The Effect of federal Oversight on For-Profit Colleges

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November 11, 2019

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Abstract

For-profit colleges experienced a 33% enrollment decline between 2010 and 2015 following an increase in federal oversight. Did oversight cause this decline? I assess the causal effect of two policies on for-profit enrollment: a significant report on misleading for-profit recruiting, and threatened federal student aid sanctions on under-performing colleges. I use a difference-in-difference framework that exploits the differential exposure of a treatment group to each policy. For the report, treatment is based on the presence of a local alternative; for sanctions, it is based on a debt-to-income threshold. Both policies significantly contributed to the enrollment decline: The report caused a 45% enrollment decline over 5 years at for-profit colleges with a nearby alternative, while the threat of sanctions led to a 122% greater enrollment decline at for-profit colleges below the performance threshold.

*Email: jfabina@bu.edu. I am indebted to Adam Guren, Simon Gilchrist, and Daniele Paserman for their invaluable support and encouragement throughout this project. I also thank Stephen Terry, Pascual Restrepo, Donald Cox, Stefania Albanesi, Kevin Shaver, Daniele Coen-Pirani, Marla Ripoll, and seminar participants at Boston University, the Green Line Macro Meeting, and University of Pittsburgh for their helpful comments and suggestions. All errors are my own.

1 Introduction

For-profit colleges in the United States experienced a dramatic 33% enrollment decline between 2010 and 2015. This dwarfs the enrollment losses at public colleges over the same period of just 3.9%. What caused these for-profit enrollment losses? Immediately preceding the for-profit decline, the federal government enacted two pieces of oversight of for-profit colleges: A Government Accountability Office (GAO) report on misleading for-profit recruiting practices and a program of federal student aid sanctions on underperfoming for-profit colleges.[1] The for-profit enrollment decline is often attributed to these pieces of oversight, however the causal effect of each has not been measured.

Determining the contribution of each policy is complicated due to their near simultaneous timing and other enrollment dynamics at play during this period. Furthermore, the ability of such oversight to substantially alter student enrollment decisions and the operation of for-profit colleges is unclear. Prospective college students often hold incorrect beliefs and lack information regarding college costs and outcomes, which a government report may not be able to significantly correct.[2] Additionally, no colleges lost access to federal aid — they were only exposed to the threat of sanctions.[3] With the Department of Education recently ending the sanctions program and increasing the information provided to prospective college students, whether under-performing and expensive for-profit colleges continue to decline or experience an enrollment recovery will depend on the relative effect of these policy tools.

I estimate the causal effect of the GAO report and the threat of federal aid sanctions on for-profit college enrollment and graduations. I exploit college- and program-level variation in exposure to each piece of federal oversight in a difference-in-differences framework. For-profit exposure to the GAO report is based on the ability of students to attend a local not-for-profit alternative college. Exposure to the threat of sanctions is determined by graduate debt-to-earnings ratio thresholds. I find that the GAO report caused a 45% enrollment decline over 5 years at for-profit colleges with a local alternative, while the threat of sanctions led to a 122% greater enrollment decline over 5 years at for-profit colleges with a failing debt-to-income ratio than those with a passing ratio.

The GAO report detailed the findings of an undercover investigation into for-profit colleges. The release of the report and the extensive media coverage that followed informed students of the deceptive recruiting practices used by many for-profit colleges to hide the high costs and poor outcomes students face. This provided a negative reputation shock to for-profit colleges and revealed that not-for-profit colleges are often a better investment. In response, some current and prospective for-profit students may have searched for a desired degree at a less-expensive alternative not-for-profit college. For-profit colleges and programs

[1] Graduates of these colleges often struggle to find employment (Deming et al. (2012)) and repay student loans (Looney and Yannelis (2015)). Graduates also experience no improved labor market outcomes over those provided by similar not-for-profit colleges, despite being more expensive to attend (Cellini and Turner (2018), Lang and Weinstein (2013), Liu and Belfield (2014)).

[2] Incorrect beliefs about college costs and outcomes have been found to affect enrollment and student aid take-up rates (Avery and Hoxby (2003),Booij et al. (2010), Barr and Turner (2017), Bettinger et al. (2009)) as well as drives the choice of major (Baker et al. (2017), Altonji et al. (2015)).

[3] Under previous government sanction programs, for-profit colleges that receive sanctions have experienced enrollment losses (Cellini et al. (2016), Darolia (2013)).

are thus exposed to the GAO report through the ability of students to switch to a nearby, less expensive and selective alternative. I estimate the causal effect of differential exposure to the GAO report as the difference in enrollment between for-profit colleges with alternatives and those without alternatives before and after the release of the report. Consider ITT Technical College and the University of Phoenix, two high-profile for-profit colleges. In Madison, WI, the ITT Tech campus offers 2-year degrees for $17,000 per year, while the University of Phoenix campus offers 4-year degrees for $11,000 per year. Students and applicants to ITT Tech have a local alternative: Madison Area Technical College, a local public college offering comparable 2-year degrees for less than a fifth of the price of ITT Tech. Students of the University of Phoenix have no such alternative however, as the only local colleges offering similar 4-year degrees are more expensive or more selective than the University of Phoenix.[4] With no substitute college available to students, the University of Phoenix in Madison is far less exposed to the GAO report than ITT Tech.

The Department of Education's Gainful Employment (GE) program threatened underperforming for-profit colleges with federal student aid sanctions. Under this program, college programs were given a grade based on average graduate debt-to-earnings ratios. Students attending a failing for-profit program were not eligible for federal student loans and grants. The GE program was announced in July of 2010 with preliminary grades released in July of 2012, however no sanctions were ever imposed before the program was cancelled in 2019. Thus enrollment declines caused by this program occur only from the threat of sanctions. Since losing federal aid eligibility strips for-profit colleges of their main source of revenue, non-passing for-profits may take action to improve graduate outcomes upon receiving a preliminary non-passing grade.[5] To do so, colleges admit fewer students in struggling programs or eliminate these programs altogether, both of which would lead to a decline in enrollment. Since the preliminary grades are released publicly, students may also respond to the GE program by avoiding programs given a poor grade. Additionally, for-profit colleges may act on private information on graduate outcomes following the program announcement in 2010, before the grades were released. The causal enrollment response to the GE program is the difference between non-passing for-profit colleges and passing for-profit colleges before and after the GE preliminary grades are released.

One complication to examining these two policies is that they occur nearly simultaneously and there is a large overlap between the treatment groups. Therefore I estimate the responses to the GAO report and GE program jointly using a two-way fixed effect model, allowing me to separate the enrollment response to each policy. Additionally, I control for observable differences between the sets of treatment and control groups that might bias the estimated treatment effects, such as the price of tuition and local employment growth. I use a panel of colleges from the Department of Education to estimate these effects. I also assess the response of college completions (graduations) to each treatment at the program level.

I find that both pieces of federal oversight caused substantial enrollment and graduation declines at treated for-profit colleges. The GAO report caused a 8% enrollment decline in 2011 and the GE program caused a 26% enrollment decline in 2013. The effects grow over

[4]These colleges are the University of Wisconsin-Madison, a selective public university, and Edgeworth College, a private nonprofit with tuition of $21,000 per year.

[5]For-profit colleges are currently subject to the "90-10 rule" under the Higher Education Act, requiring that no more than 90% of a college's revenues come from federal student aid.

time, with the GAO report causing a 45% enrollment decline by 2015. However, the threat of sanctions causes a considerably larger decline, with a 122% greater enrollment decline at failing colleges than at passing colleges by 2015. At the program level, the GAO report caused a 35% decline in graduations and the threat of sanctions caused a 46% decline in graduations by 2015. I find that the initial response to each policy occurs along the intensive margin, while more than three quarters of the long-term enrollment decline is driven by college closures. These long-term results imply that the threat of sanctions is a more effective tool than providing information for policymakers interested in lowering enrollment at expensive and under-performing for-profit colleges.

My results serve as a lower bound of the full for-profit enrollment response to each of these policies, as the control group for each treatment may also be treated. The GAO report may cause some for-profit students to forgo college altogether and enter the labor market. Both for-profits with and without an alternative may experience an enrollment decline from this response. Similarly, for-profits passing under GE rule may also be treated by the threat of sanctions if these colleges and programs take precautionary actions that lower enrollment to decrease the risk of poor graduate outcomes. For instance, a passing college might hesitate to open new programs or expand class sizes under the GE rules. In each case, the control colleges and programs may experience an enrollment decline that would not occur in the counterfactual untreated state. Therefore, the difference-in-differences between treatment and control groups is likely smaller than the full effect on enrollment of each policy.

At the program level, programs passing under GE rules that are offered at a college with a non-passing program may experience a completions decline as the college limits general admissions or closes. I repeat the DD estimation with these passing programs as an additional treatment group. I find a completions decline nearly as large as that felt by non-passing programs. This response is mainly driven by the closure of colleges where these passing programs are offered, furthering my interpretation that passing programs experienced a spillover effect when offered at colleges with non-passing programs.

A classic threat to the causal estimation of a treatment using DD is confounding trends in the post-treatment period. While I control for observable differences between treatment and control groups that may cause differential post-treatment enrollment growth, there may be additional unobservable confounding trends. For example, since exposure to the threat of sanctions depends on student outcomes, one might be concerned that a college with poor graduate outcomes may gain a reputation over time for such outcomes, and thus increasingly struggle to recruit new students. In the case of the GAO report, the existence of an alternative college may be correlated with other enrollment trends independent of the GAO report. I find parallel pre-treatment trends between each pair of treatment and control groups, providing evidence of no such differential trends prior to the treatments and suggesting that no confounding trends occur in the post-treatment period.

To further assess whether the existence of an alternative college is correlated with confounding trends following the GAO report, I estimate the DD using public colleges with and without local not-for-profit alternatives as the treatment and control groups. One would expect no treatment effect at public colleges, as no negative details regarding public college recruiting or costs were revealed in the GAO report. I find no enrollment decline at public colleges with an alternative following the GAO report, suggesting these confounding trends are not a concern. I repeat this analysis using private non-profit colleges and find a small

enrollment decline following the GAO report. This is consistent with the GAO report also causing private students to look for less expensive public colleges due to the high costs of private colleges discussed in the report.

I estimate the aggregate contribution of these two pieces of federal oversight to the total enrollment decline of the for-profit sector. For this calculation, I assume that in the absence of each treatment, treated colleges would have experienced the enrollment trends of the untreated control colleges. Using the average enrollment of the treated colleges as well as the number of such colleges, I find that these policies account for 135% of the total for-profit enrollment decline between 2010 and 2015. This implies that for-profit enrollment would have likely continued to grow in the absence of the GAO report and GE program.

My results establish that the federal government has powerful tools to regulate the for-profit higher education sector. In a market with imperfect consumer information, it might be unsurprising that government provided information changes consumer behavior. However, given the nature of the decision to attend college and the efforts of some for-profits to mislead students, the magnitude of this response is less obvious. I show that information provided by the government can substantially alter student attendance decisions. This considerable response to the GAO report may be difficult to replicate, however, as media coverage may have played a critical role in spreading the details of the report. Furthermore, I demonstrate that the mere threat of sanctions is enough to cause massive enrollment shifts in the for-profit sector. While both policies resulted in significant enrollment declines, the GE program caused a substantially larger loss of students at the worst performing for-profits.

The remainder of the paper is structured as follows. In Section 2, I describe recent college enrollment trends and detail the GAO report and GE program. I describe the data used in Section 3 and the empirical approach to estimating each effect in Section 4. In Sections 5 and 6, I detail the identification of treatment and control colleges for the information and threatened sanctions effects, respectively, and present the results of each estimation. In Section 7 I dicuss the aggregate results, and in Section 8 I conclude.

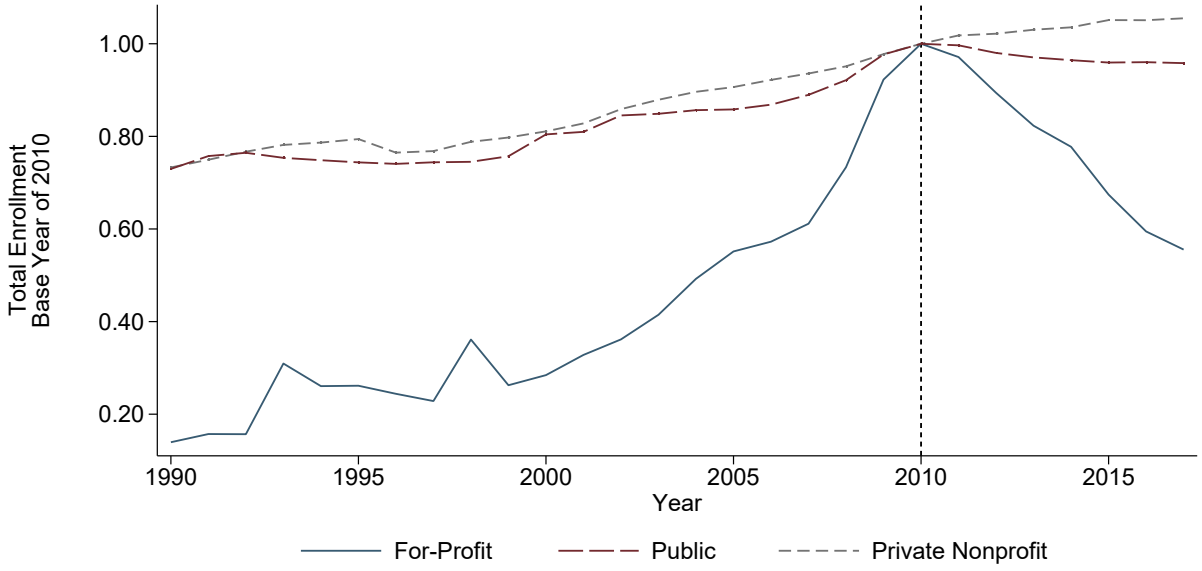
2 Background

College enrollment in the United States increased from less than 15 million students in 1995 to over 21 million students in 2010.[6] As shown in Figure 1, all three higher education sectors experienced enrollment growth during these years, however the for-profit sector more than doubled in size. By 2010, the share of college students enrolled at for-profit institutions reached a high of 11%.

College enrollment grew most rapidly in the years immediately preceding this peak. Between 2007 and 2010, public and for-profit enrollment increased by 1,650,000 and 830,000 students, respectively. Many changes to the higher education market occured during this period. The Great Recession lowered the opportunity cost of attending college as labor market conditions worsened. Prior to the onset of the recession, the U.S. government raised the maximum federal student loan caps and eliminated the ability to default on private student loans. Both of these policies increased the amount of funds available for students to

[6]Enrollment numbers are for colleges participating in Title IV federal student aid programs only.

Figure 1: College Enrollment by Sector, 1990-2016



Source: IPEDS *Note:* The vertical line indicates the academic year of the GAO report and GE proposal.

borrow and thus expanded access to higher education. Additionally, the number of for-profit colleges rose from 2,732 in 2007 to 3,194 in 2010. In contrast, there were only 18 new public colleges and 3 new nonprofit colleges over the same period.

Enrollment has declined since 2010, with 1 million fewer students attending college by 2015. While student losses occured at both for-profit and public colleges, the for-profit sector lost more than a third of enrollment, returning to 2006 enrollment levels. In contrast, public college enrollment declined by 3.9% between 2010 and 2013, and has remained near a total enrollment of 14.6 million students since then. Private nonprofit colleges saw no such enrollment decline over this time. As a result of these trends, the share of college students in the for-profit sector has fallen to less than 6%.

The large decline of the for-profit sector was preceded by an increase in federal oversight. The Government Accountability Office report on for-profit colleges was released on August 3rd, 2010, prior to the first academic year of enrollment declines at for-profit colleges. The report detailed the findings of an undercover investigation into 15 for-profit colleges in the US.[7] Government agents, posing as prospective students, applied to these colleges and recorded the efforts of recruiters to mislead them about the costs and outcomes of attending the for-profit. The report claimed that in some cases agents were pressured to commit fraud when applying for federal student aid and forced to sign documents prior to receiving cost information.[8] The GAO report also compared tuition costs for a degree or certificate offered

[7]The 15 for-profit colleges investigated were not named in the report, however they were revealed in the subsequent Senate HELP Committee hearing.

[8]In November of 2010 a revised report was released by the GAO. This follow-up report amended some examples of misleading recruiting practices to be less severe than initially reported. The findings of the report remained largely unchanged, including the relative costs and outcomes of graduates at for-profit

at for-profit colleges to the cost for the same degree at not-for-profit competitors. At 14 of the 15 for-profits investigated, tuition was more expensive than at a nearby public alternative, regardless of program. In one example provided by the report, a web page design certificate was found to cost $21,250 at a for-profit college, $4,750 at a nearby private not-for-profit college, and only $2,037 at a nearby public college (GAO (2010)).

The report was followed the next day by a Senate Health, Education, Labor, & Pensions Committee hearing on the findings. The release and hearing were covered in national news, with stories featured within 2 days of release in the Washington Post and New York Times, as well as coverage by NPR, Rueters, CNBC, CNN, and the Huffington Post. This national coverage described the efforts to mislead applicants about the poor outcomes graduates face and the high costs of attendance. Some outlets, such as the New York Times, repeated the findings of for-profit costs relative to nearby alternative public and private not-for-profit colleges. Additionally, as shown in Figure 17 of Appendix C, Google searches for for-profit colleges spiked the day the report was released, and remained high relative to earlier search frequencies in the years after the report. This widespread coverage of for-profit practices and costs may have provided current and prospective students with new information about for-profit colleges.

In July of 2010, the Department of Education under the Obama Administration announced the Gainful Employment (GE) program. Under GE rules, a for-profit college program that fails to provide graduates with gainful employment loses access to federal loan assistance for students. The Department measures gainful employment using annual student loan debt payment-to-annual earnings ratios for the cohort of students who completed the program two years prior to calculation. A passing ratio is below 8% for annual earnings or below 20% for discretionary income, with the lower of the two ratios used for the final GE status. A ratio above 12% for annual earnings or 30% for discretionary income places a program into the failing category, while ratios between passing and failing are given a "zone" or warning grade. Sanctions are imposed on programs with a failing status for two out of three consecutive years.

The Department of Education released a preliminary set of program GE grades in July of 2012. The status was calculated for all for-profit programs eligibile for federal aid during the 2011 academic year, excluding programs with 30 or fewer completions. Upon enactment of the GE rules in 2014, failing and warning programs would be at risk of losing the main source of for-profit revenue if average graduate debt-to-earnings ratios were not improved. One avenue to reduce these ratios is to limit enrollment, particularly in poorly performing programs, and devote more resources to fewer students. Another option is to eliminate non-passing programs entirely. Furthermore, students may avoid attending these non-passing colleges following the public release of GE statuses. These responses may occur following the public release of the preliminary program grades in 20120, however for-profit colleges may have also acted on private information following the program announcement in 2010.

colleges (GAO (2010)).

3 Data

The main data I use is a panel of college enrollment and completions provided by the National Center for Education Statistics Integrated Postsecondary Education Data System (IPEDS). The data set is a collection of surveys from all postsecondary institutions participating in federal financial student aid programs each year. The surveys provide college characteristics such as location, program length, selectivity, enrollment, and tuition. The series used in this paper are available beginning in the 2002-2003 academic year. Of the 3,194 for-profit colleges responding to the survey for the 2010-2011 academic year, I am able to determine exposure to the GAO report and GE program for 2,259 colleges.

The Completions survey of IPEDS provides the number of completions at the program level. For each college program, the number of Bachelor, Associate and Professional degrees earned in each academic year are reported, as well as the number of non-degree completions. The field of a program is identified using 6-digit Classification of Instruction Programs (CIP) codes. At the 2-digit level, these programs are categorized into general fields such as health, business, and education. I use these data to match for-profit colleges with not-for-profit alternative colleges that offer similar programs, as well as to perform a program-level analysis of the completions response to the GAO report and the threat of sanctions. At the 6-digit program level, I am able to determine the alternative status of 12,110 for-profit programs during the 2010-2011 academic year.

As detailed in Section 5.1, I use the commuting zone each college is located in to find local alternatives to each for-profit college and program. I match institutions to commuting zones using a 1990 county-to-commuting zone crosswalk provided by Autor and Dorn (2013).

To find colleges and programs at risk of federal student aid sanctions, I use the 2012 preliminary Gainful Employment report. This report provides the 2010-2011 academic year debt-to-earnings ratios and the corresponding GE status for each program offered by a for-profit college. The Department found 544 for-profit programs failing to meet the debt-to-earnings ratio requirements, 886 programs in the warning category, and 3,638 programs passing. Debt-to-earnings ratios were not calculated for the programs with fewer than 31 students, leaving 7,597 for-profit programs without a status.

Lastly, I obtain employment data from the Quarterly Census of Employment and Wages (QCEW) published by the Bureau of Labor Statistics. The QCEW provides quarterly and annual employment and wage data at the county, MSA, state, and national level. I merge the state level annual employment data to the IPEDS data to control for local labor market conditions when estimating the enrollment and completions responses.

4 Methods

I employ a differences-in-differences framework to estimate the causal effects of both the GAO report and the threat of sanctions on for-profit enrollment and completions. Each of these effects is identified as the difference in enrollment between treatment and control groups before and after each treatment. For the GAO report, for-profits in the treatment group are those with a nearby not-for-profit alternative. For-profits threatened with sanctions are those with a non-passing GE status in the preliminary GE report. I jointly estimate the

effect of these two piece of oversight, as discussed in this section. I provide further detail on each identification strategy separately in Sections 5.1 and 6.1.

I estimate the difference-in-differences using a two-way fixed effects model. This is a generalization of the classic two-period, two-group difference-in-differences design, allowing for the estimation of each effect over multiple lead and lag periods, as well as for controls for time-variant covariates. This provides a natural way to test for parallel pre-treatment trends, an identifying assumption discussed further below, and to observe the long-term enrollment and completions response to each treatment. The model also allows for differing intensities of the treatment and responses to the treatment across multiple treatment groups.

I estimate the following equation:

$$y_{i,t} = \alpha_i + \lambda_t + \delta_t^{GAO} D_i^{GAO} + \delta_t^{GE} D_i^{GE} + X_{i,t}\beta + \epsilon_{i,t} \tag{1}$$

where $y_{i,t}$ is log enrollment at college i at time t, D_i^{GAO} and D_i^{GE} are indicators for belonging to the treatment groups for each policy, α_i and λ_t are college and time fixed effects, and $X_{i,t}$ is a set of covariates over time.[9] When estimating the response of completions at the program level, $y_{i,t}$ is log completions at program i and α_i are program fixed effects. The coefficients of interest are δ_t^{GAO} and δ_t^{GE}. For any period t, the difference in enrollment between treatment and control group from t to the last pre-treatment year τ is $\gamma_t = \delta_\tau - \delta_t$.[10]

For the estimated effect to be a causal response to the treatment, the treatment and control colleges must exhibit common post-treatment trends in the absence of the treatment (Kahn-Lang and Lang (2018); Angrist and Krueger (1999); Angrist and Pischke (2010)). Let $y_{i,t}^0$ be the outcome for college i in the untreated state, then the common trend assumption states that

$$E(y_{i,T+1}^0|D_i = 1) - E(y_{i,T}^0|D_i = 1) = E(y_{i,T+1}^0|D_i = 0) - E(y_{i,T}^0|D_i = 0) \tag{2}$$

for post-treatment period T. This common trends assumption implies that there are no confounding trends between the treatment and control group in the post-treatment period, and therefore the estimated outcome trend differences between treatment and control colleges during the post-treatment period are due to the treatment. While these counterfactual post-treatment trends are unobservable, parallel pre-treatment trends are suggestive of common counterfactual post-treatment trends. I estimate the difference in enrollment trends between treatment and control groups for all available years of data prior to the treatment to test for parallel pre-trends. However if there is a linear trend in the pre-periods, then despite rejecting the null of non-parallel pre-trends, the linear trend may bias the post-treatment estimates. To control for such a trend, I estimate an additional specification

$$y_{i,t} = \alpha_i + \lambda_t + \delta_t^{GAO} D_i^{GAO} + \delta_t^{GE} D_i^{GE} + X_{i,t}\beta + tD^{GAO} + tD^{GE} + \epsilon_{i,t} \tag{3}$$

with the group-specific linear time trend controls tD^{GAO} and $+tD^{GE}$.

[9] I estimate the response of log enrollment instead of levels, assuming that the response of enrollment in levels varies with college-size, while the percentage response is consistent across college-size. Treatment group indicators do not vary over time, as the treatment group is determined by exposure at the time of treatment. See Sections 5.1 and 6.1 for further detail.

[10] In Appendix **??** I interact the treatment indicators for each piece of oversight to assess whether there was a greater response when treated by both the GAO report and the GE program than by either individually.

However, parallel pre-treatment trends are neither sufficient nor necessary for the common trends assumption to hold (Kahn-Lang and Lang (2018)). There may be additional confounding trends in the post-treatment period that are not observed pre-treatment. To control for such confounding trends related to observable differences between groups, I include time-varying covariate controls, denoted by $X_{i,t}$ in Equation 3 (Donald and Lang (2007); Card and Krueger (1994); Bertrand et al. (2004)). The common trends assumption becomes

$$\begin{aligned} E(y^0_{i,T+1}|X = x, D_i = 1) - E(y^0_{i,T}|X = x, D_i = 1) = \\ E(y^0_{i,T+1}|X = x, D_i = 0) - E(y^0_{i,T}|X = x, D_i = 0) \end{aligned} \tag{4}$$

The covariates I control for are cost of attendance, location type, typical program length, and current and one-year lagged state employment growth.[11] In Sections 5.1 and 6.1, I summarize these observable differences between treatment and control colleges for each identification strategy in detail.

An additional identification assumption is that the treatment of one unit does not affect the outcome of another unit. For this to hold, the untreated college must not experience a change in enrollment due to the treated colleges receiving the treatment. I discuss the feasibility of this assumption for each identification stratgey below.

5 The Effect of the GAO Report

5.1 Methods

For-profit colleges and programs are differentially exposed to the GAO report through current and prospective students' ability to attend a local not-for-profit substitute. The GAO report and subsequent news coverage provide details to current and prospective students on the misleading recruiting practices some for-profits use and the high costs of attending many for-profit colleges. In response, students may look for and attend a less-expensive alternative college option outside the for-profit sector. This student response to the GAO report lowers enrollment and completions at for-profits with an available not-for-profit alternative. Such a response does not occur at for-profits without an alternative, leaving these colleges and programs far less exposed to the GAO report.

A not-for-profit college serves as an alternative to a for-profit college if it is nearby, offers similar programs, and is less expensive and less selective than the for-profit college. Consider two prominent for-profit colleges with campuses in Madison, WI: ITT Technical College and the University of Phoenix. For-profit students often attend college locally and may not be able to travel far or relocate to attend an alternative.[12] Thus, following the release of the GAO report, students of these two for-profits may consider attending not-for-profits in the Madison commuting zone. There are three not-for-profit colleges in this commuting zone: the

[11] Location types are large city, midsize city, small city, suburbs, and towns or rural. When estimating program level effects, typical program length is replaced with degree level.

[12] For-profit colleges typically offer shorter programs, often with a vocational focus, that would require non-local students to relocate for only a short period of time. In 2010, 80% of all for-profit colleges offered 2-year or shorter programs and only 3% offered on-campus housing. Furthermore, none of the for-profits in the 2010 IPEDS survey offered varying tuition rates for in-district and out-of-district students.

University of Wisconsin-Madison, Edgewood College, and Madison Area Technical College (MATC). The cost per year of earning an Associate's degree in computer information at the non-selective ITT Tech is $17,000. At the similarly non-selective MATC, an Associate's degree in computer information costs only $3,300 per year. Students attending or considering attending ITT Tech for such a degree could switch to MATC and save over 80% of the price of tuition. However, the University of Phoenix offers only 4-year Bachelor's degrees, which are not offered at MATC. The typical student interested in a Bachelor's degree may not consider an Associate's degree to be an acceptable substitute, therfore MATC does not serve as an alternative to the University of Phoenix. Both UW-Madison and Edgeworth College offer 4-year business degrees similar to those at the University of Phoenix, however UW-Madison is a more selective college and Edgeworth is more expensive, ruling these two colleges out as viable options for a typical University of Phoenix students to attend. The University of Phoenix is therefore less exposed to the GAO report than ITT Tech.

I exploit this college-level variation in exposure to estimate a causal enrollment response to the GAO report. In the difference-in-differences framework, for-profits with a local not-for-profit alternative serve as the treatment group, while for-profits without alternatives serve as the control group. The IPEDS data allow for estimation of both the enrollment response to the GAO report at the college level and the completions response at the 6-digit CIP code program and degree level. I conduct my analysis at both levels using for-profit colleges with a not-for-profit alternative college and for-profit programs with a not-for-profit alternative program. I consider programs in the same 2-digit CIP code field and at the same degree level to be similar programs. For example, this allows all 2-year nursing degrees to serve as alternatives to other nursing degrees, as these are all common to the health 2-digit code.[13] The causal enrollment response is the difference in enrollment between these groups before and after the 2010 release of the GAO report.

This response serves as a lower bound of the full for-profit enrollment and completions response to the GAO report, since I capture only the differential response between treatment and control colleges. Along with students forgoing for-profits for not-for-profit alternatives, there may also be students who enter the labor market instead of attending a for-profit following the GAO report. Such a student response might occur at both for-profits with and without an alternative, thus both treatment and control groups experience an enrollment decline from these students entering the labor market. If this enrollment decline is independent of the existence of an alternative, it occurs equally between the groups. This portion of the student response to the GAO report is then fully differenced out in the DD framework.[14]

One might be concerned that observable differences between treatment and control groups are not independent of post-treatment enrollment trends. For instance, as shown in Table 1, the treatment colleges are on average larger and more often located in large cities. Enrollment at larger for-profit colleges or in big cities may have started to decline post-2010 for reasons

[13]Matching programs at the 6-digit and 4-digit levels is too strict, as programs such as Registered Nursing and Licensed Nursing are separated at these levels. I assume students would consider such degrees substitutes if offered at the same degree level.

[14]I control for observable differences between the groups which may also cause more students to leave for the labor market, such as tuition price. If, conditional on these observables, this response is not independent of the existence of an alternative, my causal estimate may either be smaller than the full effect of having an alternative post-GAO report or will capture some of this response in the estimate.

independent of the GAO report, biasing the estimate causal response. Selection into the treatment is also a concern, as for-profits in large cities are more likely to have an alternative and thus be exposed to the GAO report. To remove the impact of such potential confounding trends, I control for the observable differences between treatment and control groups over time with covariate trend controls. I discuss these differences in detail below. As mentioned in Section 4, the college covariates I control for are cost of attendance, location type, and typical program length. Additionally, I control for current and lagged local employment growth at the state level to remove the effect of local labor market conditions on enrollment.

One might also be concerned there are trends correlated with having a not-for-profit alternative that are independent of the GAO report. To determine whether such confounding trends exist, I estimate the difference in enrollment between public colleges with a not-for-profit alternative before and after the GAO report.[15] Since the GAO report did not reveal public colleges to be expensive or to mislead when recruiting students, one would expect no difference in enrollment trends between public colleges with and without alternatives post-2010. I conduct a similar analysis using private nonprofit colleges, however the GAO may have also caused a decline at these colleges. While not the main target of the investigation, examples of private nonprofits being more expensive to attend than public colleges were discussed in the report. In Appendix B, I provide a difference-in-difference-in-differences analysis using these not-for-profit colleges.

A final concern is that the control colleges and programs experience a spillover effect from the treatment colleges being treated. This might occur if students leave for-profits with alternatives and attend for-profits without alternatives instead. The control colleges experience a gain in enrollment following the treatment that the treatment colleges would not have experienced in the absence of the treatment. This biases the size of the enrollment response I estimate to be larger than the true response. While I cannot rule out a spillover effect, such an effect would require for-profit students to switch to other for-profits upon receiving the negative news regarding for-profits. Additionally, as shown with the example of ITT Tech and the University of Phoenix in Madison, the control colleges often offer programs not offered at a nearby not-for-profit alternative and therefore not offered at treatment for-profits. Thus the control colleges are often not alternatives to the treatment colleges.

I exclude both online and religious-focus schools when determining whether a for-profit has an alternative. The location of an online college is unobserved, thus a nearby alternative college cannot be identified. For the same reason, online colleges are also not able to serve as local alternatives. My results therefore do not capture enrollment declines at online for-profits caused by the GAO report. Furthermore, students may leave both treatment and control for-profits for online not-for-profit alternatives, which may bias my estimates. I define a college as an online institution if it is classified as distance-only in IPEDS or if more than 50% of students are listed as enrolled in online courses and not residing in the same jurisdication as the college. For religious-focus colleges, a student may attend or avoid these colleges due to the specific denomination of the school, and a nearby non-religious or religious college of a different denomination might therefore not be a viable alternative.

The treatment and control colleges differ across multiple characteristics. I summarize for-profit colleges with and without not-for-profit alternatives and their differences in Table

[15] I control for the same set of covariates as in the main specification.

1.[16] At the release of the GAO report in 2010, I am able to determine whether a for-profit has an alternative for 2,259 colleges out of the 3,194 federal aid eligible for-profits.[17] Of this sample, 90% had a not-for-profit alternative. For-profits with an alternative were larger colleges, with both a greater number of students enrolled and a higher number of programs offered. I find that for-profits with an alternative were more expensive to attend, costing on average $3,000 more annually than the control colleges. The typical degree length offered was similar across groups, however 12% of treatment for-profits were classified as 4-year colleges, compared to only 3% of the control for-profits. Under GE rules, 28% of for-profits with an alternative offered a non-passing program in the preliminary GE report, while only 15% of those without an alternative offered a non-passing. Lastly, I find that for-profits with alternatives were more often found in large cities and less often in rural areas than those without alternatives.

The 6-digit program-level treatment and control groups are similarly summarized in Table 2. As with colleges, over 90% of for-profit programs had a not-for-profit alternative in 2010, with an average of 1.6 not-for-profit alternative programs. The treated programs were larger, with 41 completions compared to 26 completions at control programs. Less than 15% of each group of programs were listed as either warning or failing in the preliminary GE report. While over half of each group were non-degree granting programs, a larger share of treated programs were associate degree granting, while the control group had a larger share of bachelor degree programs.

I plot the average of for-profit log enrollment and log completions over time by treatment group in Figure 2 to visually compare trends before and after the 2010 GAO report. The GAO report release date of August 4th, 2010 occured at the beginning of the 2010 academic year, and thus too late in the application and admission cycle for students to react and attend alternative colleges. The 2011-2012 academic year is the first year one would expect an impact on enrollment from new information. In the first panel of Figure 2, I compare trends between treatment and control groups at the college level. While both treatment and control colleges experienced enrollment losses post-2010, enrollment at for-profit colleges with an alternative began to decline 2011 while enrollment at those without alternatives did not decline until 2013. In the second panel I plot completion trends at the program-level. Completions at treatment programs declined at a faster pace than control programs beginning in 2011 and continuing through 2016. I find evidence of parallel pre-trends between 2002 and 2010 in both plots, however the treatment colleges experienced faster enrollment growth from 2007-2008. These raw trends suggest both that a decline from new information occurred and that the common trends assumption is satisfied.

5.2 Results

I plot the estimated causal response to the GAO report relative to 2010 in Figure 3 and present the coefficients in Table 3. The first panel of the figure shows the college enrollment response, corresponding to the first column of Table 3. In 2011, for-profit colleges with

[16] I further examine the distributions over time of the outcome variables and continuous covariates in Appendix A.

[17] The existence of a not-for-profit alternative could not be identified for the remaining for-profit colleges due to missing location or cost data.

Table 1: 2010 For-Profit College Characteristics by Information Treatment Group

	For-Profit Colleges with a Not-For-Profit Alternative	For-Profit Colleges without a Not-For-Profit Alternative
Mean College Characteristics		
Enrollment	476	202
	(801)	(404)
Total Cost of Attendance ($)	13,423	10,424
	(5,344)	(4,618)
2-Digit CIP Programs Offered	4.2	2.0
	(4.7)	(2.5)
6-Digit CIP Programs Offered	10.8	5.2
	(11.9)	(7.2)
with an Alternative	9.3	
	(9.7)	
Number of Colleges		
Overall	2,044	215
Share by GE Status (%)		
Passing	72	85
Warning	17	9
Failing	11	6
Share by Selectivity (%)		
Inclusive	97	100
Selective	3	
Share by Program Length (%)		
4-year	12	3
2-year	34	34
<2-year	54	63
Share by Location Type (%)		
Large City	28	11
Midsize City	12	6
Small City	13	26
Suburb	36	21
Town/Rural	11	36

Note: Data are from IPEDS and the 2012 preliminary Gainful Employment report. The sample of for-profit colleges are those for which the existence of a not-for-profit alternative could be determined and the Gainful Employment status could be identified, as detailed in Section 6.1

.

Table 2: 2010 For-Profit Program Characteristics by Information Treatment Group

	For-Profit Programs with a Not-For-Profit Alternative	For-Profit Programs without a Not-For-Profit Alternative
Average Completions	41	25
	(76)	(56)
Average Number of Alternatives	1.6	
	(3.4)	
Number of Programs		
Overall	11,084	1,026
Share by GE Status (%)		
Passing	89	88
Warning	7	6
Failing	4	7
Share by Selectivity (%)		
Inclusive	94	96
Selective	6	4
Share by Degree (%)		
Bachelor	10	30
Associate	32	16
Non-Degree	58	54
Share by Location Type (%)		
Large City	29	20
Midsize City	13	13
Small City	13	23
Suburb	35	15
Town/Rural	10	29

Note: Data are from IPEDS and the 2012 preliminary Gainful Employment report. The sample of for-profit programs are those for which the existence of a not-for-profit alternative could be determined and the Gainful Employment status could be identified, as detailed in Section 6.1

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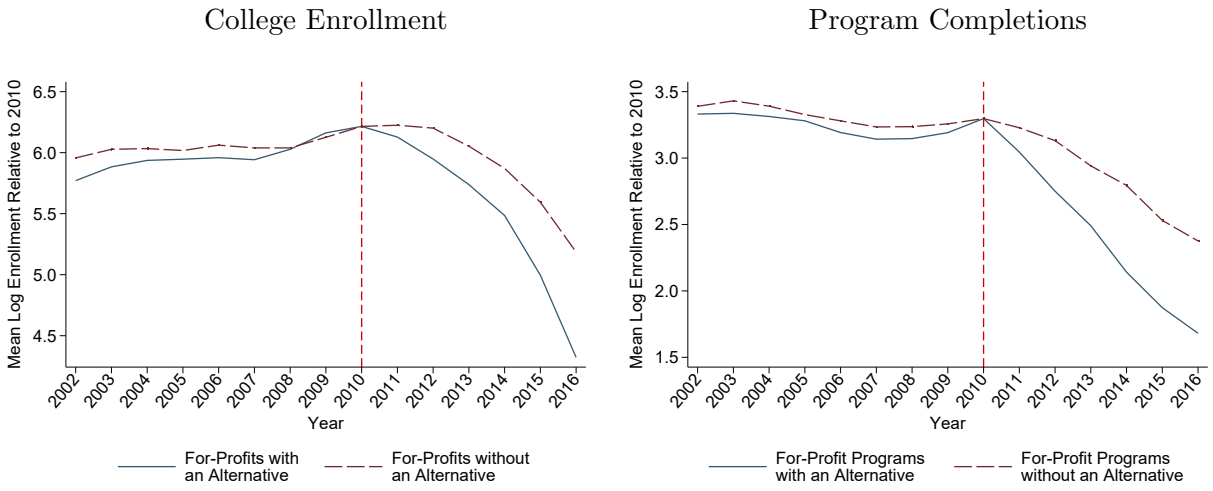
Figure 2: For-Profit Growth Before and After the GAO Report



Note: The vertical line denotes the academic year the GAO report was released. The log trends are demeaned relative to 2010 and with college and program fixed effects removed. The first panel shows enrollment trends at the college level, while the second panel shows completions trends at the 6-digit program level.

alternatives experienced a 8.1% enrollment decline from the GAO report, or an average of 37 students per treated college. This decline grew to 21.2% per college in 2012, with a total decline of 44.6% by 2015. I find no statistically significant differences in pre-treatment trends betweeen treatment and control colleges, with the exception of 2006-2007. These results hold when adding group-specific linear trends, as shown in the second column of Table 3, with slightly larger causal enrollment responses over time.

In the second panel of Figure 3, I plot the program-level completions DD. I find that for-profit programs with a not-for-profit alternative experienced a 13.7% completions decline in 2011, for an average of 5.5 fewer completions. A completions response in the same year as the initial enrollment decline is not surprising, since less-than-two-year programs account for 60% of treated programs. By 2014, I find that the GAO report caused a total drop in completions of 38.3%. I find no evidence of continued completions declines post-2014. The differences in pre-treatment trends are not statistically significantly different from zero for all pre-treatment years and the results hold with linear trend controls, again suggesting the common trends assumptions is satisfied by the treatment and control groups.

While the immediate response of program-level completions in 2011 is greater than that of college-level enrollment, the enrollment declines are greater post-2012. A larger enrollment than completions response suggests the students leaving the treatment for-profits were less likely to graduate than those who stayed. To further examine whether for-profit completions and enrollment responded differently to GAO report, I estimate the response of aggregate completions at the college-level and present the results in Figure 18 and Table 13 of Appendix C. I find similar results, with evidence of a smaller decline in completions through 2015.

One might also expect the program-level response to differ from the college-level response conditional on the share of students in treated programs per college. If the typical for-profit with an alternative college has a low share of students exposed to an alternative program, one would see a much greater decline at the program-level than the college-level. However,

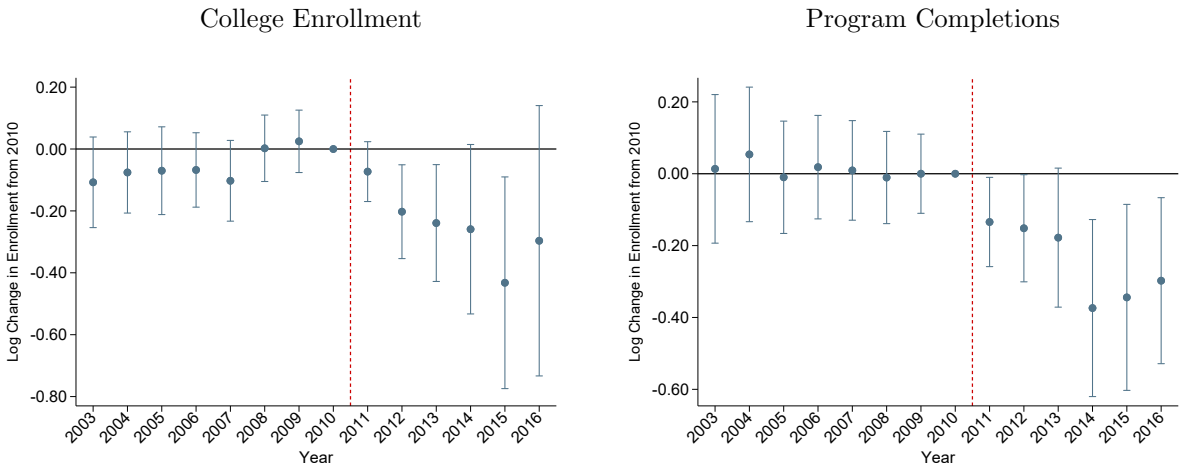
Table 3: Effect of the GAO Report on For-Profit Colleges and Programs with Alternatives

	College Enrollment		Program Completions	
	(1)	(2)	(3)	(4)
δ_t - δ_{2010}				
2005	-0.070	-0.004	-0.010	-0.025
	(0.055)	(0.041)	(0.060)	(0.055)
2006	-0.068	-0.016	0.018	0.002
	(0.046)	(0.036)	(0.056)	(0.050)
2007	-0.103**	-0.066	0.009	-0.005
	(0.050)	(0.042)	(0.053)	(0.052)
2008	0.002	0.022	-0.011	-0.021
	(0.041)	(0.038)	(0.050)	(0.045)
2009	0.025	0.037	0.000	-0.005
	(0.039)	(0.037)	(0.043)	(0.043)
2011	-0.073*	-0.096**	-0.134***	-0.135***
	(0.037)	(0.040)	(0.048)	(0.051)
2012	-0.203***	-0.243***	-0.152***	-0.154**
	(0.059)	(0.063)	(0.058)	(0.064)
2013	-0.239***	-0.296***	-0.178**	-0.178**
	(0.073)	(0.076)	(0.075)	(0.087)
2014	-0.259**	-0.332***	-0.374***	-0.375***
	(0.106)	(0.107)	(0.095)	(0.114)
2015	-0.432***	-0.523***	-0.344***	-0.343***
	(0.132)	(0.134)	(0.100)	(0.124)
College FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Covariate Trends	Yes	Yes	Yes	Yes
Linear Trend	No	Yes	No	Yes
Observations	28,353	28,353	133,912	133,912
R^2	0.205	0.205	0.200	0.200

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses.

Note: Estimates of the effect of the GAO report over time are the difference in log trends between for-profits with and without alternatives, estimated using Equation 3. The covariate trends controlled for are GE status, cost of attendance, location type, program length, and state-level employment growth.

Figure 3: Effect of the GAO Report on For-Profit Colleges and Programs with Alternatives



Note: The plotted coefficients are the difference-in-differences between treatment and control colleges and programs and between each year and 2010, estimated using Equation 3. The coefficients are presented in Table 3. Vertical bars denote 99% confidence intervals. The red dashed line denotes the timing of the treatment.

of for-profits with an alternative, 89% had a 100% share of completions in treated programs. Of those with less than a 100% share, the average share of completions in treated programs was 80%. Thus, most students at treated colleges were also in treated programs, and the size of responses at the program and college level should be similar. This is consistent with the estimated effects shown in Table 3, as the college-level and program-level coefficients lie within 90% confidence intervals.

I examine whether the enrollment and completion responses occur along the intensive margin or due to college closures. I reestimate Equation 3, this time using only for-profit colleges that remain open through 2016. The estimates are presented in Table 4 and Figure 4. I find a 6.2% decline in enrollment in 2011, 2 percentage point smaller than the effect when including closing colleges. However by 2015, I find a total enrollment decline of only 14.3%, nearly 30 percentage points smaller than the baseline estimate. At the program level, I find a 10.9% completions decline in 2011, which grows to a 25.0% decline by 2015. These results suggest the initial enrollment decline occured mainly at the intensive margin as students left for-profit colleges. However, nearly three-quarters of the later enrollment decline occurred due to college closures. At the program level, completion declines are similar to the baseline results when excluding college closures. As with the basline estimates, these results are robust to a group-specific linear time trend.

The effects of having a not-for-profit alternative post-2010 on public and private non-profit colleges and programs are presented in Figure 5 and Table 5. In the left two panels, I plot the effect on public colleges and programs. I find no post-treatment trend differences for public colleges, providing evidence of no effect of having an alternative independent of the GAO report. At the program-level however, I find evidence of a small increase in completions at public programs. As public programs with an alternative are more likely to be located in a large city, they may be more likely to be an alternative. This small increase in completions may be due to for-profit students switching into these programs.

Table 4: Intensive Margin Effect of the GAO Report

	College Enrollment		Program Completions	
	(1)	(2)	(3)	(4)
δ_t - δ_{2010}				
2005	-0.057	0.007	-0.074	-0.061
	(0.056)	(0.045)	(0.059)	(0.059)
2006	-0.064	-0.013	-0.046	-0.035
	(0.046)	(0.036)	(0.052)	(0.053)
2007	-0.029	0.009	-0.041	-0.032
	(0.050)	(0.042)	(0.052)	(0.054)
2008	0.006	0.029	-0.052	-0.046
	(0.043)	(0.041)	(0.051)	(0.048)
2009	0.038	0.048	-0.021	-0.019
	(0.039)	(0.038)	(0.040)	(0.042)
2011	-0.061**	-0.075**	-0.110**	-0.110**
	(0.029)	(0.031)	(0.048)	(0.051)
2012	-0.105**	-0.135***	-0.116**	-0.118*
	(0.041)	(0.046)	(0.052)	(0.060)
2013	-0.070	-0.112**	-0.152***	-0.154**
	(0.045)	(0.053)	(0.054)	(0.068)
2014	-0.131***	-0.188***	-0.283***	-0.287***
	(0.045)	(0.059)	(0.061)	(0.084)
2015	-0.139**	-0.210***	-0.254***	-0.259***
	(0.054)	(0.070)	(0.062)	(0.094)
College FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Covariate Trends	Yes	Yes	Yes	Yes
Linear Trend	No	Yes	No	Yes
Observations	17,287	17,287	106,053	106,053
R^2	0.109	0.109	0.123	0.123

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses.

Note: Estimates of the effect of the GAO report over time are the difference in log trends between for-profits with and without alternatives, estimated using Equation 3, now excluding colleges that close. The covariate trends controlled for are GE status, cost of attendance, location type, program length, and state-level employment growth.

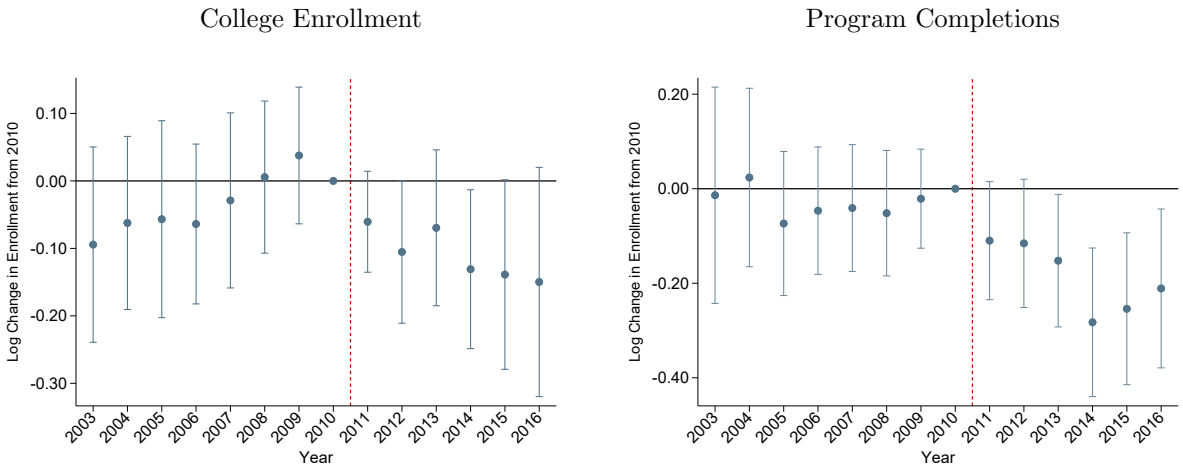
Table 5: Effect of the GAO Report on Public and Private Nonprofit Colleges and Programs

	College Enrollment		Program Completions	
	(1) Public	(2) Private	(3) Public	(4) Private
δ_t - δ_{2010}				
2005	-0.032**	0.042	-0.016	-0.006
	(0.015)	(0.029)	(0.013)	(0.017)
2006	-0.020*	0.023	-0.012	0.002
	(0.012)	(0.023)	(0.012)	(0.015)
2007	-0.016	0.019	-0.019*	0.007
	(0.010)	(0.021)	(0.011)	(0.014)
2008	0.000	0.008	-0.008	0.009
	(0.010)	(0.017)	(0.011)	(0.014)
2009	0.005	0.008	-0.000	0.020*
	(0.007)	(0.013)	(0.009)	(0.011)
2011	0.005	-0.025	0.006	-0.013
	(0.013)	(0.018)	(0.012)	(0.013)
2012	-0.007	-0.102**	0.035**	-0.033*
	(0.010)	(0.041)	(0.015)	(0.019)
2013	-0.016	-0.090*	0.034*	-0.026
	(0.013)	(0.054)	(0.019)	(0.021)
2014	-0.027*	-0.097	0.052**	-0.047**
	(0.014)	(0.073)	(0.025)	(0.022)
2015	-0.015	-0.157**	0.063**	-0.051**
	(0.016)	(0.078)	(0.029)	(0.023)
College FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Covariate Trends	Yes	Yes	Yes	Yes
Observations	24,433	16,955	1,247,133	593,712
R^2	0.071	0.052	0.011	0.022

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses.

Note: Estimates of the effect of the GAO report over time are the difference in log trends between treatment and control colleges and programs, estimated using Equation 3. The covariate trends controlled for are cost of attendance, location type, program length, college size, lagged enrollment and completions growth, and state-level employment growth.

Figure 4: Intensive Margin Effect of the GAO Report



Note: The plotted coefficients are the estimated difference-in-differences between treatment and control colleges and programs and between each year and 2010, estimated using Equation 3. These coefficients are presented in Table 4. Vertical bars denote 99% confidence intervals. The red dashed line denotes the timing of the treatment.

In the right two panels of Figure 5, I plot the results for private nonprofit colleges and programs. I find evidence of small enrollment and completions declines in this college sector. Colleges with not-for-profit alternatives experienced a 15.3% greater decline that those without alternatives by 2015. At the program level, programs with alternatives experienced a 5.1% decline over this period. As discussed in Section 5.1, the private nonprofit college sector may have been negatively impacted by the GAO report, since public alternatives were often highlighted as the least expensive option available. These results are consistent with a small causal response to the GAO report at private nonprofit colleges and programs.

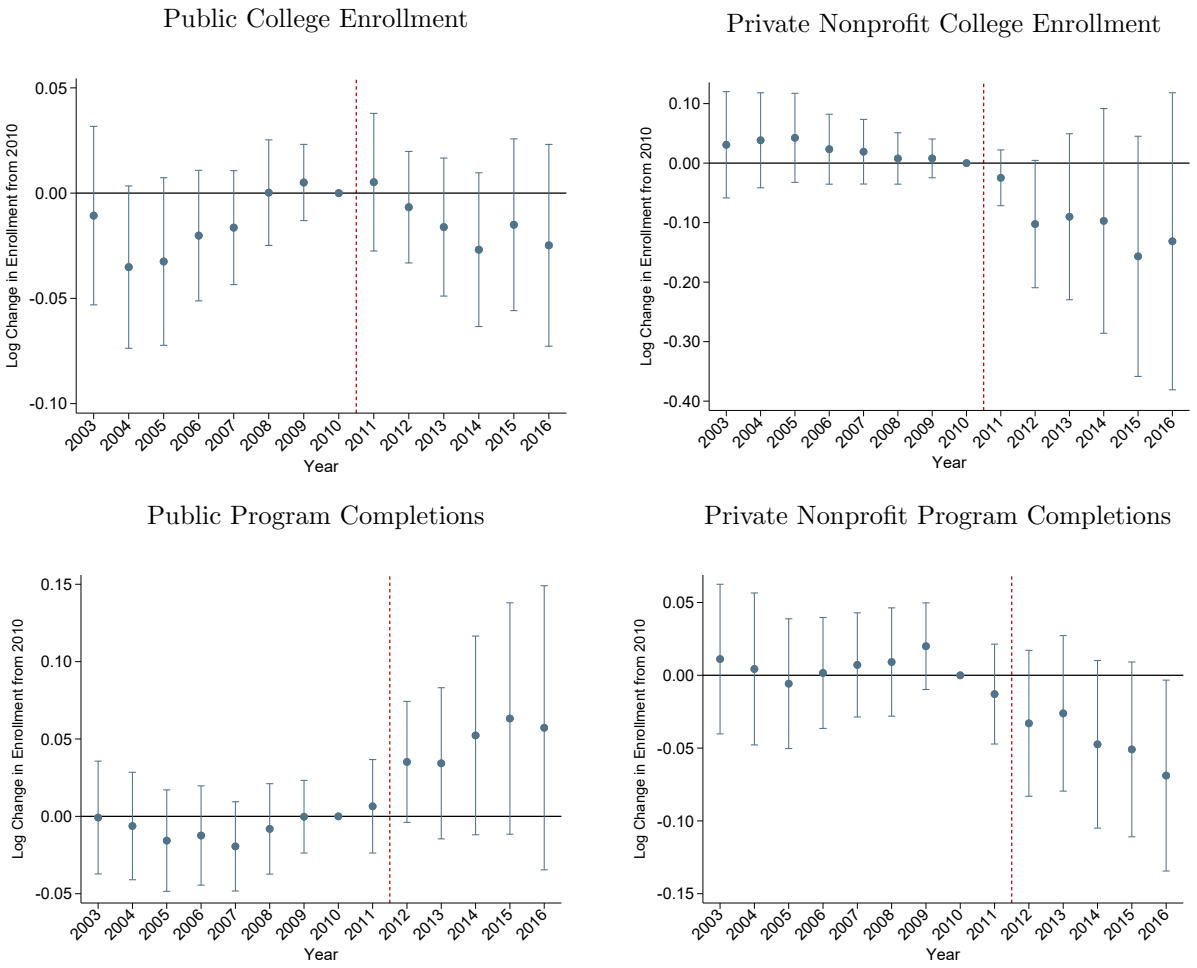
6 The Effect of Threatened Sanctions

6.1 Methods

For-profit colleges and programs with a high average graduate debt-to-earnings ratio are threatened with federal student aid sanctions. In the 2012 preliminary Gainful Employment report, for-profit programs were given a grade based on these ratios. Colleges found to have a non-passing program in this report would have to improve graduate outcomes in that program to avoid the loss of federal student aid when the GE program is enacted. To do so, colleges could take actions such as more selectively admitting students, reducing enrollment numbers in struggling programs, or eliminating these programs altogether. These actions result in lower enrollment and fewer completions in the for-profit sector. Additionally, since the preliminary GE grades and debt-to-earning ratios were released publicly, students may have also responded to the GE program by avoiding for-profits threatened with sanctions.

To estimate the for-profit college enrollment and completions response to the threat of federal student aid sanctions, I establish college and program-level variation in exposure to

Figure 5: Effect of the GAO Report on Not-For-Profit Colleges and Programs



Note: The plotted coefficients are the estimated difference-in-differences between treatment and control colleges and programs and between each year and 2010, estimated using Equation 3. These coefficients are presented in Table 5. Vertical bars denote 99% confidence intervals. The red dashed line denotes the timing of the treatment.

the threat of sanctions using the 2012 preliminary GE grades. The preliminary GE report determines grades at the 6-digit program and degree level. As detailed in Section 2, for-profit programs were given a passing, warning, or failing grade. To measure exposure at the college-level, I assign GE status based on the worst performing program offered at the college. This grading system provides a natural measure of treatment intensity. While both failing and warning colleges and programs may need to take action to avoid sanctions, failing for-profits need to improve outcomes the most and therefore may take the strongest action to limit enrollment. In the difference-in-differences framework, colleges and programs with warning or failing grades serve as the treatment groups, while those with a passing grade serve as the control group.[18] I estimate the difference in enrollment and completions between these groups before and after the release of preliminary grades in 2012.

I estimate an additional DD specification using treatment intensity as measured by the reported average debt-to-earnings ratios. Using the discrete GE grades as the treatment intensity assumes that colleges and programs just above and just below the failing or warning thresholds respond differently to the threat of sanctions. However, one might expect programs near the thresholds to respond similarly, regardless of grade, to lessen the risk of losing student aid in future years. For-profits with debt-to-earnings ratios well above the failing threshold may also be likely to take stronger actions than those just above the cutoff. Using the debt-to-earnings ratios as the measure of treatment intensity allows for variation in responses within grades.[19]

Since exposure to the threat of sanctions is based on average student outcomes, one might be concerned that poorly performing for-profits experience different enrollment trends than passing for-profits independent of the GE program. These trends would bias the estimated causal response to the threat of sanctions. For example, a college with poor outcomes may gain a negative reputation over time, leading to enrollment losses as students avoid this college. I test whether such confounding trends were occuring prior to the treatment by comparing pre-treatment trends between the for-profit groups. Parallel pre-trends are evidence that these confounding trends were not occuring prior to the GE program. While this does not preclude the existence of confounding trends in the post-treatment period, any such trends would have to begin simultaneously or shortly after the GE grades are released.

An additional potential concern is that control colleges and programs may have been impacted by the threat of sanctions. Despite a passing grade, colleges may have limited admissions or eliminated programs as precautionary measures to avoid being sanctioned. The threat of these sanctions may also have constrained growth at these colleges, as the risk of poor graduate outcomes might increase after opening new programs or expanding class sizes. The control colleges and programs would therefore experience an enrollment decline or slower enrollment growth that the treated colleges would not experience in the absence of the treatment. My results would therefore be an underestimate of the full for-profit response to the GE program. However, there might also be positive spillovers at control colleges if enrollment shifts from the non-passing for-profits to the passing for-profits. Such

[18]The programs with fewer than 31 completions that were not graded and were not at risk of losing federal student aid during the early years of the GE program. In my primary analysis, I consider these programs to be passing and not threatened by sanctions.

[19]However, since these ratios are not calculated for programs with fewer than 31 completions in 2011, this further limits the sample of programs and colleges.

enrollment shifts would cause my DD specification to overestimate the causal response to the GE program.

I am able to assess whether a spillover effect occurs at passing programs offered at a college with a non-passing program. I construct a second treatment group consisting of these programs and reestimate the DD. These programs may experience an increase in completions if students shift from the non-passing programs to passing programs at the same school, due to either actions taken by the college or decisions made by students. However, these programs might also experience a completions decline if the college limits general admissions, closes, or if students avoid all programs at the college.

I present summary statistics of passing, warning, and failing for-profit colleges as of 2010 in Table 6.[20] Of the 2,259 for-profit colleges for which the existence of an alternative could also be determined, I find that 513 had a non-passing program in the 2011 GE report, with 245 of those offering a failing program. Failing colleges had an average of 2.7 failing programs and 2.2 warning programs, while warning colleges had an average of 1.4 warning programs. The average debt-to-earnings ratio across the assigned college-level grades are similar to the official program-level thresholds. The average ratio at a failing college was 11.8%, just below the 12% threshold for an individual program to be failing. Similarly, the average ratio was 7.4% at warning colleges, slightly below the 8% warning threshold for programs. The 4% average ratio at passing colleges was well within the passing category.

Failing and warning colleges were larger, more expensive, and longer in typical program length than passing colleges. Average enrollment was 911 at failing colleges and 628 at warning colleges, compared to 341 students at passing colleges. Total average costs were similar across the two treated groups at nearly $15,000 per year, while passing colleges were $2,500 less expensive to attend. Nearly half of failing colleges offered mainly 2-year programs and 36% offered 4-year programs. A similar share of warning colleges were 2-year colleges, however 40% of warning colleges typically offered less-than-2-year programs. In contrast, almost two-thirds of passing colleges were less-than-2-year schools, with only 30% classified as 2-year colleges and only 6% as 4-year colleges. There were observable similarities across these three groups, such as location type, selectivity of admissions, and in the existence of a not-for-profit alternative.

I summarize the 2010 characteristics of 6-digit program treatment and control groups in Table 7. There were 538 failing for-profit programs, 838 warning programs, and 10,734 passing programs in 2010. In each group, close to 90% of programs had a not-for-profit alternative. Programs were also similar in size across statuses. The differences in selectivity, location type, and degree-type mirror those found at the college level.

In Figure 6, I plot the average log enrollment over the treatment period for failing, warning, and passing is the first for-profit colleges and programs. Since the preliminary grades are released in July of 2012, the 2013-2014 academic year is the first post-treatment period. With the grades released only a month before the start of the 2012-2013 academic year, one would not expect colleges to have had enough time to adjust recruiting and admissions practices or students to have been able to react to the grades. The left panel of Figure 6 shows that for-profit colleges across all GE statuses experience an enrollment decline post-2012.

[20] I further examine the distributions over time of the outcome variables and continuous covariates in Appendix A.

Table 6: 2010 For-Profit College Characteristics by Gainful Employment Status

	GE Failing College	GE Warning College	GE Passing College
Mean College Characteristics			
Enrollment	911	628	341
	(1,350)	(622)	(647)
Debt-to-Annual Earnings Ratio (%)	11.8	7.4	4.0
	(4.3)	(2.0)	(2.1)
Total Cost of Attendance ($)	15,364	14,898	12,412
	(4,917)	(4,916)	(5,326)
Programs per College	8.4	5.4	3.1
	(5.7)	(5.0)	(3.7)
Warning Programs per College	2.2	1.4	
	(2.6)	(0.8)	
Failing Programs per College	2.7		
	(2.3)		
Number of Colleges			
Overall	245	368	1646
Share by Alternative Status (%)			
No Alternative	5	5	11
Has Alternative	95	95	89
Share by Selectivity (%)			
Inclusive	91	98	98
Selective	9	2	2
Share by Program Length (%)			
4-year	36	16	6
2-year	47	44	30
<2-year	17	40	64
Share by Location Type (%)			
Large City	33	30	25
Midsize City	11	10	12
Small City	15	12	15
Suburb	31	35	35
Town/Rural	10	13	14

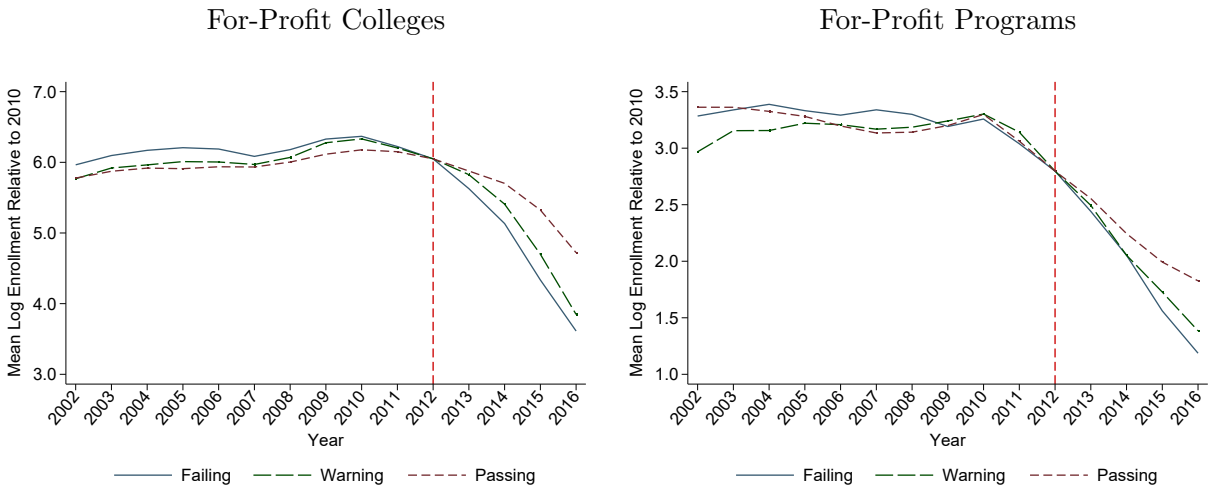
Note: Data are from IPEDS and the 2012 preliminary Gainful Employment report. The sample of for-profit colleges are those for which the Gainful Employment status could be identified and the existence of a not-for-profit alternative could be determined, as detailed in Section 5.1

Table 7: 2010 For-Profit Program Characteristics by Gainful Employment Status

	GE Failing For-Profit	GE Warning For-Profit	GE Passing For-Profit
Average Completions	41	42	39
	(66)	(69)	(75)
Debt-to-Annual Earnings Ratio (%)	15.3	9.9	4.7
	(2.9)	(1.3)	(2.3)
Number of Programs			
Overall	538	838	10,734
Share by Alternative Status (%)			
No Alternative	13	7	8
Has Alternative	87	93	92
Share by Selectivity (%)			
Inclusive	90	92	94
Selective	10	8	6
Share by Degree (%)			
Bachelor	27	13	11
Associate	48	41	29
Non-Degree	25	46	60
Share by Location Type (%)			
Large City	35	30	28
Midsize City	11	11	13
Small City	17	14	14
Suburb	29	35	33
Town/Rural	8	10	12

Note: Data are from IPEDS and the 2012 preliminary Gainful Employment report. The sample of for-profit programs are those for which the Gainful Employment status could be identified and the existence of a not-for-profit alternative could be determined, as detailed in Section 5.1

Figure 6: For-Profit Growth Before and After the Gainful Employment Preliminary Grades



Note: The vertical line denotes the academic year the Gainful Employment program was proposed. The log trends are demeaned relative to 2010 and with college and program fixed effects removed. The first panel shows enrollment trends at the college level, while the second panel shows completions trends at for-profit 6-digit programs.

The differences continue through 2016, consistent with the continuation of the GE program over this period. Additionally, these raw trends provide some evidence of a faster decline at failing colleges than at warning colleges. At the program level, I find evidence of a faster completions decline at failing and warning programs by 2014, continuing again through 2016.

6.2 Results

I report the causal response of for-profit enrollment and completions to the threat of sanctions in Table 8 and plot the coefficients in Figure 7. The left panel of Figure 7 shows the enrollment response by GE grade, corresponding to columns 1 and 3 of Table 8. Failing for-profit colleges experienced a 26.1% enrollment decline in 2013 from the threat of sanctions, while warning colleges experienced a smaller 12.8% decline. These effects grow over time, with warning and failing colleges experiencing a 76.4% and 122.3% greater enrollment decline than passing colleges by 2015. The larger response at failing for-profits than warning for-profits is consistent with failing colleges taking stronger action to limit enrollment and improve graduate outcomes. The difference in college pre-treatment trends are not statistically significantly different from zero with the exception of 2005-2006 for failing colleges. However, my results are robust to linear time trends, as shown in columns 2 and 4.

The right panel of Figure 7 shows the program-level completions response to threatened sanctions across GE statuses. I find a 13.9% decline at failing programs in 2013, however no statistically significant decline for warning programs until a 17.6% drop in 2014. Such a lag in completions declines relative to enrollment declines would occur if for-profits limited admissions in response to the the threat of sanctions while allowing currently enrolled students to finish their degrees. While there are some statistically significant trend differences pre-treatment, these results are robust to linear trend controls.

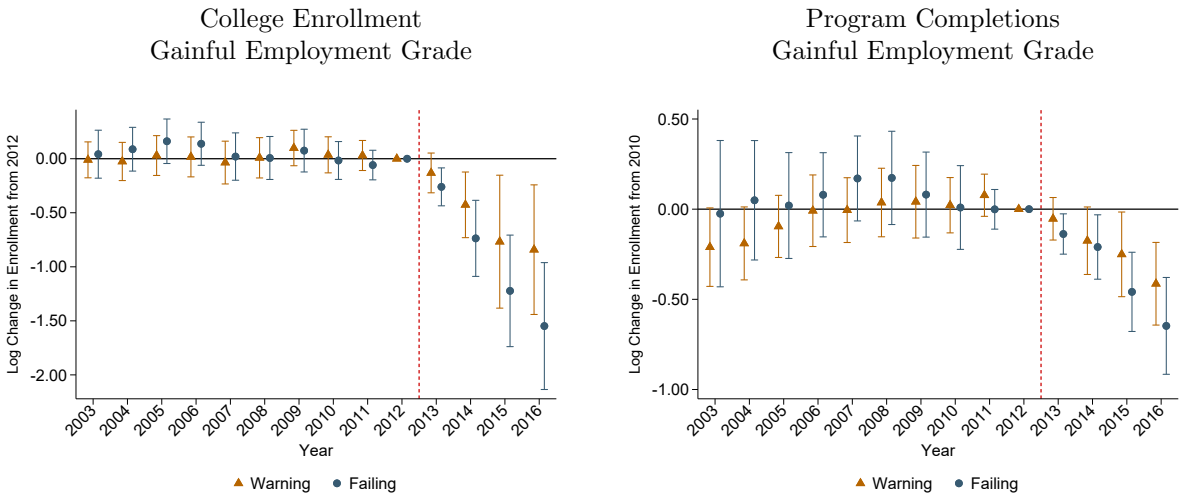
Table 8: Effect of the Gainful Employment Program on Failing and Warning For-Profits

	College Enrollment				Program Completions			
	(1) Warning	(2) Warning	(3) Failing	(4) Failing	(5) Warning	(6) Warning	(7) Failing	(8) Failing
δ_t - δ_{2012}								
2005	0.029	0.036	0.162**	0.125***	-0.096	0.067	0.020	0.035
	(0.071)	(0.036)	(0.080)	(0.046)	(0.067)	(0.048)	(0.113)	(0.079)
2006	0.017	0.024	0.138*	0.107**	-0.009	0.130**	0.080	0.090
	(0.071)	(0.042)	(0.077)	(0.046)	(0.077)	(0.065)	(0.090)	(0.094)
2007	-0.035	-0.026	0.020	-0.004	-0.005	0.111*	0.170*	0.179**
	(0.076)	(0.056)	(0.085)	(0.056)	(0.069)	(0.066)	(0.091)	(0.087)
2008	0.009	0.018	0.007	-0.010	0.037	0.129**	0.173*	0.181*
	(0.072)	(0.057)	(0.077)	(0.055)	(0.073)	(0.066)	(0.100)	(0.094)
2009	0.099	0.106**	0.075	0.064	0.041	0.109	0.081	0.086
	(0.063)	(0.051)	(0.076)	(0.062)	(0.078)	(0.073)	(0.091)	(0.085)
2010	0.036	0.041	-0.016	-0.022	0.022	0.067	0.009	0.015
	(0.064)	(0.056)	(0.068)	(0.057)	(0.059)	(0.055)	(0.090)	(0.085)
2011	0.030	0.032	-0.059	-0.061	0.077*	0.101**	-0.001	0.003
	(0.054)	(0.050)	(0.053)	(0.048)	(0.045)	(0.043)	(0.042)	(0.043)
2013	-0.131*	-0.129*	-0.260***	-0.256***	-0.053	-0.076	-0.138***	-0.141***
	(0.071)	(0.073)	(0.068)	(0.069)	(0.046)	(0.049)	(0.043)	(0.044)
2014	-0.427***	-0.425***	-0.737***	-0.726***	-0.175**	-0.222***	-0.210***	-0.217***
	(0.117)	(0.119)	(0.136)	(0.139)	(0.072)	(0.076)	(0.069)	(0.075)
2015	-0.767***	-0.767***	-1.223***	-1.207***	-0.251***	-0.321***	-0.459***	-0.468***
	(0.237)	(0.240)	(0.199)	(0.205)	(0.091)	(0.095)	(0.085)	(0.101)
College FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Covariate Trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Linear Trend	No	Yes	No	Yes	No	Yes	No	Yes
Observations	28,353	28,353	28,353	28,353	133,912	133,912	133,912	133,912
R^2	0.205	0.205	0.205	0.205	0.200	0.200	0.200	0.200

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses.

Note: Estimates of the threatened sanctions effect over time are the difference in log trends between non-passing and passing for-profits, estimated using Equation 3. The covariate trends controlled for are alternative-existence, cost of attendance, location type, program length, and state-level employment growth.

Figure 7: Effect of the Gainful Employment Program on Failing and Warning For-Profits



Note: The plotted coefficients are the estimated difference-in-differences between treatment and control colleges and programs and between each year and 2010, estimated using Equation 3. These coefficients are presented in Table 8. Vertical bars denote 99% confidence intervals. The red dashed line denotes the timing of the treatment.

Using the average graduate debt-to-earnings ratio as the measure of treatment intensity, I find a similar response to threatened sanctions as well as parallel pre-treatment trends. I plot the enrollment and completions response in Figure 8 and present the estimated coefficients in Table 9. Coefficients correspond to a 10% greater 2011 debt-to-earnings ratio. In 2013, there was a 4.7% enrollment decline at the college level. This grows to an 86.6% decline by 2015. At the program level, I find that completion declines began in 2011 instead of 2013, prior to the release of the GE preliminary grades. This is evidence of for-profits using private information on graduate outcomes to respond to the 2010 proposal of the GE sanctions. By 2015, programs with a higher debt-to-earnings ratio experienced a 51.9% greater decline in completions. Given the average debt-to-earning ratios across GE grades, these estimates are in line with the warning college estimates of Table 8, though smaller than the estimated response at failing colleges. Since using the debt-to-earnings ratio limits the sample to graded colleges and programs, I estimate the baseline DD excluding ungraded colleges and programs. As shown in Figure 20 of Appendix C, I find no substantial differences from the baseline results.

Across these specifications, I find a smaller response by 2015 of completions at the program level than of enrollment at the college level. This pattern holds when estimating the college-level response of aggregate completions, as shown in Table 14 and Figure 19 of Appendix C. The smaller completions response may be due to improving graduation rates as non-passing colleges admit fewer students. Assuming a for-profit college has knowledge of student types, a non-passing college attempting to improve graduate outcomes may limit admissions of those students most likely to struggle post-graduation. These students may also be the most likely to struggle in college, and therefore drop out at a higher percentage than other student types. Fewer drop outs results in a higher graduation rate and a larger enrollment than completions decline.

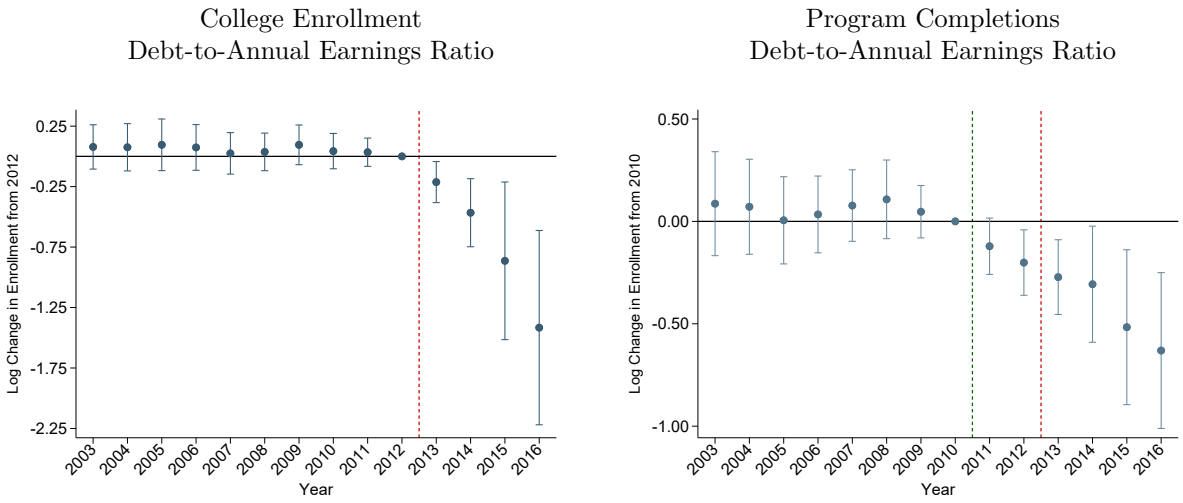
Table 9: Effect of the Gainful Employment Program on For-Profits by Treatment Intensity

	College Enrollment		Program Completions	
	(1)	(2)	(3)	(4)
δ_t - $\delta_{\text{omitted year}}$				
2005	0.095	0.034	0.005	-0.057
	(0.082)	(0.046)	(0.082)	(0.054)
2006	0.074	0.019	0.034	-0.021
	(0.073)	(0.043)	(0.072)	(0.057)
2007	0.025	-0.020	0.077	0.034
	(0.066)	(0.040)	(0.067)	(0.054)
2008	0.037	0.003	0.108	0.080
	(0.060)	(0.041)	(0.074)	(0.064)
2009	0.095	0.066	0.047	0.030
	(0.063)	(0.049)	(0.049)	(0.046)
2010	0.043	0.027		
	(0.056)	(0.047)		
2011	0.035	0.029	-0.121**	-0.103*
	(0.045)	(0.039)	(0.053)	(0.056)
2012			-0.201***	-0.173**
			(0.062)	(0.072)
2013	-0.213***	-0.205***	-0.272***	-0.235***
	(0.065)	(0.064)	(0.070)	(0.087)
2014	-0.466***	-0.450***	-0.307***	-0.260**
	(0.108)	(0.109)	(0.109)	(0.130)
2015	-0.864***	-0.840***	-0.517***	-0.456***
	(0.251)	(0.248)	(0.146)	(0.171)
College FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Covariate Trends	Yes	Yes	Yes	Yes
Linear Trend	No	Yes	No	Yes
Observations	21,237	21,237	56,662	56,662
R^2	0.215	0.216	0.251	0.251

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses.

Note: Estimates of the threatened sanctions effect over time are the difference in log trends across average graduate annual debt-to-earnings ratios, estimated using Equation 3. The covariate trends controlled for are alternative-existence, cost of attendance, location type, program length, college size, lagged enrollment and completions growth, and state-level employment growth.

Figure 8: Effect of the Gainful Employment Program on For-Profits by Treatment Intensity



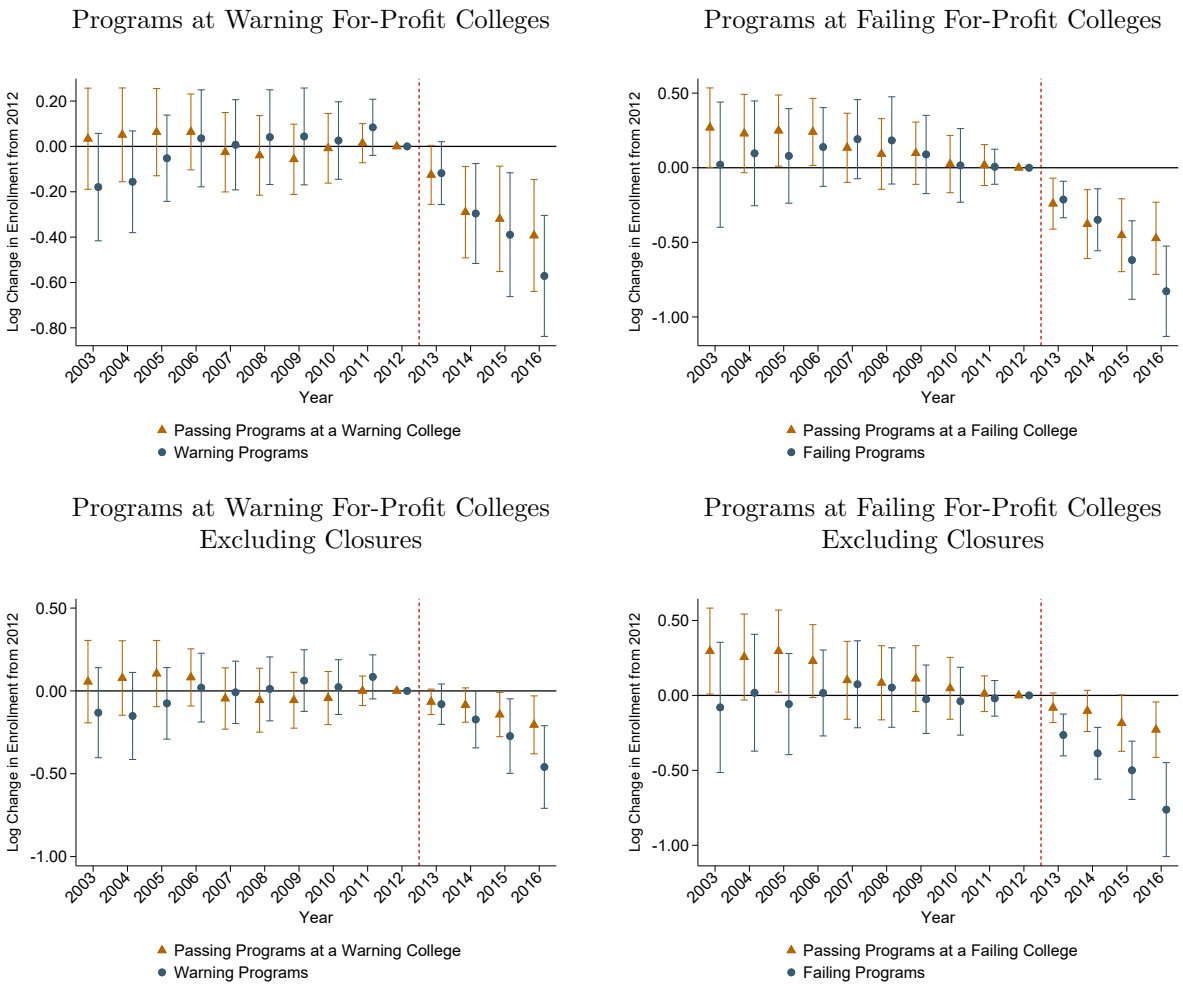
Note: Treatment intensity is the average graduate annual debt-to-earnings ratio. The plotted coefficients are the estimated difference-in-differences across treatment intensity and between each year and 2010, estimated using Equation 3. These coefficients are presented in Table 8. Vertical bars denote 99% confidence intervals. The red dashed line denotes the timing of the treatment.

I plot the completions response for passing programs at non-passing colleges in the top two panels of Figure 9 and present the coefficients in Table 10. I find evidence of a decline nearly as large as the effect on non-passing programs through 2014. In 2015 and 2016, non-passing programs begin to experience larger completions declines than these passing programs. To determine the source of these large responses, I reestimate this DD while excluding colleges that close post-GE proposal. These results are shown in the bottom two panels of Figure 9. The passing program declines are now less than half as large as before. This is evidence that a substantial share of these declines are attributable to college closures.

Lastly, I conduct a similar analysis by excluding closures to estimate the effect of the threat of sanctions at the intensive margin.[21] As shown in Figure 10 and Table 11, the 2013 enrollment decline at failing colleges is almost 20 percentage points smaller than the baseline estimation, at 7.9%. However, these results suggest a 2011 decline following the GE proposal, as these failing colleges experienced an 8% decline between 2010 and 2011. The 2013 enrollment decline at warning colleges is 12.8%, only 5 percentage points smaller than the baseline estimate. By 2015, warning and failing college enrollment declines by only 18.5% and 20.6%, substantially smaller than the baseline estimates. This is evidence that post-treatment college closures account for over three-fourths of the total enrollment decline at warning colleges, and over four-fifths of the decline at failing colleges. However, the program completion estimates are similar to the baseline estimates, suggesting a large completions decline along the intensive margin.

[21] As with the intensive margin estimation of the response to the GAO report, the sample for this estimation is different from the baseline.

Figure 9: Effect of the Gainful Employment Program on Passing Programs at Treated Colleges



Note: The plotted coefficients are the estimated difference-in-differences between treatment and control colleges and programs and between each year and 2010. These coefficients are presented in Table 8. Vertical bars denote 99% confidence intervals. The red dashed line denotes the timing of the treatment.

Table 10: Effect of the Gainful Employment Program on Passing Programs at Treated Colleges

	Full Sample		Excluding College Closures	
	(1) Warning College Spillovers	(2) Failing College Spillovers	(3) Warning College Spillovers	(4) Failing College Spillovers
δ_t - δ_{2012}				
2005	0.063	0.249***	0.105	0.296***
	(0.074)	(0.092)	(0.077)	(0.106)
2006	0.064	0.240***	0.082	0.229**
	(0.065)	(0.087)	(0.067)	(0.094)
2007	-0.026	0.134	-0.046	0.101
	(0.068)	(0.089)	(0.071)	(0.100)
2008	-0.040	0.092	-0.056	0.084
	(0.068)	(0.091)	(0.074)	(0.095)
2009	-0.057	0.097	-0.056	0.112
	(0.060)	(0.081)	(0.065)	(0.085)
2010	-0.008	0.025	-0.043	0.047
	(0.059)	(0.074)	(0.062)	(0.080)
2011	0.014	0.018	0.001	0.011
	(0.033)	(0.053)	(0.034)	(0.046)
2013	-0.125**	-0.241***	-0.066**	-0.083**
	(0.050)	(0.066)	(0.030)	(0.038)
2014	-0.290***	-0.378***	-0.085**	-0.103*
	(0.078)	(0.089)	(0.040)	(0.053)
2015	-0.319***	-0.452***	-0.143***	-0.185**
	(0.090)	(0.094)	(0.052)	(0.073)
College FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Covariate Trends	Yes	Yes	Yes	Yes
Observations	133,912	133,912	106,053	106,053
R^2	0.205	0.205	0.125	0.125

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses.

Note: Estimates of the threatened sanctions effect over time are the difference in log trends across average graduate annual debt-to-earnings ratios. The covariate trends controlled for are alternative-existence, cost of attendance, location type, program length, college size, lagged enrollment and completions growth, and state-level employment growth.

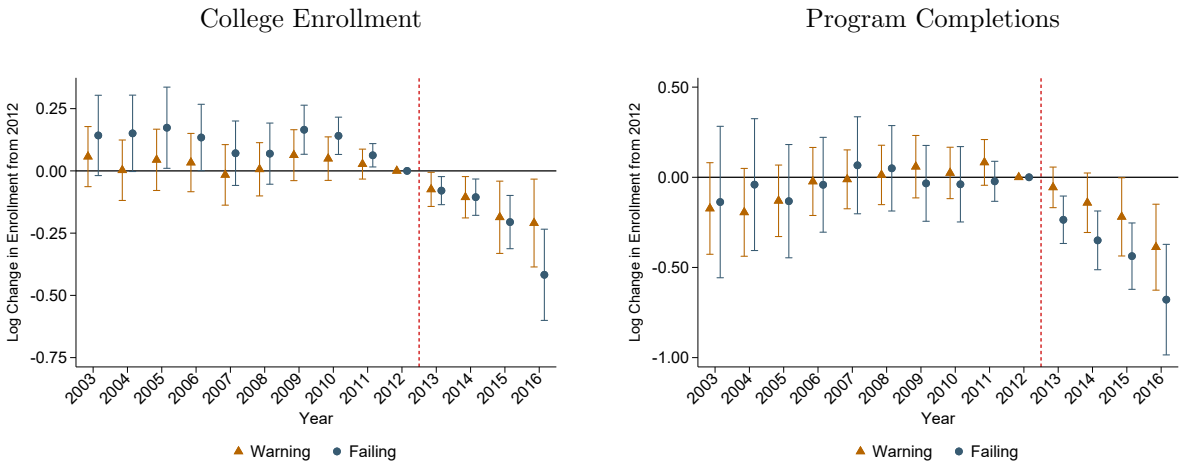
Table 11: Intensive Margin Effect of the Gainful Employment Program

	College Enrollment				Program Completions			
	(1) Warning	(2) Warning	(3) Failing	(4) Failing	(5) Warning	(6) Warning	(7) Failing	(8) Failing
δ_t - δ_{2012}								
2005	0.044	-0.000	0.173***	0.062	-0.130*	0.006	-0.132	-0.023
	(0.047)	(0.033)	(0.063)	(0.047)	(0.076)	(0.050)	(0.121)	(0.075)
2006	0.033	-0.005	0.134***	0.038	-0.023	0.094*	-0.042	0.052
	(0.045)	(0.037)	(0.051)	(0.038)	(0.073)	(0.052)	(0.101)	(0.080)
2007	-0.016	-0.047	0.071	-0.008	-0.011	0.086	0.067	0.144**
	(0.047)	(0.041)	(0.050)	(0.045)	(0.063)	(0.055)	(0.104)	(0.072)
2008	0.006	-0.017	0.069	0.006	0.013	0.091	0.050	0.111
	(0.041)	(0.036)	(0.047)	(0.043)	(0.064)	(0.058)	(0.091)	(0.080)
2009	0.063	0.046	0.165***	0.118***	0.059	0.117*	-0.034	0.011
	(0.039)	(0.035)	(0.038)	(0.038)	(0.067)	(0.063)	(0.081)	(0.072)
2010	0.049	0.038	0.141***	0.110***	0.024	0.062	-0.039	-0.009
	(0.034)	(0.030)	(0.029)	(0.029)	(0.055)	(0.049)	(0.081)	(0.076)
2011	0.027	0.023	0.063***	0.048**	0.083*	0.102**	-0.022	-0.007
	(0.023)	(0.022)	(0.018)	(0.019)	(0.049)	(0.045)	(0.043)	(0.044)
2013	-0.074***	-0.066**	-0.079***	-0.063***	-0.056	-0.075	-0.236***	-0.250***
	(0.026)	(0.028)	(0.022)	(0.022)	(0.043)	(0.047)	(0.051)	(0.050)
2014	-0.106***	-0.091**	-0.106***	-0.074**	-0.141**	-0.179**	-0.350***	-0.379***
	(0.032)	(0.036)	(0.028)	(0.030)	(0.064)	(0.073)	(0.063)	(0.065)
2015	-0.186***	-0.165***	-0.206***	-0.158***	-0.220***	-0.275***	-0.437***	-0.480***
	(0.056)	(0.062)	(0.041)	(0.044)	(0.084)	(0.093)	(0.071)	(0.076)
College FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Covariate Trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Linear Trend	No	Yes	No	Yes	No	Yes	No	Yes
Observations	22,337	22,337	22,337	22,337	106,053	106,053	106,053	106,053
R^2	0.108	0.108	0.108	0.108	0.123	0.123	0.123	0.123

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses.

Note: Estimates of the threatened sanctions effect over time are the difference in log trends between non-passing and passing for-profits. The covariate trends controlled for are alternative-existence, cost of attendance, location type, program length, college size, lagged enrollment and completions growth, and state-level employment growth.

Figure 10: Intensive Margin Effect of the Gainful Employment Program



Note: The plotted coefficients are the estimated difference-in-differences between treatment and control colleges and programs and between each year and 2010. These coefficients are presented in Table 8. Vertical bars denote 99% confidence intervals. The red dashed line denotes the timing of the treatment.

7 Aggregate Impact of the Combined Effects

Both the GAO report and the threat of federal student aid sanctions caused significant for-profit enrollment and completion declines. I conduct back-of-the-envelope calculations of the aggregate enrollment responses to determine the contribution of this federal oversight to the overall enrollment decline in the for-profit sector. I assume that in the absence of the treatment, treatment colleges would have experienced the enrollment trends experienced by the control colleges. I aggregate the causal effects using the number and average size of treatment colleges for each piece of federal oversight.

With an 8.1% enrollment decline per for-profit with a not-for-profit alternative, the GAO report resulted in an overall loss of 80,000 for-profit students in 2011. By 2012 new information from the GAO report is responsible for a loss of 210,000 students, and nearly 450,000 students by 2015. This accounts for 65% of the total enrollment decline experienced by the for-profit sector between 2010 and 2015. The 26.1% decline at failing colleges and the 12.8% decline at warning colleges in 2013 from the threat of sanctions account for 90,000 fewer students enrolled in the for-profit college sector in that year. By 2015, the aggregate enrollment loss from the threat of sanctions rises to 465,000 students, accounting for nearly 70% of the total for-profit decline.

While these are rough estimates of the aggregate effect, they suggest that the GAO report and GE proposal together can account for 135% of the for-profit enrollment losses post-2010. In the absence of both of these effects, the share of college students attending for-profits may have continued to expand through 2015.

8 Conclusion

The for-profit college sector experienced a large enrollment decline beginning in 2010, losing 33% of 2010 enrollment by 2015. This followed a period of rapid enrollment growth in the for-profit sector, which often provides worse outcomes to graduates than colleges in the not-for-profit sectors. I shed light on the causes of this enrollment decline, identifying both an effect of a GAO report and of the threat of federal student aid sanctions on for-profit enrollment. Using data from the Department of Education, I establish differential college- and program-level exposure to each policy and estimate enrollment and completion responses in a generalized difference-in-differences framework.

I find that both the GAO report and the threat of sanctions caused substantial enrollment and completion declines at for-profit colleges. The GAO report on for-profit college practices and costs caused fewer students to attend for-profits with a local not-for-profit alternative. This led to a 45% drop in enrollment at such for-profit colleges between 2010 and 2015. The threat of federal student aid sanctions decreased enrollment by 120% over the same period at for-profits with high graduate debt-to-earnings ratios, either due to poorly performing for-profits limiting enrollment or students avoiding these colleges.

On the aggregate level, I find both pieces of oversight were significant contributors to the overall for-profit enrollment decline. The enrollment response to the GAO report accounted for as much as 65% of the enrollment decline at for-profits between 2010 and 2015, while the threat of sanctions accounted for 70% of this decline. In the absence of both effects, for-profit enrollment may have continued to expand through the 2015 academic year.

My results demonstrate the considerable influence government oversight has on for-profit college enrollment. These policies are powerful tools at the disposal of policymakers interested in limiting enrollment at costly or under-performing for-profit colleges. Given the recent cancellation of the sanctions program, an interesting question going forward is whether the reversal of these policies causes a reversal of the for-profit enrollment declines. Furthermore, it remains to be seen where for-profit students went and the outcomes they faced following these pieces of oversight. Such an analysis would open the door to studying the welfare effects of these government policies.

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Appendices

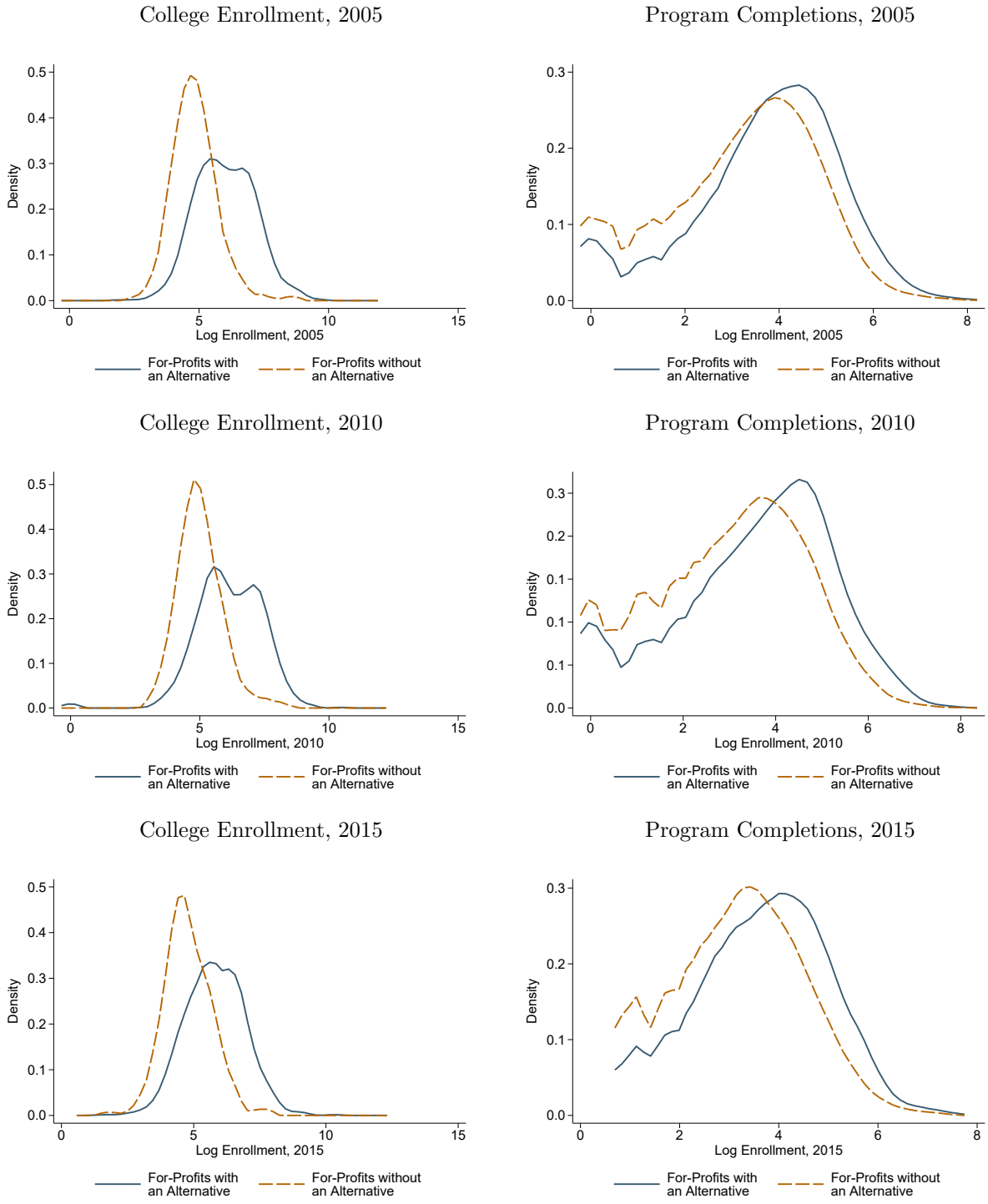
Appendix A Outcome and Covariate Balance Analysis

I compare the distributions of enrollment and completions over time at for-profit programs and colleges, as well as the distributions of the cost of attendance and local employment growth. In Figure 11, I plot the distribution over time of log enrollment and completions by exposure to the GAO report, as determined by the availability of a nearby not-for-profit alternative. As shown in the figure and discussed in Section 5.1, for-profits with an alternative have a larger mean and variance of enrollment and completions. However, there is a substantial region of common support between the groups. At the program level, the treatment programs are again larger on average than the control programs, however the region of common support is larger and the difference in variances is smaller. The distributions do not change substantially over time. These results hold for the threat of sanctions treatment groups as well, as shown in Figure 12.

I compare the distributions of the cost of attendance and local employment over time in Figures 13 and 14. These are the two non-categorical variables I control for in the main DD estimation. The treatment groups for both pieces of oversight are more expensive on average at both the college and program level. The median cost of attendance across all groups is increasing over time, however the growth is lower between 2010 and 2015 than between 2005 and 2010. Annual employment growth is similar between both sets of treatment and control groups over time.

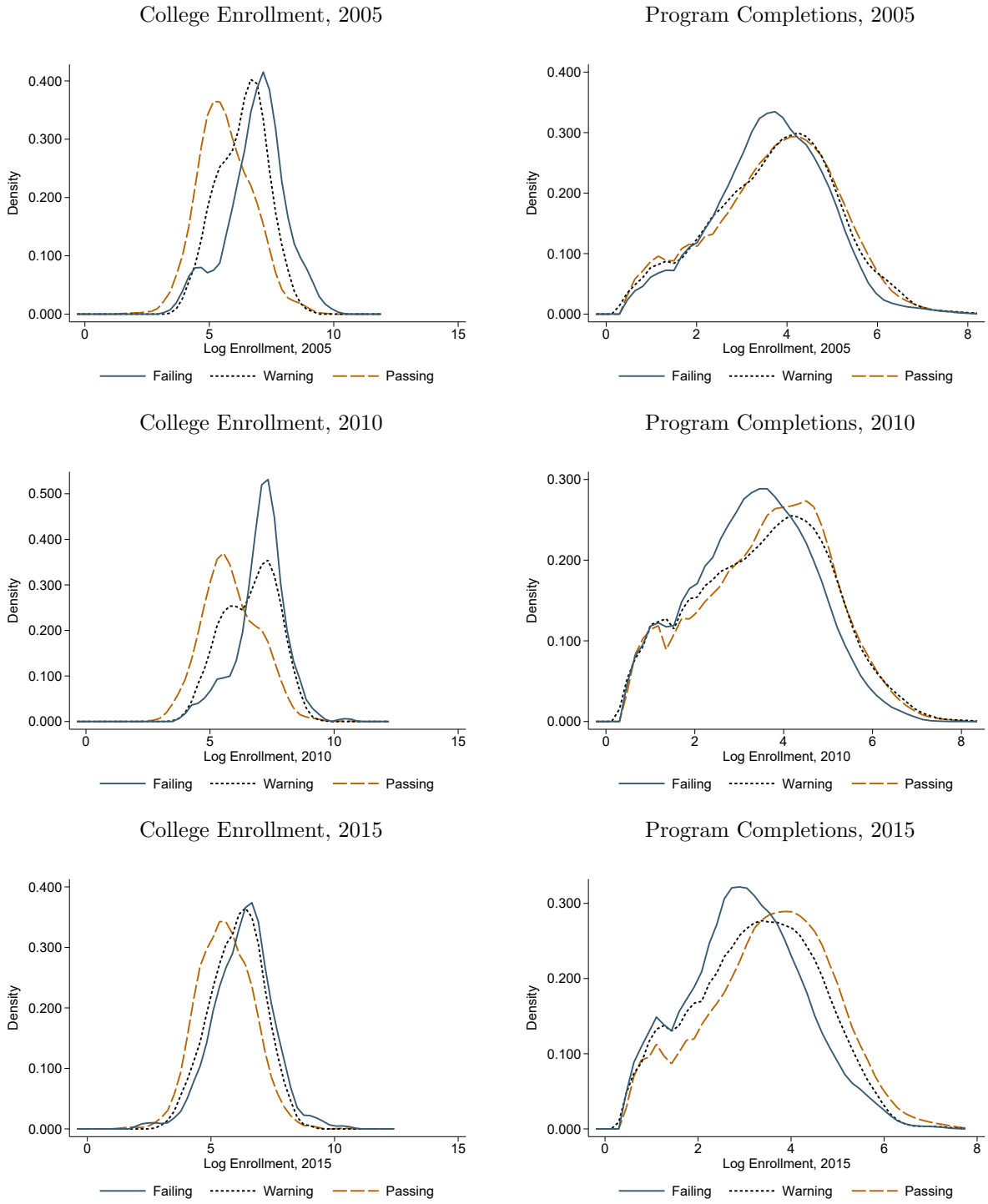
As an additional test of whether unobserved trends are confounding post-treatment differences, I estimate the DD of the cost of attendance between treatment and control groups for each treatment. I estimate Equation 3 with the annual cost of attendance now serving as the outcome of interest, $y_{i,t}$. The differences in coefficients relative to the last pre-treatment period are plotted in Figure 15. I find no statistically significant difference in costs between for-profits with and without alternatives at both the college and program levels in the post-treatment periods. The estimated differences between the coefficients are also less than 2% in each year. These results provide evidence that there are no confounding trends correlated with the cost of attendance between for-profits with and without alternatives. The results are similar for the threat of sanctions at the college level, with no statistically significant differences until 2016 and all estimated differences close to zero. However, I do find statistically significant differences at the program level. These differences are a continuation of a relative decline in costs at failing and warning for-profit programs, beginning in 2010 when the GE rules are announced. While this trend may be due to unexplained differences between the treatment and control colleges, such a decline may also reflect the efforts of for-profits with high debt-to-earning ratios to lower the cost of attendance and therefore lower graduate debt levels.

Figure 11: Density of Enrollment and Completions by GAO Report Treatment Group



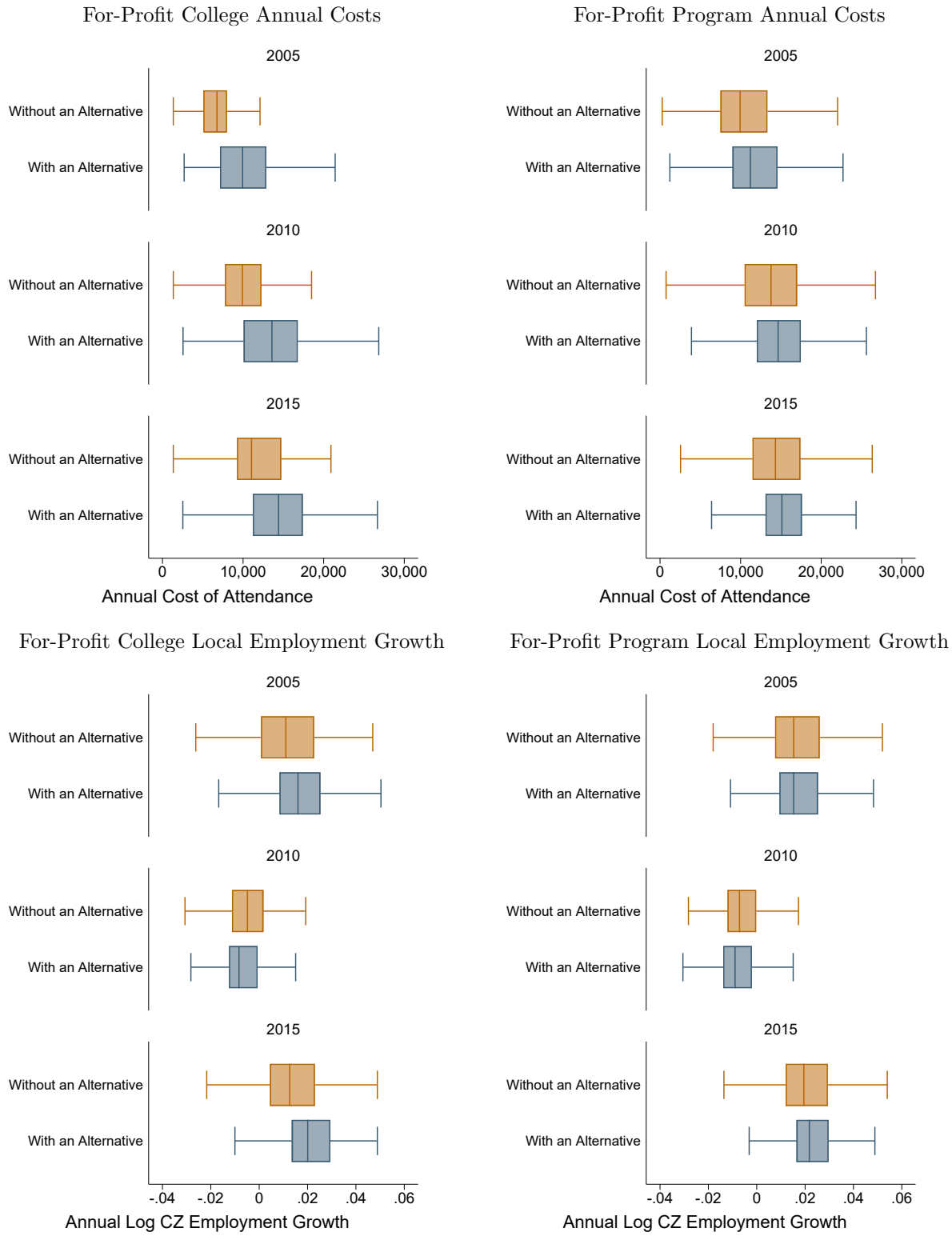
Note: For-profits with an alternative serve as the treatment group and those without an alternative are the control group. Distributions are estimated using the Epanechnikov kernel.

Figure 12: Density of Enrollment and Completions by GE Report Treatment Group



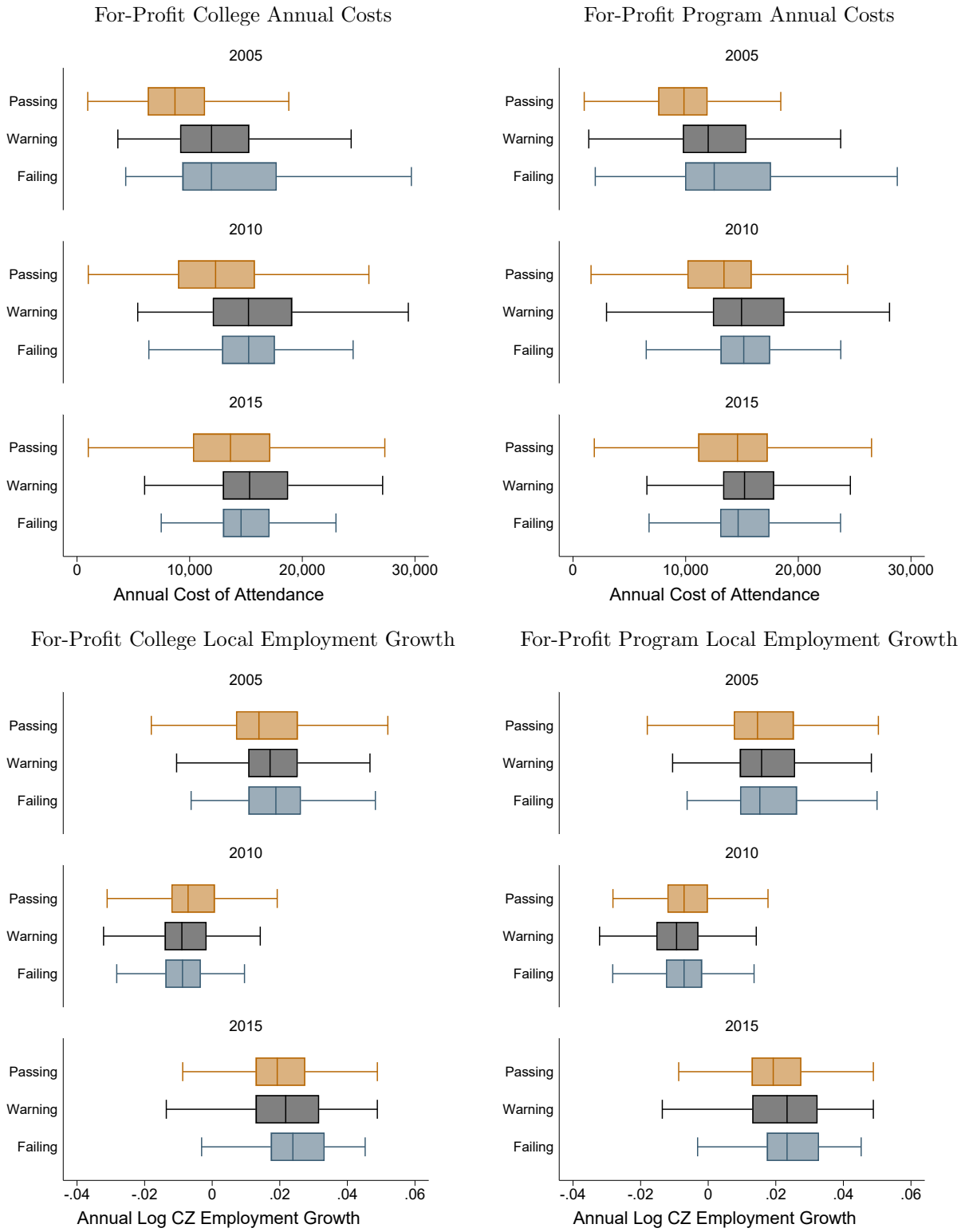
Note: Passing for-profits under Gainful Employment standards serve as the control group, while warning and failing for-profits are treatment groups. Distributions are estimated using the Epanechnikov kernel.

Figure 13: Cost of Attendance and Employment Growth by GAO Report Treatment Group



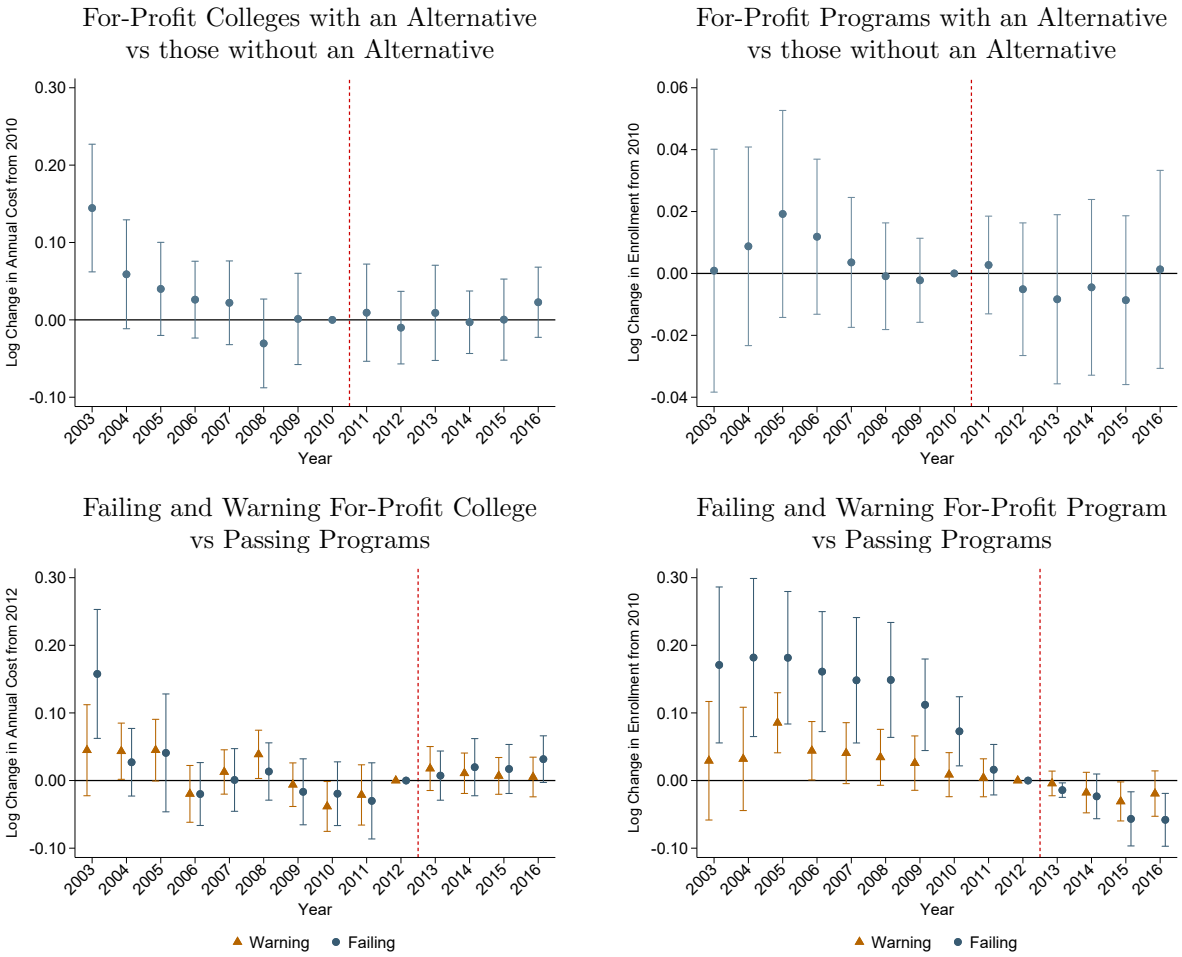
Note: For-profits with an alternative serve as the treatment group and those without an alternative are the control group. Costs are the annual tuition and fees reported per college or per program when data are available. Employment growth is the one-year annual log employment growth at the commuting-zone level. Observations that lie outside the standard boxplot range are not shown.

Figure 14: Cost of Attendance and Employment Growth by GE Report Treatment Group



Note: Passing for-profits under Gainful Employment standards serve as the control group, while warning and failing for-profits are treatment groups. Costs are the annual tuition and fees reported per college or per program when data are available. Employment growth is the one-year annual log employment growth at the commuting-zone level. Observations that lie outside the standard boxplot range are not shown.

Figure 15: Difference in Cost of Attendance betweeen Treatment and Control Groups



Note: The plotted coefficients are the estimated difference-in-differences of annual costs of attendance between treatment and control colleges and between each year and the last post-treatment year, estimated using Equation 3. Costs are the annual tuition and fees reported per college or per program when data are available. Vertical bars denote 99% confidence intervals. The red dashed line denotes the timing of the treatment.

Appendix B Difference-in-Difference-in-Differences

I supplement my analysis of the enrollment response to the GAO report by estimating the effect in a difference-in-difference-in-difference (DDD) framework. The DDD allows me to difference out both the effect of being a for-profit institution and the effect of having a not-for-profit alternative from the causal estimate. As shown in Section 5.2, public colleges did not experience an enrollment decline caused by the GAO report, and private nonprofit colleges experienced a smaller enrollment and completions decline than that of the for-profit sector. These results suggest there were no confounding trends related to having a not-for-profit alternative independent of the GAO report. This DDD exercise provides additional evidence of no such confounding trends. I estimate the difference between for-profits with an alternative and not-for-profits with an alternative and for-profits without an alternative before and after the GAO report. The regression specification becomes

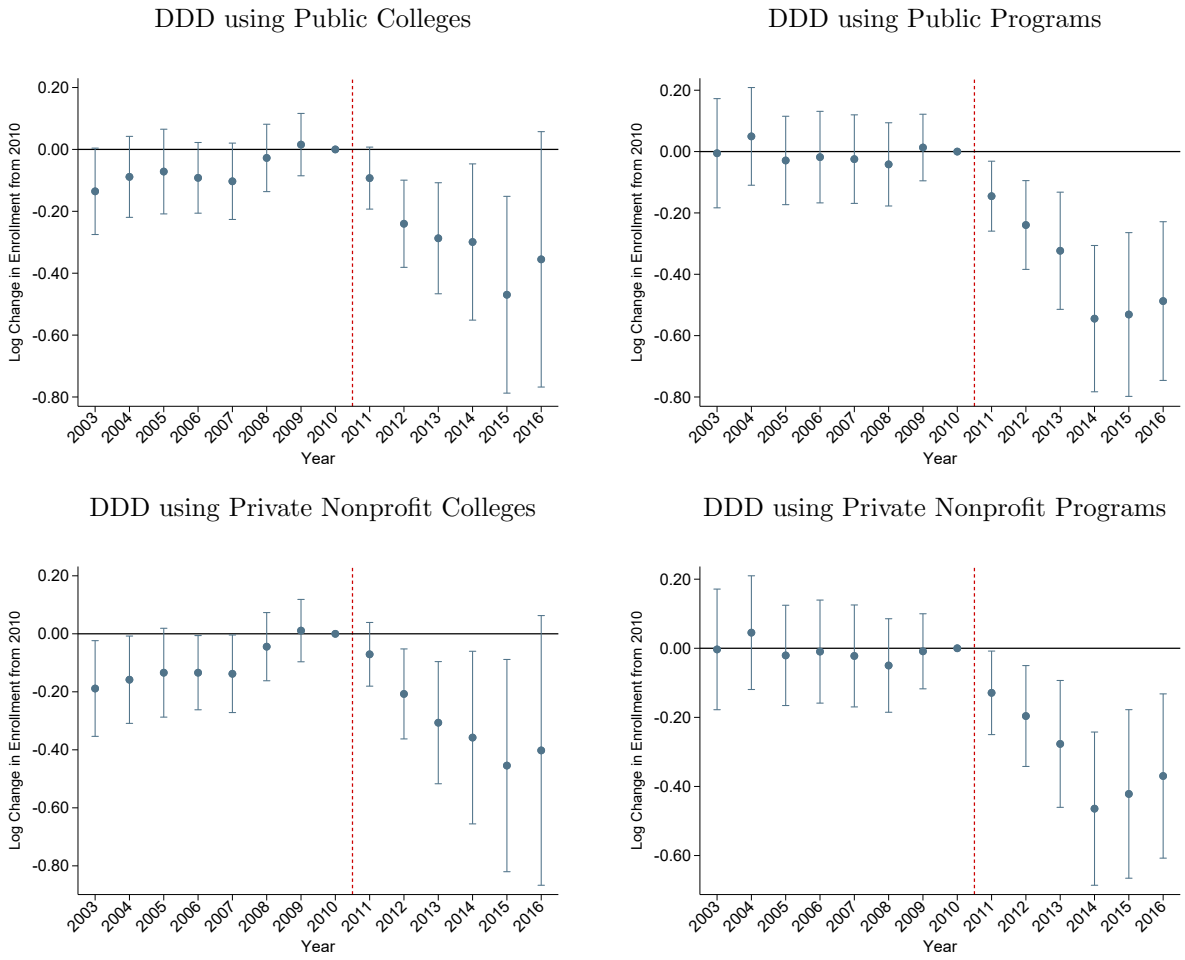
$$y_{i,t} = \alpha_i + \lambda_t + \delta_t^{GAO} D_i^{GAO} F_i + \delta_t^{GE} D_i^{GE} + X_{i,t}\beta + \epsilon_{i,t} \tag{5}$$

where F_i is an indicator for whether college i is a for-profit college. I control for the same set of covariate trends $X_{i,t}$ as in the main specifications.

I present the DDD estimatess in Figure 16 and Table 12. These results confirm the findings discussed in Section 5.2. The post-treatment declines in each specification are statistically significant and robust to a linear trend. In 2011, the GAO report caused a 9.6% enrollment decline when triple differencing using public colleges and a 7.2% decline when using private nonprofit colleges. These are both similar to the baseline DD estimate of 8.1%, however the smaller DDD estimate using private nonprofits again suggest that these colleges experienced a decline from the GAO report. This also confirms that the effect on for-profit colleges was larger than the potential effect on private nonprofit colleges. This pattern is repeated at the program level, with both DDD specifications providing a 2011 estimate near the 13.7% DD estimate. By 2015, the effect of the GAO report grows close to a 45% enrollment decline in both DDD specifications, mirroring the 44.6% baseline result. At the program level, the DDD provides causal 2015 completion declines larger than the baseline DD 35.3% estimate.

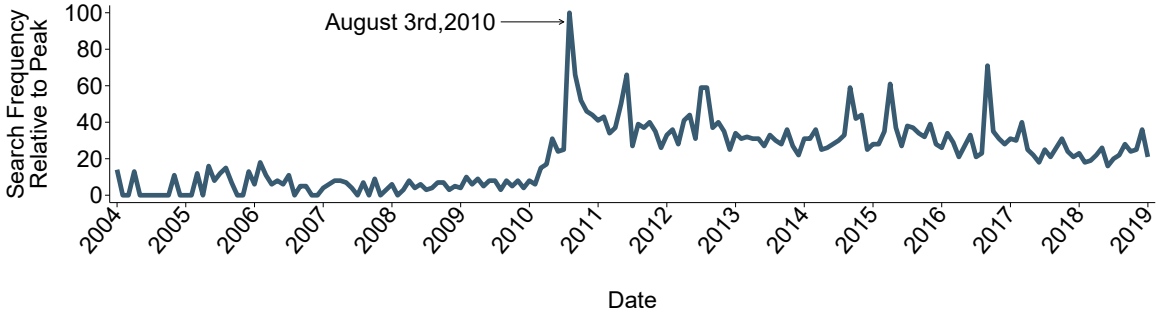
Appendix C Supplementary Figures and Tables

Figure 16: Effect of the GAO Report using Difference-in-Difference-in-Differences



Note: The plotted coefficients are the estimated difference-in-differences between treatment and control colleges and between each year and 2010, estimated using Equation 5. These coefficients are presented in Table 3. Vertical bars denote 99% confidence intervals. The red dashed line denotes the timing of the treatment.

Figure 17: Google Search Activity for For-Profit Colleges Before and After the GAO Report



Source: Google Trends (https://www.google.com/trends) *Note:* August 3rd, 2010 is the date the Government Accountability Report was released.

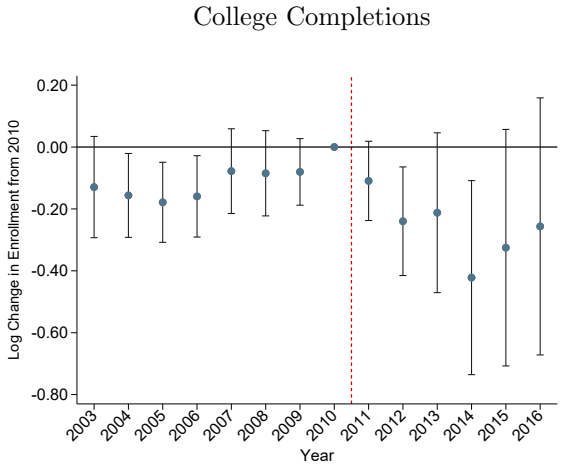
Table 12: Effect of the GAO Report using Difference-in-Difference-in-Differences

	College Enrollment				Program Completions			
	(1) Public	(2) Public	(3) Private	(4) Private	(5) Public	(6) Public	(7) Private	(8) Private
δ_t - δ_{2010}								
2005	-0.072	-0.073	-0.134**	-0.136**	-0.029	-0.031	-0.021	-0.026
	(0.053)	(0.053)	(0.059)	(0.058)	(0.056)	(0.056)	(0.056)	(0.056)
2006	-0.092**	-0.092**	-0.134***	-0.136***	-0.018	-0.019	-0.010	-0.014
	(0.044)	(0.044)	(0.049)	(0.049)	(0.058)	(0.058)	(0.058)	(0.058)
2007	-0.103**	-0.104**	-0.138***	-0.135***	-0.025	-0.025	-0.022	-0.025
	(0.048)	(0.048)	(0.052)	(0.051)	(0.056)	(0.056)	(0.057)	(0.057)
2008	-0.027	-0.030	-0.044	-0.042	-0.042	-0.042	-0.050	-0.052
	(0.042)	(0.042)	(0.045)	(0.045)	(0.052)	(0.053)	(0.052)	(0.052)
2009	0.016	0.015	0.011	0.011	0.013	0.013	-0.009	-0.010
	(0.039)	(0.039)	(0.042)	(0.042)	(0.042)	(0.042)	(0.042)	(0.042)
2011	-0.093**	-0.096**	-0.071*	-0.072*	-0.145***	-0.144***	-0.129***	-0.128***
	(0.039)	(0.039)	(0.042)	(0.042)	(0.044)	(0.044)	(0.047)	(0.047)
2012	-0.240***	-0.242***	-0.207***	-0.210***	-0.239***	-0.238***	-0.196***	-0.194***
	(0.054)	(0.054)	(0.060)	(0.059)	(0.056)	(0.056)	(0.056)	(0.056)
2013	-0.287***	-0.287***	-0.306***	-0.310***	-0.323***	-0.320***	-0.277***	-0.274***
	(0.069)	(0.069)	(0.081)	(0.081)	(0.074)	(0.074)	(0.071)	(0.071)
2014	-0.299***	-0.299***	-0.358***	-0.360***	-0.545***	-0.541***	-0.464***	-0.460***
	(0.098)	(0.097)	(0.115)	(0.115)	(0.092)	(0.092)	(0.086)	(0.086)
2015	-0.470***	-0.469***	-0.454***	-0.454***	-0.531***	-0.526***	-0.422***	-0.417***
	(0.123)	(0.123)	(0.141)	(0.142)	(0.103)	(0.103)	(0.094)	(0.095)
College FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Covariate Trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Linear Trend	No	Yes	No	Yes	No	Yes	No	Yes
Observations	52,786	52,786	45,308	45,308	1,381,045	1,381,045	727,624	727,624
R^2	0.199	0.199	0.187	0.187	0.047	0.048	0.083	0.084

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses.

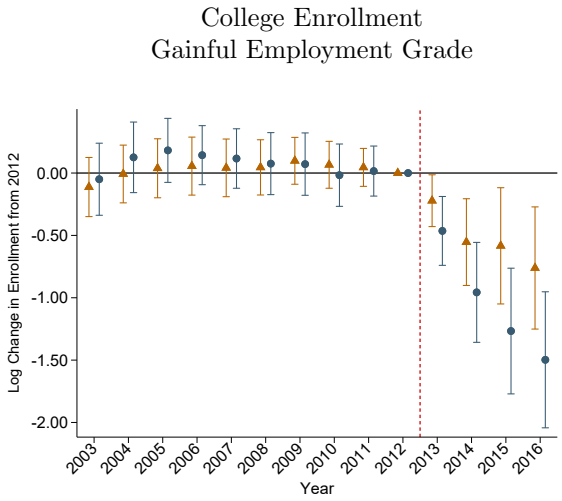
Note: Estimates of the effect of the GAO report over time are the difference in log trends between for-profits with and without alternatives. The covariate trends controlled for are GE status, cost of attendance, location type, program length, college size, lagged enrollment and completions growth, and state-level employment growth.

Figure 18: Effect of the GAO Report on Aggregate College Completions



Note: The plotted coefficients are the estimated difference-in-differences between treatment and control colleges and between each year and 2010. These coefficients are presented in Table 3. Vertical bars denote 99% confidence intervals. The red dashed line denotes the timing of the treatment.

Figure 19: Effect of the Gainful Employment Program on Aggregate College Completions



Note: The plotted coefficients are the estimated difference-in-differences between treatment and control colleges and programs and between each year and 2010. These coefficients are presented in Table 8. Vertical bars denote 99% confidence intervals. The red dashed line denotes the timing of the treatment.

Table 13: Effect of the GAO Report on Aggregate College Completions

	College Completions	
	(1)	(2)
δ_t - δ_{2010}		
2005	-0.179***	-0.093**
	(0.050)	(0.043)
2006	-0.159***	-0.096**
	(0.051)	(0.042)
2007	-0.078	-0.030
	(0.053)	(0.050)
2008	-0.085	-0.058
	(0.053)	(0.050)
2009	-0.080*	-0.068*
	(0.042)	(0.040)
2011	-0.109**	-0.135**
	(0.049)	(0.052)
2012	-0.240***	-0.286***
	(0.068)	(0.073)
2013	-0.212**	-0.278***
	(0.100)	(0.105)
2014	-0.422***	-0.508***
	(0.121)	(0.130)
2015	-0.325**	-0.430***
	(0.148)	(0.159)
College FE	Yes	Yes
Year FE	Yes	Yes
Covariate Trends	Yes	Yes
Linear Trend	No	Yes
Observations	28,355	28,355
R^2	0.209	0.209

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses.

Note: Estimates of the effect of the GAO report over time are the difference in log trends between for-profits with and without alternatives. The covariate trends controlled for are GE status, cost of attendance, location type, program length, college size, lagged enrollment and completions growth, and state-level employment growth.

Table 14: Effect of the Gainful Employment Program on Aggregate College Completions

	Warning Colleges		Failing Colleges	
	(1)	(2)	(3)	(4)
δ_t - δ_{2012}				
2005	0.038	0.126**	0.182*	0.218***
	(0.091)	(0.056)	(0.099)	(0.067)
2006	0.056	0.130**	0.143	0.173***
	(0.090)	(0.058)	(0.091)	(0.061)
2007	0.041	0.106*	0.116	0.142**
	(0.089)	(0.064)	(0.092)	(0.064)
2008	0.045	0.099	0.075	0.098
	(0.085)	(0.064)	(0.096)	(0.070)
2009	0.098	0.138**	0.072	0.089
	(0.073)	(0.058)	(0.096)	(0.077)
2010	0.066	0.094	-0.017	-0.003
	(0.073)	(0.063)	(0.096)	(0.082)
2011	0.045	0.060	0.016	0.025
	(0.059)	(0.054)	(0.077)	(0.071)
2013	-0.222***	-0.231***	-0.464***	-0.469***
	(0.080)	(0.083)	(0.107)	(0.109)
2014	-0.554***	-0.575***	-0.957***	-0.967***
	(0.134)	(0.135)	(0.155)	(0.164)
2015	-0.584***	-0.617***	-1.267***	-1.282***
	(0.180)	(0.178)	(0.195)	(0.211)
College FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Covariate Trends	Yes	Yes	Yes	Yes
Linear Trend	No	Yes	No	Yes
Observations	28,355	28,355	28,355	28,355
R^2	0.209	0.209	0.209	0.209

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses.

Note: Estimates of the threatened sanctions effect over time are the difference in log trends between non-passing and passing for-profits. The covariate trends controlled for are alternative-existence, cost of attendance, location type, program length, college size, lagged enrollment and completions growth, and state-level employment growth.

Table 15: Effect of the Gainful Employment Program on For-Profits: Excluding Ungraded Programs

	College Enrollment				Program Completions			
	(1) Warning	(2) Warning	(3) Failing	(4) Failing	(5) Warning	(6) Warning	(7) Failing	(8) Failing
δ_t - δ_{2012}								
2005	0.132*	0.049	0.284***	0.144***	0.051	-0.035	0.217**	-0.062
	(0.071)	(0.038)	(0.078)	(0.047)	(0.075)	(0.052)	(0.109)	(0.083)
2006	0.100	0.028	0.240***	0.118**	0.084	0.008	0.208**	-0.035
	(0.070)	(0.045)	(0.073)	(0.046)	(0.082)	(0.067)	(0.095)	(0.094)
2007	0.039	-0.019	0.104	0.003	0.094	0.031	0.309***	0.105
	(0.072)	(0.054)	(0.079)	(0.054)	(0.076)	(0.069)	(0.088)	(0.086)
2008	0.082	0.037	0.089	0.009	0.153*	0.102	0.321***	0.158*
	(0.068)	(0.053)	(0.067)	(0.049)	(0.078)	(0.069)	(0.091)	(0.089)
2009	0.177***	0.141***	0.164**	0.104*	0.114	0.073	0.169*	0.046
	(0.061)	(0.050)	(0.067)	(0.054)	(0.076)	(0.071)	(0.086)	(0.083)
2010	0.132**	0.110**	0.077	0.041	0.072	0.045	0.086	0.007
	(0.060)	(0.052)	(0.061)	(0.051)	(0.061)	(0.056)	(0.086)	(0.082)
2011	0.093*	0.080*	0.004	-0.013	0.074	0.063	0.000	-0.037
	(0.050)	(0.047)	(0.049)	(0.044)	(0.047)	(0.045)	(0.049)	(0.049)
2013	-0.142*	-0.127*	-0.290***	-0.270***	0.006	0.018	-0.070	-0.034
	(0.074)	(0.074)	(0.072)	(0.073)	(0.043)	(0.046)	(0.050)	(0.052)
2014	-0.446***	-0.420***	-0.794***	-0.755***	0.012	0.035	-0.023	0.051
	(0.120)	(0.121)	(0.142)	(0.144)	(0.076)	(0.079)	(0.083)	(0.092)
2015	-0.765***	-0.729***	-1.271***	-1.212***	-0.018	0.017	-0.221**	-0.108
	(0.249)	(0.250)	(0.202)	(0.203)	(0.097)	(0.102)	(0.110)	(0.129)
College FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Covariate Trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Linear Trend	No	Yes	No	Yes	No	Yes	No	Yes
Observations	21,237	21,237	21,237	21,237	56,662	56,662	56,662	56,662
R^2	0.226	0.226	0.226	0.226	0.248	0.247	0.248	0.247

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses.

Note: Estimates of the threatened sanctions effect over time are the difference in log trends between non-passing and passing for-profits. The covariate trends controlled for are alternative-existence, cost of attendance, location type, program length, college size, lagged enrollment and completions growth, and state-level employment growth.

Table 16: Spillovers of the Gainful Employment Program on Passing Program Completions: with Linear Trend

	Full Sample		Excluding College Closures	
	(1) Warning College Spillovers	(2) Failing College Spillovers	(3) Warning College Spillovers	(4) Failing College Spillovers
δ_t - δ_{2012}				
2005	0.036	0.038	0.064	0.068
	(0.055)	(0.052)	(0.062)	(0.064)
2006	0.039	0.057	0.046	0.034
	(0.047)	(0.058)	(0.049)	(0.068)
2007	-0.045	-0.018	-0.078	-0.062
	(0.048)	(0.065)	(0.055)	(0.077)
2008	-0.053	-0.028	-0.083	-0.048
	(0.055)	(0.071)	(0.064)	(0.080)
2009	-0.067	0.008	-0.079	0.013
	(0.053)	(0.067)	(0.056)	(0.077)
2010	-0.013	-0.033	-0.058	-0.018
	(0.054)	(0.066)	(0.055)	(0.073)
2011	0.012	-0.010	-0.008	-0.022
	(0.031)	(0.049)	(0.032)	(0.044)
2013	-0.120**	-0.210***	-0.060*	-0.050
	(0.052)	(0.066)	(0.032)	(0.042)
2014	-0.281***	-0.317***	-0.074	-0.039
	(0.080)	(0.091)	(0.048)	(0.064)
2015	-0.305***	-0.360***	-0.124**	-0.086
	(0.090)	(0.100)	(0.063)	(0.088)
College FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Covariate Trends	Yes	Yes	Yes	Yes
Linear Trend	Yes	Yes	Yes	Yes
Observations	133,912	133,912	106,053	106,053
R^2	0.205	0.205	0.125	0.125

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses.

Note: Estimates of the threatened sanctions effect over time are the difference in log trends across average graduate annual debt-to-earnings ratios. The covariate trends controlled for are alternative-existence, cost of attendance, location type, program length, college size, lagged enrollment and completions growth, and state-level employment growth.

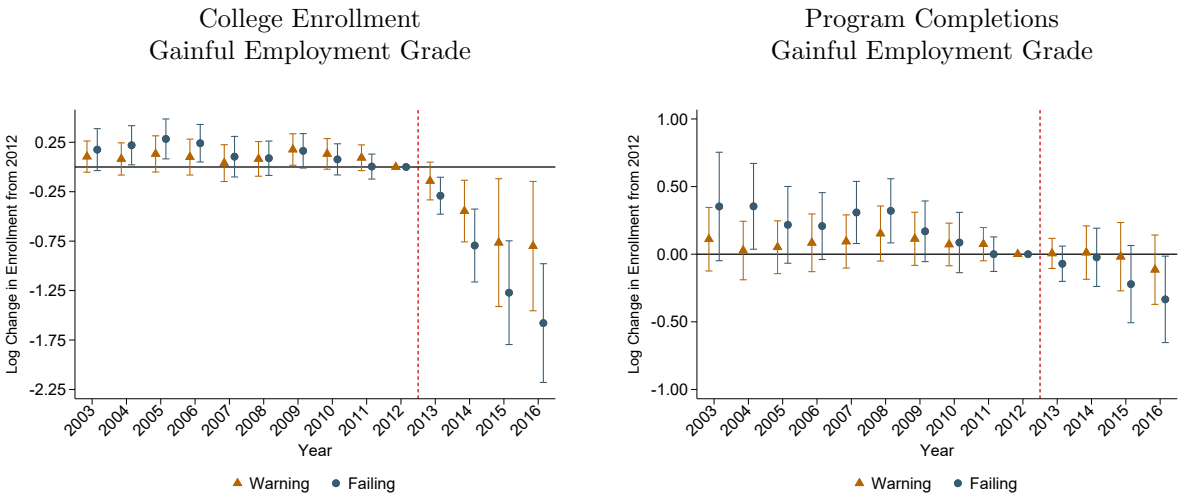
Table 17: Spillovers of the Gainful Employment Program on Passing Program Completions: Treated Programs

	Full Sample		Excluding College Closures	
	(1) Warning Programs	(2) Failing Programs	(3) Warning Programs	(4) Failing Programs
δ_t - δ_{2012}				
2005	-0.052	0.079	-0.075	-0.058
	(0.073)	(0.122)	(0.083)	(0.130)
2006	0.036	0.139	0.020	0.016
	(0.083)	(0.102)	(0.080)	(0.111)
2007	0.007	0.192*	-0.009	0.074
	(0.077)	(0.102)	(0.073)	(0.112)
2008	0.041	0.183	0.012	0.053
	(0.081)	(0.113)	(0.074)	(0.102)
2009	0.044	0.089	0.062	-0.026
	(0.082)	(0.101)	(0.072)	(0.088)
2010	0.026	0.016	0.023	-0.039
	(0.066)	(0.095)	(0.064)	(0.087)
2011	0.084*	0.007	0.085*	-0.020
	(0.048)	(0.045)	(0.051)	(0.046)
2013	-0.118**	-0.213***	-0.080*	-0.264***
	(0.054)	(0.047)	(0.047)	(0.054)
2014	-0.296***	-0.349***	-0.172***	-0.386***
	(0.085)	(0.080)	(0.066)	(0.067)
2015	-0.389***	-0.619***	-0.273***	-0.500***
	(0.105)	(0.102)	(0.087)	(0.075)
College FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Covariate Trends	Yes	Yes	Yes	Yes
Observations	133,912	133,912	106,053	106,053
R^2	0.205	0.205	0.125	0.125

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses.

Note: Estimates of the threatened sanctions effect over time are the difference in log trends across average graduate annual debt-to-earnings ratios. The covariate trends controlled for are alternative-existence, cost of attendance, location type, program length, college size, lagged enrollment and completions growth, and state-level employment growth.

Figure 20: Effect of the Gainful Employment Program on For-Profits: Excluding Ungraded Programs



Note: The plotted coefficients are the estimated difference-in-differences between treatment and control colleges and programs and between each year and 2010. These coefficients are presented in Table 8. Vertical bars denote 99% confidence intervals. The red dashed line denotes the timing of the treatment.