Is Information Enough? The Effect of Information about Education Tax Benefits on Student Outcomes

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Abstract

There is increasing evidence that tax benefits for college do not affect college enrollment. This may be because prospective students do not know about tax benefits for college or because the design of tax benefits is not conducive to affecting educational outcomes. We focus on changing awareness of tax benefits by providing information to students or prospective students. We sent e-mails and letters to students that described tax benefits for college and tracked college outcomes. For all three of our samples—rising high school seniors, already enrolled students, and students who had previously applied to college but were not currently enrolled—information about tax benefits for college did not affect enrollment or reenrollment. We test whether effects vary according to information frames and found that no treatment arms changed student outcomes. We conclude that awareness is not the primary reason that tax benefits for college do not affect enrollment.

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1. INTRODUCTION

The United States Federal Government has given money to students in the form of financial aid for higher education since the 1970s. Recently, the tax code provides additional subsidies for college-going students. Tax benefits for college rose to prominence with the Hope and Lifetime Learning Credit in 1997, can be meaningfully large for students, up to $2,500, and cost an estimated $34.6 billion in 2018 (Crandall-Hollick 2018).[1] An explicit goal of tax benefits for college was spurring additional enrollment.[2]

As tax benefits for college have grown, several studies have considered their effect on student outcomes. These studies have generally found that they did not affect college enrollment (Long, 2004; LaLumia, 2012; Bulman & Hoxby, 2015; Hoxby & Bulman, 2016). A notable exception is Turner (2011a), which found an increase in enrollment with tax aid generosity using changes in benefit generosity over time.[3] Bulman and Hoxby (2015) and Hoxby and Bulman (2016) offer the most convincing evidence on the effect of tax credits and the tuition deduction respectively. These two studies used administrative data from the IRS along with regression kink (Bulman & Hoxby, 2015) and discontinuity (Hoxby & Bulman, 2016) methods to examine the effect of tax benefits for college on student outcomes. In both cases, tax benefits were found to not affect any measured educational outcomes including enrollment or type of institution attended.[4]

The null effects of tax benefits for college stand in contrast to the large literature on financial aid. Many studies have documented that financial aid and tuition affect a variety of student outcomes.[5] Both financial aid and tax benefits for college affect the price of college, but both do

[1] The American Opportunity Tax Credit can more than cover a semester of in-state tuition for two-year schools and cover roughly one-third of a semester of four-year tuition in Texas.

[2] President Bill Clinton said of his proposed changes in education spending, which included tax credits, "My number one priority for the next four years is to ensure that all Americans have the best education in the world… every 18-year-old must be able to go to college; and every adult must be able to keep on learning for a lifetime." State of the Union Address, 1997.

[3] Turner (2011a) uses variation in the generosity of tax benefits over time which may be correlated with other factors related to college enrollment. Other studies have found that taxpayers did not maximize their credits (Turner, 2011b), and that tax credits can be captured by schools Turner (2012). In Turner (2011*b*), the finding is that on average aid is captured by schools. However, for individual students the information sent in this study is still relevant because they will receive less tax aid if they do not claim the benefits. LaLumia (2012) focuses on older students using a person-fixed effect approach and finds no effects for the sample as a whole but positive effects for some subsamples.

[4] Manoli and Turner (2018) find that larger amounts of the EITC in a student's senior year of high school increases college enrollment. However, this additional money is not directly tied to enrollment and represents the effect of additional income on college enrollment.

[5] See Deming and Dynarski (2009) for a summary of these studies. Several studies have found financial aid and tuition to

not affect student enrollment.

With a growing consensus emerging that tax benefits for college do not affect student educational outcomes, several hypotheses may explain these null effects. First, potential students may not be not aware of tax benefits. Second, tax benefits may be poorly targeted (Hoxby & Bulman, 2016). Third, the timing of tax benefits may not be not conducive to affecting student outcomes.

This paper addresses the first issue of a lack of awareness of tax credits by providing information about tax credits for college in a large-scale randomized controlled trial. We reached out to students who had used the official and universal portal for college application to public universities in Texas, ApplyTexas. We sent information, addressed from ApplyTexas, to students and potential students about tax benefits for college including the size of potential benefits, how to claim, and links for additional information.[6] We contacted students via mailed letters and e-mails and varied the type of information presented.

We test whether the information received affected student enrollment using administrative records from all public colleges and universities in Texas supplemented with enrollments outside of Texas using data from the National Student Clearinghouse (NSC). We find consistent evidence that additional information about tax benefits for college did not affect student behavior.

Furthermore, we narrow the focus of our analysis to students most likely to be affected by information about tax benefits. We examine low-income students whose enrollment may be more sensitive to aid. We also consider financially independent students whose personal finances are more directly affected by tax aid. Additionally, we consider groups of students who are historically underrepresented in higher education. Consistently, we find no effect of information about tax credits on educational outcomes—even for groups of students who are more likely to benefit from additional tax aid.

We targeted three samples of students who may be responsive to information about tax benefits for college. First, we targeted rising high school seniors who had applied to college, whom we will call the Enroll group. Many students who apply to college, including accepted

affect enrollment (Dynarksi [2003]; Cornwell, Mustard, Sridhar [2006]; Denning [2017]), graduation (Denning [Forthcoming]; Denning, Marx, Turner [Forthcoming]; Bettinger et al [Forthcoming]; Castleman Long [2016]), and major (Denning and Turley [2017]; Castleman Long Mabel [2018]).

[6] For an example of the type of information see Figure 1.

students, do not enroll, and previous interventions have been shown to mitigate this phenomenon, known as "summer melt" (Castleman, Page, & Schooley, 2014; Castleman & Page, 2015b). Second, we focused on students who were enrolled in college so we could potentially affect persistence. We will call these students the ReEnroll group. Information interventions have been shown to be effective at increasing persistence for enrolled students, which motivates our targeting of already enrolled students (Castleman & Page, 2016). Many students who start college do not finish; the six-year graduation rate is below 60 percent at four-year institutions, and financial aid has been shown to increase graduation for enrolled students (National Center for Education Statistics, 2015; Barr, 2016; Goldrick-Rab et al., 2016). Third, we targeted students who had previously applied to Texas universities or colleges but who did not enroll in Texas. We will call these people the ReApply group. "Non-traditional" students, such as students who do not start college directly after high school, students who are older, or students who have some work experience, are an increasing share of higher education. This intervention was designed to target nontraditional potential students whose enrollment has been shown to be affected by financial aid (Seftor & Turner, 2002) and information about financial aid (Barr & Turner, 2015).[7]

Our outreach included several arms of treatment that varied information. First, we varied whether the outreach discussed costs of college, benefits of college, or neither. Second, we varied the number of tax benefits that were mentioned to see if a higher perceived benefit had a different effect. Third, some people received more detailed information about the tax benefits and how to claim them while others only received the names and the maximum amounts of potential tax benefits. This treatment variation was designed to see if simpler information had a different effect than more complex and complete information. Last, some people received additional information about the Free Application for Federal Student Aid (FAFSA). Our various treatment arms were designed to see if certain types of information were more likely to affect student behavior.[8]

This outreach was inspired by several information interventions that were found to change

[7] Barr and Turner (2015) demonstrated that letters informing displaced workers about the Pell Grant increased postsecondary enrollment.

[8] These treatment variations were motivated by previous work that has found that information framing matters (Bhargava & Manoli, 2015).

behavior.[9] These interventions are appealing to policymakers due to their low cost and ease of implementation. They also appeal to two broad groups of models. In neoclassical economic models of behavior, additional information allows rational agents to optimize more effectively and, in turn, improve outcomes. The provision of information can also leverage insights from behavioral-economic models by "nudging" people to overcome behavioral biases like inattention or procrastination (Chetty, 2015).

Several studies have found that low-cost information interventions can change outcomes in higher education. These interventions include filling out a FAFSA for students, which increases college enrollment (Bettinger et al., 2012); text messages, reminders, and counselors, which affect enrollment, persistence, grades, and borrowing (Castleman & Page, 2015a, 2015b; Castleman, Owen, & Page, 2015; Barr, Bird, & Castleman, 2016; ideas42, 2016); and booklets about college coupled with fee waivers, which affect enrollment for low-income, high-ability students (Hoxby & Turner, 2013). However, this line of research frequently studies a package of treatments where information is conveyed along with other interventions, and less is known about which aspects of treatment drive success or failure. Notably, Bettinger et al. (2012) found that telling students about their aid eligibility rather than filling out the FAFSA had no effect on enrollment. Similarly, Hoxby and Turner (2013) found that information had an effect on college outcomes but that the included fee waivers were an important determinant of application effects. Also, Darolia and Harper (2018) found that letters about student loans had a minimal effect on student borrowing decisions.[10] Similarly, Booij, Leuven, and Oosterbeek (2012) showed that information about student loans in Norway did not change student loan amounts. We contribute to this literature by considering an explicitly information-only intervention that varies the presentation and framing of information in a variety of ways.

Information interventions have been found to affect behavior in tax settings as well. Bhargava and Manoli (2015) and Manoli and Turner (2014) examined outreach to people eligible for the Earned Income Tax Credit (EITC) who did not take up the EITC. These studies found that

[9] Informational and behavioral nudges have been shown to impact education outcomes that can affect educational attainment (Jensen, 2010), college enrollment (Hoxby & Turner, 2013; Castleman & Page, 2015b), school choice (Hastings & Weinstein, 2008), and student achievement (Bergman, 2014; York & Loeb, 2014).

[10] Darolia and Harper (2018) explored why this information intervention had no effect through semi-structured interviews. They find that students intentionally deferred attention for three reasons broadly categorized as denial, depression, and resignation. Students referred to their lack of understanding rather than lack of information and the frequency of contact from the financial aid office as reasons that their behavior was unaffected.

reminders can influence a decision to take up tax credits. In both studies, the targeted population received additional information. However, the experiments also changed the method that taxpayers could claim the credits by mailing simplified worksheets.

We provide evidence that the null effect of tax credits for college is not driven by a lack of information. We cannot know that students comprehended the information we sent to them. However, several characteristics of our intervention and empirical strategies suggest our outreach changed student awareness of tax benefits for college. First, the mail and e-mail came from an official source, ApplyTexas. ApplyTexas is the official portal for college application for all public universities in Texas, which means the e-mails came from a trusted channel. Moreover, all students had interacted with ApplyTexas to apply for college. Second, we show that even after accounting for e-mail open rates, there was no change in enrollment probabilities. Lastly, among students who opened the e-mails, approximately one-third opened the e-mail multiple times, suggesting engagement with the material.

This study addresses the issue of awareness of tax credits for college. Our null findings suggest that a lack of awareness cannot explain the inefficacy of tax credits for college. We discuss other reasons, including timing and targeting of benefits later in the paper. Given that we find the provision of information did not affect student outcomes, we hypothesize that information and other supports for students may be important complements to successfully influence education outcomes.[11]

2. INSTITUTIONAL BACKGROUND

Tax Credits

Tax benefits for college are a substantial expenditure estimated to be $34.6 billion in 2018 (Crandall-Hollick 2018). This is roughly the same size as the Pell Grant program, which is the largest grant for college in the United States. Not only do tax credits for college constitute a large expenditure, they have increased in recent years. In 1998 there was roughly $5 billion in

[11] Bhargava and Manoli (2015); Manoli and Turner (2014); Bettinger et al. (2012); Castleman and Page (2015a, 2015b); Castleman, Owen and Page (2015); Barr, Bird and Castleman (2016) all fit in this category where information was conveyed in addition to a connection to counseling, a change in the decision-making process, etc.

expenditures on tax credits for college (Bulman & Hoxby, 2015). A lot of this growth occurred in the 2009 tax year with the enactment of the American Opportunity Tax Credit (AOTC). Dynarski and Scott-Clayton (2016) offer an excellent overview of the history and effects of tax credits for college. At the time of this study there were five different tax benefits for college students. The first was the American Opportunity Tax Credit, a partially refundable tax credit. Second, taxpayers could deduct students' tuition and fees. Third, full-time students over the age of 19 could count in the calculation of the Earned Income Tax Credit.[12] Fourth, the Lifetime Learning Credit was available, which is less generous than the AOTC. Lastly, full-time students over the age of 19 could still qualify taxpayers for the dependent exemption.[13]

Some tax benefits can be claimed by either students or parents.[14] Parents may claim children attending college as dependents if the child receives more than half their support from the parents and the child is less than 24 years old.[15] Given the available data, we do not know if the parents or child would claim the tax benefits for college. We sent information to the e-mails and physical addresses students provided when they applied to college. These addresses may not correspond to who claims the tax benefits. However, even if children received the e-mail and parents were eligible to claim the benefit, children could inform their parents (or vice versa). Also, all students over 24 years old would be claiming education tax benefits for themselves.

There are several potential hypotheses for why tax credits do not impact college enrollment. The first is the timing of aid receipt. A student who enrolls in school in the fall of calendar year *t* does not receive tax benefits until they file taxes in year $t + 1$—sometime between February and May. The delay between student decision-making and the receipt of benefits is a minimum of five months. The delay is even more pronounced for enrollment in January of year *t* where the delay is over 12 months. This delay between enrollment and the additional funds means that tax credits are not well suited to ease credit constraints. While tax benefits may appear to work as an incentive that changes the price of college, the timing of tax benefits makes it easy for families to perceive tax benefits as a change in income rather than a change in the price of college.

Another potential reason tax benefits' do not affect student outcomes is a lack of awareness.

[12] While the Earned Income Tax Credit is not explicitly a tax credit for college, dependents who enrolled in school could qualify otherwise-ineligible households for the EITC or a more generous EITC.
[13] There is further discussion of tax benefits for college in the online appendix.
[14] The dependent exemption and Earned Income Tax Credit can only be claimed by parents.
[15] See IRS publication 501.

Many students and families may not be aware of the availability or generosity of tax benefits for college. The most obvious time for a student to learn about tax benefits for college is when they (or their parents) file taxes after college attendance. Clearly, this occurs after students have made enrollment decisions. The intervention in this paper was primarily designed to address the issue of awareness.

ApplyTexas

ApplyTexas is an official portal used by all public universities in Texas and many public community colleges. Students can create a profile and use this to submit applications to any public university in Texas as well as participating community colleges and private universities. In Fall 2015, 97 percent of first-time undergraduates in Texas public universities had used ApplyTexas. Similarly, 57 percent of first-time undergraduate community college students had used ApplyTexas.[16] Hence, our sample is represents nearly all students who are applying to Texas universities, and a substantial, albeit smaller, share of community college applicants.

3. DATA AND EXPERIMENT

Data

The data from this project come from four data sources. The first is from the ApplyTexas portal. This contains contact information and basic demographic information including race and gender as well as indicators for parental education and self-reported family income. The second source is administrative data that the Texas Higher Education Coordinating Board (THECB) collects on all students in public universities and community colleges in the state of Texas. We primarily use the information on student enrollment in the fall of 2014 as the outcome of interest, but we also have information on graduation and financial aid. The third data source is information on who opened the e-mails we sent, which was generated by the e-mail software we used. Finally, we also supplemented student enrollment with data from National Student Clearinghouse (NSC). The NSC data we use provide out-of-state enrollment college enrollment information for all high school students from Texas public high schools who graduated from 2011 to 2014, or for any

[16] These statistics were provided in an e-mail conversation with the THECB.

students who attended a public institution of higher education in Texas from 2009 to 2015.[17]

Experiment

We randomized the provision of information to students via e-mail and mailing letters. To do so, we collaborated with the Texas Higher Education Coordinating Board (THECB), who provided physical and e-mail addresses for students who had applied to any public Texas college or university using the ApplyTexas.org portal. To foster trust in the content, all letters and emails were from ApplyTexas. ApplyTexas is a well-known, official application portal.

We summarize each information treatment in Table 1. Panel A, "Treatment Arms," shows which group of students received a particular treatment. For example, the first row of Table 1 indicates whether the information was framed with the costs or benefits of college. Panel B describes when various e-mail or letters were sent out as well as when outcomes were observed. We describe Table 1, the content of each intervention, and our samples in further detail in the below.

Sample

Our sample was students who had used the ApplyTexas portal through Fall 2014. Within this sample frame, we tailored information to three groups of students. The three groups were students at different points in their college education and thus could have different responses to information about tax benefits for college. The content delivered to these students was essentially the same.[18] The groups also received the information at different times, as described below. A timeline of when communications were sent and relevant deadlines and outcomes is included in Figure 2.

Enroll Sample

The first group we targeted was high school seniors from the class of 2014 who had applied

[17] The NSC data completely covers two of the samples studied and a significant amount of the third.

[18] The exact phrasing of the information changed from the initial e-mails as we experimented with changes in the content to bypass e-mail spam filters. We did this by sending test e-mails to a set of e-mails we had access to in order to see what got caught in the spam filters. The changes were small and typically changed the punctuation of the messages.

to college. The outcome of interest for this group was enrollment in fall 2014. There are three distinct groups of students within the Enroll group. First, some high school students received treatment. Second, some students did not receive treatment but were at high schools where some students did receive treatment. We refer to this group as "Peer Treatment" in Table 1 and Figure 3. We did this to check for evidence of information diffusion. Last, some students did not receive treatment and were at high schools where no students received treatment; these are the control students.

There were two types of information treatments for the Enroll sample. First, some treated students received two emails about tax benefits, a mailer, and a separate email about filing the FAFSA. Second, at the request of the THECB, some treated students only received the email about filing the FAFSA.

The Enroll sample had 96,330 students. The steps of the randomization can be seen in Figure 3. We split the sample between high schools based on the number of students who used the ApplyTexas portal to insure that students in the Peer Treatment were at high schools with enough students where information may diffuse.

The process for high schools with 10 or more applicants is depicted in Figure 3 Panel A. For high schools with 10 or more students who applied via ApplyTexas, 20 percent of high schools were assigned to be in the control group (i.e. no students in the high school received the information). The remaining 80 percent of schools had their students split between three groups: 60 percent of students were assigned to receive information about tax credits and a separate email about the FAFSA, 20 percent were assigned to the peer treatment that did not receive any communication but were at the same high schools as students who received communication, and 20 percent of students were assigned to receive the FAFSA-only treatment. This randomization procedure allows us to test whether information diffused throughout high schools. That is, did sending information to some students in a high school create information spillovers to students who did not receive the information? This can be answered by comparing untreated students at schools where some students were treated to students at schools where no students were treated.

The process was slightly different for high schools with fewer than 10 students applying and is shown in Figure 3 Panel B. Due to the limited number of potential peers, we omitted the "peer" treatment. Among schools that had fewer than 10 students apply, 20 percent of the high

schools were assigned to be in the control group, so they had no treated students. Of the remaining schools, 80 percent of students were assigned to receive information about tax credits and the FAFSA and 20 percent to receive a FAFSA-only treatment.

The first e-mail for the Enroll group received was sent February 18, 2014 and encouraged students to file their FAFSA. Tax emails were sent on April 1, 2014, and July 16, 2014 and a letter about tax benefits was sent on June 1, 2014. We sent the tax e-mails when students could still apply to non-selective colleges and when they were making decisions about whether and where to attend college, pay fees, and make other preparations. The first tax e-mail also arrived around the deadline for tax filing, which was intended to make the information more salient.

ReEnroll Sample

The ReEnroll sample enrolled in college in the calendar (and tax) year of 2013. These students were very likely to be eligible for tax benefits for college because they were enrolled in college. They were informed about tax benefits for college in order to see if reenrollment decisions were changed. Information was sent around tax filing season to help students claim benefits they were eligible for. Larger tax refunds have been shown to increase college attendance, which motivated us to send information around tax filing season (Manoli & Turner, 2018). The first e-mail was sent on January 17, 2014, which corresponds to the beginning of the tax filing season. The second e-mail was sent March 25, 2014, which corresponds to the last three weeks of the tax filing season. The outcome of interest was reenrollment in the fall of 2014.

The ReEnroll sample has 434,887 students with 75 percent assigned to treatment and 25 percent assigned to control. We stratified treatment assignment based on application date, family income, type of school applied to, and age.

ReApply Sample

The last group of students we targeted had previously applied to college but did not enroll in Texas colleges or universities in the 2011–12 or 2012–13 school years. These students had indicated interest in college by previously applying but ultimately did not enroll in a Texas public institution. Unfortunately, we did not have access to NSC data at the time of the intervention, so some of the students included in this sample were enrolled at out-of-state institutions. The e-mails were sent to

this group around deadlines for application to non-selective institutions, including community colleges. The first e-mail was sent on November 6, 2013, and the second was sent on July 16, 2014. The outcome of interest was enrolling in either spring or fall of 2014.[19]

The ReApply sample consisted of 526,614 students with roughly 75 percent assigned to treatment and 25 percent assigned to control. There were 18 different e-mail templates used that contained different variations of the information about tax benefits for college. We again used a stratified randomization process with stratification on application date, family income, type of school applied to, and age.

Summary statistics for the three samples are presented in Table 2. The samples have similar characteristics. The samples are 43 to 45 percent male, 37 to 41 percent Hispanic, and 12 to 19 percent of the sample reports that their father had a bachelor's degree. Of the Enroll sample, 73 percent enrolls in college. Of the ReEnroll sample, 63 percent enrolls in public higher education in the next year.

Somewhat surprisingly, the ReApply sample is likely to enroll—44 percent of students enrolled at some point during the year with 14 percent enrolling out of Texas. Those who were enrolled out-of-state were likely enrolled out-of-state prior to the intervention. This relatively high rate of enrollment shows that many of these potential students were still considering enrolling in college and may respond to information about tax benefits for college.[20]

Content

The e-mails and mailings were designed by a design firm to present the information in a visually appealing way. The ApplyTexas logo and website appear at the top of each communication. The e-mails were sent from a THECB e-mail account to add legitimacy. All communication also included Spanish language versions of the information. An example of the e-mail can be seen in Figure 1.[21] The content of the e-mails was varied to test what information, if

[19] The timing of the information could potentially affect the interpretation of the results. We discuss the reasons for the timing decisions that we made above but acknowledge that timing of information could be important. However, the timing was different across the three groups and was intended to come at times where students made decisions about college enrollment or tax filing.

[20] Appendix Table A4 presents mean differences in covariates by treatment status.

[21] All e-mail templates are at https://www.dropbox.com/sh/agyvnfxr159fsir/AACpebdkeyGqvBtr4sXL8oTfa?dl=0. The

any, affected students' decisions. The content for each group is summarized in Table 1.

The first set of content variations was designed to test whether information about potential tax benefits had a different effect when coupled with information about the costs or benefits of college attendance. This was motivated by the finding that beliefs about college costs and benefits may be biased (Bleemer and Zafar (2018). In Table 1 this is referred to as "Costs v. benefits v. neutral." In the benefits variation, students were told that college graduates in Texas earn on average $33,000 more per year than high school graduates.[22] In the costs variation, students were told that tuition in Texas was $2,400 per year for two-year colleges and $7,700 per year for four-year public colleges.[23] The final variation was neutral and there was no discussion of the costs or benefits of college. For students in the ReEnroll group, the information on the costs of college was omitted because students had already paid tuition at their institution.

The second set of content variations varied how much information students were given about tax benefits for college. In Table 1 this is referred to as "Simple v complex v more tax credits." This was designed to test if a different stated maximum benefit induced larger behavioral changes. In the "More tax credits" condition, students were told the names and maximum amounts of four different tax benefits available for college enrollment.[24] In the "Simple" treatment arm, only the EITC and American Opportunity Tax Credit were mentioned with their maximum credit amounts. The contrast between these two treatments was to determine if a higher total potential benefit (four tax benefits) had a larger effect than the two tax credits. The "Complex" treatment arm included detailed information about the eligibility requirements for the EITC and AOTC. This was designed to see if detailed information about tax benefits was more or less effective than simply stating the name and maximum value of the tax credits.

The last varied information in the emails about tax benefits was about filing a Free Application for Federal Student Aid (FAFSA). Some students in the ReEnroll and ReApply sample were told they could potentially receive more financial aid by filling out the FAFSA, and a link to www.fafsa.ed.gov was included. In Table 1 this was referred to as "FAFSA reminder in

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subject for the e-mails was always the same "Tax Benefits for College."

[22] This number was derived from the American Community Survey and accessed at the Business Journals bizjournals.com.

[23] These tuition figures came from collegeforalltexans.com and are for the 2013–2014 school year.

[24] We did not mention the Lifetime Learning Credit

tax e-mail."[25]

For the Enroll sample, some students were assigned to only receive information about the FAFSA, while the majority received a separate e-mail about filing the FAFSA in addition to e-mails about tax credits for college (Table 1, "Separate FAFSA e-mail"). The FAFSA e-mails came in two varieties: a shorter notice providing a link to the FAFSA and explaining that filing the FAFSA would determine a student's eligibility for state and federal aid, and a longer notice that also had information about early deadlines, the admissions process, the IRS retrieval tool, and the federal PIN that was required at that time for FAFSA completion.

In all tax benefit communications there was a section that described the process for claiming tax credits for college. Additionally, there were links to IRS websites that contained more detailed information about tax credits.

4. ESTIMATION

To estimate the effect of this information intervention, we leverage the fact that the intervention was randomly assigned. Because treatment was assigned randomly, it should be orthogonal, in expectation, to any student characteristics that would affect college going. For the ReApply and ReEnroll groups, the primary specification is:

$$Y_i = \alpha \cdot Treat_i + \boldsymbol{X_i\beta} + \epsilon_i \quad (1)$$

where i indexes students, Y_i is an outcome (for example, enrollment in fall 2014 or spring 2014), $Treat_i$ is an indicator for students receiving some type of intervention, X_i is a vector of student characteristics, and ϵ_i is an idiosyncratic error term.[26] X_i includes indicators for gender, race, father's education, mother's education, family income, and student classification if applicable.[27] The coefficient of interest is α, which is the intent-to-treat effect of being assigned treatment.[28] The intent-to-treat parameter, α, is a policy-relevant parameter because it incorporates both the

[25] This is not to be confused with the separate FAFSA email that the Enroll group received. The ReEnroll and ReApply groups had a treatment arm with a statement about the FAFSA included as part of the tax benefits email.

[26] This equation will be estimated using ordinary least squares.

[27] Classification denotes whether the student was a freshman, sophomore, junior, or senior.

[28] For the enroll sample this would include tax e-mails, a FAFSA e-mail, and a tax mailing. For the ReEnroll and ReApply sample, treatment was only receiving an e-mail.

size of the treatment effect of the information and the fraction of students who were actually treated. Sometimes $Treat_i$ will be separated into different indicators for different variations of the intervention. For example, $Treat_i$ will be replaced with indicators for the cost, benefit, and neutral framing of tax credits for college.[29]

Equation 1 is altered in an important way for the Enroll group to account for the randomization procedure. There are three groups of students we consider: students who received treatment, students who did not receive treatment but went to school with students who did receive treatment, and students who went to a school where no students received treatment. We test for the presence of information spillovers by computing the average enrollment rates for these groups and comparing them. To account for this structure the following equation is used for the Enroll group:

$$Y_i = \alpha \cdot Treat_i + \gamma \cdot Peer_i + \boldsymbol{X_i}\boldsymbol{\beta} + \epsilon_i \quad (2)$$

where $Peer_i$ is an indicator for students who did not receive the letters and e-mail but were in schools where some students received this treatment. As a result, γ measures the effect of information spillovers within a high school. We also control for whether the student's high school had more or less than 10 students in all specifications for the Enroll sample in $\boldsymbol{X_i}$.

While we sent e-mails to all students assigned to treatment, many students did not see the e-mail for various reasons. These included having an outdated e-mail address or the e-mail being filtered out by the spam filters. If students did not open the e-mail, they are less likely to be affected by the treatment. Fortunately, the e-mail service we used tracked whether individuals opened the e-mails we sent. We used this to information in an instrumental variables (IV) framework to examine the effect of the information on students who received and opened the e-mail containing information about tax credits for college. In this context the first-stage equation becomes

$$Open_i = \theta \cdot Treat_i + \boldsymbol{X_i}\boldsymbol{\beta} + v_i \quad (3)$$

[29] We tested that all of the treatment arms had the same effect, and in all of the samples we could not reject that all treatment arms were equal to each other.

where $Open_i$ is an indicator for a student opening the e-mail and θ is the fraction of treated individuals who opened the e-mail. X_i includes indicators for the three largest e-mail providers in the sample, Gmail, Hotmail, and Yahoo, in addition to the demographics included in equation 1.[30]

The second stage becomes

$$Y_i = \eta \cdot \widehat{Open} + \boldsymbol{X_i\beta} + \epsilon_i \quad (4)$$

Y_i is a student outcome and η is the effect of a student opening an informational e-mail. The coefficient, η, is the treatment-on-the-treated parameter and accounts for the fact that not all individuals who were sent e-mails opened one. η is useful in understanding the effect of information about tax credits for college apart from issues of incomplete take up by treated students.

For the ReApply and ReEnroll groups, robust standard errors are presented. For the Enroll group, standard errors are clustered at the high-school level to account for the treatment being partially determined by high school.

Diagnostics

We checked to make sure that student characteristics were balanced across treatment and control groups in Table 3. For the Enroll sample, the treatment-group was 1.2 percentage points more likely to be male, but this is only marginally statistically significant. Similarly, students in the "peer" group were not statistically different from the control group for any covariate. For the ReEnroll sample, none of the tested covariates is statistically different from zero. For the ReApply sample, one covariate is statistically different at the 5 percent level: treated students are 0.4 percentage points less likely to be male. Taken together these results confirm that the randomization procedure allocated similar students to treatment and control groups. We controlled for these variables to account for slight differences in the composition of the treatment and control groups and to increase precision.

Table 4 presents the first stage results. Between 21 and 43 percent of treated students opened

[30] This captures differences across these providers including spam filtering and potentially sorting across e-mail providers.

the email, depending on the sample. The effects varied across the samples, which likely reflects a combination of differences in the underlying samples and slight changes in the content of the e-mail to reduce the chance that the e-mails were caught in spam filters.

5. RESULTS

Did Students Receive Information?

A shortcoming of this study is that we cannot show that students understood the information in the mail and e-mail sent to them. We try to overcome this shortcoming in several ways. First, we designed the intervention so that information was sent from ApplyTexas, the official portal for public university application in the state of Texas. We also use an instrumental variables strategy to focus on students who opened the e-mail. Our instrumental variables strategy confirms the patterns of our reduced-form analysis with an associated loss of precision.

However, opening an e-mail does not mean that the recipient understood the information in the e-mail. To this end, we know that roughly one-third of students who opened an e-mail opened it more than once. Opening the e-mail multiple times suggests that a substantial fraction of recipients intentionally engaged with the information. While this is an imperfect proxy, it suggests a level of engagement consistent with students absorbing the relevant information.

It may be that e-mail is a bad channel to convey this information. However, the Enroll group received both physical mail and e-mail, which means that students had more than one source of information on tax credits.

Did the Treatment Affect Educational Outcomes?

Table 5 presents the effects of the intervention for the Enroll sample. The main results combine all tax treatment arms into one indicator for treatment because the results do not vary by different treatment content. Panel A shows the effect of assignment to treatment, which included both physical mailings and e-mails. The estimated impacts are small and statistically insignificant. Of the control group, 73 percent enrolled, and the upper bound of the 95 percent confidence for the intent-to-treat effect is a 0.8 percentage point or a 1.1 percent increase relative to the baseline.

Students who did not receive the letter but were in high schools where some students did receive the letter were similarly unaffected by the letters. We also show that grants received, a proxy for filing the FAFSA, and student graduation did not change.[31]

In Table 5 Panel B we instrument for opening any e-mail with an indicator for assignment to treatment as outlined in equations 3 and 4. Panel B uses assignment to treatment as an instrument for opening the e-mail. These estimates focus on the effect of opening the e-mail. However, the treatment also included a letter, so this analysis examines the effect of one component of the treatment. Similarly, there are no statistically significant effects on college enrollment, with the point estimates being small and negative.

Table 6 analyzes the ReEnroll sample. Panel A shows that assignment to receive an e-mail did not change enrollment and very small treatment effects can be ruled out of +/- 0.003 percentage points. This overall zero effect could be masking an upgrading effect where students "upgraded" from community colleges to four-year institutions. Columns 2 through 3 explicitly test for this effect by considering reenrollment in Texas community colleges and public universities separately. The coefficients are similarly small and precisely estimated, suggesting that there was no upgrading from community colleges to universities.[32]

For student graduation we find no results significant at the 5 percent level. There is a very small increase in the probability of receiving a bachelor's degree from a public school in Texas, but this effect is only significant at the 10 level. Further, after applying Romano and Wolf's multiple testing correction, the *p* value is .168 (Romano & Wolf, 2005).

Instrumenting for opening an e-mail does not substantively change the conclusions—students who opened e-mails were no more likely to have enrolled in college. Our estimates can still rule out effects of +/- 1 percentage point. Reenrollment rates were 63 percent for the control group, so ruling out a 1 percentage point change rules out a very small percent change in reenrollment. The IV estimates of upgrading are larger but are still substantively small and statistically insignificant.

Table 7 presents results for the ReApply sample. We considered both enrollment in spring 2014 and fall 2014 because the e-mails were sent in time to potentially affect both enrollments. The

[31] Specifically, the indicator is for whether a student did one of the following: filed a FAFSA, filed a Texas Application for State Financial Aid (TAFSA), or received merit aid.

[32] Results are similar when considering out-of-state schools separately.

patterns are very similar to the ReEnroll group. Panel A shows very small effects of the e-mail in the intent-to-treat estimates. The upper bound of the 95 percent confidence interval is .003 for enrollment in fall 2014 and is similarly small for the spring. The IV estimates in Panel B show very precisely estimated zeros as well. In the IV specification the results are again quite small and statistically insignificant. Overall, the evidence again suggests that the information had no effect on student outcomes.

We next test if the overall zero effect is masking whether certain message variations had an impact on student outcomes. This is shown in Table 8 for each of the three treatment samples. The results are remarkably consistent and show that none of the messaging variations had any significant impact on student outcomes. Articulating the costs vs. the benefits of college attendance did not have an effect. More tax credits described, detailed information about tax credits, and simple information about tax credits similarly did not affect enrollment or application.

We also check to see if FAFSA e-mails for the Enroll sample had an effect in Table 9. The FAFSA e-mails did not affect student enrollment. None of the estimated results is significantly different from zero, either for enrollment or for our proxy of filing a FAFSA. Roughly 5 percent of students who were sent the FAFSA e-mails clicked on any link that was included.[33]

Heterogeneity

We test for heterogeneous effects because the information provided could affect some students more than others. In particular, we focus on students with varying parental income, parental education, race, and age. We present the full heterogeneity results in the appendix Tables A1, A2, and A3. Given the number of coefficients tested, several coefficients would be expected to be significant purely by chance. Occasional coefficients are positive and significant but there are no systematic patterns.[34]

[33] There is a large literature trying to understand why students do not file their FAFSA and strategies to increase filing (Bettinger et al. [2012]; Castleman and Page [2015a]; Kofoed [2017]). These and other studies justified sending this information at the THECB's request. We find similar results to Bettinger et al. (2012) where information only does not affect FAFSA filing.

[34] For instance, in the Enroll sample there is a significant coefficient on enrollment at non-Texas private universities of .76 percentage points for students with incomes greater than 80k. However, there is still no effect on overall enrollment and after a Bonferroni correction, the result is no longer statistically significant.

We present heterogeneity by self-reported family income in Table 10 in addition to other heterogeneity analysis in the appendix and find found no effect for any income group.[35] Low-income students may be more likely to respond to information about tax benefits for college. In Table 10, the omitted category is students with self-reported family income of greater than $80,000; hence, the coefficient on "Treatment" is for that group. We find that only one coefficient is statistically different from zero at the five percent level—four-year enrollment for students with family income greater than 80k. However, several coefficients are different from the omitted category of high-income students. The main take away from Table 10 is that low-income students did not increase enrollment as a result of the information.

We also consider heterogeneity for groups who historically have lower college going rates by considering parental education and student race. We also consider students who were enrolled out of state at the time of the intervention in online Appendix Table A2.

One issue with our intervention was that it was sent to the e-mail address provided at the time of application. In some cases, parents will claim the tax credits for their student's enrollment, and in other cases the students will. Students who are 24 years old or older on January 1 during the school year are financially independent and thus would claim the tax benefits. For the ReEnroll and ReApply sample, we examine heterogeneity by whether the student was 24 years old. For the ReEnroll sample, we also consider heterogeneity by whether students were listed as independent on the FAFSA.[36] We do not find that older students' or independent students' educational outcomes were affected by information about tax credits. Hence, it does not appear that information about tax benefits affected student outcomes, even when there was no ambiguity about who would claim the tax benefits.

Our consistent finding across samples is that there is was no effect for any group. There is no effect of information about tax credits, even for groups where tax aid is more likely to be effective (low-income students, first-generation college students, and underrepresented minorities).

6. DISCUSSION

[35] This is also true when we consider the ReEnroll sample, where we examine heterogeneity by parent income as reported on the FAFSA. We put income into four bins that correspond to the four quartiles of income in the data.

[36] For the heterogeneity results for the ReEnroll sample, we considered heterogeneity by parental income and dependent status only for students who had that information.

We documented that sending out information about tax credits for college did not affect enrollment. We presented evidence that our information was viewed and engaged with by students and account for this using an instrumental variables strategy. These results suggest that a lack of awareness is not the primary barrier to tax credits for college affecting student enrollment.

We next analyze who opened the e-mails to see who was engaging with the information. Students who were more likely to open the e-mail were students who had characteristics that would predict higher college enrollment. We formalize this in Table 11. We first predict whether a student will enroll based on observable covariates such as race, parental education, and reported income. We then regress an indicator for opening the e-mails on this predicted college enrollment. Moving from a predicted enrollment of 0 to 1 increases e-mail open rates by 28.6 percentage points for the Enroll sample, 8.9 percentage points for the ReEnroll sample, and 19.9 percentage points for the ReApply Sample.

Students with relatively high predicted enrollment could have opened the e-mails at higher rates for a number of reasons. First, they may have been more likely to actually receive the e-mail because we had current e-mail addresses. Second, our e-mails could have gotten past spam filters at higher rates for these groups. Lastly, students could have been equally likely to receive the e-mail but high-probability enrollment students could have been more likely to open the e-mail. In any case, the e-mails disproportionately contacted students who had characteristics that would predict higher college attendance.

We have shown that this intervention increased the number of students who opened emails about tax credits for college. Despite opening the email, students may not have comprehended the information about tax benefits. Other interventions may use different methods to convey the same information and result in higher comprehension. For example, the information may have affected student enrollment if it were conveyed verbally or via text message from school counselors, friends, or others. A "heavier-touch" intervention would be costlier, but may affect student outcomes.

We now turn our attention toward why there was no effect of our intervention. The delivery of information in this paper did not address some of the other issues about tax credits for college, such as the timing of benefits. If students face credit constraints, then information that tax aid

will be available five months after initial enrollment is less likely to affect enrollment. Our results suggest that issues of timing are a likely reason that tax benefits for college do not change enrollment behavior.

Also, it may be that tax credits for college are not well targeted to students whose enrollment is most likely to be affected by aid. Only 24 percent of tax credits for college go to families with income of less than $25,000 (Dynarski & Scott-Clayton, 2016). Our intervention did nothing to affect the targeting of benefits to low-income students.[37]

Policymakers suggested that tax credits for college would spur additional enrollment. However, tax credits for college may have been used to provide tax breaks to middle-income families. If tax relief for middle-income families is the goal, tax benefits for college are not the most straightforward way to accomplish tax breaks for middle-income families (Dynarski & Scott-Clayton, 2016)

We also are unable to measure whether the information we conveyed increased tax credit take-up. It is possible that this information did not affect student outcomes but that it did increase the take up of tax credits. Unfortunately, it is impossible to know if this occurred without a link to administrative tax data.

Our results suggest two things about designing taxes with the intention of affecting behavior. First, the benefits must be targeted to those most likely to change behavior. Second, the timing of benefit receipt relative to the desired behavior is important.

7. CONCLUSION

We show that awareness about tax credits for college did not affect student college enrollment. Furthermore, there was no effect of a variety of information frames and accounting for students who actually received the information by opening the e-mail does not change our results.

The key insight from our study and others is that tax credits for college do not affect student outcomes—*even when students receive information*. Our results suggest that the lack of an

[37] However, eligibility for the Earned Income Tax Credit is affected by college enrollment, even though it is not typically seen as a tax benefit of college. Students who enroll in college can continue to be a dependent student, which affects parents' EITC eligibility after age 18. The EITC has been shown to positively affect long-term education and earnings outcomes (Bastian & Michelmore, 2016).

educational impact of tax benefits for college stems from issues with the timing or targeting of tax benefits rather than awareness of the benefits. Alternative uses of the funds for tax benefits for college would likely increase college access and success relative to tax credits for college.

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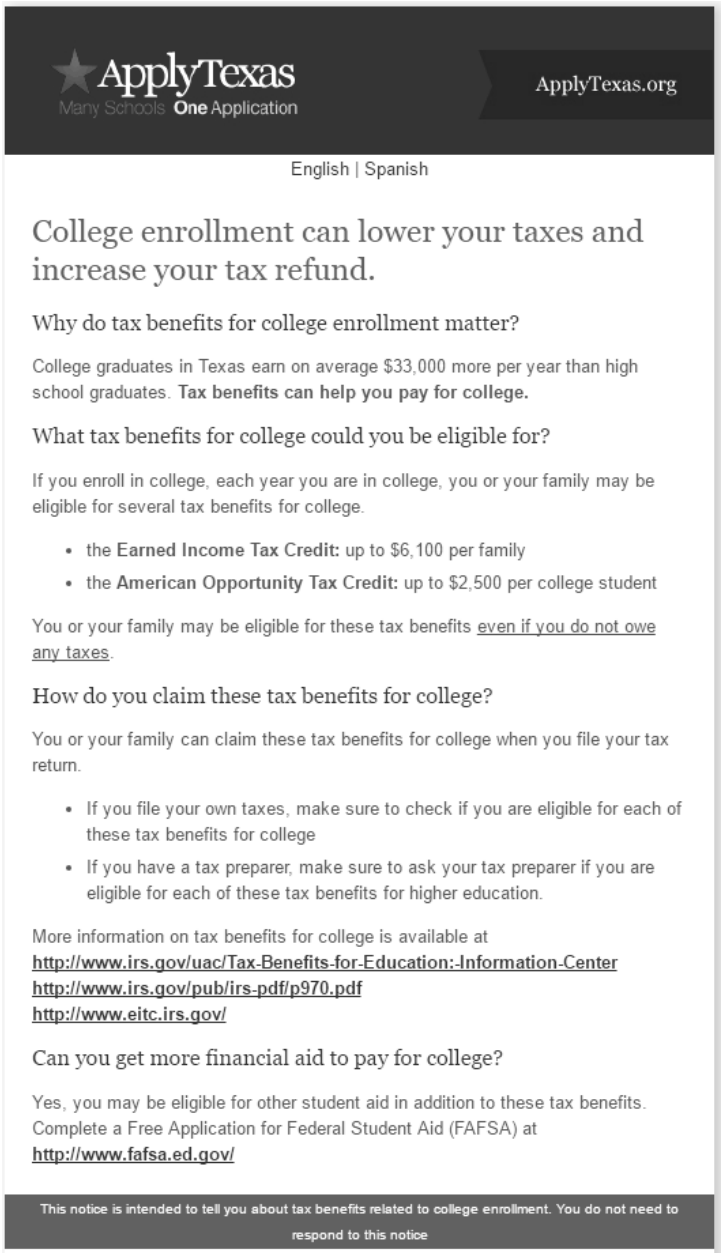
FIGURES AND TABLES

Figure 1: Example E-mail



ApplyTexas
Many Schools **One** Application

ApplyTexas.org

English | Spanish

College enrollment can lower your taxes and increase your tax refund.

Why do tax benefits for college enrollment matter?

College graduates in Texas earn on average $33,000 more per year than high school graduates. **Tax benefits can help you pay for college.**

What tax benefits for college could you be eligible for?

If you enroll in college, each year you are in college, you or your family may be eligible for several tax benefits for college.

- the **Earned Income Tax Credit:** up to $6,100 per family
- the **American Opportunity Tax Credit:** up to $2,500 per college student

You or your family may be eligible for these tax benefits even if you do not owe any taxes.

How do you claim these tax benefits for college?

You or your family can claim these tax benefits for college when you file your tax return.

- If you file your own taxes, make sure to check if you are eligible for each of these tax benefits for college
- If you have a tax preparer, make sure to ask your tax preparer if you are eligible for each of these tax benefits for higher education.

More information on tax benefits for college is available at
http://www.irs.gov/uac/Tax-Benefits-for-Education:-Information-Center
http://www.irs.gov/pub/irs-pdf/p970.pdf
http://www.eitc.irs.gov/

Can you get more financial aid to pay for college?

Yes, you may be eligible for other student aid in addition to these tax benefits. Complete a Free Application for Federal Student Aid (FAFSA) at
http://www.fafsa.ed.gov/

This notice is intended to tell you about tax benefits related to college enrollment. You do not need to respond to this notice

Figure 2: Timeline

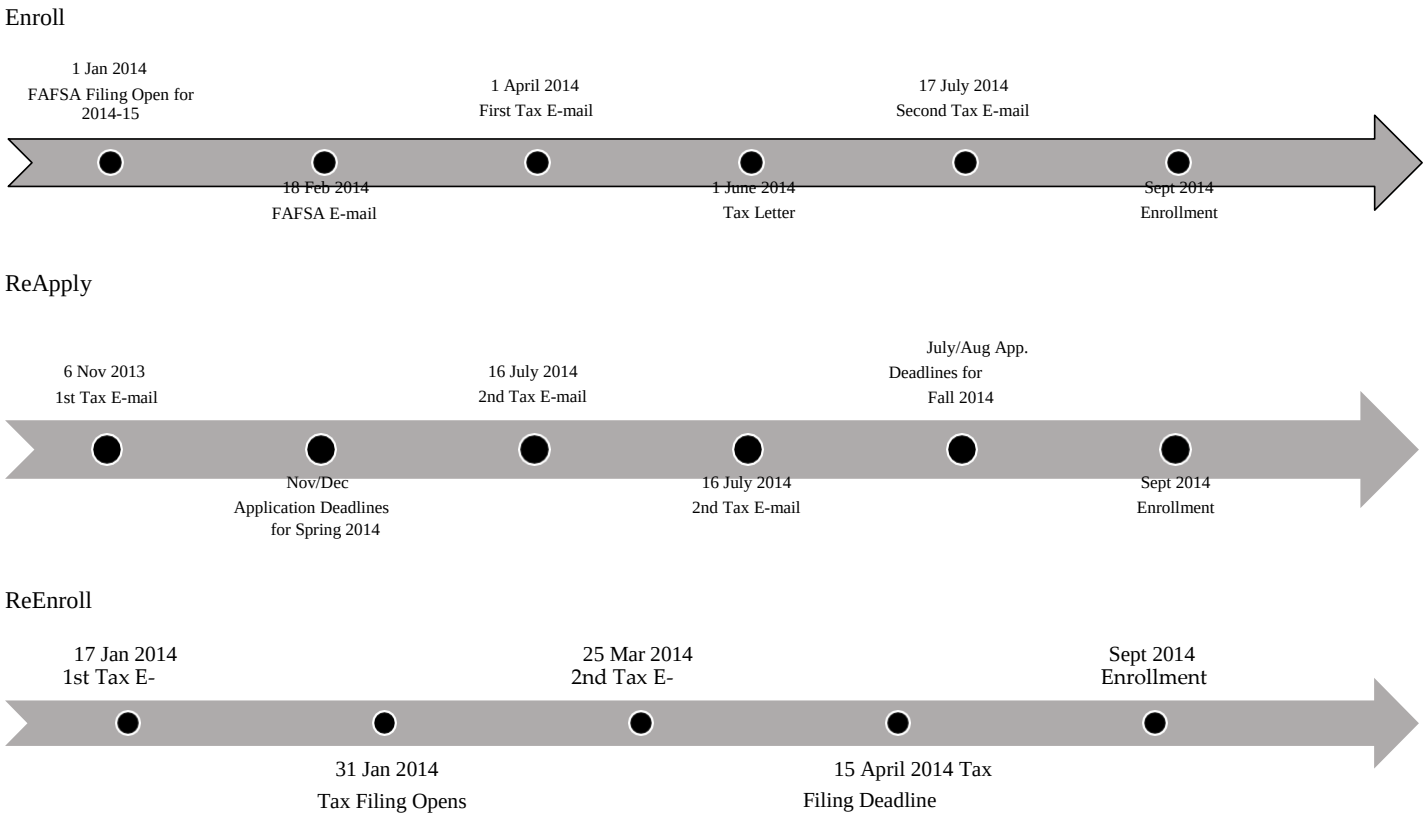
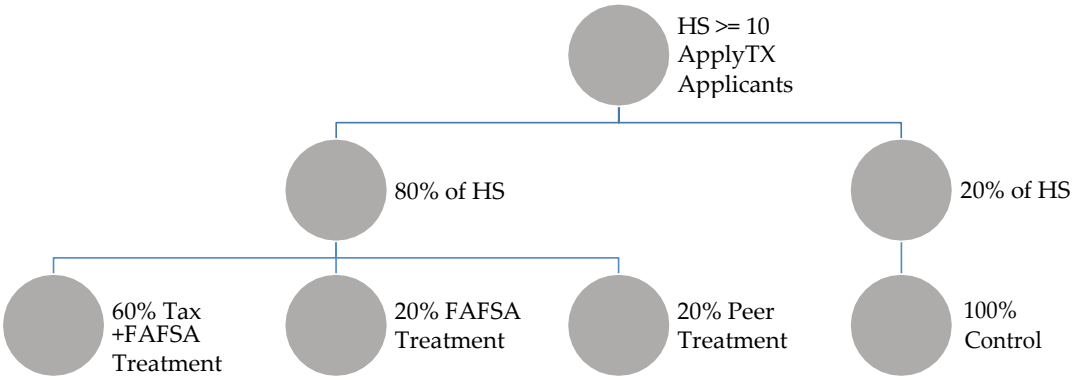


Figure 3: Enroll Treatment Randomization

Panel A



Panel B

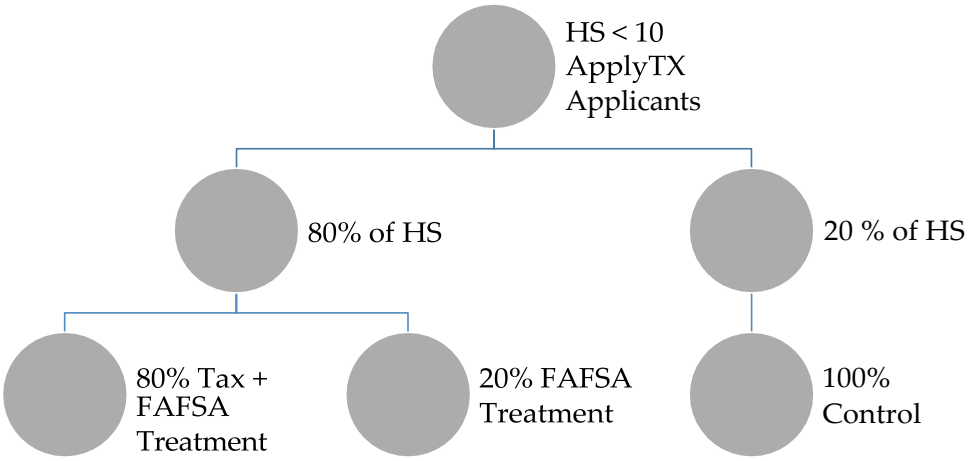


Table 1: Treatment summary

A. Treatment Arms

	Sample		
	Enroll	*ReEnroll*	*ReApply*
Costs v. benefits v. neutral	X		X
Simple v. complex v. more tax credits	X	X	X
Separate FAFSA e-mail	X		
FAFSA reminder in tax e-mail		X	X
"Peer" treatment	X		

B. Timing

	Sample		
	Enroll	*ReEnroll*	*ReApply*
First tax e-mail	1-Apr-2014	17-Jan-2014	6-Nov-2013
Second tax e-mail	16-Jul-2014	25-Mar-2014	16-Jul-2014
Letter	1-Jun-2014		
Separate FAFSA e-mail	18-Feb-2014		
Outcome	Fall 2014	Fall 2014	Spring 2014, Fall 2014

Note: Panel A describes which groups received which treatment arms for each of the three samples. See the data and experiment section for a complete description of the treatment arms. "Costs v. benefits v. neutral" indicates that the costs of college, Mincerian wage returns, or neither were included in the messaging. "Simple v. complex v. more tax credits" indicates there was variation in the amount of information about tax credits included. Separate FAFSA e-mail was a an email about filing the FAFSA. "FAFSA reminder in tax e-mail" indicates whether information on filing the FAFSA was included in the e-mail. "Peer Treatment" indicates whether the experiment was structured to test for information spillovers. Panel B shows the dates when various outreach was sent to students for each of the samples. It also states when outcomes were measured for each of the samples.

Table 2: Summary statistics

	Enroll	ReEnroll	ReApply
Variables	Mean	Mean	Mean
Male	0.45	0.43	0.43
Hispanic, non-white	0.14	0.15	0.16
Hispanic, white	0.25	0.22	0.25
Black	0.14	0.13	0.15
Asian	0.06	0.04	0.02
Other Race	0.05	0.04	0.05
Father, no high school	0.06	0.07	0.07
Father, some high school	0.07	0.08	0.09
Father, some college	0.13	0.16	0.13
Father, college	0.19	0.17	0.12
Father, graduate degree	0.12	0.09	0.07
Father, associate degree	0.04	0.05	0.04
Father, missing education	0.23	0.19	0.27
Mother, no high school	0.05	0.06	0.06
Mother, some high school	0.06	0.06	0.08
Mother, some college	0.15	0.18	0.16
Mother, college	0.22	0.19	0.13
Mother, graduate degree	0.10	0.07	0.06
Mother, associate degree	0.07	0.07	0.06
Mother, missing education	0.20	0.17	0.24
Income, 0 to 39k	0.19	0.14	0.12
Income, 40k to 79k	0.15	0.10	0.07
Income 80k or greater	0.34	0.16	0.12
Outcome: Enroll, Anywhere	0.73	0.63	0.44
Outcome: Enrolled, Texas 2yr	0.22	0.25	0.17
Outcome: Enrolled, Texas 4 yr	0.45	0.36	0.13
Freshman		0.60	
Sophomore		0.25	
Junior		0.09	
Senior		0.06	
N	80,802	434,887	526,614

Note: This table presents summary statistics for the three different analytic samples in this study. See the text for a description of the data.

Table 3: Balance of covariates

	Male	White	Father col. deg.	Mother col. deg.	Income 80k+	Predicted enroll	Freshman
Enroll							
Treatment	0.012*	0.004	0.008	0.001	0.002	-0.002	
	(0.007)	(0.027)	(0.015)	(0.016)	(0.022)	(0.007)	
Peer	0.007	-0.002	0.010	0.002	0.003	-0.002	
	(0.008)	(0.028)	(0.016)	(0.017)	(0.022)	(0.007)	
Observations	80,802	80,802	80,802	80,802	80,802	80,802	
ReEnroll							
Treatment	0.000	0.002	0.001	0.000	0.000	0.000	0.001
	(0.019)	(0.019)	(0.014)	(0.015)	(0.014)	(0.006)	(0.019)
Observations	434,887	434,887	434,887	434,887	434,887	434,887	434,887
ReApply							
Treatment	-0.004**	0.001	0.001	0.000	0.000	0.000	
	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.000)	
Observations	526,614	526,614	526,614	526,614	526,614	526,614	

Note: This table checks to see if student characteristics vary by treatment assignment. Students who only received a FAFSA e-mail are not included in this estimation for the Enroll sample. Robust standard errors are in parentheses for the ReEnroll and ReApply groups, and standard errors clustered on high school are presented for the Enroll group with * $p < .1$, ** $p < .05$, *** $p < .01$

Table 4: First stage

	Open e-mail	Open e-mail	Open e-mail
Treatment	.430***	0.336***	0.217***
	(0.004)	(0.001)	(0.001)
Excluded F(1, 2,191)	13,784	163,285.9	105,907.2
Sample	Enroll	ReEnroll	ReApply
Demographics	Yes	Yes	Yes
Observations	80,802	434,887	526,614

Note: This table shows the first stage of how many students opened e-mails that were sent to them. Each column corresponds to a different sample. Students who only received a FAFSA e-mail are not included in this estimation for the Enroll sample. Robust standard errors are in parentheses for the ReEnroll and ReApply groups, and standard errors clustered on high school are presented for the Enroll group with * $p <$ .1, ** $p <$ .05, *** $p <$ .01

Table 5: Enroll results

A. Intent to treat	Any Enrollment	Public 2yr, TX	Public 4yr, TX	Grants	Loans	"File FAFSA"
Treatment	-0.007	-0.008	-0.001	-110.2	82.3	-0.0002
	(0.007)	(0.006)	(0.008)	(85.44)	(80.2)	(0.009)
Peer	-0.006	-0.009	-0.001	-128.3	31.0	-0.002
	(0.008)	(0.007)	(0.008)	(92.05)	(86.0)	(0.010)
Bottom 95% CI	-0.022	-0.020	-0.016	-277.7	-74.8	-0.019
Top 95% CI	0.008	0.004	0.014	57.3	239.5	0.018
Control mean	0.733	0.224	0.458	4,352.2	2,413.7	0.658
B. Instrumental variables						
Open e-mail	-0.016	-0.018	-0.003	-257	193.7	-0.00007
	(0.017)	(0.014)	(0.018)	(198.9)	(185.6)	(0.022)
Peer	-0.006	-0.009	-0.001	-128.7	32.5	-0.002
	(0.008)	(0.007)	(0.008)	(92.3)	(86.0)	(0.010)
Bottom 95% CI	-0.050	-0.046	-0.038	-646.8	-170.1	-0.043
Top 95% CI	0.018	0.010	0.033	132.8	557.5	0.042
Demographics	Yes	Yes	Yes	Yes	Yes	Yes
Observations	80,802	80,802	80,802	80,802	80,802	80,802

Note: This table examines the effect of a mail and e-mail intervention to high school seniors who graduated in 2014. The outcome considered is enrollment in the fall of 2014. Panel A shows the intent to treat estimates of sending e-mails and letter. Students who only received a FAFSA e-mail are not included in this estimation. Panel B shows the effect of students opening the e-mail. Standard errors are clustered at the high school level and are in parentheses with * $p < .1$, ** $p < .05$, *** $p < .01$

Table 6: ReEnroll results

	Any enrollment	Texas CC enrollment	Texas 4yr enrollment	Grants	"File FAFSA"	Grad 4yr	Grad 2yr
A. Intent to treat							
Treatment	-0.0003	-0.002	0.002	14.72	-0.002	0.002*	-0.0001
	(0.002)	(0.001)	(0.001)	(14.07)	(0.002)	(0.001)	(0.001)
95% Confidence interval							
Bottom	-0.003	-0.005	-0.001	-12.9	-0.005	0.005	-0.002
Top	0.003	0.001	0.005	42.3	0.001	-0.00003	0.002
Control mean	0.627	0.248	0.362	2168.1	0.707	0.186	0.145
B. Instrumental variables							
Open e-mail	-0.001	-0.006	0.006	43.47	-0.007	0.008*	-0.0002
	(0.005)	(0.004)	(0.004)	(41.8)	(0.005)	(0.003)	(0.004)
95% Confidence interval							
Bottom	-0.010	-0.014	-0.002	-38.46	-0.016	0.001	-0.007
Top	0.008	0.003	0.015	125.40	0.002	0.008	0.007
Demographics	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	434,887	434,887	434,887	434,887	434,887	434,887	434,887

Note: This table examines the effect of an e-mail intervention to students who were enrolled in college in the calendar year of 2014. The outcome considered is reenrollment in the fall of 2015. Panel A shows the intent to treat estimates of sending an e-mail. Panel B shows the effect of students opening the e-mail. Robust standard errors are in parentheses with * $p < .1$, ** $p < .05$, *** $p < .01$

Table 7: ReApply results

	Enroll, fall 2014	Enroll 2yr TX, fall 2014	Enroll 4yr TX, 2014	Enroll CC spring 14	Enroll 4yr spring 14	Grants	Loans	"File FAFSA"
A. Intent to treat								
Treatment	0.001	-0.001	0.001	-0.0002	0.0002	-21.4*	-16.1	-0.001
	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(12.2)	(12.4)	(0.001)
95% Confidence interval								
Bottom	-0.002	-0.003	-0.001	-0.003	-0.002	-45.3	-40.4	-0.004
Top	0.003	0.002	0.003	0.003	0.002	2.49	8.18	0.002
B. Instrumental variables								
Opened e-mail	0.003	-0.002	0.005	-0.001	0.001	-97.8*	-74.3	-0.004
	(0.007)	(0.006)	(0.005)	(0.006)	(0.004)	(56.2)	(57.1)	(0.006)
95% Confidence interval								
Bottom	-0.011	-0.013	-0.004	-0.013	-0.008	-207.9	-186.3	-0.016
Top	0.017	0.008	0.014	0.012	0.010	12.3	37.7	0.009
Control mean	0.444	0.173	0.13	0.264	0.129	1399.3	1028.6	0.269
Demographics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	526,614	526,614	526,614	526,614	526,614	526,614	526,614	526,614

Note: This table examines the effect of an e-mail intervention to students who had applied to college from 2011 to 2012 to 2012 to 2013 but did not enroll in public colleges or universities in Texas. The outcome considered is enrollment in spring or fall of 2014. Panel A shows the intent to treat estimates of sending an e-mail. Panel B shows the effect of students opening the e-mail. Robust standard errors are in parentheses with * $p < .1$, ** $p < .05$, *** $p < .01$

Table 8: Framing

	Enroll	Reenroll	ReApply
A. Complexity			
Simple, 2 tax credits	-0.003	-0.001	0.0004
	(0.008)	(0.002)	(0.002)
Simple, 4 tax credits	-0.008	-0.002	0.001
	(0.008)	(0.002)	(0.002)
Complex, 2 tax credits	-0.009	0.003	0.00002
	(0.008)	(0.002)	(0.002)
Peer	-0.006		
	(0.008)		
B. Costs vs. benefits			
Benefits	-0.008	0.0002	0.003
	(0.008)	(0.002)	(0.002)
Costs	-0.008		-0.001
	(0.008)		(0.002)
Neutral	-0.006	0.0005	0.0004
	(0.008)	(0.002)	(0.002)
Peer	-0.006		
	(0.008)		
Control mean	0.733	0.627	0.444
Observations	80,802	434,887	526,614

Note: This table examines the effect of the types of messages that students received. The outcome considered is enrollment the fall of 2014. Panel A shows the intent to treat estimates of sending an e-mail. Panel B shows the effect of students opening the e-mail. Students who only received a FAFSA e-mail are not included in this estimation for the Enroll sample. Robust standard errors are in parentheses for the ReEnroll and ReApply groups, and standard errors clustered on high school are presented for the Enroll group with * $p < .1$, ** $p < .05$, *** $p < .01$

Table 9: FAFSA treatment

	Enroll	"Filed a FAFSA"
FAFSA, simple	-0.011	-0.007
	(0.008)	(0.010)
FAFSA, complex	-0.006	-0.003
	(0.008)	(0.010)
Tax treatment	0.001	0.004
	(0.004)	(0.004)
Peer	-0.006	-0.002
	(0.008)	(0.010)
Mean for untreated	0.733	0.660
Demographics	Yes	Yes
N	96,330	96,330

Note: This table examines the effect of e-mails about the FAFSA on enrollment. The sample is composed of high school students who had applied to college but not yet enrolled. Standard errors clustered on high school are presented for the enroll group with * $p < .1$, ** $p < .05$, *** $p < .01$

Table 10: Heterogeneity by Reported Income

	ReEnroll			Enroll			ReApply		
	Enroll	Enroll CC	Enroll 4yr	Enroll	Enroll CC	Enroll 4yr	Enroll	Enroll CC	Enroll 4yr
Treatment	-0.00471	-0.000258	-0.00650*	-0.00143	-0.00489	0.00232	0.00268	-0.00509	0.00990**
	(0.00313)	(0.00289)	(0.00383)	(0.00713)	(0.00627)	(0.0104)	(0.00403)	(0.00318)	(0.00447)
Treatment * Missing Income Report	0.00593	-0.00182	0.0110***	-0.0197*	-0.00921	-0.0119	-0.00259	0.00436	-0.00909**
	(0.00385)	(0.00354)	(0.00421)	(0.0106)	(0.00874)	(0.0120)	(0.00441)	(0.00350)	(0.00455)
Treatment * Income 0-39k	0.00416	-0.00486	0.0114*	0.0000400	-0.00179	-0.00194	-0.00112	0.00519	-0.0107*
	(0.00522)	(0.00464)	(0.00581)	(0.00912)	(0.00836)	(0.0115)	(0.00609)	(0.00454)	(0.00577)
Treatment * Income 40-79k	0.00249	0.00116	0.00414	-0.000929	0.00250	-0.00107	-0.00339	0.0133**	-0.0184**
	(0.00547)	(0.00497)	(0.00639)	(0.00886)	(0.00823)	(0.0113)	(0.00705)	(0.00540)	(0.00719)
N	434,887	434,887	434,887	80,802	80,802	80,802	526,614	526,614	526,614

Note: The omitted category is self-reported family income of greater than $80,000. Student demographics are controlled for in each regression. Robust standard errors are in parentheses for the ReEnroll and ReApply groups, and standard errors clustered on high school are presented for the Enroll group with * $p < .1$, ** $p < .05$, *** $p < .01$

Table 11: E-mail openers

	Open	Open	Open
Predicted enrollment	0.286***	0.0886***	0.199***
	(0.031)	(0.004)	(0.003)
Constant	0.043**	0.197***	0.0742***
	(0.020)	(0.003)	(0.002)
Sample	Enroll	ReEnroll	ReApply
N	80,802	434,887	526,614

Note: This table regresses whether the potential student opens an e-mail. Predicted enrollment is the predicted probability of enrollment from a logit model for enrollment with indicators for sex, race, parent education, self-reported income, and indicators for email provider.