Student Responsiveness to Earnings Data in the College Scorecard

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September 2017

Abstract

Using the universe of SAT score sends to colleges and the exact date on which these scores are sent, we estimate how students responded to the release of the U.S. Department of Education's College Scorecard in September 2015. We find that data on annual cost and graduation rate, both of which were previously available, did not impact the volume of score sends received by colleges. By contrast, we estimate that each 10 percent increase in reported earnings resulted in a 2.4 percent increase in score sends. The impact is driven almost entirely by well-resourced high schools and students. We find little evidence that the count or composition of enrolled students changed as a result of the Scorecard information shock with the exception of a slight increase in academic preparedness, as measured by SAT scores, among enrollees at colleges with higher reported earnings.

1. Introduction

In his 2015 speech introducing the College Scorecard, President Obama discussed the importance of guiding students towards colleges that are sound investments and emphasized "clear, reliable, open data" as safeguards against making poor college choices (Obama, 2015). On September 12th 2015, the federal government officially released the new College Scorecard. This initiative, crafted by the Obama administration, was a direct response to greater pushes for accountability among institutions of higher education. Over a two year period, partners from the White House's Council of Economic Advisers, the Domestic Policy Council, Office of Management and Budget, the U.S. Digital Service, the Department of Education, and the Department of the Treasury combined data sources and resources to develop the Scorecard. The publicly available data files created by these entities contain hundreds of pieces of data for thousands of postsecondary institutions; however, these data are trimmed and distilled into three main pieces of information that are relayed through infographics on a college's Scorecard: "Average Annual Cost", "Graduation Rate" and "Salary After Attending."[1] Prior to its release, information on annual cost and graduation rates were available through several well-known and well-traveled sources, including College Navigator, Big Future, and *US News and World Report*, as well as a preliminary version of the Scorecard, without college-specific median earnings.

Though the media and policy worlds were abuzz after the Scorecard release, it was unclear whether the Scorecard would nudge students into making better college decisions. Two distinct strands of literature suggest that the College Scorecard holds real potential to induce these intended changes. First, there is a growing literature on how low-cost nudges, including text message reminders, college application fee-waivers, free SAT score sends, and assisted Free Application for Federal Student Aid (FAFSA) completion can help overcome seemingly small

[1] An example infographic can be seen in Appendix Figure 1.

barriers in the transition to college (Bettinger et al., 2012; Castleman & Page, 2015; Castleman & Page, 2016, Hurwitz et al., 2017).[2] Second, the quality disclosure literature consistently shows that consumers adjust their behavior when presented with new information. In the educational context, consumers demonstrate responsiveness to the quality disclosure of primary and secondary school data, as demonstrated in school choice (e.g., Hasting and Weinstein, 2008) and housing prices (Figlio and Lucas, 2004), and consumers also respond to college quality data through rankings (e.g. Luca and Smith, 2013) and informational interventions (e.g. Hoxby and Turner, 2013).[3] In other contexts, ranging from basic goods like breakfast cereal (Ippolito and Mathios, 1990) to much more complicated and essential services like health insurance plans (Scanlon et al., 2002), quality disclosure impacts consumers' purchasing decisions.[4]

In this particular context- after a monumental disclosure of college quality by the federal government- there are several reasons to believe that students would respond to these data. First and foremost, the median earnings represent novel data that students might use to estimate their own earnings potential from attending different postsecondary institutions. There is ample evidence demonstrating that students generally lack the ability to estimate the earnings from different educational pathways (Betts, 1996; Arcidiacono, Hotz, and Kang, 2012; Wiswall and Zafar, 2015; Baker et al., 2016) and so the provision of the earnings data may afford students the opportunity to adjust their prior beliefs.[5,6] Second, the Scorecard was created with an eye

[2] See Page and Scott-Clatyon (2016) and French and Oreopoulos (2017) for comprehensive summaries of the recent research on nudges towards and through college.

[3] Research on quality disclosure in education often focuses on accountability or the supply side- how schools respond to the disclosure of information about the schools. Examples include Carnoy and Loeb (2002), Hanushek and Raymond (2004), Jacob (2005), Peterson and West (2003), Haney (2000), Cullen and Reback (2006), and Figlio and Getzler (2006).

[4] We use the word "quality" to be consistent with the economics literature on "quality disclosure." We recognize that college quality cannot be captured by a few metrics; that some metrics may be driven entirely by selection and do not represent value-added of the college.

[5] Wiswall and Zafar (2015) use an informational experiment and show that students are willing to change their majors with access to more precise information on returns to each major.

towards user-friendliness, and may be more salient and digestible to students than previously available information. Students tend to rely on heuristics and simplifying rules of thumb in the college application process (e.g. Smith, 2013; Pallais, 2015) and the Scorecard's whittling down of countless elements of college data into just three metrics may simplify the decision process for students. Finally, consumers of higher education might put more faith in the Scorecard data because these data come directly from the federal government rather than an independent third party.[7]

On the other hand, there are several reasons the Scorecard might *not* influence student decisions. First, the Scorecard might simply represent one more addition to the vast amount of information about college quality already available to students, therefore having minimal impact on decisions about where to apply. Second, the government had many choices about how to implement and present the Scorecard data, but each small decision has the potential to impact usage and responsiveness. For example, the decision of which metrics to include can impact consumer responsiveness (Scanlon, et al., 2002; Pope, 2009; Dafny and Dranove, 2008), such that including earnings 5 years or 20 years after enrollment may evoke different responses compared to the Scorecard's data, which presents earnings 10 years after enrollment. Similarly, timing of disclosure has been shown to impact consumer responsiveness (Della Vigna and Pollet, 2009), and whether a September 12th release date, which coincided with the beginning of the school year, was timed properly to garner enough attention from the media or students is an open question. Finally, even if students have preferences for the metrics presented on the Scorecard, they may lack awareness of the Scorecard or not have enough time to access these data, which

[6] Prior beliefs may include data on earnings by a select number of institutions. For example, Texas publicizes earnings for its public four-year campuses on SeekUT and California does so for its public two-year colleges on SalarySurfer.

[7] This could also have the opposite effect. Dranove and Jin (2010) discuss the merits of disclosure from regulators versus third-party certifiers.

may be particularly true for traditionally underserved students (e.g. Haskins, Holzer and Lerman, 2009; Dettling, et al. *forthcoming*).

Whether students respond to the release of the Scorecard data is an empirical question that we seek to answer. To accomplish this, we harness data on the timing and volume of official SAT score sends (a measure of student interest in a college and a proxy for an application) to 1,600 four-year postsecondary institutions with complete Scorecard data. Our sample includes nearly 36 million score sends from all SAT takers in the 2010 through 2016 high school graduation cohorts. We also observe student college enrollment decisions through National Student Clearinghouse data.

Our identification relies on the sudden release of the Scorecard data on September 12th, 2015. Using this information shock, one could simply employ a differences-in-differences identification strategy to determine whether colleges with relatively favorable Scorecard metrics receive more score sends. Our main estimation strategy necessarily goes one step further and exploits the exact date on which score sends are ordered by students to account for college-cohort specific trends. Specifically, for each cohort of students, we separate out score sends to each of these 1600 colleges into two distinct periods — those ordered by students before September 12th of the students' senior years and those ordered on September 12th or later. Aggregating score sends at the college-cohort-period level (14 data points per college, from the two periods in each of the seven cohorts), we can estimate a model akin to a triple-difference approach and determine whether colleges with better quality measures on the Scorecard receive more score sends, using the pre-September 12th score sends to account for any college-cohort specific trends.

Our primary analyses on SAT score sending yield two main findings. First, we find that the release of the Scorecard's average annual cost and graduation rate — pieces of information that were previously available — did not influence the volume of SAT score sends. This is consistent with previous work on quality disclosure whereby consumers' responses are partially determined by previously known information (Dafny and Dranove, 2008).[8] Second, and by contrast, we find that a 10 percent increase in a college's median reported earnings presented on the Scorecard led to a 2.4 percent increase in SAT score send volume. To put this estimate of choice elasticity with respect to reported earnings in context, the average "median earnings" across colleges is approximately $50,000 and the standard deviation is $9,000. For the average sampled college, a one standard deviation in median reported earnings translates into approximately 100-150 additional score sends (4.3 percent increase). To larger and more selective colleges, both of which receive many more score sends and tend to attract students most likely to pay attention to the Scorecard data, the impact of higher reported earnings can translate into many hundreds of score sends. These magnitudes are both meaningful to the colleges and are larger than estimated elasticities in related contexts.[9] The results on reported median earnings are consistent with a concurrent working paper that uses Google searches as an outcome measure. Huntington-Klein (2016) notes that his paper considers a wider set of colleges and that our outcome is similar to that of an application while his outcomes focus on college search behavior.

Next, we investigate the characteristics of students and high schools that are driving the main findings, thereby distinguishing our data and results from that of Huntington-Klein (2016).

[8] We cannot rule out that the information was not previously known and students simply do not have strong preferences for these metrics. There is ample evidence that students do not have full information when deciding on colleges (e.g. Dillon and Smith, 2017).

[9] Beffy et al. (2012) find a major choice elasticity with respect to earnings of around 0.10.

Such heterogeneity in responsiveness might result from different preferences for the Scorecard elements, differences in Scorecard awareness, and differences in prior beliefs on college quality. We find that, among private high school students, SAT score sends to colleges increased by nearly 4.2 percent for each 10 percent increase in a college's reported median earnings. Across all public and charter high schools, responsiveness to the median earnings information is smaller in magnitude — 1.6 to 1.9 percent increases for a 10 percent increase in median reported earnings — and these estimates are not statistically significant at conventional levels. Average effects conceal the fact that students from more affluent public high schools (i.e. those with the lowest fraction of students receiving free/reduced price lunch) responded to the Scorecard by sending more SAT score sends to colleges with higher median earnings. These two findings are consistent with the literature documenting that under-resourced students are more likely to make sub-optimal college choices compared to academically comparable well-resourced students (e.g. Hoxby and Avery, 2012; Hoxby and Turner, 2015; Smith, Pender, and Howell, 2013). We also find some evidence that the response to Scorecard earnings data is stronger for students with SAT scores of at least 1100 and for Asian and White students (but not Black and Hispanic students). We then show that responsiveness to the median earnings is strongest during the period coinciding with college application deadlines, when students send SAT scores to colleges to satisfy college application requirements.

We conclude by investigating whether the College Scorecard impacted student enrollment. Despite finding that the median earnings component of the College Scorecard impacted score send volume, we find little evidence that the three Scorecard components shifted total first-year college enrollment volume, the racial/ethnic composition of enrollees, or the types of high schools (public versus private) from which enrollees came. This null finding may be due to the fact that colleges are already capacity constrained, which motivates our next analysis on the composition of incoming students. We do find some modest evidence that the earnings component shifted the average academic preparation of incoming students. Our models indicate that a 10 percent increase in median earnings resulted in a 2 point (on a 1600 scale) increase in average SAT scores among first-year matriculants.

This rest of this paper proceeds as follows. Section 2 discusses the College Board and College Scorecard data, along with showing descriptive statistics and discussing the research methodology. Section 3 shows the main results, along with several robustness tests. Section 4 shows how the results differ by timing of score sends and high school and student characteristics and Section 5 discusses the enrollment results. Section 6 concludes with some remarks on the potential benefits and costs of disclosing earnings data and more general policy implications of providing students with information.

2. Data and Methodology

2.1. SAT and Score Sends

We access administrative data on all SAT takers in the graduating high school cohorts of 2010-2016. For each of the approximately 1.7 million SAT takers in each cohort, we observe student SAT scores, which consist of a critical reading, math, and writing section that are each scored on a 200 to 800 point scale. College Board data also include the identity of the student's high school, and whether the high school is classified as a public, private, or charter high school. For students attending public or charter high schools, we append data from the 2013-2014 Common Core of Data (CCD), which provides information on the racial/ethnic composition of the student body as well as the fraction of students eligible to receive free or reduced price lunch.

We also utilize rich data from the College Board on all colleges to which students sent their SAT scores (score sends). In addition to college identity, we know the exact date on which the student ordered the score to be sent. These two critical pieces of information on score sends allow us to exploit the timing of the Scorecard release to draw causal inferences about how the three prominent Scorecard elements shifted student demand for colleges. The score sends represent official documentation of a student's SAT score, which most traditional four-year colleges require as a part of the college application process. College entrance exam score sends are generally considered proxies for college applications (Card and Krueger, 2005; Pallais, 2015). Though not all score sends materialize into college applications, they are, at the very least, a good indicator of the colleges the student is considering (Smith, *forthcoming*). Under the College Board's current score send policy, students receive four free score sends with each test registration, which must be used within 10 days of the exam date.[10] Score sends are typically either sent when students register for exams, but also after the 10 day window, when students are completing college applications in the fall of their high school senior years. The college identity piece of the score sends allows us to determine how many score sends a college receives for a given cohort of students and the date of the request allows us to determine whether the score send was sent on or after the September 12th Scorecard release date. A small number of students send SAT scores to a college multiple times, as students hope to update colleges on their latest and improved SAT scores. In our main analyses, we consider all score sends, and later show that considering only a student's first or last score sends results in identical parameter estimates.

[10] The four free score sends accompany each SAT registration and the cost of sending additional SAT scores has increased slightly over the time period of interest. Also, low-income students are eligible for four additional free score sends that can be used at any time during their high school careers. Students from the 2010-2016 cohorts were also subject to the current SAT score sending policy referred to as Score Choice. Under Score Choice, students have the option to send results from the SAT administration of their choosing.

Finally, we merge all students from the 2010 through 2016 cohorts to enrollment data from the National Student Clearinghouse (NSC). This gives us a near complete picture of the first institution to which students enroll.[11] Since the 2016 high school graduating cohort has been tracked through the fall of 2016, we consider only on-time enrollment (within 180 days of high school graduation) to ensure comparability over time. We also have enrollment data for non-SAT takers who took other College Board assessments, including the PSAT/NMSQT or an Advanced Placement (AP) exam.

2.2. College Scorecard Data

The new College Scorecard is an online tool for students to compare and contrast colleges and was released to the public on September 12th, 2015.[12] As a part of this initiative, the U.S Department of Education released public-use data for approximately 7,800 colleges, much of which was repackaged from existing data sources, like the Integrated Postsecondary Education Data System (IPEDS). However, for each college, the Scorecard features prominently, through easy-to-understand infographics, three pieces of information intended to provide students with a succinct snapshot of the college: average annual cost, six-year completion rates, and median earnings (see Appendix Figure 1). Inclusion into our final sample begins with the 2,174 four-year colleges that have all three of these data elements.

Among the three main Scorecard elements, the average annual cost, also referred to as net price, reflects the college's total cost of attendance (including living expenses) net of grant aid from all sources for undergraduates receiving federal aid.[13] For students attending public

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[11] NSC covers approximately 95% of all college enrollees.

[12] An earlier version was online prior to September 12th but did not have earnings data and consequently had less fanfare.

[13] Full documentation can be found online here: https://collegescorecard.ed.gov/assets/UsingFederalDataToMeasureAndImprovePerformance.pdf

colleges, the average annual cost only reflects costs faced by in-state students. While the net price might be a relevant data point for the students eligible to receive federal Title IV aid, students not qualifying for such aid might be expected to pay considerably more than the advertised net price, particularly at colleges that do not offer merit-based aid. The six-year bachelor's completion rate reflects the fraction of full-time, first-time students who started in the fall semester and earned bachelor's degrees from their first institution. This metric is perhaps the best publicized of the three and available on numerous websites geared at informing students, including College Navigator and Big Future. In addition to a long track record of availability on IPEDS, this metric plays an influential role in the highly trafficked *US News and World Report* college rankings and is published alongside of the rank. The graduation rate does not account for the outcomes of students who transfer out of the institution or who transfer into the institution. Finally, the median earnings, which were previously unavailable at this scope and thus have the potential to represent the biggest information value-added, reflect earnings 10 years after the student first entered the postsecondary institution. Earnings data for students who did not complete degrees or who transferred to other institutions and then completed degrees are included in the original institution's data. Also, only students who received federal aid are included in the earnings statistics, so the salient data may not be representative of the typical entering student, particularly at colleges where small shares of students receive federal aid.

2.3. Variation in College Scorecard Data

How much variation is there in the Scorecard data and how does it relate to previously available information? In Figure 1, we show scatterplots of the relationships between each of the three Scorecard elements in the left-hand panels and relationships between each of the Scorecard elements and median SAT scores among enrolling students, for the subset of sampled colleges reporting average SAT scores on the 2013 IPEDS survey. Annual cost is distinctive from the other three dimensions shown in Figure 1 because it is not typically thought of as a measure of college quality. The strength of association between annual cost and the other three measures of college quality is fairly weak, with R-squared statistics in the vicinity of 0.10 to 0.20. Interestingly, the middle panels of Figure 1 reveal an upward sloping relationship between the x-axis college quality variables (SAT Composite and Median Earnings) and annual cost, which eventually transitions into a negative relationship among colleges with the highest median earnings and composite SAT scores. This downward slope is driven by the extremely generous financial aid packages provided to needy students at the nation's most selective colleges. The relationship between the two "traditional" metrics of college quality- college completion rates and SAT scores (upper right-hand panel) - is the strongest shown in Figure 1. Median earnings- a new and previously unobserved measure of college quality- is only modestly related to the traditional measures.

There is substantial variation in median reported earnings, even conditional on other metrics commonly associated with college quality. For example, the lower right panel of Figure 1 shows that, at each SAT increment, particularly above scores of 1000, there is a wide range of median earnings. Overall, these figures clearly relay that median reported earnings provide students with opportunities to revise their own impressions of college quality, ultimately influencing the student's college selection/choice behavior.

2.4. Descriptive Statistics

We aggregate the student-level SAT score send data at the college-period-cohort level for our primary analyses. We structure our data this way because only SAT scores sent on or after September 12, 2015 for the 2016 high school graduation cohort would have been influenced by the College Scorecard data. Since September 12 coincides roughly with the start of the school year, we refer to scores sent on or after this date in the final year of high school as *Senior* score sends.[14]

As we discuss in the methods section below, this within cohort disaggregation is essential because we do not want to mistakenly assign score sends from the 2016 cohort as influenced by the College Scorecard data if those scores were sent during the student's junior high school year-before the new Scorecard was released. As a result of this purposeful within cohort disaggregation, the typical college in our sample contains 14 observations, comprised of seven high school graduation cohorts of students (cohorts 2010-2016) and two "periods" for each of the seven cohorts (i.e., senior year and before senior year). We only include the 1,588 four-year colleges that receive score sends across the sample period. Colleges that do not receive score sends are primarily for-profit colleges and on occasion small non-selective non-profit colleges.[15]

The descriptive statistics shown in Table 1 are intended to orient readers to the structure of our data.[16] The annual cost of the colleges represented by these score sends is nearly $20,000, earnings are just over $50,000, and six-year graduation rates hover around 72 percent. Table 1 shows that the typical college in our sample receives between two and three times more score sends from students in their senior year than prior to that (i.e., through September 11th of the students' junior years). Figure 1 reveals that an annual cost of $20,000 per year is reflective of the typical college in our sample. By contrast, both the median earnings and six-year bachelor's completion rates accompanying the typical score send are considerably higher than those of the

[14] In practice, we consider score sends through January 13th of the student's senior year, roughly two weeks after many application deadlines have passed. At the time that these analyses were conducted, January 13, 2016 was the final date on which score send data were available.
[15] A few colleges only receive score sends in some of the periods. We include them in our analyses but later test the sensitivity of the results with a balanced panel.
[16] Appendix Table 1 shows the descriptive statistics at the student-level. We do not analyze the data at this level but do make use of student characteristics when we consider heterogeneous impacts.

sampled college average. Colleges with higher bachelor's completion rates and higher earnings tend to draw more score sends, and this was true even before the Scorecard was available for public consumption. The final two columns show enrollment from the NSC of both SAT test-takers and students who participated in any of College Board product (PSAT/NMSQT, PSAT, SAT). Over time, enrollment has been steady, and the majority of enrollees in our sample took the SAT.

2.5. Methodology and Identification

Constructing a simple difference-in-differences (DID) model with college fixed effects, as in EQ(1), would be the approach best suited towards determining the impacts of the College Scorecard on SAT score sending if the exact SAT score-sending dates were unknown. We would fit EQ(1) to data structured such that each observation represents the number of SAT scores sent to college *j* among students from each of the cohorts 2010 through 2016. In EQ(1), the vector *Cohort* represents a set of indicator variables for each of the 2010 through 2016 cohorts. Parameter estimates, $\beta_2, \beta_3, \beta_4,$ indicate the extent to which the three time-invariant College Scorecard elements altered the expected score sends received by college *j* in the 2016 cohort. [17]

$$\begin{aligned} \log(ScoreSends_{jpt}) &= \beta_0 + \beta_1 Cohort_t + \beta_2 \log(MedianEarnings_j) \times Cohort2016_t + \\ &\beta_3 \log(AnnualCost_j) \times Cohort2016_t + \beta_4 \log(GradRate_j) \times Cohort2016_t + J + \varepsilon_{jpt} \end{aligned} \qquad (1)$$

Though we show the parameter estimates from this fitted model, access to the exact dates on which scores are sent to colleges allows us to refine this traditional model to strengthen claims of causality. There are two potential drawbacks of this traditional model that we remove when constructing the final model (EQ(2)), explained below. The first drawback is common to

[17] Readers might notice the omission of standalone variables representing *log(MedianEarnings)*, *log(AnnualCost)* and *log(GradRate)* in EQ(1). These time-invariant college-specific metrics are omitted because we incorporate college fixed effects and they do not vary within colleges.

many DID analyses. Factors unrelated to the College Scorecard may have uniquely impacted students from the exposed 2016 cohort in manner that re-directs SAT score sending towards colleges with high median earnings or low net costs. Armed with data on the exact dates of score sending, we can parse college-specific score sends into two periods- those sent prior to a student's senior year and those sent during a student's senior year (September 12th or after). For the 2016 cohort, such separation is important because only the latter group of score sends could have been influenced by the Scorecard data.

Perhaps unsurprisingly, among students in a given cohort, the number of SAT scores sent prior to students' senior years to a particular college is an extremely strong predictor of the number of senior year score sends.[18] In other words, score sends prior to the College Scorecard release for the 2016 cohort provide a good indication of each college's popularity among students in this cohort, and provides valuable information on how many score sends each college might have received, even in the absence of the College Scorecard shock.

The second drawback is closely related to the first. EQ(1) specifies that we group all score sends from a cohort together. In the context of our paper, such an approach would mean grouping early score sends from the 2016 cohort with senior year score sends that were potentially influenced by the College Scorecard. Failure to account for timing of the cohort 2016 score sends could potentially understate the impact of the College Scorecard elements if no effort is made to separate the senior year score sends from the earlier score sends.

A more appropriate identification strategy takes into account the periodicity of score sends within a cohort, such that score sends from each cohort to each college are divided into two batches: those occurring on or after September 12th (the Scorecard's release date) of the

[18] Across all cohorts, we find that a 10 percent increase in pre-senior year score sends is associated with a 3.4 percent increase in senior year score sends.

student's senior year and those occurring before this date. Our main identification strategy is akin to a triple-difference estimation strategy. With an observation at the college-period-cohort level, we estimate equation (2):

$$\begin{aligned}
&\log(ScoreSends_{jpt}) = \beta_0 + \beta_1 Cohort_t + \beta_2 Senior_p + \beta_3 Cohort_t \times Senior_p + \beta_4 \log(AnnualCost_j) \times Cohort_t + \ldots \\
&\beta_5 \log(GradRate_j) \times Cohort_t + \beta_6 \log(MedianEarnings_j) \times Cohort_t + \beta_7 \log(AnnualCost_j) \times Senior_p + \ldots \\
&\beta_8 \log(GradRate_j) \times Senior_p + \beta_9 \log(MedianEarnings_j) \times Senior_p + \ldots \\
&\beta_{10} \log(MedianEarnings_j) \times Cohort2016_t \times Senior_p + \ldots \\
&\beta_{11} \log(AnnualCost_j) \times Cohort2016_t \times Senior_p + \ldots \\
&\beta_{12} \log(GradRate_j) \times Cohort2016_t \times Senior_p + J + \varepsilon_{jpt}
\end{aligned} \qquad (2)$$

In equation (2), we regress the logarithm of $ScoreSends_{jpt}$ on a vector of fixed effects for colleges (*J)*, a vector of fixed effects for $Cohort_t$, an indicator for whether the score send count represented the post September 12th period of the student's senior year separately in each cohort ($Senior_p$*)*, interactions between the vector $Cohort_t$ and the binary variable $Senior_p$, and interactions between these variables and each of the three time-invariant college-specific Scorecard elements. The parameters associated with the triple interaction terms between $Cohort2016_t$ (the indicator for the 2016 cohort), $Senior_p$ and each of the three Scorecard elements, represented by β_{10}, β_{11} and β_{12}, indicate the causal response to the release of each element of the Scorecard data. Formally, these three parameters indicate the extent to which the relationships between Scorecard elements and score send growth between the pre September 12 period and senior year period differed for the 2016 (Scorecard exposed) cohort compared to prior cohorts. Practically speaking, it is the causal impact of the Scorecard on student score sends. This method allows us to rule out the influence of college-cohort shocks that are correlated with the Scorecard data by accounting for pre-September 12th score sends in each cohort. The log-log specification allows us to interpret the coefficients as elasticities with respect to each Scorecard data element. We also test the robustness of the models to the logarithmic specification.[19] [20]

We attribute the results that follow regarding changes in students score send behavior to the release of the Scorecard data, but we also recognize that the release of the Scorecard did not happen in a vacuum and colleges with positive realizations of the metrics may exploit the good news, perhaps through advertising and outreach. This means that our results should be viewed as the net impact of the Scorecard release, which may include the effects attributable to colleges marketing their favorable Scorecard metrics to students. Also, we cannot determine whether the students are actually accessing the Scorecard data, rather than agents of the student (e.g. parents or school counselors), who in turn, guide the students.

3. Main Results

3.1 Simple Difference-in-Difference Results

In Table 2, we show the parameter estimates from fitting EQ(1)- the traditional DID model to our data. We separately fit EQ(1) to models where the outcome variable represents all score sends from a cohort, regardless of timing, as well as score sends prior to a student's senior year and score sends during the student's senior year, which, for the 2016 cohort, might have been influenced by the College Scorecard.

Our first specification (column 1) suggests that a 10 percent increase in scorecard-reported median earnings is related to a 2.15 percent increase in score sends, and that a 10 percent increase in annual cost led to a 0.77 percent decrease in score sends. The second

[19] We also try other non-linear models such as discontinuous responsiveness to exceeding an earnings threshold (e.g. $50,000) but do not see evidence that these fit the data better than the logarithmic specification.

[20] We report heteroskedastic robust standard errors. Clustering at the college level tends to yield slightly smaller standard errors, so we rely the larger, un-clustered errors to ensure that we are not making incorrect inferences.

specification cautions that tying the student responsiveness to the annual costs shown in the College Scorecard might be a misattribution. In terms of SAT score sending, students from the 2016 cohort are more sensitive to annual costs compared to students from other cohorts, but evidence of this sensitivity, even after the omission of score sends that might have been influenced by the College Scorecard (column 2), casts doubt on any causal linkage.

Comparing columns 2 and 3 offers a preview of the results when we include all score sends and fit EQ(2). If the introduction of the College Scorecard had no influence on score sending, we might expect column 3 and column 2 to reveal identical parameter estimates. While parameter estimates for annual cost and graduation rate are fairly similar between these two columns, there is a notable divergence in parameter estimates for median earnings.

3.2 Descriptive and Graphical Results

We also motivate our main specification in EQ(2) by showing separately the score sends from senior year and earlier for each cohort, after dividing sampled colleges into terciles based on the Scorecard's median earnings.[21] So, for example, Table 3 and the corresponding Figure 2 show that the 2010 high school cohort sent 107,433 score sends to colleges in the first tercile of median earnings prior to September 12th 2009. The second column shows that the same cohort sent 165,149 score sends to the same colleges on or after September 12th 2009. These patterns of the number of score sends in each cohort-period (by tercile) can be seen in Panel A of Figure 2. In columns 3, 6 and 9 (corresponding to each earning's tercile), we show the sequence of score sends for each cohort as ratios of senior year score sends to non-senior score sends (pre-September 12th). These "growth" ratios, within a cohort (by tercile), can be seen in Panel B of Figure 2.

[21] The bottom tercile of sampled colleges has median earnings less than or equal to $36,700 and the top tercile has median earnings greater than or equal to $43,900.

Columns 11, 13 and 15 in Table 3 foretell the main findings of the paper. These percentages reflect annual changes (first difference) in the score growth ratios (second difference) between earnings terciles (third difference). Using the senior/pre-senior period ratios of score sends for each median earnings tercile (columns 3, 6, and 9), we then estimate the ratio of these column estimates across terciles (columns 10, 12 and 14). Finally, we calculate the annual change in the differential ratios by tercile (columns 11, 13, and 15). The year-over-year growth in these ratios between the tercile of colleges with the highest median earnings (Tercile 3) and the other two terciles is considerably larger for the 2016 cohort than for the other cohorts. For example, the ratio of the third tercile score growth to the second tercile score growth (column 12) increased by 7.3 percent between the 2015 and 2016 cohorts. Similarly, when comparing the third and first tercile ratios (in column 14), there is some oscillation over time and then a 9.7 percent increase in 2016. Since the 2016 cohort coincides with the release of the Scorecard, we conjecture that the unexpectedly large score growth among high median earning colleges, compared to previous trends, is driven by the sudden introduction of college-specific median earnings data.

3.3 Main Regression Results

In Table 4, we show only the parameter estimates in equation (2) for the causal response to the Scorecard data (the triple-interaction terms) for the entire sample of students as well as for students attending regular public, charter and private high schools. Across the entire sample of students, a 10 percent increase in the college median's reported earnings, as reported by the Scorecard, resulted in a 2.35 percent increase in score sends. Publication of the other two Scorecard elements, which were already available to students prior to the 2015 Scorecard release date, had little or no measurable impact on score sends. Note that these estimates are nearly identical to the differences between the column 3 (senior year) and column 2 (pre-senior year) simple DID estimates in Table 2.

Compared to students at public and charter high schools, parameter estimates for private high school students suggest that they were considerably more responsive to the new median earnings component of the Scorecard. Among students from these high schools, a 10 percent increase in median earnings resulted in a 4.15 percent increase in SAT score sends. The triple interaction terms containing median earnings are not significantly different from zero among students from public and charter high schools.

3.4 Robustness and Placebo Tests

Through the magnitude and significance of the triple interaction terms, these analyses show that the relationship between the Scorecard-reported median earnings and score send volume during the post-September 12th (senior year) period is stronger than what occurred among students in previous cohorts. One plausible threat to the validity of these claims is that there are annual changes in the relationship between median earnings and post-September 12th score send growth that are driven by publicly available factors closely related to median earnings. Incorporating into our models interaction terms involving graduation rates and annual costs allays some of these concerns. We also conduct a series of tests to prove that the significance and magnitudes on the triple interaction terms in equation (1) are unique to the 2016 cohort.

In Table 5, we show the relationship between score send growth and the three Scorecard metrics separately for each cohort, by regressing the *Log(ScoreSends)* on these three interaction terms, and an indicator for *Senior*, continuing to control for the fixed effects of colleges. The positive relationships on the interaction terms between *MedianEanings* and *Senior* indicate that colleges with higher reported earnings on the September 12th, 2015 Scorecard tended to experience larger growth rates in score sends between the pre-senior and senior periods. For example, Column 1 shows that each 10 percent increase in median earnings correlates with 5.93 percent more score sends in the post-September 12th period for the 2010 cohort. There is a clear jump in the interaction between *Senior* and *log(Median Earnings)* between the 2015 cohort and the 2016 cohort, and this jump is even more dramatic among students who attend private high schools, implicating the Scorecard data as driving this paper's results.

Table 5 motivates our formal placebo tests, which appear in Table 6. In the main specifications presented in this paper (Table 4), we have tested whether the 2016 cohort interaction terms between *Senior* and the Scorecard metrics differs from the interactions between these elements in earlier cohorts. Our placebo tests evaluate whether the triple interaction terms constructed for other cohorts are also statistically significant. Such placebo tests would detect trends in the relationships between median earnings and score send growth between the pre-senior and senior periods and would also reveal whether annual fluctuations in this relationship are sufficiently large such that any triple interaction terms would appear as statistically significant. We restrict our sample to the unaffected 2010 through 2015 cohorts and refit equation (2) six times, substituting out the triple interaction terms (that were interacted with the 2016 cohort dummy) with placebo interaction terms for each of the 2010 through 2015 cohort dummies. Such testing allows us to conclude whether the relationship between *Senior* and *Log(MedianEarnings)* for each cohort differs from those existing in the other cohorts.

Table 6 shows the placebo triple interaction terms estimated from the 2010 through 2015 cohorts for all students and separately for private high school students. As in our main specification, the placebo interaction terms associated with graduation rates and annual cost are not significant at conventional levels. In contrast to our main specifications, the triple interaction terms associated median earnings are never significant at conventional levels. Combined with Table 5, these results show that time trends and spurious relationships between Scorecard metrics and score sends cannot explain the paper's main finding that score send growth for the 2016 cohort was influenced by the Scorecard's median earnings metric.[22]

We also perform several robustness and specification tests that confirm our main findings. Specifically, in Appendix Table 2 we fit models where the dependent variable, number of score sends, is not logarithmically transformed. In Column 1 of Appendix Table 3, we remove all colleges that do not receive score sends in all of the 14 periods for the 2010-2016 cohorts and find consistent results. The final two columns of Appendix Table 3 show that results are insensitive to whether we use the earliest or latest score send from a student to a college.

4. Heterogeneous Impacts on Score Sending

4.1. By Time Elapsed Since Scorecard Release

Access to the exact dates on which students sent SAT scores to colleges affords us the opportunity to split the post Scorecard period into intervals, which provides us insight on two related pieces of information. First, we can determine whether the Scorecard release induced an immediate response from students. On the one hand, the fanfare of the Scorecard release on September 12th 2015 may correspond to a sharp increase in score sends to certain colleges. On the other hand, the information may take some time to diffuse to students. Second, even if students look at the Scorecard immediately upon release, college application deadlines are

[22] Relatedly, over the cohorts in this paper, the ACT test-taking has increased, while SAT volume has remained fairly flat. As a result, the fraction of all college applications captured by SAT score sends may have decreased. Over the cohorts 2010 through 2016, the increases in ACT test-taking were gradual. In Tables 5 and 6, we rule out the possibility that our estimates are driven by gradual trends such as ACT expansion, as opposed to the abrupt shock of Scorecard information.

generally around January 1st. Therefore, the scores sent in the months preceding these application deadlines may be reflexive responses to the prompt to send four free score sends at the time of SAT registration (Pallais, 2015; Smith, *forthcoming*; Hurwitz et al., 2017) and may not be as purposeful as the non-free score sends that colleges receive near the application deadlines. Under these circumstances, responsiveness to the Scorecard's metrics would be concentrated in later months.

To assess this empirical question, we re-fit our main specification (equation 2) but divide the senior year score sends into 30 day intervals. For each of the regressions in Table 7, the post-period unit of observation only consists of the number of score sends during the 30 day period specified by the column headers. By splicing the senior year score sends into discrete smaller periods, we are able to pinpoint the timing of the student response which drove the overall effects shown in Table 4.

Table 7 shows that the Scorecard did not immediately shift score sending patterns. Rather, the main results in Table 4 are driven by student behavior in the months closer to college application deadlines. In the top panel, which focuses on all students, there is no statistical response to the release of earnings data other than after December 10th. In the lower panel, which considers only private school students, there is no statistical response right after the Scorecard is released but the October 12th-November 10th period shows an approximately three percent increase in score sends to colleges with 10 percent higher median earnings. This impact persists somewhat for the next 30 days and then jumps even higher to a nearly five percent response, in the final period. While the final period is farthest removed from the September 12th release date, it also coincides with application deadlines for first-year admissions at many institutions. Relatedly, the relatively high elasticities during the October 12th - November 10th period, particularly for students at private high schools, coincide with many colleges' early decision/early action deadlines.

These results also suggest that the treatment on the treated impacts may be substantially larger than our intent-to-treat estimates. While Table 7 shows intent-to-treat estimates, the later periods use a subset of score sends that are more deliberate, and therefore the college destinations of these score sends are more likely to have been influenced by the Scorecard data, compared to the reflexive score sends accompanying SAT registrations.[23]

4.2. By Public High School Characteristics

In light of the differential responsiveness to the Scorecard, by private/public high school status, we estimate whether similar differential responsiveness is observed across public high schools that vary on measures typically associated with school resources and cultural capital that influence the college choice/selection process (see Einhorn, 2015). To accomplish this, we divide all score sends sent by public and charter high school students from the 2010 through 2016 cohorts into terciles based on high school-level free/reduced price lunch eligibility and underrepresented minority group (Black and Hispanic/Latino) membership.[24, 25]

Table 8 offers some suggestive evidence of differential responsiveness to the Scorecard elements across students from public/charter high schools that differ on student body racial/ethnic composition and free/reduced price lunch eligibility. The results in this table are consistent with those shown in Table 4, although not quite as stark. Though we cannot conclude that responsiveness to the Scorecard differs across different types of public high schools,

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[23] For the 2016 cohort, 61 percent of scores sent during the September 12- October 11 period were SAT registration score sends, while only 4 percent of score sends in the December 10- January 13 period fell into this category.

[24] The middle tercile of free and reduced-price high schools include those where between 18 and 44 percent of students are eligible for free/reduced-price lunch. The middle tercile of URM high schools includes those where between 14 and 42 percent of students are Black or Hispanic/Latino.

[25] We omit private high schools from these analyses entirely because students at these schools generally are not eligible for free/reduced price lunch and comprehensive data on the racial/ethnic composition of students at these schools are not available.

parameter estimates suggest that students from the wealthiest high schools and those from high schools with low and medium shares of Black and Hispanic/Latino students are more responsive to the College Scorecard median earnings data.

4.3. By Student Academic Ability and Student Demographics

In Table 9, we show the estimates of the Scorecard's impact by student SAT scores.[26] For students with SAT scores below 1100, we find that the median earnings component of the Scorecard led to changes in score send volume that are not distinguishable from zero. By contrast, the parameter estimates on the triple interaction terms including median earnings for students with SAT scores between 1100 and 1390 are similar to the main results in that they are between 0.23 and 0.27 and even larger for students with the highest SAT scores. We estimate that, as a result of the Scorecard's release, an increase of 10 percent in a college's median reported earnings led to a 3 to 4 percent increase in score sends received by those colleges from very high-scoring students.

In Table 10, we separately estimate equation (1) by student race/ethnicity, and parental education and gender. Perhaps the starkest differences in responsiveness presented in this paper unfold when we disaggregate score sends by student race/ethnicity. Among Asian students, score sends increased by 5 percent for each 10 percent increase in a college's median reported earnings in the post-September 12th period as a result of the Scorecard release. Among white students, the corresponding elasticity measure is about half as large at 2.4 percent, and elasticity estimates associated with the median earnings triple interaction terms for Hispanic and Black students are not distinguishable from zero. These results are consistent with the public high school-level analyses in Table 8, in which our models suggest that score sends from students attending high

[26] We construct student SAT scores as the sum of the student's maximum math and critical reading scores across the last 5 sittings.

schools containing large shares of Black and Hispanic students are uninfluenced by the Scorecard release. Disaggregating by the student-reported parental education, we find that parameter estimates on the median earnings interaction terms increase with higher levels of parental education. We find larger parameter estimates on the earnings interaction term for females than for males, though the difference between these two estimates is not significant.

In the final column of Table 10, we determine whether the Scorecard encouraged students to send scores to geographically distant colleges which they otherwise might not have considered. The College Scorecard has the potential to reshape students' college options by highlighting the benefits of colleges that lead to favorable student outcomes. This information holds potential to expand the choice set of students residing in "education deserts", with few postsecondary options (Hillman, 2016). Despite our robust findings on score send volume, we find that none of the Scorecard elements shifted the average geographic distance between students and colleges. In other words, we cannot conclude that the college earnings (for example) shock shifted how far students were willing to travel to college.

Finally, as both robustness and generalizability tests, we re-fit EQ(2) after grouping students into three categories based on whether their home states are SAT-dominant, ACT-dominant or "Mixed," in which there is no clear test preference over the period.[27] Students living in ACT-dominant states generally have fewer selective colleges nearby, and are more likely to live in education deserts. Given that distance from home is a constraint facing many high school students during the college search process, it is possible that students in the ACT-dominant states might be less responsive to the College Scorecard as a result of limited postsecondary options.

[27] In ACT dominant states, the ratio of ACT to SAT test-takers exceeded 2:1 in all seven cohorts. In SAT dominant states, the reverse is true. SAT dominant states include CT, DE, DC, ID, ME, MD, MA, NH, NJ, NY, PA, RI, VT, VA, WA. ACT dominant states include AL, AR, CO, IL, IA, KS, KY, LA, MI, MN, MS, MO, MT, NE, NM, ND, OH, OK, SD, TN, UT, WV, WI, WY. Residency indicates the state in which the student attended high school.

On the other hand, students residing in ACT-dominant states and electing to take the SAT may differ from the typical college-aspiring students in their states, intending to enroll at distant colleges located in SAT-dominant regions. Under this scenario, SAT test-takers in ACT-dominant locations might approach the college search process with greater flexibility, and may be more responsive to the College Scorecard data. We find some suggestive evidence of this. The elasticity parameter estimate on Scorecard median earnings in Appendix Table 4 is about twice as large for students attending high school in ACT-dominant states (0.293) compared to SAT-dominant states (0.152), though these two elasticity estimates do not differ significantly.

5. College Enrollment

In Table 11, we show the results from fitting EQ(1) to student enrollment data. We present results representing a more comprehensive measure of enrollment that incorporates AP, PSAT/NMSQT SAT test-takers (top panel) as well as enrollment only among the SAT test-takers who were eligible to send SAT scores (bottom panel). Although parameter estimates associated with median earnings are positive, the first column of Table 11 shows that we are unable to conclude that the earnings shock influenced overall college enrollment numbers.

In the next six columns of Table 11, we separately estimate EQ(1) for enrollment, by racial/ethnic identity and high school origins. There are a smattering of statistically significant results, which are smaller in magnitude than the score sending impacts shown in Tables 4 and 10. For example, we find some modest evidence that African-American enrollment declined at colleges with higher median earnings on the College Scorecard- about 1.7 percent with each 10 percent increase in median earnings- possibly suggesting a small crowd-out effect. Despite the impressive effects of the Scorecard’s median earnings metric on SAT score sending among Asian students as well as students hailing from private high schools, the estimates of the earnings metrics on enrollment, while positive, do not reach conventional levels of statistical significance for these two groups.[28]

The second to last column shows that the College Scorecard did not shift the distance that students travelled to college, which is expected given the Table 10 result indicating that the Scorecard did not induce students to send scores to colleges farther from home. We do, however, find that the higher earnings colleges experienced a small bump in the average SAT scores of enrolling students. The final column shows that a 10 percent increase in median earnings translated into a 0.17 percent increase in average SAT scores, which roughly corresponds to a 2 point increase on the 1600 scale.

Overall, the impacts of the release of the Scorecard on enrollment are, at most, modest. Unfortunately, our data cannot offer any insights into why the shifts in score sending were unaccompanied by shifts in enrollment. Perhaps the additional applications submitted by these students were not accepted either because the students were not compelling applicants or there are enrollment capacity constraints. Or perhaps students relied on the Scorecard for applications, but were more deliberate during the college choice process such that other factors overshadowed the importance of median earnings.

6. Conclusion

This paper demonstrates that the introduction of the College Scorecard pushed traditionally well-resourced students' score sends towards colleges with higher reported median earnings, and that the other two prominently featured pieces of information (annual cost and graduation rate), which were previously available through a variety of sources, did not measurably influence student demand for colleges. Responding to earnings, but not annual cost,

[28] Interestingly, there is some evidence that the number of private school SAT test-takers increased at higher earnings colleges.

is consistent with Bleemer and Zafar’s (2015) information experiment to U.S. households, though, neither our work nor theirs can pin down the exact mechanism as to why one metric is more important to students than the others.

Although we estimate a clear influence of the earnings data on score sending, in the first cohort of students exposed to the College Scorecard, we find little evidence of shifts in enrollment. Though our data cannot answer this question, future research should disentangle whether the induced applicants are not enrolling because of decisions made by the applicant or by the college. This question extends broadly to all interventions and policy shocks that shift student application behavior, but not enrollment behavior. Nevertheless, the results from the score sending analyses are well-aligned with both the economics and higher education literature, which consistently reference the misinformation and lack of information that lower-income, underrepresented students carry with them into the college search/selection process (Avery, 2010; Avery, 2014; Hoxby and Turner, 2015). In each set of heterogeneity analyses that we conduct, the subgroups of students expected to enter the college search process with the most information and most cultural capital are exactly the students who responded most strongly to the Scorecard. It is unclear from these analyses whether the differential responsiveness to the Scorecard earnings data is due to gaps in awareness of these data between traditionally underrepresented students and overrepresented students or whether lower-income students are less able to adapt their college lists based on this information. Confusion about financial aid processes, particularly at the nation’s most competitive colleges (Avery et al. 2006), may unnecessarily prevent low-income students from adjusting their initial college lists in response to Scorecard-like data. Overall, there exists some risk that differential awareness or utilization of these data may increase postsecondary stratification along racial/ethnic and socioeconomic lines in the future.

The second question brought forth through this research is whether the Scorecard information can serve as an effective accountability tool. Though primarily marketed as a tool to help students make informed college choices, most policymakers and media recognize that there exists a clear accountability component to the Scorecard (Stratford, 2015). The Scorecard calls attention to the metrics that matter, and institutions falling short on outcomes like graduation rates and median earnings, while charging students steep prices for enrolling, may face new pressures to improve upon these prominently featured metrics. In the K-12 space, research has documented real efforts and improvements by schools experiencing the threat of sanctions for not reaching certain benchmarks (e.g. Chiang, 2009; Rockoff and Turner, 2010). However, an unfortunate and well documented byproduct of accountability systems is that they create incentives to game the metrics or even cheat (Aviv, 2014), which only creates the illusion of improvement. Or perhaps, as Bar-Isaac et al. (2008) point out, if college quality is multidimensional and only a few of these dimensions are disclosed and evaluated, colleges may adapt in ways that allow them to improve on reported measures of college quality, perhaps at the expense of unreported measures of college quality.

What might this mean in practice? Carnevale, Cheah and Hanson (2015) show impressive differences in earnings based on choice of college major. Colleges eager to improve the perception of college quality through improved earnings metrics might reallocate resources and student enrollment from education or liberal arts programs into pre-professional courses of study like business or engineering.

Along with the potential for students and colleges to adapt to the Scorecard, the federal government can adjust the Scorecard as they see fit. Even if the current Scorecard metrics remain in place, colleges evolve in terms of priorities and students enrolled. Will the federal government continue updating data annually to capture this evolution? Will they incorporate more or better quality measures of earnings? Will they attempt to adjust the metrics for selection? Will they proactively push the information to underserved populations? These answers may determine the long-term use of the Scorecard and who ultimately relies on the information.

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Figure 1: Relationship Between Scorecard Elements

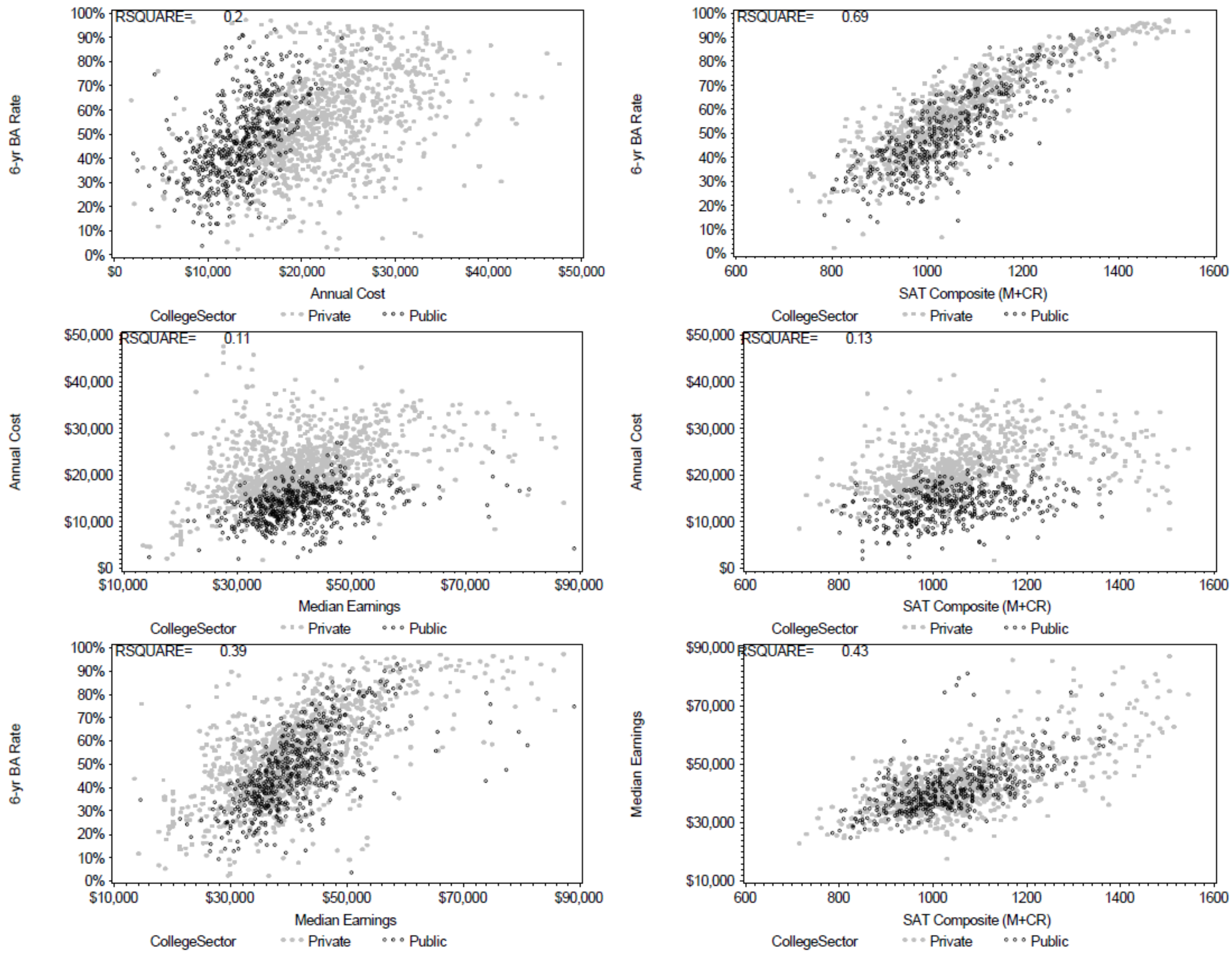
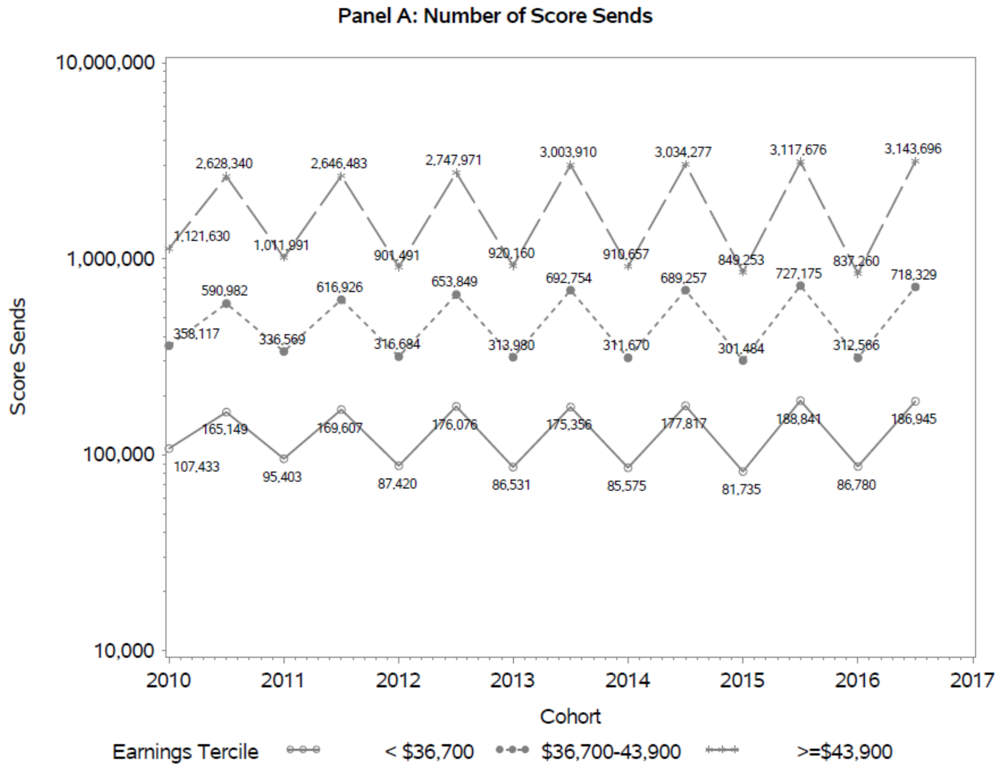


Figure 2: Score Send Volume and Growth Over Time and College Earnings Terciles



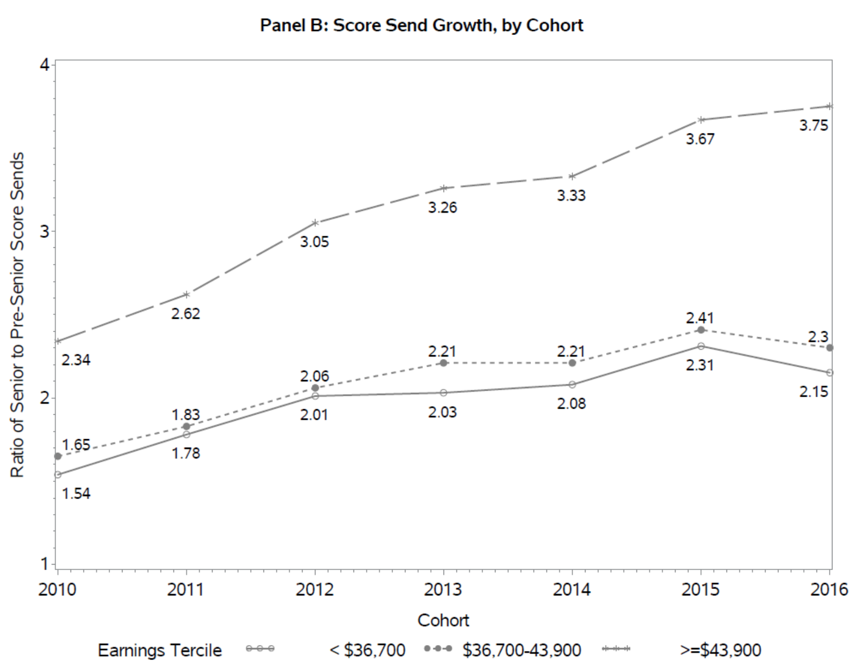


Table 1: Summary Statistics

	Colleges	Average Net Price	Average Median Earnings	6-Yr BA Grad Rate	Avg. Number of Score Sends to College: Through Sept. 11th of Senior Year	Avg. Number of Score Sends to College: After Sept. 11th of Senior Year	Avg. Number of Enrollees: SAT Test-Takers	Avg. Number of Enrollees: PSAT,SAT, AP Test-Takers
Cohort 2010	1576	$20,214	$52,405	72.1%	1,011	2,167	550	743
Cohort 2011	1579	$20,358	$52,354	71.8%	921	2,182	558	748
Cohort 2012	1577	$20,371	$52,289	71.6%	832	2,280	554	748
Cohort 2013	1576	$20,356	$52,565	71.8%	844	2,466	562	772
Cohort 2014	1581	$20,325	$52,614	71.9%	832	2,488	561	760
Cohort 2015	1575	$20,239	$52,481	71.6%	789	2,581	549	757
Cohort 2016	1574	$20,171	$52,483	71.6%	790	2,595	558	782

Notes: The College Scorecard characteristics reflect what is visible to the student. The earnings data reflect 10 year median earnings among students who began at sampled colleges in 2001-2002. The six-year BA completion rate and estimated net price presented to students come from the IPEDS 2013 survey. Score sends come from the College Board administrative data of SAT takers. College enrollment data reflect College Board test-takers and come from the National Student Clearinghouse.

Table 2: Difference-in-Difference Results, Outcome = Log (# of Score Sends)

	All Score Sends	*Score Sends Through Sept. 11th of Senior Year*	*Score Sends After Sept. 11th of Senior Year*
2016 Cohort x log(Median Earnings)	0.215***	0.037	0.270***
	(0.059)	(0.054)	(0.067)
2016 Cohort x log(Graduation Rate)	-0.044	-0.080**	-0.005
	(0.035)	(0.038)	(0.037)
2016 Cohort x log(Annual Cost)	-0.077***	-0.076***	-0.107***
	(0.028)	(0.029)	(0.033)
Observations	11,038	10,971	10,965
R-squared	0.988	0.983	0.986

Notes: Robust standard errors are presented in parentheses and statistical significance is reported as follows: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. All regressions include cohort and college fixed effects. The data come from all SAT takers in the graduating high school cohorts of 2010-2016. An observation is a college-cohort. Median earnings, graduation rates, and net price come from Scorecard and are repeated for all observations. We consider score sends through January 13th of the respective cohort.

Table 3: Increase in Score Sends Over Time

	Colleges in Tercile 1 Median Earnings			*Colleges in Tercile 2 Median Earnings*			*Colleges in Tercile 3 Median Earnings*			Tercile 2/Tercile 1		Tercile 3/Tercile 2		Tercile 3/Tercile 1	
Cohort	*Pre-Senior Score Sends (1)*	*Senior Score Sends (2)*	*Senior to Pre-Senior Ratio = (2)/(1) (3)*	*Pre-Senior Score Sends (4)*	*Senior Score Sends (5)*	*Senior to Pre-Senior Ratio = (5)/(4) (6)*	*Pre-Senior Score Sends (7)*	*Senior Score Sends (8)*	*Senior to Pre-Senior Ratio = (8)/(7) (9)*	Ratio = (6)/(3) *(10)*	% Change *(11)*	Ratio = (9)/(6) *(12)*	% Change *(13)*	Ratio = (9)/(3) *(14)*	% Change *(15)*
2010	107,433	165,149	1.54	358,117	590,982	1.65	1,121,630	2,628,340	2.34	1.07	--	1.42	--	1.52	--
2011	95,403	169,607	1.78	336,569	616,926	1.83	1,011,991	2,646,483	2.62	1.03	-4.0%	1.43	0.5%	1.47	-3.5%
2012	87,420	176,076	2.01	316,684	653,849	2.06	901,491	2,747,971	3.05	1.03	-0.6%	1.48	3.5%	1.51	2.9%
2013	86,531	175,356	2.03	313,980	692,754	2.21	920,160	3,003,910	3.26	1.09	6.2%	1.48	0.2%	1.61	6.4%
2014	85,575	177,817	2.08	311,670	689,257	2.21	910,657	3,034,277	3.33	1.06	-2.2%	1.51	1.8%	1.60	-0.5%
2015	81,735	188,841	2.31	301,484	727,175	2.41	849,253	3,117,676	3.67	1.04	-1.9%	1.52	1.0%	1.59	-0.9%
2016	**86,780**	**186,945**	**2.15**	**312,566**	**718,329**	**2.30**	**837,260**	**3,143,696**	**3.75**	**1.07**	**2.2%**	**1.63**	**7.3%**	**1.74**	**9.7%**

Notes: The College Scorecard earnings data reflect 10 year median earnings among students who began at sampled colleges in 2001-2002. Terciles are unweighted and the first tercile are colleges with median earnings less than $36,700, the second tercile is between $36,700 and $43,900, and the third tercile is greater than or equal to $43,900. Score sends come from the College Board administrative data of SAT takers.

Table 4: Main Results, Outcome = Log (# of Score Sends)

	All Students	*From Public High School*	*From Private High School*	*From Charter High School*
Senior x 2016 Cohort x log(Median Earnings)	0.235**	0.157	0.415***	0.193
	(0.108)	(0.112)	(0.107)	(0.147)
Senior x 2016 Cohort x log(Graduation Rate)	0.065	0.123*	-0.004	0.041
	(0.068)	(0.070)	(0.066)	(0.104)
Senior x 2016 Cohort x log(Annual Cost)	-0.025	-0.028	-0.062	0.063
	(0.057)	(0.060)	(0.058)	(0.081)
Observations	21,936	21,762	20,719	16,010
R-squared	0.969	0.967	0.956	0.882

Notes: Robust standard errors are presented in parentheses and statistical significance is reported as follows: *** p<0.01, ** p<0.05, * p<0.1. All regressions include cohort and college fixed effects and a dummy variable for *PostPeriod* . The data come from all SAT takers in the graduating high school cohorts of 2010-2016. An observation is a college-cohort-period and the number of score sends for a college is aggregated across students in the cohort and period where period is either before or on/after September 12th of the students' senior years. Median earnings, graduation rates, and net price come from Scorecard and are repeated for all observations. We consider score sends through January 13th of the respective cohort.

Table 5: Score Sending Prior to Senior Year (Before Sept. 12th) and during Senior Year (After Sept. 11th), by Cohort
Outcome = Log (# of Score Sends)

	All Students, High School Cohort:						
Cohort:	*2010*	*2011*	*2012*	*2013*	*2014*	*2015*	*2016*
Senior x log(Median Earnings)	0.593***	0.585***	0.626***	0.737***	0.611***	0.649***	0.823***
	(0.090)	(0.093)	(0.090)	(0.087)	(0.092)	(0.093)	(0.110)
Senior x log(Graduation Rate)	0.167***	0.131**	0.169***	0.175***	0.257***	0.225***	0.279***
	(0.057)	(0.059)	(0.054)	(0.056)	(0.057)	(0.056)	(0.071)
Senior x log(Annual Cost)	0.195***	0.242***	0.241***	0.186***	0.181***	0.169***	0.173***
	(0.053)	(0.054)	(0.051)	(0.050)	(0.052)	(0.053)	(0.060)
Observations	3,132	3,141	3,138	3,134	3,140	3,125	3,126
R-squared	0.980	0.980	0.979	0.981	0.978	0.978	0.974
	Private High School Students						
Senior x log(Median Earnings)	0.865***	0.727***	0.830***	0.818***	0.816***	0.903***	1.233***
	(0.109)	(0.107)	(0.105)	(0.114)	(0.109)	(0.104)	(0.111)
Senior x log(Graduation Rate)	0.302***	0.324***	0.308***	0.416***	0.364***	0.333***	0.299***
	(0.078)	(0.068)	(0.072)	(0.083)	(0.066)	(0.065)	(0.071)
Senior x log(Annual Cost)	0.337***	0.353***	0.346***	0.315***	0.332***	0.331***	0.286***
	(0.065)	(0.063)	(0.058)	(0.064)	(0.064)	(0.067)	(0.061)
Observations	2,973	2,977	2,957	2,972	2,939	2,959	2,942
R-squared	0.972	0.973	0.971	0.972	0.971	0.971	0.969

Notes: Robust standard errors are presented in parentheses and statistical significance is reported as follows: *** p<0.01, ** p<0.05, * p<0.1. All regressions include cohort and college fixed effects and a dummy variable for whether the score sends were sent during the student's senior year (September 12th or after). The data come from all SAT takers in the graduating high school cohorts of 2010-2016. An observation is a college-cohort-period and the number of score sends for a college is aggregated across students in the cohort and period where period is either before or on/after September 12th of the students' senior years. Median earnings, graduation rates, and net price come from Scorecard and are repeated for all observations. We consider score sends through January 13th of the respective cohort.

Table 6: Placebo Tests, Outcome = Log (# of Score Sends)

	All Students					
Cohort:	*2010*	*2011*	*2012*	*2013*	*2014*	*2015*
Senior x 201X Cohort x log(Median Earnings)	-0.053	-0.062	-0.018	0.118	-0.007	0.021
	(0.091)	(0.087)	(0.084)	(0.082)	(0.086)	(0.092)
Senior x 201X Cohort x log(Graduation Rate)	-0.026	-0.063	-0.024	-0.011	0.080	0.048
	(0.060)	(0.055)	(0.051)	(0.051)	(0.055)	(0.059)
Senior x 201X Cohort x log(Annual Cost)	-0.005	0.043	0.043	-0.014	-0.022	-0.046
	(0.051)	(0.047)	(0.046)	(0.046)	(0.047)	(0.051)
Observations	18,810	18,810	18,810	18,810	18,810	18,810
R-squared	0.971	0.971	0.971	0.971	0.971	0.971
	Private High School Students					
Senior x 201X Cohort x log(Median Earnings)	0.059	-0.113	-0.031	-0.017	0.021	0.081
	(0.104)	(0.099)	(0.099)	(0.103)	(0.100)	(0.100)
Senior x 201X Cohortx log(Graduation Rate)	-0.050	-0.021	-0.030	0.114	-0.002	-0.008
	(0.071)	(0.064)	(0.068)	(0.075)	(0.063)	(0.066)
Senior x 201X Cohort x log(Annual Cost)	0.018	0.014	0.025	-0.031	-0.010	-0.017
	(0.060)	(0.057)	(0.053)	(0.056)	(0.057)	(0.062)
Observations	17,777	17,777	17,777	17,777	17,777	17,777
R-squared	0.958	0.958	0.958	0.958	0.958	0.958

Notes: Robust standard errors are presented in parentheses and statistical significance is reported as follows: *** p<0.01, ** p<0.05, * p<0.1. All regressions include college fixed effects and a dummy variable for *Senior*. The data come from all SAT takers in the graduating high school cohorts of 2010-2015. An observation is a college-cohort-period and the number of score sends for a college is aggregated across students in the cohort and period where period is either before or on/after September 12th of the students' senior years. Median earnings, graduation rates, and net price come from Scorecard and are repeated for all observations. We consider score sends through January 13th of the respective cohort. For each regression, we use a placebo treatment year, denoted by *201XCohort,* and show estimates of the placebo impact (the triple interaction terms).

Table 7: Scorecard Impacts by Score Send Dates, Outcome = Log (# of Score Sends)

	All Students			
Post Period Interval:	*Sept. 12-Oct. 11*	*Oct. 12-Nov. 10*	*Nov. 11-Dec. 10*	*After Dec. 10*
Senior x 2016Cohort x log(Median Earnings)	0.092	0.102	0.071	0.414***
	(0.095)	(0.105)	(0.118)	(0.130)
Senior x 2016Cohortx log(Graduation Rate)	0.099*	0.088	0.012	0.071
	(0.059)	(0.065)	(0.079)	(0.086)
Senior x 2016Cohort x log(Annual Cost)	-0.097*	0.008	-0.072	-0.007
	(0.050)	(0.056)	(0.065)	(0.073)
Observations	21,650	21,736	21,626	21,605
R-squared	0.973	0.967	0.956	0.947
	Private High School Students			
Senior x 2016Cohort x log(Median Earnings)	0.135	0.308***	0.204*	0.495***
	(0.101)	(0.105)	(0.123)	(0.134)
Senior x 2016Cohortx log(Graduation Rate)	0.030	-0.022	-0.048	-0.059
	(0.066)	(0.068)	(0.080)	(0.079)
Senior x 2016Cohort x log(Annual Cost)	0.024	0.087	-0.062	0.016
	(0.054)	(0.056)	(0.067)	(0.071)
Observations	19,533	19,892	19,810	19,732
R-squared	0.954	0.951	0.936	0.924

Notes: Robust standard errors are presented in parentheses and statistical significance is reported as follows: *** p<0.01, ** p<0.05, * p<0.1. All regressions include cohort and college fixed effects and a dummy variable for *PostPeriod* . The data come from all SAT takers in the graduating high school cohorts of 2010-2016. An observation is a college-cohort-period and the number of score sends for a college is aggregated across students in the cohort and period where period is either before September 12th of the students' senior years or in the 30 day post-period time frame. Median earnings, graduation rates, and net price come from Scorecard and are repeated for all observations. We consider score sends through January 13th of the respective cohort.

Table 8: Results by Students from High Schools With Differing Fractions of Under-Represented Minority and Free and Reduced-Price Lunch Eligibility
Outcome = Log (# of Score Sends)

	From High School with Low URM	*From High School with Medium URM*	*From High School with High URM*	*From High School with Low FRL*	*From High School with Medium FRL*	*From High School with High FRL*
Senior x 2016 Cohort x log(Median Earnings)	0.214*	0.240**	0.008	0.211*	0.124	0.053
	(0.114)	(0.118)	(0.123)	(0.114)	(0.119)	(0.120)
Senior x 2016 Cohort x log(Graduation Rate)	0.049	0.066	0.140*	0.080	0.062	0.104
	(0.074)	(0.070)	(0.074)	(0.075)	(0.072)	(0.074)
Senior x 2016 Cohort x log(Annual Cost)	0.010	-0.002	0.024	-0.015	0.067	0.046
	(0.063)	(0.064)	(0.064)	(0.065)	(0.062)	(0.063)
Observations	20,518	20,869	21,021	20,123	21,061	21,267
R-squared	0.961	0.958	0.953	0.960	0.959	0.952

Notes: Robust standard errors are presented in parentheses and statistical significance is reported as follows: *** p<0.01, ** p<0.05, * p<0.1. All regressions include cohort and college fixed effects and a dummy variable for *Senior*. The data come from all SAT takers in the graduating high school cohorts of 2010-2016. An observation is a college-cohort-period and the number of score sends for a college is aggregated across students in the cohort and period where period is either before or on/after September 12th of the students' senior years. Median earnings, graduation rates, and net price come from Scorecard and are repeated for all observations. We consider score sends through January 13th of the respective cohort. The middle tercile of under-represented minority students includes observations from high schools where between 13.98 percent and 42.22 percent of students are Black or Hispanic according to the 2013-2014 Common Core of Data (CCD). The middle tercile of free/reduced price lunch eligibility is also defined using data from the 2013-2014 CCD. This tercile is bounded by observations from high schools where eligibility for free/reduced-price lunch ranges from 18.07 percent to 44.24 percent.

Table 9: Results by Student SAT Scores, Outcome = Log (# of Score Sends)

	Student Composite SAT (Math+Critical Reading)							
	<900	*900-990*	*1000-1090*	*1100-1190*	*1200-1290*	*1300-1390*	*1400-1490*	*1500+*
Senior x 2016 Cohort x log(Median Earnings)	0.111	0.053	0.146	0.260**	0.273**	0.232*	0.370**	0.335*
	(0.115)	(0.118)	(0.119)	(0.110)	(0.118)	(0.129)	(0.158)	(0.184)
Senior x 2016 Cohort x log(Graduation Rate)	0.184**	0.102	0.065	0.051	-0.019	0.082	-0.095	0.167
	(0.072)	(0.069)	(0.071)	(0.070)	(0.074)	(0.088)	(0.111)	(0.149)
Senior x 2016 Cohort x log(Annual Cost)	-0.039	-0.047	-0.031	-0.078	-0.004	0.016	0.103	0.232**
	(0.060)	(0.061)	(0.063)	(0.060)	(0.065)	(0.073)	(0.080)	(0.091)
Observations	21,327	21,082	20,986	20,522	19,735	18,259	15,618	11,038
R-squared	0.942	0.948	0.954	0.957	0.956	0.952	0.947	0.942

Notes: Robust standard errors are presented in parentheses and statistical significance is reported as follows: *** p<0.01, ** p<0.05, * p<0.1. All regressions include cohort and college fixed effects and a dummy variable for *Senior*. The data come from all SAT takers in the graduating high school cohorts of 2010-2016. An observation is a college-cohort-period and the number of score sends for a college is aggregated across students in the cohort and period where period is either before or after/on September 12th of the students' senior years. Median earnings, graduation rates, and net price come from Scorecard and are repeated for all observations. We consider score sends through January 13th of the respective cohort. Student SAT scores are constructed as the sum of the maximum critical reading and math scores across the student's last five exam attempts.

Table 10: Results by Student Demographics

	Student Race/Ethnicity: Outcome = Log (# of Score Sends)				Parental Education: Outcome= Log (# of Score Sends)					Student Gender: Outcome=Log (# of Score Sends)		Student Residency: Outcome= Log(Miles) between student and score send college
	Asian	*Black*	*Hispanic*	*White*	*No Response*	*Less than HS Degree*	*HS Degree*	*Some College*	*BA or Higher*	*Male*	*Female*	
Senior x 2016 Cohort x log(Median Earnings)	0.506***	0.068	0.045	0.240**	0.216*	0.017	0.104	0.202*	0.220**	0.142	0.211**	0.036
	(0.134)	(0.124)	(0.121)	(0.114)	(0.128)	(0.142)	(0.122)	(0.112)	(0.109)	(0.113)	(0.104)	(0.123)
Senior x 2016 Cohort x log(Graduation Rate)	-0.001	0.065	0.064	0.109	0.146*	0.049	0.096	0.114	0.043	0.193***	0.030	-0.092
	(0.090)	(0.073)	(0.080)	(0.070)	(0.079)	(0.090)	(0.080)	(0.072)	(0.068)	(0.071)	(0.066)	(0.073)
Senior x 2016 Cohort x log(Annual Cost)	-0.046	0.000	-0.063	-0.008	-0.042	-0.074	-0.017	-0.024	-0.002	-0.030	-0.044	0.099*
	(0.073)	(0.062)	(0.065)	(0.062)	(0.077)	(0.074)	(0.064)	(0.060)	(0.057)	(0.058)	(0.057)	(0.059)
Observations	18,619	20,165	20,167	21,248	19,760	17,853	20,155	21,221	21,667	21,442	21,738	21,874
R-squared	0.951	0.944	0.945	0.967	0.921	0.917	0.941	0.954	0.968	0.964	0.964	0.833

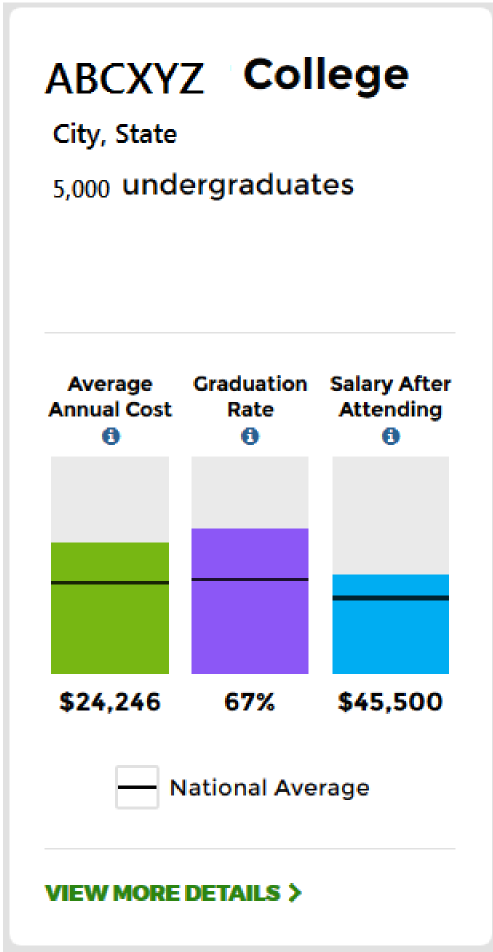
Notes: Robust standard errors are presented in parentheses and statistical significance is reported as follows: *** p<0.01, ** p<0.05, * p<0.1. All regressions include cohort and college fixed effects and a dummy variable for *Senior*. The data come from all SAT takers in the graduating high school cohorts of 2010-2016. An observation is a college-cohort-period and the number of score sends for a college is aggregated across students in the cohort and period where period is either before or on/after September 12th of the students' senior years. Median earnings, graduation rates, and net price come from Scorecard and are repeated for all observations. We consider score sends through January 13th of the respective cohort. Parental Education is defined as the maximum parental education earned across both parents.

Table 11: Difference-in-Difference Results, Enrollment

	PSAT/NMSQT, SAT, and AP Test-Takers								
	Log (Enrollment)	*Log(Asian Enrollment)*	*Log(African-American Enrollment)*	*Log(Hispanic Enrollment)*	*Log (White Enrollment)*	*Log (Public/Charter High School Enrollment)*	*Log (Private High School Enrollment)*	*Log (Distance Travelled to College)*	*Log (Average SAT Scores of Enrollees)*
2016 Cohort x log(Median Earnings)	0.079	0.081	-0.172**	-0.071	-0.012	0.060	0.097	-0.094	0.017**
	(0.063)	(0.072)	(0.075)	(0.064)	(0.052)	(0.066)	(0.059)	(0.060)	(0.007)
2016 Cohort x log(Graduation Rate)	0.056	0.109**	0.126**	0.065	0.043	0.063	-0.031	0.031	-0.007
	(0.061)	(0.054)	(0.056)	(0.046)	(0.045)	(0.065)	(0.041)	(0.058)	(0.006)
2016 Cohort x log(Annual Cost)	-0.026	0.005	0.031	-0.007	-0.041	-0.040	0.013	0.003	-0.003
	(0.028)	(0.037)	(0.032)	(0.031)	(0.027)	(0.030)	(0.028)	(0.031)	(0.003)
Observations	9,892	9,017	9,657	9,674	9,730	9,887	9,774	9,891	9,838
R-squared	0.968	0.958	0.943	0.953	0.978	0.970	0.962	0.915	0.954
	SAT Test-Takers Only								
2016 Cohort x log(Median Earnings)	0.100*	0.055	-0.130*	-0.002	0.020	0.054	0.204***	-0.056	0.017**
	(0.057)	(0.076)	(0.074)	(0.065)	(0.054)	(0.060)	(0.066)	(0.069)	(0.007)
2016 Cohort x log(Graduation Rate)	0.018	0.173***	0.114**	0.043	0.022	0.038	-0.046	0.039	-0.007
	(0.052)	(0.054)	(0.054)	(0.044)	(0.041)	(0.056)	(0.047)	(0.057)	(0.006)
2016 Cohort x log(Annual Cost)	-0.026	-0.049	0.038	-0.017	0.007	-0.023	-0.022	-0.017	-0.003
	(0.027)	(0.038)	(0.035)	(0.033)	(0.028)	(0.030)	(0.032)	(0.037)	(0.003)
Observations	9,838	8,472	9,372	9,339	9,608	9,810	9,470	9,836	9,838
R-squared	0.975	0.959	0.944	0.954	0.978	0.976	0.961	0.917	0.954

Notes: Robust standard errors are presented in parentheses and statistical significance is reported as follows: *** p<0.01, ** p<0.05, * p<0.1. All regressions include cohort and college fixed effects. An observation is a college-cohort. Sampled colleges include those which also appear in our score send analyses. Enrollment data reflect only students who participated in a College Board program and entered a postsecondary institution on-time (within 180 days of HS graduation).

Appedix Figure 1: Example Screen Shot from College Score Card Website



Note: Appearance differs slightly from actual Scorecard due to redaction of college's name, city, and enrollment.

Appendix Table 1: Student-Level Summary Statistics

	Mean	*Std. Dev.*	*Min*	*Max*
White	0.533	0.499	0.000	1.000
Asian	0.128	0.334	0.000	1.000
Black	0.104	0.305	0.000	1.000
Hispanic	0.147	0.354	0.000	1.000
Male	0.447	0.497	0.000	1.000
Female	0.552	0.497	0.000	1.000
Parental Education - No Response	0.070	0.256	0.000	1.000
Parental Education - Less Than HS Degree	0.047	0.212	0.000	1.000
Parental Education - HS Degree	0.096	0.295	0.000	1.000
Parental Education - Some College	0.199	0.399	0.000	1.000
Parental Education - BA or Higher	0.586	0.492	0.000	1.000
From Public High School	0.782	0.413	0.000	1.000
From Private High School	0.164	0.370	0.000	1.000
From Charter High School	0.017	0.129	0.000	1.000
From Low-URM High School	0.251	0.434	0.000	1.000
From Medium-URM High School	0.261	0.439	0.000	1.000
From High-URM High School	0.270	0.444	0.000	1.000
From Low-FRL High School	0.230	0.421	0.000	1.000
From Medium-FRL High School	0.274	0.446	0.000	1.000
From High-FRL High School	0.278	0.448	0.000	1.000
SAT Score	1098.753	204.429	400.000	1600.000
Number of Score Sends	5.717	4.635	1.000	148.000
Number of Score Sends Before September 12th	1.512	2.803	0.000	109.000
Number of Score Sends On or After September 12th	4.206	4.296	0.000	129.000
Average Graduation Rate of Score Sends	0.676	0.147	0.022	0.972
Average Annual Cost of Score Sends	$ 19,228.307	$ 5,543.765	$ 1,776.000	$ 47,611.000
Average Median Earnings of Score Sends	$ 50,044.080	$ 9,044.681	$ 14,100.000	$ 116,400.000

Notes: The data come from 6,241,695 SAT takers in the graduating high school cohorts of 2010-2016. Data on racial composition of high schools and free/reduced price lunch eligibility apply only to public and charter schools. The middle tercile of under-represented minority students includes observations from high schools where between 13.98 percent and 42.22 percent of students are Black or Hispanic according to the 2013-2014 Common Core of Data (CCD). The middle tercile of free/reduced price lunch eligibility is also defined using data from the 2013-2014 CCD. This tercile is bounded by observations from high schools where eligibility for free/reduced-price lunch ranges from 18.07 percent to 44.24 percent.

Appendix Table 2: Alternative Specifications, Outcome = # of Score Sends

	All Students	*From Public High School*	*From Private High School*	*From Charter High School*
Senior x 2016 Cohort x log(Median Earnings)	1,260.635***	903.127***	292.503*	62.417***
	(480.796)	(311.034)	(163.223)	(14.357)
Senior x 2016 Cohort x log(Graduation Rate)	355.338*	243.009*	57.548	10.553
	(213.470)	(142.422)	(68.840)	(8.335)
Senior x 2016 Cohort x log(Annual Cost)	-289.155	-227.271	-27.877	-17.775**
	(233.440)	(159.645)	(70.452)	(8.943)
Observations	21,936	21,762	20,719	16,010
R-squared	0.838	0.857	0.771	0.797
	Panel B			
Senior x 2016 Cohort x Median Earnings	0.024*	0.017**	0.005	0.001***
	(0.013)	(0.008)	(0.004)	(0.000)
Senior x 2016 Cohort x Graduation Rate	1,469.876**	1,057.583**	312.016	41.553**
	(643.508)	(429.452)	(197.294)	(20.467)
Senior x 2016 Cohort x Annual Cost	-0.023	-0.018*	-0.004	-0.001**
	(0.015)	(0.010)	(0.005)	(0.000)
Observations	21,936	21,762	20,719	16,010
R-squared	0.849	0.864	0.792	0.798

Notes: Robust standard errors are presented in parentheses and statistical significance is reported as follows: *** p<0.01, ** p<0.05, * p<0.1. All regressions include cohort and college fixed effects and a dummy variable for *Senior*. The data come from all SAT takers in the graduating high school cohorts of 2010-2016. An observation is a college-cohort-period and the number of score sends for a college is aggregated across students in the cohort and period where period is either before or on/after September 12th of the students' senior years. Median earnings, graduation rates, and net price come from Scorecard and are repeated for all observations. We consider score sends through January 13th of the respective cohort.

Appendix Table 3: Robustness Tests, Outcome = Log (# of Score Sends)

	Balanced Panel	*Using Earliest Score Send to Each College*	*Using Latest Score Send to Each College*
Senior x 2016 Cohort x log(Median Earnings)	0.193*	0.232**	0.227**
	(0.108)	(0.109)	(0.110)
Senior x 2016 Cohort x log(Graduation Rate)	0.103	0.073	0.044
	(0.068)	(0.069)	(0.069)
Senior x 2016 Cohort x log(Annual Cost)	-0.029	-0.024	-0.026
	(0.058)	(0.058)	(0.058)
Observations	21,210	21,934	21,924
R-squared	0.967	0.967	0.965

Notes: Robust standard errors are presented in parentheses and statistical significance is reported as follows: *** p<0.01, ** p<0.05, * p<0.1. All regressions include cohort and college fixed effects and a dummy variable for *Senior*. The data come from all SAT takers in the graduating high school cohorts of 2010-2016. An observation is a college-cohort-period and the number of score sends for a college is aggregated across students in the cohort and period where period is either before or on/after September 12th of the students' senior years. Median earnings, graduation rates, and net price come from Scorecard and are repeated for all observations. We consider score sends through January 13th of the respective cohort.

Appendix Table 4: Results by Dominant College Entrance Exam, Outcome = Log (# of Score Sends)

	SAT dominant	*ACT dominant*	*Mixed*
log(Median Earnings)	0.152	0.293**	0.187*
	(0.115)	(0.120)	(0.111)
Senior x 2016 Cohort x log(Graduation Rate)	0.113	0.016	0.050
	(0.077)	(0.084)	(0.067)
Senior x 2016 Cohort x log(Annual Cost)	-0.013	-0.036	-0.023
	(0.065)	(0.062)	(0.057)
Observations	20,193	18,538	21,686
R-squared	0.969	0.942	0.965

Notes: Robust standard errors are presented in parentheses and statistical significance is reported as follows: *** p<0.01, ** p<0.05, * p<0.1. SAT dominant states include those where the ratio of SAT test-takers to ACT test-takers exceeded 2:1 over the entire 7 cohort period. In ACT dominant states, the ratio of ACT to SAT test-takers exceeded 2:1 in all seven cohorts. SAT dominant states include CT, DE, DC, ID, ME, MD, MA, NH, NJ, NY, PA, RI, VT, VA, WA. ACT dominant states include AL, AR, CO, IL, IA, KS, KY, LA, MI, MN, MS, MO, MT, NE, NM, ND, OH, OK, SD, TN, UT, WV, WI, WY. Residency indicates the state in which the student attended high school.