An Empirical Examination of the Bennett Hypothesis in Law School Prices

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Abstract: Whether colleges increase tuition in response to increased federal student loan limits (the Bennett Hypothesis) has been a topic of debate in the higher education community for decades, yet most studies have been based on small increases to Pell Grant or undergraduate student loan limits. In this paper, I leverage a large increase in graduate student lending limits that took place in 2006 followed by an expansion of federal income-driven repayment programs to examine whether law schools responded by raising tuition and whether student debt levels also increased. Using data from 2001 to 2015 across public and private law schools and both interrupted time series and difference-in-differences analytical techniques, I found rather modest relationships across both public and private nonprofit law schools. I conclude with some possible explanations for the lack of strong empirical support for the Bennett Hypothesis.

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1. Introduction

A great deal of attention has been paid to the rapidly rising price of an undergraduate education in the United States. Between the 1996-97 and 2016-17 academic years, the inflation-adjusted listed price of tuition and fees increased by 112% at public four-year colleges and by 68% at private nonprofit colleges (Ma, Baum, Pender, & Welch, 2016). Yet post-baccalaureate prices have received relatively little attention although tuition prices for professional programs such as law, medicine, and business have risen at similar rates (Baum & Steele, 2017) and graduate enrollments have risen faster than undergraduate enrollments since 2000 (McFarland et al., 2017). For example, law students now face sticker tuition and fee prices of nearly $80,000 at public schools and $135,000 at private schools during the three-year course of full-time study (author's calculations using American Bar Association data).

Unlike many students in research doctoral programs (PhD), students pursuing professional doctorates in fields such as law (JD) and medicine (MD) pay most of the price tag of their education. Both law and medical students have only about 20% of total charges covered by scholarships—or one-third of what PhD students receive (Baum & Steele, 2017). This has resulted in student debt rapidly increasing among professional students, with law students seeing the largest increase. In 2012, 87% of law students with debt graduated with a median debt of $140,616, up from $88,634 in inflation-adjusted dollars in 2004 (Delisle, 2014). Students in medical fields borrowed more (an average of $161,772), but median wages for lawyers are half of that for primary care physicians ($118,160 versus $251,578) (Bureau of Labor Statistics, 2017). With starting salaries of new lawyers now below $70,000 (Kowarski, 2017), law students face debt-to-earnings ratios well above students in other graduate and professional fields. In this

paper, I focus on law schools because of the potential implications of high debt burdens on their graduates.

One factor that some people believe contributes to increases in both tuition prices and student debt burdens is the availability of federal financial aid that students can access to finance their education. The idea that colleges take advantage of increased loan eligibility is widely attributed to William Bennett, who was President Reagan's Secretary of Education. In a 1987 opinion piece in *The New York Times* entitled "Our Greedy Colleges," he wrote: "Increases in financial aid in recent years have enabled colleges and universities blithely to raise their tuitions, confident that Federal loan subsidies would help cushion the increase" (Bennett, 1987).

This theory, which became known as the Bennett Hypothesis, has been hotly debated for decades and has been the subject of a number of empirical studies. However, all of this research was for the undergraduate student population and mainly focused on the relationship between federal grant aid and tuition prices (e.g., Archibald & Feldman, 2016; Heller, 2013; Stoll, Bradley, & Mahan, 2014). Yet although post-baccalaureate students hold nearly about 40% of all outstanding student debt (Delisle, 2014), only one prior study (Kelchen, 2019) has examined graduate education programs (in business and medicine). There has been no empirical research examining whether the Bennett Hypothesis holds for law schools.

I conduct the first empirical examination of the Bennett Hypothesis in legal education in this paper by leveraging two policy changes that increased the availability and attractiveness of federal student loans in professional education and focusing on law school students. Prior to 2006, law students (along with most other professional students) could borrow $18,500 per year in federal loans. This amount is well below typical tuition and fees at private law schools and

slightly more than tuition and fees at public law schools, meaning that private loans or self-financing were needed to cover the rest of the cost of attendance. The Higher Education Reconciliation Act of 2005 created the Grad PLUS loan program as of July 1, 2006, which allowed students to borrow up to the full cost of attendance in federal loans. The College Cost Reduction and Access Act of 2007 created a Public Service Loan Forgiveness (PSLF) program that significantly reduced repayment amounts for students working at qualified nonprofits for a period of ten years as of October 1, 2007 along with other income-driven repayment (IDR) options that took effect in 2009.

Together, these two policies have the effect of increasing access to credit for legal education by allowing more students to borrow the full cost of attendance via the Grad PLUS program and allowing at least some students to repay less than their initial student debt amount under IDR. This creates an ideal test of the Bennett Hypothesis because if a college is inclined to maximize total revenues from its law school, the two aforementioned policy changes may provide an opportunity for institutions to gain additional revenue by increasing sticker prices. However, this also would increase graduates' debt burdens, potentially leaving taxpayers with a portion of the total balance for any forgiven loans under income-driven repayment plans amid concerns that the federal government is underestimating the amount of loans that will be forgiven (U.S. Government Accountability Office, 2016). There is some evidence that some colleges may be leveraging IDR and PSLF to shift the repayment burden to taxpayers through programs such as Georgetown Law's Loan Repayment Assistance Program (Mathews, 2013), but the implications of these programs on tuition prices has not been examined in spite of the potential importance to taxpayers who would pay for forgiven loans.

Using a sixteen-year panel of dataset on public and private nonprofit law schools and two different types of comparison groups, I explored the following research questions:

(1) Did tuition/fees for law school students increase at a faster rate following the creation of the Grad PLUS program in 2006 and the expansion of income-driven repayment in 2007?

(2) Did the student debt burden of law school graduates increase at a faster rate following the creation of the Grad PLUS program in 2006 and the expansion of income-driven repayment in 2007?

2. Summary of Graduate and Professional School Borrowing

A large and growing percentage of educational expenses for graduate and professional students is being covered by student loans. Between the 2000-01 and 2010-11 academic years, total federal borrowing by graduate students increased from $14.4 billion to $38.4 billion (in inflation-adjusted dollars). Borrowing then declined slightly to $35.6 billion in the 2015-16 academic year as overall enrollment in graduate school stayed flat after increasing by 40% in the 2000s (Baum, Ma, Pender, & Welch, 2016; National Center for Education Statistics, 2017). Graduate students make up only 15% of all students who are eligible to receive federal financial aid, but they are responsible for about 38% of all federal loan dollars (author's calculation using data from Baum et al., 2016 and National Center for Education Statistics, 2017).

Although graduate students have been able to access federal loans since 1958, graduate student borrowing was greatly expanded in 1992 with the creation of a federal unsubsidized loan program. Beginning in 1993, most graduate and professional students could borrow up to $8,500 per year in subsidized loans (in which interest is not charged until six months after the student

leaves school) and up to $18,500 in subsidized and unsubsidized loans (in which interest accrues immediately) each year. Students were subject to a lifetime borrowing limit of $65,500 in subsidized loans and $138,500 in subsidized and unsubsidized loans (FinAid.org, 2017), with students in certain health-related fields having higher limits (Federal Student Aid, 2008).

Rising tuition prices and stagnant federal borrowing limits meant that more students turned to private loans or savings in order to pay for graduate or professional education. Law students were more likely than all other types of graduate or professional students to take out private loans in the 1990s and 2000s, with the percentage of law students with private loans rising from 13% in 1995-96 to 28% in 1999-2000 and 36% in 2003-04 (Woo & Shaw, 2015). The availability and terms of private educational loans are based in part on a student's creditworthiness, which is not a factor for subsidized and unsubsidized federal loans and could affect whether students are able to access credit for attending college.

The creation of the Grad PLUS program in 2006 addressed this potential gap in financing graduate and professional education by allowing students to borrow up to the full cost of attendance without any other lifetime borrowing limits.[2] Grad PLUS loans are different from other federal loans in that a student must not have an 'adverse credit history'—or provide a cosigner if the borrower has adverse credit (Federal Student Aid, 2015). But this standard is easier for borrowers to meet than the requirements to receive a private loan at competitive interest rates, making credit available to more students. Law students quickly turned away from

[2] Two changes occurred to graduate student lending since the Grad PLUS program, neither of which had a substantial effect on students' ability to access credit. In July 2007, the annual borrowing limit increased to $20,500 through a $2,000 increase in unsubsidized loan limits, while the annual limit for subsidized loans and the lifetime borrowing limit remained unchanged. The Budget Control Act of 2011 eliminated all subsidized loans for graduate and professional students as of July 2012, but the $20,500 annual and $138,500 lifetime borrowing limits for any federal loans outside the Grad PLUS program remained.

private loans, with 20% of students holding private loans in 2007-08 and just 5% in 2011-12. By 2011-12, 59% of law students took out PLUS loans and graduates had nearly $55,000 in Grad PLUS debt. As a comparison, just five percent of MBA students, three percent of PhD students, and 34% of medical students took out Grad PLUS loans in 2011-12 (author's calculations using data from the National Postsecondary Student Aid Study; Woo & Shaw, 2015).

The other major change to graduate and professional student lending occurred with the expansion of income-driven repayment (IDR) options. A small Income-Contingent Repayment (ICR) plan was created in the 1992 Higher Education Act reauthorization, but it was only available to students at a select number of colleges and the terms (up to 20% of discretionary income—above 150% of the federal poverty line—for up to 25 years) were not appealing to most students. In 2007, the College Cost Reduction and Access Act created an Income-Based Repayment (IBR) program that allowed all federal loan borrowers to access an income-driven program that forgave any remaining debt after 25 years of paying 15% of the borrower's discretionary income.[3] Allowing grad students to access IDR likely contributed to the decline in private borrowing, as interest rates for private loans were often competitive with Grad PLUS but private loans did not offer income-driven repayment plans (Bhole, 2017).

IDR options were further expanded in the early 2010s with the creation of three new plans (an updated IBR plan, Pay As You Earn, and Revised Pay As You Earn). These options can allow graduate and professional students to pay 10% of their discretionary income for as little as 20 years (Federal Student Aid, n.d.). All of these plans do tax any remaining balances

[3] In the initial legislation, subsidized and unsubsidized graduate loans were eligible for IBR while Grad PLUS loans were not. However, Grad PLUS loans were included in the final rules that implemented IBR in 2008 in what was likely a drafting error (Shireman, 2017).

that are forgiven, although these programs are too new for anyone to have actually faced a tax bill for forgiveness and multiple bills have been introduced over the last several years to eliminate these taxes (U.S. Government Accountability Office, 2016).

The other key piece of the 2007 legislation was the creation of the Public Service Loan Forgiveness (PSLF) program, in which borrowers working for government or qualified nonprofit agencies for a period of ten years while making payments could have the remainder of their loans forgiven without facing any tax burden on the forgiven balance. Law schools helped lead the push for PSLF in an effort to encourage students to choose public-interest law fields, especially as many law school graduates have high debt-to-income ratios under extended payment plans (Chapman & Lounkaew, 2015; Schrag, 2007). Although the Department of Education does not make specific data on law students' usage of IDR and PSLF available, overall trends suggest that law students are likely using these programs at high rates. As of June 2017, 36% of all Grad PLUS borrowers had ever enrolled in IDR and 11% had submitted a PSLF certification form, with the typical Grad PLUS borrower holding $140,000 in federal loans (Emrey-Arras, 2018).

3. Theoretical Framework and Literature Review

The underlying premise of the Bennett Hypothesis is that colleges will take advantage of the increased availability of federal financial aid dollars to increase their tuition prices. Economic theory suggests that the number of students seeking an education would increase so long as the demand curve is not completely inelastic and some students are credit constrained. Prior research among undergraduate students finds evidence of both demand elasticity and credit constraints (e.g., Hemelt & Marcotte, 2011; Lochner & Monge-Maranjo, 2012), creating the potential conditions to support the Bennett Hypothesis.

Colleges currently have two main incentives to increase graduate and professional tuition whenever possible. The first is to diversify revenue. Many private colleges are heavily reliant on a combination of undergraduate tuition dollars and endowment proceeds, while many public colleges face stagnant per-student real appropriations and limits on undergraduate tuition increases (Armstrong, Carlson, & Laderman, 2017; Desrochers & Hurlburt, 2016; Laderman & Carlson, 2017). Professional programs face an even stronger incentive to push for increased tuition prices due to the growth of responsibility centered management budgeting initiatives that allow programs to keep a portion of any tuition that they generate (Kosten, 2016; Strauss & Curry, 2002). Law schools frequently turn over 25% to 30% of their revenue to the administration, which brings complaints from law school deans that they are subsidizing other departments on campus (Segal, 2011).

The second reason to increase prices whenever possible is Bowen's (1980) revenue theory of costs, which stated that reputation is associated with a high price tag and that colleges will seek to raise and spend as much money as possible as a result.[4] This is augmented by the push from private-sector rankings providers such as *U.S. News & World Report* to directly reward programs that have high per-student expenditures--with the justification that additional resources will result in a higher-quality educational environment (Morse, 2016). There is also some evidence of the "Chivas Regal" effect, in which colleges that slipped in the rankings raised their tuition in an effort to appear more prestigious (Askin & Bothner, 2016).

[4] Colleges often counter mentions of Bowen's revenue theory of costs with Baumol's (1967) cost disease hypothesis, in which the heavy reliance on highly-skilled labor explains cost increases. Over the long term, it appears that Baumol's cost disease is responsible for most of the rise in educational costs (Archibald & Feldman, 2008). However, there is some recent evidence that research universities' actions regarding staffing patterns that would broadly fall under Bowen's theory contribute to rising costs more than Baumol's cost disease (Martin & Hill, 2013; 2014).

The Bennett Hypothesis is related to Bowen's theory in the sense that colleges may not feel the need to be as efficient in their operations if students have ready access to financial aid that can help them pay for college. To this point, nearly all of the empirical literature examining the veracity of the Bennett Hypothesis examines undergraduate tuition and fee prices instead of considering graduate or professional programs. The only exception is Kelchen (2019), who found at most weak relationships between federal student loan limits and tuition prices among medical schools and MBA programs.

The best available research on the Bennett Hypothesis with respect to undergraduate federal student loans finds a modest relationship between increased borrowing limits and listed tuition prices at certain types of institutions. Lau (2014) found that for-profit colleges increased tuition prices by $.51 for each one-dollar increase in loan limits, compared to $.25 among community colleges. Lucca, Nadauld, and Shen (2015) estimated that increases in subsidized loans were more strongly associated with tuition increases ($.45 to $.60 for each dollar in loans) than unsubsidized loans (between zero and $.17). They found stronger evidence of the Bennett Hypothesis at for-profit and expensive private nonprofit colleges than other sectors of higher education. This fits with Gillen's (2012) Bennett Hypothesis 2.0, which opined that selective colleges may respond to the availability of additional federal financial aid by raising their tuition more than less-selective institutions.

There has been more research examining whether increases in Pell Grant awards for undergraduate students are associated with higher listed tuition prices. These findings generally support mixed (Rizzo & Ehrenberg, 2004; Singell & Stone, 2007) or modest positive relationships (Lau, 2014; Lucca et al., 2015; Turner, 2017). Cellini and Goldin (2014) took a different empirical strategy, comparing the prices of for-profit colleges that received federal

financial aid to those that did not and finding some support for the Bennett Hypothesis. A study by Baird, Kofoed, Miller, and Wenger (2018) looked at colleges' pricing strategies following a change to federal veterans' benefits for undergraduate tuition reimbursement and showed that for-profit institutions increased tuition in states with increased benefits and cut tuition in states with decreased benefits.

There are two main differences between undergraduate and graduate programs that could affect the relationship between federal student aid and tuition prices. The first difference is that changes to federal student loan limits and maximum Pell Grants for undergraduate students have been quite modest over time. Undergraduate loan limits increased by between $2,000 and $3,000 per year (with a lifetime increase of $8,000 for dependent students and $11,500 for independent students) between the 2006-07 and 2008-09 academic years—the first increases since 1993 (Wei & Skomsvold, 2012). The maximum Pell Grant increased by $1,500 between the 2006-07 and 2011-12 academic years, but changed little over the preceding and following five-year periods (author's calculations using Federal Student Aid data). The implementation of Grad PLUS, on the other hand, could allow law students to take out $40,000 per year in additional federal loans based on their cost of attendance.

Because the Grad PLUS program allows students to borrow up to the full cost of attendance, IDR provides much larger benefits to graduate students than undergraduate students (who can take out up to $31,000 in loans subject to income-driven repayment if they are dependent and $57,500 if independent). This is particularly true among borrowers who plan to pursue Public Service Loan Forgiveness since most individuals working for government or nonprofit agencies receive modest wages. Delisle and Holt (2014) examined law students' debt and income patterns and showed that a law school graduate with the median income of $59,000

per year would not have monthly payments under PSLF increase once debt exceeded $55,000 per year. Meanwhile, a 75th percentile earner of $95,000 per year would reach the 'zero marginal cost' threshold at $117,000 in debt—still below the median debt in the sample of $140,000.

4. Data, Methods, and Sample

To examine whether law schools' tuition prices or the student debt burdens of graduates was affected by changes to federal student loan policies, I constructed a sixteen-year panel dataset from a number of sources. Details on the specific data sources, analytic methods, and institutions included in the analyses are provided below.

4.1. Data

My primary data source for these analyses is *U.S. News & World Report*'s annual guidebooks containing rankings of graduate and professional programs. Each year, the magazine gathers information from participating institutions across a range of disciplines such as education, business, engineering, medicine, and law to both develop its program-level rankings and to help prospective students choose which program to attend. This is the most comprehensive source of program-level information on law schools over time, as the U.S. Department of Education generally does not publish (or in many cases collect) program-level data for nonprofit institutions and the American Bar Association's publicly-available datasets are not available for as many years and lack some important metrics.

My team of research assistants and I entered and coded a number of data elements from the *U.S. News* guides from 2003 through 2018, reflecting data from the 2001-02 through 2016-17 academic years. The *U.S. News* guides contain two outcomes of interest. The first outcome—and

the one which most directly tests the Bennett Hypothesis—is the listed tuition rates for full-time students. I explored both in-state and out-of-state tuition prices for public law schools, while private institutions charged the same tuition price to all students. In 2005-06 (the last year before Grad PLUS), listed tuition prices were above the previous annual federal loan limit of $18,500 at all but two private law schools (BYU and Howard) and for out-of-state students at 65 of 78 public law schools. However, only eight public law schools charged in-state tuition rates higher than the previous federal loan limit, suggesting that they may have been less affected by the creation of Grad PLUS loans. For a small number of programs that reported per-credit tuition prices instead of overall prices, I multiplied the per-credit price by 30 to reflect a full-time course load that would allow students to graduate in three years.

The second measure is the mean student loan debt graduates incurred to attend law school, which covers about 90% of all law students who take on debt to finance their education. This measure generally aligns with data on student debt from federal sources. The institutional-level average debt in the *U.S. News* data was $111,878 for 2016 graduates, compared to $112,030 for 2015 and 2016 graduates in based on new program-level College Scorecard data and a student-level average of $114,273 for third-year law students in 2015-16 in the National Postsecondary Student Aid Study. However, the College Scorecard excludes private loan debt, while coverage of private debt in the other two sources is uncertain. The *U.S. News* guides also contained a number of other data elements that can be used as covariates in the regressions to help control for other factors that could be affecting law school pricing or debt levels. This is important due to large changes to the landscape of legal education following the Great Recession. The number of first-year law students rose steadily from 43,518 in fall 2000 to a high of 52,488 in fall 2010 before declining sharply to 37,071 in fall 2015 (American Bar Association,

n.d.; Ward, 2016). I used enrollment levels, the percentage of part-time students, the percentage of female and racial/ethnic minority students, acceptance rates, median LSAT scores, and median GPAs from the *U.S. News* guides as covariates. Notably, Asian students are included in the racial/ethnic minority category and I averaged 25th and 75th percentile LSAT and GPA values to estimate the median.

To account for how students may choose law schools based on employment outcomes (a demand-side measure), I included two employment measures from the *U.S. News* guides. The first is the estimated median salary (average of the 25th and 75th percentiles) of graduates employed full-time in the private sector, and the second is the percentage of graduates who were employed in the private sector. These measures are available approximately one year after a student graduates, with the guidebook published in spring 2017 including data collected late in 2016 and early in 2017 on students who graduated in 2015.

Two other data sources provided supplemental information that both acted as a check to the data law schools reported to *U.S. News* and filled in missing data for less than five percent of programs each year (generally less-selective law schools that declined to complete the *U.S. News* survey). The American Bar Association's Official Guide to ABA-Approved Law Schools and their Standard 509 Information website contained information on tuition, living expenses, student demographics, and LSAT/GPA scores for each academic year from 2004-05 forward. These data were used to supplement *U.S. News* data because student debt levels and the earliest years of the panel were unavailable.

The U.S. Department of Education's Integrated Postsecondary Education Data System (IPEDS) contained information on law schools' in-state and out-of-state tuition prices during this

period of study, although data were not available in the 2009-10 academic year and only available for a limited number of colleges in the 2008-09 academic year. These three sources yielded nearly identical values for common data elements (with correlations typically greater than 0.9), likely meaning that law schools provided reasonably accurate data to *U.S. News*.

I also used IPEDS data on three other variables that could affect graduate tuition pricing—the percentage of enrollment that is graduate and professional students, tuition revenue as a percentage of overall revenue, and per-student endowment revenue—to reflect the reliance on graduate enrollment and the institution's overall financial health. For the small percentage of institutions that reported combined financial data with other institutions in their system due to sharing the same Federal Student Aid OPEID (Jaquette & Parra, 2014), I assigned the same per-FTE values to all colleges that share the same OPEID.

As an additional comparison, I examined tuition and student debt changes over time for undergraduate students relative to law students. Data on in-state and out-of-state tuition prices for undergraduate students came from IPEDS, while median student debt burdens of bachelor's degree recipients came from the College Scorecard. This measure averaged two consecutive cohorts (for example, the 2007-08 data included students who graduated in 2007 and 2008). Additional variables from IPEDS for undergraduates included the percentage of female and racial/ethnic minority students.

4.2. Sample

My primary sample consisted of the 85 public and 114 private nonprofit law schools in the 50 states or Washington, DC that were accredited by the American Bar Association and

operated at some point between the 2001-02 and 2015-16 academic years—nearly a census of accredited programs after taking a small percentage of missing data into account. This excluded seven for-profit colleges, only two of which were operating and had available data for at least two years prior to the 2006 introduction of Grad PLUS loans. This restriction also made sense because some of the IPEDS covariates (such as endowment values) were either unavailable or in a different format for for-profit colleges. Summary statistics of the law school dataset by institutional type in the 2014-15 and 2015-16 academic years can be found in Table 1.

[Insert Table 1 here]

For an additional analysis, I used a comparison group consisting of 590 institutions classified as baccalaureate or master's-level institutions that awarded fewer than 100 master's degrees annually in the 2010 Carnegie classifications and that did not have law schools. This is an appropriate comparison group because these institutions could not have used additional funding from Grad PLUS loans to help subsidize undergraduate students in any meaningful way, as could be the case at institutions with large graduate programs.

4.3. Methods

To examine my research questions, I used two different analytic models (each of which was done separately for public and private nonprofit institutions) to examine the extent to which the Bennett Hypothesis holds for law schools. My primary analysis is based on an interrupted time series (ITS) model that tests for whether there is a change in the trends for each of the outcomes after the Grad PLUS program was implemented in 2006 and students could borrow up

to the full cost of attendance. The institutional fixed effects regression equation with institution-clustered standard errors is the following for college *i* in year *t*:

$$Y_{it} = \beta_0 + \beta_1 Time_t + \beta_2 Post_t + \beta_3 (Time * Post)_t + \beta_4 X_{i(t-k)} + \epsilon_{it} + \mu_i, \quad (1)$$

where *Y* represents the (logged) outcome of interest for college *i.* For the tuition measure, covariates from the prior year were used to reflect the institution's characteristics as it sets prices and to reflect the data available to students who read the *U.S. News* guidebook while choosing programs. For student debt, I used covariates from two years prior to allow for graduates to experience one year of law school under the new pricing system and thus have the potential to increase debt burdens. *Time* represents the number of years since the beginning of the panel (2001-02), *Post* is an indicator variable equal to 0 each year before the 2006-07 academic year and 1 after that period, and (*Time*Post*) reflects the number of academic years since July 1, 2006 (with the 2006-07 academic year being equal to 1).[5]

The *X* vector includes three sets of control variables: demographic characteristics from *U.S. News* data (program size, percent full-time, racial/ethnic distribution, and percent female), selectivity measures from *U.S. News* (acceptance rate, undergraduate GPA, and LSAT score), and institutional financial characteristics from IPEDS (graduate student share of overall enrollment, tuition reliance, and per-student endowments). The lags for control variables (represented by *k*) were one year for the tuition outcome to reflect the institution's characteristics as it sets prices and two years for the debt outcomes to allow graduates to experience one year of

[5] I also ran all models using unlogged values and the results are generally similar. For the sake of brevity, they are not presented in the paper but are available upon request from the author.

schools under the new pricing system.[6] This lag also means that the covariates do not suffer from endogeneity issues. Finally, ϵ represents an idiosyncratic error term and μ an institution-specific error term. All financial metrics were inflation-adjusted into 2016 dollars using the Consumer Price Index and logged in order for the results to be interpreted as percent changes.

For one set of analyses, I used the entire length of the dataset (through the 2015-16 academic year). But since first-year student enrollment in law schools declined by 30% between the fall 2010 and fall 2015 semesters (American Bar Association, n.d.; Ward, 2016), it could be possible that changes in the demand for legal education could also affect prices. I addressed this concern in two ways. First, I limited the time period in one set of analyses to end in the 2011-12 academic year (which used 2010-11 enrollment as a covariate). Second, I controlled for student enrollment in my models in an effort to capture any effects of enrollment changes.

Another specification that I ran alongside the baseline interrupted time series model was a panel regression model with year fixed effects instead of the post-2006 indicator. This model allowed for a more careful examination of whether any potential changes in tuition prices and student debt burdens were larger immediately following the implementation of Grad PLUS in 2006, the expansion of IDR policies beginning in 2007, or at some other time.

Because law schools' ability to raise their prices may vary by student demand (with more-selective programs possibly being able to raise tuition by larger amounts if additional federal financial aid is available), I ran models to test for the possibility of differential effects by selectivity. I divided law schools into two comparably-sized groups based on LSAT scores in the

[6] Since tuition prices measured here are for first-year law students, any sort of fixed tuition rates while a student is enrolled are not relevant in this analysis because the price can change for each new cohort of students.

2005-06 academic year. Schools with median LSAT scores at or below 157 in that year were classified as less selective alongside law schools that were not open or did not report LSAT scores in 2005-06, while schools above 157 were classified as being more selective. These models built upon equation (1) by including a dummy variable for selectivity and an interaction term between post-2006 and selectivity (the key term of interest in the model). Because selectivity is time-invariant in this model, I used random effects estimators; I also ran model (1) separately by selectivity group as a robustness check (since it has institutional fixed effects).

As an additional analysis, I used a difference-in-differences (DD) strategy that tested for whether there was a change in tuition prices or debt burdens for law students following the implementation of Grad PLUS loans relative to a comparison group of undergraduate students at institutions that received little or no money from Grad PLUS loans. The random effects regression equation (with institution-clustered standard errors) is the following for college *i* in year *t*:

$$Y_{it} = \theta_0 + \theta_1 Law_i + \theta_2 YrsPost_t + \theta_3 (Law * YrsPost)_{it} + \theta_4 X_{i(t-k)} + \epsilon_{it} + \mu_i, \qquad (2)$$

where *Law* is a dummy variable for law schools (versus undergraduate programs) and *YrsPost* is the number of years since July 1, 2006. Using a random effects model allows for the *Law* coefficient to be shown, but I also ran fixed effects models as a robustness check. The other coefficients are nearly identical, meaning that the choice of random or fixed effects models does not matter in this analysis.

The key variable of interest is *Law*YrsPost*, which represents the differential slope for each year following 2006 between law schools and undergraduate institutions; a positive value

would reflect a sharper increase in law students' prices faced and debt relative to undergraduate students. There were two blocks of control variables in this analysis lagged by *k* years (one year for tuition and two years for debt): demographic measures from IPEDS (percent female and percent minority) and the institutional financial characteristics discussed above. Because debt burdens for undergraduates were only available in the College Scorecard through the 2014-15 academic year at the time of this analysis, the debt models use one fewer year of data than the tuition models.

4.4. Falsification tests

To address concerns regarding pre-treatment trends in both the ITS and DD models, I conducted two different tests. The first test was to visually examine trends in tuition and student debt for both law students and undergraduate students, with the law/undergraduate pairing for each outcome of interest shown as a separate panel of Figure 1. A visual examination shows no sharp jumps in law school tuition prices or debt burdens immediately prior to 2006, which suggests that law schools were unable to anticipate the creation of Grad PLUS. Visually comparing the trends in law student tuition and debt relative to undergraduates yields some concerns that public law school tuition may be increasing more quickly than undergraduate tuition prior to 2006 and that debt of both public and private nonprofit law school graduates may also be increasing at a faster rate than for undergraduates.

[Insert Figure 1 here]

I conducted a falsification test for the ITS model that used 2005 as the beginning of Grad PLUS instead of 2006. The key variable in the falsification test is the post-2005 dummy variable,

which reflects whether there was a jump in tuition prices or debt burdens immediately following 2005. As shown in Appendix A, the post-2005 dummy variable was statistically significant at $p<.10$ for two variables. Public out-of-state tuition was positive for the full model, while public in-state tuition was negative for the truncated model. Given the lack of strong statistical significance and the differing direction of coefficients, it appears that law schools did not respond to Grad PLUS prior to its implementation.

4.5. Limitations

The first limitation of this study is that it is possible that the additional availability of federal student loan dollars affected who ended up enrolling in law schools, as it is plausible that improved access to student loans and/or more generous IDR terms would encourage more students to enter law school (e.g. Crespi, 2014). To check this, I ran regression models using enrollment levels as the outcome of interest instead of pricing levels or student debt. The coefficients (available upon request) show no significant relationship between the implementation of the Grad PLUS program and enrollment levels at public colleges and a negative relationship at private nonprofit colleges. These results taken together suggest that additional students were not induced to enroll based on the availability of Grad PLUS loans, although it still is possible that there were changes in the types of students who enrolled.

A concern regarding the difference-in-differences models is that undergraduate students were affected by some of the same student loan changes as graduate students (the expansion of income-driven repayment programs), while also being affected by different policy changes. Undergraduate loan limits were increased twice following 2006, but by a much smaller amount that what graduate students saw in 2006 (Wei & Skomsvold, 2012). Given some evidence that

certain types of colleges responded to this increase in loan amounts by raising tuition (Lucca et al., 2015), this could mean that ITS coefficients are smaller than DD coefficients due to the modest increases in undergraduate loan limits and because undergraduates were also able to use income-driven repayment programs.

I am unable to separate the creation of the Grad PLUS loan program in 2006 and the expansion of income-driven repayment programs that began in 2007 and continued through the early 2010s. This means that the results can be interpreted as a combination of the increase in loan limits (the traditional Bennett Hypothesis) and the expansion of income-driven repayment programs (which also affect price sensitivity). However, these concerns can be partially overcome by considering both the immediate post-2006 change in pricing strategies and debt alongside the trends following 2006. I am unable to address whether the rate of cross-subsidization across programs changed during the period of my study. It is possible that colleges increased law students' tuition to moderate tuition increases on undergraduates, but that is impossible to discern in my dataset.

Finally, although examining listed tuition prices follows the original Bennett Hypothesis, I could not examine tuition prices after institutional grant aid and therefore could not address issues of price discrimination. Data on the percentage of students receiving grant aid was last collected by *U.S. News* in the 2010-11 academic year and there was a high rate of missing data on that measure in previous years, while the median amount of grant aid received was only among students who received grants. The average tuition discount was 25% at private law schools and 28% at public law schools in 2013-14 after increasing about half a percentage point each year since 2000 (Archer, 2015). The upward trend in discount rates appears to be

continuing in recent years based on a nonrepresentative sample of private law schools (NACUBO & AccessLex Institute, 2017). This is higher than the typical discount rate for undergraduates at public colleges, which was approximately 16% during the 2000s, but is much lower than the estimated 45% discount rate that undergraduates at private nonprofit colleges currently enjoy (Hillman 2012; National Association of College and University Business Officers, 2018).

5. Results

In this section, I discuss the results from two different types of analyses. I first discuss the interrupted time series results comparing trends in law school pricing and debt burdens following the introduction of Grad PLUS loans in 2006 and then discuss the difference-in-difference results comparing law schools and undergraduate institutions over the same time periods.

5.1. Interrupted time series results

I first examined whether the creation of Grad PLUS loans was associated with changes in tuition and fee prices in law schools—the most direct test of the Bennett Hypothesis. The results from the interrupted time series regressions (Table 2) provide relatively little support for the Bennett Hypothesis. At public law schools, the pre-treatment trajectory included large annual tuition and fee increases. However, the post-2006 dummy variable (the immediate treatment effect, represented by a change in level) was not statistically significant from zero for the full length of the panel when including covariates and was negative and significant (3-4 percent) in the shorter panel for both in-state and out-of-state tuition prices. The years*post-2006 dummy variable (the change in slope, representing the treatment effect over time) was approximately -3 percent in both the full and abbreviated panels. This suggests that the rate of increase slowed at

public law schools—running counter to the Bennett Hypothesis. Among private nonprofit law schools, there was modest evidence to support the Bennett Hypothesis in the full panel, with a 1.8 percent jump in tuition and fees after 2006 in the model with all controls. Yet this was not statistically significant in the shorter model that ended before the sharp decline in law school enrollment in the early 2010s, which suggests that other factors may have affected prices during the 2010s.

[Insert Table 2 here]

Turning to student loan debt of graduates as the outcome of interest (Table 3), there was no evidence of a strong relationship between the introduction of Grad PLUS loans and student debt. For both public and private nonprofit law schools, the change in neither level nor slope was consistently statistically significant from zero in the models with control variables. This matches the inconsistent relationships between Grad PLUS implementation and tuition prices, and also suggests that there were no major increases in borrowing for non-tuition portions of the cost of attendance such as living expenses.

[Insert Table 3 here]

As a robustness test (Appendix B), I used year fixed effects to see whether the year coefficients jumped immediately after 2006. Although these coefficients were statistically significant immediately after the creation of Grad PLUS in the tuition and fees regressions, the changes were similar to prior years and thus there did not appear to be a large break in the trendline following 2006. The student debt coefficients were not significantly different from the class of 2006 until 2008 for both public and private law schools.

The final set of ITS analyses were by institutional selectivity, with two different models being included here. I first ran interaction models with a focus on the interaction between the post-2006 dummy variable and more selective law schools (Table 4). Among public law schools, more selective institutions increased their tuition and fees following 2006 at faster rates than less selective institutions (7.7 percent for the full panel and 6.0 percent for the truncated panel). This relationship suggests that law schools with strong student demand were able to more strongly capitalize on their market power for in-state students after Grad PLUS became available. This also suggests the possibility that some students were facing credit constraints, as the full cost of attendance at public law schools was higher than federal student loan limits prior to the creation of Grad PLUS.

However, this difference by selectivity was not present for out-of-state tuition at public law schools or among private nonprofit law schools (where there was a slight negative for the truncated panel). It is worth noting that students attending these institutions would have faced significant credit constraints that would have required private loans or other sources of financing prior to Grad PLUS, meaning that there may have been a substitution away from private loans and toward Grad PLUS. There were no differences by selectivity in the typical debt burden of graduates. Appendix C contains results of the primary model (from Tables 2 and 3) run separately by selectivity category, with the results being fairly consistent across the two groups of public colleges and there being some evidence that graduates of less selective private nonprofit law schools had a larger increase in debt than graduates of more selective programs. This suggests that less selective private law schools, where students may have struggled to gain access to financing on the private market, may have benefited from the creation of the Grad PLUS program.

[Insert Table 4 here]

5.2. Difference-in-differences results

The second analytic strategy was to use difference-in-differences models to compare law schools and undergraduate institutions before and after the 2006 creation of Grad PLUS loans. Turning first to tuition and fees (Table 5), public law schools raised in-state tuition prices between 2.5 and 3.8 percent faster per year following 2006 than other colleges raised in-state prices for undergraduate students. Out-of-state tuition also increased at a higher rate for law students following 2006, with the annual increase being between approximately two points larger than for undergraduate students. Part of this differential may be attributable to the Bennett Hypothesis, while another part could be due to limits that some states or university systems put on undergraduate tuition and/or fee increases (e.g., Armstrong et al., 2017). These findings are different than the interrupted time series results, which suggest a null or negative relationship between the creation of Grad PLUS and public law schools' tuition and fee prices.

[Insert Table 5 here]

Private law schools actually had their tuition increase by 0.4 percent less per year than the rate of change for undergraduate students over the full time period, yet the coefficient was insignificant when restricting analysis prior to the decline in law school enrollment. Overall, this suggests that Grad PLUS loans do not appear to be driving tuition increases at private law schools over time.

Finally, Table 6 contains the results of difference-in-differences results with student debt of graduates as the outcome of interest. The models with covariates do not show that student debt for public law school graduates increased faster than for bachelor's degree recipients. However,

the private models are significant, with the full panel having a negative 0.2 percent coefficient and the truncated panel having a positive 1.2 percent coefficient.

[Insert Table 6 here]

6. Discussion and Future Work

Ever since William Bennett hypothesized that increased federal student loan availability would result in higher tuition prices (Bennett, 1987), the so-called Bennett Hypothesis has been hotly debated by researchers and policymakers alike. Yet most studies either focus on federal grant programs or try to isolate effects based on incremental increases to federal borrowing limits. In this study, I provide the a quasi-experimental analysis of the creation of the federal Grad PLUS loan program in 2006—which represented an increase in federal loan eligibility of tens of thousands of dollars per year for many law students.

My analyses suggest that the large increase in federal borrowing limits did not induce law schools to substantially increase tuition and fees or student debt burdens. However, the difference-in-differences models do suggest that any responses that do exist relative to undergraduate institutions may be concentrated among public law schools instead of private nonprofit programs. This runs counter to prior literature at the undergraduate level (Lau, 2014; Lucca et al., 2015), but could be explained by the lack of tuition and fee controls in professional education that often exist for bachelor's degree programs at public colleges and universities.

I offer three potential factors that could explain the relatively modest relationship between law school prices and the availability and attractiveness of federal student loans for students. The first potential explanation is that law schools as a whole did not actively attempt to engage in additional rent-seeking behavior by increasing their prices in an effort to gain

additional tuition revenue. Some programs have responded to particular incentives under Public Service Loan Forgiveness to shift the incidence from students to taxpayers (such as the Georgetown Law example discussed earlier), but there appear to be few institutions that sharply increased tuition rates above and beyond what they were doing prior to 2006. It is also worth noting that my sample excluded the small number of for-profit law schools, where research based on for-profit undergraduate institutions suggests stronger support for the Bennett Hypothesis (e.g., Lucca et al., 2015).

The second potential explanation is that since law schools are an example of a higher education marketplace in which prospective students have reasonably good information about individual programs, a program that increases tuition more than its competitors may see a decline in enrollment—and potentially overall tuition revenue. If all law schools raised tuition by a similar amount, this concern could be lessened. But law schools may have become more concerned about pricing and student debt in the aftermath of the Great Recession, even before enrollment fell precipitously.

Finally, it is possible that the number of law students who faced credit constraints prior to the adoption of the Grad PLUS program could have been relatively low due to the presence of a robust private loan market during the 2000s. There is evidence that the adoption of Grad PLUS, combined with improved income-driven repayment terms, induced students to switch from private to federal loans during this period (Bhole, 2017). It is likely that Grad PLUS loans extended access to capital to at least some students, but a sizable percentage of students could already access loans up to the full cost of attendance.

Grad PLUS loans and income-driven repayment plans have generated a great deal of discussions in policy circles in recent years due to their implications on the federal budget. Grad PLUS loans are currently counted as generating profits for the federal government, although the growth of income-driven repayment programs may change that assumption going forward (Congressional Budget Office, 2017; U.S. Government Accountability Office, 2016). The results of this study suggest that policymakers may want to be less concerned about colleges using the availability of Grad PLUS loans as a way to fill institutional coffers, but more research needs to be done to confirm these findings in other professional programs. Notably, research should be done using programs in which fewer students would be interested in pursuing Public Service Loan Forgiveness to see whether the presence of PSLF may have affected the relationships present among law students.

Finally, more research should be done to explore whether the availability of Grad PLUS loans and income-driven repayment programs changed the types of students who enrolled in law schools (even though these programs did not appear to affect total enrollment numbers) or their post-graduation outcomes. If Grad PLUS loans reduced credit constraints for certain groups of students who may not have been able to get private loans, then the racial, gender, and/or socioeconomic makeups of law schools' applicant pools (and potentially their student bodies) may have also changed as a result. The availability of income-driven repayment programs for all federal loans could have also induced more students to pursue lower-paying work in public service fields, yet there has been no research to this point examining whether that has actually happened.

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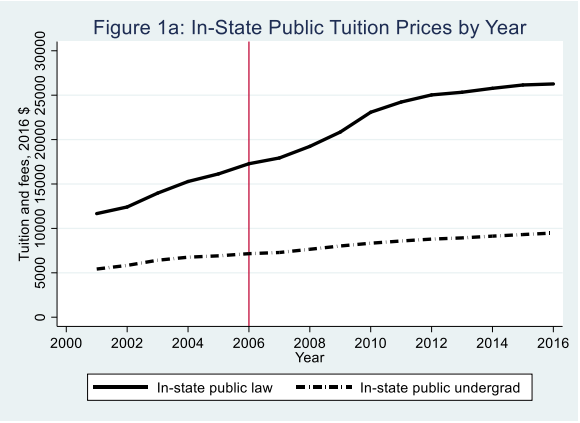
Figure 1a: In-State Public Tuition Prices by Year
Tuition and fees, 2016 $
0 5000 10000 15000 20000 25000 30000
2000 2002 2004 2006 2008 2010 2012 2014 2016
Year
In-state public law
In-state public undergrad

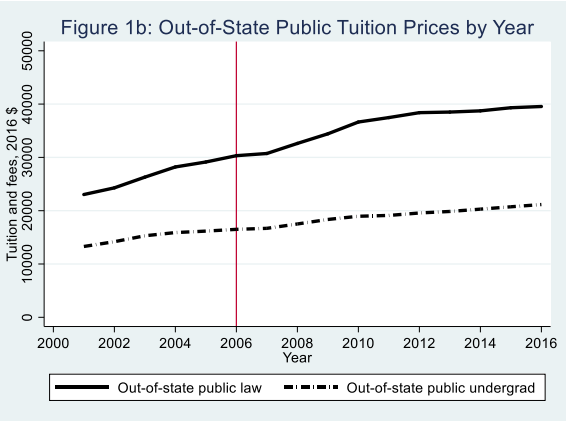
Figure 1b: Out-of-State Public Tuition Prices by Year
Tuition and fees, 2016 $
0 10000 20000 30000 40000 50000
2000 2002 2004 2006 2008 2010 2012 2014 2016
Year
Out-of-state public law
Out-of-state public undergrad

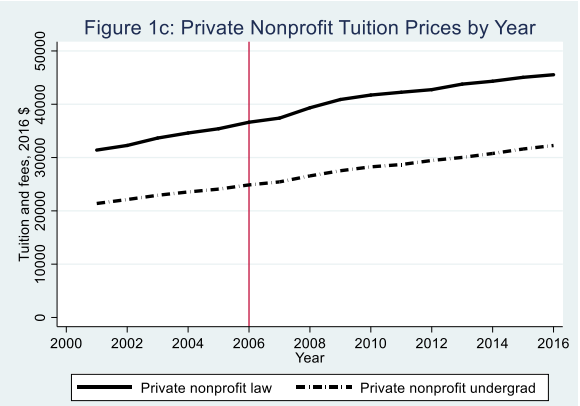
Figure 1c: Private Nonprofit Tuition Prices by Year
Tuition and fees, 2016 $
0 10000 20000 30000 40000 50000
2000 2002 2004 2006 2008 2010 2012 2014 2016
Year
Private nonprofit law
Private nonprofit undergrad

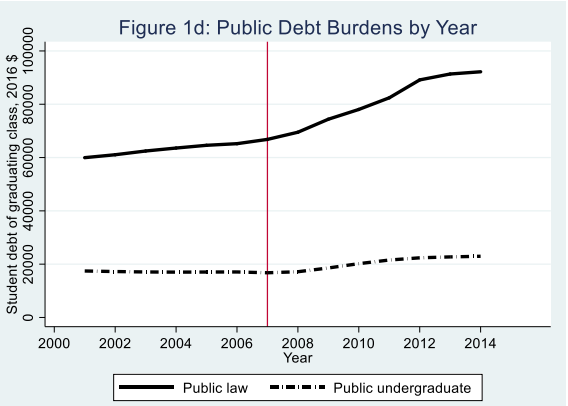
Figure 1d: Public Debt Burdens by Year
Student debt of graduating class, 2016 $
0 20000 40000 60000 80000 100000
2000 2002 2004 2006 2008 2010 2012 2014
Year
Public law
Public undergraduate

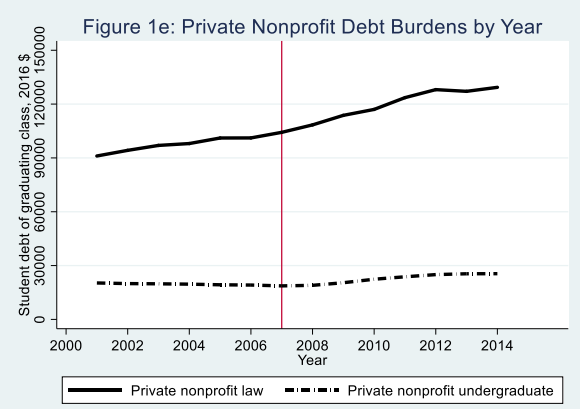
Figure 1e: Private Nonprofit Debt Burdens by Year
Student debt of graduating class, 2016 $
0 30000 60000 90000 120000 150000
2000 2002 2004 2006 2008 2010 2012 2014
Year
Private nonprofit law
Private nonprofit undergraduate

Table 1: Summary statistics of the law school dataset.

	Public		Private nonprofit	
Characteristic	Mean	(SD)	Mean	(SD)
Outcomes of interest (2015-16 academic year, 2016$)				
In-state tuition and fees	24,044	(10,343)	45,049	(8,827)
Out-of-state tuition and fees	39,348	(8,538)	--	--
Debt of 2015 graduates	92,967	(22,569)	126,103	(27,042)
Law school demographic controls (2014-15 academic year)				
Total enrollment	498	(207)	644	(354)
Percent part-time students	9.2	(13.2)	12.9	(14.5)
Percent female students	45.5	(5.6)	49.3	(5.0)
Percent minority students	25.5	(15.5)	27.0	(12.8)
Demand-side measures (2015 graduates)				
Median full-time private sector salary	80,992	(26,873)	87,028	(32,653)
Percent employed in the private sector	47.6	(9.3)	51.8	(10.4)
Law school selectivity controls (2014-15 academic year)				
Percent of students admitted	49.0	(14.3)	54.9	(18.3)
Median LSAT	155.9	(5.1)	155.1	(6.8)
Median GPA	3.40	(0.19)	3.33	(0.23)
Institutional control variables (2014-15 academic year)				
Percent of revenue from tuition	27.3	(12.3)	58.5	(24.2)
Percent of enrollment as grad students	30.2	(21.5)	49.1	(25.3)
Per-student endowment (in 2016$)	33,412	(44,589)	135,287	(291,821)
Maximum number of law schools	85		113	

Sources: U.S. News and American Bar Association data (outcomes and law school control variables), Integrated Postsecondary Education Data System (institutional control variables).

Table 2: Interrupted time series results for law schools' tuition prices.

Public law schools	In-state tuition and fees				Out-of-state tuition and fees			
	Full dataset		Through 2011-12		Full dataset		Through 2011-12	
Variable	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Years since start of panel	0.089***	0.080***	0.090***	0.088***		0.064***	0.057***	0.064***
(pre-treatment trajectory)	(0.005)	(0.010)	(0.005)	(0.010)		(0.004)	(0.007)	(0.004)
Post-2006 dummy	0.024**	0.010	-0.041***	-0.038***		0.011	0.011	-0.037***
(change in level--immediate treatment effect)	(0.011)	(0.011)	(0.011)	(0.012)		(0.011)	(0.012)	(0.008)
Years*post-2006 dummy	-0.042***	-0.031***	-0.022***	-0.026**		-0.032***	-0.021***	-0.017***
(change in slope--treatment effect over time)	(0.006)	(0.011)	(0.005)	(0.012)		(0.005)	(0.007)	(0.005)
Includes control variables?		X		X			X	
Number of law schools	84	77	83	72		84	77	83
Adjusted R-squared	0.315	0.465	0.250	0.325		0.326	0.475	0.261

Private nonprofit law schools	Tuition and fees			
	Full dataset		Through 2011-12	
Variable	(1)	(2)	(3)	(4)
Years since start of panel	0.032***	0.031***	0.032***	0.036***
(pre-treatment trajectory)	(0.002)	(0.004)	(0.002)	(0.004)
Post-2006 dummy	0.028***	0.018***	0.000	0.002
(change in level--immediate treatment effect)	(0.005)	(0.006)	(0.004)	(0.005)
Years*post-2006 dummy	-0.009	-0.007	0.000	-0.006
(change in slope--treatment effect over time)	(0.002)	(0.005)	(0.002)	(0.004)
Includes control variables?		X		X
Number of law schools	113	112	112	108
Adjusted R-squared	0.232	0.259	0.180	0.144

Sources: See Table 1.

Notes:

(1) * represents p<.10, ** represents p<.05, and *** represents p<.01.

(2) All financial variables have been logged and inflation-adjusted into 2016 dollars using the Consumer Price Index.

(3) Regressions have a one-year lag between control variables and outcomes metrics and also include institutional fixed effects.

Table 3: Interrupted time series results for law school graduates' debt burdens.

	Public law schools				Private nonprofit law schools			
	Full dataset		Through 2011-12		Full dataset		Through 2011-12	
Variable	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Years since start of panel	0.022***	0.058*	0.024***	0.033***	0.026***	0.009	0.026***	0.028***
(pre-treatment trajectory)	(0.008)	(0.030)	(0.007)	(0.008)	(0.006)	(0.014)	(0.006)	(0.007)
Post-2006 dummy	-0.043	-0.038	-0.050*	-0.022	-0.010	0.013	-0.060**	-0.029
(change in level--immediate treatment effect)	(0.027)	(0.024)	(0.029)	(0.030)	(0.016)	(0.017)	(0.024)	(0.022)
Years*post-2006 dummy	0.023**	-0.017	0.023*	0.001	0.004	0.018	0.020**	0.009
(change in slope--treatment effect over time)	(0.009)	(0.031)	(0.013)	(0.015)	(0.007)	(0.014)	(0.009)	(0.010)
Includes control variables?		X		X		X		X
Number of law schools	84	76	80	74	112	109	109	104
Adjusted R-squared	0.218	0.177	0.075	0.159	0.197	0.139	0.127	0.147

Sources: See Table 1.

Notes:

(1) * represents p<.10, ** represents p<.05, and *** represents p<.01.

(2) All financial variables have been logged and inflation-adjusted into 2016 dollars using the Consumer Price Index.

(3) Because of how data are reported to *U.S. News*, there is one more pre-treatment year (the same year of data includes debt for 2006 graduates [pre] and tuition prices in 2006-07 [post]).

(4) Regressions have a two-year lag between control variables and outcomes metrics and also include institutional fixed effects.

Table 4: Interrupted time series results by institutional selectivity.

Public law schools	In-state tuition		Out-of-state tuition		Debt of graduates	
Variable	Full	Through 2011-12	Full	Through 2011-12	Full	Through 2011-12
Years since start of panel	0.079***	0.087***	0.057***	0.062***	0.060***	0.072***
	(0.007)	(0.008)	(0.006)	(0.006)	(0.019)	(0.022)
Post-2006 dummy	-0.036**	-0.065***	0.006	-0.029*	-0.057*	-0.058
	(0.017)	(0.019)	(0.016)	(0.016)	(0.033)	(0.040)
Years*post-2006 dummy	-0.030***	-0.028***	-0.021***	-0.019***	-0.020	-0.041*
	(0.008)	(0.009)	(0.007)	(0.007)	(0.020)	(0.024)
More selective law school	0.167***	0.213***	0.148***	0.202***	0.053	0.059
	(0.051)	(0.052)	(0.036)	(0.042)	(0.047)	(0.057)
Post-2006 dummy*more selective	0.077***	0.060***	0.007	0.010	0.034	0.048
	(0.017)	(0.018)	(0.015)	(0.014)	(0.032)	(0.037)
Number of law schools	77	72	77	72	76	72
Adjusted R-squared	0.576	0.530	0.601	0.534	0.422	0.316

Private nonprofit law schools	Tuition and fees		Debt of graduates	
Variable	Full	Through 2011-12	Full	Through 2011-12
Years since start of panel	0.030***	0.033***	0.013	0.016
	(0.003)	(0.003)	(0.013)	(0.013)
Post-2006 dummy	0.022***	0.011	0.022	-0.022
	(0.007)	(0.007)	(0.022)	(0.026)
Years*post-2006 dummy	-0.006*	-0.004	0.015	0.022
	(0.003)	(0.004)	(0.014)	(0.015)
More selective law school	0.116***	0.164***	0.018	0.031
	(0.032)	(0.035)	(0.038)	(0.042)
Post-2006 dummy*more selective	-0.010	-0.017**	-0.016	-0.015
	(0.007)	(0.007)	(0.022)	(0.024)
Number of law schools	109	108	109	104
Adjusted R-squared	0.452	0.361	0.284	0.233

Sources: See Table 1.

Notes:

(1) * represents p<.10, ** represents p<.05, and *** represents p<.01.

(2) All financial variables have been logged and inflation-adjusted into 2016 dollars using the Consumer Price Index.

(3) More selective law schools had a median LSAT above 157 in 2005-06 (the median value that year. Schools that were not open in 2005 are classified as less selective.

(4) All models include all control variables shown in Table 1, with a one-year lag for tuition outcomes and a two-year lag for debt outcomes. Results with fewer controls are substantively similar and available upon request.

Table 5: Difference-in-differences results for tuition prices at law schools versus undergraduate institutions.

Public institutions	In-state tuition and fees				Out-of-state tuition and fees			
	Full dataset		Through 2011-12		Full dataset		Through 2011-12	
Variable	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Law school vs. undergraduate institution	0.897***	0.791***	0.938***	0.917***	0.771***	0.695***	0.716***	0.705***
	(0.058)	(0.073)	(0.041)	(0.039)	(0.049)	(0.068)	(0.032)	(0.035)
Number of years post-2006	0.023***	0.025***	0.037***	0.038***	0.016***	0.018***	0.022***	0.022***
	(0.002)	(0.002)	(0.001)	(0.001)	(0.002)	(0.003)	(0.002)	(0.002)
Law school*number of years post-2006	0.025***	0.024***	0.034***	0.029***	0.015***	0.015***	0.025***	0.024***
	(0.003)	(0.003)	(0.002)	(0.002)	(0.003)	(0.003)	(0.002)	(0.002)
Includes control variables?		X		X		X		X
Number of institutions	209	201	426	415	209	201	426	415
Adjusted R-squared	0.647	0.651	0.592	0.599	0.605	0.621	0.531	0.543

Private nonprofit institutions	Tuition and fees			
	Full dataset		Through 2011-12	
Variable	(1)	(2)	(3)	(4)
Law school vs. undergraduate institution	0.505***	0.524***	0.497***	0.503***
	(0.030)	(0.043)	(0.039)	(0.035)
Number of years post-2006	0.027***	0.030***	0.030***	0.030***
	(0.001)	(0.001)	(0.001)	(0.001)
Law school*number of years post-2006	-0.003**	-0.004***	0.002	0.001
	(0.001)	(0.001)	(0.001)	(0.001)
Includes control variables?		X		X
Number of institutions	576	568	867	852
Adjusted R-squared	0.249	0.292	0.203	0.210

Sources: See Table 1.

Notes:

(1) * represents p<.10, ** represents p<.05, and *** represents p<.01.

(2) All financial variables have been logged and inflation-adjusted into 2016 dollars using the Consumer Price Index.

(3) Regressions have a one-year lag between control variables and outcomes metrics and also include year fixed effects.

Table 6: Difference-in-differences results for student debt burdens at law schools versus undergraduate institutions.

	Public				Private nonprofit			
	Full dataset		Through 2011-12		Full dataset		Through 2011-12	
Variable	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Law school vs. undergraduate institution	1.361***	1.391***	1.347***	1.373***	1.600***	1.643***	1.588***	1.589***
	(0.030)	(0.032)	(0.032)	(0.031)	(0.019)	(0.022)	(0.020)	(0.021)
Number of years post-2006	0.025***	0.026***	0.040***	0.041***	0.017***	0.017***	0.036***	0.035***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)
Law school*number of years post-2006	0.005***	0.001	0.005**	0.004	0.003**	-0.002**	0.014***	0.012***
	(0.002)	(0.002)	(0.003)	(0.003)	(0.001)	(0.001)	(0.002)	(0.002)
Includes control variables?		X		X		X		X
Number of institutions	426	417	422	415	861	850	854	846
Adjusted R-squared	0.815	0.842	0.776	0.790	0.873	0.892	0.853	0.860

Sources: See Table 1.

Notes:

(1) * represents p<.10, ** represents p<.05, and *** represents p<.01.

(2) All financial variables have been logged and inflation-adjusted into 2016 dollars using the Consumer Price Index.

(3) Regressions have a two-year lag between control variables and outcomes metrics and also include year fixed effects.

(4) Because of how data are reported to *U.S. News*, there is one more pre-treatment year (the same year of data includes debt for 2006 graduates [pre] and tuition prices in 2006-07 [post]).

(5) All debt burdens are for graduates only.

Appendix A: ITS falsification tests using 2005 as adoption of Grad PLUS (instead of 2006).

Panel A: Full panel.	Public law schools			Private nonprofits	
Variable	In-state tuition	Out-of-state tuition	Debt of graduates	Tuition and fees	Debt of graduates
Years since start of panel	0.095***	0.064***	0.055	0.035***	0.002
	(0.015)	(0.010)	(0.056)	(0.005)	(0.014)
Post-2005 dummy	0.014	0.017*	0.000	0.009	-0.007
	(0.011)	(0.010)	(0.037)	(0.006)	(0.019)
Years*post-2005 dummy	-0.046***	-0.028**	-0.015	-0.010*	0.025*
	(0.016)	(0.011)	(0.057)	(0.006)	(0.014)
Number of law schools	77	77	76	109	109
Adjusted R-squared	0.468	0.479	0.173	0.394	0.140
Panel B: Through 2011-12 only.	Public law schools			Private nonprofits	
Variable	In-state tuition	Out-of-state tuition	Debt of graduates	Tuition and fees	Debt of graduates
Years since start of panel	0.111***	0.078***	0.080	0.043***	0.023
	(0.016)	(0.010)	(0.055)	(0.005)	(0.018)
Post-2005 dummy	-0.025*	-0.016	0.024	-0.005	-0.018
	(0.013)	(0.011)	(0.045)	(0.005)	(0.023)
Years*post-2005 dummy	-0.052***	-0.034***	-0.049	-0.012**	0.010
	(0.017)	(0.011)	(0.058)	(0.005)	(0.020)
Number of law schools	72	72	72	108	104
Adjusted R-squared	0.310	0.268	0.081	0.124	0.052

Sources: See Table 1.

Notes:

(1) * represents p<.10, ** represents p<.05, and *** represents p<.01.

(2) All financial variables have been logged and inflation-adjusted into 2016 dollars using the Consumer Price Index.

(3) Because of how data are reported to *U.S. News*, there is one more pre-treatment year for debt (the same year of data includes debt for 2005 graduates [pre] and tuition prices in 2005-06 [post in the falsification test]).

(4) All models include all control variables and institutional fixed effects. Models with fewer control variables yield substantively similar results and are available upon request.

Appendix B: Coefficients from year fixed effects for interrupted time series models.

	In-state tuition and fees (public)		Out-of-state tuition and fees (public)		Tuition and fees (private)	
Year	Coeff.	(SE)	Coeff.	(SE)	Coeff.	(SE)
2003	-0.136***	(0.018)	-0.109***	(0.015)	-0.056***	(0.008)
2004	-0.051***	(0.008)	-0.041***	(0.008)	-0.022***	(0.005)
2006	0.054***	(0.007)	0.042***	(0.006)	0.036***	(0.004)
2007	0.083***	(0.014)	0.057***	(0.013)	0.051***	(0.009)
2008	0.156***	(0.014)	0.117***	(0.013)	0.102***	(0.009)
2009	0.226***	(0.018)	0.163***	(0.015)	0.140***	(0.009)
2010	0.297***	(0.016)	0.223***	(0.023)	0.163***	(0.010)
2011	0.334***	(0.021)	0.242***	(0.018)	0.174***	(0.010)
Number of law schools	77		77		109	
Adjusted R-squared	0.371		0.384		0.289	

	Student debt (public)		Student debt (private)	
Year	Coeff.	(SE)	Coeff.	(SE)
2003	-0.158**	(0.065)	-0.079**	(0.035)
2004	-0.082*	(0.041)	-0.052*	(0.027)
2005	-0.009	(0.024)	-0.023	(0.022)
2007	0.029	(0.026)	0.006	(0.035)
2008	0.045*	(0.026)	0.068***	(0.021)
2009	0.066*	(0.041)	0.122***	(0.022)
2010	0.144**	(0.054)	0.149***	(0.027)
2011	0.194***	(0.034)	0.200***	(0.027)
Number of law schools	76		109	
Adjusted R-squared	0.085		0.048	

Sources: See Table 1.

Notes:

(1) * represents p<.10, ** represents p<.05, and *** represents p<.01.

(2) All financial variables have been logged and inflation-adjusted into 2016 dollars using the Consumer Price Index.

(3) Regressions have a one-year lag between the measurement of control variables (as shown in Table 1) and outcomes metrics (two years for debt outcomes) and also include institutional fixed effects.

(4) The reference year (not shown) is 2005 for tuition and fees and 2006 for student debt.

(5) All years from 2001 to 2015 are included in the model, but only years closer to 2006 are shown in this table.

Appendix C: Interrupted time series results by institutional selectivity.

	Public law schools						Private nonprofit law schools			
Full panel	In-state tuition		Out-of-state tuition		Debt of graduates		Tuition and fees		Debt of graduates	
Variable	Less selective	More selective	Less selective	More selective	Less selective	More selective	Less selective	More selective	Less selective	More selective
	0.081***	0.076***	0.060***	0.057***	0.052	0.059***	0.037***	0.029***	-0.013	0.025*
Years since start of panel	(0.011)	(0.009)	(0.011)	(0.008)	(0.040)	(0.017)	(0.005)	(0.004)	(0.013)	(0.014)
Post-2006 dummy	0.018	-0.004	0.008	0.010	-0.094*	0.006	0.024**	0.017**	0.008	-0.010
	(0.020)	(0.019)	(0.021)	(0.016)	(0.054)	(0.024)	(0.009)	(0.008)	(0.034)	(0.020)
	-0.043***	-0.023**	-0.024*	-0.024***	-0.011	-0.015	-0.017***	-0.001	0.043*	0.003
Years*post-2006 dummy	(0.012)	(0.010)	(0.013)	(0.008)	(0.043)	(0.018)	(0.006)	(0.004)	(0.025)	(0.014)
Number of law schools	36	41	36	41	36	40	60	49	60	49
Adjusted R-squared	0.339	0.609	0.306	0.416	0.076	0.268	0.434	0.181	0.045	0.067
	Public law schools						Private nonprofit law schools			
Through 2011-12 only	In-state tuition		Out-of-state tuition		Debt of graduates		Tuition and fees		Debt of graduates	
Variable	Less selective	More selective	Less selective	More selective	Less selective	More selective	Less selective	More selective	Less selective	More selective
	0.085***	0.087***	0.072***	0.064***	0.050	0.082***	0.041***	0.033***	0.007	0.042**
Years since start of panel	(0.012)	(0.009)	(0.011)	(0.007)	(0.053)	(0.019)	(0.006)	(0.004)	(0.025)	(0.016)
Post-2006 dummy	-0.024	-0.050**	-0.016	-0.042**	-0.105	0.006	0.000	0.006	-0.034	-0.052**
	(0.013)	(0.023)	(0.021)	(0.018)	(0.070)	(0.031)	(0.010)	(0.008)	(0.029)	(0.024)
	-0.033**	-0.023**	-0.030**	-0.019**	-0.013	-0.046**	-0.011*	-0.003	0.032	0.000
Years*post-2006 dummy	(0.014)	(0.011)	(0.013)	(0.008)	(0.059)	(0.021)	(0.006)	(0.004)	(0.029)	(0.017)
Number of law schools	33	39	33	39	33	39	59	49	55	49
Adjusted R-squared	0.289	0.439	0.272	0.162	0.007	0.171	0.270	0.090	0.009	0.002

Sources: See Table 1.

Notes:

(1) * represents p<.10, ** represents p<.05, and *** represents p<.01.

(2) All financial variables have been logged and inflation-adjusted into 2016 dollars using the Consumer Price Index.

(3) More selective law schools had a median LSAT above 157 in 2005-06 (the median that year). Schools that were not open in 2005 are considered less selective.

(4) All models include all controls and institutional fixed effects. Models with fewer controls yield substantively similar results and are available upon request.