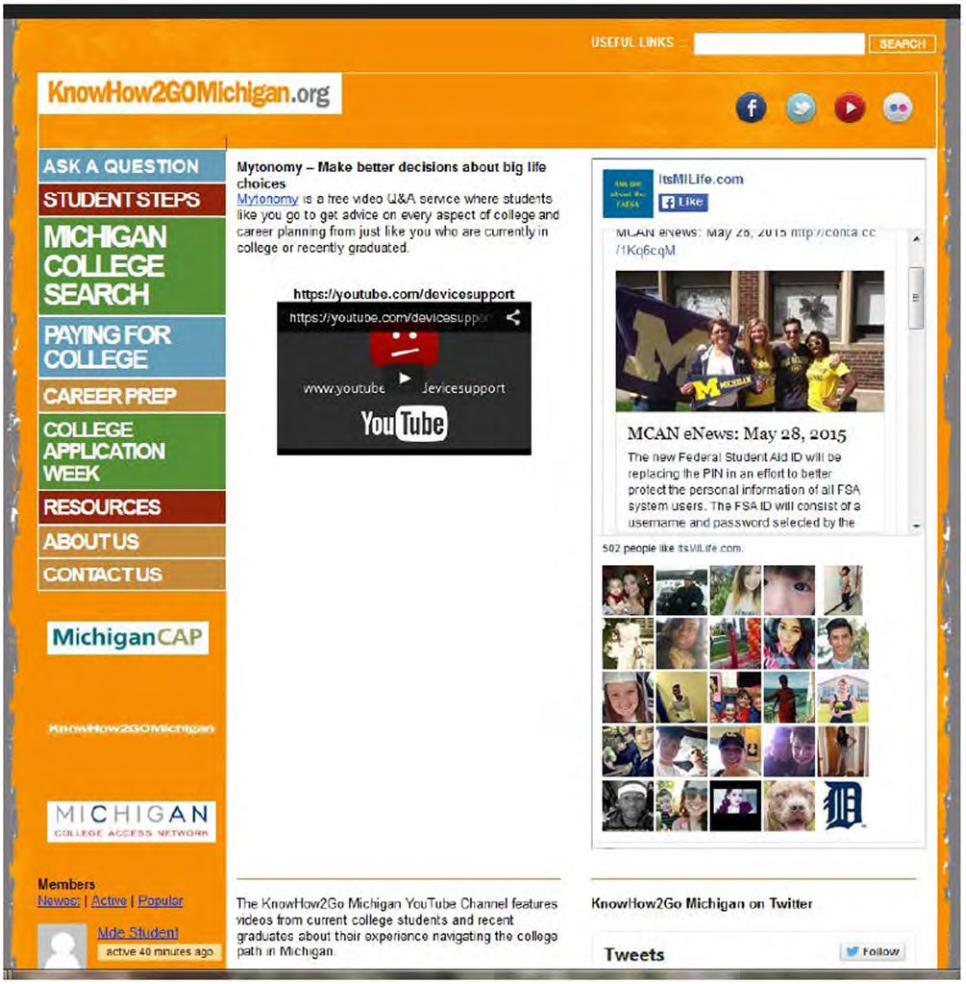


Figure 3. College Information Website One—KnowHow2GOMichigan.org. [Color figure can be viewed at wileyonlinelibrary.com]

maintained by The College Board (see Figure 4). Both sites were established with the intent to provide information about the college and financial aid application process, but The College Board website is well funded, more attractive, and arguably better designed and easier to navigate. On the other hand, most students enroll in college in the state in which they attended high school, and many financial aid opportunities are state-specific. The MCAN website focuses on Michigan-specific college and financial aid information. The website randomization allows a test of whether any effects of the intervention on college enrollment outcomes differ by the website to which a student is directed.[7]

[7] Note that I can only track web-browsing behavior at *https://KnowHow2GOMichigan.org*. I contacted The College Board to ask permission to track web-browsing behavior at their website for this project, but the request was denied.

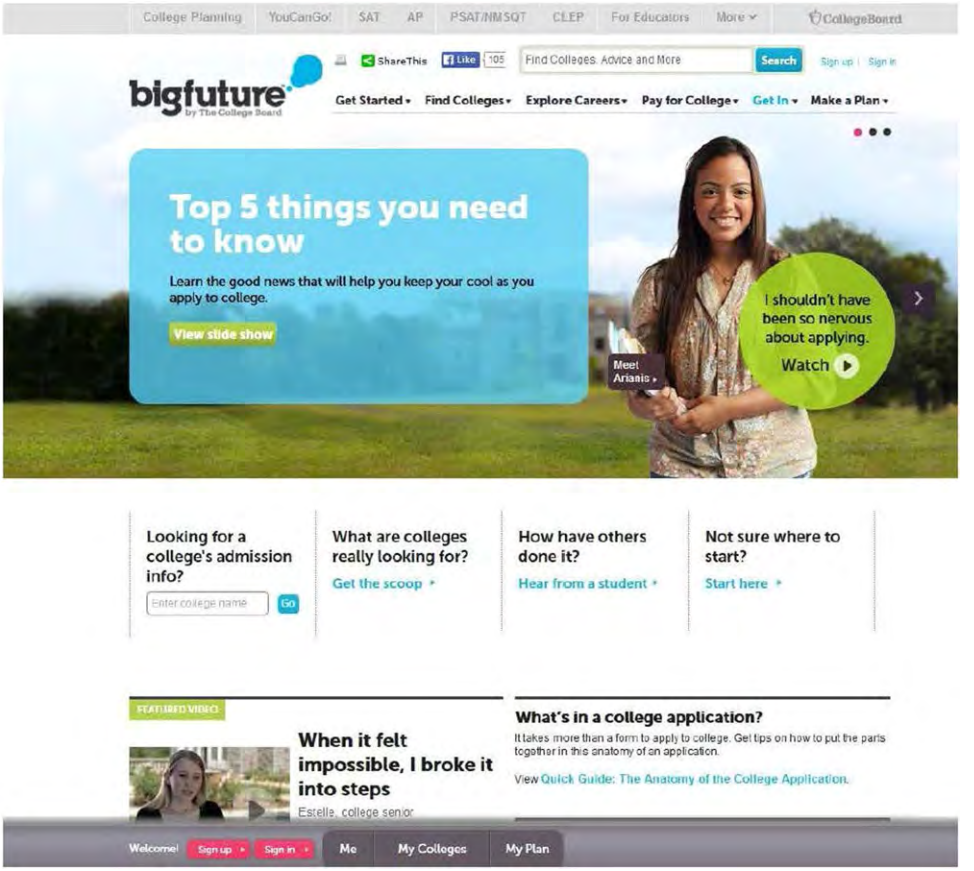


Figure 4. College Information Website Two—College Board BigFuture. [Color figure can be viewed at wileyonlinelibrary.com]

The experimental sample contains the 49,156 11th-grade public school students in Michigan during 2013/2014 who scored at least a 20, the statewide median, on the ACT, which was a mandatory exam for juniors in Michigan at this time. An ACT score of 20 was also the 25th percentile of Michigan students in the classes of 2008, 2009, and 2010 who earned a Bachelor's degree within six years of high school graduation. ACT Inc. cites a score of 20 as likely qualifying a student for admission to a "traditional" four-year institution.[8] In summary, the ACT score of 20 as the sample cutoff reflects my choice of a threshold that represents students with a good chance of admission to, and success at, a somewhat selective four-year institution.

I randomly assigned half of the 49,156 students in the experimental sample to a treatment group and half to a control group.[9] The 24,578 students in the control group received no additional information as part of this study. The 24,578

[8] See ACT Inc (2002). A score of 18 to 21 likely qualifies a student for admission to non-selective institutions, 20 to 23 to traditional institutions, 22 to 27 to selective institutions, and 27 to 31 (or higher) to highly selective institutions.
[9] I stratified the random assignment by sex, race, free lunch status, urbanicity, and ACT score.

students assigned to the treatment group received the intervention described above.

Data

I use an individual-level dataset containing all 11th-grade students in Michigan public schools during the 2013/2014 school year. The bulk of these data comes from the Michigan Department of Education (MDE) and Michigan Center for Educational Performance and Information (CEPI) student longitudinal database. These data contain time-invariant demographics such as student sex and race, as well as time-variant characteristics measured during eleventh grade, such as free and reduced-price lunch eligibility, which I use as a proxy for student economic disadvantage. Students' ACT score is from the ACT exam that all students take during the spring of eleventh grade as part of Michigan's statewide standardized testing.[10] The score is an official ACT score usable for college admissions. Students' grade point average (GPA) during eleventh grade, which I use as an additional baseline achievement measure, is also obtained from the MDE and CEPI database.

I use student names and home addresses to mail the letter, though this personally identifiable information was stripped from the dataset after the mailing was complete and replaced with unique student identifiers. Student addresses are reported to MDE from districts three times per year, during the fall, winter, and end-of-school-year. I used the most recent address at the time of setting up the experiment, which was winter of eleventh grade.

For the purposes of this experiment I set up a web tracking system linked to the unique student identifiers in order to track which students navigate to the web address provided in the mailing and to examine their web-browsing on the *KnowHow2GOMichigan.org* college information website. In addition to viewing which students access the website and where they navigate, I observe the date and time they access the site, the amount of time they spend on each page, their IP address, their internet browser (e.g., Microsoft Explorer, Mozilla Firefox), and any link they click to exit the website.

Student-level postsecondary enrollment information is obtained by matching students to the National Student Clearinghouse (NSC). The NSC is a non-profit organization that houses postsecondary enrollment information on over 90 percent of undergraduate enrollment nationwide, though the coverage rate for Michigan during this period was over 95 percent (Dynarski, Hemelt, & Hyman, 2015). Most of the non-participating institutions were for-profit colleges. Prior to stripping the data of the identifying information, the Michigan Department of Education sent the full names and dates of birth for the students in my sample to the NSC, which matched the students to its database. While any errors in the matching should be balanced by treatment status, it is possible that treatment could induce students into or out of non-participating postsecondary institutions, which would bias the treatment effects on postsecondary enrollment downward or upward, respectively. However, given the greater than 95 percent coverage rate in Michigan, any such bias would be very small.

Finally, high school characteristics, such as urbanicity (i.e., urban, suburban, town, or rural status) and fraction of students eligible for free or reduced-price lunch,

[10] See Hyman (2017a) and Garlick and Hyman (2018) for more details about the mandatory ACT exam in Michigan. I generally refer to the ACT composite score, though the data also include the ACT math, science, reading, and English subscores. The mandatory ACT in Michigan does not include the optional writing portion of the exam.

Table 1. Sample means (balance table).

		RCT sample		
	Entire cohort (1)	All (2)	Control (3)	Treatment (4)
Student Demographics				
Female	0.500	0.516	0.516	0.516
White	0.743	0.851	0.850	0.852
Black	0.168	0.064	0.064	0.064
Hispanic	0.048	0.030	0.031	0.030
Asian	0.030	0.045	0.045	0.045
Economically Disadvantaged (ED)	0.434	0.269	0.269	0.268
Special Education	0.090	0.020	0.019	0.021
School Characteristics				
City	0.194	0.097	0.098	0.097
Suburb	0.470	0.538	0.537	0.538
Town/Rural	0.336	0.365	0.365	0.365
High ED	0.630	0.502	0.502	0.502
Num. 11th Graders	265.7	294.7	294.4	295.0
Charter	0.047	0.028	0.029	0.027
Title I	0.148	0.076	0.076	0.076
Student Achievement				
Grade 11 GPA	2.68	3.20	3.20	3.21
State Math Score	0.026	0.647	0.649	0.646
State Reading Score	0.019	0.692	0.691	0.693
ACT Composite	19.8	24.1	24.1	24.1
ACT Math	19.7	23.5	23.5	23.5
ACT Science	20.2	24.0	24.0	24.0
ACT English	19.0	24.0	24.0	24.0
ACT Reading	19.9	24.4	24.4	24.5
Observations	101,845	49,156	24,578	24,578

Notes: Table shows sample means. For every variable, I test whether the mean of the treatment group (column 4) equals the mean of the control group (column 3), with none rejecting equality at conventional significance levels. High (low) school economic disadvantage is above (below) the median fraction eligible for free or reduced-price lunch in the RCT sample.

are obtained from the National Center for Education Statistics (NCES) Common Core of Data (CCD).

Balance

Table 1 reports sample means by treatment status for the experiment. The first column reports student characteristics for the entire cohort of Michigan 11th-grade students with a valid ACT score during 2013/2014 in order to show the differences between that sample and the experimental sample of students who scored at least a 20 (column 2). Columns 3 and 4 split the main sample into the control and treatment groups, respectively.[11]

[11] In Table A1, I report sample means by treatment arm and mailing timing, testing the equality of each mean against the control group. As in Table 1, balance is strong, with only four of the over 200 means

Comparing the entire cohort of 11th-grade students to those who scored at least a 20 on the ACT, the latter (experimental) sample has a slightly higher fraction female (51.6 percent compared to 50.0 percent), higher fraction white (85.1 percent compared to 74.3 percent) and Asian (4.5 percent compared to 3.0 percent), and lower fraction black (6.4 percent compared to 16.8 percent), Hispanic (3.0 percent compared to 4.8 percent), and economically disadvantaged (26.9 percent compared to 43.4 percent). Students in the experimental sample are less likely to be from high schools in cities (9.7 percent compared to 19.4 percent), and more likely to be from high schools in suburban areas (53.8 percent compared to 47 percent) and towns/rural areas (36.5 percent compared to 33.6 percent). Highly economically disadvantaged high schools, defined as those above the median fraction eligible for free or reduced-price lunch, are less represented in the experimental sample than in the entire cohort (50.2 percent compared to 63 percent). Students in the experimental sample are, by design, positively selected on measures of student achievement. For example, the mean 11th-grade GPA is 3.20 compared to 2.68 among the entire cohort. Similarly, the mean ACT composite score is 24.1 compared to 19.8 among the entire cohort.

I now turn to columns 3 and 4 to explore balance between the treatment and control groups. I find no statistically significant differences between the two groups across any of the student or school characteristics that I examine. The statistical tests are conducted by regressing each characteristic on the treatment indicator, clustering the standard errors at the school level. The F-test that all characteristics are jointly equal to zero has a *p*-value of 0.967. In summary, Table 1 provides strong evidence of balance, and thus that it is valid to estimate the causal impact of the intervention by comparing mean outcomes in the treatment group to those in the control group.

RESULTS

Given that the experiment was successful at randomizing students into treatment and control groups, I now turn to presenting the results of the experiment. In this section, I focus first on take-up of treated students entering their password on the gateway website, and describe their website browsing behavior. I then examine impacts on postsecondary outcomes by comparing treated- to control-group students, focusing initially on postsecondary enrollment and then on persistence through college.

Take-Up

Many of the treated students in this study may never open the envelope mailed to them by the Michigan Department of Education, or may open it and read the letter but never navigate to the recommended *micollegeinfo.org* web address. In this section, I explore what fraction of treated students "take-up" the intervention by navigating to *micollegeinfo.org* and entering their password on that website.[12] In Table 2, column 1, I present the take-up rate for the overall treatment group in row 1, and then in subsequent rows present heterogeneity in the take-up rate by

being statistically different from the control group. All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's website and use the search engine to locate the article at http://onlinelibrary.wiley.com.

[12] Included in the denominator is the approximately 2 percent of letters that were returned with an invalid or outdated address.

Table 2. Treatment group take-up rates and heterogeneity.

		Difference		
	Take-up rate (1)	Pctg. pts. (2)	Percent (3)	P-value (4)
All Treated Students	0.098			
By Student Demographic				
Male	0.099			
Female	0.097	0.003	2.6	0.527
Non-White	0.124			
White	0.094	0.030	24.2	0.000
ED	0.107			
Non-ED	0.095	0.012	12.5	0.011
By School Demographic				
City	0.114			
Suburb	0.096	0.018	19.2	0.009
Town/Rural	0.097	0.017	18.0	0.016
High ED	0.099			
Low ED	0.097	0.002	2.1	0.662
By Student ACT Score				
20–22	0.074	0.063	85.1	0.000
23–25	0.091	0.046	50.8	0.000
26+	0.137			
By Letter Content				
Affordability	0.120			
How to Apply	0.087	0.032	37.1	0.000
College Choice	0.102	0.018	18.0	0.003
All Three	0.084	0.036	43.6	0.000
By Letter Timing				
October	0.100			
November	0.099	0.001	1.3	0.804
December	0.095	0.005	5.3	0.287
Monday	0.097	0.002	2.2	0.574
Thursday	0.099			

Notes: The sample is the 24,578 students in the treatment group. Column 1 shows mean take-up rates for the entire treatment group (row 1) and by student and letter characteristics (subsequent rows). Column 2 shows the difference in the take-up rate between the indicated group and the omitted group in percentage points. Column 3 represents this difference as a percent relative to the indicated group's level. Column 4 shows the p-value of the test of equality between the two groups. ED = Economically Disadvantaged.

student and school characteristics and by student ACT score. In the final rows of Table 2, I present take-up rate by treatment arm and mailing timing. Column 2 shows the differences in take-up rate between these groups, and column 3 presents these differences in percent terms. Finally, column 4 shows the *p*-value from the test of equality of the take-up rate across the groups.[13]

Overall, 9.8 percent of students navigated to *micollegeinfo.org* and entered their password. I find no significant difference in take-up by student gender. Non-white

[13] Statistical tests are conducted by separately estimating for each characteristic (e.g., sex, race, letter content) a regression of an indicator for take-up on the group indicator(s). These regressions are estimated for the treated students only, and standard errors are clustered at the school level. *P*-values are from the test that the coefficient(s) on the group indicator(s) equals zero.

students were 3 percentage points (24 percent) more likely than white students to navigate to the website (statistically significant difference, with *p*-value of 0.000). Economically disadvantaged students were 1.2 percentage points (13 percent) more likely than economically advantaged students to take-up (*p*-value of 0.011). Urban students were 1.8 and 1.7 percentage points (19 percent and 18 percent) more likely to take-up than suburban and town/rural students, respectively (*p*-values of 0.009 and 0.016).[14] Counter to the result by student economic disadvantage, there was no difference in take-up by school economic disadvantage. Students with higher ACT scores were much more likely to navigate to the site, with a take-up rate of 13.7 percent for students with scores of 26 or greater, 85 percent higher than the rate of 7.4 percent for students with scores of 20 to 22 (*p*-value of 0.000).[15] Taken together, these results suggest that high-scoring, economically disadvantaged and high-scoring, minority students have the greatest rates of take-up. Figure 5 shows this to be true: high-scoring, economically disadvantaged students have a take-up rate of 16.6 percent (Figure 5a) and high-scoring, non-white students have a take-up rate of 17.2 percent (Figure 5b).

In addition to understanding which types of students have unmet need regarding information about the college and financial aid application process, my experiment also aims to identify the topics about which students seek information. As described earlier, I explore this issue by splitting the treatment group into four arms that receive different letter versions, each with a different bolded phrase meant to highlight a specific information barrier to college application. If I find that some versions of the letter cause higher take-up, this would suggest that students find those barriers more salient.

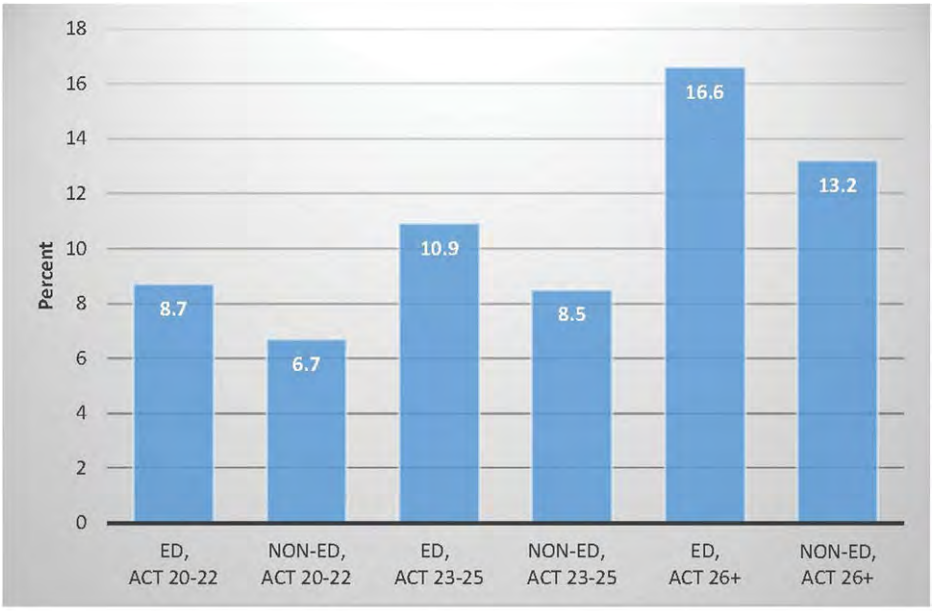
I find substantial differences in take-up across the different versions of the letter (see Table 2). The letter that emphasized college affordability produced the highest take-up rate, 1.8 percentage points (18 percent) higher than the letter that emphasized college choice (*p*-value from test of equality is 0.003), and 3.2 percentage points (37 percent) higher than the letter that emphasized how to apply (*p*-value of 0.000). This pattern is the same for economically disadvantaged and advantaged students (see Figure 6).

The letter that included all three barriers produced the lowest take-up rate. There are at least two plausible explanations for this finding. First, prior work in survey methodology has found benefits to emphasizing certain information through the use of boldface, italics, or underlined print, but that these returns diminish when too much information is emphasized (Dillman, Smyth, & Christian, 2009). By putting all three phrases in boldface, I may have emphasized too much information relative to the letters where I emphasized only a single barrier. Second, prior work in economics has found evidence of "left-digit-bias," where people focus on the leftmost digits of numbers (Busse et al., 2013; Lacetera et al., 2012). Students who received the letter that included all three barriers may have responded similarly, focusing primarily on the first of the three barriers presented. The first barrier listed was "how to apply," which when included on its own produced the lowest take-up rate of 8.7 percent, nearly identical to the 8.4 percent rate for the "all three" letter version.

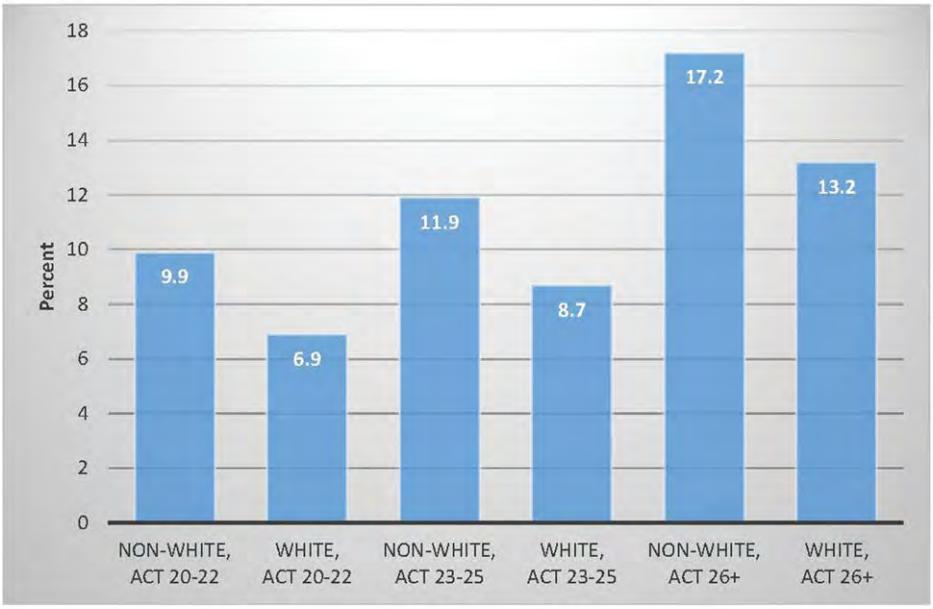
Finally, I find that the timing of the mailing, either by month or by day of the week had no impact on take-up.

[14] This pattern does not reflect differences by student economic disadvantage, as the pattern by urbanicity is similar for economically disadvantaged and advantaged students (see Figure A4a). All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's website and use the search engine to locate the article at http://onlinelibrary.wiley.com.

[15] There is even greater variation in take-up within the group scoring 26 or higher, with take-up of 10 to 13 percent for students scoring 26 to 29, and take-up of 16 to 32 percent for students scoring 30 to 36 (see Figure A4b).



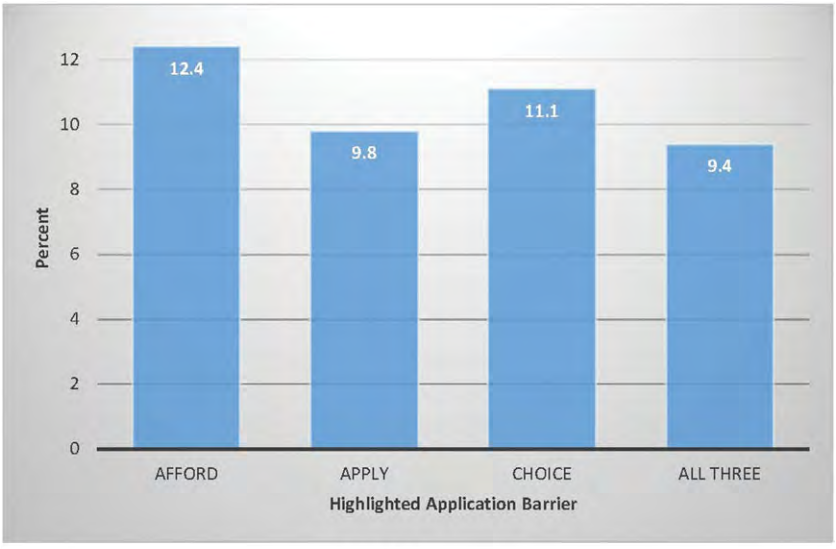
(a)



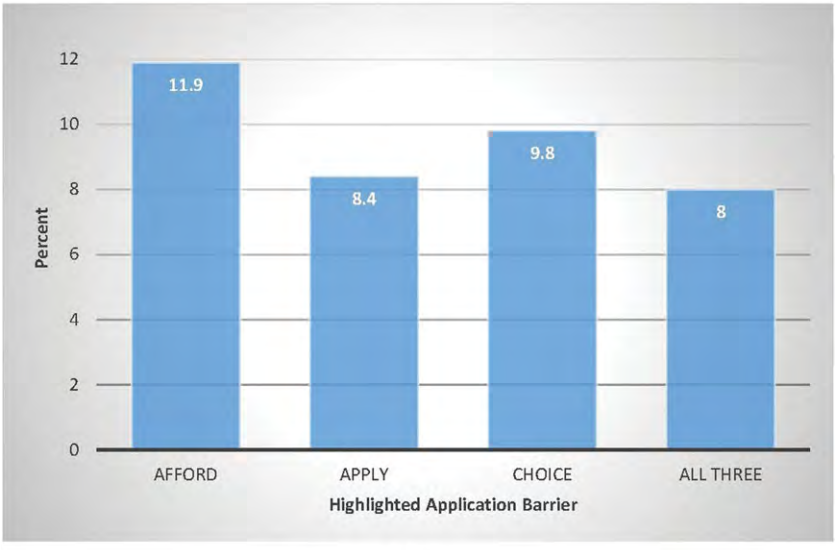
(b)

Notes: Figure 5(a) shows letter take-up rates by student economic disadvantage (ED) and ACT score. Figure 5(b) shows take-up rates by student race and ACT score. Take-up is defined as a student entering his or her password into the gateway website, micollegeinfo.org. The pairwise differences in take-up in Figure 5(a) are all statistically significant at the 95 percent level or higher, with the exception of the 8.7 and 8.5 percent take-up among ED, low-scoring students and non-ED, medium-scoring students, respectively. The only pairwise differences in Figure 5(b) that are not significant at the 90 percent level or higher are the 9.9 and 11.9 (bars 1 and 3), 9.9 and 8.7 (bars 1 and 4), and 11.9 and 13.2 (bars 3 and 6).

Figure 5. Take-Up by Student Economic Disadvantage, Race, and ACT Score. [Color figure can be viewed at wileyonlinelibrary.com]



(a)



(b)

Notes: Figures 6(a) and 6(b) show letter take-up rates for economically disadvantaged (ED) and non-ED students, respectively, by letter version. Take-up is defined as a student entering his or her password into the gateway website, micollegeinfo.org. The only pairwise differences in Figure 6(a) that are statistically significant at the 95 percent level or higher are between "Afford" (12.4) and "Apply" (9.8), and between "Afford" (12.4) and "All Three" (9.4). The only pairwise difference in Figure 6(b) not statistically significant at the 95 percent level or higher is the difference between "Apply" (8.4) and "All Three" (8.0).

Figure 6. Take-Up by Student Economic Disadvantage and Letter Content. [Color figure can be viewed at wileyonlinelibrary.com]

The heterogeneity by student and school characteristics arguably represents a revealed preference approach to determining which types of students (or their parents) desire information about the college and financial aid application process. I find that, consistent with past work (e.g., Hoxby & Avery, 2013), the students with the most unmet need for this information are extremely high-achieving, economically disadvantaged and minority students. I also provide a revealed preference approach to determining students' perceived information barriers to college application, and find that college affordability is the barrier that resonates most for both economically disadvantaged and advantaged students alike.

Finally, because I observe the date and time that students enter their password on the *micollegeinfo.org* gateway website, I can examine the timing of when students (or their parents) tend to go online, revealing novel information about how students and their families learn about applying to college. In Figures A5 and A6,[16] I show the distributions of the time-of-day and day-of-week that students first enter their password online, and also the number of days and weeks between the mailing and when students first enter their password. Students tend to enter their password in the evening and early morning (8:00 p.m. to 3:00 a.m.), though are relatively more likely to enter it during the afternoon (2:00 p.m. to 8:00 p.m.) on the weekend than during the week. Students are most likely to first enter their password within a couple days after the mailing, though a non-trivial share file away the letter and first enter their password several months after the mailing, especially students mailed the letter in October relative to those mailed the letter in December.

Browsing Behavior

For the 1,167 students who entered their password on the gateway site and were directed to *KnowHow2GOMichigan.org,* I can examine their web-browsing behavior as another way to learn which college information barriers students perceive as most important. Please note that this is a descriptive analysis with no claim to causality, as there exists no adequate control group.

Panel A of Table 3 examines which types of webpages students navigated to within the site.[17] I focus on three tabs on the main vertical menu-bar on the left-hand-side of the homepage (see Figure 6) that correspond loosely to the information barriers highlighted in the three letter versions. These tabs are labeled: "Paying for College," "Student Steps," and "Michigan College Search." I find that 35.9 percent of students clicked on the tab "Paying for College," while only 13.3 and 13.5 percent, respectively, navigated to the "Student Steps" and "Michigan College Search" pages. As in the prior analysis of take-up, these results demonstrate students' revealed interest in learning about college affordability. Students who received the letter emphasizing college affordability were even more likely to navigate to the "Paying for College" page (51.5 percent), while students who received the letter highlighting "learn which college is right for you" were more likely to navigate to the "Michigan College Search" page (21.2 percent).[18]

[16] All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's website and use the search engine to locate the article at http://onlinelibrary.wiley.com.

[17] In general, the mean number of pages that students navigated to on the website, including the homepage, was three pages. Students spent an average of three minutes navigating the website.

[18] Note that these differences could partially reflect the letter type changing students' browsing behavior by prompting them to focus on the highlighted barrier, but could also reflect the letter type changing the composition of students navigating to the website to favor those students interested in the highlighted barrier.

Table 3. College information website browsing behavior.

Panel A. What Types of Webpages Did Students Navigate To?

		By letter content			
Percent navigating to:	All letters (1)	Affordability (2)	Apply (3)	Choice (4)	Includes all 3 (5)
Paying for College	35.9	51.5	26.5	24.4	37.1
Student Steps	13.3	13.9	11.1	15.6	11.7
College Search	13.5	8.6	11.1	21.2	13.3

Panel B. What External Links Did Students Navigate To?

Top five websites students leave to:	Percent	Cum. percent
Michigan College Access Portal	25.3	25.3
MDE Michigan Scholarship Website	15.2	40.5
Search for Scholarships	12.0	52.5
See if You're Eligible for a Pell Grant	7.1	59.6
See if You're Eligible for an Academic Competitiveness Grant	7.1	66.7
Other	33.3	100.0

Notes: The sample is the 1,167 students who entered their password on the gateway website and were directed to the *KnowHow2GOMichigan.org* college information website. Panel A shows the percent of students who navigated to different pages on that website by the letter version they were sent. Panel B shows the top five external website links to which students navigated.

Table 3, panel B focuses on the students who left the *KnowHow2GOMichigan.org* website by clicking on a link to an external site. This panel shows the top five websites that students visited and the percentage of these students who visited that site. Four of these five sites are related to college affordability (e.g., "MDE Michigan Scholarship Website" and "See if You're Eligible for a Pell Grant"), and the top two sites are Michigan-specific (e.g., "Michigan College Access Portal"). These results again reinforce the revealed desire for information about college affordability, but also highlight that students tend to seek state-specific information about the college and financial aid application process.[19]

College Enrollment

I now turn to estimating the effects of the intervention on postsecondary enrollment, choice, and persistence. The randomized nature of the experiment motivates a straightforward empirical strategy comparing the mean postsecondary outcome among the treatment group to that in the control group. Specifically, I estimate the following OLS regression:

$$y_{is} = \beta_0 + \beta_1 Treat_i + X_{is} + \mu_{is} \quad (1)$$

where y_{is} is the postsecondary outcome for student, i, from high school, s; *Treat* is an indicator for whether the student was mailed a letter; X includes the baseline

[19] I present web-browsing behavior separately by student economic disadvantage in Table A2. Economically disadvantaged students are somewhat more likely to click on the tab "Student Steps" and less likely to click on the tab "Paying for College"; however, these differences are not statistically significant. There is little difference between the two types of students in the external links to which students navigate.

student- and school-level covariates presented in Table 1; and μ is a random disturbance term.[20]

Results based on the estimation of equation (1) are presented in Table 4, where the dependent variable is an indicator for whether a student enrolls in any college within two years after scheduled on-time high school graduation based on the year the student was in eleventh grade.[21] The standard errors reported in Table 4 and all subsequent tables are clustered at the school-level to allow for within-school autocorrelation of the disturbance term.

Among the entire sample, there is a near zero and statistically insignificant impact on the probability a student enrolls in any college (Table 4, column 1, row 1). The 0.001 coefficient is precise enough (standard error of 0.003) that I can rule out effects larger than 0.7 percentage points for the average student in my sample.

Looking by student characteristics, the effect is larger, but still statistically insignificant for males.[22] There is a 1.4 percentage point increase in college enrollment among economically disadvantaged students, which is statistically significant at the 95 percent level and represents a 1.8 percent increase off the control mean of 76.4 percent. The coefficient for economically advantaged students is small and statistically insignificant, and I can reject the equality of the coefficients for the economically disadvantaged and advantaged samples, with a p-value of 0.019. The point estimate for non-white students is similar in magnitude to the point estimate for economically disadvantaged students, but is statistically insignificant given the smaller sample size and thus lower statistical precision.[23]

This similarity in the point estimates may reflect that non-white students in my sample are more likely to be economically disadvantaged. Alternatively, effects could be larger for minority students, even conditional on income, due to institutional racism and potential differences in college-going culture by race. To examine heterogeneity by race conditional on heterogeneity by income, I include as independent variables in a single regression: 1) treatment, 2) treatment interacted with economic disadvantage, and 3) treatment interacted with minority status. After controlling for heterogeneity by income, I still find that the effect for non-white students is positive, similar to the effect among economically disadvantaged students, and statistically imprecise.

I present heterogeneity by school characteristics (i.e., urbanicity and economic disadvantage) in the final five columns of Table A6. It is worth noting that while the pattern of heterogeneity tends to show positive coefficients for the more disadvantaged groups, this pattern is not observed by student ACT score or by

[20] See Table A3 for the main postsecondary enrollment, choice, and persistence results estimated excluding the vector of controls. The results are nearly identical, consistent with the strong balance presented in Table 1. Given that the outcome variables are dichotomous, I also show in Table A4 that the results are nearly identical when estimated using logit instead of OLS. All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's website and use the search engine to locate the article at http://onlinelibrary.wiley.com.

[21] This outcome can be thought of as a liberal measure of on-time college enrollment that in addition to capturing students who graduate high school on time and immediately enroll in college, also captures students either graduating high school on time and taking a gap year before enrolling, or students who take an extra year to graduate high school and then enroll the following fall.

[22] For each student subgroup examined in Table 4, I examine balance across treatment and control for the 22 characteristics presented in Table 1. I find near perfect balance, with no statistically significant differences for most subgroups, and one statistically significant difference for three subgroups. For every subgroup, I conduct an F-test that all characteristics are jointly equal to zero, and fail to reject the null in every case.

[23] I show in Table A5 that, consistent with Hoxby and Avery (2013), the results for economically disadvantaged students are larger at schools where there are a lower number of high-achieving, economically disadvantaged students. However, due to small sample sizes, the differences across the groups are statistically imprecise.

Table 4. Effects on college enrollment, by student subgroup.

		Sex		Economic disadvantage		Race		ACT score	
Dependent variable	All students (1)	Male (2)	Female (3)	ED (4)	Non-ED (5)	Non-white (6)	White (7)	Low (8)	High (9)
Enroll in College	0.001	0.007	−0.005	0.014**	−0.005	0.013	−0.002	0.000	0.001
	(0.003)	(0.005)	(0.004)	(0.007)	(0.003)	(0.009)	(0.003)	(0.005)	(0.004)
	0.843	*0.811*	*0.873*	*0.764*	*0.872*	*0.838*	*0.844*	*0.798*	*0.891*
Observations	49,156	23,799	25,357	13,199	35,957	7,311	41,845	25,481	23,675

Notes: Each point estimate is from a separate regression. Standard errors in parentheses are clustered at the school level. Control means are in italics below the standard errors.
*** = significant at 1 percent level; ** = 5 percent level; * = 10 percent level.

school economic disadvantage.[24] Also, the positive impact for economically disadvantaged students but not students at economically disadvantaged schools is consistent with the previous result that take-up is higher among economically disadvantaged students, but not at economically disadvantaged schools.[25] This contrasts with prior work examining the impacts of education policy, specifically class size reduction during elementary school, on students' college enrollment, which finds more dramatic effects for students at the most economically disadvantaged schools than for economically disadvantaged students (Dynarski, Hyman, & Schanzenbach, 2013).

I turn now to examining effects by college type (Table 5). The increase in college enrollment among economically disadvantaged students is driven by increases in attendance at four-year institutions. Economically disadvantaged students are 1.7 percentage points, or 3.2 percent, more likely to attend a four-year college during the two years after scheduled on-time high school graduation (significant at the 95 percent level). Again, I can reject equality of the coefficients for the economically disadvantaged and advantaged samples, with a p-value of 0.030. There is a small, negative, and statistically insignificant effect on enrolling only at a two-year college.[26] There is a marginally significant increased probability that economically disadvantaged students attend a selective college, defined as being in the top two Barron's selectivity categories (e.g., the second to highest Barron's category includes the University of Michigan). There is also a statistically significant increase in selective college enrollment among students at economically disadvantaged high schools. The near zero estimates for any college enrollment among low-scoring students and students at economically disadvantaged schools masks some switching from two-year only to four-year enrollment, though these results are statistically imprecise. This pattern may also be at play in rural schools, where there is a marginally significant decrease in two-year enrollment, and similar sized (but insignificant) increase in four-year enrollment.[27]

Given the many outcomes and subgroups that I examine, I present in Table A6 the college enrollment results adjusted for multiple hypothesis testing. I control for the false discovery rate (FDR), or the proportion of rejections that are "false discoveries" (type I errors) by calculating and reporting q-values following Benjamini and Hochberg (1995), Benjamini and Yekutieli (2001), and Anderson (2008). The q-value for each coefficient approximates the p-value after adjusting for the FDR within a family of outcomes, which in my case includes the four enrollment outcomes (i.e., any, four-year, two-year, and selective). The q-values show that after adjusting for

[24] I include males as a "disadvantaged" group in this statement, because their college enrollment rate is lower than for females both in my experimental sample, and more broadly in Michigan and across the U.S. (see Bailey & Dynarski, 2011; Conger, 2015; Conger & Long, 2013).

[25] See Table A7, which shows take-up, enrollment, and persistence effects for four groups: 1) economically advantaged students at economically advantaged schools, 2) economically advantaged students at economically disadvantaged schools, 3) economically disadvantaged students at economically advantaged schools, and 4) economically disadvantaged students at economically disadvantaged schools. The results confirm that effects are driven by economically disadvantaged students regardless of school economic disadvantage. All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's website and use the search engine to locate the article at http://onlinelibrary.wiley.com.

[26] For ease of interpretation, I define two-year enrollment as enrolling in a two-year school and not a four-year school, so that two- and four-year enrollment are mutually exclusive, and so the coefficients and control means in Table 4 sum to those shown in row 1 for any enrollment. There is a statistically insignificant 0.6 percentage point increase among economically disadvantaged students enrolling at a two-year school (including those who also enroll in a four-year school).

[27] I also examine in-state versus out-of-state attendance and find that the results are driven by increases at in-state institutions. I split four-year colleges into public versus private institutions, and find that the effects are somewhat more concentrated among privates than publics.

Table 5. Effects on college enrollment, by college type and student subgroup.

		Sex		Economic disadvantage		Race		ACT score	
Dependent variable	All students (1)	Male (2)	Female (3)	ED (4)	Non-ED (5)	Non-white (6)	White (7)	Low (8)	High (9)
By College Level									
Four-Year	0.003	0.006	−0.000	0.017**	−0.002	0.010	0.002	0.006	−0.000
	(0.004)	(0.006)	(0.005)	(0.008)	(0.005)	(0.010)	(0.004)	(0.006)	(0.005)
	0.675	*0.629*	*0.719*	*0.536*	*0.726*	*0.694*	*0.672*	*0.567*	*0.791*
Two-Year (only)	−0.002	0.001	−0.005	−0.003	−0.002	0.003	−0.003	−0.006	0.001
	(0.003)	(0.005)	(0.004)	(0.007)	(0.004)	(0.007)	(0.004)	(0.005)	(0.004)
	0.168	*0.182*	*0.154*	*0.228*	*0.146*	*0.145*	*0.172*	*0.231*	*0.100*
Enroll in Selective Four-Year	0.003	0.004	0.002	0.005*	0.002	0.008	0.002	0.002	0.004
	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)	(0.006)	(0.002)	(0.002)	(0.004)
	0.081	*0.084*	*0.078*	*0.033*	*0.098*	*0.136*	*0.071*	*0.013*	*0.154*
Observations	49,156	23,799	25,357	13,199	35,957	7,311	41,845	25,481	23,675

Notes: Each point estimate is from a separate regression. Standard errors in parentheses are clustered at the school level. Control means are in italics below the standard errors.
*** = significant at 1 percent level; ** = 5 percent level; * = 10 percent level.

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multiple hypothesis testing, the enrollment effects among economically disadvantaged students are only marginally significant at the 10 percent confidence level, suggesting that these results should be interpreted with caution.

Note that I present only Intent-to-Treat (ITT) estimates, which I believe are more policy-relevant than Treatment-on-the-Treated (TOT) estimates given that no policy can force students to open an envelope, read the enclosed letter, and navigate to the suggested website. Furthermore, it is not clear that estimating TOT effects is appropriate in this context, given that reading the letter even without navigating to the gateway website could influence students' behavior. Nevertheless, when I estimate TOT effects that instrument for take-up with random assignment, I find an identical pattern of results that is approximately ten times larger. For example, the TOT for economically disadvantaged students enrolling in any college is 13.5 percentage points, or 17.7 percent, and for enrolling in a four-year college is 16.1 percentage points, or 30 percent. While these TOT estimates may appear large, other light-touch information interventions have found similarly large TOT estimates. For example, Hoxby and Turner (2013) found TOT estimates of 47.6 percent and 30.8 percent for the number of college applications submitted and the number of colleges to which students were admitted, respectively.

I now proceed to examine whether the impacts on college enrollment exhibited heterogeneity by letter content, timing, or the website to which students were directed. I focus on economically disadvantaged students, given that they are the group for which I observed positive and statistically precise impacts of the intervention.

Table 6 presents these results, where for each column the sample is the control group plus the portion of the treatment group noted in the column header. While the results tend to be underpowered given the focus on economically disadvantaged students and particular subsets of the treatment group, several interesting patterns emerge. First, students who receive the college choice letter, and for whom we saw were subsequently more likely to navigate to the "College Search" portion of the Michigan website, may exhibit a switching effect, with a statistically imprecise decrease in two-year enrollment and large, statistically significant increase in four-year enrollment. Second, the effects appear to be driven by the letters mailed in October and November. There is a smaller and statistically insignificant effect of letters mailed in December, which is well into college-application season, and perhaps too late to impact student decisions. The effects by website are nearly identical revealing either that the website design is unimportant, or that any possible gains due to the better design of The College Board site may be offset by the more comprehensive state-specific information in the Michigan site.

College Persistence

While college entry has been rising in recent decades, college completion has remained flat (Bound, Lovenheim, & Turner, 2010). A key concern with policies that boost college-going is that they may induce marginal students to attend but not persist through college. This concern exists for any policy that increases college enrollment, such as Head Start (Deming, 2009), primary school class size reduction (Dynarski, Hyman, & Schanzenbach, 2013), or increased school funding (Hyman, 2017b; Jackson, Johnson, & Persico, 2016). However, the concern is even greater for light-touch policies, such as the one implemented in this study, because such interventions reduce informational and administrative hurdles to the college application process, but may not provide students with any lasting improvements in their skills or knowledge that can help them persist through college. If students induced into college do not persist to graduation,

Table 6. Effects on college enrollment for economically disadvantaged students, by treatment type.

		Letter content				Letter timing			Website	
Dependent variable	Overall (1)	Affordability (2)	How to apply (3)	College choice (4)	All three (5)	October (6)	November (7)	December (8)	Michigan site (9)	College board site (10)
Enroll in College	0.014**	0.015	0.017	0.017	0.009	0.021**	0.017*	0.006	0.014	0.015*
	(0.007)	(0.011)	(0.011)	(0.011)	(0.011)	(0.010)	(0.010)	(0.010)	(0.009)	(0.009)
	0.764	*0.764*	*0.764*	*0.764*	*0.764*	*0.764*	*0.764*	*0.764*	*0.764*	*0.764*
By College Level										
Four-Year	0.017**	0.015	0.011	0.033***	0.010	0.022*	0.027**	0.003	0.018*	0.016*
	(0.008)	(0.012)	(0.013)	(0.013)	(0.012)	(0.012)	(0.011)	(0.012)	(0.010)	(0.009)
	0.536	*0.536*	*0.536*	*0.536*	*0.536*	*0.536*	*0.536*	*0.536*	*0.536*	*0.536*
Two-Year (only)	−0.003	−0.001	0.006	−0.016	−0.001	−0.002	−0.010	0.003	−0.004	−0.002
	(0.007)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)	(0.010)	(0.010)	(0.008)	(0.008)
	0.228	*0.228*	*0.228*	*0.228*	*0.228*	*0.228*	*0.228*	*0.228*	*0.228*	*0.228*
Enroll in Selective Four-Year	0.005*	0.001	0.001	0.007	0.013**	0.010**	0.008*	−0.001	0.007*	0.004
	(0.003)	(0.004)	(0.005)	(0.005)	(0.005)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
	0.033	*0.033*	*0.033*	*0.033*	*0.033*	*0.033*	*0.033*	*0.033*	*0.033*	*0.033*
Observations	13,199	8,251	8,260	8,254	8,264	8,795	8,805	8,819	9,911	9,898

Notes: The sample includes only economically disadvantaged students. Each point estimate is from a separate regression. Standard errors in parentheses are clustered at the school level. Control means are in italics below the standard errors. Website refers to the college information website to which students are randomly directed. Further details on treatment types available in the text.
*** = significant at 1 percent level; ** = 5 percent level; * = 10 percent level.

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Table 7. Effects on college enrollment and persistence.

		Economic disadvantage	
Dependent variable	All (1)	ED (2)	Non-ED (3)
Enroll in College and Persist to Second Year	−0.003	0.006	−0.007
	(0.004)	(0.008)	(0.004)
	0.742	*0.606*	*0.793*
Enroll in Four-Year College and Persist to Second Year	−0.001	0.002	−0.002
	(0.004)	(0.008)	(0.005)
	0.547	0.388	0.606
Immediately Enroll in College	0.000	0.013*	−0.004
	(0.003)	(0.007)	(0.004)
	0.820	*0.737*	*0.851*
Immediately Enroll in Four-Year College	0.004	0.010	0.002
	(0.004)	(0.008)	(0.005)
	0.613	*0.486*	*0.660*
Immediately Enroll in College And Persist to Third Year	−0.001	0.001	−0.002
	(0.004)	(0.008)	(0.005)
	0.654	*0.502*	*0.710*
Immediately Enroll in Four-Year College And Persist to Third Year	0.004	−0.002	0.006
	(0.004)	(0.007)	(0.005)
	0.496	*0.337*	*0.554*
Observations	49,156	13,199	35,957

Notes: Each point estimate is from a separate regression. Standard errors in parentheses are clustered at the school level. Control means are in italics below the standard errors.
*** = significant at 1 percent level; ** = 5 percent level; * = 10 percent level.

then the effects on enrollment rates of such policies would overstate the programs' benefits.[28]

Given these concerns, I next examine the effects of the intervention on students' likelihood of enrolling in and persisting through college. The most recent college enrollment data available for this study are for fall 2017, which is the third fall after scheduled on-time high school graduation. Thus, for students who enroll during the first two years after scheduled on-time high school graduation (the measure used thus far), we can examine whether these students persist to the second year of college.[29] In Table 7, row 1, I find that the effect among economically disadvantaged students on the probability of enrolling and persisting to the second year of college equals 0.6 percentage points, which is attenuated by about half relative to the 1.4 percentage point enrollment impact, and is no longer statistically significant. Assuming that the intervention does not negatively impact the persistence rate of students who would have enrolled in the absence of the intervention, this result, while statistically imprecise, suggests that students induced into college by the

[28] It is worth noting that at least some amount of college dropout is rational and welfare improving from an economic standpoint given the option value of college attendance: enrolling in college provides students with the option, but not the obligation, to continue after learning whether college is right for them (e.g., Stange, 2012).
[29] Note that throughout this section, I define persistence as persisting either at the same or a different institution.

intervention may persist to their second year at a lower rate than the inframarginal enrollee. The pattern is similar and, if anything, somewhat more severe for enrolling in and persisting at four-year institutions.

I next examine enrollment and persistence to year three. To do so, I must redefine my measure of college enrollment to only capture enrollment within the year directly after scheduled on-time high school graduation, rather than enrollment within two years. Doing so allows me to examine whether students who enroll immediately in 2015/2016, persist through 2016/2017 and into their third year of college in fall 2017. I first present the results on immediate enrollment. The intervention increases immediate enrollment among economically disadvantaged students by 1.3 percentage points, nearly identical to before, though the effect on four-year enrollment is only 1.0 percentage points, which is smaller than before and statistically insignificant.[30] The point estimate among economically disadvantaged students for enrolling in any college and persisting to year three is near zero (0.1 percentage points), though with a 95 percent confidence interval that includes the 1.3 percentage point immediate enrollment effect.

Finally, because treatment could impact the timing of first enrollment, I present in Table A8[31] effects on attendance in year two (2016/2017) unconditional on year one enrollment, and attendance in year three (fall 2017) unconditional on year one or year two enrollment. Enrollment in year two unconditional on enrollment in year one, for example, includes students who enroll during year one and persist to year two, but also captures students who enroll for the first time in year two. Examining these later year enrollments, unconditional on prior enrollment, I find a similar pattern of results as in Table 7, showing effects on enrollment in year two and year three (unconditional) that are smaller in magnitude than the year one enrollment, and are statistically insignificant.

It is important to note that while the point estimates on enrolling and persisting through college for economically disadvantaged students are systematically smaller than those on enrollment, the standard errors preclude firm conclusions. The 95 percent confidence intervals all include the original 1.4 percentage point enrollment effect. While the results are too imprecise to infer that all or nearly all marginal students induced into college ultimately drop out, they are at least suggestive that the persistence rate through college for these marginal students is somewhat lower than the inframarginal college enrollee.

MECHANISMS

The suggestive lower persistence rate of the marginal students induced into college by the intervention raises the question of what leads these students to drop out at a higher rate. In Table 8, I explore a few possible mechanisms for why these students may not persist through college. First, recall that effects on enrollment were driven by increases in four-year enrollment, with possible increases at selective colleges and decreases at two-year schools. One mechanism could be that students are being induced into "reach" institutions where they are among the lowest achievers and possibly less qualified to succeed at such colleges, similar to the concern raised in

[30] This immediate enrollment result no longer captures students enrolling in a two-year school for their first year and then transferring to a four-year school for their second year. Thus, the smaller immediate four-year enrollment effect, but same size immediate any enrollment effect, is consistent with the earlier finding that the intervention caused some students to switch away from enrolling only at a two-year institution.

[31] All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's website and use the search engine to locate the article at http://onlinelibrary.wiley.com.

Table 8. Exploring drop-out mechanisms.

		Economic disadvantage	
Dependent variable	All (1)	ED (2)	Non-ED (3)
Panel A. Student-College Match			
Enroll in Safety College Only ("Undermatch")	−0.001	−0.005	0.001
	(0.004)	(0.008)	(0.005)
	0.392	*0.390*	*0.392*
Ever Enroll in Match College	−0.000	0.018**	−0.007
	(0.004)	(0.008)	(0.005)
	0.333	*0.268*	*0.357*
Ever Enroll in Reach College ("Overmatch")	0.003	0.005	0.002
	(0.003)	(0.005)	(0.003)
	0.133	*0.115*	*0.139*
Panel B. Institution Persistence Rate			
Enroll in Low-Persistence College Only	0.001	0.001	0.002
	(0.004)	(0.008)	(0.004)
	0.289	*0.376*	*0.258*
Ever Enroll in High-Persistence College	−0.001	0.014*	−0.006
	(0.004)	(0.008)	(0.005)
	0.554	*0.388*	*0.615*
Panel C. Enrollment Intensity			
Enroll Part-Time Only	0.000	0.005	−0.001
	(0.003)	(0.007)	(0.004)
	0.173	*0.230*	*0.152*
Ever Enroll Full-Time	0.000	0.009	−0.003
	(0.004)	(0.008)	(0.005)
	0.670	*0.534*	*0.720*
Observations	49,156	13,199	35,957

Notes: Each point estimate is from a separate regression. Standard errors in parentheses are clustered at the school level. Control means are in italics below the standard errors. Safety, match, reach, low-persistence, and high-persistence colleges are explained in text.
*** = significant at 1 percent level; ** = 5 percent level; * = 10 percent level.

response to affirmative-action postsecondary policies (see Arcidiacono & Lovenheim, 2016).

I find little support for this possible mechanism. I categorize students as enrolling in a safety college, or "undermatching" (e.g., Bowen, Chingos, & McPherson, 2011; Dillon & Smith, 2017; Hoxby & Avery, 2013), enrolling in a match college, or enrolling in a reach college (i.e., "overmatching").[32] I find a negative, statistically insignificant effect on only enrolling in a safety college. The positive enrollment effect

[32] I define match, reach, and safety colleges broadly following Dillon and Smith (2017), such that a match college is one where the student's ACT score is within the interquartile range of entering freshman at that institution during 2015/2016, which I acquire from the federal Department of Education's Integrated Postsecondary Education Data System (IPEDS). A reach school is one where the student's score is at or below the 25th percentile. A safety school is one where the student's score is at or above the 75th percentile score, and also includes non-selective four-year and two-year colleges that do not require the ACT or SAT. For each four-year college in IPEDS, I use the interquartile range for the college entrance exam (ACT or SAT) that is reported by more entering freshmen. In cases where this is the SAT, I convert

for economically disadvantaged students is driven by increases at match colleges, with an increase of 0.18 percentage points that is statistically significant at the 95 percent level. There is also a small, statistically insignificant increase (0.5 percentage points) in enrolling at a reach college. While there are arguments for why students could be less likely to persist at either safety or reach colleges, there is little reason to expect lower persistence at match colleges. Thus, the student-college mismatch hypothesis does not appear to explain the lack of persistence through college among students induced to enroll by this intervention.

It is possible that students are induced into match colleges, but that these colleges tend to have relatively low persistence and graduation rates. I examine this possible mechanism by splitting colleges into those with relatively low persistence rates and those with relatively high persistence rates.[33] The effect among economically disadvantaged students for enrolling only at a low-persistence college is near zero and statistically insignificant, while the point estimate for enrolling at a high-persistence college is 1.4 percentage points (marginally significant). To the extent that two-year colleges have lower persistence rates than four-year colleges, and especially selective four-year colleges, this result is consistent with the earlier enrollment results by college type. Thus, enrollment at low-persistence colleges does not appear to explain the lack of persistence for the marginal economically disadvantaged students induced into college by this intervention.

Finally, I examine enrollment intensity as a possible mechanism. Students who enroll primarily part-time are less likely to persist through college and earn a college degree (Shapiro et al., 2017). If the economically disadvantaged students induced by this intervention to enroll in college do so primarily part-time, then this could explain their high dropout rate. While the results are statistically imprecise, I do not find any convincing evidence that this mechanism is at play. The point estimate for enrolling only on a part-time basis is 0.5 percentage points, and is 0.9 for ever enrolling full-time (neither is statistically significant). While the results are imprecise, there is certainly no clear evidence that most students induced into college are doing so primarily on a part-time basis.

In summary, none of the mechanisms that I can test empirically with available data provide any support for the results on college persistence. After ruling out these mechanisms, it seems the most likely explanation is that these economically disadvantaged students are, by definition, marginal in that they would not have enrolled in the absence of this extremely light-touch intervention, but did enroll after being treated with the intervention. These marginal students may be less academically prepared or able to succeed in college than the inframarginal student. As an attempt to examine whether they are less academically prepared, I compare observable baseline achievement of these students, and find that treatment and control students who enrolled in college (either at a two-year or at a four-year college) have nearly identical and statistically indistinguishable ACT scores and 11th-grade GPAs. Thus, changes in student composition, at least along observed achievement measures, do not provide an explanation.

I conclude that the same unobserved characteristics of these high-achieving, economically disadvantaged students that may have been partially responsible for them

scores to the ACT metric using publicly available concordance tables. For students who attend multiple colleges, I treat students as enrolling in a safety college if they only enroll in a safety college, but I treat students enrolling in a match or reach college as doing so if they ever enroll in such a college.

[33] I take all colleges attended in my experimental sample and divide them into low-persistence colleges, where the mean persistence rate is below the median rate in my data, and high-persistence colleges, where the mean persistence rate is above the median. I use persistence to the second year of college, but the pattern of results is identical if I use persistence to year three.

not enrolling in college in the absence of the intervention (for example, not having family or friends who attended and are subsequently familiar with college, having challenging family circumstances that require their time and attention, having to financially support their immediate or extended families, etc.), likely also led to these students dropping out of college. An alternative explanation is simply that the persistence rates of the marginal and inframarginal enrollees were actually similar, which is a real possibility given the statistical imprecision of both the enrollment and persistence results.

CONCLUSION

I conduct a statewide experiment in Michigan with nearly 50,000 high-achieving high school seniors. Treated students are mailed a letter from the Michigan Department of Education encouraging them to consider college and providing them with a web address for a college information website. I find that very high-achieving, economically disadvantaged, and very high-achieving, minority students are the most likely to navigate to the website. Small changes to letter content affect take-up. For example, highlighting college affordability induces 18 percent more students to the website than highlighting college choice, and 37 percent more than highlighting how to apply to college. There were zero impacts of the letter on college enrollment among the entire sample. However, there was a suggestive pattern of small increases among disadvantaged groups, such as economically disadvantaged students and racial minorities. While statistically imprecise, results on persistence through college suggest that the marginal students induced into college by the intervention appeared to persist at a lower rate than the inframarginal college enrollee.

In many ways, the finding that this extremely inexpensive and light-touch policy had zero impact on college enrollment overall is unsurprising and suggests that there is no "free lunch" from light-touch college-going interventions for the average high-achieving student. Nevertheless, the fact that mailing the letter seemed to have a positive impact on the enrollment of economically (and otherwise) disadvantaged students represents an important contribution to the literature examining college-going interventions. Unlike prior work finding positive effects of light-touch interventions, this intervention was extremely inexpensive, operated at scale, and was not targeted only to students who had already taken concrete steps toward applying to college. At a cost of approximately fifty cents per student to print and mail the letters,[34] this intervention is among the cheapest rigorously evaluated college-going interventions of which I am aware.

To examine the relative cost-effectiveness of this intervention at increasing college enrollment, I compare the policy to other light-touch interventions that increase college-going among low-income populations. I create an index of cost-effectiveness by dividing a policy's cost by the proportion of students it induces into college. For example, assuming a $0.50 per student cost and focusing on the 1.4 percentage point enrollment increase among economically disadvantaged students, the amount spent by this intervention to induce a single economically disadvantaged child into college is $36 (= $0.50/0.014).[35] I focus here on the effects for economically disadvantaged students, because that is the sample for most comparable studies. However, the

[34] The cost of the experiment was slightly higher than fifty cents per student due to the staffing costs for setting up the gateway website and tracking students entering that site and their browsing behavior at the publicly available Michigan website. However, a state wishing to implement this intervention could bypass this gateway site, directing students outright to the publicly available website.

[35] One way to think of this calculation is as follows: if 1,000 economically disadvantaged students are treated with the policy at a cost of $0.50 per student, 14 will be induced to attend college

1.3 percentage point impact on enrollment observed both for non-white and for urban students suggests a cost to induce a single non-white or urban student into college of $38 (= $0.50/0.013).

Carrell and Sacerdote (2017) evaluate a mentoring intervention that, if targeted toward women (as there was no impact for men), costs $1,200 per additional enrollee ($300/0.25). The H&R Block FAFSA assistance program (Bettinger et al., 2012) costs $1,100 per student induced into college (= $88/0.08). The virtual college assistant evaluated by Page and Gelbach (2017) costs $333 per college enrollee ($11/0.033). Hoxby and Turner (2013), though focusing on college match and not the extensive margin of enrollment, spent $6 per student and improved the college match rate by 5 percentage points, for a cost of $120 ($6/0.05) per student induced into a better-fit college. Castleman and Page (2015) evaluate a text messaging campaign that costs $100 per student induced into college ($7/0.07). To my knowledge, the most cost-effective of any rigorously evaluated light-touch intervention is the text messaging campaign evaluated by Bird et al. (2017), with a cost per low-income student induced into college of $45 ($0.50/0.011).

While keeping in mind the zero impact for the average student, and the relatively low statistical precision of the heterogeneity analysis, at $36 per low-income student induced into college (or $38 per minority or urban student), the suggestive enrollment effects for disadvantaged groups would be among the most cost-effective of any previously evaluated light-touch college-going intervention. However, the results also suggest that the marginal low-income students induced into college may have dropped out at a higher rate than the inframarginal student, raising questions about the welfare implications for these students.

To explore possible welfare implications, I conduct a simple cost-benefit calculation. The likely benefit to these students is the earnings increase they experience from having additional years of schooling. Recent empirical work shows a causal increase in earnings of between 9 and 14 percent from one additional year at a four-year college (see Oreopoulos & Petronijevic, 2013). The likely cost to these students is the cost of attendance, including any debt incurred, as well as the opportunity cost of employment. The average annual tuition for in-state students at Michigan four-year colleges is approximately $19,000 (NCES, 2018), and average annual wage and salary income for 18- to 22-year-olds in Michigan with a high school degree and not currently in school is approximately $12,000 (Ruggles et al., 2018).

These estimates suggest that each year of college costs $31,000 (= $19,000 + $12,000), and increases annual earnings by a minimum of $1,080 (= 0.09 × $12,000), which, over a 45-year career, would increase lifetime earnings by roughly $48,600 (= $1,080 × 45).[36] While this back-of-the-envelope calculation relies on strong assumptions, and a complete cost-benefit analysis is beyond the scope of this paper, this calculation suggests that the intervention may have somewhat increased the welfare of the low-income students it induced into college.

The finding, albeit suggestive, that this low-cost intervention increased enrollment for certain student subgroups represents an important contribution to the literature that can help guide future policy surrounding the design and implementation of college-going nudges. Unlike past studies, the students did not select into the sample by taking a college entrance exam, signing up with The Common Application, or

(= 1,000 × 0.014) at a total cost of $500 (= $0.50 × 1,000). Thus, the cost per student induced into college is $36 (= $500/14).

[36] This calculation assumes that the real discount rate equals the average year-to-year increase in salary. For example, a discount rate of 3 percent would substantially decrease the present value of the lifetime increase in earnings. But an average year-to-year increase in salary of 3 percent would raise the $1,080 earnings premium by 3 percent each year, canceling out the decrease due to the 3 percent discount rate.

otherwise intending to enroll in a four-year college. Furthermore, the fact that the marginal students induced into college by the intervention may have been more likely to drop out highlights both the importance of programs that support marginal enrollees through college (e.g., Bettinger & Baker, 2014; Castleman & Page, 2016; Oreopoulos & Petronijevic, 2018), and, for researchers, the necessity of examining persistence when studying college-going interventions.

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APPENDIX

STATE OF MICHIGAN
DEPARTMENT OF EDUCATION
LANSING

RICK SNYDER
GOVERNOR

MICHAEL P. FLANAGAN
STATE SUPERINTENDENT

[Student first and last name]
[Student address]
[Student city, state zip]

Dear [Student first name],

Congratulations on your score of [ACT Score] on the ACT, which you took as part of the Michigan Merit Exam in March. Your score suggests that you are ready to enroll and succeed in college. You are receiving this message as a free service from the Michigan Department of Education to ensure that students who are qualified to succeed in college have the information necessary to successfully navigate the application process.

The following link contains information and resources to help you ***learn how to apply to college***, and more. After navigating to the link, you will need to enter your personal password, provided below.

College Information Website: **micollegeinfo.org**
Your Personal Password: [password]

By entering your password, you will automatically be entered into a drawing to ***receive a free iPad Mini!****. You can also scan below to navigate to the above link.

If you have any questions about this letter, please call (517)-258-0294 or email hymanj@michigan.gov.

Congratulations again on your ACT score and readiness to succeed in college. I am excited for the benefits that your college attendance and future successes can bring to you and to the State of Michigan. Good luck!

Sincerely,

Venessa A. Keesler, Ph.D.
Deputy Superintendent, Education Services
Michigan Department of Education

*NO PURCHASE NECESSARY. Complete rules at micollegeinfo.org/rules.pdf.

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Figure A1. Letter Version Two (How to Apply).

STATE OF MICHIGAN
DEPARTMENT OF EDUCATION
LANSING

RICK SNYDER
GOVERNOR

MICHAEL P. FLANAGAN
STATE SUPERINTENDENT

[Student first and last name]
[Student address]
[Student city, state zip]

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By entering your password, you will automatically be entered into a drawing to ***receive a free iPad Mini!**** You can also scan below to navigate to the above link.

If you have any questions about this letter, please call (517)-258-0294 or email hymanj@michigan.gov.

Congratulations again on your ACT score and readiness to succeed in college. I am excited for the benefits that your college attendance and future successes can bring to you and to the State of Michigan. Good luck!

Sincerely,

Venessa A. Keesler, Ph.D.
Deputy Superintendent, Education Services
Michigan Department of Education

*NO PURCHASE NECESSARY. Complete rules at micollegeinfo.org/rules.pdf.

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Figure A2. Letter Version Three (College Choice).

STATE OF MICHIGAN
DEPARTMENT OF EDUCATION
LANSING

RICK SNYDER
GOVERNOR

MICHAEL P. FLANAGAN
STATE SUPERINTENDENT

[Student first and last name]
[Student address]
[Student city, state zip]

Dear [Student first name],

Congratulations on your score of [ACT Score] on the ACT, which you took as part of the Michigan Merit Exam in March. Your score suggests that you are ready to enroll and succeed in college. You are receiving this message as a free service from the Michigan Department of Education to ensure that students who are qualified to succeed in college have the information necessary to successfully navigate the application process.

The following link contains information and resources to help you learn: a) ***how to apply to college***, b) ***how to make college affordable***, and c) ***which college is right for you***. After navigating to the link, you will need to enter your personal password, provided below.

College Information Website: **micollegeinfo.org**
Your Personal Password: [password]

By entering your password, you will automatically be entered into a drawing to ***receive a free iPad Mini!****. You can also scan below to navigate to the above link.

If you have any questions about this letter, please call (517)-258-0294 or email hymanj@michigan.gov.

Congratulations again on your ACT score and readiness to succeed in college. I am excited for the benefits that your college attendance and future successes can bring to you and to the State of Michigan. Good luck!

Sincerely,

Venessa A. Keesler, Ph.D.
Deputy Superintendent, Education Services
Michigan Department of Education

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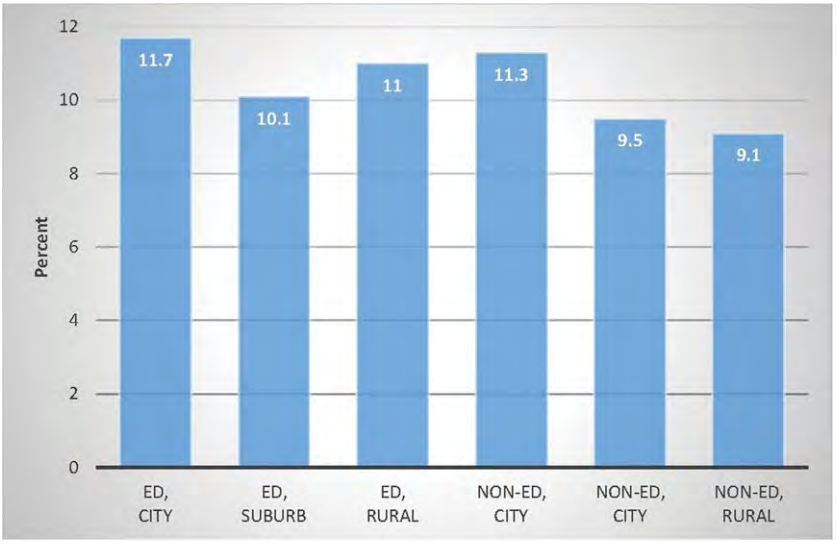
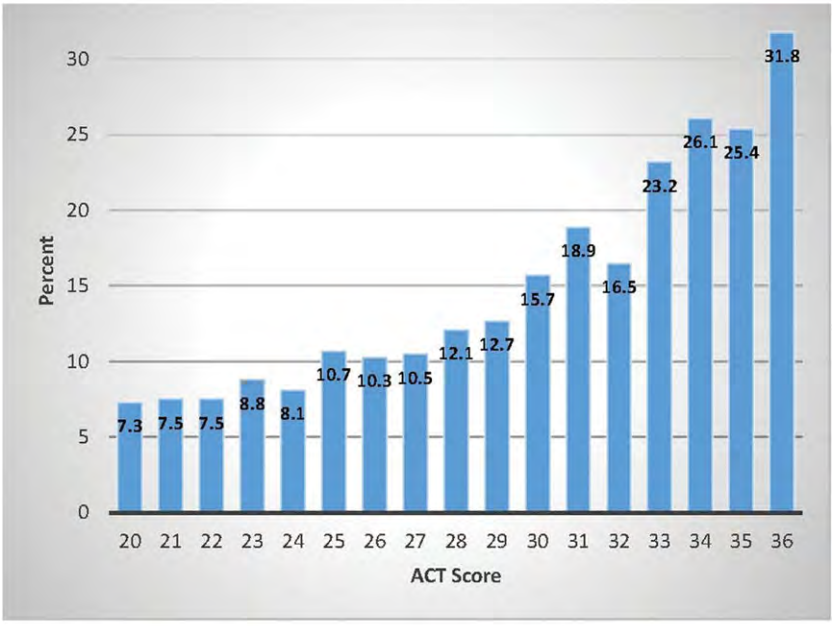
Figure A3. Letter Version Four (Includes All Three Highlighted Phrases).



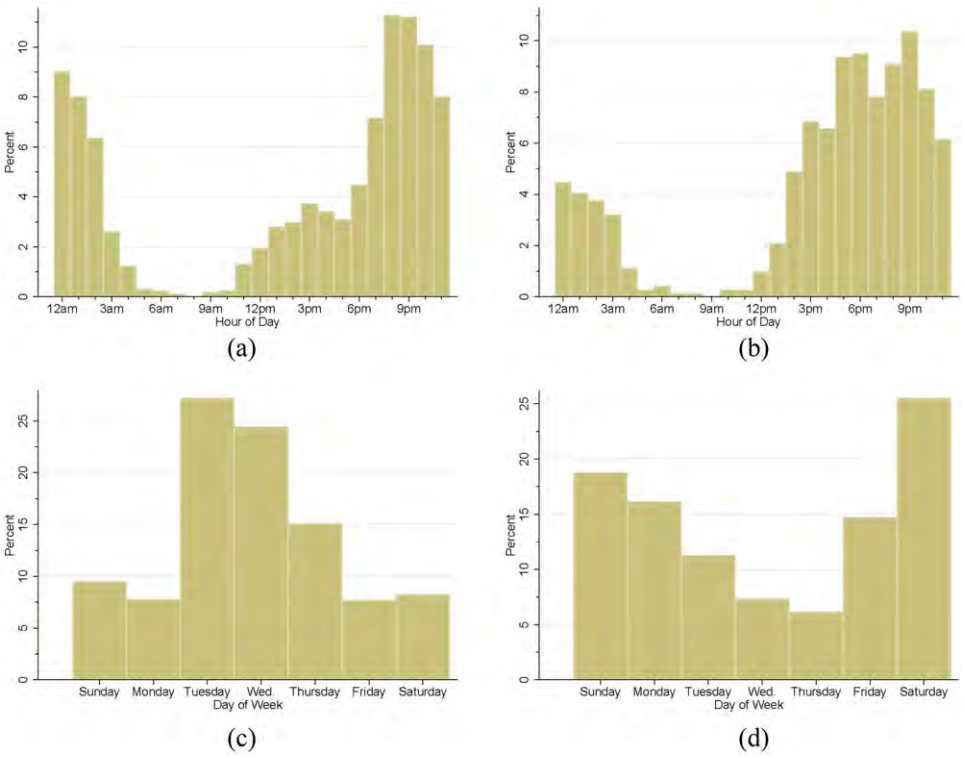
(a)



(b)

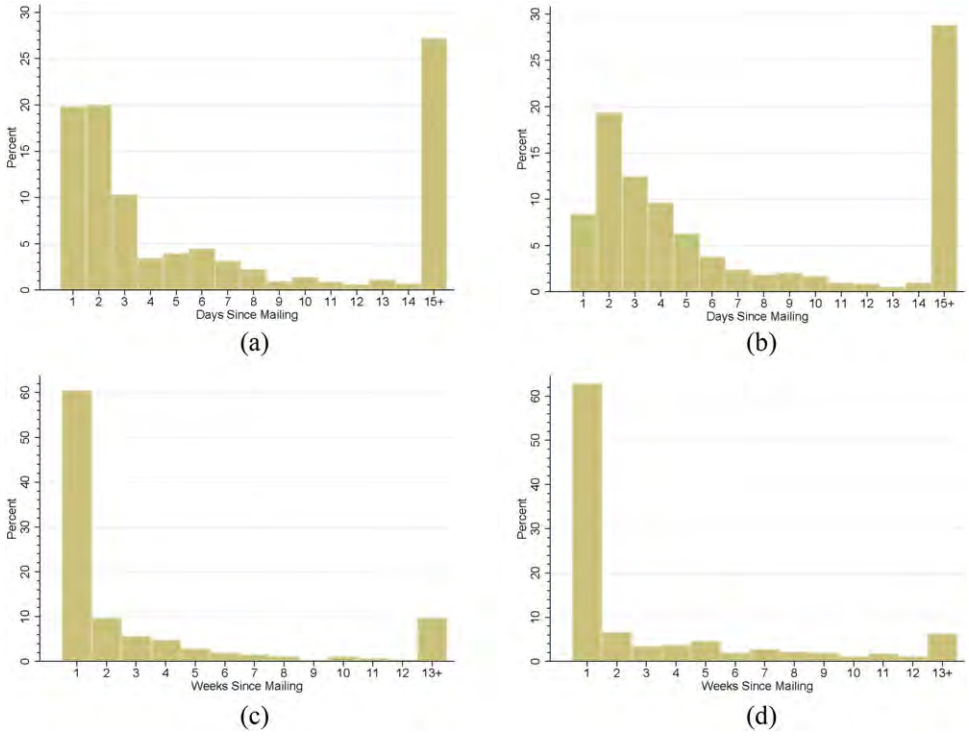
Notes: Figure A4(a) shows letter take-up rates by student economic disadvantage and high school urbanicity. Figure A4(b) shows rates by student ACT score. Take-up is defined as a student entering his or her password into the gateway website, micollegeinfo.org.

Figure A4. Take-Up by Student ACT Score, Economic Disadvantage, and Urbanicity.



Notes: Figures include all students who entered their password into the gateway website, micollegeinfo.org. Subfigure A5(a) shows the time-of-day students first entered their password when they first entered it on a weekday, and subfigure A5(b) when they first entered it on a Saturday or Sunday. Subfigure A5(c) shows the day-of-week students first entered their password, for those who were mailed the letter on a Monday, and subfigure A5(d) for those mailed a letter on Thursday.

Figure A5. Time-of-Day and Day-of-Week That Students Entered Password.



Notes: Figures include all students who entered their password into the gateway website, micollege-info.org. Subfigure A6(a) shows the number of days between the mailing and when students first entered their password, for those mailed a letter on Monday, and subfigure A6(b) for those mailed a letter on Thursday. Subfigure A6(c) shows the number of weeks between the mailing and when students first entered their password, for those mailed a letter during October, and subfigure A6(b) for those mailed a letter during December.

Figure A6. How Long After Letter Receipt Did Students Enter Password?

Table A1. Sample means (balance table), by treatment arm.

		RCT sample: ACT score > = median											
				Treatment group									
					By bolded phrase				By letter timing				
	Entire cohort (1)	Entire RCT sample (2)	Control group (3)	All letters (4)	Afford (5)	Apply (6)	Choice (7)	All 3 (8)	Oct. (9)	Nov. (10)	Dec. (11)	Monday (12)	Thursday (13)
Student Demographics													
Female	0.500	0.516	0.516	0.516	0.516	0.516	0.515	0.516	0.507	0.515	0.525	0.516	0.516
White	0.743	0.851	0.850	0.852	0.855	0.848	0.854	0.852	0.853	0.855	0.849	0.852	0.852
Black	0.168	0.064	0.064	0.064	0.063	0.065	0.063	0.065	0.064	0.062	0.066	0.064	0.064
Hispanic	0.048	0.030	0.031	0.030	0.027	0.031	0.029	0.033	0.028	0.031	0.031	0.030	0.030
Asian	0.030	0.045	0.045	0.045	0.046	0.045	0.046	0.044	0.047	0.044	0.045	0.045	0.046
ED	0.434	0.269	0.269	0.268	0.268	0.267	0.268	0.269	0.267	0.268	0.270	0.268	0.268
Special Education	0.090	0.020	0.019	0.021	0.022	0.023	0.020	0.019	0.019	0.020	0.023**	0.022	0.020
School Characteristics													
City	0.194	0.097	0.098	0.097	0.095	0.098	0.096	0.098	0.096	0.099	0.096	0.097	0.096
Suburb	0.470	0.538	0.537	0.538	0.540	0.537	0.539	0.538	0.539	0.537	0.539	0.539	0.538
Town/Rural	0.336	0.365	0.365	0.365	0.365	0.365	0.365	0.365	0.365	0.365	0.365	0.364	0.366
High ED	0.630	0.502	0.502	0.502	0.493	0.500	0.507	0.507	0.505	0.498	0.503	0.503	0.501
Num. 11th Graders	265.7	294.7	294.4	295.0	296.5	294.0	295.7	293.9	292.9	295.3	296.8	295.6	294.5
Charter	0.047	0.028	0.029	0.027	0.031	0.027	0.025*	0.026	0.031	0.026	0.025**	0.027	0.027
Title I	0.148	0.076	0.076	0.076	0.078	0.074	0.074	0.078	0.08	0.076	0.071	0.076	0.076
Student Achievement													
Grade 11 GPA	2.68	3.20	3.20	3.21	3.20	3.21	3.21	3.20	3.21	3.21	3.20	3.20	3.21
State Math Score	0.026	0.647	0.649	0.646	0.647	0.651	0.637	0.65	0.653	0.648	0.638*	0.644	0.648
State Reading Score	0.019	0.692	0.691	0.693	0.687	0.696	0.694	0.694	0.692	0.696	0.69	0.688	0.697
ACT Composite	19.8	24.1	24.1	24.1	24.1	24.1	24.1	24.1	24.1	24.2	24.1	24.1	24.1
ACT Math	19.7	23.5	23.5	23.5	23.5	23.5	23.4	23.5	23.5	23.5	23.4	23.4	23.5
ACT Science	20.2	24.0	24.0	24.0	24.1	24.1	24.0	24.0	24.1	24.1	24.0	24.0	24.1
ACT English	19.0	24.0	24.0	24.0	24.0	23.9	24.0	24.0	24.0	24.1	23.9	24.0	24.0
ACT Reading	19.9	24.4	24.4	24.5	24.4	24.5	24.5	24.5	24.4	24.5	24.4	24.4	24.5
Observations	101,845	49,156	24,578	24,578	6,118	6,172	6,136	6,152	8,190	8,192	8,196	12,288	12,290

Notes: Table shows sample means by treatment status. For every variable, separate statistical tests of equality are conducted for each treatment group (columns 4 through 13) relative to the control group (column 3). Different treatment groups (i.e., bolded phrase and letter timing) explained in text. ED = Economically Disadvantaged. High (low) school ED is above (below) the median fraction eligible for free or reduced-price lunch in the RCT sample.
*** = significant at 1 percent level; ** = 5 percent level; * = 10 percent level.

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Table A2. College information website browsing behavior, by student economic disadvantage.

Panel A. What Types of Webpages Did Students Navigate To?

Percent navigating to:	Not economically disadvantaged (1)	Economically disadvantaged (2)	P-value of test of equality (3)
Paying for College	37.3	32.7	0.130
Student Steps	12.3	15.6	0.121
College Search	13.6	13.1	0.800

Panel B. What External Links Did Students Navigate To?

	Not economically disadvantaged		Economically disadvantaged	
Top websites students leave to:	Percent	Cum. percent	Percent	Cum. percent
Michigan College Access Portal	26.7	26.7	20.8	20.8
MDE Michigan Scholarship Website	14.7	41.4	16.7	37.5
Search for Scholarships	12.0	53.4	12.5	50.0
See if You're Eligible for a Pell Grant	8.0	61.4	4.2	54.2
See if You're Eligible for an Academic Competitiveness Grant	9.3	70.7	0.0	54.2
MDE Grants Available List	1.3	72.0	8.3	62.5
Other	28.0	100.0	37.5	100.0

Notes: The sample is the 1,167 students who entered their password on the gateway website and were directed to the *KnowHow2GOMichigan.org* college information website. Panel A shows the percent of students who navigated to different pages on that website by the letter version they were sent. Panel B shows the top five external website links to which students navigated.

Table A3. Enrollment and persistence effects excluding controls.

		Economic disadvantage	
Dependent Variable	All (1)	ED (2)	Non-ED (3)
Enroll in College	0.001	0.015**	−0.004
	(0.003)	(0.007)	(0.004)
Enroll in Four-Year College	0.004	0.018**	−0.001
	(0.004)	(0.008)	(0.005)
Enroll in Two-Year College (only)	−0.003	−0.003	−0.003
	(0.003)	(0.007)	(0.004)
Enroll in Selective Four-Year College	0.003	0.006*	0.003
	(0.003)	(0.003)	(0.003)
Enroll in College and Persist to Second Year	−0.002	0.007	−0.006
	(0.004)	(0.008)	(0.004)
Enroll in Four-Year College and Persist to Second Year	0.001	0.003	0.000
	(0.005)	(0.008)	(0.005)
Immediately Enroll in College	0.001	0.014*	−0.003
	(0.004)	(0.008)	(0.004)
Immediately Enroll in Four-Year College	0.006	0.011	0.003
	(0.005)	(0.009)	(0.005)
Immediately Enroll in College And Persist to Third Year	0.000	0.002	−0.001
	(0.004)	(0.008)	(0.005)
Immediately Enroll in Four-Year College And Persist to Third Year	0.006	−0.001	0.008
	(0.005)	(0.008)	(0.006)
Observations	49,156	13,199	35,957

Notes: Table shows main results from the paper excluding the vector of controls from the estimating equation. Each point estimate is from a separate regression. Standard errors in parentheses are clustered at the school level.
*** = significant at 1 percent level; ** = 5 percent level; * = 10 percent level.

Table A4. Enrollment and persistence effects estimating using logit.

		Economic disadvantage	
Dependent variable	All (1)	ED (2)	Non-ED (3)
Enroll in College	0.000	0.014**	−0.005
	(0.003)	(0.007)	(0.003)
Enroll in Four-Year College	0.003	0.017**	−0.002
	(0.004)	(0.008)	(0.004)
Enroll in Two-Year College (only)	−0.003	−0.003	−0.002
	(0.003)	(0.007)	(0.004)
Enroll in Selective Four-Year College	0.002	0.006**	0.001
	(0.002)	(0.003)	(0.003)
Enroll in College and Persist to Second Year	−0.003	0.006	−0.007
	(0.004)	(0.008)	(0.004)
Enroll in Four-Year College and Persist to Second Year	−0.000	0.002	−0.001
	(0.004)	(0.008)	(0.005)
Immediately Enroll in College	0.000	0.013*	−0.004
	(0.003)	(0.007)	(0.004)
Immediately Enroll in Four-Year College	0.004	0.010	0.002
	(0.004)	(0.008)	(0.005)
Immediately Enroll in College And Persist to Third Year	−0.001	0.001	−0.002
	(0.004)	(0.008)	(0.005)
Immediately Enroll in Four-Year College And Persist to Third Year	0.005	−0.002	0.007
	(0.004)	(0.007)	(0.005)
Observations	49,156	13,199	35,957

Notes: Table shows main results from the paper estimating using logit and presenting marginal effects. Each point estimate is from a separate regression. Standard errors in parentheses are clustered at the school level.
*** = significant at 1 percent level; ** = 5 percent level; * = 10 percent level.

Table A5. Effects for economically disadvantage (ED) students by the number of treated ED students in the school.

		# of treated ED students	
Dependent variable	All schools (1)	Low (2)	High (3)
Take-Up Rate	0.107	0.111	0.103
Enroll in College			
Any College	0.014**	0.017*	0.012
	(0.007)	(0.010)	(0.010)
	0.764	*0.758*	*0.770*
Four-Year College	0.017**	0.022**	0.011
	(0.008)	(0.011)	(0.011)
	0.536	*0.518*	*0.556*
Two-Year (Only)	−0.003	−0.005	0.001
	(0.007)	(0.010)	(0.009)
	0.228	*0.240*	*0.214*
Enroll in College and Persist			
To Second Year	0.006	0.006	0.004
	(0.008)	(0.011)	(0.010)
	0.606	0.577	0.637
To Third Year	0.001	−0.003	0.002
	(0.008)	(0.012)	(0.010)
	0.502	*0.476*	*0.530*
Observations	13,199	6,630	6,569

Notes: The sample includes only economically disadvantaged (ED) students. Columns 2 and 3 split schools into those below and above the median number of treated ED students at the school, which is 14. Each point estimate is from a separate regression. Standard errors in parentheses are clustered at the school level. Control means are in italics below the standard errors.
*** = significant at 1 percent level; ** = 5 percent level; * = 10 percent level.

Table A6. Effects on college enrollment adjusting for multiple hypothesis testing.

		Sex		Economic disadvantage		Race		ACT score		School urbanicity			School economic disadvantage	
Dependent variable	All students (1)	Male (2)	Female (3)	ED (4)	Non-ED (5)	Non-white (6)	White (7)	Low ACT (8)	High ACT (9)	City (10)	Suburb (11)	Town/ rural (12)	High (13)	Low (14)
Enroll in College	0.001	0.007	−0.005	0.014*	−0.005	0.013	−0.002	0.000	0.001	0.013	−0.001	−0.001	0.001	−0.000
	(0.003)	(0.005)	(0.004)	(0.007)	(0.003)	(0.009)	(0.003)	(0.005)	(0.004)	(0.011)	(0.004)	(0.005)	(0.005)	(0.004)
	0.870	*0.366*	*0.440*	*0.088*	*0.607*	*0.378*	*0.708*	*0.957*	*0.972*	*0.445*	*0.944*	*0.844*	*0.819*	*0.990*
By College Level														
Four-Year	0.003	0.006	−0.000	0.017*	−0.002	0.010	0.002	0.006	−0.000	−0.002	−0.000	0.008	0.006	−0.000
	(0.004)	(0.006)	(0.005)	(0.008)	(0.005)	(0.010)	(0.004)	(0.006)	(0.005)	(0.012)	(0.005)	(0.007)	(0.006)	(0.005)
	0.622	*0.366*	*0.954*	*0.088*	*0.607*	*0.451*	*0.708*	*0.377*	*0.972*	*0.878*	*0.944*	*0.313*	*0.486*	*0.990*
Two-Year (only)	−0.002	0.001	−0.005	−0.003	−0.002	0.003	−0.003	−0.006	0.001	0.015	−0.000	−0.009*	−0.005	−0.000
	(0.003)	(0.005)	(0.004)	(0.007)	(0.004)	(0.007)	(0.004)	(0.005)	(0.004)	(0.010)	(0.004)	(0.006)	(0.005)	(0.004)
	0.622	*0.909*	*0.440*	*0.685*	*0.607*	*0.700*	*0.708*	*0.377*	*0.972*	*0.445*	*0.944*	*0.313*	*0.486*	*0.990*
Enroll in Selective Four-Year	0.003	0.004	0.002	0.005*	0.002	0.008	0.002	0.002	0.004	0.008	0.001	0.004	0.006*	−0.001
	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)	(0.006)	(0.002)	(0.002)	(0.004)	(0.009)	(0.003)	(0.003)	(0.003)	(0.003)
	0.622	*0.366*	*0.645*	*0.093*	*0.607*	*0.378*	*0.708*	*0.377*	*0.972*	*0.504*	*0.944*	*0.313*	*0.059*	*0.990*
Observations	49,156	23,799	25,357	13,199	35,957	7,311	41,845	25,481	23,675	4,777	26,433	17,946	24,678	24,478

Notes: Each point estimate is from a separate regression. Standard errors in parentheses are clustered at the school level. In italics below the standard errors are q-values based on the distribution of p-values within each column. Asterisks reflect statistical significance calculated using q-values. High (low) school economic disadvantage is above (below) the median fraction eligible for free or reduced-price lunch.

*** = significant at 1 percent level; ** = 5 percent level; * = 10 percent level.

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Table A7. Effects by student and school economic disadvantage (ED).

		Non-ED student		ED student	
Dependent variable	All students and schools (1)	Non-ED school (2)	ED school (3)	Non-ED school (4)	ED school (5)
Take-Up Rate	0.098	0.095	0.094	0.109	0.106
Enroll in College					
Any College	0.001	−0.005	−0.004	0.028*	0.009
	(0.003)	(0.004)	(0.006)	(0.014)	(0.008)
	0.843	*0.886*	*0.853*	*0.779*	*0.759*
Four-Year College	0.003	−0.003	−0.002	0.015	0.017*
	(0.004)	(0.006)	(0.008)	(0.016)	(0.009)
	0.675	*0.765*	*0.672*	*0.574*	*0.523*
Two-Year (Only)	−0.002	−0.002	−0.002	0.013	−0.008
	(0.003)	(0.004)	(0.006)	(0.014)	(0.008)
	0.168	*0.121*	*0.181*	*0.205*	*0.236*
Enroll in College and Persist					
To Second Year	−0.003	−0.008	−0.005	0.014	0.002
	(0.004)	(0.005)	(0.007)	(0.015)	(0.009)
	0.742	0.822	0.751	0.662	0.586
To Third Year	−0.001	−0.001	−0.004	0.004	−0.002
	(0.004)	(0.006)	(0.008)	(0.016)	(0.009)
	0.654	*0.746*	*0.659*	*0.569*	*0.478*
Observations	49,156	21,081	14,876	3,397	9,802

Notes: Each point estimate is from a separate regression. Standard errors in parentheses are clustered at the school level. Control means are in italics below the standard errors.
*** = significant at 1 percent level; ** = 5 percent level; * = 10 percent level.

Table A8. Year 2 and Year 3 enrollment effects (unconditional).

		Economic disadvantage	
Dependent variable	All (1)	ED (2)	Non-ED (3)
Enrolled in Year 1	0.000	0.013*	−0.004
	(0.003)	(0.007)	(0.004)
	0.820	*0.737*	*0.851*
Enrolled in Year 1 in Four-Year College	0.004	0.010	0.002
	(0.004)	(0.008)	(0.005)
	0.613	*0.486*	*0.660*
Enrolled in Year 2	−0.003	0.004	−0.006
	(0.004)	(0.008)	(0.004)
	0.749	*0.617*	*0.798*
Enrolled in Year 2 in Four-Year College	0.000	0.003	−0.001
	(0.004)	(0.008)	(0.005)
	0.571	0.411	0.630
Enrolled in Year 3	−0.001	0.003	−0.002
	(0.004)	(0.008)	(0.005)
	0.669	*0.517*	*0.724*
Enrolled in Year 3 in Four-Year College	0.003	0.006	0.002
	(0.004)	(0.007)	(0.005)
	0.553	*0.379*	*0.617*
Observations	49,156	13,199	35,957

Notes: Each point estimate is from a separate regression. Standard errors in parentheses are clustered at the school level. Control means are in italics below the standard errors.
*** = significant at 1 percent level; ** = 5 percent level; * = 10 percent level.