Pricing, Income and College Major Choice *

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Job Market Paper †

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Abstract

This paper studies the implications of short-term costs imposed by pricing structures on college major choice and the role of financial constraints. I examine the effect of major-specific pricing policies on major choice and on the distribution of low-income students across majors. Using rich student-level administrative data from the University of Wisconsin-Madison, I implement a difference-in-difference strategy that exploits the introduction of a surcharge policy in the Engineering and Business programs. I find that raising the program specific tuition by $1000 (11%) decreases the probability of graduating in Business by 33% and in Engineering by 12% and that this is driven by the response of low-income students. I then exploit this price variation to identify the labor market returns for these majors. Using these estimates, I find that students are highly responsive to prices despite large earnings losses from switching majors. Motivated by the empirical evidence, I develop and estimate a structural model of college major choice that quantifies the importance of direct price effects and credit constraints. The model estimates suggest that credit constraints rationalize the sensitivity of students to changes in pricing structures. Complementing price differentials with expansions of borrowing limits and means-tested subsidies can minimize the distortion created by pricing. Relaxing borrowing limits allows students to borrow against future income to pay higher tuition, and means-tested grants decrease the net price of the major for low-income students.

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1 Introduction

Access to higher education is a key policy measure to improve intergenerational mobility due to the increasing earnings premium associated with a college degree (Oreopoulos and Petronijevic, 2013). More recently, the "type" of college education or college major choice has been established as an important determinant of earnings (Berger, 1988; Arcidiacono, 2004; Gemici and Wiswall, 2014; Kirkeboen et al., 2016; Hastings et al., 2013), and has been at the center of work on heterogeneous human capital. Although a vast body of work has focused on understanding the distributional consequences of pricing and subsidy policies on college choice (Bettinger, 2004; Dynarski, 2008; Goldrick-Rab et al., 2016; Kane, 2006), the sensitivity of major choice to tuition and financial aid policies remains relatively unexplored, particularly for low-income students. The answer to this question is important as short term costs imposed by pricing structures can potentially influence socio-economic gaps through major sorting. If low-income students are sorting into low-earnings majors, this may have implications for their socio-economic mobility (Patnaik et al., 2020). The earnings capability of students is of particular concern to policy makers due to growing student debt in the past decade.

This paper examines the relationship between major-specific pricing, family income, and college major choice. I study the consequences of a particular form of pricing, namely major-specific pricing. Against the backdrop of declining state funding for public universities in the United States, an increasing number of universities have adopted major-specific pricing. Out of 143 public research universities, 83 universities have adopted surcharge policies, typically in Engineering and Business Schools (Wolniak et al., 2018). The surcharges vary in their amount across universities: ranging from 40 dollars to 1,896 dollars, or 2%-60% of resident base tuition (Nelson, 2008). The higher tuition is intended to price in higher instructional costs as well as high future earnings (Altonji and Zimmerman, 2017; Stange, 2015). Despite the fact that many universities employ a surcharge to correct for cross-subsidization across majors due to uniform pricing, many use subsidies as financial incentives to encourage students to select certain majors. For instance, Florida and Ohio have employed subsidies tied to declaring a major in STEM fields to encourage students to major in STEM.

This paper addresses the following questions. First, I study how major-specific pricing affects college major choice and any unintended consequences of pricing on other academic outcomes. Second, I examine how students sort by income into majors and whether higher prices decrease access to these majors for low-income students. Third, I explore the mechanisms behind these responses. Employing this price variation, I estimate the labor market returns for majors and the earnings loss for students who switch their majors. I then examine the relative importance of pecuniary and non-pecuniary considerations and the role of credit constraints in major choice. Based on our understanding of these mechanisms, I explore the responses to combinations of alternative pricing, subsidies and borrowing policies.

I answer these questions using the case of University of Wisconsin-Madison, where major-specific pricing was introduced in the Business and Engineering programs. The policy introduced

a flat surcharge for all students who declared a major in Business in 2007. A similar surcharge was introduced for Engineering in 2008. I implement a difference-in-difference strategy that employs two sources for variation. First, I rely on variation in exposure to the policy reform across admission cohorts. Next, to account for underlying differences across admission cohorts, I exploit variation based on family income in the impact of the flat surcharge. I use rich student level administrative data that includes student demographics and detailed transcript information. Similar to previous studies, I can observe the income of a smaller group of students who file for financial aid (the "FAFSA population"). However, for the full sample, I observe several characteristics that are strongly correlated with income, such as out-of-state status. My methodology relies on differences in the tuition schedule and income between the non-resident population (*low-treatment intensity*) and resident population (*high-treatment intensity*). I then verify that the differences between these populations are due to differences in income.

The results from the difference-in-difference estimator indicate that an increase in the relative tuition of 1,000 dollars (11 percent) decreases the probability of graduating in both Engineering and Business by 1 p.p. (12 percent) and 4 p.p. (33 percent) respectively. I empirically assess this strategy by verifying that these patterns are driven by differences in income and not by other underlying differences between the two groups. I find that that these results are robust to several proxies of income status such as zip code income, as well as unmet need (price net of aid received). Davis et al. (2019) in their paper find that differential tuition are often hidden costs and that this information is not available to students when they make the decision to go to college. Therefore it is unlikely to influence student's extensive margin to choose UW-Madison. I also test this empirically and find that the responses are robust to controlling for average tuition in students' home states. I then conduct a series of tests to provide further evidence that these are demand-side responses by ruling out supply-side channels such as changes in GPA requirements and program quality. A placebo test confirms that there is no change in differences between residents and non-residents in other majors. I provide further evidence that these are demand side responses by replicating similar effects on a measure of intended major created by using rich information on course-taking behavior.

The price elasticity of major choice, in addition to being a policy-relevant parameter, also provides identifying variation to recover other parameters of interest. First, I propose a novel instrument, in the form of the price change policies, to identify the labor market returns for college majors. Using unique survey information on initial career earnings, linked to administrative data, I implement this instrumental variable strategy. I find that students lose substantial initial earnings of 20,776 dollars (in 2003 dollars) from switching away from these high-return majors,and these losses are even larger in terms of projected lifetime earnings. This is in line with the literature that finds a low responsive of major choice to earnings(Altonji, 1993; Montmarquette et al., 2002; Beffy et al., 2012; Wiswall and Zafar, 2015), although in this case it could be due to either direct effects or through credit constraints. I then conduct a cost-benefit analysis using the causal estimates from the difference-in-difference estimator and the instrumental variable estimator. This analysis suggests that the short-term revenue gains for the university are accompanied by long-term consequences

on earnings and tax revenue losses to state.

Because canonical models of human capital (Becker et al., 1980) cannot explain why individuals respond to prices despite large earnings losses, I next develop a model of college major choice which considers direct price effects and frictions in the form of credit constraints. The estimates of the model guide policy recommendations, which are quite different depending on the relative importance of tastes, direct price effects and credit constraints. In the model, I incorporate elements from models of college major choice (Altonji et al., 2016a) and borrowing constraints in higher education (Abbott et al., 2019). In this model, students consider the earnings gain from majors net of the costs of specializing in the major and non-pecuniary preferences over majors. Students are constrained by borrowing limits and GPA restrictions which reflects the current institutional environment. I estimate this model using a simulated method of moments approach in which I match shares of students in each major, overall and conditional on income and measures of ability. The quasi-experimental variation in prices provides additional identifying variation to recover the underlying preference parameters, particularly the relative importance of non-wage considerations and borrowing constraints. The borrowing behavior is also identified by matching moments on borrowing by income and by exploiting borrowing limits existing in the federal student loan system.

To validate the predictive power of the model for pricing experiments, I simulate the difference-in-difference estimator, an unmatched parameter using the model estimates and replicate the reduced- form effect [1]. Using the model estimates, I study the responsiveness of students to a combination of policy levers such as need-based aid and borrowing-based policies. First, I explore a regime of proportional pricing where the surcharge is proportional to the base tuition. This policy charges a higher price to out-of-state students who pay a substantially higher base tuition and are a less responsive group relative to residents. I find that in the presence of borrowing constraints, students are sensitive to changes in amount of the proportional prices of majors and that the magnitude of these responses is marginally higher in the case of subsidies than surcharges.

Next, I explore a different regime of cross-subsidization where low-income students are provided means-tested grants, in addition to facing differential prices. Here I find that sufficiently subsidizing low-income students may result in an increase in the share of low-income students in a major. In the most extreme case, a full subsidy to low- income students would increase the share of low-income students in Business by approximately 11 percent relative to the case with no subsidies. Finally, I use the model to quantify the importance of borrowing constraints on college major choice, something that the literature has not addressed so far. If I relax borrowing limits, these pricing policies can be implemented with a minimal distortion in the share of students in these majors. In the scenario where we have both price differential and borrowing limits, the share of low-income students in business is approximately 12 percent higher than the case with no price differentials and borrowing limit. With no borrowing limits, the scenario that implements

[1]This approach is in the spirit of recent work (Attanasio et al., 2012; Chaparro et al., 2020; Todd and Wolpin, 2006) validating structural models by simulating the treatment effect of experiments. In this case, I am applying their approach to replicate the natural experiment created by the policy change.

differential prices sees a negligible change relative to the case of equal pricing. These findings suggest that universities can combine pricing policies that raise revenue with additional financial aid policies and borrowing policies to minimize the distortion created by price differentials.

Contribution. This paper brings together the literature on the impact of costs of higher education with a parallel stream of research that studies the determinants of college major choice. My paper is one of the first papers about the effect of tuition policy on the distribution of low-income students across majors. To my knowledge, it is also the first to incorporate credit constraints and study the relationship between major choice, family income and borrowing decisions. This allows me to study income sorting and contribute to the literature on sorting into college majors, that has largely focused on ability sorting. Patnaik et al. (2020) in their recent review highlight major choice by socio-economic status such as income as a key area for future research. Additionally, this paper contributes to a vast body of work that studies the returns to education and more recently, the returns to college major. I propose a new instrument to estimate the returns to college major in the form of changes in prices.

First, I add to the literature on tuition and schooling choice by examining the effects on pricing, aid and borrowing policies on college major choice. Most of the literature on tuition policy and financial incentives has focused on the responsiveness of college enrollment, persistence, and completion decisions (Bettinger, 2004; Dynarski, 2008; Goldrick-Rab et al., 2016; Kane, 2006; Langelett et al., 2015; Turner, 2004; Scott-Clayton, 2011; Scott-Clayton and Zafar, 2019). I add to this body of work by examining the responsiveness of college majors to differential costs as well as combinations of tuition, aid and borrowing policies. There is a small body of work that has found mixed effects of major specific pricing policies. While Stange (2015) finds a decline in the shares of students declaring Engineering and Business majors in response to higher prices, Evans (2017) and Denning and Turley (2017) find mixed evidence on the effects of financial incentives for low-income students tied to pursuing STEM majors. Similarly, Castleman et al. (2018) finds that unconditional subsidies [2] result in students choosing more STEM credits. In contrast to this, Andrews and Stange (2019) find that higher prices result in an increase in the share of Low Income students and rationalize this by the provision of aid to low-income students to defray these costs. If low-income students are the most price sensitive students (Jacob et al., 2018), it is interesting that policies of pricing and subsidies would generate different responses and therefore merit a careful study of pricing structures.

I add to this literature by analyzing how pricing policies influence income sorting, with a focus on low-income students. The context of a specific university allows a careful examination of responses while controlling for other supply side effects that may potentially confound the estimates. I also look at the effects of pricing on a rich set of academic outcomes, some of which have not been previously examined such as academic performance, ability sorting and intention to major. Further, using a structural model, I simulate pricing schemes that reflect the key differences in the environment in earlier work. This helps rationalize some of these differences and generalize the findings to a variety of pricing, aid and borrowing policies. Although earlier work has evaluated

[2] subsidies that are not tied to the major

policies on pricing and subsidies, the literature is silent on the interaction of these policies with borrowing. I find the first evidence that the response to pricing of college majors depends critically on the borrowing avenues available to students.

Second, my paper also speaks to a large literature which has studied the role of family income on college attendance, (Hayashi et al., 1996; Lochner and Monge-Naranjo, 2011; Brown et al., 2012). Although earlier work argues that the effects of credit constraints on schooling decisions are modest (Cameron and Taber, 2004; Carneiro and Heckman, 2002), recent work (Belley and Lochner, 2007; Hotz et al., 2018) point towards an increasing role of family income in post-secondary schooling choices. There remains a gap in the literature regarding the relationship between family income and major choice. [3] The literature is largely inconclusive about the causal effect of family income on college major choice and various mechanisms such as non-pecuniary tastes, direct price effects and credit constraints. I employ changes in tuition policy to understand changes in sorting by income into college majors to address the correlation of family income with unobserved characteristics such as ability. Additionally, I develop a model that studies borrowing and college major choice jointly, within the same framework. There is a gap between models of college major choice that typically do not include a borrowing and credit constraints Altonji et al. (2016b) and models of schooling with credit constraints that do not study college major choice Abbott et al. (2019). I address this gap by incorporating college major choice, borrowing decisions and credit constraints within the same framework. The identification of these models is typically challenging since exogenous variation to major choice is rare. The differential prices provide identifying variation that allows me to contribute to the identification of major choice models.

Third, I also add to a growing literature in the economics of higher education which has focused on the determinants of college major choice[4]. In particular, my paper focuses on how supply-side policies such as major-specific pricing shift student's major choice. Although there is a large literature on the demand of college majors, much less is known about supply side policies. Most of these studies focus on variation in supply side constraints such as number of seats or majors offered (Bordon and Fu, 2015; Cook, 2020; Bianchi, 2019). This paper adds to this body of work by analyzing the responsiveness of major choice to short-term financial incentives. The literature on major choice has focused on ability sorting (Arcidiacono, 2004). This paper is one of the first papers to contribute evidence on income sorting into majors [5] and the first paper to analyze the mechanisms contributing to this sorting using a structural model.

Finally, I contribute to the body of work that estimates the returns to schooling by proposing a new instrument to estimate the labor market returns to college majors. Several recent papers have used variation in demand side-driven labor market returns (Beffy et al., 2012; Abramitzky et al., 2019) or exploited eligibility requirements in various countries(Bertrand et al., 2010; Hastings et al., 2013; Kirkeboen et al., 2016; Andrews et al., 2017) to study the responsiveness of college major

[3]The exception is Ma (2009) finds that descriptively, lower socio-economic status (SES) children are more likely to major in lucrative college majors. However, it does not address potential confounders such as differences in ability and preferences that have been accounted for in studies on the effects of family income on schooling decisions.

[4]See Arcidiacono et al. (2012), Altonji et al. (2016a) and Patnaik et al. (2020) for a review of the literature

[5]The exception is recent work by Andrews and Stange (2019)

to earnings. I contribute to this literature by proposing a new instrument to estimate the labor market returns to these majors: the policy change in prices across majors. As long as students are responsive to these prices, pricing instruments can estimate the labor market returns to majors with the high prices.

Organization. The structure of the paper is as follows: Section 2 provides the paper's motivation and the baseline major sorting of students by income. Section 3 provides the background of the institutional policy, and Section 4 introduces the data. Next, Section 5 discusses the identification strategy followed by empirical results of the paper in Section 6. Section 7 discusses the identification of earnings returns and Section 8 conducts a cost benefit analysis. Next, Section 9 that addresses concerns about potential threats to identification. Motivated by the empirical evidence, Section 10 describes a model of college major choice with credit constraints and its estimation is discussed in Section 11. Finally, Section 12 discusses counterfactual education policies and I conclude the paper in Section13.

2 Motivation: Baseline Sorting

In order to more generally motivate the importance of studying the relationship between income and major choice, I use panel data from the National Longitudinal Survey of Youth (NLSY97) that tracks the post secondary school decisions of students as well as their parental income as well as standardized test scores (Armed Services Vocational Aptitude Battery or ASVAB scores) that can proxy for general ability. I then regress dummies for whether you completed your 4 year college degree and your major conditional on completing your college degree on your average real family income in the four years prior to going to college, ASVAB Arithmetic and Writing Ability and several other controls such as race and gender. I report the results of this regression in Table 2.

I find that there is gradient of family income and major choice, particularly in the case of Science and Business Majors. An increase in average real income of $10,000$ dollars is associated with a decrease in the probability of majoring in science, conditional on completing a four-year degree. Interestingly, a similar increase in real income is associated with a significant increase in the probability of majoring in Business. I therefore establish that there is sorting on family income into majors, even after controlling for measures for ability in math and verbal skills. The magnitude of this association is not much smaller than the relationship between family income and the decision to drop out. However, this association is not causal evidence since family income can be correlated with several unobservable characteristics that may influence the choice of major such as tastes.

Shocks to family income are often correlated to shocks to other unobservable characteristics that may affect major choice. However, shocks to the *cost of major* can provide arguably exogenous variation in the ability to pay for college, that could induce sorting by family income into college majors. The introduction of major specific differential tuition policy provides such exogenous increase in the relative cost of majors. I zoom in and exploit the introduction of differential tuition policy in a large public university to study the effects on post secondary outcomes for low-income

students. In the next section, I will describe the institutional set up in this large public university and the details of the policy. To provide some comparability from the context of UW-Madison to the estimates from the NLSY, I regress major choice on quartiles of family income and quartiles of act score similar to the estimates reported in Table 2. I report these results in Table 9. The patterns are similar. For example, students from higher income backgrounds are more likely to major in Business and less likely to major in education. Similarly, students in the second and third quartile of the income distribution are more likely to major in Engineering. Next, I will describe the institutional context of the policy.

3 Institutional Background

This section outlines the institutional background for the tuition policy. I begin by characterizing the University of Wisconsin, after which I describe the details of the differential tuition policy. The University of Wisconsin - Madison is a large flagship public research university in the state of Wisconsin. Being the flagship university, it is a highly selective university with about 60 percent of the applicant pool being admitted. The undergraduate program on average has freshman enrollment of approximately 5,000 students per year with an average ACT score of 28 points. Although the majority of the students are in-state students, there is a smaller non-resident and international population as well as a small population from the neighboring state of Minnesota, with which Wisconsin shares a tuition-reciprocity agreement. The tuition schedule varies depending on residency status, with residents, on average, paying less than a third of the tuition paid by non-resident students. Due to the tuition reciprocity agreement, students from Minnesota are subject to the same tuition schedule as residents from Wisconsin. For this reason, for the rest of the paper, I will be referring both students from Wisconsin and Minnesota as residents.

How representative is UW-Madison?

Before focusing on the case of UW-Madison, it is useful to understand how representative is UW-Madison relative to other schools. Table 1 reports characteristics of UW-Madison relative to other Big Ten Universities, which are large selective universities in the Mid West region of the United States. UW-Madison consists of a large student body of about 27,000 students. With the exception of Northwestern University and the University of Michigan, it is more selective than most other universities. At the same time, the in-state tuition is much lower than other universities, although the out-of-state tuition is quite similar to the other Big-Ten Schools. The only exception is Michigan which has a substantially higher out-of-state tuition.

Tuition Policy

The university follows a plateau system of tuition similar to other public universities. The undergraduate semester tuition increases until 12 credits, then plateaus until 18 credits and then increases further. On average, full-time students enroll between 15 and 18 credits per semester.

As was discussed earlier, like most public universities, UW-Madison charges differential tuition for in-state, out-of-state students and students from Minnesota. The tuition for non-resident students is almost double the tuition paid by in-state students whereas the tuition for students from Minnesota is determined by the Minnesota Reciprocity Agreement (1992). The agreement allows for students for Minnesota to pay the minimum of the tuition paid by in-state students in Wisconsin and the tuition paid by instate students in the University of Minnesota- Twin Cities. Therefore the tuition for these students is close to the tuition assessed for resident students. Table 3 provides an example of the *per semester* tuition for 15 credits in 2018. Given the high tuition for non-resident students, most of these non-resident students typically come from high-income families, with the exception of a small group of students who receive merit aid, athletic scholarship and are specifically recruited by the university.

Table 4 shows that there is considerable variation in the amount of tuition paid, by demographics. The source of variation in the tuition is a combination of the increases in tuition by year, residency, surcharge by school, number of credits and time to degree. The largest differences in tuition are for resident and non-resident students. For example, non residents on average pay a cumulative tuition of 88,010 dollars for their undergraduate degrees whereas residents pay 33,010 dollars for their degrees.

In addition to the tuition differential by residency, the university also introduced a major-specific tuition policy, that I will be studying in this paper. The School of Business introduced a field-specific surcharge in 2007 followed closely by the School of Engineering in 2008. The structure of the surcharge is unique. The amount of the surcharge is paid by all students who major in the school, and does not depend on the number of credits enrolled in courses in the school or the overall number of credits. It also does not depend on residency or family income. The surcharge is levied only *after* declaring a major in the school. Therefore students on average pay this surcharge on declaring their major in their sophomore year and generally end up paying it for 4 semesters, if we assume that most students graduate in 4 years. Table 5 reports the the timing and amount of the surcharge. The School of Business started charging 1,000 dollars per year for all students who majored in the school of Business starting in 2007. The introduction of the policy was more gradual by the School of Engineering. It began in 2008 with a charge of 500 dollars per semester and then was increased to 700 dollars per semesters in 2009 for every student majoring in the college of Engineering.

Gaming the System?

There may be some concerns that such a system of levying a flat surcharge at the time of declaration may lead to a strategic response from the students in terms of the timing of declaration. Since the surcharge is levied once you declare the major, students may have incentives to delay declaring major to latest time possible. However, the pre-Business requirements include a requirement for at most 85 degree credits before applying to the school of Business. If students take an average of 15 credits per semester, then students can accumulate these credits in less than 6 semesters or 3

years. Most students apply to the school in the spring semester of their first year. Therefore, while there may be some strategic incentives to delay, students cannot delay their declaration beyond the the end of second year. Additionally, the Business courses are generally high demand courses and, students who declare the Business major do have preference for enrollment over students who don't. This creates additional hurdles to delay declaration to avoid paying the surcharge.

Major Declaration: Timing and Choice Set

Most students in the School of Business up until 2015 declare their major in their sophomore year. Therefore, over the period of study, there are rarely any direct admits. As in most other universities in the United States, the college of Business is a competitive school. Students have to fulfill certain course requirements (referred to as "Pre Business Requirements") and are admitted to the school of Business in the fall of their sophomore year. Approximately 60 percent of the applicants are admitted to the school of Business based on a combination of their GPA as well as an essay component. There is no sharp discontinuity in the GPA of the students who are admitted and those who aren't since there are other subjective criteria such as the essay that are included in the admissions process. Once these students are admitted, they have to satisfy a GPA requirement in every semester as upper classmen for automatic progression in the Business school.

In contrast, the School of Engineering admits both students in their freshmen year (direct admits) as well as students who are admitted in their sophomore year (indirect admits). The admission into the School of Engineering is determined by the Admissions policy of the university and therefore is not determined by an explicit cutoff for high school GPA or a standardized test. The School of Engineering also has progression requirements with regard to course completion and GPA. Since many of the courses in the freshman year are not housed in the college of Engineering, it is fairly easy to switch to a different major such as Chemistry or other Natural Sciences.

The other schools in the university are Agriculture and Life Sciences, School of Human Ecology, School of Pharmacy, School of Medicine, School of Nursing, School of Education and College of Letters and Science. The College of Letters and Science is the largest school and awards degrees to 60 percent of the student population. Majors such as Natural Sciences, Economics and Humanities are all housed in the College of Letters and Science.

4 Data

This paper combines information from various sources that I outline below. The primary source of data is administrative student-level data from the University of Wisconsin - Madison. I complement that with information from university reports and archives as well as nationally representative surveys. I then describe the sample I will use for this study.

4.1 *Data Description*

In this section, I describe the key data sources that I will draw from for my analysis.

4.1.1 *UW-Madison administrative data*

Demographics and Test Scores. The data from Wisconsin consists of student level records of students. This includes information about their demographic characteristics such as gender, race, residency at the time of entrance, geographic information such as home state and home zip code, and measures of student ability such as high school GPA, standardized test scores such as ACT and SAT scores. Most importantly, the data contains student level records about their degree completion as well as their choice of major and the school associated with the major.

Transcript Information. In addition to information about student demographics and their degree, the data also contains detailed transcript information such as primary program in each semester as well as courses taken and the grade performance in those courses in each year. This is particularly useful for two reasons. First, it allows us to carefully study the choice of major beyond the *final* major as has been the case with most studies so far. The history of course taking provides valuable information about the intention to major. Since school such as the School of Business have course requirements for being admitted. The course taking behavior allows me to construct a measure of the intention to major. Additionally, the rich data allows the study of the dynamics of major choice for example, in terms of persistence both within the school as well as in the university.

Financial Aid. I supplement the above information with financial aid information. This provides additional information for the population of students that file for federal financial aid that is they fill out the Free Application for Federal Student Aid (FAFSA). This provides more detailed information on family income and the student aid package including financial aid as well as student loans. I focus on the sample of dependent students and consider information on their first year financial aid. This data is informative by allowing us to account for various sources of financial aid such as aid and borrowing behavior.

4.1.2 *Other Complimentary Data Sources*

Institution level information. To compare UW-Madison to other institutions, I use the Integrated Post Secondary Data System (IPEDS) for university level data on enrollment, completions and institution characteristics.

Earnings Information. I complement the university level administrative data with data from several nationally representative surveys. I also use data from the National Longitudinal Survey of Youth (97) as well as American Community Survey (ACS), and aggregate data from Post Secondary Education Outcomes Explorer (PSEO) for documenting wage, employment and major choices and relating these to family income.

Tuition Schedules. Finally to track tuition schedules as well as information on university policies and budget, I extensively use reports released by the University as well as the Wisconsin Legislative

Bureau.

School Specific Budget Allocation. Reports released by the College of Engineering and the School of Business have also been digitized to create a dataset of school specific expenditure for documenting the institutional context of this policy.

4.2 *Sample and Descriptive Statistics*

The sample for this study is the universe of *domestic* students who were enrolled in the university between 2000 and 2012. I exclude transfer students since their program choices may have been determined by their prior college. I restrict the sample to all degree-seeking undergraduate students, which is the relevant population for the purpose of this study. Note: that I also exclude all students who major in the school of pharmacy, school of nursing and school of medicine due to the low degree of substitution between these majors and Engineering and Business majors.

Table 6 shows the demographic composition of sample of students in the university. More than half of the students are female. The racial composition of the sample point to a majority of white students (82 percent) followed by other racial and ethnic groups. Given that the university is a public university, 62 percent of the population are resident students. About 12 percent of the population are Minnesota residents and approximately 26 percent are non-resident students who do not reside in Minnesota. Although UW-Madison is a large public university, approximately 60 percent of the sample applies for federal student aid using the Free Application for Federal Student Aid (FAFSA) and only 18 percent of the sample is eligible for Pell Grants.

Moving on, I describe the characteristics of the demographic and test score population of the resident and non resident sample, before and after the policy. Table 8 displays the characteristics of the residents and non-residents depending on whether they were in a cohort before or after the policy was implemented. The gender composition of both residents and non-residents remains similar before and after the policy. Note: that the average test scores for both ACT Math and English components increase after the policy, particularly for non-residents. This is consistent with an upward trend for test scores over the last decade

There are also considerable differences in the FAFSA completion rate by residency. Figure A-2 reports that approximately 60 percent of resident population files for FAFSA whereas only 40 percent of the-non resident population files for federal student aid. Among the students that do file for FAFSA, I report their family income in Table 7. On average, resident family is income is much lower than the non resident family income. Residents, on average, have a real family income of 82,000 dollars whereas non residents who file for FAFSA have a family income of 120,000 dollars.

5 Empirical Strategy

My primary identification strategy is a difference-in-difference (DD) strategy. I exploit variation by income in the impact of the policy (the first difference) and variation across cohorts in exposure

to the policy (the second difference). I describe this strategy in more detail below.

5.1 *Defining Treatment*

Under the differential tuition policy, the surcharge operates as a flat fee. I argue that the effect of this policy differs based on the family income of the student. For example, a student with a family income of 500,000 dollars would be relatively unaffected by a surcharge of 1,000 dollars relative to a student with a family income of 50,000 dollars. This intuitive argument relies on the theory of decreasing marginal utility of income as well as liquidity constraints that are commonly argued as reasons for low-income students being more price sensitive than high income students.

In an ideal setting I would have the family income measured perfectly for all students. However, due to data restrictions, that are common to most studies such as this, income is available only for students who file for financial aid through the Free Application for Federal Student Aid (also known as FAFSA). However, for the full sample, we can observe variables that are highly correlated with income such as out-of-state status and zip code income. These are available for both FAFSA as well as non-FAFSA filers and can be linked to details of transcript information.

Additionally, even though the surcharge is flat, there is a substantial difference in the surcharge as a percent of the total tuition paid. For the non-residents, the surcharge is a small percent of the overall tuition (an additional 500 dollars per semester would be approximately 2 percent of their base semester tuition) are *less* affected by the policy. On average non-residents come from high-income backgrounds since they are able to afford the high tuition. Whereas the differential is between 10-15 percent of the base semester tuition for residents and they are *more* affected by the policy.

I discuss several measures of family income to argue that there are key compositional differences in terms on income between residents and non-residents. The measures that I use are the following. First, I use income at a granual geography, namely the zip code level. I create this measure by merging on zip code level mean income released by the US Census Bureau in the 5 year ACS estimates for 2006-2010, which overlaps with the time period for which we observe and study the policy change. Since neighborhoods are fairly segregated and there is a large literature that establishes income based sorting into neighborhoodsthis measure is strongly correlated with the true income. The advantage of this measure is that I can recover proxies of income for all students in the sample, rather than just the students who file the FAFSA. Although, this is a more noisy measure, it is also more representative. Figure A-3 plots the mean zip code income for residents and non residents. It is clear that non-residents come from much richer neighborhoods than residents. In particular, there is long right tail for non residents that is not the case for residents. Second, I use the sample of students who file for aid through the FAFSA. This would be roughly 60 percent of the population. When students file the FAFSA, they report their family adjusted gross income as well as their family assets. These are a sum of their parental family income and the student family income (if they are independent). Figure 2 plots the distribution of family income data by residency using a box plot. The plot reports the mean and the 25th and 75th percentile of

the distribution of family income by residency.

These measures confirm that not only do non-resident students have a substantial higher income, even in the FAFSA sample, but also there is larger dispersion in the family income in non residents. Motivated by these differences in income by residency status, I define treatment based on residency groups.Having established that non-residents are from families with higher income and pay a much higher base tuition, I argue that for non-residents, the effect of the surcharge on their major choice would be negligible. I therefore assign the residents to the *High Treatment Intensity* group and non-residents to the *Low Treatment Intensity* group and analyze the differences in outcomes between these two groups before and after the policy. I implement a difference-in-difference strategy and complement this with event study analyses. The primary identifying assumptions of this strategy are that the policy variation is exogenous and that two groups were trending in a parallel manner before the policy was introduced. I will first begin by verifying this using the event study framework.

5.2 *Event Study Strategy*

One of the key identifying assumptions of difference-in-differences is that the resident and the non-resident population were trending in a parallel manner before the introduction of the policy. I test for the presence of parallel trends using an event student specification. I implement the following linear probability model (LPM) specification for outcome Y_{ic} for individual i of admit cohort c and for policy year τ^*:

$$\begin{aligned} Y_{ic} = \beta_0 + \beta_1 R_{ic} + \overbrace{\sum_{\tau=2000}^{\tau^*-5} \boldsymbol{\alpha_\tau} \mathbb{1}(\tau = c) R_{ic}}^{\text{No Surcharge Cohorts}} \\ + \underbrace{\sum_{\tau=\tau^*-3}^{\tau^*-1} \boldsymbol{\alpha_\tau} \mathbb{1}(\tau = c) R_{ic}}_{\text{Partial Exposure Cohorts}} + \underbrace{\sum_{\tau=\tau^*}^{T} \boldsymbol{\alpha_\tau} \mathbb{1}(\tau = c) R_{ic}}_{\text{Full Exposure Cohorts}} + \lambda_c + X_{ic}'\gamma + \varepsilon_{ic} \end{aligned} \tag{1}$$

where Y_{ic} is a dummy that is equal to 1 if individual i of admit cohort c graduated in the major and zero if the individual graduated in a different major. R_{ic} is a dummy for resident, $\mathbb{1}(\tau = t)$ is an indicator variable for being admitted in cohort $\tau = c$ and λ_c is a fixed effect for admit cohorts. I also include a rich set of individual level controls (X_{ic}) for gender, race, high school GPA, age at entrance, ACT math score and verbal score as well as dummies for missing high school GPA, gender, race, test scores. ACT scores imputed for students who took the SAT. To account for correlated errors, I cluster the standard errors at the level of the treatment (Abadie et al., 2017), namely at the residency-admit cohort level. Due to the small number of clusters (26 clusters: 13 cohorts × 2 residency types), I apply the correction for the small number of clusters by using the wild score bootstrapping (Cameron and Miller, 2015; Kline and Santos, 2012)

The parameters of interest is α_τ, namely the differential probability of graduating in the major of residents relative to non-residents in cohort τ. There are three types of admit cohorts that are of interest. First, are the admit cohorts that on average graduated before the policy was introduced. For the rest of the discussion, I refer to them as the "No Surcharge" cohorts. The second group is the students who were already admitted before the policy was introduced but may have been subject to the policy in their sophomore or senior years. I refer to this group as the "Partial Exposure" cohorts. Finally, the third group of students are the students who were admitted on or after the year of the policy change, who I refer to as the "Full Exposure" cohorts. Under the assumption of the average number of of graduation being four years, the earliest partial treat group is the cohort admitted four years before the policy change.

I normalize the difference between residents and non-residents for the last cohort that would be untreated i.e. the "No-Surcharge" cohort to be zero so that all coefficients (α_τ) are relative to this cohort. Under the assumption of parallel trends, we should expect to see that the coefficients α_τ for the cohorts τ in the "No Surcharge" cohorts should be small and not significantly different from zero. Since the policy was grandfathered in, the coefficients $\alpha_\tau$for the "Partial Exposure" group might be zero or negative depending on how responsive are these students and their "dosage" of the treatment (in terms of the number of years of exposure to the policy). Finally, if the price increase was effective in reducing the demand from the resident population for the newly admitted "Full Exposure cohorts" for the cohorts τ admitted after the policy, we expect to find that the coefficients α_τ for these admit cohorts will be negative. Further, if some of the later cohorts experience a higher surcharge or a "higher dosage" of the treatment, we might expect those students to be more responsive. Additionally, if we expect there may be a time for students in later cohorts to become more aware of the surcharge, these effects may accumulate over time.

The lagged implementation as well as the grandfathering of the policy also allows a study of the *amount* of the tuition differential. In order to understand the responsiveness of students to the dollar amount of the differential, I implement a similar DD estimation where the treatment is now continuous. The treatment is now defined as the total amount of tuition differential that the student expects to pay throughout the four years of under-graduation. The amount varies by the cohort due to the difference in the number of years of exposure to the treatment as well as the changes in the amount of differential policy. The specification is given by the following:

$$Y_{ic} = \beta_0 + \beta_1 R_{ic} + \sum_{\tau \neq \tau^* - 1} \alpha_\tau (P_{i\tau} - P_{i\tau}^{L\&S}) R_{ic} + (P_{i\tau} - P_{i\tau}^{L\&S}) + X_{ic}' \beta_2 + \varepsilon_{ic} \tag{2}$$

where R_{ic} is one if individual i of cohort c was a resident. $(P_{ic} - P_{ic}^{LS})$ is the price differential between the major and Letter and Science. α represents the differential of residents relative to non residents between cohorts before and after the policy. Y_{ic} is equal to one if individual i of cohort c graduated in the major.

5.3 *Difference-in-Differences Specification*

I now describe the difference-in-difference framework that essentially considers the mean effect across the three groups of cohorts that I described earlier.

Baseline Specification

In my baseline specification, I use the following linear probability model. For the outcome Y_{ic} for individual i of admit cohort c:

$$\begin{aligned} Y_{ic} &= \beta_0 + \beta_1 R_{ic} + \beta_2 \text{Partial Exposure}_{ic} + \beta_3 \text{Full Exposure}_{ic} \\ &\quad + \alpha_1 \text{Partial Exposure}_{ic} R_{ic} + \alpha_2 \text{Full Exposure}_{ic} R_{ic} + X'_{ic}\gamma + \varepsilon_{ic} \end{aligned} \tag{3}$$

Y_{ic} is equal to one if individual i of admit cohort c graduated in the major and 0 if the individual graduated in any other major. R_{ic} is a dummy for residents and Partial Exposure is a dummy for students who were already in school when the policy was enacted, Full Exposure is a dummy for students who were admitted *after* policy was enacted, X_{ic} includes controls for individual specific characteristics such as gender, race, high school GPA, age at entrance, ACT math score and verbal score as well as dummies for missing high school GPA, gender, race, and test scores. Similar to earlier, all standard errors are clustered at the residency-admit cohort level.

The coefficient α_1 represents the differential effect on the residents (high-treatment group) relative to non-residents (low-treatment group) for the Partial Exposure cohorts (relative to the No Exposure cohorts) and the coefficient α_2 represents the differential effect on residents relative to non-residents for the Full Exposure cohorts (relative to the No Exposure cohorts). These parameters of interest, α_1 and α_2 therefore capture two different effects. One, α_1 the effect of the policy on earlier cohorts continuing in the surcharged major. Two, the coefficient α_2 is the effect of the surcharge on new cohorts entering as freshmen.

Additional Specifications

I also use a specification that exploits three differences between students: residency, admit cohort and zip code income to implement a triple difference strategy. For individual i of admit cohort c:

$$\begin{aligned} Y_{ic} &= \beta_0 + \beta_1 R_{ic} + \beta_2 \text{Partial Exposure}_{ic} + \beta_3 \text{FullTreat}_{ic} \\ &\quad + \alpha_1 \text{ Partial Exposure}_{ic} R_{ic} + \alpha_2 \text{FullTreat}_{ic} R_{ic} \\ &\quad + \alpha_3 \text{ Partial Exposure}_{ic} Z_{ic} + \alpha_4 \text{FullTreat}_{ic} Z_{ic} \\ &\quad + \alpha_5 \text{ Z}_{ic} R_{ic} + \alpha_6 \text{Z}_{ic} R_{ic} \\ &\quad + \alpha_7 \text{ Z}_{ic} R_{ic} \text{ Partial Exposure}_{ic} + \alpha_8 \text{Z}_{ic} R_{ic} \text{ Partial Exposure}_{ic} + \beta_4 X_{ic} + \varepsilon_{ic} \end{aligned}$$

where $Z_i c$ is the zip code income and the rest of the notation follows the earlier specifications.

Finally, for the variables based on income, I use the following empirical specification. Since this sample is considerably smaller, I separate the admit cohorts into those admitted before the

treatment and those admitted after the treatment. I justify this design based on results from the event study which I will describe in the next section.

$$Y_{ic} = \beta_0^{inc} + \beta_1^{inc}\text{Below Median}_{ic} + \beta_3^{inc}\mathbb{1}(t >= \tau) + \alpha^{inc}\mathbb{1}(t >= \tau*)\text{Below Median}_{ic} + \beta_2^{inc}X_{ic} + \varepsilon_{ic} \quad (4)$$

where Below Median$_{ic}$ is one if individual i of cohort c had income that was below the median income in the sample. The year of the policy change is given by τ^* and the symbol $\mathbb{1}(t >= \tau*)$ is a dummy that equal to one if cohort c was admitted to the university after the calendar year of the policy change $\tau*$. Y_{ic} is the outcome of interest, which in this case is the probability of graduating with the surcharged major. The coefficient α^{inc} here therefore can be interpreted as the effect of the levy of the tuition differential on poor versus non-poor students. If α^{inc} is negative then, the differential policy results in a decrease in the probability of graduating in Business for poor students relative to non-poor students.

6 Empirical Results

6.1 *Effects on Graduating in Major*

Figure 4 reports the coefficients α_τ for every admit cohort τ where the coefficient is normalized to zero for the year the last "No Treat" group. The event study in figure 4 points no pre-trends for the "No surcharge cohorts" for both the Engineering and Business surcharge policies.

For the partial exposure cohorts, there seems to be a negative effect in the probability of doing Business for the Business surcharge policy although for most cohorts this effect is not statistically different from zero at the 5 percent level of significance. The effects of the policy mostly lies in the Full Exposure cohorts that are admitted after the policy was introduced. There is a significant fall in the probability of graduating in Business for residents relative to non-residents for these cohorts of students.

On the other hand, I find no effects for the partially exposed cohorts for the Engineering surcharge policy. This suggests that for students who were upperclassmen when the policy was introduced, there was no effect on their choice of major. For the Full Exposure cohorts, the effects are larger as well as significantly different from zero for the cohorts that were admitted on or after 2009 and faced a higher differential of 1400 dollars per year suggesting that the amount of the surcharge or the "degree of treatment" also matters for students' responsiveness. The effects of this policy persist and increase for later cohorts suggesting that there may be information frictions that are resolved for later cohorts.

My findings also point to different elasticity for Engineering and Business to prices. The Business surcharge resulting in a substantially larger decrease in the probability of graduating in Business than the Engineering surcharge for the probability of graduating in Engineering. Students may respond differently depends on the major in which the surcharge is implemented. Majors differ in many dimensions such as expected earnings, difficulty and rigidity of coursework, the presence of close substitutes, as well as students tastes for majors, all of which may be contributing

to different responses to pricing policies.

As discussed earlier, I also study the effects of changes in the dollar amount of the differential. Figure 5 shows that these effects are largest for 4000 dollars of overall undergraduate tuition differential for Business and 5600 dollars of overall undergraduate tuition differential for Business. Note that these cohorts were also the students that were freshmen when the policy was introduced. The figure indicates that the differential doesn't offset the switching cost of majors for students who are already upperclassmen.

Next, I estimate the difference-in-difference specification which essentially estimates the mean effect for each group of cohorts.

6.2 *Difference-in-Differences Results*

Table 11 shows the estimates for the Business and the Engineering surcharge. I find that the introduction of the policy resulted in a decrease in the rate of graduation in Engineering for residents (relative to non-residents) by 1.23 p.p. on a base of 9 percent. The policy had a larger decrease of 4 p.p. (on a base of 12 percent) on probability of graduating with a Business degree. This is a substantial response to costs that are fairly small relative lifetime earnings trajectories.

An important implication of the policy is potential differences in the effects of the policy for various sub-groups of students. In order to evaluate the effect of this policy on sub-populations, I estimate the DD coefficients for various demographic sub-samples. A common hypothesis of this policy is that students from marginal socio-economic grounds are more responsive to changes in these relative prices. I therefore explore the effects of the higher price on sub-samples by gender, race and pre-college achievement.

The results indicate that the effects on graduating in Business were significantly larger for women although this is not the case for Engineering. This may be partially driven by the low number of women in Engineering, only 3 percent of women graduate in Engineering. I also group the sample to white and non-white students. I group together all non white groups together since collectively, they make up 15 percent of the population. Contrary to what we might expect, the effects are larger and statistically significant for white students for both the surcharges. Although we do see a decrease in the share of non-white students in Business, this decline is not statistically significant. Finally, the effects on the share graduating in Business is not heterogeneous by above median and below median act scores, which can be proxy for pre-college ability. This suggests the effects are similar across the test score distribution. However, the share of above median test score students in Engineering decreases by 2.76 p.p. (on a base on 15 percent), as opposed to an effect very close to 0 for below median act score students. This might be partly driven by the low number of non-white, female or below median test score students in the Engineering field as in clear from the base shares of these subgroups in Engineering.

A natural threat to this identification strategy is that differences between resident and non-resident groups are coming from differences in underlying characteristics other than family income levels. To address these concerns, I explore alternative but closely related definitions of treatment

to examine the robustness of the effects of the policy to the definition of the treatment groups. I compare the measures based on residency to the measures based solely on income. However, these characteristics are available for the sample of students who file for financial aid. Since this sample is substantially smaller, I divide students into cohorts that were admitted before or after the policy was introduced (pre and post cohorts). The responsiveness of partially treated cohorts is not a concern here since we verified in our event study that these cohorts haev negligible responses and so we can group them with the no exposure cohorts. I then consider students below-median income students as the high-treatment group and the above-median income students as the low treatment intensity group.

In Table 13, I report the estimates from the difference in difference based on different measures of income. I report four specifications in this table. The first specification reports the estimates the effect of the difference in difference when the treated group is considered to be residents. The second specification reports the coefficient when the the treated group is students below the median income where income is defined as mean zip code income. This can be interpreted as the the effect of the tuition surcharge on students from poor neighborhoods compared to those from non-poor neighborhoods. The treatment in this case operates at a finer geography than the treatment that is based on residency which depends on your home state.

The third specification considers treatment as those who were below the median income in the population who file the FAFSA. The income here is the true income reported in the FAFSA. Here , the coefficient α^{inc} then can be interpreted as the effect of the tuition surcharge policy on poor students versus non-poor students, where the group of non-poor students is somewhat truncated since we do not include students who do not file for FAFSA and as we had discussed earlier, these students are most likely students from high income families who do not need financial aid. Finally, the fourth specification reports the results from the specification where treatment is residents who are below the median income relative to residents who are not below the median income. Therefore the coefficient α^{inc} can be interpreted as the effect of the lower income holding the amount of tuition paid as constant.

Discussion. The coefficients reported can be interpreted in the following manner. The implementation of the differential tuition policy decreases the probability of graduating with a Business degree for residents relative to non residents by 4 p.p.. Whereas, the policy decreases the probability of graduating with a Business degree for students who come from low-income neighborhoods (zip codes) relative to high income neighborhoods (zip codes) by 1.53 p.p.. The same effect for below median income students relative to above median income students is 1.2 p.p. in the FAFSA application pool and the effect largely remains unchanged when we look at below median income residents relative to above median income residents. Across all the specifications, the effect of the policy results in a decrease in probability of graduating with a degree in Business.

In order to make sense of the difference in the estimate magnitudes across the different samples, I will describe the key differences in the treatments. Comparing estimates between specifications three and four, we see the effect of the policy on a sample that faces differences in income and

tuition the same tuition versus a sample that faces differences only in income. Since the effects are largely the same, the effect of the tuition is largely felt by the low-income resident population rather than the low-income non resident population.

Next the differences between specifications 2 and 3 come from selection in the sample who file for FAFSA and the measurement error in zip code level income. If we assume that the the measurement error is mean independent, then this results in an attenuation bias in the coefficient relative to the true coefficient in the full sample of students with true income. Since we expect that attrition in the FAFSA sample income distribution to come from the right tail of the income distribution, the coefficient in specification three should show a lower effect than the true effect in the full sample of students. The estimates in Table 13 show that the FAFSA income estimate is lower than the estimate with zip code income, which is biased towards zero, this suggests that effect on the poor students relative to non-poor students would be even larger for the full sample of students and that this is a lower bound on the estimate.

Now the differences between specification 1 and 2 are twofold. First, in specification 1, the resident group comprises of both poor and non-poor residents and the non resident group specifies both poor and non-poor residents as well. Poor residents are students who have the largest increase in tuition and lowest income and so they must see the largest decrease. Poor non residents see a small percent decrease in tuition and have a low-income so their effect must be smaller. Non-poor non-residents would see a large percent increase in tuition and have a high income and so they should be least affected by the policy. Non-poor residents would be the group that faces a large percent increase in tuition as well as a high income and so their effect would depend on whether the change in price sufficiently moves their decision to major in Business even with the larger income. If the price effect is large enough we should see a larger decrease in the residents relative to nonresidents rather than the coefficient reported in specification 2.

Finally, I also use a specification that exploits a triple difference strategy. Table 12 reports the results of this exercise. I find that in fact the effects of the policy are larger for students with a lower family income in the case of business as indicated by the positive coefficient for the interaction of residency, full exposure and zipcode income. However, the corresponding estimates for engineering are noisy and should be interpreted with caution.

Next, for the sample who file for FAFSA, I explore robustness of the effect to a number of other measures. Table 14 reports the estimates of the coefficient α_{inc} using other measures of poor or non-poor status. These measures include below median Family Adjusted Income, below median Expected Family Contribution, which is a non linear function of family assets, family AGI as well as other characteristics such as number of siblings in college. There is an S shape with the aid increasing initially with an increase in family income and then decreasing. This non linearity implies that it is not obvious if the effects persist even after we account for aid. Additionally, there is considerable heterogeneity by residency in the distribution of these aid functions.For this reason I also construct two additional measures that capture the extent of “unmet need".

First, I create a value of estimated tuition using the year of the cohort and residency and assign them the tuition charged for that particular residency for that year. The first measure of unmet

need subtracts from the estimated tuition, the expected family contribution as well as total aid, which includes total loans, total gift aid as well as total work study. The second measure of unmet need subtracts from the estimated tuition, the expected family contribution as well as gift aid. Thus, these measures allow us to consider the students who are most in need rather than just low-income students, since the aid function for Federal aid is decreasing with an increase in income. Even with these measures, we do continue to see a decrease in the probability of doing Business as well as Engineering.

These exercises confirm that these differences between residents and non residents are coming from income differences between the two groups.

Where do these students go?

A natural question that emerges from this policy is what are the majors that these students substitute towards. The answer to this question may be important for analyzing the net earnings losses to these students. For example if students are substituting towards other majors such as Economics that are equally lucrative, then earnings loss accruing from these substitution patterns may not be as much of a concern.

The alternative schools that are available in the choice set of these students are the Agriculture and Life Sciences (ALS), Letters and Science (L & S), School of Education (EDU), School of Human Ecology (HEC). I use the baseline specification used so far but replace the outcome variable by the other schools that are available to the students. The results of this diff-in-diff exercise are reported in Table 15. It is clear that students substitute towards majors in College of Letters and Sciences in both instances of the differential tuition policy. Since Letters and Sciences includes majors such as Physical Sciences as well as other Social Sciences like economics, the findings indicate that students may be switching to majors that we consider as close substitute majors in response to this policy. Although we would like to look at substitution patterns at finer categorizations of majors within Letters and Sciences, these categorizations are substantially smaller cohorts of students.

6.3 *Ability Sorting*

Test score composition, Time to Degree, and Credit Load

In this section, I evaluate whether there are any changes to the composition of residents. in terms of their pre-college achievement where ACT test scores serve as a proxy for pre-college achievement. The objective of this exercise is to understand whether ability sorting is sensitive to this policy as well. I implement a similar DD exercise as earlier with the outcome being ACT scores (in Reading and Math) as well as whether students are in a particular section of the distribution *conditional* on graduating with a Business or Engineering major respectively. Table 19 reports the estimates of this exercise. The policy has effects on the composition of the classes, conditional on graduating in the surcharged field. There is a decrease in the ACT math test score of students of residents relative to non-residents. The surcharge has a stronger negative effect for students above the

median ACT Math scores for the students graduating in Business as well as Engineering. Thus the policy results in negative ability sorting. This negative sorting can be rationalized by the fact that typically Engineering and Business majors are often over-subscribed and there are often application processes for applying for these majors. Therefore if these prices cause a decrease in the share of residents, this may be accompanied by allowing some more students of lower ability to major in these programs who would not have qualified otherwise. This would result in negative ability sorting that we observe in our difference in difference analysis.

The tuition surcharge operates as a fee for every semester enrolled. Therefore, the surcharge increases the opportunity cost of graduating for every semester enrolled. An unintended consequence of this policy could be a decrease in the time to degree of residents relative to non-residents. Alternatively, the policy has also changed the ability mix of the students and therefore could possibly, increase the time to degree if the composition of the students moves towards lower ability students. The answer to this question depends on the relative importance of both these effects.

Table 20 reports the outcome of these estimates. Focusing on the sample of students who graduate in Business, I look at the effects on time to degree using the same difference-in -difference exercise as mentioned earlier. I find that the Business differential resulted in a decrease in the time to degree of students of -0.16 years (3 percent). If we discretize the time to degree and evaluate the probability of graduating in less than a certain number of years, we also find an increase in the probability of graduating in less than four years by 0.153 p.p. and this coefficient estimate is significantly different from 0 at the one percent level. As we would expect, these margins are less flexible for the partial exposure cohorts and for that reason, we don't see any significant changes in these margins for the part exposure cohorts. Similarly, the surcharge in Engineering has no significant effects on this outcome. A potential explanation could be differences in course structure.

6.4 Intention to Major, Retention and Dynamics

The discussion so far has focused on understanding the "final" choice of major. However, a substantial body of work has viewed the choice of major as a dynamic decision (Arcidiacono et al., 2016a; Stinebrickner and Stinebrickner, 2014). Therefore, understanding the timing of the choices influenced by higher pricing can be important to making policy recommendations. This might be particularly relevant for Engineering where a proportion of the students are direct admits. Additionally, retention in STEM courses such as Engineering is often a concern, particularly for low-income and URM students (Arcidiacono et al., 2016b). For marginal students, additional tuition burden can result in higher number of switches or dropout from the university.

Effects on Intended Major

One might argue that the difference-in-difference estimates might not be solely driven by the responses of students. The outcome of whether students major in Business is a combination of their intention to major as well as their eligibility for qualifying in the program. This becomes

particularly important in high demand courses such as Business. For Engineering, it is less clear, since students are admitted to this program in their freshmen year and apply for the course when they are applying to the university. The high demand major is then assigned to students who apply for the program as well as meet the eligibility requirements. In the case of the School of Business, there are course requirements that are necessary to be eligible to apply. These include courses in Math, Psychology, Economics and English. Table 16 reports the details of the number of credits as well as the courses included in the course requirements. Further, students also submit an application with an essay component as well as their overall GPA so far. The School of Business does not have a sharp GPA cutoff for admitting students.

Ideally we would like to use an indicator for whether the student was registered as an intended Business major in the administrative records. Unfortunately, the administrative data do not have a direct measure of the *intended major* for Business in the sample of interest. I therefore create a measure of the *intention* to major in Business that is equal to one if the student satisfies all the course requirements outlined in Table 16. If students indeed respond to these policies, we should also see an effect on the application behavior and this margin is not affected the admission criteria.

I implement the same DD research design where the outcome is now the *intention* to major in Business as opposed to graduation in Business major. Table 17 reports the estimates of this regression. The specification is the following that is similar to the earlier specifications. For individual i in admit cohort c,

$$Y_{ic}^{intend} = \beta_0 + \beta_1 R_{ic} + \beta_3 \mathbb{1}(t >= \tau) + \alpha \mathbb{1}(t >= \tau) R_{ic} + \beta_2 X_{ic} + \varepsilon_{ic} \tag{5}$$

Where Y_{ic}^{intend} is a dummy if the student intended to major in Business based on the coursetaking behavior. I find that the there is also a decrease in the intention to major in Business of a similar magnitude of 3.6 p.p.. Thus, there must be changes we find in the outcome of major, correspond to the changes in the application behavior in response to the policy.

Overall retention

The effects of pricing on retention remains relatively unanswered in the literature. For example, if the higher price increases the cost of staying an additional semester in college, these policies may push out students who were on the margin for staying in college. This additional cost can also be interpreted as the cost of experimentation. For example, a rich literature has modeled college major choice as an outcome of a learning process where grade signals serve as signals of unknown ability. If the price of pursuing another semester increases, this also increases the cost of learning. If the cost is sufficiently large, this may incentivize students to not pursue the high price major and either switch to a different major or potentially dropout. Ideally we would like to study major specific retention. It is particularly difficult to study retention prior to declaration, since most universities do not have students declare a major in their earlier years. Therefore, the switching from students who intended to major is difficult to measure and there has not been a lot of work on major-specific retention.

In this section, I explore the dynamics of these decisions and whether pricing plays a role. I implement the difference in difference estimation that differs from the previous specification in two ways. I follow admit cohorts across different academic years in school. I then compare the retention (in college) for students for each year in school of admit cohorts before and after the policy. Table 18 reports the estimates from the difference-in-difference regressions. The outcome variable in this regression is year specific retention in college. The sample of students is all students admitted in a particular year. The results suggest that there was a decrease in retention for residents relative to non residents for cohorts admitted after the policy. Unlike previous specifications, we also find that there is an effect on partial exposure cohorts in terms of retention.

7 Identifying Earnings Returns

A natural question that emerges is the extent of earnings losses to the students who respond to the price increase. For example, what would be the earnings change for a student who switched from Business to social sciences as a result of this price change.

Typically studies that estimate the returns to majors take advantage of GPA cutoffs in universities (Andrews et al., 2017; Kirkeboen et al., 2016). These strategies are often aimed at addressing several issues of selection that have been raised by (Altonji et al., 2016a). To this end, I propose a new instrument. The exogenous variation in relative prices provides a continuous instrument to identify the wage"return" to Business majors relative to majors that didn't have the price change. The advantage of this instrument is that price changes are quite common in universities in the United States and therefore this strategy can be implemented for many universities. Typically most studies estimating returns use restricted administrative earnings data in the United States from the UI system in the state (Andrews et al., 2017). This information is based on students who chose to stay back in the state of the college, and are typically the resident population. Thus earnings returns exclude students who migrate to other states and potentially earn a different distribution of earnings. Therefore, the instrument variable strategy which relies on comparing outcomes of resident and non resident students in response to the price change is not suited for this earnings data. To address this issue, I use survey information collected by the university on the outcomes of these students. This includes information on earnings from their first job. The disadvantage of this dataset is that unlike UI records we cannot track students for several years after graduating.

I begin by describing the relationship between initial earnings and choice of major and then proceed to describe the instrumental variable (IV) strategy.

7.1 What predicts initial earnings? Role of initial ability, college performance and specialization

What predicts earnings and how does it vary by major? I use a simple OLS regression where I regress initial earnings on ACT math and verbal test scores, gender and race categories, residency

and post categories. In additional specifications, I include dummies for chosen major as well as College GPA. I also run the same OLS specifications with high school GPA as a measure of achievement instead of ACT scores. Table 21 reports the results from this specification. I find that both high school GPA and ACT test scores are strong predictors of initial earnings, although the coefficient is almost half if we include controls for major as well as cumulative GPA. Women, Hispanic and Black students, and residents have lower initial earnings. Interestingly, cumulative GPA is not a significant predictor once we include ACT test scores. This suggests that ACT test scores have strong predictive power for initial earnings. Given that employers typically observe cumulative GPA as opposed to test scores, this suggests that employers seem to not renumerate for additional human capital accumulated in college. Additionally, similar to Kinsler and Pavan (2015), the coefficients on majors do not change significantly after controlling for cumulative GPA. As they mention, this may not necessarily indicate that ability sorting is mostly accounted for by test scores since GPA is endogenous to choice of major and that there are differences in grading standards across majors. The same is not true for high school GPA suggests that there may be differences in how high school GPA and ACT test scores map into earnings. For the rest of my analysis, I will focus on analysis based on ACT test scores.

7.2 Identifying Returns: Instrument Variable Approach

Price variation can be used to estimate the "wage" returns to Business majors. The instrument is the *continuous* variation in relative price of Business relative to majors that did not see the price increase, interacted with test scores. There are three identifying assumptions for the price to be a valid instrument: monotonicity, exclusionary restriction and relevance.

First, the exclusionary restriction requires that changes in price affect earnings only through the choice of major. This seems a reasonable assumption since the differential tuition in Business school was made by the the School of Business who were not responding to earnings. Second, monotonicity is satisfied by the law of demand: the lower the relative price, the higher the chance of majoring in Business. Finally, relevance of instrument can be tested by the first stage. Note that the discussion of the effect of the policy on the choice of major so far contributes to the argument that there is a strong first stage.

Table 22 reports the coefficients of the IV regression for graduating in Business. Note that the sample does not allow identification of the returns to Engineering due to power issues. Panel A reports the estimates of the first stage. I also control for dummies of gender, race, residency, below median test scores and the post period (after the policy) The F stats in all the specifications is above 10 and ranges between 12.8 and 76.06 (Stock and Yogo, 2002). The instrument therefore above 10 and satisfies standard rule of thumb tests for a weak first stage. The IV estimates suggest that students experience an earnings gain of 20,776 dollars by graduating in Business. The IV estimate is higher than the OLS estimates suggesting that the complier group is the group of students who gain more than than the average difference in earnings. Interestingly, this can also be suggestive evidence that earnings differences are not what motivate students to respond to these prices.

I also conduct the IV estimation in sub-samples of students who are below median ACT test scores, below median income and both below median income and test scores. Here the instrument is just the continuous price variation. The first stage is the strongest for the last sub group and the coefficient on the relative price is also the largest. For this group the earnings loss is 16,721 dollars.

8 Cost-Benefit Analysis: Who gains and who loses?

In order to assess the effectiveness of this policy, I conduct a cost-benefit analysis. The benefits of this policy to the school is the revenue generated by the school in the period over which the surcharge was implemented. Note that this is an upper bound on the revenue generated by the department since in many state universities, the increase in autonomy of revenue generation of the school is often accompanied by a decrease in grants from the university (a phenomenon referred to as the *fly-paper effect*).

Table 23 reports the total revenue generated due to the policy against the total revenue generated by the university. The upper bound on the increase in revenue for school is given by the demand of the program times the amount of differential paid over an undergraduate degree. Since the differential applies to two years, the mean amount of the differential paid by each student is 2,000 dollars.

From the perspective of the students, the cost of the program is the change in earnings due to not pursuing the high-price major. From Section 7.2, the instrumental variable estimate provides us the earnings loss precisely for the group of students who did not pursue the major. Since the instrumental variables provides us causal estimates of the initial earnings loss, we can extrapolate these earnings over the life-cycle to generate lifetime earnings losses for these students. The present discounted value of earnings loss is calculated using a discount rate of 3 percent (Avery and Turner, 2012) for a time horizon of ten years. The loss is the difference between Business and other majors (excluding Engineering). I use information from the most recent survey of the American Community Survey for 2018 that provides earnings by majors. I use information on annualized earnings and restrict the sample to full-time and full-year workers (52 weeks). Figure A-4 displays the age-earnings profiles by major categories. Business and STEM majors earn more than Humanities, Social Science and Education majors at all points of the life cycle and the steeper profiles for Business and STEM suggest that earnings in these majors grow at a higher rate. I then group majors into broader categories and separately regress earnings on age and age squared for each major to generate major and age specific growth rates. Using this information, I extrapolate the earnings for Business and Non-Business Majors for a ten year horizon.

Table 23 reports the earnings losses from not choosing Business for the group of students responsive to this policy. I find that the these students cumulatively lose 279, 003 dollars in a post-graduation ten year horizon suggesting that the lifetime earnings losses can be substantial. Although, the literature does not provide us a directly comparable estimate, similar lifetime earnings gains from majors have been calculated by Hershbein and Kearney (2014) who report that lifetime-earnings between majors range from 800,000 to 1.2 million dollars. Their estimates of

differences are larger since they are considering a longer time horizon. Unlike the earnings losses estimates from Section 7.2, they do not control for selection in initial earnings and are not restricted to students graduating from a selective flagship public university. Similarly, Avery and Turner (2012) calculate the lifetime-earnings of a college degree to be 1.19 million dollars, which is nearly double of a high school degree and 335,000 more than earnings from a typical associates degree.

Using the tax schedule for the state of Wisconsin, I calculate the tax revenue loss incurred from the policy. The average income tax rate for Wisconsin residents is 6.27 percent for annual earnings between 23,520 and 258,950 dollars plus 1,046.64 dollars [6]. Using this schedule, I estimate the loss to the stock of tax revenue due to the decrease in lifetime earnings incurred by the students. The per-student loss in tax revenue is calculated as 17,494 dollars. To account for out-of-state migration of residents after graduation, I use survey information on the percent of resident students leaving Wisconsin after graduating in Business as reported in A-3 as 60 percent. Therefore, the loss in tax revenue per cohort of students is 3 million dollars. A potential concern could be that these policies result in changes in the percent of students migrate out of state for example, in the case of Cal Grants in California (Bettinger et al., 2019). Past work has found mixed effects on the effects of subsidies and cross-state tuition differences on out-migration rate(Cornwell et al., 2006; Pallais, 2009; Kennan, 2015). However, these cross state tuition differences and subsidy levels are much more generous and the timing of these subsidies in most cases is prior to college enrollment, so that students are not locked into their choice of college. In the case of differential pricing policies, students are typically locked into their college choices and so we expect effects on migration to be not substantial. In order to check the sensitivity of these estimates, I also constrast these results to those derived from OLS estimates of wage regressions.

Moving on, I now discuss several potential threats to the validity of the empirical design and my approach towards addressing them.

9 Robustness

There are two main concerns about identification of the difference in difference. First, there may be other supply side policies from the school that may confound the estimates. Second, there may be systematic changes in the composition of the residents and non resident groups around the time of the policy change that may be generating these results. I know discuss my approach for addressing these concerns.

9.1 Other Supply Side Policies

Selectivity and Admission Criteria

A potential confounder of these estimates is that there may be changes in the admissions policy that coincide with the introduction of this policy. One objective indicator of changes in admissions

[6] refer to the Wisconsin Department of Revenue Website for details on the tax schedule (https://www.revenue.wi.gov/Pages/FAQS/pcs-taxrates.aspx)

policy is the GPA cutoff set by the university. In the United States, most universities or schools do not have an externally set GPA cutoff (Andrews et al., 2017). In UW-Madison as well, admission to the School of Business relies on a combination of completion of course requirements, GPA as well as an essay component.

Conversations with university officials suggested that there were no notable changes in the admissions process criteria during the time of the policy. In order to confirm this, I exploit the rich information in the data. Using the semester level data, I can back out the composition of students in terms of their GPA prior to being admitted. That is, for each cohort, I consider the distribution of the GPA of accepted students, but before they were admitted in that program. If there were substantial changes to this criteria, we would see a change in the distribution of the GPA after the policy was introduced. Figure 7 plots the GPA cutoff for each cohort of admitted students. The closest proxy to a GPA cutoff would be the minimum GPA admitted. However, since this can be a significantly noisy measure, I look at the 5th percentile of the pre-admission GPA for students in the program. There has not been substantial changes in the GPA cutoffs around 2007, which is the time of the introduction of the policy. Similarly, the overall distribution of pre-admission GPA in terms of the mean and 75th percentile for the students who are admitted has also not changed during the time of the policy. This suggests that there were no substantial supply side policies that may have changed the exclusivity of the program.

Program Quality and Expansion

This section continues the discussion on the responses of the university and addresses concerns that these policies may result in changes in program quality. For example, the increase in revenue can potentially result in an increase in the class size i.e. expansion of the program or in an increase in the quality of the program. The quality of the program can be measured in many different ways. For example, in the case of higher education, the number of instructors or the budget allocation towards instruction and student services can be informative about the quality of the program.

Program specific budget information over this time period is not readily available, across institutions. I therefore use archival information from the university on the historical budget allocations, referred to as the *Red Books*. These red books contain information about the overall budget across the University of Wisconsin-System as well as details of the budget within the various schools. This allows me to track the budget allocated to the School of Business as well as the School of Engineering and the breakdown.

Figure 8 plots the historical trends in budget expenditure by the School of Business. There are five potential categories of budget expenditure namely: instruction, research, academic support, student services and miscellaneous. If the policy change resulted in any changes in budget expenditure, we would expect to see changes from the trend in the budget composition. As Figure 8 shows, although there is an upward trend in instructional expenditure over the entire period, there is no noticeable trend break around the time of the policy. Therefore, this suggests that the amount of revenue generated did not substantially change the composition of the budget and

consequently change the resources provided for instruction.

A back of the envelope calculation of the revenue raised as a fraction of the total expenditure by the school of Business suggests that it constitutes a very small increase of the total budget. Therefore, it is expected that this would not substantially affect the quality of the program.

The second potential channel is an increase in the number or quality of instructors or the faculty to student ratio. The faculty-student ratio has been traditionally used as a measure of the quality of instruction in several settings Using semester level course-taking information, I construct a similar measure of number of course-takers per instructor for every year for all courses that are categorized under the umbrella of the Business school. Interestingly, with higher revenue, there is an increase in the number of coursetakers per instructors in Business although there is no substantial break from trend as shown in Figure 9.

9.2 Other Potential Mechanisms

In this section, I rule out the presence of other mechanisms that may be generating these results.

Placebo Test

One possible threat to identification is that recession may have affected non-residents differently than residents. Therefore any changes in the share of non residents doing Business can be attributed to the recession rather than to the change in prices. To address this concern, I conduct a placebo test where the placebo outcome is a dummy for graduating in agriculture and life sciences. This is a major that is unlikely to be a substitute to Business and Engineering majors and would be also be affected by the recession. Therefore, if we conduct our difference in difference exercise for agriculture and life sciences as an outcome, we should expect to find a null effect on the outcome. Table 15 reports the outcome for this. It is clear that there is a null effect for agriculture and life science which serves as a placebo major, suggesting that our results in Business are emerging from the price and are specific to Business rather than the systematic difference between residents and non-residents across all majors in the year of the policy.

Composition of non residents

One potential concern is that there might be changes in the composition of non residents. This may alter the decision of out-of-state students to choose to pursue their undergraduate education in UW-Madison. I addressed this in the baseline specifications by controlling for the average cost of education in their home state. I do this by using information on from the IPEDS on instate tuition by year and collapse this information by the state. I include the log of the average instate tuition in their home state in their admit year as a control in the specifications.

An additional source of concern is that, there might have been changes in labor market opportunities in the home states of non-residents that may be different than the changes in labor market opportunities for residents. To address this concern, I plot the GDP growth rates for 3

states that comprise 80 percent of the under graduate population: Wisconsin, Illinois, Minnesota and Michigan in Figure 6. I use data from the Bureau of Economic Analysis on the time series of annual state level growth rates for these states. The trends for these states are largely similar during the recession. The exception is Michigan which saw a larger decline in GDP growth rate during the recession, although Michigan residents make up less than 5 percent of the population of undergraduate population.

10 Mechanisms: College major choice model with borrowing

Although the reduced form estimates provide us an understanding of the effects of the policy and the heterogeneity in these effects, they are unable to shed light on the mechanisms that might be generating these outcomes. Further, they allow us to explore a limited set of policies. In order to expand the breadth of policies that I can evaluate, I impose some more structure on the problem. Using the model and estimates from the reduced form analysis, I will be able to study the responsiveness of students to a combination of policy levers such as merit aid, need based aid and borrowing based policies.

The heterogeneity in response to differential tuition policy can be coming from two possible channels: direct price effects and borrowing constraints. For example, if the effects are indeed coming from borrowing constraints, then the policy recommendation to minimize the distortions in student behavior would rely on relaxing borrowing limits. This may not be useful if the heterogeneity in response comes from direct price effects The distinction in this case is particularly important as the policy implications are quite different in each case. We therefore need a model to understanding the mechanisms and make sound policy predictions.

I develop a simple framework to highlight the key predictions of pricing policies on college major choice. I use a simple model of human capital accumulation. The modeling decisions are driven by the availability of data and the ability to identify the key parameters of the model. The model brings together features of models of college major choice (Altonji et al., 2016b) with features of models that study savings and credit constraints in the context of human capital accumulation (Abbott et al., 2019). Typically college major choice has been studied in isolation from savings decisions. However, with the increase in the levels of student debt and income based repayment policies, there is an important gap in the literature regarding the mechanisms in such models.To my knowledge, this is the first paper to model and quantify how income, borrowings and college major choice interact within the same framework. In doing so, I may abstract from several other features of these models that may not be central to my research question but those that a rich literature has studied such learning about one's own ability as well as learning about the market. Given that these prices are typically levied for upperclassmen, most of the uncertainty facing students about college has been resolved and so omitting learning would not be unreasonable in this framework.

10.1 *College major choice with a borrowings decision*

I follow the approach of Altonji et al. (2016a) in their lifecycle model of educational choices and combine it with features of models of borrowing constraints studied frequently in the context of higher education policies (Abbott et al., 2019; Brown et al., 2012). Unlike Altonji et al. (2016a), I focus on the period of specialization in college and abstract from the decision to attend college. This model is specified to be flexible enough to understand how pricing and short term borrowing constraints interact.

Timing. The model begins after students have enrolled in college and are making a specialization decision. The decision to not attend college is not available in my sample and therefore, I will model decisions of students after they have enrolled in college. There are two stages in the model. Stage 1 is the stage of college specialization and Stage 2 is the labor market stage.

Stage 1. The model is focused on studying the interaction of tuition and borrowing and its effects on major choice. Since these policies are often tied to a *declared major*, and are typically not experienced by freshmen, I focus on the specialization decision as a one stage problem. Importantly, since the major specific tuition applies only to higher level students with a declared major, the model can abstract from the process of learning in the earlier periods of being in college. This process of learning, that may be critical in studying other aspects of college major choice such as ability sorting, may not be as important in the case of income sorting. I will abstract from uncertainty about ability and assume that uncertainty about ability has been resolved prior to entering Stage 1.

Stage 2. This stage represents the labor market stage. Students graduate from college and enter the labor market. Their earnings in the market depends on their specialization in the previous stage as well as their ability. Agents also repay their student debt after completing college. In keeping with the time horizon of standard student debt plans, Stage 2 is equivalent to ten years in the labor market. Due to these assumptions, the model does not have to account for issues later in the life-cycle.

Initial Conditions. Each student enters the model with the initial conditions, Ω_1. This includes their family income (Y), their pre-college ability, $\theta = \{\theta_m, \theta_v\}$ and cumulative GPA so far (GPA). Their pre-college ability is two-dimensional, representing math and verbal ability and is measured by their ACT math and verbal test scores. Ability (θ) and family income (Y) have a joint distribution $F(\theta, Y)$.

$$\Omega_1 \equiv \{ \underbrace{Y}_{\text{family income}} , \underbrace{\text{GPA}}_{GPAsofar} , \underbrace{\theta}_{\text{pre college ability}} \} \tag{6}$$

Individual's Problem.

The choice problem is given by the following expression:

$$V_1(\Omega_1) = \max_{j,d} \left\{ \left\{ \phi V_1^j(\Omega_1) + \nu_j(\Omega_1) + \epsilon_j \right\}_{j \in J}, \phi V_1^d(\Omega_1, \zeta_d\ ) + \epsilon_d \right\} \tag{7}$$

where $k \in J$ if $\mathbb{1}(\text{GPA} > \text{GPA}_k^*)$ for $k = h, l$. Here, $V_1^j(\Omega_1, \epsilon_j)$ denotes the pecuniary value of choosing a major j in Stage 1. Additionally, students also have an outside option of dropping out (d). The value of choosing the outside option d is given by $V_1^d(\Omega_{t=0}, \epsilon_d, \zeta_{d0})$. Additionally, students also have idiosyncratic taste shocks specific to each major, ϵ_k and I assume that this shock is type 1 extreme value distributed. Finally, students also have a non-pecuniary benefit from choosing each major $\nu_j(\Omega_1)$ that is a function of their observable characteristics. Students cannot choose their major freely. Typically, there are admissions requirements in schools such as business and engineering. To account for this, students face an additional restriction. The GPA of the student prior to choosing themajor must be higher than the threshold (GPA_k^*) that is specific to each major.

Then, the pecuniary value associated with choosing major j is defined as:

$$\begin{aligned} V_1^j(\Omega_1) \quad = \quad & \max_{\{c_1, b\}} \left\{ u_1^j(c_1|\Omega_1) + \beta^c \mathbb{E}\left\{ \{ V_{L2}^j(\Omega_2, \zeta_j) \} \right\} \right\} \\ & \text{subject to} \\ & \qquad c_1 = Y^* + b - P_j + g(Y, \theta) \\ & \qquad Y^* = m(Y) \end{aligned} \tag{8}$$

There are three components in the value of choosing major j. First, is the flow utility from consumption in stage 1, $u_1^j(c_1|\Omega_1)$ which depends on major j. The second component is the continuation value of graduating in major j, $V_{L2}^j(\Omega_2, \zeta_j)$. This is value from entering the the labor market and specializing in major j. In stage 2, the information set is updated to Ω_2. Additionally ζ_d represents an individual and major specific earnings shock realized at the time of entering the labor market for students who dropout.

Stage 1 Budget Constraint. The policy environment of differential pricing most directly operates through changing the budget constraint of students and therefore merits careful modeling of the financial constraints. The budget constraint in period 1 is a function of the parental contributions to college (Y^*), borrowing amount (b), price of college (P_j) as well as grants $g(Y, \theta)$. Parental contributions to college (Y^*) are a function of family income Y.Price of college is the policy relevant parameter that includes the cost of living as well as tuition cost. Unlike most other papers of schooling, P_j is modeled to depend on the choice of major.

I follow the setup in Abbott et al. (2019) to reflect current US borrowing and financial aid environment. There are three type of loans available to students: subsidized loans, unsubsidized loans, which are both forms of Federal student loans and private loans, which are provided by the

private market. These loans are available to students depending on their wealth. For example, subsidized loans are available to poor students or income quartile 1 students and also are subject to a limit. For other students and poor who would like to borrow in excess of the this limit, unsubsidized loans are available that are subject to a higher limit.

Students with high income parents can also borrow from private loans at an interest rate r_p. As is the case, typically these private loans are not available to poor students since the rates can be prohibitively high for poor parents with low credit scores (Ionescu and Simpson, 2016). I therefore make an equivalent assumption that poor students cannot access these private loans. Finally, the financial aid students receive is given by $g(\theta, Y)$. Even though a majority of federal aid is need based, institutional aid is provided based on both need and merit. Therefore, I allow grants to depend on family income (Y) as well as θ that is $g(\theta, Y)$.

Labor Market. The value of entering the labor market conditional on graduating in major j is given by $V_2^j(\Omega_2)$. Formally, the information set is updated to include the borrowing amount from the previous period and the choice of specialization.

$$\Omega_2 = \{b, j, \theta\}$$

Now, the labor market value is defined as:

$$\begin{aligned} V_{L2}^j(\Omega_2, \zeta_j) &= \max_{\{c_t\}_t} \sum_{t=0}^{T} \beta^{t-1} \mathbb{E}\{u^L(c_t)\} \\ &\quad \text{subject to} \\ &\qquad c_t \leq W_{jt} - (1 + r^L)(b/T) \\ &\qquad W_{jt} = f_{jt}(\theta, \zeta_j) \\ &\qquad \zeta_j \sim N(0, \sigma_j^2) \end{aligned}$$

The problem is essentially agents choosing their consumption in each period to maximize the present discounted value of their consumption. Agents in this simplified setting do not make a savings decision and instead consume their earnings in each period net of their student loan repayment [7]. The repayment scheme modeled here reflects the fixed payment repayment scheme wherein students repay a fixed payment in every period for upto T years.

Value from outside option. The value of not choosing either majors is equivalent to dropping out. In period 1 is given by $V_1^d(\Omega_1, \zeta_d)$. Students directly enter the labor market and don't have to repay any student debt. Since the family contributions are contingent on going to college, agents do not receive family contributions if they choose the outside option.

[7] One concern about this assumption is that not allowing students to smooth consumption in the labor market might be constraining students more. However, I also estimate a version of the model where students are allowed to perfectly smooth consumption and I find that the estimates remain largely unaffected by this assumption.

$$
\begin{aligned}
V_1^d(\Omega_1, \zeta_d) &= \max_{\{c_t\}_t} \sum_{t=0}^{T} \beta^{t-1} \mathbb{E} u^d(c_t) \\
&\quad \text{subject to} \\
&\qquad c_t \leq W_{dt} \\
&\qquad W_{dt} = f_{dt}(\theta, \zeta_d) \\
&\qquad \zeta_d \sim N(0, \sigma_d^2)
\end{aligned}
$$

Model Parameterization and solution. Moving on, I describe the model parameterization. First I describe the flow utility from specialising in major j, $u_1^j(c|\Omega_1, \epsilon_j)$. I assume that there are two additively separable components representing utility from consumption and utility from non-pecuniary component. I assume that utility from consumption has a CRRA form with a CRRA coefficient of γ and the utility from the non-pecuniary component $\nu_j(\Omega_1)$ is a linear function of θ and gender f. Similarly the flow utility from consumption in the labor market both conditional on graduating with a major as well as choosing the outside option also assumes a similar constant relative risk aversion (CRRA) form with the coefficient of relative risk aversion being γ.

The functional assumption on ϵ results in a close form solution of the probability of graduating in each major since ϵ, is i.i.d Type 1 extreme value distribution. Additionally, if we log transform the relationship we obtain the following expression for the log odds ratio,

$$
\pi_{j|\Omega_1} = \frac{\exp(\phi V_1^j(\Omega_1) + \nu_j(\Omega_1)}{\sum_{k \in J} \exp(\phi V_1^j(\Omega_1) + \nu_j(\Omega_1)) + \exp(\phi V_1^d(\Omega_1, \zeta_d))}
$$

$$
\underbrace{\log \frac{\pi_{j|\Omega_1}}{\pi_{d|\Omega_1}}}_{\tilde{\pi}_{j|\Omega}} = \phi V_1^j(\Omega_1) + \nu_j(\Omega_1) - \phi V_1^d(\Omega_1, \zeta_d)
$$

Here $\tilde{\pi}_{j|\Omega_1}$ is the log odds ratio between major j relative to the outside option.

10.2 *Identification: Using price variation for identifying structural parameters*

In this section, I discuss the identification of the structural parameters. An important issue for identification, is the relative importance of non-pecuniary or taste parameters relative to pecuniary gains. Typically, pecuniary gains are defined as expectations of lifetime earnings in the case of college majors. A body of work has used methods such as informational interventions (Wiswall and Zafar, 2015) as well as Business cycle fluctuations (Beffy et al., 2012) to identify the responsiveness

to pecuniary returns. I contribute to this body of work by using a novel source of variation, namely the price variation induced by the policy to identify the importance of pecuniary gains. By comparing the levels of the responses of cohorts before and after the price change policy as well as the differences in the responsiveness of residents and non residents, I can identify the scaling parameter for the utility function, denoted as ϕ. The identification closely follows the identification in the design based section of the analysis. If the price change is the only difference between the cohorts before and after the policy change, then we can identify the utility gain from pecuniary components by comparing the choices of the students before and after the policy change.

Additionally, this model is different from previous models of college major choice by considering borrowing behavior. Borrowing is identified by explicitly matching moments on the borrowing behavior of residents and non residents. Additionally, the variation induced by changes in the federal borrowing limits provides an additional source of variation. The increases in the borrowing limits provide an exogenous shift to the amount students can borrow and therefore is useful in identifying borrowing constraints.

Deriving the difference-in-difference estimand

To highlight the relationship between the difference-in-differences and the structural model, I will derive the equivalent of the difference in difference estimates using the parameters for the structural model.

In a cross section, if we are interested in understand the difference in choice probabilities by income, I highlight the bias that would result without policy variation. To simplify this analysis I consider the difference in the average relative choice probabilities between below and above median students. I therefore integrate the average relative choice probabilities over the relevant area of the joint distribution of income.

$$
\begin{aligned}
& \mathrm{E}[\widetilde{\pi}_{j|\Omega} \mid Y < \overline{Y}] - \mathrm{E}[\widetilde{\pi}_{j|\Omega} \mid Y > \overline{Y}] \\
= \; & \mathrm{E}[\phi V_1^j(\Omega_1) + \nu_j(\Omega_1) - \phi V_1^d(\Omega_1, \zeta_d) \mid Y < \overline{Y}] - \mathrm{E}[\phi V_1^j(\Omega_1) + \nu_j(\Omega_1) - \phi V_1^d(\Omega_1, \zeta_d) \mid Y > \overline{Y}] \\
= \; & \phi \Big\{ \mathrm{E}[V_1^j(\Omega_1) - V_1^d(\Omega_1, \zeta_d) \mid Y < \overline{Y}] - \mathrm{E}[V_1^j(\Omega_1) - V_1^d(\Omega_1, \zeta_d) \mid Y > \overline{Y}] \Big\} \\
& + \underbrace{\mathrm{E}[\nu_j(\Omega_1) \mid Y < \overline{Y}] - \mathrm{E}[\nu_j(\Omega_1) \mid Y > \overline{Y}]}_{\text{Bias}} \qquad (9)
\end{aligned}
$$

Unless we know the joint distribution of income and ability, the bias parameter cannot be identified without imposing further parameteric assumptions. Thus, cross sectional variation in income would be useful in identifying ϕ if we know the joint distribution.

Using the policy variation, we can achieve identification of the scaling parameter ϕ if the taste function is time invariant. Let Ω_{post} be the information set after the policy change and Ω_{pre} is the information set for cohorts before the policy change. If the taste function has a time varying

component that is the same for high and low-income students, we can use a second difference and hence we can isolate the scaling parameter ϕ. The argument is analogous to the difference in difference estimation.

$$\begin{aligned}
&\overbrace{\mathrm{E}[\tilde{\pi}_{j|\Omega_{\text{post}}} \mid Y < \overline{Y}] - \mathrm{E}[\tilde{\pi}_{j|\Omega_{\text{post}}} \mid Y > \overline{Y}]}^{\Delta\pi_{j|\Omega_{\text{post}}}} - \overbrace{\mathrm{E}[\tilde{\pi}_{j|\Omega_{\text{pre}}} \mid Y < \overline{Y}] - \mathrm{E}[\tilde{\pi}_{j|\Omega_{\text{pre}}} \mid Y > \overline{Y}]}^{\Delta\pi_{j|\Omega_{\text{pre}}}} \\
&= \phi \Bigg\{ \overbrace{\mathrm{E}[V_1^j(\Omega_{\text{post}}) - V_1^d(\Omega_{\text{post}}, \zeta_d) \mid Y < \overline{Y}] - \mathrm{E}[V_1^j(\Omega_{\text{post}}) - V_1^d(\Omega_{\text{post}}, \zeta_d) \mid Y > \overline{Y}]}^{\Delta Z_{j|\Omega_{\text{post}}}} \\
&\qquad - \overbrace{\mathrm{E}[V_1^j(\Omega_{\text{pre}}) - V_1^d(\Omega_{\text{pre}}, \zeta_d) \mid Y < \overline{Y}] - \mathrm{E}[V_1^j(\Omega_{\text{pre}}) - V_1^d(\Omega_{\text{pre}}, \zeta_d) \mid Y > \overline{Y}]}^{\Delta Z_{j|\Omega_{\text{pre}}}} \Bigg\}
\end{aligned} \tag{10}$$

We can then rewrite this to obtain an expression for ϕ:

$$\phi = \frac{\Delta\pi_{j|\Omega_{post}} - \Delta\pi_{j|\Omega_{pre}}}{\Delta Z_{j|\Omega_{post}} - \Delta Z_{j|\Omega_{pre}}} \tag{11}$$

This expression is analogous to the difference in difference estimator. Thus, an estimator for phi would rely on variation between cohorts admitted before and after the policy and on variation by family income of the students. The underlying assumption here is that on average, non pecuniary-tastes are unchanged after the policy was introduced. Although in the current specification of the model, I am able identify the model using cross-sectional variation in income since I observe data on income and ability, as long as we have differential prices. However, the parametric assumption on the non-pecuniary benefits is not necessary for identification. The difference in difference estimator provides additional identifying power and therefore serves as an over-identifying test of the model.

Moving on, I describe the estimation of the model, results the model fit as well as the validation of the model using the difference in difference estimator.

11 Model Parameterization, Estimation and Results

The estimation proceeds in the several steps. I parameterize the model for four choices specific to our context: Business, Engineering, other majors and the outside option (dropout).

The estimation procedure can be broken down into two steps. First, I fix certain parameters based on the literature. There are a set of parameters that are estimated outside the model. The rich nature of the data allows me to estimate a large number of parameters outside the model. These include the wage process, grant function, as well as the joint distribution of skill and income. The last set of parameters are estimated within the model using the method of moments, conditional

on other estimated parameters. These include preference parameters in the utility function. I will now discuss in detail the estimation of these parameters.

11.1 Externally set Parameters

In this section, I discuss the first stage parameters that are externally set. Coefficient of relative risk aversion and the discount factor is set at values used in the literature at 2 and 0.97 respectively. Following Abbott et al. (2019), I parameterize the borrowing environment to match the current US environment. The borrowing limits are set at the four-year cumulative borrowing limit, for the subsidized and unsubsidized loans. The interest rate on these loans is set as the 4 year interest rate. The relationship between grants, test scores and income can estimated directly from the data. I regress the total aid, which includes both need based and merit aid on the quartiles of income and test score. Table 10 reports the coefficients of the aid function. As we would expect, higher income students receive much lower aid. Interestingly, students with higher test scores also receive less aid. I estimate the wage regression outside the model using information from the post graduate student survey. Further, growth rates of earnings by major are calculated using information from the American Community survey. Note that growth rates are constant in the estimation of the model due to data restrictions. In practice, earnings dispersion could vary over the life cycle. in order to characterize the outside option of dropout, I assume the following. First, if students dropout, they earn a high school graduate salary in UW-Madison. To reflect the current setting, I will assume that there is a GPA cutoff for Business but not for other majors. In the absence of explicit cutoffs, I use the minimum GPA admitted in Business as was discussed in Section 9.

11.2 Other Parameters

I estimate the remaining parameter vector (ω) by a Simulated Method of Moments (SMM) approach. The estimator minimizes the quadratic distance between vectors of simulated moments $M(\omega)$ and data moments M_{data} where the moments are weighted by the weighting matrix W

$$\omega^* = \text{argmin}_{\omega}[M(\omega) - M_{\text{data}}]' W [M(\omega) - M_{\text{data}}]' \tag{12}$$

The moment vector M_{data} includes aggregate shares that is the share of students who choose the high price major or dropout. It also includes the share of below-median income students who choose each of these alternatives, the share of below median math and verbal test score students in each of the 3 major categories and the share of females in each alternative. Finally, I also target moments on the amount of borrowing for low-income and high income students separately. I use 25 moments to identify 13 parameters.

11.3 Results

Moving on, I describe the results from the estimated model as well as the model fit.

11.4 Parameter Estimates and Elasticities

Table 24 reports the parameter estimates from the model estimation. These estimates include the taste parameters for staying in school and graduating in each major. As is clear, higher ability students have a preference towards graduating in Business than not in Business. Additionally, women have a preference for not graduating in Business as well. Students do have a substantial psychic gain from going to college.

In order to better interpret the magnitudes of the parameter estimates, I calculate the elasticity of major choice with respect to price and income. I begin with a baseline case of equal pricing across majors. For each individual, I simulate a 10 percent increase in the relative price of Business relative to the other majors. I then simulate the change in the probability of graduating in Business in response to this price change for each individual. I also repeat the same exercise for income. Table 25 reports the estimates of these. Column 1 reports the mean, median, 25th percentile and 75th percentile of the distribution for the full sample. Columns 2 and 3 report the same moments for the population below median income and above median income respectively. As we would expect, there is a great deal of heterogeneity in the sample. The mean or median response in the full sample is close to 0. However, low-income students are more responsive than the high income students. Additionally, the 25th and 75th percentile of the distribution is considerably different than the mean suggesting that there is a smaller population who is quite responsive to the policy change.

11.5 Model Fit

In this section, I discuss the model fit for the targeted moments. Figure 10 provides the description of the moments from the data as well the simulated moments from the model. The moments targeted include overall shares in each major, the share of below-median income students in each major. Additional I include the probability of being below-median overall score test score, below-median ACT math test score, below-median ACT reading test score, being female conditional on being in each major. Finally, I target borrowing amounts of below-median income and above-median income students. The model does a reasonable job of matching the share of students in each major, overall and by income and test score levels. The model slightly over predicts the average borrowing by income.

11.6 Model Validation: Replicating the Policy Experiment

A recent line of work has employed experiments or design based estimates to identify rich structural models without imposing additional assumptions (Attanasio et al., 2012; Chaparro et al., 2020). Another body of work validates their structural models by reproducing experimental results using data simulated from their structural model (Todd and Wolpin, 2006). In the same vein, I use the variation induced by the policy to identify key structural parameters.

Similar to existing work that has studied combined structural model estimation with reduced

form estimates, I validate my model by simulating the quasi experimental setting in our reduced form and estimate the difference-in-difference in the reduced form by using the simulated choices that are generated by the estimates of the model. The specification is the following:

$$Y_{ic} = \gamma_1 + \gamma_2 \mathrm{R}_{ic} + \gamma_3 \mathrm{Post}_{ic} + \gamma_4 \mathrm{R}_{ic} \times \mathrm{Post}_{ic} + X'_{ic}\gamma + \epsilon_{ic} \tag{13}$$

Y_{ic} is equal to one if individual i of admit cohort c graduated in the major and 0 if the individual graduated in any other major. R_{ic} is a dummy for residents and Post_{ic} is a dummy for students who were admitted *after* policy was enacted, X_{ic} includes controls for individual specific characteristics such as gender, race, and ACT math score and verbal scores .

This estimator relies on variation between residents and non-residents, before and after the policy and therefore is an unmatched parameter in the data. Figure 11 reports the coefficients of interaction between resident and post dummies from the difference-in-difference estimator in the model as well as the reduced form. The figure reports the coefficient estimated for the full sample as well as the sample of low-income students. The model and data point estimates are similar although the model estimates a slightly higher response, particularly for low income students. The model estimate lies within the confidence interval of the reduced form effect. Therefore, we can be assured that the model predicts the outcomes of pricing experiments reasonably well. Having established this, I will now consider the responses of students to counterfactual education policies.

12 Counterfactual Analysis of Education Policies

In this section, I use the estimated model to simulate various counterfactual pricing and borrowing policies. Since there are some cohorts that do experience a price change versus those that don't, I begin by establishing a baseline where I simulate the choices if there was equal pricing. I then evaluate the responses to various pricing policies relative to that baseline.

12.1 Proportional Major-specific Pricing

In this first counterfactual, I simulate the choices of the population of students under different regimes of pricing. Specifically, I first change the ratio of the price of Business to other majors to be 0.25, 0.5 and 0.75, which correspond to proportional subsidies in Business and then 1.25, 1.5 which correspond to proportional surcharges to Business. I then simulate the shares of students in each major, overall and by income status and calculate the percent change in shares relative to the baseline scenario (equal pricing). Figure 12 reports percent change in share of students doing Business for the full sample in Panel A and for below median income students in Panel B.

As we would expect, a subsidy in Business results in a substantial increase in the share of low-income students in Business are more price sensitive and for the same price change, their response is larger than for the full sample, particularly for higher levels of the subsidy. Interestingly, students are much more responsive to subsidies than to higher prices suggesting that linear extrapolation of the differences-in-differences estimates may result in biased inference.

These responses can be the result of both a price effect as well as a borrowing constraint. In the next section, I discuss the results of the counterfactual exercises that relax borrowing limits in order to understand the importance of these channels.

12.2 Relaxing Borrowing Limits

I simulate four possible scenarios: higher price in Business but no borrowing limit, equal pricing in Business and borrowing limit, and then equal pricing in both majors as well as no borrowing limits. If borrowing limits do have bite, then a policy of relaxing borrowing limits can be quite effective in increasing the share of students in Business.

Figure 13 reports the share of students in each of the three scenarios described earlier. On comparing the shares across these scenarios, it is clear that the largest differences in shares of below median income students is between the case where there are borrowing limits and where there aren't.

In the scenario where we have both price differential and borrowing limits, the share of low-income students in business is approximately 12 percent higher than the case with no price differentials and borrowing limit. With no borrowing limits, the scenario that implements differential prices sees a negligible change relative to the case of equal pricing. This suggests that if we are interested in maintaining the share of students in Business as well as the composition of low-income students, we can increase the amount of loans available to students while also increasing the price in Business. Given the substantial earnings gains in Business, students would be willing to borrow the additional amount.

12.3 Proportional Pricing with Means-Tested Grants

An alternative to increasing borrowing limits is means tested grants. This is most similar to the case of Texas, where low-income students were provided aid in order to cover the cost of the tuition differential. Andrews and Stange (2019) found that there was an increase in the share of low-income students in the high price majors.

In order to rationalize the differences in the policy responses, I simulate a policy similar to the Texas policy. Here the baseline case is different from earlier since we are interested in differences in responses due to the grant. I begin by simulate a price schedule representing ten percent proportional pricing that is the differential for business is 10 percent of the baseline tuition in other major. I then simulate several cases where below median income students are provided varying levels of aid up until a full subsidy.

Figure 14 reports the change in the share of students in Business overall and for poor students in response to the policy. The means-tested subsidies are quite effective in changing the shares of students. Even a modest 25 percent subsidy results in an increase in the share of students in the high price major. In this case, the university would still make revenue. In the most extreme case, a full subsidy to low-income students would increase the share of low-income students in Business by approximately 11 percent relative to the case with no subsidies, this is substantially

large. Even at full subsidy, this policy does not require additional resources other than that raised by the price. Even if the department would like to raise additional revenue and not provide a full subsidy to students, it can do so and would still be able to decrease the number of low-income students opting out of these majors.

13 Conclusion

This paper exploits the introduction of a tuition surcharge policy in a large selective university to answer the question of how these differential costs can affect sorting of low-income students into these majors. Using the unique structure of the policy, I implement a difference in difference design and compare the out-of-state (high income) students to the in-state students (low-income) students. I then use the variation across cohorts to look at whether these policies do change the major choice of students.

I find that these policies resulted in a decrease in the demand for the surcharged majors in both Business and Engineering but a larger decline in Business. The policy can also have unintended consequences on several other academic margins. A series of robustness checks rule out the possibility of the effects being confounded with other channels.

This paper also proposes a instrument in the form of price variation induced by the policy to estimate the returns to the major. The IV estimates are larger than the OLS estimates suggesting that the complier group is actually the group with higher earnings.

Motivated by the empirical evidence, I develop a model of college major choice with credit constraints. Students weigh the earnings gain net of the costs of specializing in the major against non-wage considerations. Borrowing limits and GPA restrictions constrain their choice set. Counterfactual policy experiments suggest that students are fairly sensitive to prices of majors. Relaxation of borrowing limits and means tested subsidies can attenuate the effects of these pricing policies.

Finally, a back of the envelope suggests that even the upper bound on the revenue results in an overall loss to the state government that is balancing these tuition policies for public universities against improving the human capital of its future workforce. Therefore, a careful consideration of the longer term effects of this policy is important for policy makers.

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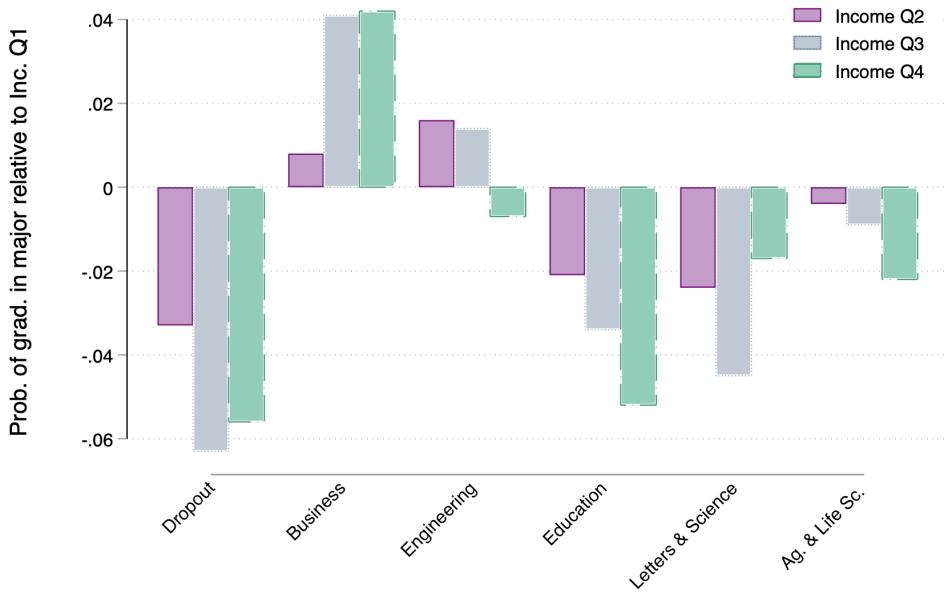
Tables and Figures

Figure 1: Income gradient in Major Choice

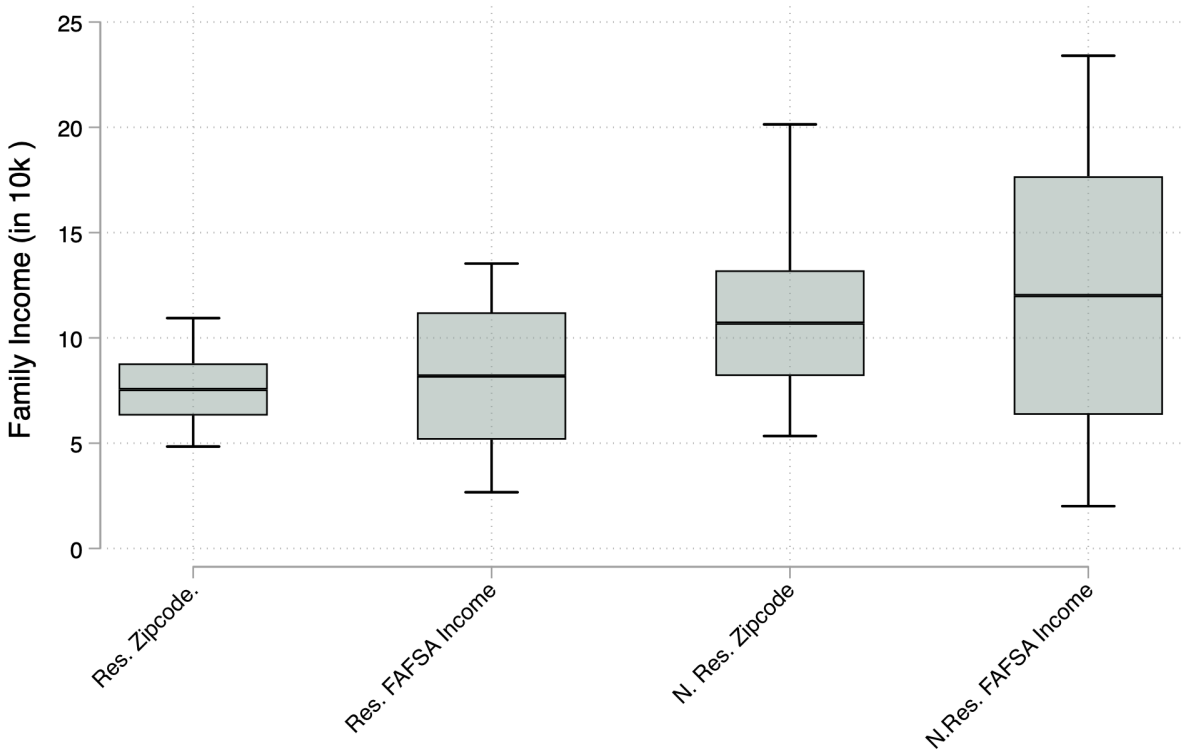


Note: This figure reports the coefficients for the quartiles of income for the regression of major dummy on quartiles of income and test scores ($Q_4 > Q_3 > Q_2$). These quartiles are defined as quartiles of the distribution of income and test scores for students enrolled UW-Madison. The specification I use is the following:

$$Y = \gamma_1 + \gamma_2 Q_2^{inc} + \gamma_3 Q_3^{inc} + \gamma_4 Q_4^{inc} + \gamma_5 Q_2^{testscore} + \gamma_6 Q_3^{testscore} + \gamma_7 Q_4^{testscore} + \epsilon$$

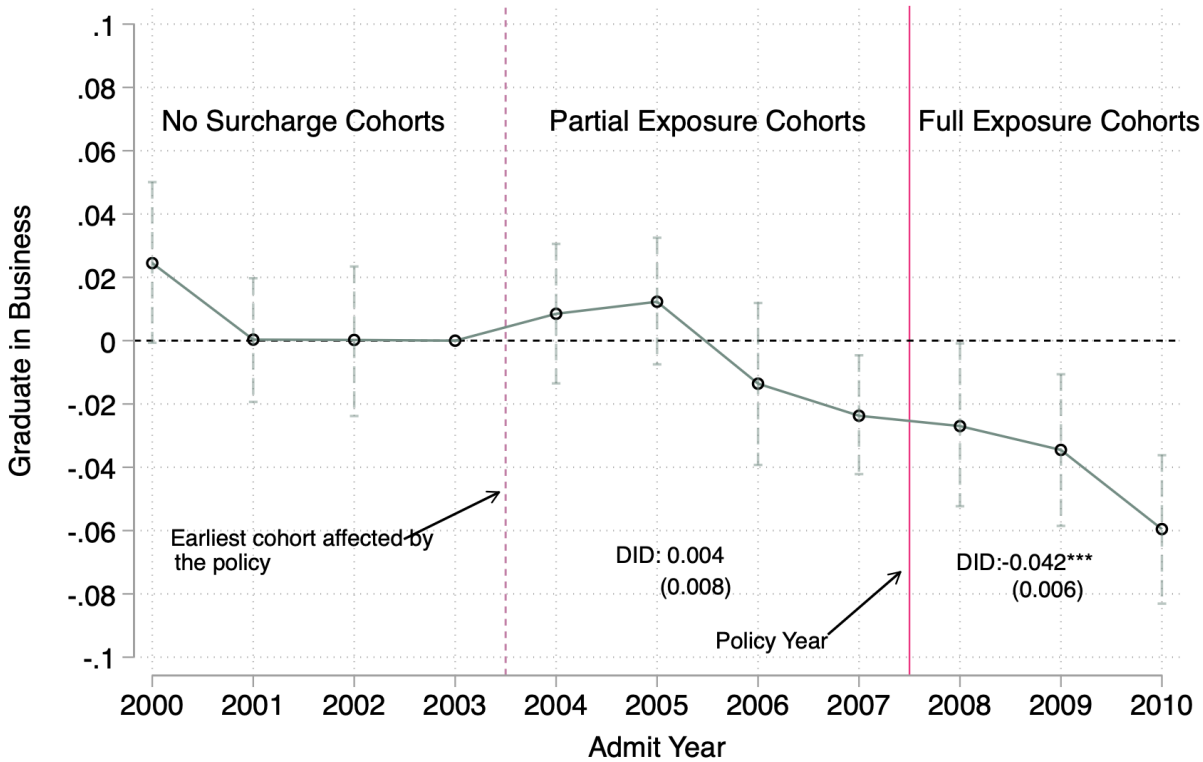
The sample includes all domestic and dependent undergraduate students that filed for Free Application for Federal Student Aid (FAFSA). The income is defined as income in their first year application.

Figure 2: Family income measures by residency



Note: This figure reports the distribution by residency of the mean family income and mean zip code income for domestic undergraduate students in cohorts admitted between the years 2000 and 2012. The zip code level mean income is obtained from data released by the US Census Bureau in the 5 year ACS estimates for 2006-2010.The sample consists of students who filed for the Free Application for Federal Student Aid (FAFSA) in these cohorts.

Figure 3: Event Study Analysis for Graduation in Business

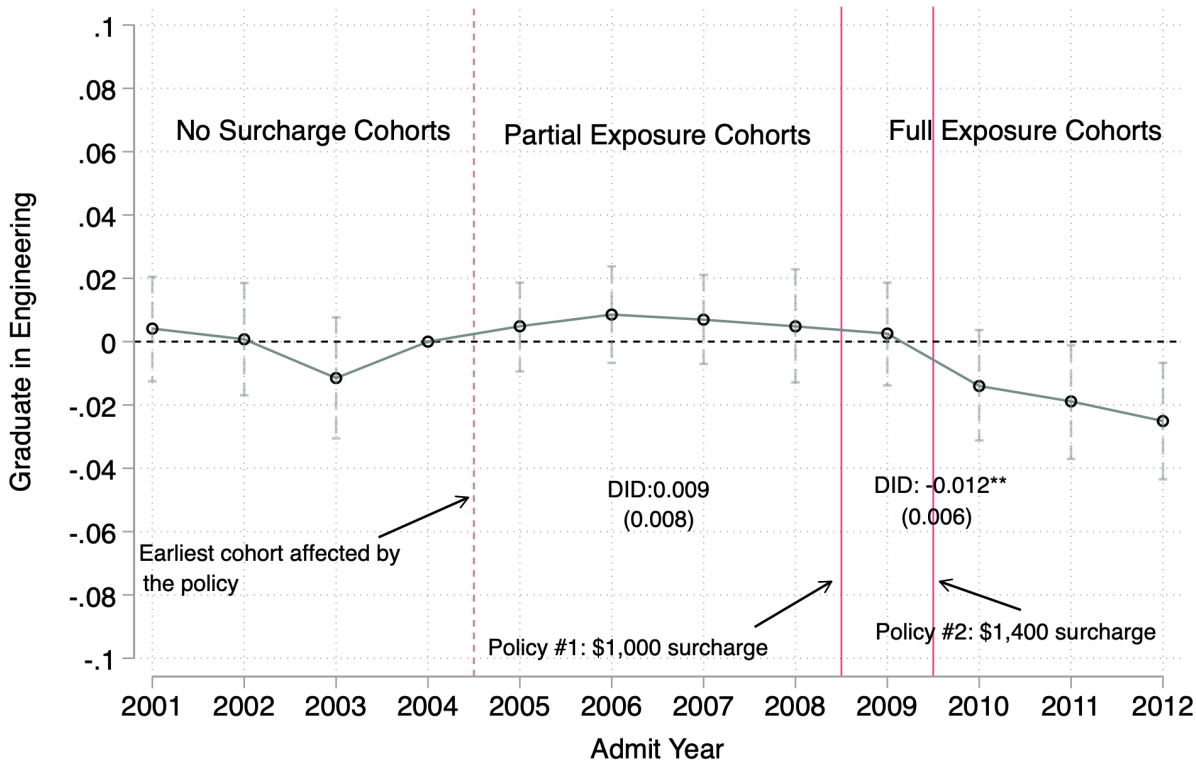


Note: This figure reports the coefficients from the event study with the following specification:

$$Y_{ic} = \beta_0 + \beta_1 R_{ic} + \overbrace{\sum_{\tau=2000}^{\tau^*-5} \boldsymbol{\alpha_\tau} \mathbb{1}(\tau = c) R_{ic}}^{\text{No Surcharge Cohorts}} + \overbrace{\sum_{\tau=\tau^*-3}^{\tau^*-1} \boldsymbol{\alpha_\tau} \mathbb{1}(\tau = c) R_{ic}}^{\text{Partial Exposure Cohorts}} + \overbrace{\sum_{\tau=\tau^*}^{T} \boldsymbol{\alpha_\tau} \mathbb{1}(\tau = c) R_{ic}}^{\text{Full Exposure Cohorts}} + \lambda_c + X_{ic}'\gamma + \varepsilon_{ic}$$

where Y_{ic} is a dummy for whether individual i of admit cohort 'c' graduated in the major. $\tau*$ is the year of the policy change in Business. R_{ic} is a dummy for resident, $\mathbb{1}(\tau = t)$ is an indicator variable for admit cohort $\tau = c$ and λ_c is a fixed effect for admit cohorts. X_{ic} includes individual level controls such gender, race, high school GPA, age at entrance, ACT math score and verbal score as well as dummies for missing high school GPA, gender, race, test scores. ACT scores imputed for students who took the SAT. 95% confidence intervals are clustered at rthe esidency and admit cohort level. The sample consists of degree seeking undergraduate students.

Figure 4: Event Study Analysis for Graduation in Engineering

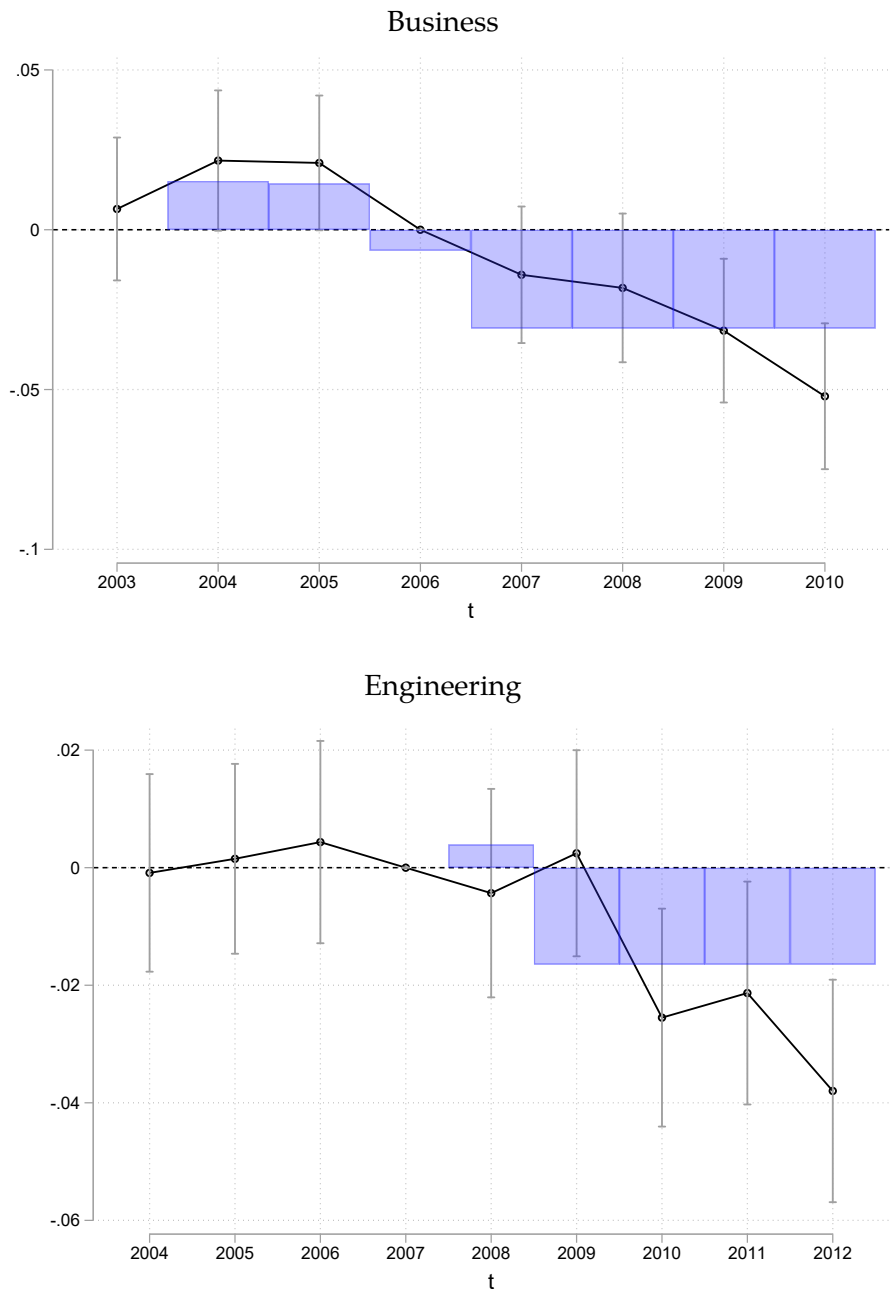


Note: This figure reports the coefficients from the event study with the following specification:

$$Y_{ic} = \beta_0 + \beta_1 R_{ic} + \overbrace{\sum_{\tau=2000}^{\tau^*-5} \boldsymbol{\alpha_\tau} \mathbb{1}(\tau = c) R_{ic}}^{\text{No Surcharge Cohorts}} + \overbrace{\sum_{\tau=\tau^*-3}^{\tau^*-1} \boldsymbol{\alpha_\tau} \mathbb{1}(\tau = c) R_{ic}}^{\text{Partial Exposure Cohorts}} + \overbrace{\sum_{\tau=\tau^*}^{T} \boldsymbol{\alpha_\tau} \mathbb{1}(\tau = c) R_{ic}}^{\text{Full Exposure Cohorts}} + \lambda_c + X_{ic}'\gamma + \varepsilon_{ic}$$

where Y_{ic} is a dummy for whether individual i of admit cohort 'c' graduated in the major. $\tau*$ is the year of the first policy change in Engineering. R_{ic} is a dummy for resident, $\mathbb{1}(\tau = t)$ is an indicator variable for admit cohort $\tau = c$ and λ_c is a fixed effect for admit cohorts. X_{ic} includes individual level controls such gender, race, high school GPA, age at entrance, ACT math score and verbal score as well as dummies for missing high school GPA, gender, race, test scores. ACT scores imputed for students who took the SAT. 95% confidence intervals are clustered at the residency and admit cohort level. The sample consists of degree seeking undergraduate students.

Figure 5: Effect of the Amount of the differential



Note: The connected graph in this figure reports the coefficients from the event study with the following specification:

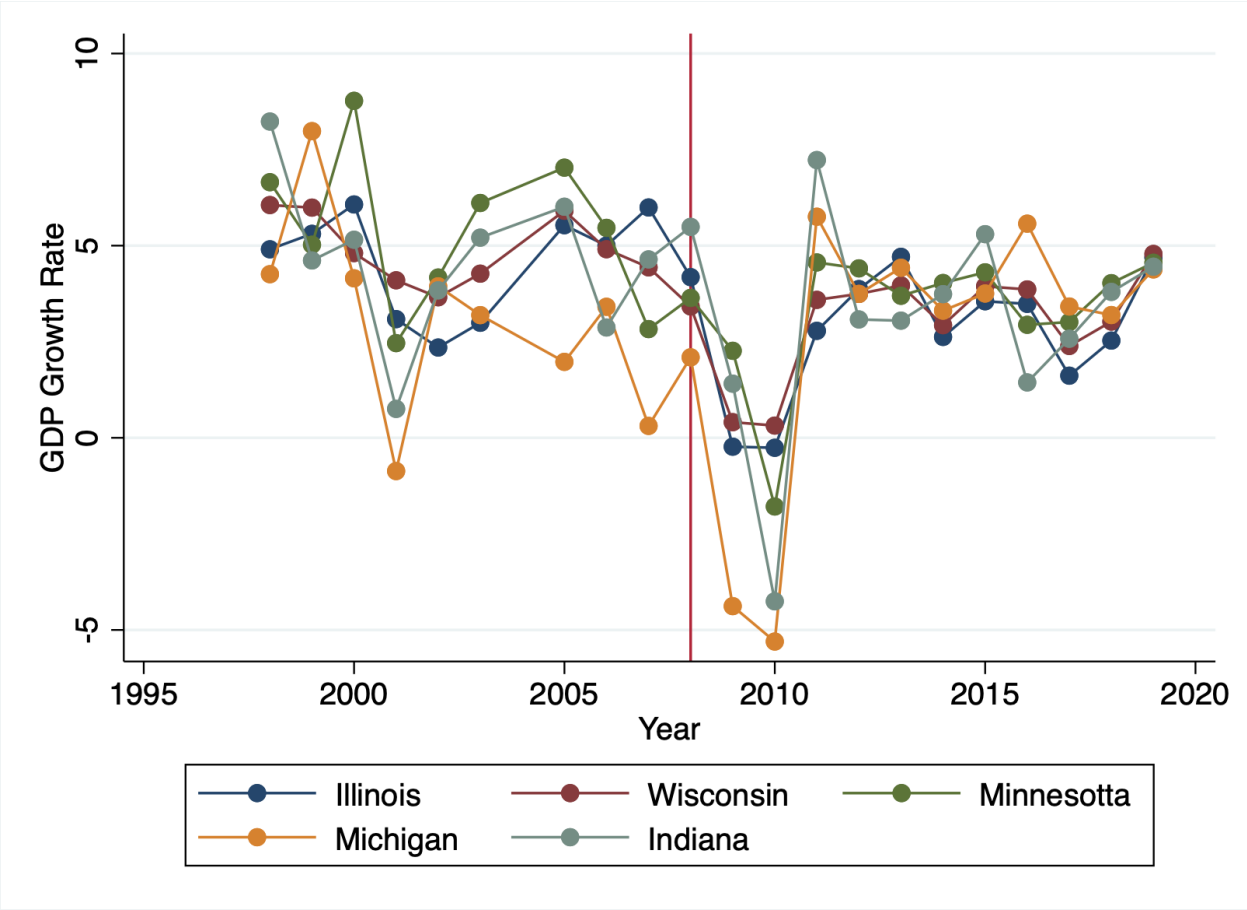
$$Y_{ic} = \beta_0 + \beta_1 R_{ic} + \beta_2 X_{ic} + \sum_{\tau \neq \tau^* - 1} \alpha_\tau \mathbb{1}(\tau = t) R_{ic} + \lambda_c + \varepsilon_{ic}$$

where R_{ic} is one if individual i of cohort c was a resident. α_τ represents the differential of residents relative to non residents in cohort τ relative to one year prior to the implementation. Y_{ic} is equal to one if individual i of cohort c graduated in the major. The bar graphs show the effect of the amount of the differential and plot the coefficient estimates from the following specification:

$$Y_{ic} = \beta_0 + \beta_1 R_{ic} + \beta_2 X_{ic} + \beta_3 (P_{ic} - P_{ic}^{LS}) + \alpha (P_{ic} - P_{ic}^{LS}) R_{ic} + \varepsilon_{ic}$$

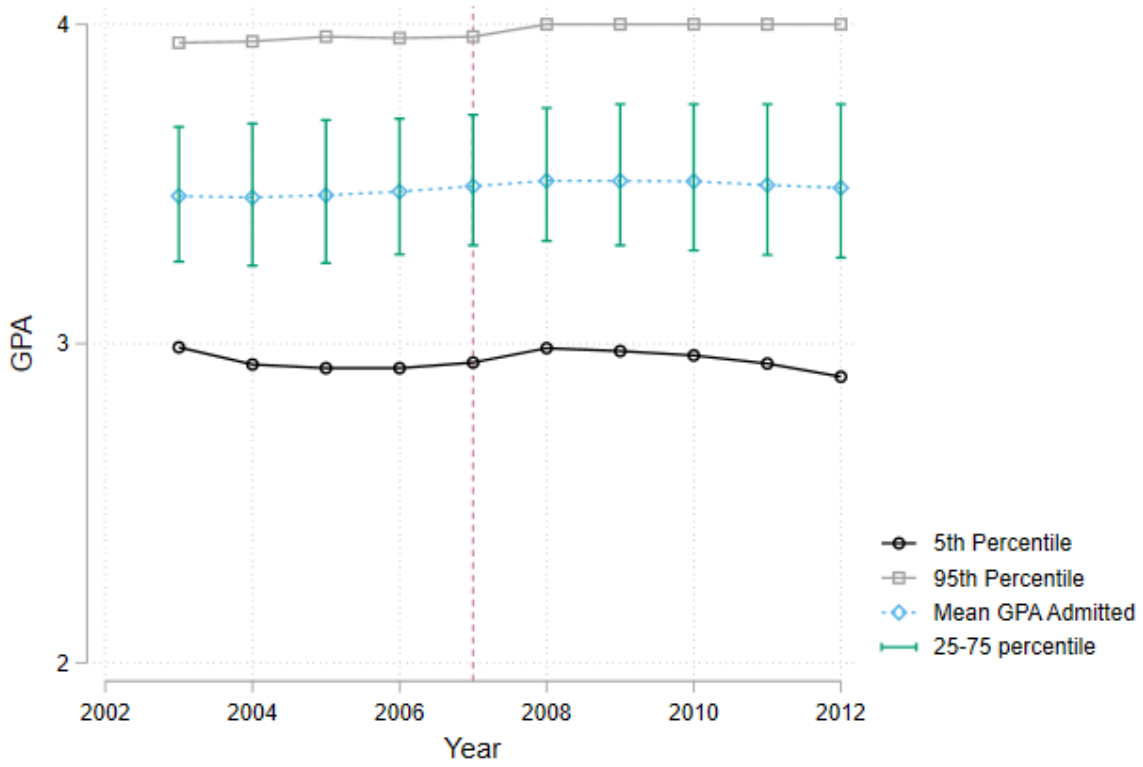
$(P_{ic} - P_{ic}^{LS})$ is the price differential between the major and Letter and Science. Controls include dummies for gender and race categories, ACT test scores high school GPA, age at entrance and dummies for missing covariates. Additionally, for students who take the SAT have imputed values for ACT.

Figure 6: Growth Rate of GDP



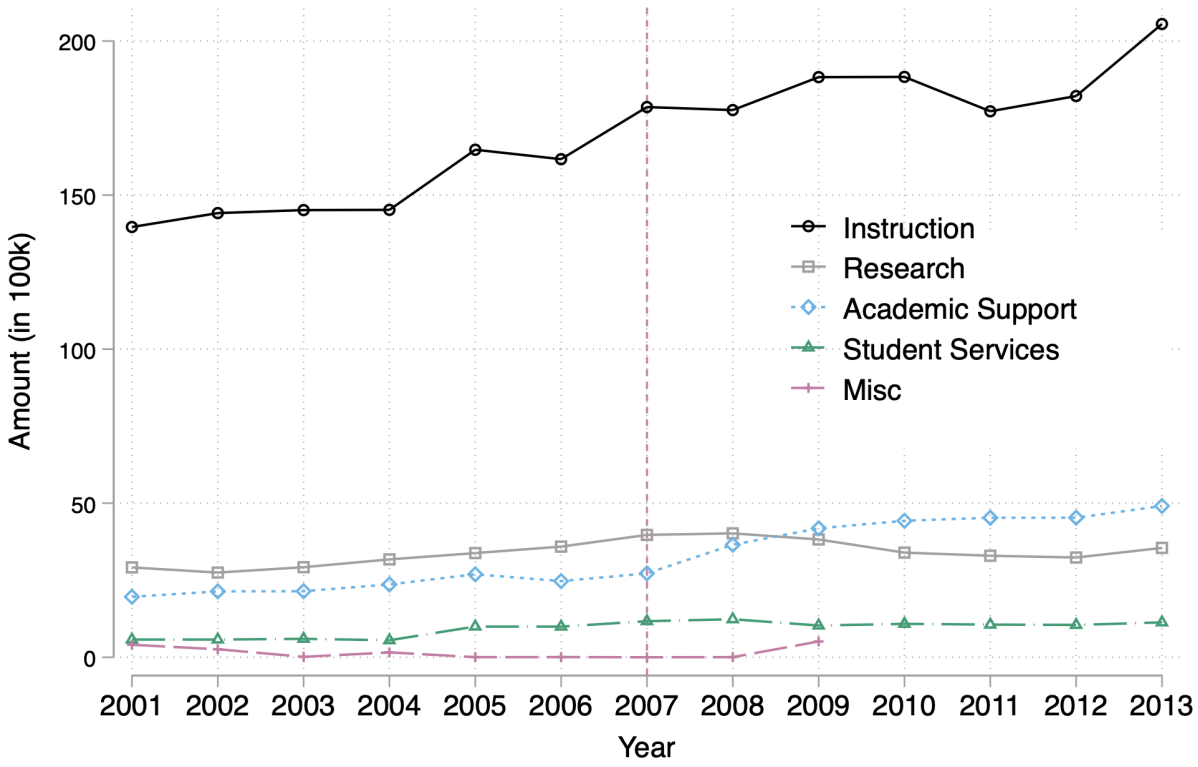
Note: This figure reports the time series of GDP growth rates by state that have the 5 largest percent of the population of undergraduate population in UW-Madison.
Source: Annual GDP time series by Bureau of Economic Analysis.

Figure 7: Distribution of GPA of students admitted by year



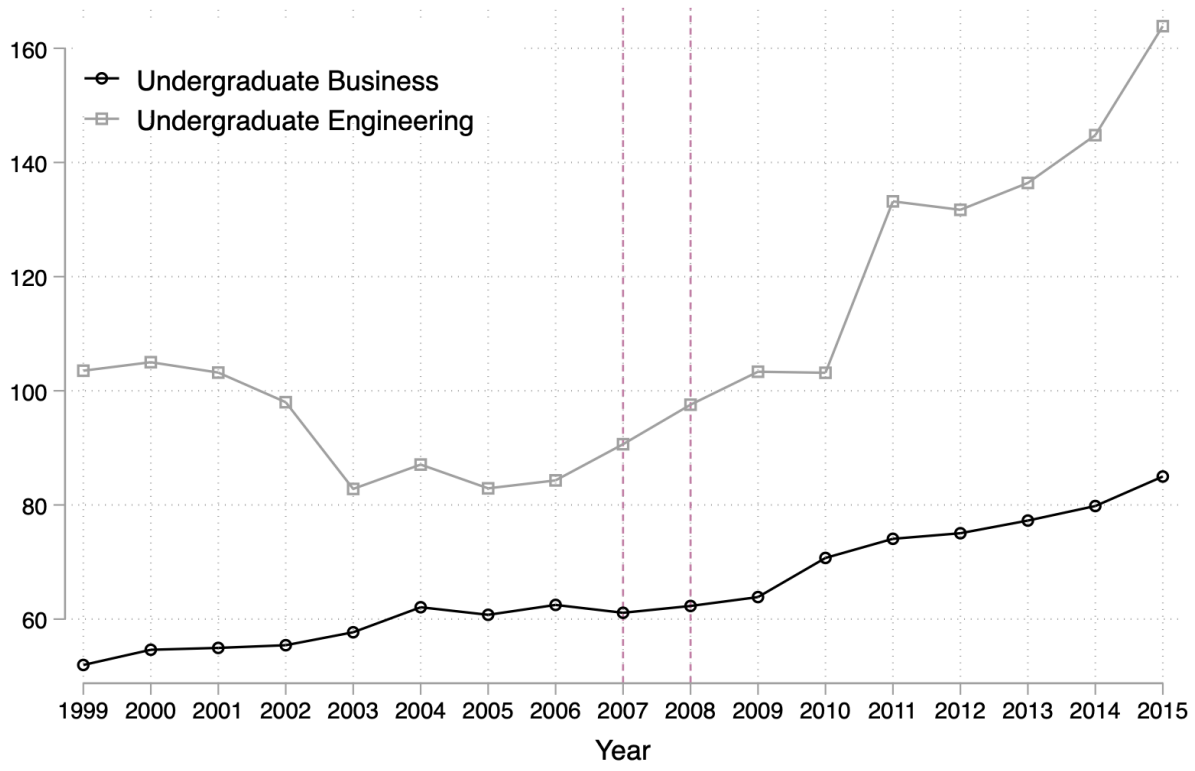
Notes: This figure reports the distribution of overall GPA of the cohorts admitted to the School of Business for each calendar year. The GPA reported on the vertical axis is the cumulative GPA of admitted students in the semester prior to entering the program. The horizontal represents the calender year. The figure reports the Mean, 5th, 25th , 75th percentile and 95th percentile of the distribution. Note that the School of Business does not have a sharp cutoff for admitting students and instead, relies on a combination of grades, course requirements and an essay component.

Figure 8: Budget Expenditure of the School of Business



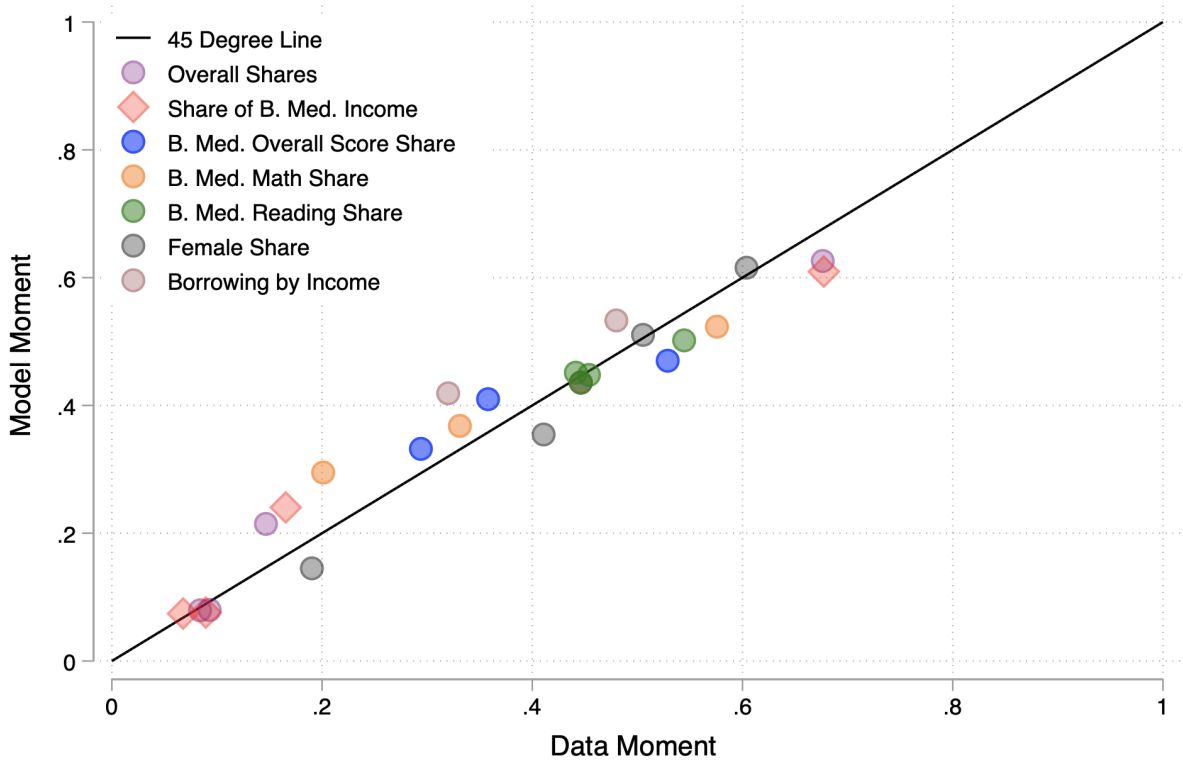
Notes: This figure reports the historical time series of budget expenditure of the School of Business in UW-Madison. The information is based on a database I create that collects and digitizes information from University Budget Reports or "Red Books" that report the amount of expenditure in each category by schools The Red Books were provided by the UW system Administration. These books report 4 broad categories of expenditures: instruction, research, academic support, student services and miscellaneous category.

Figure 9: Course Enrollment per instructors



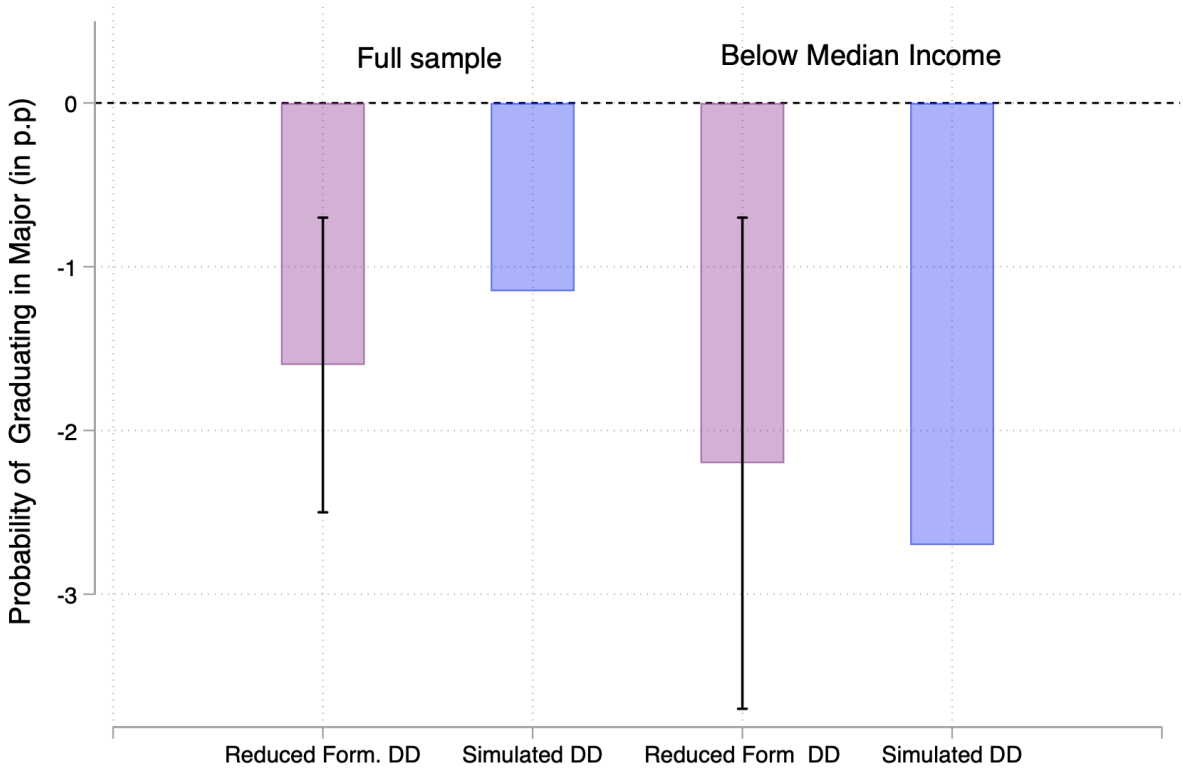
Notes: This figure reports the ratio of undergraduate course enrollment to number of instructors by year for all courses provided by the School of Business and the School of Engineering. The sample includes all undergraduate students enrolled in UW-Madison between 1999 and 2015.

Figure 10: Model Fit



Notes: This figure reports the goodness of fit for the targeted moments in the structural model estimation. The model is estimated using simulated method of moments approach. The vertical axis reports the moments generated by the model and the horizontal axis represents the data moments. The moments targeted include overall shares in each major, the share of below-median income students in each major. Additional I include the probability of being below-median overall score test score, below-median ACT math test score, below-median ACT reading test score, being female conditional on being in each major. Finally, I target borrowing amounts of below-median income and above-median income students.

Figure 11: Model Validation: Replicating Reduced form Price Change



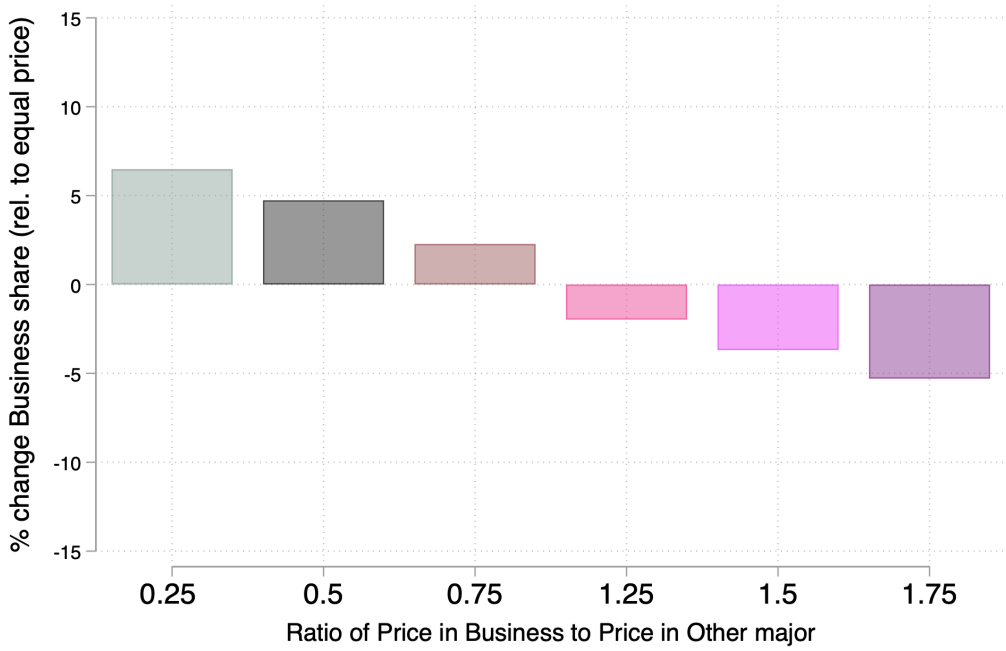
Notes: This figure reports the coefficients of interaction between resident and post dummies from the difference-in-difference estimator using simulated choices in the model as well as the reduced form difference in difference using the true choices from the data. I use the following specification:

$$Y_{ic} = \gamma_1 + \gamma_2 \text{Resident}_{ic} + \boldsymbol{\gamma}_3 \text{Post}_{ic} + \gamma_4 \text{Resident}_{ic} \times \text{Post}_{ic} + X'_{ic}\gamma + \epsilon_{ic}$$

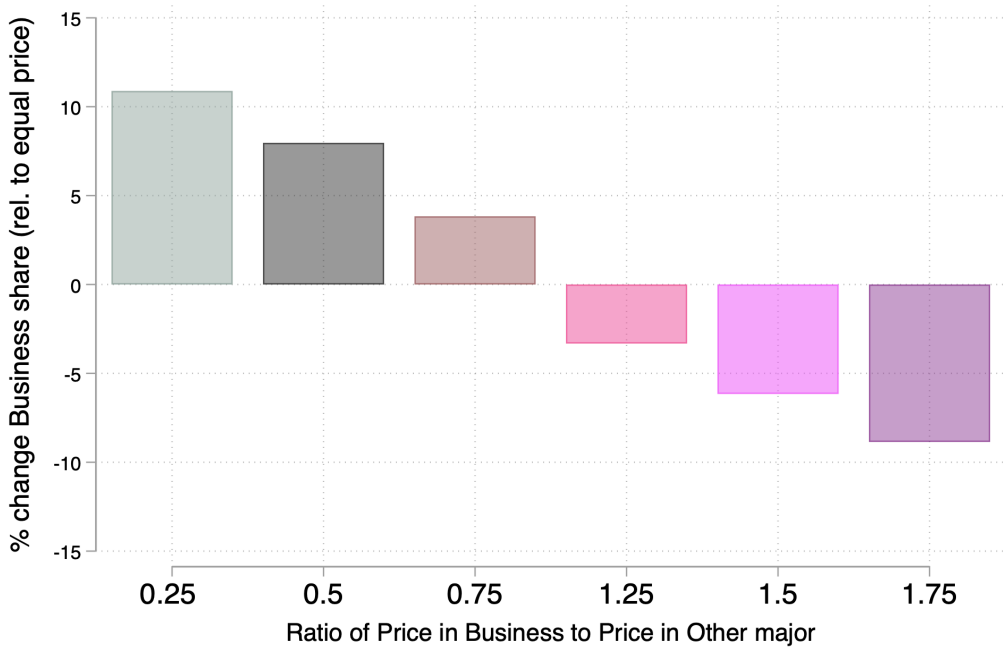
Y_{ic} is equal to one if individual i of admit cohort c graduated in the major and 0 if the individual graduated in any other major. R_{ic} is a dummy for residents and Post_{ic} is a dummy for students who were admitted *after* policy was enacted, X_{ic} includes controls for individual specific characteristics such as gender, race, and ACT math score and verbal scores. The figure reports the 95 percent confidence intervals for the reduced form estimates. Additionally, the figure reports these coefficients estimated for the full sample as well as for the sample of students below median income.

Figure 12: Counterfactual Proportional Pricing

Panel A: *Change* in Share of Students in Business

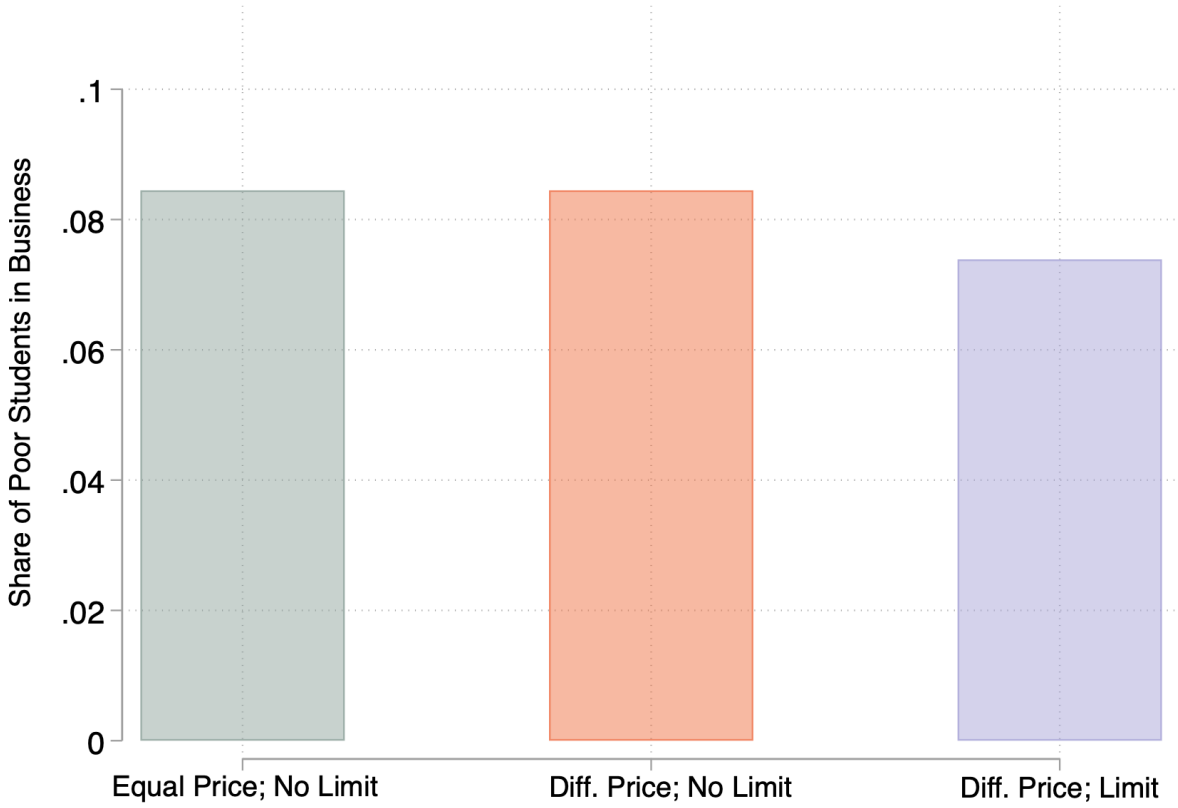


Panel B: *Change* in Share of Low Income students in Business



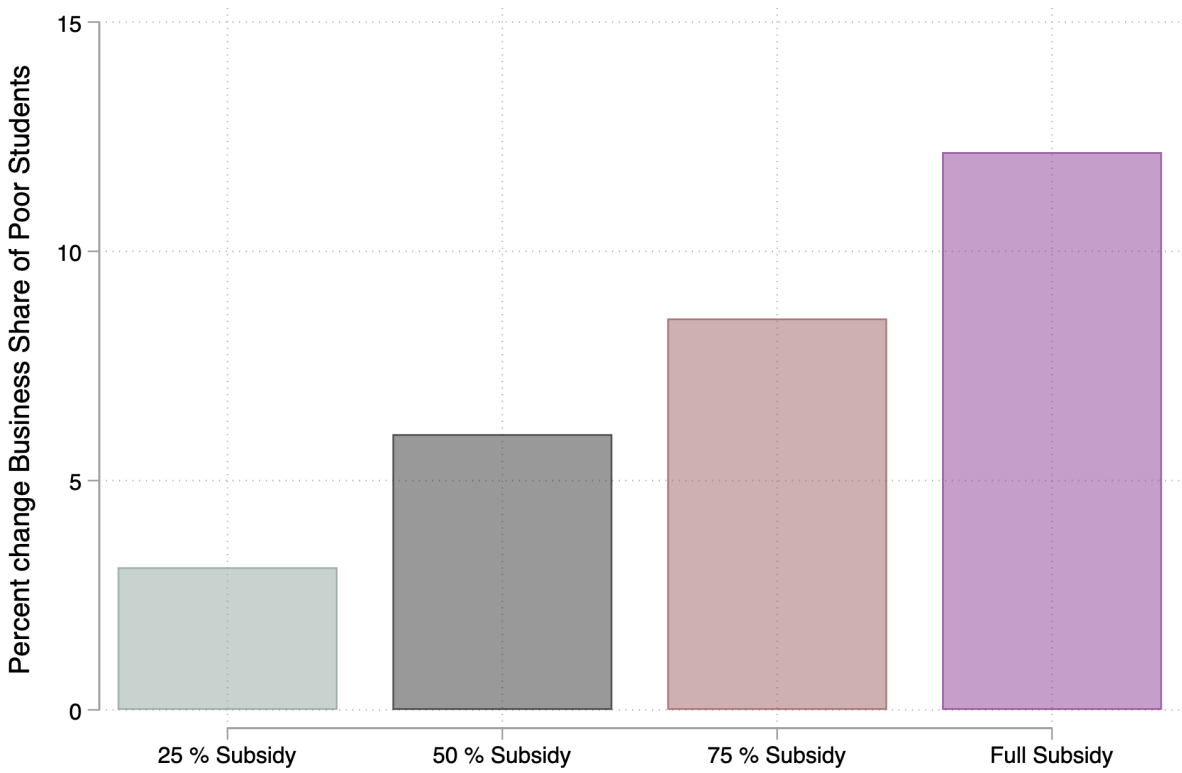
Notes: This figure reports the results of the counterfactual exercise of changing the surcharge type and amount simulated by the model estimates. The counterfactual policy shown here is a proportional pricing policy where the surcharge in Business is a proportion of the base tuition paid. The horizontal axis shows the ratio of the Price in Business relative to the Price in the Other Major. For example, 0.25 means that there is a proportional subsidy for studying Business i.e. $\frac{P_{bus}}{P_{notbus}} = 0.25$ whereas 1.25 means that there is a surcharge for studying Business $\frac{P_{bus}}{P_{notbus}} = 1.25$. The vertical axis shows the change in share of students in Business relative to the baseline of equal pricing $\frac{P_{bus}}{P_{notbus}} = 1$ across majors for each amount of the surcharge. Panel A reports these percent changes for the full sample and Panel B reports these percent changes for the sample of students below median income (low-income students).

Figure 13: Counterfactual Policy: Changing borrowing limits



Notes: This figure reports the results of the counterfactual exercise of changing combinations of pricing and borrowing policies. The figure reports the share of low income students (poor students) in the high price major, Business in three counterfactual scenarios. Counterfactual # 1 is the equal price; no limit counterfactual when there is equal pricing across all majors and no borrowing limits. Counterfactual # 2 is the differential price but no limit counterfactual when there is a higher price in Business but no borrowing limits. Counterfactual # 3 is the price and limit counterfactual is when there is differential pricing with a higher price in Business as well as borrowing limits. The responses are generated by simulating choices of students under each of these schemes using the estimates of the preference parameters from the model.

Figure 14: Counterfactual: Means-Tested Aid Specific to Major



Notes: This figure reports the results of the counterfactual exercise where students are provided means-tested aid in addition to the differential prices across majors. The means tested subsidy is provided to all students below the median income. The pricing policy shown here is a proportional pricing policy where the surcharge in Business is a proportion of the base tuition paid. The horizontal axis shows the percent subsidy in Business to below median income students. The vertical axis shows the percent change in share of students in Business relative to the baseline of differential prices across majors with no subsidy. The responses are generated by simulating choices of students under each of these schemes using the estimates of the preference parameters from the model.

Table 1: UW-Madison compared to other Big Ten Universities

		SAT Math		SAT Critical Reading		Tuition	
	Number Students	25th	75th	25th	75th	Instate	Out of State
Northwestern U.	8158	679.3	767.9	661.4	744.30	34.47	34.47
Indiana U.	29220	520.7	637.9	504.3	616.4	6.66	21.66
U. Iowa	18371	552.3	678.1	502.3	641.5	5.04	18.20
U. Maryland	23603	605	707.9	574.3	677.9	6.10	19.60
U. Michigan	24926	635.9	739.4	591.0	694.4	9.99	30.46
Michigan State	32439	529.6	659.3	468.6	610.0	8.94	22.80
U. Minnesota	26876	594.5	715.5	539.1	678.2	8.08	16.27
U. Nebraska	25723	527.1	667.1	506.4	652.1	4.81	14.28
Ohio Stae U.	41192	576.4	680	534.3	640.7	7.21	19.34
Penn State U.	60778	560.7	667.1	529.3	628.6	11.65	22.13
U. Wisconsin	27307	625	724	549.0	667	6.26	20.30
Purdue U.	29647	538.6	667.1	500.7	612.1	6.75	21.06

Note: This table reports the mean characteristics for Big Ten Research Universities such as average tuition (in $ 10,000) and mean SAT math and critical reading scores. These calculations are based on data from the IPEDS database.

Table 2: Regression of Major Choice on Family Income

	Dependent Variable: Dummy for Graduating in Major						
	College	Arts	Science	Social Science	Engineering	Business	Education
Real Family Income	0.015***	-0.001	-0.003**	-0.001	-0.001	0.008***	-0.002*
(in 10k)	(0.001)	(0.002)	(0.002)	(0.002)	(0.001)	(0.002)	(0.001)
ASVAB	0.094***	-0.031**	0.028**	-0.032**	0.0399***	0.004	-0.009
Arithmetic Ability	(0.013)	(0.013)	(0.013)	(0.015)	(0.009)	(0.017)	(0.012)
ASVAB	0.011	0.068***	0.003	0.047***	-0.024**	-0.064***	-0.030**
Writing Ability	(0.013)	(0.013)	(0.013)	(0.015)	(0.009)	(0.017)	(0.012)
Observations	3,297	1,315	1,315	1,315	1,315	1,315	1,315
R-squared	0.154	0.042	0.025	0.032	0.058	0.035	0.039

Notes: This table reports the coefficients for the regression of major choice on family income and asvab scores. The sample is students surveyed in National Longitudinal Survey of Youth 97 (NLSY 97). The dependent variable is a dummy for the category of major. I control for dummies of race and gender. Real Family Income is the household income averaged over the four years prior to going to college measured in 2000$. ASVAB scores are standardized and adjusted for age at the time of the test. College is a dummy for whether the student attended four year college and for the other categories, the sample is restricted to students who went to college. Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 3: Tuition Schedule for 15 credits in 2018 by Semester

Undergraduate	Base Tuition
Resident	5277
Non Resident (Domestic)	18402
Non Resident Tuition	18902
Minnesota	7170

Notes: This table reports the undergraduate base tuition schedule by residency for a credit load of 15 credits in academic year of 2018-2019.

Table 4: Tuition by Demographics

	Mean	SD
Female	53.77	28.81
Male	56.12	30.32
Black	65.16	35.64
White	50.7	26.39
Hispanic	66.62	34.24
Other	58.62	30.60
Resident	36.72	8.04
Non- resident	98.51	17.15
Minnesota Reciprocity	44.77	10.02
No. of observations	76,736	

Note:Standard deviation are enclosed in parentheses. This table reports the total undergraduate tuition paid during their college by$1,000. The tuition is calculated by adding the total number of credits in each semester enrolled and residency status of the students. This is reported by demographic characteristics, pooled over the time period 2000-2012.

Table 5: Field Specific Differential Per Year

Academic Year	Surcharge Amount per Year	
	Business	Engineering
Before 2007	0	0
2007-2008	1000	600
2008-2009	1000	1000
2009-2010	1000	1400
2010 Onwards	1000	1400

Note: This table reports the timing and amount of the major specific tuition amount (per year) introduced in UW-Madison in Business and Engineering Schools. The surcharge reported is the annual surcharge and is levied once the student has declared a major in either schools.

Table 6: Demographic Compostion

		Mean	SD
Panel A: Gender	Female	54.24 %	
	Male	45.76 %	
Panel B: Race	Black	2.40 %	
	White	84.7 %	
	Hispanic	3.50 %	
	Other	6.85 %	
Panel C: International Status	International	3.67 %	
Panel D: Residency	Resident	60.80 %	
	Non- resident	26.78 %	
	Minnesota	12.43 %	
Panel E: Test Score	ACT Score (Engl)	27.86	4.03
	ACT Score (Math)	28.02	3.75
Panel G: FAFSA Completed	FAFSA Completed	58.72 %	
	Pell Eligible	18.94 %	
	No. of observations	76,736	

Note: This table reports the demographic composition of the undergraduate student population for the cohorts that were admitted between 2000 and 2012. FAFSA is abbreviation for applying for Free Application for Federal Student Aid (FAFSA).

Table 7: Family Income by Residency

	Resident	Non Resident
ZIP code income (in $10k)	7.5	11.5
FAFSA completion rate (%)	61	39
Family Income (in $10k)	8.2	12

Note: This table reports the mean family income by residency. Family Income is reported in 2003 10,000 dollars.

Table 8: Sample by Residency and Policy Status

	Resident		Non Resident	
	Pre 2007	Post 2007	Pre 2007	Post 2007
Female	55.00	53.89	54.8	53.07
Male	45.00	46.11	44.80	46.93
Black	2.56	3.73	1.98	2.28
White	75.6	68.10	88.9	90.04
Hispanic	3.76	6.77	2.29	3.44
Other	7.25	8.11	5.49	7.67
ACT Score	66.52	62.93	57.73	58.33
(Verbal & Reading)	(111.29)	(85.91)	(52.27)	(42.59)
ACT Score (Math)	27.79	29.25	27.65	28.12
	(3.72)	(3.92)	(3.71)	(3.61)
Observations	10,009	10,264	29,957	26,506

Note:This table reports the mean characteristics for resident and non-resident populations before and after 2007. Standard deviation are enclosed in parentheses. The sample includes degree seeking undergraduate students in the University of Wisconsin-Madison admitted between 2000 and 2012.

Table 9: Regression of Family Income on College Major choice

	Dependent Variable: Dummy for Graduating in Major						
	Dropout	Business	Engineering	Education	Letters & Sc.	Human Ecology	Ag. & Life Sc.
ACT Quartile 2	-0.048***	0.029***	0.026***	-0.023***	0.020***	-0.038***	-0.014***
	(0.004)	(0.004)	(0.003)	(0.004)	(0.006)	(0.003)	(0.004)
ACT Quartile 3	-0.068***	0.033***	0.044***	-0.039***	0.029***	-0.059***	-0.009**
	(0.004)	(0.004)	(0.003)	(0.003)	(0.006)	(0.003)	(0.004)
ACT Quartile 4	-0.084***	0.024***	0.084***	-0.057***	0.045***	-0.078***	-0.019***
	(0.004)	(0.003)	(0.003)	(0.003)	(0.006)	(0.003)	(0.004)
Family Income Quartile 2	-0.035***	0.018***	0.014***	0.007**	-0.025***	-0.002	-0.006*
	(0.004)	(0.003)	(0.003)	(0.003)	(0.005)	(0.002)	(0.004)
Family Income Quartile 3	-0.060***	0.050***	0.013***	0.001	-0.052***	-0.001	-0.009*
	(0.004)	(0.004)	(0.004)	(0.004)	(0.007)	(0.003)	(0.005)
Family Income Quartile 4	-0.058***	0.047***	-0.008***	-0.001	-0.015***	0.004	-0.027***
	(0.004)	(0.003)	(0.003)	(0.003)	(0.005)	(0.002)	(0.004)
Mean of Dependent Variable	0.159	0.113	0.120	0.0697	0.520	0.0509	0.127
R Squared	0.020	0.017	0.079	0.027	0.011	0.034	0.005
Observations	35,473	35,473	35,473	35,473	35,473	35,473	35,473

Note: This table reports the coefficients for the quartiles of income for the regression of major dummy on quartiles of income and test scores ($Q_4 > Q_3 > Q_2$). These quartiles are defined as quartiles of the distribution of income and test scores for students enrolled UW-Madison. The specification I use is the following:

$$Y = \gamma_1 + \boldsymbol{\gamma}_2 Q_2^{inc} + \boldsymbol{\gamma}_3 Q_3^{inc} + \boldsymbol{\gamma}_4 Q_4^{inc} + \gamma_5 Q_2^{testscore} + \gamma_6 Q_3^{testscore} + \gamma_7 Q_4^{testscore} + \epsilon$$

The sample includes all domestic and dependent undergraduate students that filed for Free Application for Federal Student Aid (FAFSA). The income is defined as income in their first year application.Controls for gender, race dummies, residency and dummies for missing gender, race and test scores. Sample includes students who complete for Free Application for Federal Student Aid (FAFSA). Outcome variables are for the first year in college. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 10: Aid function

	Total Aid
Family Income Quartile 2	-0.411***
	(0.007)
Family Income Quartile 3	-0.516***
	(0.012)
Family Income Quartile 4	-
ACT score Quartile 2	-0.183***
	(0.011)
ACT score Quartile 3	-0.208***
	(0.011)
ACT score Quartile 4	-0.220***
	(0.010)
Observations	35,473

Note: This table reports the coefficients of the regression:

$$Y = \gamma_1 + \boldsymbol{\gamma}_2 Q_2^{inc} + \boldsymbol{\gamma}_3 Q_3^{inc} + \boldsymbol{\gamma}_4 Q_4^{inc} + \gamma_5 Q_2^{testscore} + \gamma_6 Q_3^{testscore} + \gamma_7 Q_4^{testscore} + \epsilon$$

where Y is the dependent variable is regressed on quartiles of income and ACT test scores ($Q_4 > Q_3 > Q_2$). These quartiles are defined as quartiles of the distribution of income and test scores for students enrolled UW-Madison. Sample includes students who complete Free Application for Federal Student Aid (FAFSA). Outcome variables are for the first aid year in college. Robust standard errors in parentheses. Total aid includes both gift aid and loans, gift aid includes both institutional and federal aid and loans includes both private and federal aid. All income measures are in 2003 dollars.

Table 11: Difference-in-Difference Estimates

Sample	Dependent Variable: Indicator for Graduating in Surcharged Major						
	Full Sample	Women	Men	White	Non White	Above Median ACT Score	Below Median ACT Score
	Panel A: Business Surcharge						
Resident X Part Exposure	-0.007	-0.008	-0.007	-0.004	-0.030*	-0.013	-0.003
	(0.00788)	(0.00933)	(0.0133)	(0.00868)	(0.0184)	(0.0134)	(0.00995)
Resident X Full Exposure	-0.0421***	-0.0421***	-0.0366***	-0.0491***	-0.0256	-0.0394***	-0.0316***
	(0.00700)	(0.00831)	(0.0118)	(0.00767)	(0.0168)	(0.0117)	(0.00900)
Observations	76,736	47,183	39,115	75,916	10,383	38,185	38,480
R-squared	0.026	0.022	0.015	0.026	0.023	0.016	0.025
Mean of Dependent Variable	0.123	0.0937	0.159	0.127	0.0934	0.156	0.0878
	Panel B: Engineering Surcharge						
Resident X Part Exposure	0.00900	-0.00106	0.0226*	0.00714	0.0195	0.00614	0.00773
	(0.00618)	(0.00542)	(0.0120)	(0.00679)	(0.0150)	(0.0118)	(0.00601)
Resident X Full Exposure	-0.0123**	-0.00478	-0.0183*	-0.0142**	-0.00698	-0.0276***	-0.00317
	(0.00556)	(0.00489)	(0.0108)	(0.00608)	(0.0139)	(0.0104)	(0.00557)
Observations	76,736	47,183	39,115	75,916	10,383	38,185	38,480
R-squared	0.087	0.028	0.059	0.090	0.068	0.068	0.042
Mean of Dependent Variable	0.0939	0.0355	0.164	0.0958	0.0797	0.156	0.0364

Note: This table reports the coefficients of the Difference in Difference estimates for the effect of the Business School Surcharge in Panel A and Engineering School Surcharge in Panel B. The reported coefficients are α_1 and α_2 for the following specification:

$$\begin{aligned} Y_{ic} &= \beta_0 + \beta_1 R_{ic} + \beta_2 \text{Partial Exposure}_{ic} + \beta_3 \text{FullTreat}_{ic} \\ &\quad + \alpha_1 \text{ Partial Exposure}_{ic} R_{ic} + \alpha_2 \text{FullTreat}_{ic} R_{ic} + \beta_4 X_{ic} + \varepsilon_{ic} \end{aligned}$$

where Y_{ic} is equal to one if individual i of admit cohort c graduated in the major and 0 if the individual graduated in any other major. R_{ic} is a dummy for residents (includes both Wisconsin residents and students from the tuition reciprocity state, Minnesota), Partial Exposure is a dummy for students who were already in school when the policy was enacted, Full Exposure is a dummy for students who were admitted *after* policy was enacted, X_{ic} includes additional covariates. These include dummies for female, Black, Hispanic and other race (white is the omitted category); high school GPA; age at entrance; ACT math score, verbal and writing scores; dummies for missing high school GPA, gender, race and test scores. The test score included in the specification is the most recent test score for students who take multiple attempts. ACT scores are imputed for students who report SAT Scores. All standard errors are clustered at the residency X admit cohort level. Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1
Source and Sample: The estimation is conducted on administrative data from UW-Madison. The sample is restricted to domestic students who were admitted between 2000 and 2012.

Table 12: Difference-in-difference-in-difference results

		Business Differential	Engineering Differential
Panel A: Diff-in-Diff	Part Exposure X Resident	-0.004	0.009
		(0.008)	(0.005)
	Full Exposure X Resident	-0.042***	-0.0123**
		(0.006)	(0.006)
	Observations	76,736	76,736
Panel B: Diff-in-Diff-in-Diff	Part Exposure X Resident	0.001	-0.001
	X zip code Income	(0.002)	(0.002)
	Full Exposure X Resident	0.006***	-0.002
	X zip code Income	(0.002)	(0.002)
	Observations	72,594	72,594

Note: This table compares the coefficients of the Difference in Difference estimates in Panel A with the Difference in Difference in Difference estimates in Panel B. In Panel A the reported coefficients are α_1 and α_2 for the following specification:

$$\begin{aligned} Y_{ic} &= \beta_0 + \beta_1 R_{ic} + \beta_2 \text{Partial Exposure}_{ic} + \beta_3 \text{FullTreat}_{ic} \\ &\quad + \alpha_1 \text{ Partial Exposure}_{ic} R_{ic} + \alpha_2 \text{FullTreat}_{ic} R_{ic} + \beta_4 X_{ic} + \varepsilon_{ic} \end{aligned}$$

where Y_{ic} is equal to one if individual i of admit cohort c graduated in the major and 0 if the individual graduated in any other major. R_{ic} is a dummy for residents (includes both Wisconsin residents and students from the tuition reciprocity state, Minnesota), Partial Exposure is a dummy for students who were already in school when the policy was enacted, Full Exposure is a dummy for students who were admitted *after* policy was enacted, X_{ic} includes additional covariates. These include dummies for female, Black, Hispanic and other race (white is the omitted category); high school GPA; age at entrance; ACT math score, verbal and writing scores; dummies for missing high school GPA, gender, race and test scores. The test score included in the specification is the most recent test score for students who take multiple attempts. ACT scores are imputed for students who report SAT Scores. In Panel B, the reported coefficients are α_7 and α_8 for the triple difference specification:

$$\begin{aligned} Y_{ic} &= \beta_0 + \beta_1 R_{ic} + \beta_2 \text{Partial Exposure}_{ic} + \beta_3 \text{FullTreat}_{ic} \\ &\quad + \alpha_1 \text{ Partial Exposure}_{ic} R_{ic} + \alpha_2 \text{FullTreat}_{ic} R_{ic} + \\ &\quad + \alpha_3 \text{ Partial Exposure}_{ic} Z_{ic} + \alpha_4 \text{FullTreat}_{ic} Z_{ic} \\ &\quad + \alpha_5 \text{ Z}_{ic} R_{ic} + \alpha_6 \text{Z}_{ic} R_{ic} \\ &\quad + \alpha_7 \text{ Z}_{ic} R_{ic} \text{ Partial Exposure}_{ic} + \alpha_8 \text{Z}_{ic} R_{ic} \text{ Partial Exposure}_{ic} + \beta_4 X_{ic} + \varepsilon_{ic} \end{aligned}$$

where $Z_i c$ is the zip code income. All standard errors are clustered at the residency X admit cohort level. Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1
Source and Sample: The estimation is conducted on administrative data from UW-Madison. The sample is restricted to domestic students who were admitted between 2000 and 2012.

Table 13: Effect of the Differential Tuition Policy using different Measures

		Below Median Income		
	Residency	Mean zip code Inc	FAFSA completers	FAFSA Residents
Panel A: Business Surcharge				
Treat X Post	-0.041***	-0.0153***	-0.013***	-0.012***
	(0.006)	(0.006)	(0.005)	(0.006)
Observations	76,736	72594	35,473	24,138
Mean of Dependent Variable	0.12	0.108	0.09	0.1
Panel B : Engineering Surcharge				
Treat X Post	-0.013***	-0.0110***	-0.006	-0.005
	(0.005)	(0.006)	(0.005)	(0.006)
Observations	76,736	72594	35,473	24,138
Mean of Dependent Variable	0.09	0.117	0.117	0.126

Note: Robust standard errors in parentheses. Column 1 reports the results for treatment group based on residency. Column 2 shows the results based on average zip code earnings (based on ACS 2006-2010 estimates). For columns 3 and 4, sample includes students who complete the for Free Application for Federal Student Aid (FAFSA) and the outcome variables for the same columns are for the first year aid year. This table reports the coefficients for the following specification:

$$Y_{ic} = \beta_0^{inc} + \beta_1^{inc}\text{Below Median}_{ic} + \beta_3^{inc}\mathbb{1}(t >= \tau) + \alpha^{inc}\mathbb{1}(t >= \tau)\text{Below Median}_{ic} + \beta_2^{inc}X_{ic} + \varepsilon_{ic}$$

where Below Median$_{ic}$ is one if individual i of cohort c had income that was below the median income in the sample. τ is year of the policy change. $\mathbb{1}(t >= \tau)$ is equal to one if cohort c was admitted to the university after the calendar year of the policy change τ. Y_{ic} is the outcome of interest, which in this case is the probability of graduating with a business degree. Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 14: Robustness of the effects of the Differential Tuition Policy

X_{inc}	Dependent Variable: Dummy for Graduating in High Price Major			
	Family Adjusted Gross Income	Expected Family Contribution	Net Price	Net Price (Only Gift Aid)
Panel A : Business Surcharge				
$\mathbb{1}\{X_{inc} < Median(X_{inc}\}$ X Post	-0.012***	-0.007	-0.012***	-0.021***
	(0.005)	(0.005)	(0.006)	(0.006)
Observations	35,473	35,473	35,473	35,473
Mean of Dependent Variable	0.104	0.104	0.09	0.09
Panel B : Engineering Surcharge				
$\mathbb{1}\{X_{inc} < Median(X_{inc}\}$ X Post	-0.006*	-0.003	0.007	-0.002
	(0.005)	(0.005)	(0.008)	(0.002)
Observations	35,473	35,473	35,473	35,473
Mean of Dependent Variable	0.117	0.117	0.12	0.12

Note: This table reports the coefficients for the following specification.

$$Y_{ic} = \beta_0^{inc} + \beta_1^{inc}\text{Below Median}_{ic} + \beta_3^{inc}\mathbb{1}(t >= \tau) + \alpha^{inc}\mathbb{1}(t >= \tau*)\text{Below Median}_{ic} + \beta_2^{inc}X_{ic} + \varepsilon_{ic}$$

where Below Median$_{ic}$ is one if individual i of cohort c had income that was below the median income in the sample. The year of the policy change is given by τ^* and the symbol $\mathbb{1}(t >= \tau*)$ is a dummy that equal to one if cohort c was admitted to the university after the calendar year of the policy change $\tau*$ or the Post cohorts. Y_{ic} is the outcome of interest, which in this case is the probability of graduating with the surcharged major. The coefficient α^{inc} here therefore can be interpreted as the effect of the levy of the tuition differential on poor versus non-poor students. Controls for gender, race, and ACT score. Dummies for missing gender, race and test scores. Post is an indicator for cohorts admitted after the policy was introduced. Sample includes students who complete for Free Application for Federal Student Aid (FAFSA). Outcome variables are for the first year in college. Robust standard errors in parentheses. Real Net Price is calculated as estimated tuition (Based on residency and aid year) net of EFC and total aid. Real Net Price (only gift aid) is calculated as estimated tuition net of EFC and total *gift Aid*. All income measures are in 2003 dollars.

Table 15: Where do they go instead

	Dependent Variable: Dummy for Graduating Major					
	Agriculture and Life Sc.	Engineering	Letters and Science	Business	Education	Human Ecology
	Panel A: Business Surcharge					
Resident X Part Exposure	-0.00343	0.000425	0.00470	-0.00805	0.00549	0.000874
	(0.00559)	(0.00536)	(0.0109)	(0.00710)	(0.00544)	(0.00476)
Resident X Full Exposure	-0.00461	-0.0114**	0.0659***	-0.0409***	-0.00547	-0.00347
	(0.00532)	(0.00510)	(0.00982)	(0.00651)	(0.00495)	(0.00417)
Observations	76,736	76,736	76,736	76,736	76,736	76,736
R-squared	0.013	0.087	0.065	0.026	0.027	0.030
Mean of Dependent Variable	0.0997	0.0939	0.582	0.123	0.0622	0.0394
	Panel B: Engineering Surcharge					
Resident X Part Exposure	-0.00190	0.00887*	0.00122	-0.0103	0.00298	-0.000828
	(0.00514)	(0.00489)	(0.00990)	(0.00653)	(0.00481)	(0.00441)
Resident X Full Exposure	-0.00712	-0.0109**	0.0693***	-0.0405***	-0.00904**	-0.00167
	(0.00512)	(0.00486)	(0.00918)	(0.00619)	(0.00448)	(0.00394)
Observations	76,736	76,736	76,736	76,736	76,736	76,736
R-squared	0.013	0.087	0.066	0.026	0.028	0.030
Mean of Dependent Variable	0.0997	0.0939	0.582	0.123	0.0622	0.0394

Note: This table reports the coefficients of the Difference in Difference estimates for the effect of the Business School Surcharge in Panel A and Engineering School Surcharge in Panel B. The reported coefficients are α_1 and α_2 for the following specification:

$$\begin{aligned} Y_{ic} &= \beta_0 + \beta_1 R_{ic} + \beta_2 \text{Partial Exposure}_{ic} + \beta_3 \text{FullTreat}_{ic} \\ &\quad + \alpha_1 \text{ Partial Exposure}_{ic} R_{ic} + \alpha_2 \text{FullTreat}_{ic} R_{ic} + \beta_4 X_{ic} + \varepsilon_{ic} \end{aligned}$$

where Y_{ic} is equal to one if individual i of admit cohort c graduated in the major and 0 if the individual graduated in any other major. R_{ic} is a dummy for residents (includes both Wisconsin residents and students from the tuition reciprocity state, Minnesota), Partial Exposure is a dummy for students who were already in school when the policy was enacted, Full Exposure is a dummy for students who were admitted *after* policy was enacted, X_{ic} includes additional covariates. These include dummies for female, Black, Hispanic and other race (white is the omitted category); high school GPA; age at entrance; ACT math score, verbal and writing scores; dummies for missing high school GPA, gender, race and test scores. The test score included in the specification is the most recent test score for students who take multiple attempts. ACT scores are imputed for students who report SAT Scores. All standard errors are clustered at the residency X admit cohort level.
Source and Sample: The estimation is conducted on administrative data from UW-Madison. The sample is restricted to domestic students who were admitted between 2000 and 2012. Standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

Table 16: Course requirements for Application to School of Business

Course	Description	Number of Credits
	Math	
Math 211	Calculus	5
Math 217	Calculus with Algebra and Trigonometry II	5
Math 221	Calculus and Analytic Geometry	5
	Psychology	
Psych 201	Introduction to Psychology	4
Psych 202	Introduction to Psychology	3
Psych 281	Introduction to Psychology	4
	Economics	
Econ 101	Microeconomics	4
Econ 111	Principles of Economics	4
	English	
English 100	Introduction to College Composition	3
Comm Arts 100	Introduction to Speech Composition	3
English 118	ESL: Academic Writing II	3

Note: This table reports course requirements for eligibility of application to the school of Business. The table reports the course number, the description as well as the number of credits required in each course.

Table 17: Effects on Intention to Major in Business

	All	All	Women	Women	Men	Men	White	White	Non White	Non White	Above Med. Test Score	Above Med. Test Score	Below Med. Test Score	Below Med. Test Score
	Intend	Final	Intend	Final	Intend	Final	Intend	Final	Intend	Final	Intend	Final	Intend	Final
PartExposure	-0.007	-0.011**	-0.008	-0.002	-0.007	-0.019**	-0.006	-0.011*	-0.009	-0.014	-0.008	-0.014	-0.000	-0.005
X Resident	(0.005)	(0.005)	(0.006)	(0.006)	(0.008)	(0.008)	(0.006)	(0.006)	(0.012)	(0.012)	(0.007)	(0.008)	(0.008)	(0.007)
FullExposure	-0.028***	-0.038***	-0.044***	-0.045***	-0.010	-0.027***	-0.025***	-0.037***	-0.028**	-0.039***	-0.029***	-0.034***	-0.013*	-0.016**
X Resident	(0.005)	(0.005)	(0.006)	(0.007)	(0.007)	(0.007)	(0.005)	(0.005)	(0.011)	(0.011)	(0.006)	(0.007)	(0.007)	(0.006)
Observations	100,900	100,900	53,538	53,538	47347	47347	83,107	83,107	17,793	17,793	48,274	48,274	43,636	43,636
R- squared	0.034	0.011	0.026	0.008	0.036	0.007	0.035	0.012	0.039	0.034	0.059	0.017	0.038	0.016
Mean	0.068	0.074	0.054	0.062	0.085	0.090	0.064	0.073	0.093	0.084	0.062	0.088	0.0712	0.0569

Note: The specification is the following that is similar to the earlier specifications. For the outcome Y_{ic} for individual i of admit cohort c:

$$\begin{aligned} Y_{ic} &= \beta_0 + \beta_1 R_{ic} + \beta_2 \text{Partial Exposure}_{ic} + \beta_3 \text{Full Exposure}_{ic} \\ &\quad + \alpha_1 \text{Partial Exposure}_{ic} R_{ic} + \alpha_2 \text{Full Exposure}_{ic} R_{ic} + X'_{ic}\gamma + \varepsilon_{ic} \end{aligned}$$

Y_{ic} is equal to one if individual i of admit cohort c graduated in the major and 0 if the individual graduated in any other major. R_{ic} is a dummy for residents and Partial Exposure is a dummy for students who were already in school when the policy was enacted, Full Exposure is a dummy for students who were admitted *after* policy was enacted, X_{ic} includes controls for individual specific characteristics such as gender, race, high school GPA, age at entrance, ACT math score and verbal score as well as dummies for missing high school GPA, gender, race, and test scores. Similar to earlier, all standard errors are clustered at the residency-admit cohort level. *** p<0.01, ** p<0.05, * p<0.1

Table 18: Effects on Overall Retention

	Overall Retention			
	First Year Retention	Second Year Retention	Third Year Retention	Fourth Year Retention
	Panel A: Engineering Surcharge			
Resident X Part Exposure	-0.0152***	-0.0183***	-0.0282***	-0.0452***
	(0.00415)	(0.00630)	(0.00755)	(0.00858)
Resident X Full Exposure	-0.0269***	-0.0438***	-0.0408***	-0.0416***
	(0.00356)	(0.00543)	(0.00662)	(0.00761)
Observations	112,709	112,709	112,709	112,709
R-squared	0.041	0.052	0.101	0.110
Mean of Dependent Variable	0.964	0.897	0.798	0.659
	Panel B: Business Surcharge			
Resident X Part Exposure	-0.0192***	-0.0262***	-0.0330***	-0.0481***
	(0.00467)	(0.00699)	(0.00832)	(0.00937)
Resident X Full Exposure	-0.0276***	-0.0464***	-0.0440***	-0.0514***
	(0.00406)	(0.00606)	(0.00726)	(0.00822)
Observations	112,709	112,709	112,709	112,709
R-squared	0.041	0.051	0.101	0.110
Mean of Dependent Variable	0.964	0.897	0.798	0.659

Note: Robust standard errors in parentheses. This table reports the coefficients of the Difference in Difference estimates for the effect of the Business School Surcharge in Panel A and Engineering School Surcharge in Panel B. The reported coefficients are α_1 and α_2 for the following specification:

$$\begin{aligned} Y_{ic} &= \beta_0 + \beta_1 R_{ic} + \beta_2 \text{Partial Exposure}_{ic} + \beta_3 \text{FullTreat}_{ic} \\ &\quad + \alpha_1 \text{ Partial Exposure}_{ic} R_{ic} + \alpha_2 \text{FullTreat}_{ic} R_{ic} + \beta_4 X_{ic} + \varepsilon_{ic} \end{aligned}$$

where Y_{ic} is the dependent variable for individual i of admit cohort c. R_{ic} is a dummy for residents (includes both Wisconsin residents and students from the tuition reciprocity state, Minnesota), is a dummy for students who were already in school when the policy was enacted, Full Exposure is a dummy for students who were admitted *after* policy was enacted, X_{ic} includes additional covariates. These include dummies for female, Black, Hispanic and other race (white is the omitted category); high school GPA; age at entrance; ACT math score, verbal and writing scores; dummies for missing high school GPA, gender, race and test scores. The test score included in the specification is the most recent test score for students who take multiple attempts. ACT scores are imputed for students who report SAT Scores. All standard errors are clustered at the residency X admit cohort level.
Source and Sample: The estimation is conducted on administrative data from UW-Madison. The sample is restricted to domestic students who were admitted between 2000 and 2012. Standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

Table 19: Test Score Composition

	Dependent Variable				
	ACT Math Score	ACT Verbal + Writing Score	Below 25th Percentile	Below Median	Below 75th Percentile
			Panel A: Business Surcharge		
Resident X Part Exposure	-0.0300	-1.168**		0.0430**	-0.0339
	(0.216)	(0.554)	(0.0209)	(0.0209)	(0.0307)
Resident X Full Exposure	-0.613***	-0.0384	-0.00960	0.0345*	0.00508
	(0.190)	(0.489)	(0.0185)	(0.0186)	(0.0262)
Observations	11,267	11,267	11,334	11,334	11,334
R-squared	0.095	0.098	0.049	0.009	0.003
Mean of Dependent Variable	29.27	55.09	0.111	0.110	0.220
			Panel B: Engineering Surcharge		
Resident X Part Exposure	-0.134	-1.286*	-0.00905	0.0552***	-0.0123
	(0.246)	(0.766)	(0.0176)	(0.0194)	(0.0367)
Resident X Full Exposure	-0.893***	-1.048*	-0.00622	0.0294*	0.0399
	(0.200)	(0.614)	(0.0142)	(0.0163)	(0.0291)
Observations	8,632	8,632	8,678	8,678	8,678
R-squared	0.062	0.117	0.028	0.008	0.003
Mean of Dependent Variable	30.55	55.32	0.0451	0.0706	0.220

Note: This table reports the coefficients of the Difference in Difference estimates for the effect of the Business School Surcharge in Panel A and Engineering School Surcharge in Panel B. The reported coefficients are α_1 and α_2 for the following specification:

$$\begin{aligned} Y_{ic} &= \beta_0 + \beta_1 R_{ic} + \beta_2 \text{Partial Exposure}_{ic} + \beta_3 \text{FullTreat}_{ic} \\ &\quad + \alpha_1 \text{ Partial Exposure}_{ic} R_{ic} + \alpha_2 \text{FullTreat}_{ic} R_{ic} + \beta_4 X_{ic} + \varepsilon_{ic} \end{aligned}$$

where Y_{ic} is the dependent variable for individual i of admit cohort c. R_{ic} is a dummy for residents (includes both Wisconsin residents and students from the tuition reciprocity state, Minnesota), Partial Exposure is a dummy for students who were already in school when the policy was enacted, Full Exposure is a dummy for students who were admitted *after* policy was enacted, X_{ic} includes additional covariates. These include dummies for female, Black, Hispanic and other race (white is the omitted category); high school GPA; age at entrance; dummies for missing high school GPA, gender and race. The test score included in the specification is the most recent test score for students who take multiple attempts. ACT scores are imputed for students who report SAT Scores. All standard errors are clustered at the residency X admit cohort level. Source and Sample: The estimation is conducted on administrative data from UW-Madison. The sample is restricted to domestic students who were admitted between 2000 and 2012 conditional on choosing the surcharged major. Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 20: Academic Performance conditional on Graduating in Surcharged Major

	Dependent Variable					
	Cumulative GPA	Cumulative Credits	Time to Degree	Graduate 4 years or less	Graduate 5 years or less	Graduate 6 years or less
	Panel A: Business Surcharge					
Resident X Part Exposure	-0.00291	0.403	-0.0531	0.0156	-0.00201	0.0104***
	(0.0183)	(0.997)	(0.0324)	(0.0315)	(0.00812)	(0.00362)
Resident X Full Exposure	-0.0193	-1.307	-0.162***	0.153***	0.00566	0.00407**
	(0.0156)	(0.825)	(0.0257)	(0.0267)	(0.00689)	(0.00189)
Observations	11,334	11,334	11,334	11,334	11,334	11,334
R-squared	0.048	0.021	0.066	0.072	0.005	0.004
Mean of Dependent Variable	3.461	139.3	4.180	0.722	0.990	0.999
	Panel B: Engineering Surcharge					
Resident X Part Exposure	0.0479	1.179	-0.0510	0.0448	0.0205	-0.000771
	(0.0342)	(1.314)	(0.0542)	(0.0413)	(0.0234)	(0.00986)
Resident X Full Exposure	-0.0280	2.552**	-0.0145	-0.000826	0.0161	-0.00248
	(0.0276)	(1.084)	(0.0429)	(0.0332)	(0.0181)	(0.00822)
Observations	8,678	8,675	8,678	8,678	8,678	8,678
R-squared	0.042	0.065	0.048	0.041	0.018	0.004
Mean of Dependent Variable	3.320	150.2	4.617	0.299	0.932	0.992

Note: This table reports the coefficients of the Difference in Difference estimates for the effect of the Business School Surcharge in Panel A and Engineering School Surcharge in Panel B. The reported coefficients are α_1 and α_2 for the following specification:

$$\begin{aligned} Y_{ic} &= \beta_0 + \beta_1 R_{ic} + \beta_2 \text{Partial Exposure}_{ic} + \beta_3 \text{FullTreat}_{ic} \\ &\quad + \alpha_1 \text{ Partial Exposure}_{ic} R_{ic} + \alpha_2 \text{FullTreat}_{ic} R_{ic} + \beta_4 X_{ic} + \varepsilon_{ic} \end{aligned}$$

where Y_{ic} is the dependent variable for individual i of admit cohort c. R_{ic} is a dummy for residents (includes both Wisconsin residents and students from the tuition reciprocity state, Minnesota), Partial Exposure is a dummy for students who were already in school when the policy was enacted, Full Exposure is a dummy for students who were admitted *after* policy was enacted, X_{ic} includes additional covariates. These include dummies for female, Black, Hispanic and other race (white is the omitted category); high school GPA; age at entrance; ACT math score, verbal and writing scores; dummies for missing high school GPA, gender, race and test scores. The test score included in the specification is the most recent test score for students who take multiple attempts. ACT scores are imputed for students who report SAT Scores. All standard errors are clustered at the residency X admit cohort level. Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Source and Sample: The estimation is conducted on administrative data from UW-Madison. The sample is restricted to domestic students who were admitted between 2000 and 2012 conditional on choosing the surcharged major.

Table 21: Regression of Earnings on Observable Characteristics

	Dependent Variable: Initial Earnigns					
	I	II	III	IV	V	VI
High School GPA	1,736***	625.4**	526.3*			
	(334.5)	(283.2)	(286.8)			
ACT Math Score				941.1***	432.5***	431.0***
				(47.57)	(42.40)	(42.86)
ACT Verbal Score				-109.0***	-11.14	-12.22
				(22.07)	(19.32)	(19.85)
Female	-8,763***	-4,999***	-5,046***	-6,724***	-4,278***	-4,286***
	(307.7)	(273.2)	(274.0)	(314.7)	(279.1)	(280.8)
Age at Entrance	-422.8	-339.6	-329.9	120.5	-29.11	-30.18
	(387.7)	(327.4)	(327.3)	(380.8)	(328.1)	(328.2)
Hispanic	-2,238**	-1,406*	-1,282*	-351.6	-462.6	-453.6
	(907.7)	(766.0)	(767.9)	(888.8)	(765.4)	(766.4)
Black	-1,493	-441.2	-217.5	1,915*	1,394	1,408
	(1,163)	(981.0)	(986.2)	(1,154)	(993.2)	(995.1)
Other Race	1,744**	1,834***	1,859***	1,055	1,590***	1,593***
	(698.7)	(589.7)	(589.6)	(679.4)	(585.4)	(585.6)
Resident	-2,385***	-1,726***	-1,719***	-1,593***	-1,494***	-1,496***
	(328.8)	(278.3)	(278.3)	(311.5)	(268.9)	(269.1)
Agriculture School		1,381*	1,653**		1,220	1,252
		(769.8)	(779.8)		(763.5)	(775.3)
Business School		13,321***	13,352***		12,499***	12,507***
		(699.1)	(699.0)		(698.5)	(699.4)
Engineering School		16,918***	17,069***		15,771***	15,793***
		(733.6)	(736.7)		(735.3)	(741.3)
Human Ecology School		3,403***	3,590***		3,515***	3,534***
		(873.2)	(877.2)		(867.2)	(870.9)
Letters and Science		819.8	951.5		509.8	529.2
		(673.3)	(675.9)		(669.7)	(674.7)
Cumulative GPA			810.8**			92.14
			(375.1)			(388.2)
Observations	6,376	6,376	6,376	6,376	6,376	6,376
R-squared	0.123	0.377	0.377	0.171	0.386	0.386

Note : This table reports the coefficients for the regression of initial earnings on fixed individual characteristics. Columns I, II and III report the regression of initial earnings on high school GPA with other controls whereas Columns IV, V, VI report the regression of initial earnings on standardised SAT and ACT test scores. Columns I and IV include controls for gender, race dummies and Residency. Column II and V also include major fixed effects in addition to those controls. Columns III and VI include the cumulative GPA at graduation in addition to the major fixed effects as well as controls. Earnings are based on self reported survey measures from the Post Graduation Outcomes Survey collected by the University of Wisconsin at the time of graduation. Sample includes students who were admitted between 2003 and 2012. Earnings are reported in 2000 $ and are winsorized at the 5th and 95th percentile. Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 22: Earnings Returns: an IV approach

	I	II	III	IV	V
	OLS	Instrument Variable Estimate			
Sample		Full Sample	Below Median Math Test Score	Below Median Income	Below Median Test score and Income
		Panel A : First Stage Dependent Variable Business Major Indicator			
P_{bus}/P_{notbus}		-1.148***	-0.423***	-0.538***	-0.861***
		(0.114)	(0.118)	(0.131)	(0.134)
P_{bus}/P_{notbus} X ACT Math		0.0279***			
		(0.00245)			
Fstat of Excluded Instrument		76.06	12.80	16.74	41.30
		Panel B: Second Stage Dependent Variable: Initial Earnings			
Graduated in Business	13434.7***	20776.2**	34881.2***	27762.3***	16721.9**
	(323.7)	(8042.6)	(2953.3)	(7018.6)	(5669.6)
Observations		5364	2908	2659	1492

Note: This table reports the results of the Instrumental variable regression. Dependent variable in the second stage is the earnings in the first job after graduating (in 2003 dollars). Whereas the dependent variable in the first stage is a dummy for graduating in Business. Columns I and II report the OLS and the IV estimates for the full sample, Columns III, IV, and V report the IV estimate for the subsample of students below the median test score, below the median income and below the median test score and income respectively. Controls include gender, race and dummy for below median ACT test scores, a dummy for residency and a dummy for the post periods. The sample includes students who completed an earnings survey provided by the University of Wisconsin- Madison. Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 23: Cost Benefit Analysis

	I	II
	IV and Diff in Diff	OLS and Diff in Diff
Per Cohort Demand Decrease	286	286
Initial Earnings Loss	20776	13434
	PDV over 10 year horizon	
	Earnings Loss	
Per Student	279,003	184,623
Per Cohort (millions)	80	52
	Tax Revenue Decrease	
Per Student	17494	11576
Out of state Migration Rate For Business (%)	40.9	40.9
Per Cohort (millions)	3	2.4
	Revenue Gained	
Per Student	2,000	2,000
Per Cohort (millions)	2	2

Note : This table reports the cost benefit analysis of the policy change. Per cohort demand decrease is calculated using the difference in difference estimator. The per cohort demand decrease is calculated for the post policy change cohort of residents. Initial Earnings loss is the IV estimate of the initial earnings loss in column I and OLS estimate in column II. The present discounted value of earnings loss is calculated using a discount rate of 3 percent (Avery and Turner 2012) for a time horizon of ten years. The loss is the difference between Business and other majors (excluding Engineering). The major specific earnings growth is calculated using the American Community Survey. I consider the sample of full time full year employees and regress the log of earnings on age and age squared, separately for the Business and other majors to obtain growth rates. The tax revenue is calculated using the income tax schedule for Wisconsin. The total revenue is given by the size of the total cohort in Business times the revenue for each student (assuming they pay the surcharge for two years).

Table 24: Utility Estimates

Category	Description	Value
ϕ	Scaling Factor of CRRA utility	0.25
Major fixed effect	Business	-0.45
	Engineering	-0.47
	Other	0.54
(ACT Math /100) Interactions	Business	0.78
	Engineering	0.92
	Other	0.81
(ACT Verbal/100) Interactions	Business	0.81
	Engineering	0.89
	Other	0.91
Female Interaction	Business	-0.37
	Engineering	-0.93
	Other	0.53

Note: This table reports the parameter estimates of the structural model. The parameters are estimated using Simulated Method of Moments (SMM) using information from the administrative student records from UW-Madison combined with moments on financial aid from UW-Madison as well as survey information on earnings upon graduation. The sample includes all students who were admitted between 2000 and 2012 to UW-Madison.

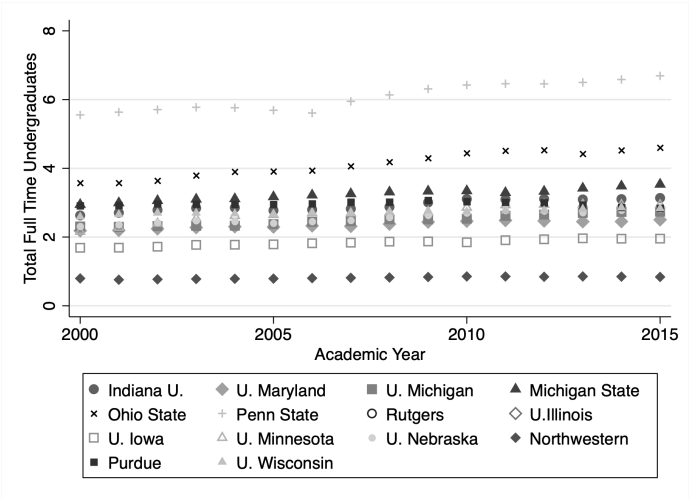
Table 25: Implied Elasticities

		All	Below Median Income	Above Median Income
Price	Mean	-0.352	-0.690	-0.006
	Median	-0.005	-0.013	-0.004
	P25	-0.023	-1.300	-0.008
	P75	-0.003	-0.004	-0.003
Income	Mean	0.185	0.374	-0.008
	Median	-0.005	0.001	-0.007
	P25	-0.009	-0.006	-0.012
	P75	0.209	0.737	-0.004

Note: This table reports the elasticities of major choice with respect to price and income generated by the model. All value are calculated by simulating the probability of graduating in Business (from the model) with respect to a 10 percent change in income or price. The table reports the Mean, Median, 25th and 75th Percentile of the distribution,

Appendix Tables and Figures

Figure A-1: UW-Madison compared to other Big Ten Universities



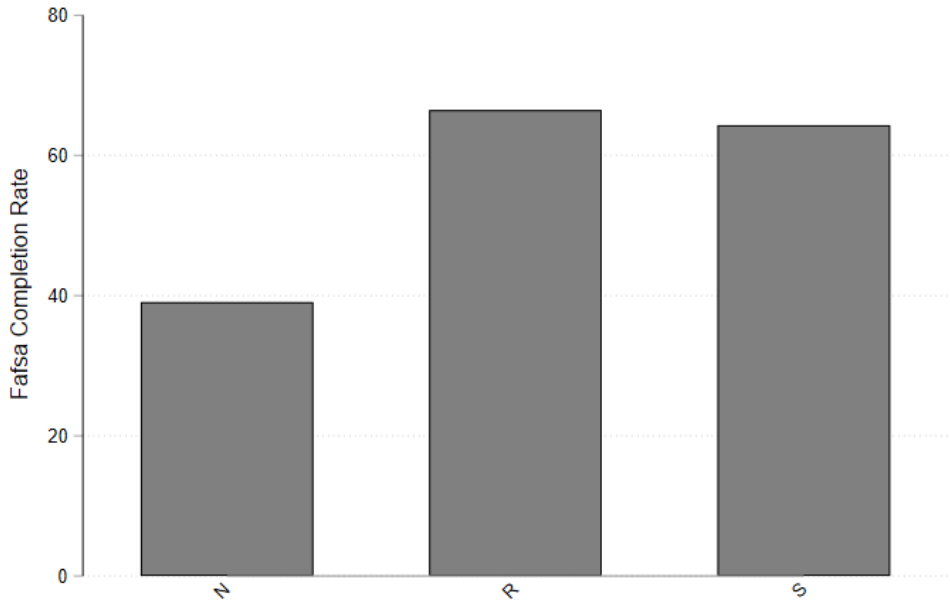
Note: This figure reports the trends in full time enrollment in UW-Madison as compared to other Big Ten universities. The data is based on information from the IPEDS.

Table A-1: History of Program Specific Differential in Big Ten Research Universities

Big Ten Research University	Year	Engineering	Business
Indiana University	2008		Yes
University of Maryland	Not Yet		
University of Michigan	1989		
Pennsylvania State University	2002	Yes	Yes
Rutgers University	1992		
University of Illinois	1994		
University of Iowa	2007	Yes	
University of Minnesota	Not Yet		
University of Nebraska	2004	Yes	
Purdue University	1999		
University of Wisconsin - Madison	2008	Yes	Yes

Note: This table reports the history of program specific differential tuition in Big Ten Research Universities for Business and Engineering majors. For more details on the background of differential tuition, refer to Wolniak et al. (2018) and Nelson (2008).

Figure A-2: Free application for Federal Student Aid Completion by residency



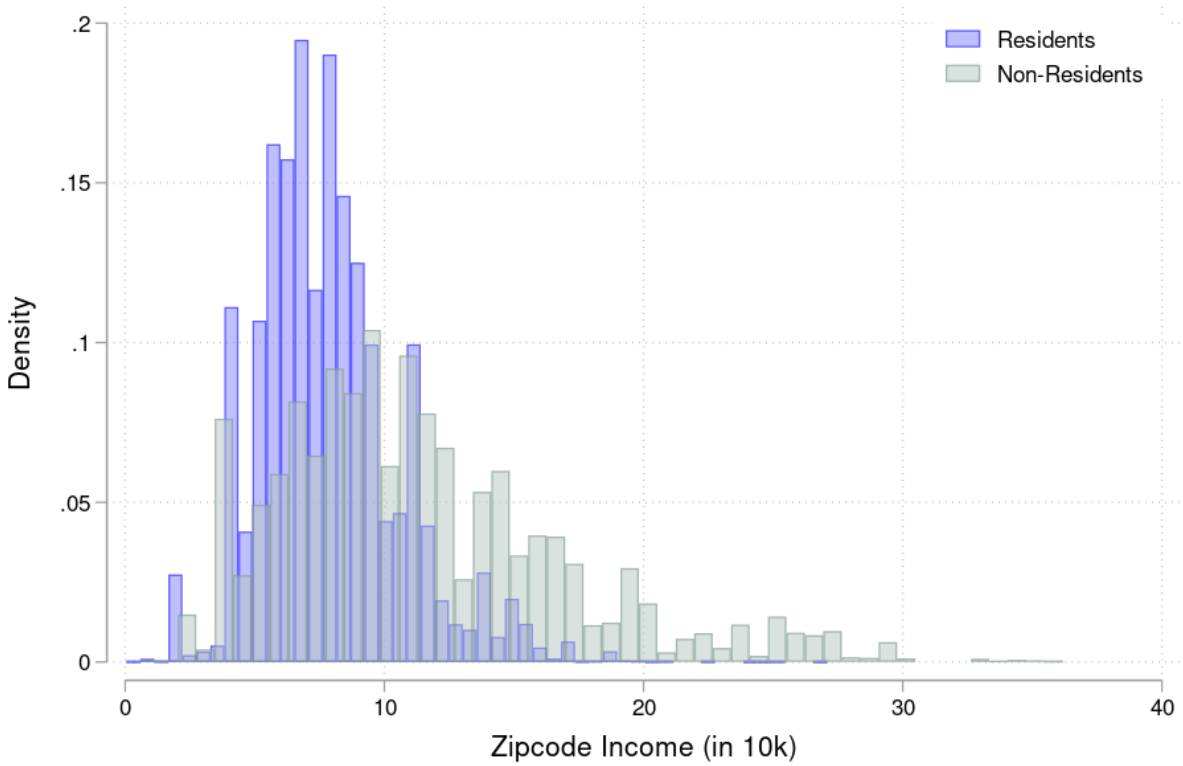
Note: This figure reports the application rates for the Free Application for Financial Student Aid (FAFSA) for domestic undergraduate students in cohorts admitted between the years 2000 and 2012. The figure breaks down the FAFSA application rate by Residency. 'N' stands for non-residents whereas 'R' stands for resident and 'S' stands for students from Minnesota.

Table A-2: Earnings from the Post Graduation Outcome Survey

	All	Non Residents	Residents	Diff.	Std. Error	Obs.
Employed in Wisconsin	0.456	0.229	0.517	-0.288***	0.016	5349
Government or Not for Profit	0.240	0.220	0.246	-0.026^{+}	0.013	6246
For Profit	0.698	0.720	0.692	0.028^{+}	0.014	6246
Self Employment	0.013	0.016	0.013	0.003	0.004	6246
Salary	33266.746	33849.236	33116.845	732.391^{+}	401.853	6376
Observations	6376					

Note : This table reports summary statistics for post graduation outcomes using data from the Post Graduation Outcomes Survey by residency. The columns represent the statistics for the full sample, non-residents, residents, the mean difference and the standard error of the difference as well as the number of observations.

Figure A-3: Mean income by residency



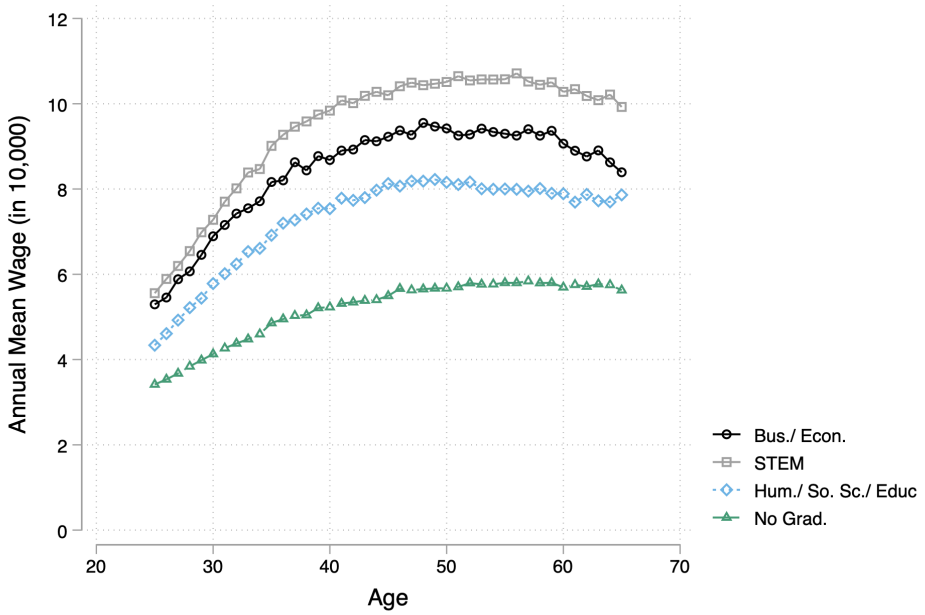
Note: This figure reports the distribution by residency of the mean ZIP code income for domestic undergraduate students in cohorts admitted between the years 2000 and 2012. The zip code level mean income is obtained from data released by the US Census Bureau in the 5 year ACS estimates for 2006-2010.

Table A-3: Earnings from the Post Graduation Outcome Survey

	All	ALS	BUS	EDU	EGR	HEC	L&S
Employed in Wisconsin	0.456	0.607	0.326	0.613	0.367	0.676	0.496
Government or Not for Profit	0.240	0.294	0.027	0.737	0.052	0.236	0.381
For Profit	0.698	0.618	0.931	0.211	0.919	0.678	0.541
Self Employment	0.013	0.029	0.007	0.017	0.002	0.017	0.017
Salary	33266.746	27792.495	40580.305	25175.978	44967.245	28550.949	27154.890

Note : This table reports summary statistics for post graduation outcomes using data from the Post Graduation Outcomes Survey by graduating major. The columns represent the statistics for the full sample, agriculture and life sciences major (ALS), Business (BUS), Education (EDU), Engineering (EGR) Human Ecology School (HEC) and Letters and Science (*L&S*).

Figure A-4: Age Earnings profiles by major



Note: This figure reports annualized full time earnings from age 22 to age 65 conditional on graduating in a particular major. The earnings are based on data from the American Community Survey for 2018. The figure reports the annual mean wage in $ 2018 dollars