

Broken Tax Breaks? Evidence from a Tax Credit Information Experiment with 1,000,000 Students

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Abstract

There is increasing evidence that tax credits for college do not affect college enrollment. This may be because prospective students do not know about tax benefits for credits or because the design of tax credits is not conducive to affecting educational outcomes. We focus on changing the salience of tax benefits by providing information about tax benefits for college using a sample of over 1 million students or prospective students in Texas. We sent emails 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 salience is not the primary reason that tax credits 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, tax benefits for college have been employed as an additional subsidy for college-going students. Tax benefits for college rose to prominence with the Hope and Lifetime Learning Credit in 1997 and have grown since. Tax benefits for college tuition have quickly risen in magnitude and are expected to be \$18.4 billion in 2016, or approximately 60% the size of expenditures on Pell Grants ([Joint Committee on Taxation, 2017](#)).

As tax benefits for college have grown, several studies have studied tax benefits for college's affect student outcomes. These studies have generally found that they did not affect college enrollment ([Long, 2004](#); [LaLumia et al., 2012](#); [Bulman and Hoxby, 2015](#); [Hoxby and Bulman, 2016](#)) where [Turner \(2011a\)](#) finds an increase in enrollment with tax aid generosity.¹ [Bulman and Hoxby \(2015\)](#); [Hoxby and Bulman \(2016\)](#) offer the most convincing evidence on the effect of both tax credits and the tuition deduction respectively. These two studies use administrative data from the IRS along with regression kink ([Bulman and Hoxby, 2015](#)) and discontinuity ([Hoxby and Bulman, 2016](#)) methods to examine the effect of tax benefits for college on student outcomes. In both cases, tax benefits are not found to affect any measured educational outcomes including enrollment or type of institution attended.

The lack of effects of tax benefits for college stands in contrast to the the large literature on financial aid. Many studies have documented that financial aid affects student enrollment decisions.² Both financial aid and tax benefits for college affect the price of college but both do not affect student enrollment.

With a growing consensus emerging that tax benefits do not affect student educational

¹Other studies have found that taxpayers did not maximize their credits ([Turner, 2011b](#)), and that tax credits can be captured by schools [Turner \(2012\)](#). [LaLumia et al. \(2012\)](#) does find that for some students tax credits increase enrollment but finds no effects for the sample as a whole.

²See [Deming and Dynarski \(2009\)](#) for a summary of these studies.

outcomes, questions about why these tax credits do not affect enrollment have arisen. Are tax credits for college ineffective because students are not aware of them? Are tax credits for college ineffective because of the timing? Are tax credits for college ineffective because they are poorly targeted ([Hoxby and Bulman, 2016](#))? This paper addresses the first question and the issue of salience by providing information about tax credits for college in a large scale randomized controlled trial with over 1,000,000 students in Texas. 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 on tax credits for college including the potential benefits, how to claim, and links for additional information.³ We presented the information via mailed letters and emails and varied the type of information presented. We then tracked whether the information received affected student enrollment outcomes 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 targeted three groups of students. First, we targeted rising seniors who had applied to college. Many students who apply to college including accepted students do not enroll and previous interventions have been shown to mitigate this phenomenon, which is known as “summer melt” ([Castleman, Page and Schooley, 2014](#); [Castleman and Page, 2015b](#)). Second, we focused on students who had enrolled in college with the intention of potentially affecting persistence. Information interventions have been shown to be effective at increasing persistence for enrolled students, which motivates our targeting of already enrolled students ([Castleman and Page, 2016](#)). Many students who start college do not finish; the six year graduation rate is below 60% 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](#); [Denning, 2017](#)). Third, we

³For an example of the type of information see Figure 1.

targeted students who had previously applied to Texas universities or colleges but who did not enroll in Texas. “Non traditional” students, such as students who do not start college directly after high school, students who are older, or students have some work experience, are an increasing share of higher education. This intervention was designed to target older people whose enrollment has been shown to be affected by financial aid (Seftor and Turner, 2002) and information about financial aid (Barr and Turner, 2015).⁴

This outreach was inspired by several information interventions that were found to change behavior.⁵ 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 and improving the effectiveness of public policies (Chetty, 2015).

Several studies have found that low-cost information interventions can change outcomes in higher education contexts. These interventions can range from filling out a FAFSA for students increasing college enrollment (Bettinger et al., 2012), text messages, reminders, and counselors affecting enrollment, persistence, and borrowing (Castleman and Page, 2015a,b; Castleman, Owen and Page, 2015; Barr, Bird and Castleman, 2016; ideas42, 2016), to booklets about college and fee waivers affecting enrollment for low-income, high ability students (Hoxby and Turner, 2013). However this line of research frequently studies a package of treatments where information is conveyed along with other treatments, and less is known about which aspects of each drive success or failure. Notably, Bettinger et al. (2012) find that telling students how much aid they would qualify

⁴Barr and Turner (2015) demonstrated that letters informing displaced workers about the Pell Grant increased post-secondary enrollment.

⁵Informational and behavioral nudges have been shown to impact education outcomes that can affect educational attainment (Jensen, 2010), college enrollment (Hoxby and Turner, 2013; Castleman and Page, 2015b), school choice (Hastings and Weinstein, 2008), and student achievement (Bergman, 2014; York and Loeb, 2014).

for 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 the included fee waivers were an important determinant of the application effects. Also, [Darolia \(2016\)](#) finds that letters about student loans had minimal effect on student borrowing decisions. 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 \(2015\)](#); [Manoli and Turner \(2014b\)](#) examine outreach to people eligible for the Earned Income Tax Credit (EITC) who did not take up the EITC and find that reminders can influence a decision to take up tax credits. In both of these studies the targeted population received additional information. However, the experiments also changed the method that taxpayers could claim the credits by mailing simplified worksheets. The present study exists at the intersection of education and tax credit interventions.

We provide evidence that the null effect of tax credits for college is not driven by a lack of information. We can not definitively know that students comprehended the information we sent to them. However, several characteristics of our intervention and empirical strategies provide evidence that students even among students who opened our emails did change enrollment. First, the mail and email came from an official source, ApplyTexas. ApplyTexas is the official portal for college application for all universities in Texas which made the emails come from an official channel. Moreover, all students had interacted with ApplyTexas to apply for college. So while we cannot know if students comprehended the information, the source of the information was trusted and official. We also show that even after account for email open rates, there was no change in enrollment probabilities. Among students who opened the emails, approximately one third opened the email multiple times suggesting engagement with the material.

Our study addresses the issue that tax credits for college may not be salient for stu-

dents when they make their decision by providing them with this information. Our null findings suggest that a lack of salience cannot explain the inefficacy of tax credits for college. We discuss other reasons including timing and targeting of benefits later in the paper.

Further, given that we find the provision of information did not affect student outcomes, we hypothesize that information and interventions that reduce the cost of action may be important complements to successfully influence education outcomes.⁶ Information-only interventions have more mixed impacts on education outcomes ([Bettinger et al., 2012](#); [Darolia, 2016](#)).

The rest of the paper proceeds as follows. Section 3 describes the institutional background and the intervention. Section 2 describes the intervention. Section 4 describes the data and estimating strategy. Section 5 presents the results. Section 6 discusses the findings. Finally, Section 7 concludes.

2 Experiment

In collaboration with the Texas Higher Education Coordinating Board (THECB), we obtained physical address and email information for students who had applied to all public Texas colleges or universities using the [ApplyTexas.org](#) portal. ApplyTexas is an online portal that allows students to apply to any Texas public university as well as to participating community colleges and private colleges. Our sample is formed of students who had used the ApplyTexas portal from Fall 2011 to Fall 2014. We sent both emails and physical letters to students in our sample. Messages to students were from ApplyTexas. This was intentional as 1) all students had interacted with ApplyTexas and 2) ApplyTexas is a well known and official application portal. This was done to imbue messages with authority

⁶Bhargava and Manoli (2015); Manoli and Turner (2014b); Bettinger et al. (2012); Castleman and Page (2015a,b); 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.

and importance.

Samples

We targeted three groups of students using the ApplyTexas sample as a base. The three groups were students who were at different points in their college education and thus could have different responses to information about tax credits for college. The content delivered to these students was essentially the same.⁷ The groups also received the information at different times, as described below. The description of the three groups follows below and the timing and contents of treatment are summarized in Table 1. 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 of the class of 2014 who had applied to college. The intent was to provide information to students and study how their decision to enroll in college in the fall after their high school graduation was affected. This group will be referred to as the “Enroll group.” The Enroll group had an email sent on April 1, 2014 and July 16, 2014. They also received a letter sent on June 1, 2014. Many of these students intend to enroll in college but do not for a variety of reasons. This phenomenon is known as summer melt. (Castleman and Page, 2014) The information provided was designed to potentially affect the enrollment decision. We sent this email when students could still apply to non selective colleges and when they were making decisions about whether and where to attend college, pay fees, make other preparations, etc. The first email also arrived around the deadline for tax filing which may have made it more salient.

The THECB requested that some students in the enroll sample only receive the FAFSA

⁷The exact phrasing of the information changed from the initial emails as we experimented with changes in the content to bypass email spam filters. We did this by sending test emails to a set of emails 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. Also, as discussed later, the Enroll sample received slightly different information.

emails so some treated students only received an email about the FAFSA. However, all other treated students received mail about tax credits, two emails about tax credits, and an email about filing the FAFSA. The first email the Enroll group received was sent Feb 18, 2014 and encouraged students to file their FAFSA. Two variations of this email were sent: one was a simple message about filing the FAFSA and the other had more detail about the process for filing the FAFSA.

ReEnroll Sample: These students enrolled in college in the calendar (and tax) year of 2013. These students were very likely to be eligible for tax credits for college because they were enrolled in college. They were informed about tax credits for college in order to see if reenrollment decisions were changed. This will be referred to as the “ReEnroll group.” Information was sent around tax filing season to help student claim benefits they were eligible for. Larger tax refunds have been shown to increase college attendance which motivated information being sent around tax filing season ([Manoli and Turner, 2014a](#)). The first email was sent on January 17, 2014 which corresponds to the beginning of the tax filing season. The second email 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.

ReApply Sample: The last group of students we targeted had previously applied to college from Fall 2011 to Spring 2013 but did not enroll in college 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. We sent emails to this group with the intention of testing whether this changed students decision to enroll to college. This will be referred to as the “ReApply group.” The emails were sent to this group around deadlines for application for non selective institutions, including community colleges, to affect enrollment. The first email 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.

The timing of the information could potentially affect the interpretation of the results.

We discuss the reasons for the timing decisions 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.

Content

The emails 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 emails were sent from a THECB email account to add legitimacy to the message conveyed. All communication also included Spanish language versions of the information. An example of the email can be seen in Figure 1.⁸ The content of the emails was varied to test what information, if any, affected student's 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 credits had a different effect when coupled with information about costs or benefits of college attendance. In the benefits variation, students were told that college graduates in Texas earn on average \$33,000 more per year than high school graduates.⁹ In the costs variation, students were told that tuition in Texas was \$2,400 per year for 2-year colleges and \$7,700 per year for 4-year public colleges.¹⁰ 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 final variation was neutral and there was no discussion of the costs or benefits of college.

The second set of content variations was about how much information students were given about tax credits for college. This was designed to test if a different maximum

⁸All email templates can be seen at the following [link](#). The subject for the emails was always the same "Tax Benefits for College."

⁹This number was derived from the American Community Survey and accessed at the Business Journals [bizjournals.com](#).

¹⁰These tuition figures came from [collegeforalltexas.com](#) and are for the 2013-2014 school year.

benefit induced larger changes. In one treatment arm students were told the names and maximum amounts of four different tax benefits available for college enrollment.¹¹ In another, only the EITC and American Opportunity Tax Credit (AOTC) 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. In the last treatment detailed information about the eligibility requirements for the EITC and AOTC was included. 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 information that was varied was information about filing a Free Application for Federal Student Aid (FAFSA). 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. For the Enroll sample, some students were assigned to only receive information about the FAFSA while the majority received a separate email about filing the FAFSA in addition to emails about tax credits for college. The FAFSA emails 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 credit 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.

¹¹We did not mention the Lifetime Learning Credit

Randomization

Enroll Sample: The enroll sample consists of 80,802 students. The steps of the randomization can be seen in Figure 3. We wanted to test for informational spillovers and so we structured the randomization to have three groups: students who received treatment, students at schools where no one received treatment, and students who did not receive treatment but were at schools where some students did receive treatment. In order to accomplish this we first split the sample between high schools that had more or less than 10 students use the ApplyTexas portal.

For high schools with more than 10 students who applied via ApplyTexas, 20% of high schools were assigned to be in the control group where no students in the high school received the information. The remaining 80% of schools had their students split between three groups. 60% of students assigned to receive information about tax credits, 20% assigned to a peer group that did not receive any communication but were at the same high schools as students who received communication, and 20% of students were assigned to receive a FAFSA only treatment at the request of the THECB. This enables a test of whether information was 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 students who did not receive the information at schools where some students did to students at schools where no student received the information.

The process was slightly different for high schools with fewer than 10 students applying. Due to the limited number of potential peers, we omitted the “peer” treatment. Among schools that had fewer than 10 students apply, 20% of the high schools were assigned to be in the control group and had no treated students. Of the remaining schools, 80% of students were assigned to receive information about tax credits and 20% to receive a FAFSA only treatment.

The ReApply sample consists of 526,614 students with roughly 75% assigned to treat-

ment and 25% assigned to control. There were 18 different email templates used that contained different variations of the information about tax credits for college. A stratified randomization process was employed to randomly assign students to treatment arms or the control group. We stratified on application date, family income, type of school applied to, and age.

A similar process was employed for the 434,887 students in the ReEnroll sample with 75% being assigned to treatment and 25% being assigned to control. We did not include variations about the price of college because enrolled students had already paid tuition at the time of our contact.

3 Institutional Background

3.1 Tax Credits

Tax credits for college are a substantial expenditure totalling \$31.8 billion in 2013. 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 expenditures on tax credits for college ([Bulman and 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. 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.

Several studies examine the effect of tax credits for college on student outcomes. Tax credits have generally been shown to not effect college enrollment (Long, 2004; LaLumia et al., 2012). LaLumia et al. (2012) studies older, potential students in the NLSY79 and finds that overall there is no overall effect on enrollment. However, for older men whose educational expectations were below early life expectations, the authors finds an effect. Turner (2011a) however finds an increase in enrollment with tax aid generosity. Other studies have found that taxpayers did not maximize their credits (Turner, 2011b), and that tax credits can be captured by schools (Turner, 2012).¹² Bulman and Hoxby (2015); Hoxby and Bulman (2016) offer convincing evidence on the effect of both tax credits and tuition deductions respectively. These two studies use administrative data from the Internal Revenue Service along with regression kink and regression discontinuity designs methods to examine the effect of tax benefits for college on student outcomes. In both cases, the authors find tax benefits do not affect any educational outcomes including enrollment or type of institution attended. We discuss the details of the various tax benefits for college in the Appendix.

The lack of an enrollment effect for tax credits contrasts with the findings for grant aid. The lack of an enrollment effect for tax credits puzzle and several potential hypotheses for the null effect exist. 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 credits may appear to work as an incentive that changes the price of college, the timing makes it easy for families to perceive tax credits as a change in income rather than a change in the price of college.

¹²In Turner (2011b), the finding is that on average aid is captured by schools. However, for individual students the information sent is still relevant because they will receive less tax aid if they do not file.

Another potential reason for tax credits' null effect on student outcomes is a lack of salience. Many students and families may simply not be aware of the availability or generosity of tax credits for college. The most obvious time for a student to learn about tax credits for college is when they (or their parents) file taxes after college attendance. However, this occurs after students have made enrollment decisions. The intervention studied was primarily designed to address the issue of salience. Given our null result, salience does not seem to be the key reason that tax credits for college do affect student enrollment.

3.2 ApplyTexas

ApplyTexas is a portal used by all public universities in Texas and many public community colleges.¹³ 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% of first time undergraduates in Texas Public Community Colleges had used ApplyTexas. Similarly 57% of community college of first time undergraduate community college students had used ApplyTexas.¹⁴ Hence, our sample is very representative of students who are applying to Texas universities but is less representative of community college applicants. ApplyTexas is an official state portal which gives the information conveyed additional importance.

4 Data and Estimation Strategy

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 THECB collects on all students

¹³The Texas Higher Education Coordinating Board is responsible for the administration of the site.

¹⁴These statistics were provided in an email conversation with the THECB.

in public universities and community colleges in the state of Texas. We will primarily be using the information on student enrollment in the Fall of 2014 as the outcome of interest but also have information on graduation and financial aid. The third data source is information on who opened the emails we sent which was generated by the email software we used. Finally, we also supplement student enrollment with National Student Clearinghouse (NSC). The NSC data we use has enrollments for all high school students enrolled in Texas public high schools who graduated from 2011 to 2014, or for any students who enrolled in a public institutions of higher education in Texas from 2009 to 2015. This should cover the entire Enroll sample and the entire ReEnroll sample. The ReApply sample is also covered for high school seniors who graduated from 2009-2014 but would not cover students who applied from Fall 2011 to Fall 2014 who were not high school seniors from 2009-2014.

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

Somewhat surprisingly, the ReApply sample is likely to enroll with 44% of students enrolling, with 14% enrolling out of Texas. Those who were enrolled out of state likely were enrolled out of state prior to the intervention but the data available did not allow us to track out of state enrollment. This relatively high rate of enrollment shows that these potential students may well respond to information about college enrollment because many of them actually did enroll, despite previous nonenrollment.

In order to estimate the effect of this information intervention we will leverage the fact that the intervention was randomly assigned. Because treatment was assigned randomly it should be orthogonal to any student characteristics that will affect college going. For

the ReApply and ReEnroll groups the primary specification will be of the form

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

where i indexes students, Y_i is an outcome (either 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.¹⁵ X_i includes indicators for gender, race, father education, mother's education, family income, and student classification if applicable.¹⁶ The coefficient of interest is α which is the intent-to-treat effect of being assigned treatment.¹⁷ The intent to treat parameter α , is a policy-relevant parameter because it incorporates both the size of the treatment effect 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.¹⁸

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 estimation is used for the enroll group

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

where $Peer_i$ is an indicator for students who did not receive the letters and email but were

¹⁵This equation will be estimated using Ordinary Least Squares.

¹⁶Classification denotes whether the school was a freshman, sophomore, junior or senior.

¹⁷For the enroll sample this would include tax emails, a FAFSA email, and a tax mailing. For the ReEnroll and ReApply sample, treatment was only receiving an email.

¹⁸We tested that all of the treatment arms had the same effect and in all of the samples we could not reject that they had the same effect which justifies combining the treatment arms.

in schools where some students received the email. As a result, γ measures the effect of information spillovers within a high school.

While we sent emails to all students assigned to treatment, many students did not see the email for various reasons. These included having an outdated email or our email being filtered out by the spam filters. If students did not at least open the email, they are very unlikely to be affected by the treatment. Fortunately the email service we used tracked whether the recipients of emails opened their email. We use this information in an instrumental variables framework to examine the effect of the information on students who received and opened the email containing information about tax credits for college. In this context the first stage equation becomes:

$$Open_i = \theta \cdot Treat_i + X_i\beta + \nu_i \quad (3)$$

where $Open_i$ is an indicator for a student opening the email and θ reveals the fraction of students who were sent email that opened an email. X_i includes indicators for the three largest email providers in the sample, Gmail, Hotmail, and Yahoo.¹⁹ The second stage becomes

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

Y_i is a student outcome like enrollment and η is the effect of a student opening an email containing information about tax credits for college. η is the treatment on the treated parameter and accounts for the fact that not all students who were sent emails opened the email. η is useful in understanding the effect of information about tax credits for college apart from issues of incomplete take 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 treatment being determined in

¹⁹This captures differences across these providers including spam filtering and potentially sorting across email providers.

part by high school.

Diagnostics

We check to make sure that student characteristics are balanced across treatment and control groups in Table 3. For the Enroll sample, the treatment group is 1.2 percentage points more likely to be male but this is only marginally statistically significant. Similarly, students in the “peer” group are 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% level, treated students are .4 percent points less likely to be male. Taken together these results confirm that the randomization procedure allocated similar students to treatment and control groups. We control for these variables to account for slight differences in the composition of the treatment and control groups and to increase precision.

5 Results

5.1 Did Students Receive Information?

A shortcoming of this study is that we can not show definitively that students understood the information in the mail and email sent to them. We try to overcome this shortcoming in several ways. We designed the intervention so that information was sent from ApplyTexas, the official portal for public university application in the state of Texas. Further, all students in the sample had submitted an application via ApplyTexas and were familiar with its official capacity. This was not an email from an entity that was not known or unofficial.

We also use a instrumental variables strategy to address if students internalized the information is that focus on students who opened the email. This narrows our focus

to treated students who had a chance to consume the information. Our instrumental variables strategy confirms the patterns of our reduced form analysis with an associated loss of precision.

However, opening an email does not mean that the recipient understand the information in the email. To this end, we know that roughly one third of students who opened an email opened it more than once. Opening the email 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.

Moreover, the Enroll group received both physical mail and email which means that students had more than one source of information on tax credits. It may be that email is a bad channel to convey this information. For the Enroll sample, they also received this information via a different channel, namely postal mail.

Enroll

Table 4 presents the effects of the intervention for high school seniors. The main results will combine all tax treatment arms into one indicator for treatment though the results do not vary by different treatment content. Panels A shows the effect of assignment to treatment which included both physical mailings and emails. The estimated impacts are small and statistically insignificant. The standard errors are a bit larger for this sample because there are fewer students. 73% of the control group enrolled and the top of the 95% confidence for the intent to treat effect is 1.4 percentage points or a 1.9 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.

As mentioned in Section 4, not everyone who was assigned to receive an email actually opened an email. To adjust for this, we instrument for opening any email with an indicator for assignment to treatment in Panel B as outlined in equations 3 and 4. Panel

B uses assignment to treatment as an instrument for opening the email. These estimates focus on the effect of opening the email. However, the treatment also included a letter so this examines the effect of one component of the treatment and are hence a lower bound. Similarly there are no statistically significant effects on college enrollment with the point estimates being small and negative. Panel C presents the first stage and shows that 43% of students opened the emails that were sent.

ReEnroll

Table 5 considers students who were enrolled in college and whether they reenrolled in the following year. Panel A shows that assignment to receive an email did not change enrollment and very small effects can be ruled out of $\pm .003$ percentage points. This overall zero effect could be masking an upgrading effect where students “upgraded” from community colleges to four year institutions. Columns 2-3 explicitly test for this 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.²⁰

We also show that grants received did not change. We also have a proxy for students filing the FAFSA which did not change.²¹ We also consider whether students graduated in that year and find no results significant at the 5% level. There is a very small increase in the probability of graduating from a four year school in Texas but this is only significant at the 10% level. Further after applying the Romano Wolf multiple testing correction the p value is .168 (Romano and Wolf, 2005)

Instrumenting for opening an email does not substantively change the conclusions—students who opened emails were no more likely to have enrolled in college. Our estimates can still rule out effects of ± 1 percentage point. Reenrollment rates were 63% for

²⁰Results are similar when considering out of state schools separately

²¹Specifically 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.

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. Panel C shows that 33.6% of treated students did open the email which provides evidence that some students did receive information about tax credits for college.

ReApply

Table 6 presents results for the intervention for students who had previously applied to college but were not currently enrolled in a Texas public institution. We consider both enrollment in Spring 2014 and Fall 2014 because the emails were sent in time to potentially affect both enrollments. The patterns are very similar to the ReEnroll group. Panel A shows very small effects of the email in the intent-to-treat estimates. The top of the 95% confidence interval is .003 for enrollment in Fall 2014 and is similarly small in Spring. The IV estimates in Panel B show very precisely estimated zeros as well. It is worth noting that 21.7% of students opened the emails that were sent. This is smaller than the other interventions largely because we were better able to design the emails to get past the spam filters as time went on and the ReApply emails were the first emails we sent. 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.

Message Variations

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 7 for each of the three treatment samples. The intent to treat estimates of the effect of treatment on any enrollment. 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.

The way information is framed has been shown to affect program take up.²² Further, simplified information has been shown to be effective which motivated our use of simplified and more complex presentations ([Hastings and Weinstein, 2008](#); [Bhargava and Manoli, 2015](#)). Information about benefits has been found to affect student investment in some contexts which motivated our descriptions of the costs and benefits of schooling ([Jensen, 2010](#)).

FAFSA Emails

We also check to see if FAFSA emails for enrolled students had an effect in Table 8. The FAFSA emails did not affect student enrollment. None of the estimated results are significantly different from zero, either for enrollment or our proxy of filing a FAFSA. Roughly 5% of students who were sent the FAFSA emails clicked on any link that was included.²³

Heterogeneity

We test for heterogeneous effects by demographic information. The type of information available differs by the sample. But we find that 4% of the coefficients tested are statistically different at the 5% level which is consistent with what would be expected by chance. In particular we examine heterogeneous effects for students who are more likely to be affected. We examine heterogeneity by family income, financial dependence, and whether a student filed the FAFSA. These results are available upon request.²⁴

²²See [Saez \(2009\)](#) for instance)

²³There 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.

²⁴We interact treatment with indicators for race, reported family income, indicators for father's education, indicators for mother's education, and an indicator for being over age 24 for the ReEnroll and ReApply

6 Discussion

We document that sending out information about tax credits for college did not affect enrollment. We present evidence that our information was viewed and engaged with by some students and account for this using instrumental variables. Our results suggest that information is not the primary barrier to tax credits for college affecting student enrollment.

Even with evidence that many students engaged with the information, we still find that it did not effect enrollment. We analyzed who opened the emails we sent to see who was engaging with the information. Students who were most likely to open the email were students who had characteristics that would predict higher college enrollment. We examine what predicts who opens the emails and find that female students, students with higher parental education, and Asian students were more likely to open emails. In fact, students who eventually enrolled or reenrolled were more likely to open the email.²⁵

It is not clear if students with relatively higher probability of enrollment opened the emails is because they were more likely to receive the email because we had current email accounts or were more likely to get through spam filters for these groups, or if students were equally likely to receive the email but students from groups with higher college going rates were more likely to open the email. In either case, the emails disproportionately contacted students who had characteristics that would predict higher college attendance. This suggests that students who engaged with the information were the students who were least likely to be on the margin of college attendance.

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

sample. We have more information for the ReEnroll sample and additionally check heterogeneity by family income as reported on the FAFSA (in bins and linearly), class standing, and financial dependence as reported on the FAFSA for the ReEnroll sample.

²⁵Full results of this exercise are available upon request.

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

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% of tax credits for college go to families with income less than \$25,000. (Dynarski and Scott-Clayton, 2016) However, 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 affecting EITC eligibility after age 18 if they enroll in college. This tax benefit of college is likely to affect lower income students than the American Opportunity Tax Credit or Lifetime Learning Credit.²⁶ It may be that tax credits for college were intended to serve another purpose—provide tax breaks to middle income families. If this is the goal, tax benefits for college are not the most straightforward way to accomplish tax breaks for middle income families. (Dynarski and 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 as we document but that it did increase take up of tax credits. Unfortunately, it is impossible to know if this occurred without a link to administrative tax data.

7 Conclusion

Using a sample of over 1 million students in Texas we show that information about tax credits for college did not affect student college enrollment. We show that there was no effect for any of the treatment arms irrespective of content. We also show that accounting for students who actually received the information by opening the email does not change our results.

²⁶EITC benefits have been shown to affect college enrollment.(Manoli and Turner, 2014a)

The key insight from our study and others is that tax credits for college do not affect student outcomes—*even when students receive information them*. Our results suggest that the lack of an educational effect of tax credits for college stems from issues with the timing or targeting of tax credits rather than salience. Alternative uses of the funds for tax credits for college would likely increase college access and success relative to tax credits for college. As it stands now, tax credits for college operate mostly as a subsidy to middle income college goers and their families.

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8 Figures and Tables

Figure 1: Example Email

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

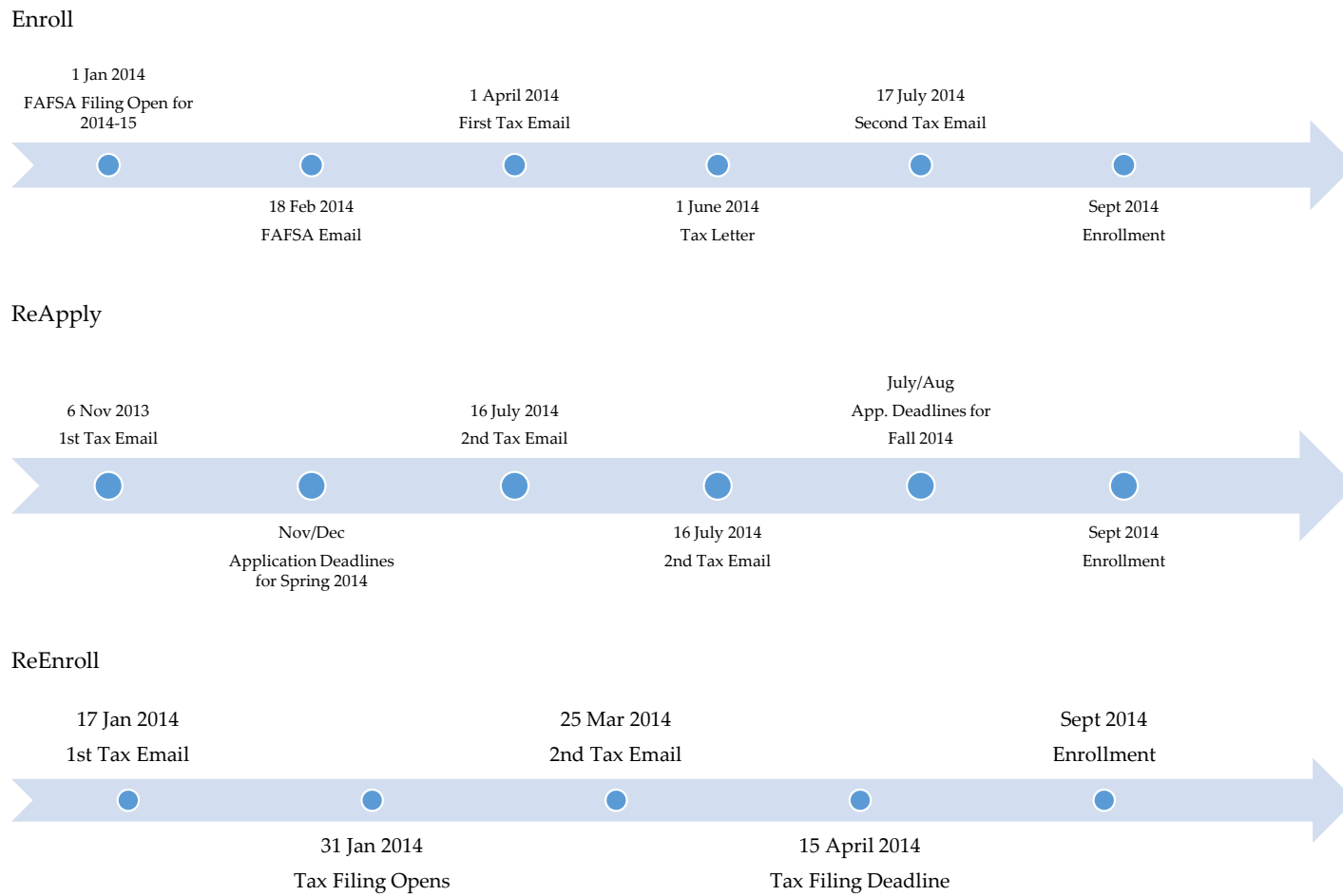


Figure 3: Enroll Treatment Randomization

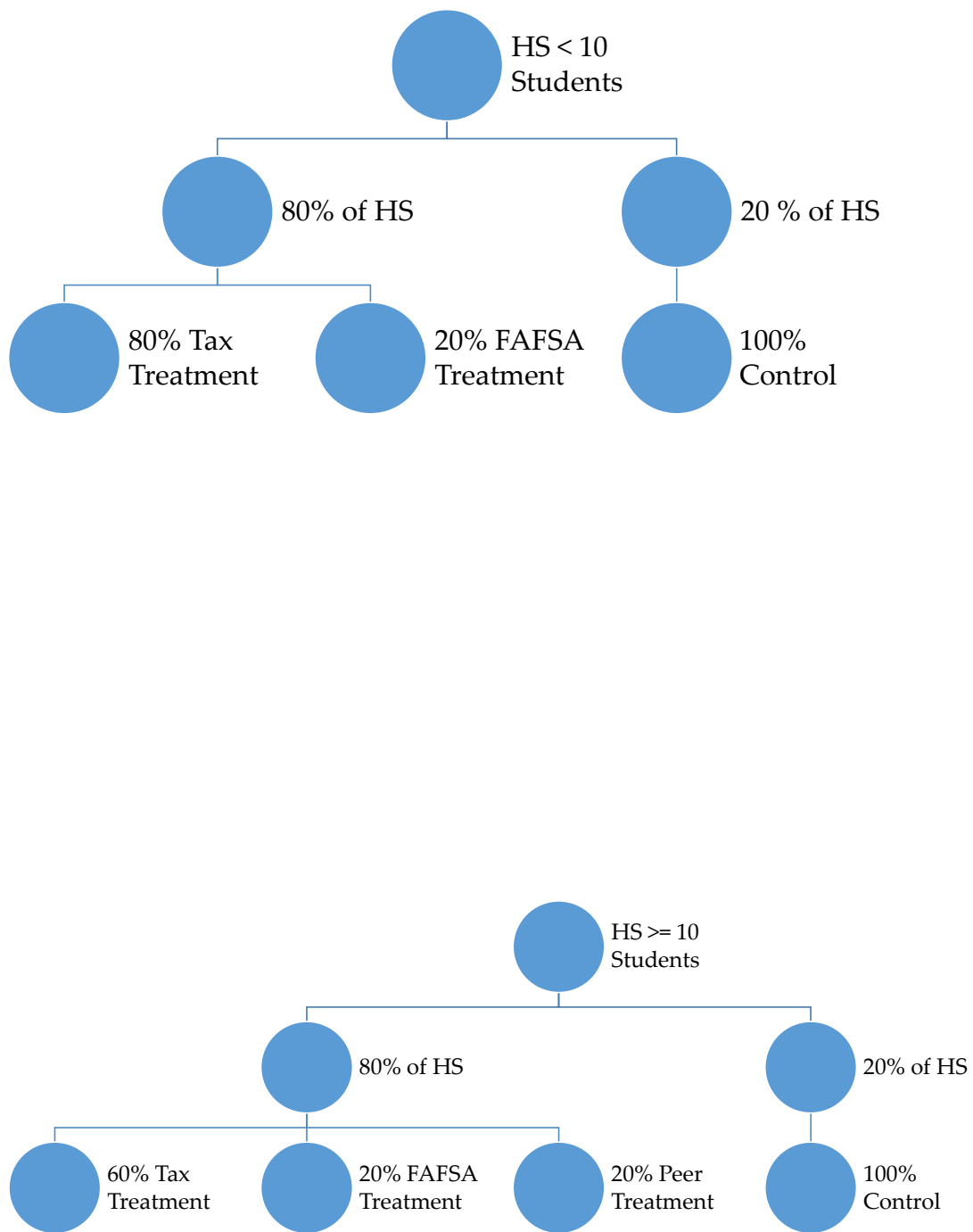


Table 1: Treatment Summary

		Samples	
	Enroll	ReEnroll	ReApply
Treatment Arms			
Costs v. Benefits v Neutral	X		X
Simple v Complex v More Tax Credits	X	X	X
Separate FAFSA email	X		
FAFSA reminder in Tax Email		X	X
“Peer” Treatment	X		
Timing			
First Tax Email	1-Apr-2014	17-Jan-2014	6-Nov-2013
Second Tax Email	16-Jul-2014	25-Mar-2014	16-Jul-2014
Letter	1-Jun-2014		
Separate FAFSA Email	18-Feb-2014		
Outcome	Fall 2014	Fall 2014	Spring 2014, Fall 2014

Table 2: Summary Statistics

Variables	Enroll		ReEnroll		ReApply	
	Mean	Std. Dev	Mean	Std. Dev	Mean	Std. Dev
Male	0.45	0.50	0.43	0.49	0.43	0.50
Hispanic, non-white	0.14	0.35	0.15	0.36	0.16	0.36
Hispanic, white	0.25	0.43	0.22	0.42	0.25	0.43
Black	0.14	0.34	0.13	0.33	0.15	0.36
Asian	0.06	0.23	0.04	0.19	0.02	0.15
Other Race	0.05	0.22	0.04	0.21	0.05	0.21
Father, no high school	0.06	0.24	0.07	0.25	0.07	0.26
Father, some high school	0.07	0.26	0.08	0.27	0.09	0.29
Father, some college	0.13	0.33	0.16	0.36	0.13	0.34
Father, college	0.19	0.39	0.17	0.37	0.12	0.32
Father, graduate degree	0.12	0.32	0.09	0.28	0.07	0.25
Father, associate degree	0.04	0.20	0.05	0.22	0.04	0.19
Father, missing education	0.23	0.42	0.19	0.39	0.27	0.44
Mother, no high school	0.05	0.22	0.06	0.24	0.06	0.24
Mother, some high school	0.06	0.24	0.06	0.25	0.08	0.27
Mother, some college	0.15	0.35	0.18	0.39	0.16	0.37
Mother, college	0.22	0.41	0.19	0.39	0.13	0.34
Mother, graduate degree	0.10	0.29	0.07	0.26	0.06	0.23
Mother, associate degree	0.07	0.25	0.07	0.26	0.06	0.24
Mother, missing education	0.20	0.40	0.17	0.37	0.24	0.43
Income, 0 to 39k	0.19	0.40	0.14	0.35	0.12	0.32
Income, 40k to 79k	0.15	0.36	0.10	0.30	0.07	0.25
Income 80k or greater	0.34	0.47	0.16	0.36	0.12	0.32
Enroll, Anywhere	0.73	0.45	0.63	0.48	0.44	0.50
Enrolled, Texas 2yr	0.22	0.41	0.25	0.43	0.17	0.38
Enrolled, Texas 4 yr	0.45	0.50	0.36	0.48	0.13	0.34
Freshman			0.60	0.49		
Sophomore			0.25	0.43		
Junior			0.09	0.29		
Senior			0.06	0.23		
N	80,802		434,887		526,614	

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* (0.007)	0.006 (0.027)	0.008 (0.015)	0.001 (0.016)	0.002 (0.022)	-0.002 (0.007)	
Peer	0.007 (0.008)	-0.008 (0.027)	0.011 (0.015)	0.002 (0.017)	0.003 (0.022)	-0.003 (0.007)	
Observations	80,802	80,802	80,802	80,802	80,802	80,802	
ReEnroll							
Treatment	0.000 (0.019)	0.002 (0.019)	0.001 (0.014)	0.000 (0.015)	0.000 (0.014)	0.000 (0.006)	0.001 (0.019)
Observations	434,887	434,887	434,887	434,887	434,887	434,887	434,887
ReApply							
Treatment	-0.004** (0.002)	0.001 (0.002)	0.001 (0.001)	0.000 (0.001)	0.000 (0.001)	0.000 (0.000)	
Observations	526,614	526,614	526,614	526,614	526,614	526,614	

This table checks to see if student characteristics vary by treatment assignment. Robust standard errors are in parenthesis 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: Enroll Results

	Any Enrollment	Public 2yr, TX	Public 4yr, TX	Grants	Loans	File FAFSA
A. Intent to Treat						
Treatment	-0.008 (0.008)	-0.008 (0.006)	-0.002 (0.008)	-111.8 (85.3)	75.7 (80.4)	-0.001 (0.009)
Peer	-0.004 (0.008)	-0.009 (0.007)	0.002 (0.008)	-122.2 (91.76)	55.54 (85.98)	-0.001 (0.010)
Demographics	Yes	Yes	Yes	Yes	Yes	Yes
B. Instrumental Variables						
Open Email	-0.017 (0.017)	-0.018 (0.014)	-0.004 (0.018)			
Peer	-0.004 (0.008)	-0.009 (0.007)	0.002 (0.008)			
Control Mean	0.733	0.224	0.458	4352.2	2413.7	0.658
Demographics	Yes	Yes	Yes	Yes	Yes	Yes
C. First Stage						
Treatment	.431*** (.004)					
Demographics	Yes					
Observations	80,802	80,802	80,802	80,802	80,802	80,802

This table examines the effect of an mail and email 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 emails and letter. Students who only received a FAFSA email are not included in this estimation. Panel B shows the effect of students opening the email and Panel C is the first stage where opening the email is the outcome. Standard errors are clustered at the high school level and are in parenthesis with * $p < .1$ ** $p < .05$ *** $p < .01$

Table 5: ReEnroll

	Any Enrollment	Texas CC Enrollment	Texas 4yr Enrollment	Grants	FASFA Filed	Grad 4yr	Grad 2yr
A. Intent to Treat							
Treatment	-0.000 (0.002)	-0.002 (0.001)	0.002 (0.001)	14.72 (14.07)	-0.002 (0.002)	0.002* (0.001)	0.000 (0.001)
Control Mean	0.627	0.248	0.362	2168.1	0.707	0.186	0.145
B. Instrumental Variables							
Open Email	-0.001 (0.005)	-0.006 (0.004)	0.006 (0.004)	43.47 (41.8)	-0.007 (0.005)	0.007* (0.003)	-0.000 (0.004)
Control Mean	0.627	0.248	0.362	2168.1	0.707	0.186	0.145
C. First Stage							
	Open Email						
Treatment	0.336*** (0.001)						
Observations	434,887	434,887	434,887	434,887	434,887	434,887	434,887
Demographics	Yes	Yes	Yes	Yes	Yes	Yes	Yes

This table examines the effect of an email intervention to students who had 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 email. Panel B shows the effect of students opening the email and Panel C is the first stage where opening the email is the outcome. Robust standard errors are in parenthesis with * $p < .1$ ** $p < .05$ *** $p < .01$

Table 6: ReApply Results

	Enroll, Fall 14	Enroll 2yr TX, Fall 14	Enroll 4y TX, Fall 14	Enroll CC Spr 14	Enroll 4y Spr 14	Grants	Loans	File FAFSA
A. Intent to Treat								
Treatment	0.001 (0.002)	-0.001 (0.001)	0.001 (0.001)	0.000 (0.001)	0.000 (0.001)	-21.4* (12.2)	-16.1 (12.4)	-0.001 (0.001)
B. Instrumental Variables								
Opened Email	0.003 (0.007)	-0.002 (0.006)	0.005 (0.005)	-0.001 (0.006)	0.001 (0.004)	-97.8* (56.2)	-74.300 (57.1)	-0.004 (0.006)
Control Mean	0.444	0.173	0.13	0.264	0.129	1399.3	1028.6	0.269
C. First Stage								
	Open Email							
Treatment	0.217*** (0.001)							
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

This table examines the effect of an email intervention to students who had applied to college from 2011-12 to 2012-13 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 email. Panel B shows the effect of students opening the email and Panel C is the first stage where opening the email is the outcome. Robust standard errors are in parenthesis with * $p < .1$ ** $p < .05$ *** $p < .01$

Table 7: Framing

	Enroll	Reenroll	ReApply
A. Complexity			
Simple, 2 Tax Credits	-0.004 (0.008)	-0.001 (0.002)	0.000 (0.002)
Simple, 4 Tax Credits	-0.009 (0.008)	-0.002 (0.002)	0.001 (0.002)
Complex, 2 Tax Credits	-0.010 (0.008)	0.003 (0.002)	0.000 (0.002)
Peer	-0.004 -0.00806		
B. Costs vs Benefits			
Benefits	-0.008 (0.008)	0.000 (0.002)	0.003 (0.002)
Costs	-0.008 (0.008)		-0.001 (0.002)
Neutral	-0.006 (0.008)	0.000 (0.002)	0.000 (0.002)
Peer	-0.004 (0.008)		
Control Mean	0.733	0.627	0.444
Observations	80,802	434,887	526,614

This table examines the effect of an 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 email. Panel B shows the effect of students opening the email and. Robust standard errors are in parenthesis 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 8: FAFSA Treatment

	Enroll	Filed a Fafsa
FAFSA, simple	-0.011 (0.008)	-0.007 (0.010)
FAFSA, complex	-0.006 (0.008)	-0.003 (0.010)
Tax Treatment	0.001 (0.004)	0.004 (0.004)
Peer	-0.004 (0.008)	-0.001 (0.010)
Mean for Untreated	0.733	0.658
Demographics	Yes	Yes
N	96,330	96,330

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

A Description of Tax Credits for College

This section describes tax benefits for college in more detail. The rules described apply to the 2013 tax year but are similar to the 2016 tax year. The information for this sections comes from IRS publications 970, 596, and 501. ([Internal Revenue Service, 2013a,b,c](#))

First, the American Opportunity Tax Credit (AOTC) started in the 2009 tax year. Students are eligible if they are pursuing a degree, are enrolled at least half time, have not finished the first four years of higher education, and have not claimed the AOTC or Hope credit for more than four tax years, and have no felony drug convictions. The maximum value of this tax credit is \$2,500 of which \$1,000 is refundable. This means that even if a family has no tax liability, they can receive up to \$1,000 as a tax refund.

Second, the tuition and fees deduction allows tax files to deduct up to \$4,000 of qualified educational expenses from taxable income. Filers cannot claim the tuition and fees deduction and the AOTC for the same student in the same tax year. Also, the tax filer's income must be less than \$160,000 if married filing jointly or \$80,000 if the tax filer is single.

Third, the Lifetime Learning Credit(LLC) is a nonrefundable tax credit that has a maximum of \$2,000. In order to claim the credit, tax filers who file jointly must have an income of less than \$127,000 or \$63,000 if single. There is no limit on the number of years this credit can be claimed. Tax filers cannot claim the LLC in the same year they claim the tuition and fees deduction. Tax filers also cannot claim the LLC and AOTC on the same expenses.

Fourth, students may qualify taxpayers for the dependent exemption. In order to qualify for a dependent exemption, children must be under the age of 19 at the end of the year unless they are enrolled in college. If a child is enrolled in college, they qualify their parents for a dependent exemption until they are 24 years old. In 2013, the dependent exemption was \$3,900.

Finally, the Earned Income Tax Credit (EITC) is a refundable tax credit for tax filers

with low to moderate income. Tax filers must have earned income to qualify for the credit and the exact amount of the refund depends on the family structure and income of the tax filer. Importantly, additional qualifying children can increase the tax benefit. Children must be under age 19 at the end of the tax year unless they are a student, in which case they must be under age 24. Hence, if a student enrolls in college their parents may qualify for a higher EITC benefit.