The Effect of Differential Pricing on Undergraduate Degree Production by Field

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Abstract

In the face of declining state support, many public universities have introduced differential pricing by undergraduate level and/or program as an alternative to across-the-board tuition and fee increases. Differential tuition aligns prices more closely with instructional costs and students' ability to pay post-graduation. This paper provides the first estimates of the consequences of these policies on the allocation of students to majors. Results suggest a reduction in the share of degrees awarded in engineering after the introduction of differential pricing and an insignificant reduction in the share of degrees awarded in business. Differential pricing for nursing is not associated with a reduction in nursing degrees, which may reflect a greater supply response to differential pricing for particularly oversubscribed fields such as nursing. There is some evidence that student groups already underrepresented in these majors are particularly affected by the new pricing policies.

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I. Introduction

Many universities now charge undergraduate students more for upper division coursework and for certain high-cost majors such as engineering and business. For instance, at the University of Michigan in Ann Arbor, in-state upper-division engineering and computer science students pay $8,087 per term, 23% higher than students in most other undergraduate programs. This practice, referred to as “differential tuition,” is a way for universities and programs to generate additional revenue in the face of declining state support that is often viewed as equitable, given the higher instructional costs and higher earnings of graduates of these programs.

Differential pricing has been advocated by economists and documented by institutional researchers and educational scholars periodically for quite some time (Hoenack and Weiler, 1975, Yanikoski and Wilson, 1984, Siegfried and Round, 1997). Proponents point out that differential tuition eliminates the regressive cross-subsidies implicit in non-differentiated pricing, making price more reflective of resource utilization and ability to pay. Critics worry, however, that such practices may undermine efforts to get more students – particularly low income students, women, and minorities – to enter science and engineering and also discourage student exploration (Redden, 2007).

Historically, most American universities charged all undergraduate students the same price regardless of level or program. In fact, an absence of differential pricing was one of the “puzzles” identified in the seminal work by Rothschild and White, writing in 1993. Only recently has differential tuition begun to overturn this convention. A recent survey by Ehrenberg (2012) found that 42 percent of public doctoral institutions charged differentially either by field or level, with field-based differentials much more common. These practices are less common at non-doctoral institutions, but still prevalent (18% and 30% at master’s and bachelor’s institutions, respectively). The enactment of these practices has grown steadily since the mid-1990s with no sign of slowing down (Cornell Higher Education Research Institute, 2012). If this trend continues, differential pricing will soon be the new standard model for pricing in higher education. While the price response of overall enrollment and of college choice has been studied extensively, as have the determinants of major field choice, there is no empirical evidence on the consequences of differential pricing. This paper attempts to fill the gap in our understanding.

The effect of differential pricing is estimated using data on the mix of degrees awarded by 142 large public research universities from 1990 to 2010. Fifty of these 142 universities adopted differential pricing for engineering, business, or nursing during this time period. These three fields are the most common targets for differential pricing and also account for a sizable share of all undergraduate students. Employing a difference-in-differences and event-study strategy, I compare changes in the share of degrees awarded in certain fields at these universities to changes at schools that did not alter their tuition policy during the same time period. Several different plausible control groups – colleges that adopt differential pricing at different times, colleges that considered adopting (but didn't), non-adopters in the same region and selectivity category – are used to estimate the counterfactual time trend that adopters would have experienced had they not enacted price differentials. The event-study model finds no evidence that schools adopting differential pricing policies were trending differently than control schools prior to adoption.

The results indicate that differential pricing for engineering is associated with a statistically significant 1.1 percentage point decrease in the share of degrees awarded in engineering after three years (on a base of 14.7 percent). The analogous figure for business is an (imprecise) 0.8 percentage point decrease in the business share within three years (on a base of 19.5 percent). Differential pricing for nursing is actually associated with a 0.8 percentage point *increase* in the nursing share (on a base of 4.4 percent), though this is imprecise and not significantly different from zero. These patterns are robust across a number of specifications, covariate adjustments, different control groups, and samples. I also find that women and minorities have larger proportionate effects than male and white students. Using individual-level data from the National Postsecondary Student Aid Study, I find no evidence that additional institutional grant aid offsets the increased tuition for impacted majors.

Different responses across fields may reflect differences in demand parameters, that the supply response differs across fields, or that fields are in different initial equilibrium states since the effects I uncover combine both a demand and supply response. It is possible that additional revenue enables an expansion in the supply of nursing positions while engineering revenue is used to improve quality and attract better (though fewer) students.

This paper provides the first evidence on the consequence of a new model for pricing in higher education, which has grown significantly and is likely to become the norm in the near future. Graduate training has long differentiated price based on instructional cost and students' willingness (or ability) to pay, but this has become widespread in undergraduate education only recently. Given the great public policy interest in increasing the number of bachelor's degrees awarded in science and engineering (Executive Office of the President, 2012) and nursing (Institute of Medicine, 2010), an understanding of the role of pricing in these outcomes is critical. This analysis also contributes to the literature on student decision-making more generally. Students face a multitude of choices once they enter college – study intensity, major, employment, class selection – that are relatively understudied, but may have important long-term consequences. Furthermore, students' decisions may respond to financial incentives differently at various stages of the educational pipeline, though the timing of incentives has also received little attention. Understanding where financial incentives are strongest (or weakest) informs how they should best be targeted. For instance, if upper-division students are unresponsive to field-specific prices, then financial incentives targeting the major choice of these students may be ineffective.

This paper proceeds as follows. The next section provides a brief background on differential pricing. A theoretical framework for interpreting the empirical results is presented in Section III. Previous literature is discussed in Section IV. Section V describes the data used in the analysis and the empirical strategy. Results and robustness are discussed in Section VI. Section VII concludes.

II. Background: Rationale For and Prevalence of Differential Pricing

Proponents of differential pricing cite two primary rationales (Hoenack and Weiler, 1975, Siegfried and Round, 1997, Nelson 2008). First, differentials make the price the students experience align more closely actual instructional costs, eliminating the implicit cross-subsidy across major fields that results from the conventional practice of charging similar tuition fees to all undergraduate students regardless of the cost of instructing them. The cost of instruction differs tremendously between upper and lower division coursework and across programs even within institutions. For instance, recent analysis of cost data from four large state postsecondary systems (Florida, Illinois, New York-SUNY, and Ohio) indicated that upper division instruction costs approximately 40% more per credit hour than lower division instruction, and that upper-division engineering, physical science, and visual/performing art was approximately 40% more costly than the least costly majors (SHEEO, 2010). In fact, an earlier but more extensive cost study found that more than three-fourths of the variance in instructional cost across institutions is explained by the disciplinary mix within an institution (U.S. Department of Education 2003).

The consequence is that lower division students subsidize upper-division students and students in costly majors are subsidized by those in less expensive ones.

Second, tuition differentials better align prices with students' ability to pay post-graduation. Lower division includes many students who eventually drop out, while students that have advanced to upper division are more likely to graduate and earn more. Engineering, science, and business majors tend to earn more and have higher returns than education and humanities majors, even after controlling for differential selection of major by ability (Arcidiacono 2004). Higher earnings upon graduation mean that graduates with these degrees are thus in a better position to finance higher tuition fees with loans. Again, non-differentiated pricing implicitly creates cross-subsidization that runs counter to differences in post-schooling earnings and ability to pay. In addition to being regressive, this pattern of cross-subsidization is highly unusual; profit-maximizing firms in other markets are predicted to charge based on marginal cost and willingness to pay.

Some opponents of the changes worry that tuition differentials will adversely affect student choice, particularly for low-income students (Nelson 2008). A related concern is that differential tuition practices will make it even more difficult to increase participation in STEM fields and in health professions such as nursing, as some of these fields are often the target of tuition differentials. Others worry that differential tuition will discourage student exploration (Redden, 2007), resulting in worse matches between students and majors or occupations.

As documented by Yanikoski and Wilson (1984), a few large public universities, such as the University of Illinois and the University of Michigan, have charged more for upper division coursework and for high-cost majors for quite some time. However, many more universities have recently implemented explicit differential tuition and fee schedules by level and program as an alternative to across-the-board tuition and fee increases. In a broad survey of 165 public research universities, Nelson (2008) found that 45% of schools have at least one undergraduate program with differential tuition or fees in 2008, with most implementing them in the past decade. This share was up to 57% by 2011 (Reed 2011). Many more, such as the University of California System, have recently considered and rejected such a scheme (Gordon 2009, University of California Office of the President 2009) or have commissioned studies of pricing practices at other institutions as a possible first step to considering such schemes (University of Washington Office of Planning and Budgeting 2011). Differential pricing by level, independent of major program, is rarer, but still present at some institutions (Simone 2010, Ehrenberg, 2012). A recent survey found a continuation of this trend: 42% of all public doctoral institutions had some form of tuition differential in 2010-2011, as did many public masters and bachelors-level public institutions (18% and 30%, respectively) (Ehrenberg, 2012). The enactment of these practices has grown steadily since the mid-1990s with no sign of slowing down (Cornell Higher Education Research Institute, 2012).

III. Theoretical framework

The introduction of differential pricing by program could induce both a demand and supply response, so the combined effect on the sorting of students into majors is theoretically ambiguous. This section introduces a simple demand-supply framework to understand the reduced form effect uncovered in the empirical analysis. Throughout I assume that program-specific price at each institution, P, is set externally (e.g., by a Board of Trusties or legislature), so that individual departments and students act as price-takers and P does not necessarily equate supply and demand.

On the demand side, individuals weigh the long-term expected benefits of studying a particular program against the short-term costs of doing so, as is typical in the human capital framework (Becker 1964). Individuals choose the major for which the difference between expected benefit and cost is the greatest. Denote the net value that individual *i* receives from receiving a degree in major *k* at university *j* to be $V_{ijk} = \beta_1 Earnings_i(Q_{jk}) + \beta_2 P_{jk} + \beta_3 Effort_{ijk} + \delta_{jk} + \varepsilon_{ijk}$. The most salient benefit is the financial return, $Earnings_i(Q_{jk})$, which is a function of the quality of major *k* at university *j*, where quality includes such things as class size, faculty prestige, and classroom technology. Benefits also include the non-financial aspects of careers associated with each major (δ_{jk}). In most previous analysis of major choice, costs consist of the individual-specific non-financial effort costs ($Effort_{ijk}$) stemming from the difficulty of completing each major. For instance, large differences in effort cost and study requirements exist between majors (Babcock and Marks 2011, Stinebrickner and Stinebrickner 2008).[2] A differential tuition policy also creates financial cost differences by program, P_{jk}, which may also influence demand. Under common assumptions[3], the marginal effect of an increase in price on the share of students choosing major *k* at school *j* is:

$$\frac{dShare_{jk}}{dP_{jk}} = \left(\beta_2 + \beta_1 \frac{dEarning_{jk}}{dQ_{jk}} \frac{dQ_{jk}}{dPrice_{jk}}\right)(Share_{jk})(1 - Share_{jk}).$$

β_2 reflects the pure price effect, which is likely negative since higher prices should discourage students from entering impacted fields, holding all else constant. However, if students also value earnings ($\beta_1 > 0$), there are positive returns to major quality ($\frac{dEarning_{jk}}{dQ_{jk}} > 0$), and impacted

[2] Differences in required study time between majors could also be thought of as differences in the opportunity cost of time not available for work, given that many students combine work and schooling.

[3] If unobserved determinants of demand for major *k* (ε_{ijk}) are assumed to be i.i.d. with a type 1 extreme value distribution and individual heterogeneity of returns and effort cost is ignored, then major choice probabilities take the conditional logit form with the marginal effect given in (1).

programs use some of the additional revenue to improve quality ($\frac{dQ_{jk}}{dP_{jk}} > 0$), then demand for major k could actually increase when its price is raised. Thus the demand response could reflect both a price and quality response, which are likely to be opposite signed. The key mediator is how much colleges reinvest additional revenue to improve the quality of impacted majors.[4]

On the supply side, individual programs (e.g. the Engineering department or school) at universities choose program size (number of students) and program quality to maximize utility, subject to the constraint that the costs of students and quality must equal total program revenue. Let N_{jk} be the number of students and Q_{jk} be program quality, which cost $C_N(N_{jk})$ and $C_Q(Q_{jk})$, respectively. Programs receive a budget allocation Y_{jk}, which does not depend on tuition revenue, and a share θ of their students' tuition revenue. A program's budget constraint is thus $Y_{jk} + \theta P_{jk} N_{jk} = C_N(N_{jk}) + C_Q(Q_{jk})$. A maximizing department will choose size and quality to equate the marginal rate of substitution with the ratio of marginal net cost: $\frac{\frac{\partial U}{\partial Q}}{\frac{\partial U}{\partial N}} = \frac{\frac{\partial C_Q}{\partial Q}}{\frac{\partial C_N}{\partial N} - \theta P}$. Under standard assumptions, an increase in P_{jk} (or the share of revenue returned to departments, θ) will result in an increase in supply (N_{jk}), but an ambiguous change in quality (Q_{jk}) due to offsetting income and substitution effects. Departments receive more revenue with which they can improve quality (an income effect), but the change also alters the relative net costs of size and quality, providing an incentive to shift resources from the latter to the former (the substitution effect). The supply of slots in field k is upward sloping with price.

[4] This discussion simplifies things by assuming that the effort costs and non-financial benefits of a given major are not altered when its price increases.

Even if departments do not alter quality, the effect of a change in price will depend on the nature of the initial equilibrium.[5] If a department initially has excess supply (more slots available than students demand at the externally-set initial price), then demand effects will dominate and equilibrium quantity will decline as price increases. However, if initial price is too low to equate supply and demand (resulting in excess demand), an increase in price will enable a department to expand capacity and equilibrium quantity will increase with price.

To summarize, demand theory is unambiguous in predicting that differential pricing should discourage students from entering the impacted fields, holding all else constant. However, if impacted programs use the additional revenue to improve quality, the net effect on demand will be ambiguous since quality improvements will increase demand. Furthermore, if the equilibrium at initial prices is one of over-demand (a shortage of available seats), then higher prices may permit oversubscribed departments to expand supply and increase the total number of students. Thus, we may expect to see a range of effects across majors and institutions, depending on the major-specific elasticity of demand, the extent to which additional revenue is used to improve instructional quality, the elasticity of supply, and the nature of the equilibrium point at initial prices. The average combined effect of all these mechanisms across all institutions is thus an empirical question. While the data do not permit the separate identification of these various channels, it is important to keep in mind that the reduced form effects I estimate are a combination of responses by students (demand) and institutions (supply). This may be, however, the effect most relevant to policy-makers who have the ability to set prices, but do not directly control what individual departments do with any additional revenue.

[5] For instance, if department preferences over quality and enrollment can be characterized as Cobb-Douglas, then income and substitution effects will offset and there will be no adjustment in quality when department-specific price increases.

IV. Previous Literature

There is a large body of evidence showing that students' enrollment, persistence, and college choices are influenced by net college price. A consensus estimate is that a $1,000 change in college price (1990 dollars) is associated with an approximately 3-5 percentage point difference in enrollment rates (Kane 2006). Evidence on the effect of college price on persistence and degree completion is rarer, but most studies suggest that persistence and completion are modestly responsive to prices for at least some groups (Bettinger 2004, Turner 2004, Dynarski 2008, DesJardins and McCall 2010, Goldrick-Rab et al 2011). Price also appears to be a strong predictor of the specific college students choose to attend (Long, 2004, Jacob McCall and Stange, 2012). All of this work exploits variation that affects prices of all majors simultaneously, so it sheds little light on the independent price effects across majors.

Previous research on the determinants of major choice has focused on expected earnings, student tastes or preferences, and student ability. Berger (1988) finds that students respond to predicted lifetime earnings across majors, rather than starting salaries, consistent with a standard economic life-cycle model. Montmarquet, Cannings, Mahseredjian (2002) extend this approach by including uncertainty about successful completion for each major. Arcidiacono (2004) estimates a dynamic structural model to control for selection into major and finds that student ability, preferences, and earnings all impact student choice of major. Exploiting differences in major-specific returns over the business cycle to eliminate selection bias, Beffy, Fougere, and Maurel (2011) find that the elasticity of major choice to expected earnings is significant, but low. They conclude that nonpecuniary factors are a primary determinant of major choices.[6] Griffith

[6] Very recently, researchers have begun to collect subjective expectations of earnings in each major in an attempt to isolate the effect of earnings expectations while relying on fewer assumptions about expectations (Arcidiacono, Hotz, and Kang 2011; Stinebrickner and Stinebrickner 2011; Wiswall and Zafar (2011). These papers all conclude

(2010) finds that academic background, grade performance, and the educational focus of the institution explain a great deal of the higher exit rate of women and minorities from STEM fields. Some fields may also be attractive as gateways into further education: Eide and Waehrer (1998) conclude that option to attend graduate school make cause some students to choose majors with low earnings.

There has been no research on how major-specific prices affect students' major choice. Furthermore, students' responses to earnings differences by major (for which there is evidence) may provide a poor guide to the likely effects of differential tuition. On one hand, upfront tuition may be more salient than long-run earnings differences, and thus the price elasticity may be greater than the earnings elasticity. One the other, tuition differentials between majors are small relative to earnings differences, so may be less important to student choices. Rothstein and Rouse (2011) find that the form of financial aid influences major and career choice: greater debt leads graduates to pursue higher-paid jobs and majors. This suggests that students may weigh short-term and long-term financial considerations differently, so price and earnings responses may be very different. I add to this literature by explicitly estimating the price response of major choice.[7]

Evidence on the response of institutions to price (or resources more generally) is also limited, though the research that does exist has found that institutions reallocate real resources when faced with changes in their budgets and that these reallocations have real impacts on students. For instance, Brown et al (2010) find that negative endowment shocks lead universities to reduce hiring (or accelerate the firing) of both faculty and support personnel (but not

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that future earnings are an important consideration in students' major choice, though preferences and ability/background may be even more important.

[7] Hoenack and Weiler (1975) and Berg and Hoenack (1987) discuss the implementation of cost-related tuition (an earlier name for "differential tuition") at the University of Minnesota and also present simulation results of the likely consequences. Neither of these papers directly assesses the impact of the policy, however. Hoenack and Weiler (1975) simulate major-specific price responses using the enrollment response to distance to approximate the enrollment response to differential tuition.

university administrators), but positive endowment shocks have no effect on these measures of real resources. Using changes in per-student funding arising from exogenous variation in cohort size across states over time, Bound and Turner (2007) conclude that funding for public universities has a large impact on both the quantity and the quality of college graduates because supply is far from perfectly elastic. A reduction in per-student state appropriations thus reduces collegiate attainment and the production of college-educated workers. Zhang (2009) finds that greater state appropriations at public universities are associated with higher graduation rates, though which mediating factors (improved quality of instruction, more support services, more generous institutional aid) explain this relationship is not assessed. At the institution level, greater tuition revenue has a direct and strong relationship with instructional spending (Leslie, Slaughter, Taylor, and Zhang, 2012) at public research universities. I am not aware of any evidence on the reallocation of resources across departments within institutions in response to greater revenue generated by specific departments. Though if institution-level evidence is any guide, we'd expect departments to increase both program quality and quantity (number of students) in response to differential pricing.

V. Empirical Implementation

A. Data and sample

Information on differential tuition prices by undergraduate major or program is not readily available from any standard data source. The most common source for tuition information, the Integrated Postsecondary Education Data System (IPEDS), only publishes differentials by in-state status.[8] I have obtained data on tuition differentials by program compiled by Nelson (2008). This data contains the incremental tuition or fees charged to different majors above base tuition

[8] IPEDS does currently collect program-specific tuition prices for some institutions, but these are vocational-oriented institutions and programs, not bachelors-granting undergraduate institutions. IPEDS did collect differential information for a few select years in the 1980s, but the reliability and completeness of this information is not clear.

(in percentage terms) for the 2007-2008 academic year at 161 public research universities.[9] Seventy-four of these institutions had differential tuition for at least one program in 2007-2008. The data also contains information on the year of differential enactment and which schools considered (but did not implement) differential pricing. Of the 161 institutions, the precise timing of differential adoption was unavailable for 19 institutions, so my analysis focuses on the 142 remaining institutions (55 that adopted differential pricing for at least one program). My analysis focuses on the 50 institutions that had implemented differentials for engineering, business, and nursing majors as of the 2007/2008 academic year. These three fields are the most common fields in which differentials were enacted that also affect a sizable number of students. Though differentials for architecture and fine arts are also common, these impact a very small number of students and are ignored in my analysis. Table A1 in the appendix list the schools that adopted differential tuition policies for these three majors, along with the magnitude and timing of adoption. One limitation of the data is that the timing of field-specific differentials was not obtained, so I have assumed that differentials for all majors at a school were adopted at the same time. If schools enacted differentials for different fields during different years, then the timing may be misclassified, creating attenuation bias in my estimates.

The primary outcome I examine is the share of undergraduate degrees awarded by field, which is assessed using the IPEDS Degrees and Certificates Conferred (Completions) module. The raw data includes the number of students who complete a postsecondary program by Classification of Instructional Programs (CIP) code and level by sex and race. From this data I calculate the fraction of bachelor's degrees awarded in engineering, business, and nursing for each institution in each year from 1990 to 2008 overall and by sex and race. The full dataset thus

[9] These were the 165 public research intensive and extensive institutions defined by 2000 Carnegie Classification categories 15 and 16. I exclude UCSF, CUNY-Graduate, U Maryland-Baltimore, and U Puerto Rico which had specialized undergraduate programs.

contains 2698 observations (142 institutions X 19 years), though several specifications restrict this sample in different ways. Most importantly, many specifications restrict the sample to include only four years before and after the implementation of differential pricing for those institutions that adopt such policies so that baseline major shares for these institutions are estimated with observations close to the time of adoption. The resulting sample size is smaller (2,304 for Engineering, 2,234 for Business, and 2,489 for Nursing). This outcome data was supplemented with year-specific freshmen enrollment, tuition (in-state and out-of-state differential), resources (full-time faculty, state appropriations, and spending per FTE), and student attributes (% full-time, % in-state, Pell grant amount per FTE). Institutions are grouped into three selectivity categories, using the Barron's taxonomy (most or highly competitive, very competitive, competitive or less competitive).

Table 1 presents summary statistics of my analysis sample. Institutions that adopt differential pricing tend to be slightly larger and better resourced and are more likely to be in the "very competitive" category. Across all schools and years, business majors represent 18% of the sample, engineering 8%, and nursing 3%. Though the fraction of students choosing nursing is comparable across the three groups, institutions with differentials tend to have more engineering and business majors than colleges without differentials. Given these apparent differences between institutions with and without differentials, it will be important to control for observed (and unobserved) differences between colleges that may correlate with both major choice and the adoption of differential pricing.

To analyze how differential pricing affects the composition and financial aid of students in impacted fields, I also analyze individual-level data from the 1996, 2000, 2004, and 2008 waves of the National Postsecondary Student Aid Study (NPSAS).[10] My NPSAS analysis sample consists of undergraduate students who attended one of these 142 universities, excluding students attending multiple institutions during the survey year, a few whose undergraduate level is missing, and any students whose major field is either missing or undecided/undeclared. I also restrict attention to full-time, full-year students so that financial aid differences do not reflect enrollment intensity. Across all four years, the NPSAS student sample contains approximately 18,000 students attending one of 141 universities.[11]

B. Identification strategy

As shown in Figure 1, institutions adopted differential pricing for these programs at different times throughout the past two decades. Using this staggered adoption, my basic empirical strategy is to compare changes in major shares at universities that have recently adopted differential tuition pricing to changes at universities that did not alter their tuition policy during the same time period. To implement this difference-in-differences strategy, I estimate regressions of the form:

$$EngShare_{jt} = \beta EngDiff_{jt} + \alpha X_{it} + \delta_t + \lambda_j + \varepsilon_{jt} \tag{1}$$

In this specification, $EngShare$ is the fraction of degrees awarded in engineering at university j during year t. $EngDiff$ is an indicator for whether j charges differential tuition for engineering during year t, X is a vector of time-varying institutional controls, δ is a set of year fixed effects, λ is a full set of school fixed effects, and ε is an error term. Aggregate time trends in major choice

[10] An earlier version of this paper also used the NPSAS to assess major choice, but estimates from this analysis (which found no statistically significant effects of differential pricing) were extremely imprecise and thus abandoned in favor of using the IPEDS completions data. Using IPEDS completions data generates confidence intervals that are three to five times narrower and also permits the testing for pre-treatment balance using an event-study approach.

[11] One of my 142 analysis institutions do not appear in the NPSAS. I have rounded the number of students to the nearest five hundred. Missing information on SAT score reduces this sample to 12,000 for analysis that relies on non-missing SAT scores. Using a balanced sample of institutions that appear in all waves of the NPSAS generates qualitatively similar estimates.

across all institutions (e.g. changes in the popularity of the business major) are accounted for by year fixed effects. School fixed effects control for average differences in field prevalence across institutions that may be related to the adoption of differential tuition policies. Time-varying school characteristics control for any changes in student population or school resources at the institution-level that may correlate with adoption of differential tuition. This specification is conceptually equivalent to estimating a separate difference-in-differences model for each school that implemented differential tuition, then pooling these school-specific estimates. The coefficient of interest (β) is the change in the change in share of degrees granted in engineering following the adoption of differential pricing for engineering. I estimate (1) separately for the three majors that have differential tuition most frequently – engineering, business, and nursing – and that also represent a sizeable share of all college students. Differentials for small majors such as architecture and fine art are also common, but are expected to impact very few students. Standard errors are clustered by institution, to address the possibility that errors within schools are not independent.

The simple difference-in-differences specification assumes that outcomes for treatment and control schools would trend similarly in the absence of treatment. While inherently not testable, the panel data does allow one to test whether treatment and control schools were trending similarly in the years leading up to the adoption of differential pricing by the former. To do so, I estimate an event-study specification:

$$EngShare_{jt} = \sum_{k=-3}^{k=4^{+}} \beta^{k} StartEngDiff_{jt+k} + \alpha X_{it} + \delta_{t} + \lambda_{j} + \varepsilon_{jt} \qquad (2)$$

In the event study specification, $StartEngDiff_{jk}$ indicates that institution j adopted differential pricing for engineering k years before year t. The parameter β^{k} is the change in share of degrees granted in engineering k years after the adoption of differential pricing relative to the omitted category (k = -4 or earlier). For instance, β^{-3} is the change in share 3 years before adoption, β^{0} is the share change in the year of adoption and β^{4+} is the share change 4 or more years after adoption (all relative to four or more years before adoption). A suggestive test of the common trends assumption is that all the pre-treatment coefficeints are equal to zero. Another limitation of the simple difference-in-differences specification is that a new pricing policy may take a few years before affecting degree production, but it is not obvious how quickly this will happen. The event study specification has the additional benefit of quantifying how quickly policy effects develop.

It is worth contrasting this observational approach to the ideal setting in which to estimate causal effects. In the ideal setting, each program at each university would be assigned a distinct price. Randomization of this price vector would ensure that prices were independent of other student and institutional characteristics influencing the number of students pursuing each major. Given randomization, the simple correlation between price and major share would provide an estimate of the causal effect of the former on the latter. In this project, I use the adoption of differential tuition policies within intuitions over time as an approximation of this experimental ideal. I assume that differential tuition prices are as if randomly assigned, conditional on covariates and fixed (time-invariant) differences between universities.

Choice of control group (institutions that do not alter tuition policies during the analysis window) is central to identification, as these institutions control for aggregate trends in the popularity of impacted majors. The key identifying assumption of the difference-in-differences approach is that treatment institutions would experience the same time pattern of major shares as control institutions, if they had not implemented differential tuition. To probe the robustness of my base results, which use all institutions to generate this counterfactual, I also present results using different sets of institutions as a control group: (1) institutions enacting any type of differential tuition by 2007/2008; (2) institutions that considered (but did not enact) differential tuition by 2007/2008; and (3) institutions matched to treatment institutions based on census division, Barron's selectivity category, or state through the inclusion of different sets of fixed effects. State fixed effects allow the desirability of a major to vary by state, exploiting within-state differences in the adoption of differential tuition policies. For instance, the relative desirability of majoring in engineering at University of Oregon (no differential tuition for engineering) will serve as a counterfactual for the relative desirability of majoring in engineering at Oregon State and Portland State Universities (both enacted differential tuition for engineering in 1994) in each year. Since the models also control for institution fixed effects, any time-invariant differences across institutions that may be correlated with differential tuition and major choice will not confound estimates of β.

Since major-specific price differentials are not experimentally assigned, there are several threats to identification that confound estimates of β. First, lifetime earnings differences across majors and unobserved student preferences for majors (or the jobs that certain majors lead to) cannot be directly entered as time-varying controls. If differential tuition is implemented for specific majors precisely when they become more desirable or lucrative at specific schools, β will suffer from omitted variable bias. It thus may appear that students actually prefer to pay higher prices. To address this I test for pre-trends and also make comparisons to schools within the same region or state and those that considered (but did not implement) differential tuition. Presumably demand for impacted majors was sufficiently high at these latter institutions to warrant a formal consideration of differential tuition. Region- and state-specific time trends control for any time-varying determinants of major share that are common to all institutions in the same geographic area, such as labor market conditions or K-12 preparation. A lack of differential trends between adopting and non-adopting universities immediately before treatment occurs would also suggest policy adoption is not correlated with unobserved factors. Given the many political and legislative hurdles to adopting differential pricing, it is unlikely that institutions are able to control policy adoption with yearly precision.

Another possible confounder is financial aid. The vast majority of financial aid is based on need or general merit and is independent of program of study, so will not bias estimates of β. The only Federal financial aid program that specifically considers major is the SMART Grant, which provided large grants to Pell upperclassmen majoring in physical, life, or computer sciences, mathematics, technology, or engineering or a critical foreign language from 2006-2010. Since this program was available to students at all institutions, regardless of differential pricing, its existence should not bias my estimates. However, it is possible that institutions may re-direct some of the additional revenue collected from differential tuition to financial aid for students in affected majors. I explicitly examine whether schools with differential tuition provide more institutional aid to students in affected majors conditional on merit and income.

Finally, I cannot rule out the possibility that institutions happen to implement other policies coincident with differential tuition. For instance, if differential tuition accompanied changes in the entry requirements for different majors or outreach by impacted departments, then my estimates will confound the pricing effect with these other policies as well. It should be reiterated that my estimates may combine a demand price response, a quality response, and changes in supply resulting from major-specific price differentials. Separately distinguishing demand and supply would require a different setting in which price was altered for only one side of the market in isolation.

VI. Results

A. "Case-study" Evidence

I first document how the major share changes following each school's adoption of differential tuition. For each university that implemented differential pricing for engineering, business, or nursing between 1990 and 2008, I calculate the change in the fraction in each major following the policy change. The left panel of Figure 2 plots the distribution of these school-specific changes for the three majors. While there is substantial heterogeneity in schools' experience following the introduction of differential pricing, the majority of schools experienced a decrease in the fraction of students majoring in engineering and business. In contrast, a majority of schools experienced an *increase* in the fraction of students majoring in nursing when differential pricing for nursing was introduced. Since many things could be determining time trends in major choice at individual colleges and also be correlated with differential pricing, one should not necessarily interpret these raw estimates as causal effects. For instance, changes in the demand for certain fields within states that happen to correlate with changes in pricing policy may cause the simple change over time to not equal the causal effect of differential pricing on major share. The right column of Figure 2 plots the distribution of these school-specific changes after controlling for major-specific time trends using colleges in the same region and Barron's selectivity group as controls.[12] This method controls for any time trends in the popularity of certain majors within regions and selectivity category. Though the distribution of estimates

[12] The histograms plot the distribution of treatment effects estimated by school-specific difference-in-differences models. For each college that enacted differential tuition, I estimate a separate regression of $MAJORSHARE_{tj}$ on $DIFF_{tj}$ (=1 if the college had differential tuition during year t), $SWITCHER_j$ (a dummy for the college under study) and year dummies on a sample that includes the SWITCHER college and any other control colleges in the same census division and Barron's category (most/highly competitive, very competitive, competitive/less/noncompetitive). The histograms plot the distribution of estimated coefficients on $DIFF_{tj}$.

changes somewhat, the original pattern remains. This general pattern –negative effects of differential pricing on the fraction of degrees awarded in engineering or business and positive or minimal effects on the fraction awarded in nursing – persists throughout a number of different identification strategies and robustness checks.

B. Main Results

Figure 3 presents estimates of the event study model separately by field using the restricted (+/- 4 year window) sample.[13] The figure plots the point estimates and 95% confidence interval for the β^k coefficients in equation (2). Consistent with the assumption that differential pricing was not implemented when these three majors were trending differently at treatment and control schools, the point estimates on the pre-treatment years are close to zero and insignificant. This finding gives some credibility to the key difference-in-differences assumption that treatment and control schools would have trended similarly if not for the adoption of differential pricing. However, the share of degrees awarded in engineering or businesses eventually drops following the enactment of differential pricing, while the nursing share increases. These event-study estimates also suggest that any treatment effects may take 3 to 4 years to emerge, as the point estimates experience their most notable change three years after differential pricing was enacted. To gain precision and to facilitate the comparison of many specifications, my preferred specification is a difference-in-differences model that permits separate effects for the immediate (0, 1, and 2 years after the policy was enacted) and medium-run (3 and 4 years after) time periods. Table 2 presents these difference-in-differences results.

Columns (1), (5), and (9) present the raw correlation between differential tuition policies and major share. University-year observations in which differentials are in place for engineering and

[13] Event-study estimates using the full balanced panel (not restricted to 8 year window around policy adoption) are qualitatively very similar, though larger in magnitude.

nursing are coincident with greater number of degrees awarded in these majors. The raw correlation for business majors is small, negative and insignificant. This raw correlation may overstate the positive effect of tuition differentials (or, rather, understate the negative effect) if differentials are implemented by universities whose students are predisposed to choose impacted majors, as a simple revenue-maximization goal would suggest universities should do. For instance, students with high SAT math scores are more likely to major in engineering and business and thus colleges with high SAT students may be more likely to implement tuition differentials. To address some of these concerns, columns (2), (6), and (10) control for year and university fixed effects. In these models, the effect of differential pricing on major share is identified by changes in major share within universities following the introduction of price differentials, relative to the time path of major share predicted by other (non-treatment) colleges. In all three cases, the point estimate becomes more negative and, in the case of engineering, becomes statistically significant. Specifications (3), (7), and (11) separate the post-treatment observations into two periods (0 to 2 years after adoption vs. 3+ years). Consistent with the event-study estimates, the effect of differential pricing on the major shares are larger three years after enactment than immediately following. The final specifications restrict the analysis sample to include observations for treatment schools only within an eight-year window around the year differential pricing was enacted. Thus observations far from the time of the policy change are not used to identify the pre- or post-period school averages used to calculate the treatment effects. This restriction has the effect of diminishing the estimated effect for engineering and business share. This final (preferred) specification indicates that differential pricing for engineering is associated with a statistically significant 1.1 percentage point decrease in the share of degrees awarded in engineering within three years (on a base of 14.7 percent). The analogous figure for business is an (imprecise) 0.8 percentage point decrease in the business share within three years (on a base of 19.5 percent). Differential pricing for nursing is actually associated with a 0.8 percentage point *increase* in the nursing share (on a base of 4.4 percent), though this is imprecise and not significantly different from zero. The 95% confidence interval permits me to rule out negative effects larger than 0.37 percentage points.

Given the magnitude of the price increase associated with these policies (increase in price of engineering by 14.5%, business by 13.7% and nursing by 18.9%.), these represent fairly large elasticities. For engineering and business, the implied elasticities are positive 0.51 and 0.30, respectively. For nursing, the elasticity is positive and almost unity (elasticity = 1.0).

C. Robustness of Main Results

The key untestable assumption of the difference-in-differences approach is that the time path for the outcome experienced by control schools provides a valid counterfactual for the time path of treatment schools in absence of the treatment. That is, the time trend in fraction of students graduating with a degree in engineering at schools that did not adopt differential tuition is what adopters would have experienced had they not implemented differential pricing. Given the centrality of this counterfactual time path to the validity of difference-in-differences estimates, the choice of control group is critical. My base model uses all non-adopters to form the control group, both schools that had differential tuition policies in place throughout the time period and those that never implemented one. Table 3 examines the robustness of the main findings to the choice of control group used to estimate the counterfactual time trends. The first column reports the base model, taken from columns (4), (8), and (12) from Table 2.

Column (2) controls for observable differences in time-varying characteristics between treatment and control universities (time-invariant differences are absorbed by the school fixed effects). I control for the in-state list tuition price, out-of-state tuition differential, full-time faculty to student ratio, state appropriations per student, instructional and academic support spending per student, fraction of students that are full-time, fraction in-state, and the average Pell grant per student. These covariates control for time-varying differences in overall prices, resources, and student characteristics between treatment and controls that may happen to correlate with both degree mix and the adoption of differential pricing. These controls leave the baseline estimates virtually identical. Column (3) includes controls for the simultaneous adoption of differential pricing in related fields.[14] The presence of differentials for other (related) fields is relatively uncommon and has no impact on the point estimates.

Columns (4), (5), and (6) alter the control group by restricting the sample to students only attending schools that either adopted differential tuition during the analysis period or that are arguably more similar to adopters than a typical non-adopter school. These control groups include schools that have adopted some form of differential by 2007/8 in any field (column (4)), only universities that adopted a differential in the given major (5), and the 16 schools that considered (but did not adopt) tuition differentials in any field (6). The main qualitative results are generally robust to these various control groups, though the magnitudes of the point estimates does change somewhat. In (4) and (6), engineering differentials are associated with a 1.1 percentage point drop in the engineering share after three years. Specification (4) is the only anomaly, with a much smaller, but negative, and insignificant coefficient for engineering. It should be noted that this specification has a substantially smaller sample size than the others so I cannot reject that coefficients are different. The coefficients for business and nursing change only slightly, remaining negative for business and positive for nursing, but insignificant for both.

[14] I include controls for differential pricing for architecture, computer science, or physical science when examining engineering share, liberal arts when examining business share, and other health professions and physical therapy when studying nursing share.

Columns (7) through (10) alter the control group used to generate counterfactual time trends by estimating time trends that are specific to various college characteristics. These models permit distinct time trends by census division (7), Barron's selectivity category (8), institution state (9), and the interaction between division and Barron's category (10). For instance, if there was an increased demand for engineers from selective colleges on the west coast which happened to coincide with the adoption of differential tuition policies at some west coast schools, then specification (10) would control for this source of omitted variable bias. Identification comes from comparisons between the trends in degree share of adopters and non-adopters among similarly-selective schools in the same region. Specification (9) permits time trends to vary by institution state, exploiting within-state variation in the adoption of differential tuition. The base results are robust to all these alternative control groups. The point estimates for engineering share are remarkably stable and those for business and nursing only become larger in magnitude, though are still insignificant.

D. Heterogeneity

A primary concern voiced by opponents of differential pricing is that certain groups would be particularly affected. For instance, if minority or low-income students are particularly price-sensitive, then they may be dissuaded from entering more high-priced fields. Differential responses would be worrisome given that these fields are particularly lucrative and that there is already concern about underrepresented minority and female representation in many fields. To test for response heterogeneity, I re-estimate the base model separately by gender and race. The outcome variables are the share of all degrees awarded to individuals in each group at time t that were in engineering, business, and nursing. Table 4 presents these results. The point estimate for the 3-year impact on engineering share is similar for most gender and racial groups, but given the large differences in initial major share across groups, the percent reduction is much larger for women than men and underrepresented minorities than white students. Interestingly, the absolute and proportional response is greatest for Asian students, despite their high initial share in engineering. For business, there is less variation across gender and race in the baseline degree share, so similar absolute effects across groups results in similar proportionate effects for men and women and for black and white students. As for engineering, the effect for Asian students is large both absolutely and proportionately. Contrary to the pattern for these other racial/ethnic groups is the experience of Hispanic students, for which the point estimate is positive (but statistically insignificant). Lastly, Panel C presents the results for nursing. Differences across groups are more difficult to interpret as the estimates are much less precise relative to the initial major share than for engineering and business. But the point estimates are positive (though not statistically significant) for all gender and racial groups. For men, the point estimate is significant and implies an extremely large proportionate increase in the share of men majoring in nursing following the introduction of differential pricing for nursing.

Table 5 presents additional evidence on whether differential pricing altered the characteristics of students who enter impacted fields using individual-level data from the NPSAS. A benefit of the individual data is that I can test for changes in characteristics not available in the aggregate IPEDS data, such as test scores and socioeconomic status. I regress each student characteristic on dummies for being in each impacted major, indicators for whether the institution charged differentially for the majors during the survey year, and interactions between major and differential pricing.[15] In this difference-in-differences specification, coefficients on the interactions test whether the characteristic changed more for the impacted

[15] The models also include a full set of year and institution fixed effects. Qualitative results do not change if I include the major indicator, differential pricing indicator, and interaction for each field one at a time.

fields than other fields following the introduction of differential pricing. For instance, if women were driven from studying engineering when differential pricing was introduced, the coefficient on the engineering interaction should be negative in column (1). Though there are substantial differences in student characteristics across fields (men and high SAT math students more likely to enter engineering, high income students more likely to enter business), there are few significant changes in student characteristics following the introduction of differential pricing. There is some evidence that differential pricing for engineering students is associated with fewer Pell recipients entering engineering and shift towards students with higher SAT scores, but no other changes are significant. It should be noted that due to the relatively few students in each major at each institution, estimates are imprecise and I cannot rule out modest changes in student characteristics following the introduction of differential pricing.

E. Financial Aid

One way that institutions can use revenue generated by differential pricing is to provide additional financial aid to students in impacted majors, partially offsetting the tuition increase. George-Jackson, Rincon, and Garcia (2011) found that minorities studying engineering at two universities received financial aid packages that offset differential tuition. Table 6 presents estimates of the effect of differential pricing on the share of list price covered by institutional grant aid using the same difference-in-differences model used to examine student characteristics. Institutional grant aid covers 15% of the tuition list price across our entire sample. Coefficients on the interactions test whether institutional grant aid changed more for the impacted fields than other fields following the introduction of differential pricing. For instance, if business schools redirected the revenue generated from differential pricing to more grant aid for undergraduate business students, the coefficient on the business interaction should be positive. I find no evidence that differential pricing leads to a reallocation of institutional grant aid across majors. Whether controlling for an extensive set of individual controls (SAT score, female, minority, undergraduate level, EFC) or looking at specific student subgroups, the interaction coefficients are never significant.

VII. Implications and Conclusions

This paper provides the first evidence on the consequences of differential pricing by undergraduate program in postsecondary education. Many colleges are now using this strategy to increase revenue in the face of declining state support. Given the differences in instructional costs and earnings premiums across majors, some view this practice as an equitable and politically feasible alternative to across-the-board tuition and fees increases. I find that differential pricing is associated with a sizable reduction in the fraction of degrees granted in engineering: the elasticity of engineering share with respect to price is -0.51. Business share is slightly less responsive (elasticity = -0.30), though this is not significant at conventional levels. Differential pricing for nursing is actually associated with a large *increase* in the nursing share (elasticity = +0.97), though this is imprecise and not significantly different from zero. Consistent with the concern of some critics of this development, I also find that women and minorities have larger proportionate effects than male and white students. It does not appear that additional institutional grant aid offsets the increased tuition for impacted majors.

This study has relevance for a number of different policies. Most directly, the results inform the likely consequences of colleges' use of differential pricing, both overall and for specific subgroups such as low-income students. Previous research on the effect of price on college enrollment or college choice and the effect of expected earnings on major choice are unlikely to provide much guidance to the likely effects of differential pricing by program. My results suggest that implementing these differentials may indeed impact student choice. Furthermore, since differentials may reduce demand, these policies may not raise as much revenue as expected. It is important for colleges to understand how the revenue and student impact of differential pricing compares to alternative pricing schemes such as across-the-board tuition increases or tuition increases for wealthier or out-of-state students. This paper informs one side of this calculation.

The experience with differential pricing may also be informative about the likely impact of financial incentives designed to alter students' field of study. For instance, the impact of the national SMART grant program on major may be difficult to assess since it affected students similarly across the country. Thus it may be difficult to separate the existence of the SMART program from other changes occurring nationally over time. However, the responsiveness of students to differential tuition may thus shed light on the effects of grants or tuition subsidies for targeted majors. That fact that potential engineering students appear to respond to differential pricing suggests that students' major choice may also respond to other financial incentives such as the SMART grant. This study also contributes to our understanding of how students respond to financial incentives at different stages of the college process. Choices may respond to financial incentives differently before college entry, while enrolled in lower division coursework, or closer to graduation, though the timing of incentives has received little attention. Though it is difficult to pin-point precisely when college major choices are made, these results suggest that even decisions made during college can be responsive to price. Understanding where financial incentives are strongest (or weakest) informs how they should best be targeted.

This study has a number of limitations that should be addressed in subsequent work. First, I study the experience of 142 large public research universities, 50 of which adopted differential tuition during the analysis period for engineering, business, or nursing. While these schools represent an important segment of the U.S. postsecondary landscape, their experience may not be typical of other segments, such as smaller public and private colleges, for-profits, and sub-baccalaureate institutions. Future data collection on differentials should target these institutions and examine the consequences.

Second, my data does not permit me to separate demand from supply factors, which combine to determine the sorting of students into majors. Different observed responses across fields may reflect differences in demand parameters, a supply or quality response that differs across fields, or that fields are in different initial equilibrium states since the effects I uncover combine both a demand and supply response. It is possible that additional revenue enables an expansion in the supply of nursing positions while engineering revenue is used to improve quality and attract better (though fewer) students. Uncovering just how and whether programs reallocate resources or increase capacity in response to this new revenue stream would help to interpret my findings and would be a welcome complement to the present study.

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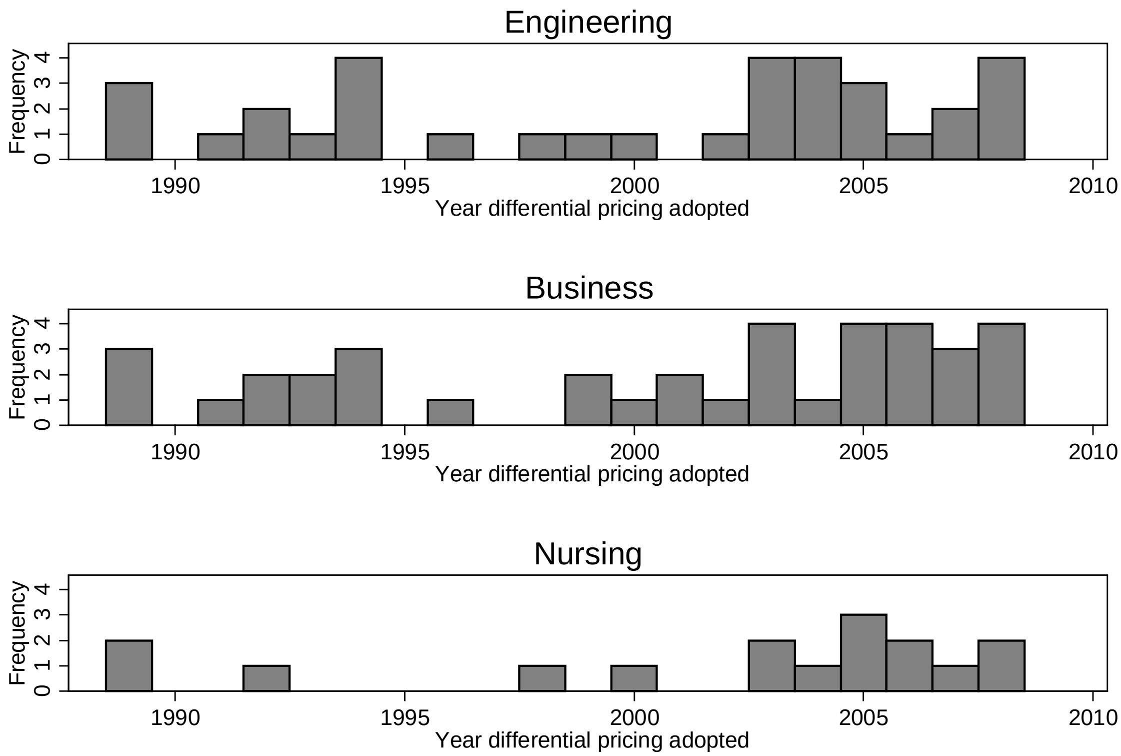
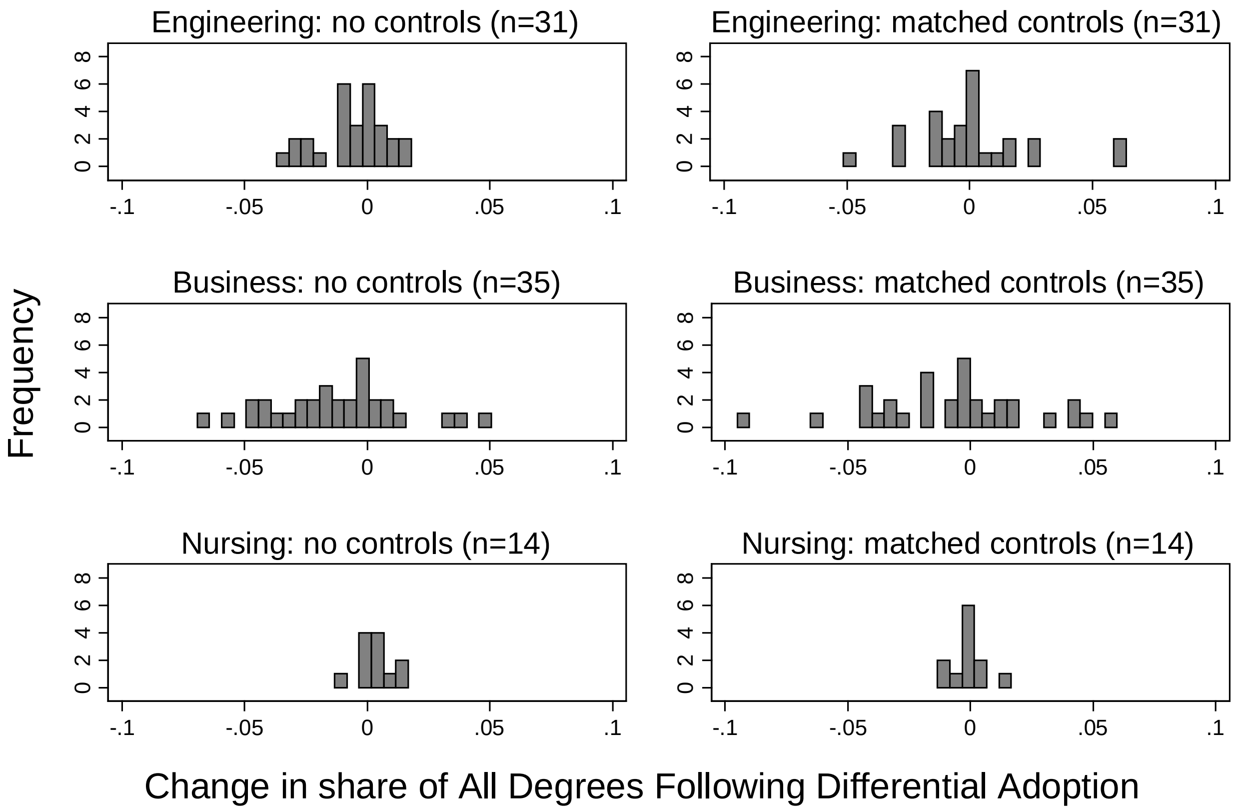
Figure 1. Timing of Adoption of Differential Pricing



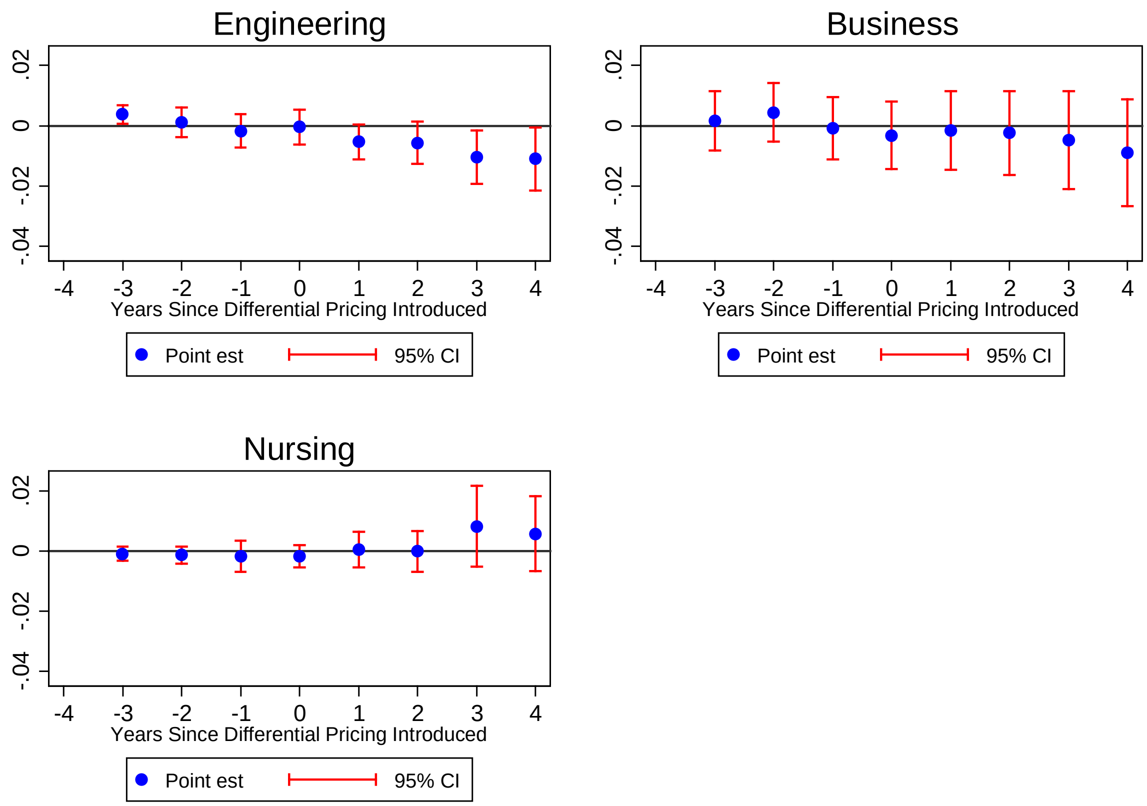
Schools that adopted differential pricing prior to 1990 are plotted in 1989

Figure 2. Distribution of Treatment Effects at Individual Universities



Left column plots the distribution of changes in major share of degrees granted following the introduction of differential tuition at each school. Right column plots distribution of school-specific estimates from regression with one treatment school matched with control schools in same Barrons category and census region.

Figure 3. Event-study Estimates of Effect of Differential Pricing on Major Share



Notes: Graphs plot the point estimates from the event study model in equation (2) using the restricted (+/- 4 year window) sample. Institution sample includes 142 institutions with known adoption dates for differential pricing. Dependent variable is the share of degrees awarded in the specified field.

Table 1. Summary Statistics of Institutional Sample

			Had at least one differential	
	All schools	Never had differential	Timing known	Timing unknown
Pricing differential				
Has differential in engineering during year	0.11	0.00	0.29	n/a
Has differential in busines during year	0.12	0.00	0.30	n/a
Has differential in nursing during year	0.04	0.00	0.11	n/a
College characteristics				
Total BA degrees granted (1,000)	2.97	2.92	3.09	2.81
Current freshmen enrollment (1,000)	2.75	2.62	2.96	2.69
In-state tuition + fees (sticker price) ($1000)	4.76	4.72	4.90	4.54
Out-of-state differential (% over in-state)	1.89	1.95	1.84	1.76
Full-time faculty per 100 FTE	6.34	6.10	6.55	6.88
State appropriations per FTE ($1000)	10.00	10.30	9.57	9.91
Instructional spending per FTE ($1000)	9.10	9.06	9.17	9.12
Academic support spending per FTE ($1000)	2.34	2.34	2.31	2.46
Undergraduates % full-time	0.80	0.80	0.80	0.77
Pell grant amount per FTE ($1000)	0.62	0.66	0.56	0.63
Freshmen enrollment % instate	0.81	0.84	0.78	0.76
Most/highly competitive	0.17	0.20	0.16	0.11
Very competitive	0.30	0.24	0.36	0.37
Competitive/less/noncompetitive	0.53	0.56	0.47	0.53
Share of bachelors degrees awarded in				
Engineering	0.08	0.07	0.10	0.07
Business	0.18	0.17	0.19	0.18
Nursing	0.03	0.03	0.03	0.06
Observations	3,059	1,653	1,045	361
Number of colleges	161	87	55	19

Notes: Full sample includes observations for 161 public research universities for 19 years (1990 to 2008). Analysis sample includes the 87 non-differential schools and the 55 differential schools for which precise information about the timing of adoption of differential pricing was obtained. Data on differential pricing comes from Nelson (2008), college characteristics come from IPEDS and the Delta Cost Project, and share of bachelors degrees awarded by category comes from IPEDS.

Table 2. Effect of Differential Tuition on Composition of Degrees Awarded, Main Results

	Dept Var: Share Engineering				Dept Var: Share Business				Dept Var: Share Nursing			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Have differential in year	0.059**	-0.011**			-0.001	-0.008			0.015**	-0.001		
	(0.028)	(0.005)			(0.015)	(0.005)			(0.006)	(0.002)		
Adopted differential 0-2 years earlier			-0.004	-0.004**			-0.004	-0.004			-0.003	0.000
			(0.004)	(0.002)			(0.004)	(0.004)			(0.002)	(0.002)
Adopted differential 3+ years earlier			-0.017**	-0.011***			-0.012	-0.008			0.004	0.008
			(0.007)	(0.004)			(0.009)	(0.007)			(0.003)	(0.006)
Constant	0.079***	0.097***	0.097***	0.088***	0.181***	0.213***	0.213***	0.210***	0.027***	0.023***	0.023***	0.021***
	(0.009)	(0.002)	(0.002)	(0.002)	(0.007)	(0.003)	(0.003)	(0.004)	(0.003)	(0.001)	(0.001)	(0.001)
Sample	All years	All years	All years	+/- 4 years	All years	All years	All years	+/- 4 years	All years	All years	All years	+/- 4 years
Additional controls	None	Year FE School FE	Year FE School FE	Year FE School FE	None	Year FE School FE	Year FE School FE	Year FE School FE	None	Year FE School FE	Year FE School FE	Year FE School FE
Observations	2,698	2,698	2,698	2,304	2,698	2,698	2,698	2,234	2,698	2,698	2,698	2,489
R-squared	0.027	0.979	0.979	0.978	0.000	0.906	0.906	0.913	0.008	0.903	0.903	0.918
Outcome mean	0.147	0.147	0.147	0.147	0.195	0.195	0.195	0.195	0.044	0.044	0.044	0.044

Notes: Robust standard errors clustered by school in parentheses. *** p<0.01, ** p<0.05, * p<0.1. All specifications include 142 schools, though the number of schools that adopted a differential tuition policy varies between fields. Model is estimated using OLS. Outcome mean is for colleges that eventually adopted tuition differentials in the pre-differential period.

Table 3. Robustness of Main Results to Choice of Control Group and Other Covariates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Sample	Base model: All colleges	All colleges	All colleges	Adopted any differential by 2008	Adopted differential in this major	Adopted differential in this major + considered any differential	All colleges	All colleges	All colleges	All colleges
Additional Controls	None	Institutional control variables	Differential in related field	None	None	None	Census division X Year FE	Barrons X Year FE	State X Year FE	Barrons X Census Division X Year FE
Panel A: Engineering (mean = 0.147)										
Adopted differential 0 to 2 years earlier	-0.004**	-0.004**	-0.003	-0.004*	0.000	-0.004*	-0.004*	-0.004**	-0.005	-0.005
	(0.002)	(0.002)	(0.003)	(0.002)	(0.003)	(0.002)	(0.002)	(0.002)	(0.000)	(0.000)
Adopted differential 3+ years earlier	-0.011***	-0.011***	-0.010*	-0.011***	-0.003	-0.011**	-0.010***	-0.011***	-0.011	-0.008
	(0.004)	(0.004)	(0.005)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.000)	(0.000)
Observations	2,304	2,271	2,304	651	252	708	2,304	2,304	2,304	2,304
Panel B: Business (mean = 0.195)										
Adopted differential 0 to 2 years earlier	-0.004	-0.004	-0.003	-0.004	-0.003	-0.003	-0.005	-0.005	-0.007	-0.007
	(0.004)	(0.004)	(0.004)	(0.005)	(0.004)	(0.005)	(0.004)	(0.004)	(0.000)	(0.000)
Adopted differential 3+ years earlier	-0.008	-0.007	-0.008	-0.010	-0.009	-0.006	-0.011	-0.012*	-0.016	-0.014
	(0.007)	(0.007)	(0.007)	(0.008)	(0.009)	(0.008)	(0.007)	(0.007)	(0.000)	(0.000)
Observations	2,234	2,201	2,234	600	277	733	2,234	2,234	2,234	2,234
Panel C: Nursing (mean = 0.044)										
Adopted differential 0 to 2 years earlier	0.000	-0.001	-0.000	0.000	0.002	-0.001	-0.000	0.000	0.001	-0.000
	(0.002)	(0.002)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.000)	(0.000)
Adopted differential 3+ years earlier	0.008	0.007	0.007	0.008	0.012	0.007	0.007	0.008	0.012	0.008
	(0.006)	(0.006)	(0.007)	(0.006)	(0.008)	(0.006)	(0.006)	(0.007)	(0.000)	(0.000)
Observations	2,489	2,453	2,489	855	114	570	2,489	2,489	2,489	2,489

Notes: All specifications include year fixed effects, college fixed effects, and are restricted to 4 years before and after the adoption of a price differential for each school. Column (2) includes in-state list tuition price, out-of-state tuition differential, full-time faculty to student ratio, state appropriations per student, instructional and academic support spending per student, fraction of students that are full-time, fraction in-state, and the average Pell grant per student. Column (3) includescontrols for differential pricing for architecture, computer science, or physical science (Panel A), liberal arts (Panel B), or other health professions and physical therapy (Panel C). Robust standard errors clustered by school in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Outcome mean is for colleges that eventually

Table 4. Response Heterogeneity by Gender and Race

	Women (1)	Men (2)	Black (3)	White (4)	Hispanic (5)	Asian (6)	Other race (7)
	Panel A: Engineering						
Outcome mean	0.0736	0.215	0.0952	0.128	0.121	0.196	0.200
Adopted differential 0 to 2 years earlier	-0.003**	-0.006*	-0.013*	-0.004	-0.009	-0.031**	-0.002
	(0.001)	(0.003)	(0.007)	(0.003)	(0.013)	(0.014)	(0.009)
Adopted differential 3+ years earlier	-0.010**	-0.012**	-0.015	-0.012***	-0.014	-0.050**	-0.012
	(0.004)	(0.005)	(0.011)	(0.004)	(0.018)	(0.019)	(0.016)
3+ year coefficient/mean	-0.136	-0.056	-0.158	-0.094	-0.116	-0.255	-0.060
Observations	2,304	2,304	1,709	1,712	1,695	1,707	1,705
	Panel B: Business						
Outcome mean	0.163	0.234	0.165	0.183	0.170	0.246	0.244
Adopted differential 0 to 2 years earlier	-0.003	-0.005	-0.002	-0.004	0.001	-0.017	-0.018
	(0.004)	(0.005)	(0.011)	(0.004)	(0.013)	(0.017)	(0.012)
Adopted differential 3+ years earlier	-0.008	-0.009	-0.006	-0.009	0.015	-0.026	-0.030*
	(0.006)	(0.008)	(0.013)	(0.006)	(0.016)	(0.022)	(0.016)
3+ year coefficient/mean	-0.049	-0.038	-0.036	-0.049	0.088	-0.106	-0.123
Observations	2,234	2,234	1,665	1,668	1,651	1,663	1,661
	Panel C: Nursing						
Outcome mean	0.070	0.011	0.037	0.049	0.037	0.033	0.022
Adopted differential 0 to 2 years earlier	-0.001	0.002**	0.010	0.000	0.005	0.004	0.003
	(0.003)	(0.001)	(0.011)	(0.002)	(0.006)	(0.004)	(0.004)
Adopted differential 3+ years earlier	0.008	0.006**	0.009	0.001	0.004	0.008	0.019*
	(0.009)	(0.003)	(0.012)	(0.009)	(0.012)	(0.013)	(0.010)
3+ year coefficient/mean	0.114	0.545	0.243	0.020	0.108	0.242	0.864
Observations	2,489	2,489	1,847	1,850	1,833	1,845	1,843

Notes: All specifications include year fixed effects, college fixed effects, and are restricted to 4 years before and after the adoption of a price differential for each school. Regressions for race groups are limited to 1995-2008. Robust standard errors clustered by school in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Outcome mean is for the specified group at colleges that eventually adopted tuition differentials in the pre-differential period.

Table 5. Effect of Differential Pricing on Student Composition in Impacted Fields using NPSAS Microdata

	Outcomes (sample mean)						
	Female (0.518)	Minority (0.175)	Pell (0.228)	SAT math (0.098)	SAT verbal (0.056)	Income, $thousands (81.71)	EFC , $thousands (14.42)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Engineering major	-0.373***	-0.001	-0.007	0.520***	-0.031	7.116	0.308
	(0.016)	(0.013)	(0.017)	(0.037)	(0.037)	(6.038)	(0.728)
Have engineering differential	-0.013	-0.003	0.020	-0.214**	-0.158*	0.620	0.495
	(0.029)	(0.019)	(0.017)	(0.093)	(0.094)	(4.661)	(0.896)
(Engineering major) X (Have engineering differential)	0.013	-0.015	-0.043*	0.198**	0.243***	-2.959	1.363
	(0.028)	(0.028)	(0.023)	(0.081)	(0.071)	(7.722)	(1.339)
Business major	-0.129***	0.011	-0.040***	0.103***	-0.208***	10.164***	2.738***
	(0.016)	(0.011)	(0.011)	(0.029)	(0.031)	(2.008)	(0.529)
Have business differential	-0.008	-0.018	-0.029	0.052	0.168	5.543	0.463
	(0.034)	(0.023)	(0.025)	(0.072)	(0.112)	(5.260)	(1.193)
(Business major) X (Have business differential)	0.025	-0.015	-0.005	-0.033	0.029	-4.886	-1.146
	(0.031)	(0.024)	(0.023)	(0.054)	(0.057)	(4.343)	(1.208)
Health major	0.194***	0.008	-0.008	-0.178***	-0.282***	-2.968	-0.686
	(0.018)	(0.012)	(0.013)	(0.045)	(0.042)	(1.870)	(0.464)
Have health differential	-0.000	-0.027	0.060**	0.073	-0.120	-3.457	-0.490
	(0.042)	(0.029)	(0.030)	(0.127)	(0.156)	(6.241)	(1.666)
(Health major) X (Have health differential)	0.099*	-0.016	0.041	-0.023	-0.012	0.202	0.392
	(0.054)	(0.036)	(0.075)	(0.093)	(0.135)	(8.019)	(2.178)
Observations	18,105	18,105	18,105	12,202	12,202	18,105	18,105
R-squared	0.096	0.125	0.054	0.232	0.177	0.051	0.054

Notes: All specifications include year fixed and institution fixed effects. Sample includes only full-time, full-year students attending one of 142 institutions with complete differential pricing information. Family income and expected family contribution (EFC) are in 2009 dollars. Specifications (4) and (5) have fewer observations due to missing SAT information for some students. Robust standard errors clustered by school in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 6. Effect of Differential Pricing on Institutional Aid to Students in Impacted Fields

	Dependent variable: (Institutional grants) / (list tuition+fees)					
	All students		In-state	Lower division	Upper division	Fourth year
	(1)	(2)	(3)	(4)	(5)	(6)
Engineering major	0.042**	0.033**	0.048***	0.027	0.045**	0.037
	(0.016)	(0.016)	(0.018)	(0.025)	(0.021)	(0.024)
Have engineering differential	-0.020	-0.013	-0.028	-0.034	-0.004	-0.002
	(0.033)	(0.033)	(0.032)	(0.046)	(0.038)	(0.034)
(Engineering major) X (Have engineering differential)	-0.012	-0.016	0.001	0.014	-0.031	-0.009
	(0.019)	(0.019)	(0.022)	(0.035)	(0.026)	(0.038)
Business major	-0.019**	-0.011	-0.019*	-0.023	-0.017	-0.018
	(0.009)	(0.009)	(0.011)	(0.014)	(0.013)	(0.020)
Have business differential	0.018	0.006	0.022	0.059	-0.009	0.021
	(0.041)	(0.043)	(0.036)	(0.038)	(0.057)	(0.051)
(Business major) X (Have business differential)	0.014	0.015	0.021	0.034	-0.008	0.033
	(0.018)	(0.017)	(0.016)	(0.028)	(0.026)	(0.043)
Health major	-0.013	-0.009	-0.019	-0.014	-0.012	-0.011
	(0.012)	(0.012)	(0.012)	(0.018)	(0.014)	(0.018)
Have health differential	-0.016	-0.009	-0.023	-0.000	-0.041	-0.081
	(0.044)	(0.046)	(0.043)	(0.065)	(0.059)	(0.053)
(Health major) X (Have health differential)	0.009	0.006	-0.003	-0.019	0.028	0.024
	(0.025)	(0.025)	(0.026)	(0.025)	(0.046)	(0.068)
Additional controls	No	Yes	No	No	No	No
Observations	18,039	18,039	15,693	7,010	11,029	7,369
R-squared	0.062	0.089	0.071	0.090	0.070	0.087
Outcome mean	0.150	0.150	0.151	0.165	0.138	0.144

Notes: All specifications include year fixed and institution fixed effects. Sample includes only full-time, full-year students attending one of 142 institutions with complete differential pricing information. Additional controls in specification (2) include female, minority, normalized SAT math and verbal score, dummy for missing SAT score, undergraduate level, instate, and expected famility contribution. Robust standard errors clustered by school in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A1. Institutions with Differential Pricing for Engineering, Business, and Nursing in 2008

Institution	Year adopted	Amount of differential (% over base tuition)		
		Engineering	Business	Nursing
University of South Alabama	2008	8		
University of Arkansas Main Campus	2000	16	14	
University of Arkansas at Little Rock	2001		3	
University of Arizona	1993	12	16	
Colorado State University	2006	6	9	
University of Colorado Denver	1989	14	2	147
University of Colorado at Boulder	1984	38	59	
University of Northern Colorado	2006		7	5
University of Hawaii at Manoa	2007		12	39
Iowa State University	2007	19		
University of Iowa	2007	19		
University of Illinois at Chicago	1992	25	8	26
University of Illinois at Urbana-Champaign	1994	45	45	
Indiana University-Purdue University-Indianapolis	2008		10	16
Purdue University-Main Campus	1999	8	13	
Kansas State University	2003	15	8	
University of Kansas	1994	16	40	
University of Kentucky	2005		6	
University of Louisville	2004	3		
Louisiana Tech University	2006		3	4
Michigan Technological University	2004	11		
Oakland University	2005			2
University of Michigan-Ann Arbor	1989	7		
Missouri University of Science and Technology	1996	23	23	
Montana State University	2003	5	8	8
The University of Montana	2001		22	
North Dakota State University-Main Campus	1998	13		12
University of Nebraska-Lincoln	2004	24		
University of New Hampshire-Main Campus	1991	8	8	
Rutgers University-New Brunswick	1992	11	2	
Rutgers University-Newark	1993		4	
Miami University-Oxford	2007		7	
Oregon State University	1994	30		
Portland State University	1994	24	7	
University of Oregon	1999		10	
Pennsylvania State University-Main Campus	2008	6	6	20
Temple University	1989		2	21
Clemson University	2006		17	
University of South Dakota	2005		30	58
University of Memphis	2002	10	12	
The University of Texas at Arlington	2004	4	13	8
The University of Texas at Austin	2003	12	16	8
The University of Texas at Dallas	2005	15		
The University of Texas at El Paso	2000			2
University of Houston	2005	6	6	
University of Utah	2007		35	
Utah State University	2003	2	31	
Virginia Commonwealth University	2008	31	6	
Virginia Polytechnic Institute and State University	2008	12		
University of Wisconsin-Madison	2008		16	
University of Wisconsin-Milwaukee	2005	9	9	13

Source: Glen Nelson. Blank indicates that no differential for this particular field.