Managers, Markets, and Money: Essays on the Economics of Higher Education

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Abstract

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This dissertation explores the organizational and market mechanisms that influence student success in higher education, beyond student access to financial and support resources. This work finds that factors such as leadership compensation, quality and characteristics, the structure of outside labor markets, competitive pressures, and the structure of financial aid mediate student outcomes. These results suggest that a successful higher education policy needs to take into account how institutional governance, market forces interact with existing student support policies to ensure that public and institutional incentives are aligned in ensuring an efficient use of resources.

The first chapter focuses on how college presidents' compensation responds to taxation, and how their behavior changes in turn. I use a provision of the 2017 Tax Cuts and Jobs Act, which introduced a 21% excise tax on nonprofit compensation above a $1 million threshold, which applied to private nonprofit college presidents and increased the cost to institutions of their total compensation. I use an event study design to estimate the effect of this tax on presidential compensation, finding that presidential compensation fell by 26% and their probability of exit increased by 19%. Presidents adapted their behavior in response to this drop in compensation by prioritizing easily observable financial outcomes instead of student expenditure and success, increasing net income by 30% and investment returns by 23%, while the most taxed presidents decreased student net financial aid by 0.56% per additional 1% of their tax burden. The impact of the TCJA also varies by president demographics: female presidents' compensation decreases were twice as large as those

found for their male counterparts. I also find that the returns to experience are substantial, as presidents coming from the private sector or government underperform their counterparts with higher education experience.

The second chapter, joint with Stephanie Riegg Cellini and Kathryn Blanchard, explores institutional responses to changes in the labor market faced by their graduates in a particular context. In the United States, licenses are required for entry into many different occupations. Requirements vary by state and occupation, but many licenses require a minimum number of training or instructional hours. We consider the impact of these hours requirements on students and postsecondary institutions, with a particular focus on cosmetology (also known as hairstyling or beauty), the field that requires the highest number of training hours in the largest number of states. We implement a difference-in-difference design based on state-level changes in licensing hours for cosmetologists between 2011 and 2019. We ask how and whether changes to hours requirements influence student outcomes and institutional behavior. We find that lowering required hours is likely beneficial for students as it increases completion, lowers tuition, and expands enrollment among some groups of students. Larger institutions appear to reduce their tuition by less than smaller institutions. We find no detectable effects on student debt or cosmetologist earnings.

The third chapter explores the effects and limits of emergency financial aid based on direct transfers to students. In this paper, I evaluate the impacts of an emergency financial aid program on short-term academic outcomes in the context of the Covid-19 pandemic. Using administrative data from a large public college system, I estimate a regression discontinuity model leveraging the assignment rule for additional funding based on expected economic need to evaluate the effects of marginal increases in aid. The regression discontinuity model finds no significant academic impacts of marginal increases in grant aid, consistent with similar results in the literature.

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1 Talent at the top: effects of taxation on college presidents' compensation and performance

1.1 Introduction

The responses of managers and organizations to monetary incentives are a central question across fields of economics, and one with particular relevance in education. A growing body of evidence underscores the role of effective leadership in improving institutional performance: for example, principal quality is positively associated with student achievement in primary and secondary schools in the U.S. (Bartanen, Grissom, and Rogers 2019, Coelli and Green 2012). In higher education, where institutions are disproportionately larger and more complex—averaging 162 instructors and 396 staff members (IPEDS, 2022)—quality governance is even more important. If colleges were classified as firms, their scale would place them in the top 0.5% of U.S. businesses by size, highlighting both the complexity of their administration and the importance of incentives for individuals at the top of these organizations. Moreover, support programs generally regarded as effective in fostering student success, such as academic advising and career support, depend on a competent administration to function effectively.

This paper explores two main questions. First, how do institutions respond to increases in the cost of governance? Second, how does the compensation of college presidents affect institutional performance? A natural experiment for the first would randomly vary the cost of executive salaries across colleges, while the second would manipulate president compensation directly and observe downstream outcomes. I leverage a quasi-experimental setting that approximates both, using the implementation of the 2017 Tax Cuts and Jobs Act (TCJA) as a source of plausibly exogenous variation. The TCJA introduced a 21% excise tax levied on nonprofit institutions for any compensation above $1 million paid to a covered employee, effectively increasing the cost of retaining highly paid college presidents. This tax is paid by the institution based on the total compensation of the employee, regardless of how this compensation is distributed between base salaries, bonuses, non-taxable compensation (for the employee) and deferred compensation. This setting provides an opportunity to examine how colleges respond to higher governance costs and how changes in presidential compensation affect institutional behavior.

To answer these questions, I use executive compensation data from IRS 990 filings and construct a novel dataset on presidents' educational and professional backgrounds. I implement an event study design that compares outcomes across institutions and presidents exposed and unexposed to the tax, including president fixed effects to isolate within-individual changes over time. I find that the TCJA led to a significant reduction in presidential compensation—on average, 26 percent —implying a tax pass-through rate of 123%, implying potential institutional overcorrections or increased institutional bargaining power. Compensation reductions were concentrated in non-taxable and "other" forms of pay, suggesting a strategic shift in how institutions structure executive compensation in response to tax policy. Furthermore, these results do not appear to be driven by composition bias driven by new, lower-paid presidents, as a within-president event study design finds similar responses. Among the highest-paid presidents, a 1% increase in distance to the tax threshold is associated with a 0.59% reduction in total compensation and a 1.39% reduction in other compensation.

Presidents appear to adapt their behavior following these changes. First, the tax led to a 19 percent increase in the probability of presidential exit, particularly among those with the highest compensation levels. Second, those who remained shifted their focus toward easily measurable financial outcomes. After the TCJA's implementation, treated institutions increased net income by 30 percent, improved investment returns by 23 percent, and allocated 9 percent more of their budgets to academic support. Although counterintuitive, this behavioral response is consistent with the structure of presidential contracts, which often lack clear short-term performance clauses but are renegotiated every 3 to 5 years, on average. These financial outcomes are readily legible to boards, suggesting that presidents may have responded to compensation cuts by reorienting their efforts toward performance indicators that could strengthen their future bargaining positions.

Lastly, I find that the effects of the TCJA vary by president characteristics. Female presidents experienced nearly twice the reduction in compensation compared to their male counterparts (40% vs. 23%), suggesting unequal exposure to cuts or differences in contract structures. Presidents without a PhD or with prior experience outside academia also underperform relative to peers with stronger academic credentials, suggesting high returns to sector-specific experience. These findings underscore the role of leadership characteristics in shaping institutional responses to policy shocks and suggest the value of more deliberate approaches to presidential selection and compensation design.

This paper contributes to several strands of related literature. In first place, this paper is one of the first to analyze the effects of management quality and incentives in higher education, as part of a broader literature on the effects of management in educational outcomes. This paper is tied both a broad literature finding mixed outcomes of financial incentives in education (Berlinski and Ramos 2020, Biasi 2021, Cowan and Goldhaber 2018, Goodman and Turner 2013, Mbiti et al. 2019, Steele, Murnane, and Willett 2010), a literature that finds that leadership quality has positive effects on educational outcomes (Bartanen, Grissom, and Rogers 2019, Branch, Hanushek, and Rivkin 2012, Coelli and Green 2012, Grissom and Bartanen 2019, Miller 2013, Muñoz and Prem, forthcoming), and a literature that finds mixed effects of interventions aimed at improving managerial quality in education, suggesting substantial sorting and returns to experience (Asim et al. 2024, Beg, Fitzpatrick, and Lucas 2023, Fryer et al. 2017, Muralidharan and Singh 2020, Romero et al. 2022). This paper ties these strands together, by extending their results to higher education, a setting with larger institutions where leaders face higher-powered incentives. Furthermore, this paper extends a strand of research on the effects of top managers in government settings (Fenizia 2022, Munoz and Otero 2022, Limodio 2021), by extending their research questions to a space where large private institutions coexist with public institutions in carrying out public functions.

Also, this paper is one of the first to provide causal evidence in a broad literature that studies the correlates of the compensation of college presidents. While there is a broad range of correlates of presidential compensation (Bai 2014, Bartlett and Sorokina 2005, Civera et al. 2024, Cheng 2014, He and Callahan III 2017, Huang and Chen 2013, Hunt, Tandberg, and Park 2019, Monks 2007), these correlates are vulnerable to the critique raised for similar studies in the private sector by Edmans, Gabaix, and Jenter (2017). As compensation arrangements are endogenous, resulting from the interaction between executives, boards, compensation consultants, and the labor market, they are inevitably correlated with observable and unobservable firm, industry, and executive characteristics. This makes it impossible to interpret any observed correlation between executive pay and firm outcomes as a causal relationship. To address this weakness requires a source of variation affecting executive compensation, which this paper is the first to use in a higher education context.

Finally, this paper contributes to the multiple evaluations of the effects of the Tax Cuts and Jobs Act, and finds results in line with the existing evidence, by providing an evaluation of institutional and individual response to the tax . A broad set of these evaluations focus on the effects of the TCJA on private sector executive compensation (Balsam, Evans III, and Yurko 2019, De Simone, McClure, and Stomberg 2022,Durrant, Gong, and Howard 2021,Luna, Schuchard, and Stanley 2023,Ohrn 2023), usually finding that executive compensation increased after the implementation of a similar measure that eliminated the performance-based pay exception. Additionally, other studies find that firm-level investment increased as a result of the reform (Crawford and Markarian 2024,Wagner, Zeckhauser, and Ziegler 2020,Chodorow-Reich et al. 2024), but that charitable giving was severely desincentivized (Han, Hungerman, and Ottoni-Wilhelm 2024).

1.2 Institutional context

1.2.1 What do college presidents do?

The role of a college president involves setting the overall strategy for the institution and managing the interests of a wide range of stakeholders, with fundraising, public relations, and financial sustainability being usually of central importance. For this reason, the role has been compared by college presidents as comparable to the role of a CEO in a private company. [1]. This role, as a result, involves a large amount of communication, with a college president giving 175 speeches in a given year, all of them geared to motivate and engage all stakeholders into supporting the organizational mission. [2].Furthermore, the role is heavily shaped by the individual style and priorities of presidents. James Duderstadt, president of the University of Michigan, states that

> "Each presidency is characterized by a distinctive style that, over time, tends to affect—or infect—the rest of the institution. Contributing to this style are the way the president approaches the challenge of leadership; the nature of the president's working relationships with students, faculty, and staff; the spirit of teamwork the president inspires among other university leaders; and even the character of university events." Duderstadt (2009)

One of the most important responsibilities for presidents is ensuring the continued financial viability of their institutions. This involves continued relationships with donors and alumni, aiming to secure financial support for the institution - as a result, presidents spend significant time and effort in fundraising and related activities. Additionally, presidents often engage in lobbying efforts, working with local, state, and federal governments to ensure that proposed policies support, or at least do not harm, the institution's mission. For example, this role requires continued engagement with federal regulations, via the process of negotiated rulemaking with the Department of Education in issues that affect students, or negotiations with the National Science Foundation or other external research funders to ensure financing. Furthermore, their role requires contact with state and local governments, given the existence of local educational support programs, such as loans and grants, and the outsized impacts that institutions can have on local economies. As a result, the role involves a significant amount of travel.

On top of setting strategies and ensuring financial sustainability, college presidents also have to exercise leadership within the campus community, as they are expected to be actively involved in

1. What Does a College President Do? US News.
2. The President's Many Roles, Inside Higher Education.

ensuring the welfare of the institutional community. This responsibility involves multiple aspects, such as addressing student issues that may result in disruptive protests, and reconciling the interests of faculty and administration. At institutions where collegiate sports are a major factor, presidents are also expected to address the issue if their teams are underperforming as well. Presidents face additional responsibilities outside of campus as well, as they often are expected to serve in institutional associations, such as the American Association of Universities, as well as possible advisory positions in government or outside paid engagements in the boards of external companies.

The role is year-round, with different pressures depending on the season. During the fall, presidents often deal with incoming students and their issues, while the winter break is focused on receptions and fundraising. During the spring, administrators usually focus on budgets, while, as the academic year is over, the summer is focused on political and regulatory issues. The pressures involved and the increasing amounts of responsibility are leading to shorter tenures, both due to the personal costs involved and the difficulty in maintaining the trust of many, usually antagonistic, actors involved at the same time.

1.2.2 How are college presidents hired and paid?

Colleges and universities are increasingly relying on executive-search firms, or "headhunters," to fill key leadership positions, including presidents, provosts, and even deans. This trend reflects a relatively recent shift, with the global search-firm industry now valued at nearly $13 billion, including a U.S. market share of $4 billion. Despite limited research on their role and cost in higher education, these firms have become crucial for managing leadership transitions. Closed searches have surged over the past two decades, driven by concerns that presidential candidates seeking new positions might lose their positions or cause potential donor unrest if their intentions to change positions were discovered. The use of these firms has recently extended as well to other positions, such as vice-presidents, provosts, and even deans. [3]

After an agreement is made, presidential hiring is increasingly marked by complex contracts

3. Hiring a Search Firm? Do Your Homework First. Chronicle of Higher Education.

that resemble those of corporate CEOs rather than the letters of appointment of traditional academic leaders. While the specifics of these agreements are often difficult to obtain—especially at private universities— Finkelstein and Wilde find, using a sample of 116 public university presidential contracts, that presidential contracts have grown more intricate, with terms and incentives mirroring those found in CEO contracts. This "CEO-ization" of the university presidency highlights a shift in how university leaders are compensated and managed, reflecting a more corporate approach to higher education governance. [4]

University presidential contracts are typically multiyear agreements, with initial terms ranging from three to five years and renewals commonly extending for five years. Compensation includes a base salary, deferred compensation, and tax-optimized agreements. Incentives such as signing, performance, retention, and completion bonuses are often determined by the president and approved by the governing board, though they are rarely made public or defined in measurable terms. Presidents also receive extensive benefits, including life and disability insurance, health coverage, and special retirement benefits, often structured to minimize tax liability via the addition of 'gross-up' payments intended to offset any remaining tax liabilities. Perks like housing and cars are common, as they are required for fulfilling their travel and hosting duties. Contracts usually provide provisions for outside activities, including speaking engagements and paid corporate directorships, which may require board approval. Evaluations typically involve the president setting annual goals for board review, but generally use general language. Termination clauses frequently guarantee payouts, sometimes for the full remaining contract term, or at a maximum of one year's salary.

1.3 Data

I use compensation data from the Chronicle of Higher Education between 2012 and 2019, along with IPEDS data on financial and academic outcomes for colleges. This period of time allows me to compare compensation trends before and after the implementation of the TCJA. Below, I describe

4. The CEO-ization of the President's Contract. Inside Higher Ed.

the data sources and sample construction and present descriptive statistics.

1.3.1 IRS 990 filings

The Chronicle of Higher Education provides executive compensation data for private colleges between 2012 and 2019, based on the IRS 990 filings for each college listing highly compensated and key employees, as defined by the IRS, in the respective Schedule J. This data, available at the individual level, provides individual names, titles, occupational categories (presidents, athletic employees, academics, administrators, medical personnel, lawyers, and former employees), and total compensation. Additional data is available for presidents: their compensation broken down by base salary, bonuses, other compensation, non-taxable compensation, and deferred compensation, along with their start and end dates in the position. This dataset provides a detailed panel of key employees, providing insight into institutional governance and compensation. The Chronicle, however, does not provide data on the universe of private colleges and universities, reporting data only for colleges with at least 100 million in annual revenue.

The Chronicle changed its criteria for inclusion into their data in 2017, from institutions with the largest 500 endowments to institutions with annual revenues of at least $100 million, leading to significant attrition in their original data. To ensure that my results are not driven by reporting or selection issues, I balance the panel to institutions that report at least one year after 2017. This data restriction leads to a sample with 262 institutions and 457 presidents spread between 2012 and 2019. [5] Additionally, all compensation information is presented in 2018 constant dollars.

1.3.2 IPEDS

I complement the Chronicle compensation data with IPEDS data for colleges between 2012 and 2019, to test the relationship between changes in compensation and the presence of effects on institutional-level outcomes.

5. This restriction leads to dropping 377 colleges out of 666, 56% of all the institutions that ever appear in the sample. However, these dropped colleges are disproportionately small, affecting a minority of students. Out of a total student population of 1.447.054 students in 2017, this restriction leads to dropping 27% of them.

As presidents, by their own admission, tend to focus on financial outcomes, I focus on financial outcomes that presidents are more likely to be able to influence - both by setting the strategic direction of the institution, and by spending priorities. Specifically, I focus on:

- Total revenue
- Total expenditures
- Net income (revenues minus expenditures)
- Tuition revenue
- Returns on investments.

However, additional resources are not necessarily translated into increased access to resources for students, as they can be directed into overhead or other institutional goals depending on policy priorities. To address this issue, I use the following measures:

- Total expenditure in instruction per capita: includes expenses for instruction, across both credit and noncredit activities.
- Total expenditure in research: includes activities specifically organized to produce research outcomes and commissioned by an agency either external to the institution or separately budgeted by an organizational unit within the institution.
- Expenditure in academic support: includes libraries, administration, personnel development, curriculum development, and IT.
- Net grant aid per capita, defined as the portion of scholarships and fellowships granted by an institution that exceeds the amount applied to institutional charges such as tuition and fees or room and board.

Table 1.1 describes the components of compensation and outcomes for the sample. Treated institutions, defined as institutions paying their presidents \$1M or more in 2017, offer substantially higher presidential compensation, with a mean of \$1,284,677 compared to \$553,128 for control institutions. The first fact is that college presidents receive most of their compensation as base salaries, with bonuses being less than 10% of total mean compensation and the median bonus being zero. This is consistent with the structure of the job, as multiple tasks are required and performance monitoring by the board is imperfect, leading to contracts where salary is the main driver of total compensation, and consistent with previous research. Significantly, other aspects of compensation, such as other kinds of compensation or non-taxable (in kind) compensation, play a larger role than bonuses, accounting for between 14% and 20% of total compensation. Turnover rates are slightly lower in the treated group (8%) than in the control group (13%), possibly reflecting greater stability in leadership. Treated institutions also report significantly larger financial resources, with mean total assets of \$7,384.32 million compared to \$1,097.81 million in the control group, and much higher total liabilities and endowments. Revenue generation is stronger among treated institutions across tuition, public funding, private funding, and investment returns. Finally, treated institutions spend more per capita on instructional and academic support, as well as research, suggesting a greater investment in educational and research outcomes.

However, this sample is not representative of the universe of non-profit or public institutions - and any intuitions about the presidents of these institutions is not likely to extend to the presidents of smaller non-profit or public insitutions. Table 1.2 compares institutions in the sample with the universe of private non profit institutions, and the universe of public institutions, using available IPEDS data on institutional characteristics, enrollment, and finances. Treated institutions are much larger, with an average enrollment of 15,485, compared to 7,689 in the sample, 1,747 in all non-profits, and 8,964 in all public institutions. The racial composition shows that treated institutions have a higher percentage of Asian students (10.12%) but lower percentages of Black and Hispanic

Table 1.1: Descriptive statistics, compensation and outcomes

	Control		Treated		Full sample	
	Mean	Median	Mean	Median	Mean	Median
Total compensation	553129	495001	1284677	1088551	719293	560739
Turnover	0.13	0	0.08	0	0.12	0
Base salary	412826	399458	792341	769145	499029	437489
Bonuses	34157.6	0.05	140073	0.05	58215.3	0.05
Non-taxable compensation	47329.8	36776.1	92713.1	62556.3	57638.2	39319.8
Other compensation	57776.3	17240.6	257134	83327.5	103059	22899
Total assets	1097.81	574.13	7384.32	2848.58	2534.11	701.92
Total liabilities	288.96	168.4	2246.84	1016.88	736.28	199.83
Total endowment	605.79	230.99	3922.62	1363.28	1360.5	299.2
Total revenue	244.7	151.66	1883.28	871.51	619.08	177.04
Total expenditures	219.34	137.73	1692.18	806.38	555.85	160.79
Net income	25.36	6.76	191.11	41.54	63.23	9.04
Tuition revenue	110.49	76.91	352.18	265.22	165.39	92.49
Public funding	15.28	2.81	253.57	112.39	69.41	3.44
Private funding	27.92	14.8	245.84	104.81	77.42	18.16
Investment return	40.27	9.42	311.34	56.96	101.84	12.62
Instructional expenditure per capita	99515.9	79446.2	333983	171818	153651	87850.1
Research	14.09	1.5	284.38	135.76	75.49	2.17
Academic support	24.11	14.62	140.65	62.36	50.58	17.05
Net aid per capita	798.74	0	6216.47	0	2049.61	0
Observations	467		1589		2056	

students compared to the broader population of non-profits and public institutions. The treated institutions also have a greater focus on graduate education, with 39.86% of their students in post-graduate programs, compared to just 6.46% in public institutions. Unlike public and other non-profit institutions, treated institutions do not have open admissions, with a highly selective average admission rate of 35.27%. Financially, treated institutions are far wealthier, with average assets of \$9,094 million and endowments of \$4,818.9 million, significantly surpassing other groups. Finally, their revenue and expenses are substantially higher, with treated institutions earning an average net income of \$243.4 million, compared to only \$2.7 million among all non-profit institutions.

Table 1.2: Comparison of institutions in the sample with the universe of private non-profit and public institutions

	Treated	Sample	All nonprofits	All public
Enrollment	15485	7689	1747	8964
Percent Black	7.94%	9.11%	13.28%	13.37%
Percent Hispanic	9.45%	9.36%	9.28%	13.53%
Percent Asian	10.12%	6.48%	3.90%	4.06%
Percent Female	52.91%	56.58%	55.80%	57.63%
Percent post-grad	39.86%	26.05%	28.22%	6.46%
Open admissions	0.00%	1.49%	18.73%	65.61%
Admission rate	35.27%	53.24%	78.48%	89.45%
Assets	9094.0	2307.7	191.4	93.0
Liabilities	2500.7	643.1	59.4	180.7
Endowment	4818.9	1178.3	97.6	96.4
Revenue	2350.6	587.2	59.8	207.9
Expenses	2107.1	500.6	57.1	197.5
Net income	243.4	86.6	2.7	10.4
N	53	261	1971	1725

Source: IRS 990 filings and IPEDS component, 2012-2019.

1.3.3 Who are the college presidents?

I manually collected comprehensive education and employment data for all the presidents in my sample. This dataset includes detailed information on their educational background, specifying the institutions and fields of study for their undergraduate and advanced degrees. Additionally, I gathered data on their professional experience, documenting their roles and institutions in their two most recent positions.

Table A-1 describes presidents' levels of education. The table shows the distribution of college presidents' highest degrees and their fields of study, providing insight into the academic backgrounds of leaders in higher education. The most common degree held by college presidents is a PhD, with 1,007 individuals holding one. Among these, 17.78% had unspecified fields ("Unavailable"), while Education (16.09%) and Social Sciences (11.22%) were the next most represented fields. Philosophy and Religious Studies (9.24%) and Psychology (5.46%) also made up significant portions. Notably, fields like Engineering (3.97%), History (3.87%), and Business (2.58%)

are less common but still relevant. For those with an MA (93 presidents), the most frequent field was Business, Management, Marketing, and Related Support Services, accounting for 31.18%. Health Professions and Education followed at 10.75% and 8.60%, respectively. In contrast, JD holders (136 presidents) had no detailed breakdown by field, and for BA holders (92 presidents), the vast majority (86.96%) had unspecified fields of study.

Table A-2 provides an overview of the institutions where college presidents obtained their highest degrees, broken down by the degree level (PhD, JD, and MA). The data suggests a concentration of degrees from elite universities but also highlights a wide variety of institutions contributing to leadership in higher education. For PhD holders, Harvard leads with 5.09% of all PhDs awarded, followed by the University of Pennsylvania (3.27%) and Yale (3.18%). Despite these prominent institutions, a significant portion of PhD holders (56.45%) received their degrees from other universities, and 18.64% of PhD data is listed as "Unavailable." Among JD holders, Harvard again stands out, accounting for 13.97% of all law degrees, with Yale (7.35%) and the University of Virginia (4.41%) following. While top law schools are well-represented, nearly 39% of JDs were awarded by other institutions, and 17.65% of JD data is unavailable. For MA degrees, Harvard (8.60%) and the University of Iowa (4.30%) are prominent, with Yale and Drexel University also contributing. A notable 25.81% of MA degree data is unavailable, and 40.86% of presidents received their MA from other institutions.

Table A-3 presents the last role of college presidents, highlighting that the majority of college presidents come from academic leadership roles, particularly from positions such as Vice President, President, Provost, and Dean. A significant 74.85% of presidents had their last job in academia. Of these, 28.67% were Vice Presidents, 25.55% were already serving as Presidents, 18.21% were Deans, and 18.01% were Provosts. Smaller percentages held positions as Directors, Chairs, Professors, or other academic administrative roles. Outside academia, 3.61% of presidents previously worked in government roles, while 3.77% came from the non-profit sector, and 10.02% transitioned from the private sector. A smaller portion, 1.88%, had their last job in religious organizations. For 5.87% of the presidents, the data on their prior work experience is unavailable.

1.4 Effects of the TCJA

1.4.1 Policy

Congress undertook a wide-ranging revision of the tax code in 2017 via the Tax Cuts and Jobs Act (TCJA), with provisions affecting the non-profit sector - and, by extension, private colleges and universities, since they are classified as nonprofits (501(c)(3) organizations). While some measures are aimed at individuals, such as changes in the tax deduction for charitable donations and an increase in estates exempt from estate tax, most measures are aimed at closing loopholes for nonprofit organizations, putting them on a similar footing to private sector firms. In first place, the TCJA eliminated deductions for unrelated business income obtained by nonprofits carrying out revenue-raising activities outside their core mission, disallowing offsetting revenue from unrelated businesses with losses from another unrelated business to minimize tax burdens. In second place, the TCJA imposed a 1.4% excise tax on the net investment income of colleges of universities with large endowments, affecting institutions with at least 500 students and at least $500.000 in investment assets per student. This excise tax applies to approximately 40 institutions in the country and is intended to equalize the tax treatment between foundations and universities, as foundations were already subject to a similar excise tax. Finally, the TCJA imposed a 21% excise tax on remuneration in excess of 1 million for covered employees. This threshold is constant and is not indexed to inflation. On average, this tax affects the top 20% of the compensation distribution.

The implementation of this excess compensation tax follows the given stages for nonprofit organizations, as defined in Section 501(a). The identification of covered employees falls to the individual organization, regardless of the remuneration paid to any of its employees. The IRS defines covered employees as any one of the 5 highest compensated employees in the organization for the year, or any employee who has ever been defined as a covered employee. The excise tax applies to all aspects of compensation, regardless of its composition and presence of non-taxable income, and includes compensation received from related organizations, but excludes Roth IRA contributions and any compensation to medical professionals for medical services. If an employee is subject to the tax, the nonprofit organization calculates the amount of tax due, pro-rating if necessary its contribution to the total due if it does not pay the total remuneration of the employee.

1.4.2 Effects on compensation

To identify the effects of the TCJA on presidents' outcomes, I implement an event study at the institution level of the form

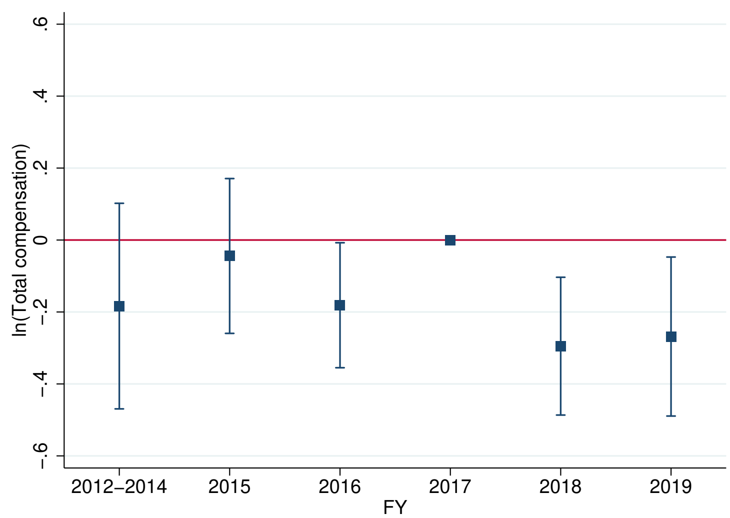
$$y_{ist} = \sum_{t \neq 2017} \beta_t \times T_{st} \times I(t = t) + \gamma_s + \eta_t + \gamma_s \cdot t + e_{st} \tag{1.1}$$

where y_{st} is the outcome for institution s in year t, T_{ist} is an indicator variable for whether the institution was paying its president \$1M or more in 2017, γ_s is an institution fixed effect, η_t is a year fixed effect, and $\gamma_s \cdot t$ is an institution-specific time trend. The coefficient of interest, β_t, captures the average difference in outcomes for employees subject to the excise tax before and after its implementation. The reference year is 2017, the year the TCJA was passed, as its implementation began in 2018. I bin all years prior to 2014 in a single coefficient, and cluster standard errors and the institution level to address temporal correlation issues. It is worth recalling that, due to the inclusion of unit-specific trends, the counterfactual becomes the outcome compared to the reference period, net of underlying linear trends in the counterfactual between that period and now - i.e., a comparison within individuals, instead of across the treatment and control group. (Miller (2023))

The key identifying assumption permitting a causal interpretation of the β_t parameters is that the compensation of covered employees subject to the tax would have trended similarly to the compensation of other employees, absent the introduction of the excise tax. This assumption is likely to hold, as the passage of the TCJA was unexpected prior to the 2016 election, making it unlikely that institutions anticipated the reform. Additionally, the event study results show no evidence of anticipatory effects for college presidents, providing additional evidence in favor of the parallel trends assumption.

Figures 1.1 and 1.2 present point estimates and confidence intervals from estimating Equation 1 for log total compensation and presidential turnover as outcomes. Total compensation decreases markedly, falling by 29% in the first year and by 26 percent with respect to the baseline year. Additionally, turnover also increases significantly - institutions affected by the TCJA are more likely to see presidential turnover, by 19 % in the first year and 15% in the second year. These patterns are consistent with the effects of a payroll tax, or an increase in the cost of specialized skills: institutions shift this cost increase on to their employees, or attempting to replace them for lower-paid alternatives. Furthermore, these results imply an institutional tax elasticity of -1.23.

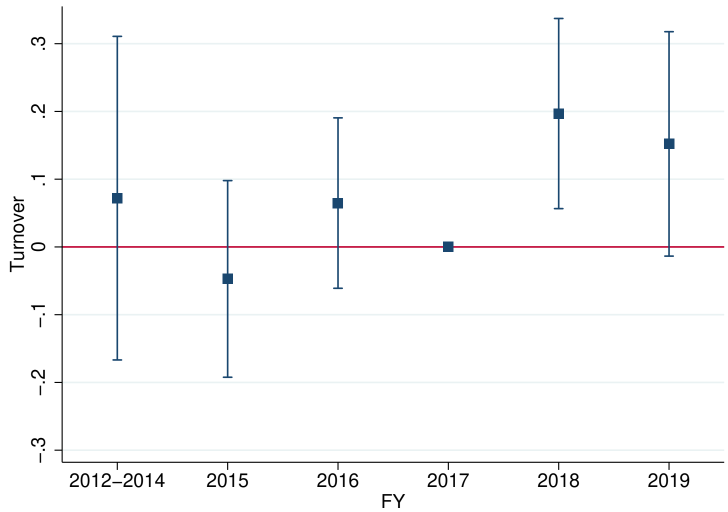
Figure 1.1: Effects of the TCJA on compensation



Notes: Regressions include institution and year fixed effects and institution-specific time trends. Standard errors are clustered at the institution level. Pre-treatment periods before 2014 are binned into a single coefficient.

Figure 1.3 presents the above results for the disaggregated components of presidential compensation - a result of interest, as the TCJA affects primarily non-taxable and other forms of com-

Figure 1.2: Effects of the TCJA on turnover



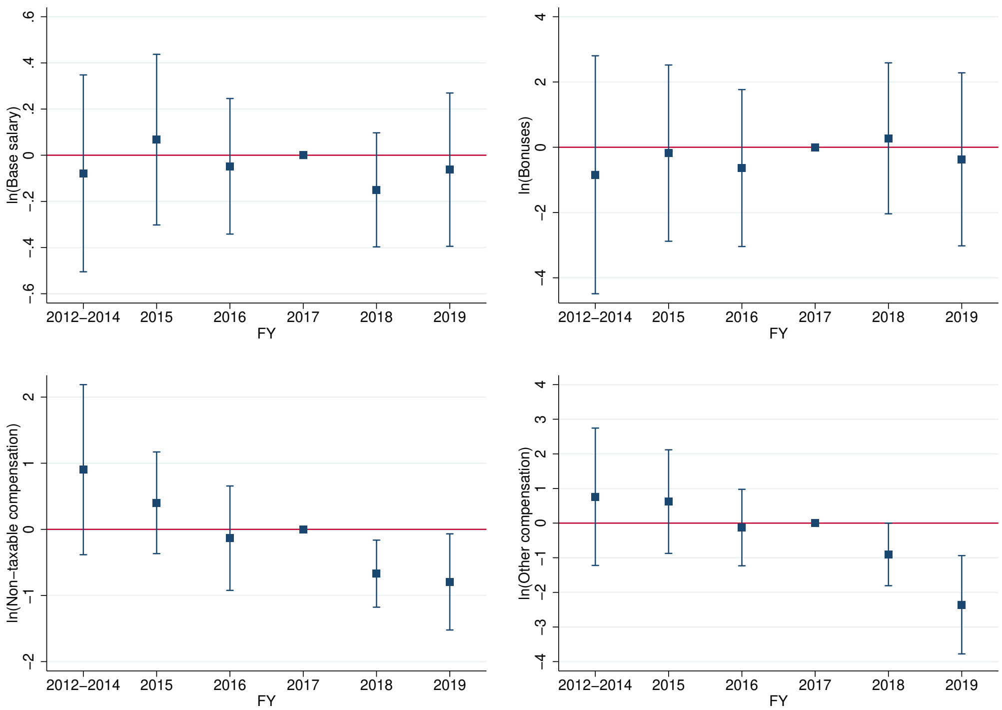
Notes: Regressions include institution and year fixed effects and institution-specific time trends. Standard errors are clustered at the institution level. Pre-treatment periods before 2014 are binned into a single coefficient.

pensation, instead of top-line salaries and bonuses. Non-taxable compensation falls by 0.66 log points in the first year (49%), and 0.79 log points in the second year (55%). Furthermore, other compensation falls significantly, by 0.9 log points in the first year (60%), and 2.35 log points in the second year (91%). However, base salaries and bonuses are unaffected. This pattern is consistent with both the compensation structure and the objectives of the TCJA - as compensation agreements tend to be rigid, especially regarding salaries and bonuses, institutions adjust via less transparent aspects of compensation.

Individual vs. composition effects

Since the tax is levied at the institutional level, the previous model was estimated at the institution level as well. However, these results are likely to be composed of within-president changes in compensation, and changes in average compensation caused by presidential turnover, as new

Figure 1.3: Effects of the TCJA on compensation by components



Notes: Regressions include institution and year fixed effects and institution-specific time trends. Standard errors are clustered at the institution level. Pre-treatment periods before 2014 are binned into a single coefficient.

presidents tend to be lower-paid than their predecessors. [6] To disentangle the effects of turnover vs. within-president changes in compensation, I estimate an event study of the form:

$$y_{ist} = \sum_{t \neq 2017} \beta_t \times T_{ist} \times I(t = t) + \gamma_s + \delta_i + \eta_t + \gamma_s \cdot t + e_{ist} \tag{1.2}$$

where y_{ist} is the outcome for individual i in institution s in year t, T_{ist} is an indicator variable for whether the institution was paying its president \$1M or more in 2017, γ_s is an institution fixed effect, δ_i is an employee fixed effect, η_t is a year fixed effect, and , and $\gamma_s \cdot t$ is an institution-specific

6. A complementary approach to avoid composition effects is to keep only the institutions in the sample who did not change their presidents after the implementation of the TCJA and re-estimate the above models. The results of this approach are similar to the results in this section, and are available upon request.

time trend. As before, I bin all years prior to 2014 in a single coefficient, and cluster standard errors and the institution level to address temporal correlation issues. The results of this model can be interpreted as a within-president effect of the changes in compensation of the TCJA.

Table 1.3 presents point estimates and confidence intervals from estimating Equation 1 for log total compensation and presidential turnover as outcomes, for models including and excluding individual president fixed effects. As before, total compensation for individual presidents decreases, falling by 23% in the first year and by 20% with respect to the baseline year, almost perfectly in line with the additional burden induced by the tax. Additionally, turnover also increases at the individual level - presidents at institutions affected by the TCJA are more likely to see presidential turnover, by 15%, but the effect appears to decay rapidly. As the results are similar to institution-level results, it is plausible that results are driven by individual changes in compensation, instead of composition effects due to president turnover. Furthermore, this implies a smaller individual tax elasticity of -1.095, compared to an institutional tax elasticity of -1.38.

Table 1.3: Event study, institution level vs. president-level effects

	(1) ln(Total compensation)	(2) Turnover	(3) ln(Total compensation)	(4) Turnover
$t-3$	-0.138 (0.143)	0.0566 (0.118)	0.0599 (0.162)	-0.113 (0.133)
$t-2$	-0.0418 (0.109)	-0.0354 (0.0751)	0.0873 (0.121)	-0.134 (0.0913)
$t-1$	-0.180** (0.0883)	0.0744 (0.0658)	-0.103 (0.0985)	-0.00840 (0.0692)
$t+1$	-0.267*** (0.0949)	0.184*** (0.0679)	-0.232** (0.101)	0.150** (0.0602)
$t+2$	-0.244*** (0.0927)	0.169** (0.0698)	-0.201* (0.104)	0.103 (0.0831)
Institution FE	Yes	Yes	Yes	Yes
President FE	No	No	Yes	Yes

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Appendix Figure A-3 presents the above results for the disaggregated components of presidential compensation for the within-president model - a result of interest, as the TCJA affects primarily non-taxable and other forms of compensation, instead of top-line salaries and bonuses. Non-taxable compensation falls by 0.56 log points (43%) in the first year, and 0.75 log points (53%) in the second year. Furthermore, other compensation falls significantly, by 1.08 log points (67%) in the first year, and 2.05 log points (88%) in the second year. However, as before, base salaries and bonuses are unaffected.

1.4.3 Threats to identification and robustness tests

Selection into treatment issues

A first threat to identification is the fact that institutions or presidents may strategically alter their compensation in order not to be affected by the tax, especially for individuals close to the cut-off. For example, a president earning marginally more than a million dollars would be worse off than a president just under this threshold, and manipulate their compensation structure in order to avoid being subject to the tax. This manipulation would change the composition of the treatment and control groups, changing the interpretation of the estimates and possibly affecting their respective trends. However, there are no presidents who earned more than a million dollars in 2017 or before, and who subsequently earned less than this amount during the following years at the same institution - avoiding biases due to changes in the composition of the treatment and control group.

Anticipation effects

This identification strategy could be biased if institutions anticipated the tax increase, giving them time to adjust by preemptively altering presidential compensation patterns or dismissing particularly high-paid presidents. However, it is likely that this tax was unexpected for several reasons. First, the most discussed provision of the TCJA regarding academia was the introduction of the endowment tax, while the excess compensation tax received minimal attention, and the elimination of deductions for unrelated business income was scarcely mentioned. A source even referred to the excess compensation tax as "another cost of doing business" that would be incorporated into the already substantial expenses of hiring top employees, alongside search fees, salaries, and benefits packages [7]. Second, the TCJA did not immediately go into full effect; although colleges began complying with the new law, the IRS issued additional guidance in 2018 that clarified the definition of covered employees and the payments subject to the tax [8]. Lastly, the pretrends in the event study design do not show a clear trend, further suggesting that the legislation and its effects were not anticipated.

Joint test of pre-treatment coefficients

Testing the parallel trends assumption by individual inspection of the pre-treatment coefficients is likely to involve issues with low statistical power to detect any violation (Roth (2022)). In first place, I implement the Freyaldenhoven et al. (2021) recommendation and implement a joint test for all pre-treatment coefficients to be jointly equal to zero. Appendix Table A-4 presents the results of this approach. I find that pre-treatment coefficients are not jointly statistically significant at the 5% level for presidential turnover, total compensation, base salaries, bonuses, and non-taxable compensation, suggesting that the parallel trends assumption holds in this setting.

Rambachan and Roth (2023) sensitivity analysis

Finally, I implement the Rambachan and Roth (2023) approach based on bounding the true treatment effect, using the intuition that pre-trends are informative about violations of parallel trends. Their central insight is that the violations of parallel trends in the post-treatment period cannot be substantially larger than those in the pre-treatment period, by some constant M larger than the maximum violation of parallel trends in the pre-treatment period. The value of $M = 1$, for instance, imposes that the post-treatment violation of parallel trends is no larger than the largest pre-treatment violation of parallel trends. This approach can be used for sensitivity analyses, by reporting different confidence intervals for the treatment effect for different values of M, and report-

7. Tax Law's Effects on Colleges Unfolding. Inside Higher Ed.
8. Interim Guidance Under Section 4960

ing the breakdown value of M - the largest value for which the treatment effect is still statistically significant.

I present the results of this analysis in Appendix Figures A-4, A-5, and A-6 for a standard 5% significance level. I find that the critical M values are close to 0.15 for presidential turnover and 0.5 for total compensation, indicating that for the result to remain statistically significant the maximum violation of parallel trends in the post-treatment period can be, at most, 15% and 50% of the maximum pre-treatment violation of parallel trends. For the statistically significant results on the effects of the TCJA (other compensation and deferred compensation paid), I find that the critical M value is close to 0.55 for non-taxable compensation and 1.25 for other compensation. As the results are driven by changes in non-taxable and other compensation, it is possible to suggest that they are robust to possible deviations from the parallel trends assumption in the post-treatment period.

I repeat this analysis for the specification including president fixed effects in Appendix Figures A-7, A-8, and A-9 , and find broadly similar results - the critical M values are close to zero for presidential turnover and 0.3 for total compensation, indicating that for the result to remain statistically significant the maximum violation of parallel trends in the post-treatment period can be, at most, zero and 30% of the maximum pre-treatment violation of parallel trends. For the statistically significant results on the effects of the TCJA (other compensation and deferred compensation paid), I find that the critical M value is close to 0.2 for non-taxable compensation and 1 for other compensation.

Two-way fixed effects approach

I also estimate a standard two-way fixed effects model of the form:

$$y_{st} = \alpha(D_{st} \times Post_{st}) + \gamma_s + \eta_t + \gamma_s \cdot t + e_{st} \tag{1.3}$$

where y_{st} is an outcome for institution s in year t, D_{st} is a indicator for whether the institution paid its president \$1M in 2017, $Post_{st}$ is an indicator for years after policy implementation, γ_s is an institution fixed effect, η_t is a year fixed effect, and $\gamma_s \cdot t$ is an institution-specific trend. This approach is complementary to the event study approach described above, but involves trade-offs, as it provides additional power for identifying treatment effects for the full post-treatment period, at the cost of averaging any existing dynamic treatment effects.

Appendix Figure A-10 describes the result of this model. This specification finds that total compensation decreases by 26 percentage points, with turnover increasing by 19 percentage points, while non-taxable compensation falls by 44 percentage points and other compensation falls by 1.36 log points (75%). A similar specification in Appendix Figure A-11 with the addition of president fixed effects to address composition effects, finds similar results: total compensation decreases by 17 percentage points, with turnover increasing by 13 percentage points, while other compensation falls by 1.21 log points (71%).

1.4.4 Effects on institutional outcomes

After establishing how institutions pass on the cost of the tax on to their presidents, the next question is how sitting presidents respond to this change in compensation. While standard theory predicts that presidents will reduce their effort or change jobs - a relevant margin, as the probability of exit increases by 19%, sitting presidents may not be able to reduce their effort due to their selection into the position and the characteristics of their contract. Instead, effort can be expected to remain constant or even increase as a result of the structure of their contracts. As discussed earlier, presidential contracts are renegotiated with the board every three to five years. In this setting, presidents can be incentivized to improve their bargaining position in the next renegotiation by shifting effort to easily improved and observable metrics, especially financial ones - a pattern intensified by the unclear relationship between financial outcomes and student success and the multiple academic, research and social objectives pursued by their institutions.

Based on the effects of the TCJA on presidential compensation and turnover, and the fact that presidents explain a significant fraction of variation in academic and financial outcomes, I test whether the introduction of the TCJA affected these institutional outcomes. I use a standard two-way fixed effects model of the form:

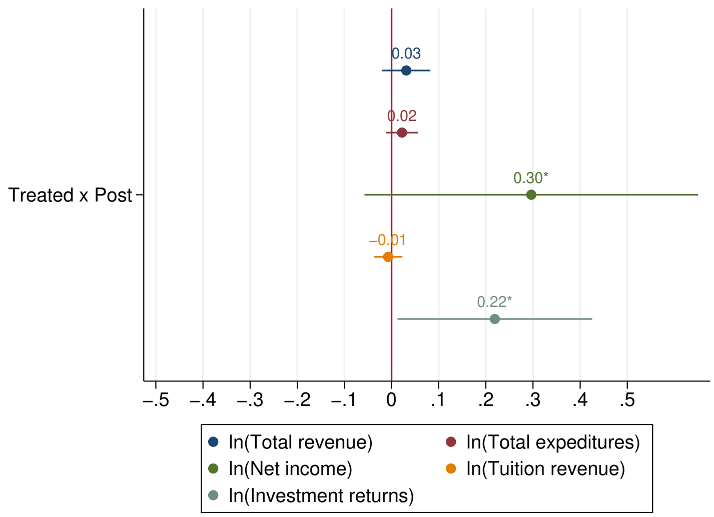
$$y_{st} = \alpha(D_{st} \times Post_{st}) + \gamma_s + \eta_t + \gamma_s \cdot t + e_{st} \tag{1.4}$$

where y_{st} is an outcome for institution s in year t, D_{st} is a indicator for whether the institution paid its president \$1M in 2017, $Post_{st}$ is an indicator for years after policy implementation, γ_s is an institution fixed effect, η_t is a year fixed effect, and $\gamma_s \cdot t$ is an institution-specific trend. Standard errors are clustered at the institution level.

First, I test whether the parallel trends assumption is appropriate in this setting, by estimating an event study design (not reported) and reporting the joint test of pre-treatment coefficients being jointly equal to zero. Table A-5 presents the results of this test, finding that the test is not statistically significant for any of the outcomes, providing evidence in favor of the parallel trends assumption.

Figure 1.4 presents the coefficients of this model for revenue composition, and Figure 1.5 presents the coefficients of this model for expenditure composition. On average, presidents increased net income (revenues minus expenditures) by 30 percentage points, while investment returns also increased by 22 percentage points. Furthermore, expenditures were also affected, as academic expenditures increased by 9 percent. This result implies that, when faced with a worsened bargaining position, due to their increased cost to the institution, presidents deliver improvements on selected, easily legible outcomes. In first place, presidents improve revenues via the most flexible tool available to them - investment returns, as other components of revenue adjust more slowly. In second place, presidents improve net incomes, an easily observable financial metric. Third, presidents increase expenditures on administration and programs that are easily legible to the board, instead of investing on student support projects that may be less visible.

Figure 1.4: Effects of the TCJA on revenues



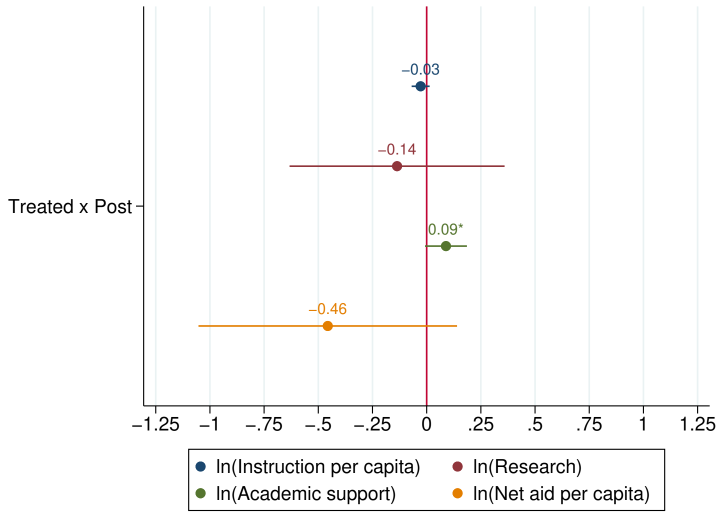
Notes: coefficients from a two-way fixed effect regression on a binary treatment indicator including institution and year fixed effects, and institution-specific time trends. Standard errors are clustered at the institution level. * p<0.1, ** p<0.05, *** p<0.01

Appendix Figure A-12 presents the coefficients of this model for revenue composition, and Figure A-13 presents the coefficients of this model for expenditure composition, for individual presidents, excluding possible composition effects. On average, investment returns increased by 23 percent, while there were no statistically significant effects on expenditures, showing broadly similar patterns as before. However, the null result on expenditures points towards new presidents, instead of existing ones, increasing expenditures as a means to shore up their new positions inside the institution.

1.4.5 Heterogeneous effects by treatment intensity

As higher-paying institutions have a larger tax burden, they face higher absolute costs and different incentives than less-affected institutions. As a result, higher-paying institutions may react

Figure 1.5: Effects of the TCJA on expenditures



Notes: coefficients from a two-way fixed effect regression on a binary treatment indicator including institution and year fixed effects, and institution-specific time trends. Standard errors are clustered at the institution level. * p<0.1, ** p<0.05, *** p<0.01

differently to the tax. To address this issue, I test whether the introduction of the TCJA affected institutional compensation and outcomes differently based on their prospective absolute tax costs. I use a standard two-way fixed effects model of the form:

$$y_{st} = \alpha(Distance_{st} \times Post_{st}) + \gamma_s + \eta_t + \gamma_s \cdot t + e_{st} \tag{1.5}$$

where y_{st} is an outcome for institution s in year t, D_{st} is the distance from the \$1M threshold of an institution's president salary in 2017, $Post_{st}$ is an indicator for years after policy implementation, γ_s is an institution fixed effect, η_t is a year fixed effect, and $\gamma_s \cdot t$ is an institution-specific trend. Standard errors are clustered at the institution level.

Figure 1.6 presents the coefficients of this model for presidential compensation outcomes. On average, an additional 1% distance from the threshold reduces total compensation by an extra

0.59%, and increases the probability of turnover by 0.28%. Furthermore, other compensation falls by an additional 1.39 % for each additional 1% distance from the threshold.

Figure 1.6: Effects of the TCJA on compensation, continuous treatment

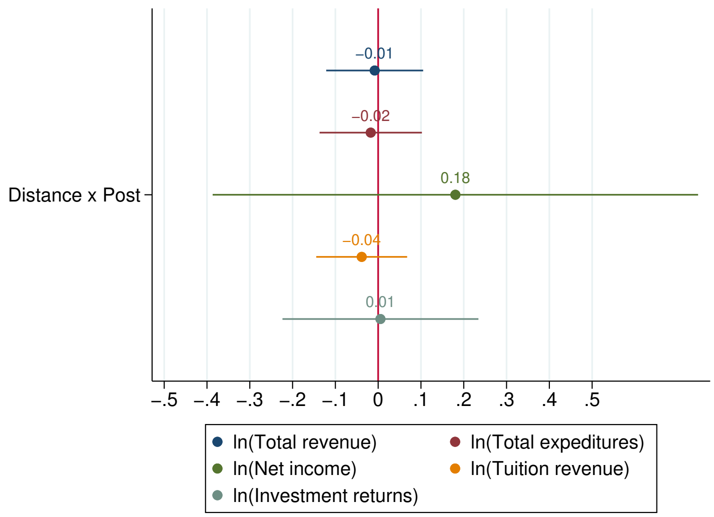
Notes: coefficients from a two-way fixed effect regression on a continuous treatment measure including institution and year fixed effects, and institution-specific time trends. Standard errors are clustered at the institution level. * $p<0.1$, ** $p<0.05$, *** $p<0.01$

Appendix Figure A-14 presents the coefficients of this model for presidential compensation outcomes, including individual president fixed effects. On average, an additional 1% distance from the threshold reduces total compensation by an extra 0.41%, but does not affect the overall probability of turnover. Furthermore, other compensation falls by an additional 1.30 % for each additional 1% distance from the threshold.

Figure 1.7 presents the coefficients of this model for revenue composition, and Figure 1.8 presents the coefficients of this model for revenue composition. On average, there are no effects on revenues or revenue composition, and few effects on expenditure composition. The only notable result is that an additional 1% distance from the threshold reduces net aid per capita by an extra

0.59%. As net aid is aid after institutional charges, this result implies additional incentives for cost-cutting via either reducing aid (unlikely) or increasing institutional charges to match existing grant aid.

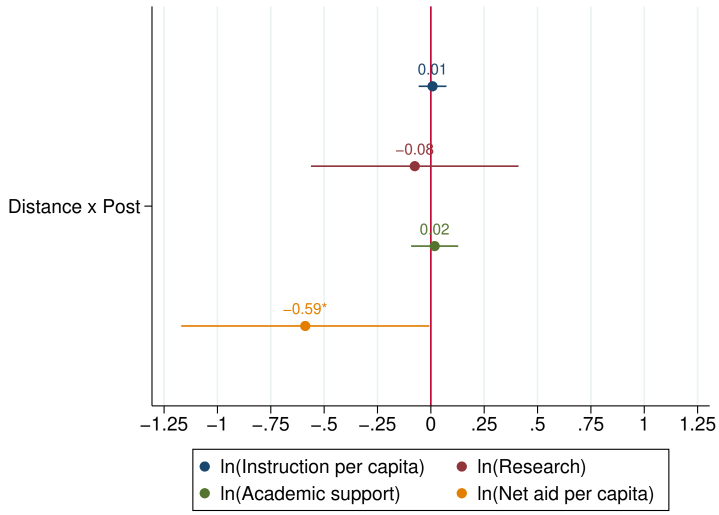
Figure 1.7: Effects of the TCJA on revenue, continuous treatment



Notes: coefficients from a two-way fixed effect regression on a continuous treatment measure including institution and year fixed effects, and institution-specific time trends. Standard errors are clustered at the institution level. * p<0.1, ** p<0.05, *** p<0.01

Appendix Figure A-15 presents the coefficients of this model for revenue composition, and Appendix Figure A-16 presents the coefficients of this model for revenue composition, including individual president fixed effects. As before, there are no effects on revenues or revenue composition, and few effects on expenditure composition. The only notable result is that an additional 1% distance from the threshold reduces net aid per capita by an extra 0.56%, close to the earlier result. The similarity implies that this policy is not due to new presidents changing institutional priorities by cutting costs, but instead is due to existing presidents adjusting the use of existing resources.

Figure 1.8: Effects of the TCJA on expenditures, continuous treatment



Notes: coefficients from a two-way fixed effect regression on a continuous treatment measure including institution and year fixed effects, and institution-specific time trends. Standard errors are clustered at the institution level. * p<0.1, ** p<0.05, *** p<0.01

1.4.6 Heterogeneous effects by president characteristics

Examining how presidents' characteristics affect outcomes is also valuable, since factors as demographics, education, and experience can mediate the individual effects of the TCJA via their skills, bargaining power and outside options. Demographic circumstances may affect presidents' bargaining, while presidents with more relevant experience or different qualifications may have better institutional management skills or outside options, leading to stronger leverage in pay discussions and different effects of the tax by group. In this setting, I use the original event study specification, using the institution as the level of analysis. However, the small sample sizes in some subpopulations will lead to large standard errors.

Figure 1.9 presents the results of an event study approach by gender on total compensation and turnover. I find that the effects of the TCJA are disproportionately concentrated on female presidents, with their compensation falling between 40 and 50 log points (approximately 33-40%). Effects for male presidents are closer to the overall result, with their compensation falling by 21%. However, beyond a dip on the probability of turnover for female presidents in the first period, there are no significant patterns by gender on turnover. This result is likely to reflect imbalances in bargaining power by gender, as female presidents may face either direct discrimination or weaker outside labor market options.

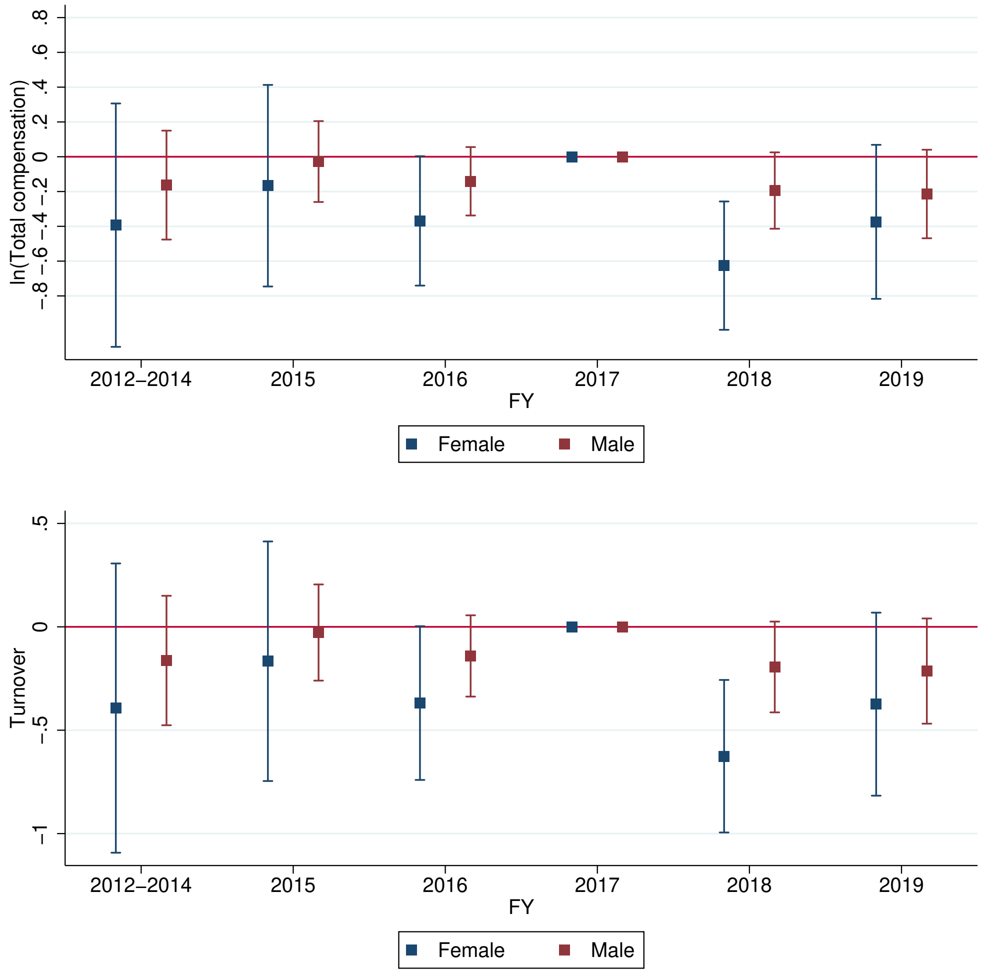
Figure 1.10 presents the results of an event study approach by education levels on total compensation and turnover. The effects of the TCJA are disproportionately concentrated on presidents holding PhDs, with noisy coefficients for other education levels - a consistent result with a large majority of college presidents holding PhDs.

Figure 1.11 presents the results of an event study approach by last position levels on total compensation and turnover. I find that the effects of the TCJA tend to be concentrated in two groups - former college presidents and individuals coming from the public sector face the largest falls in compensation, while other groups appear to be unaffected. However, there is no clear pattern related to turnover. As before, this result is likely to reflect different bargaining positions - presidents who were presidents before at another institution are closer to the end of their careers, while former public servants may have limited options, so their outside options may be weaker than those of individuals with additional room for career advancement, such as former academic vice-presidents, or with options for lateral moves, such as former private sector or non-profit employees.

1.5 Covariates of presidential productivity

As seen above, college presidents can predict institutional outcomes, and respond to monetary incentives by either leaving or changing their behavior, which is reflected in metrics of financial performance, as seen with the effects of the TCJA. Their demographic characteristics also influence their bargaining power, with the tax affecting them unevenly. However, examining the impact of demographics on outcomes directly may be challenging, as presidents are matched with institu-

Figure 1.9: Effects of the TCJA on compensation and turnover by gender

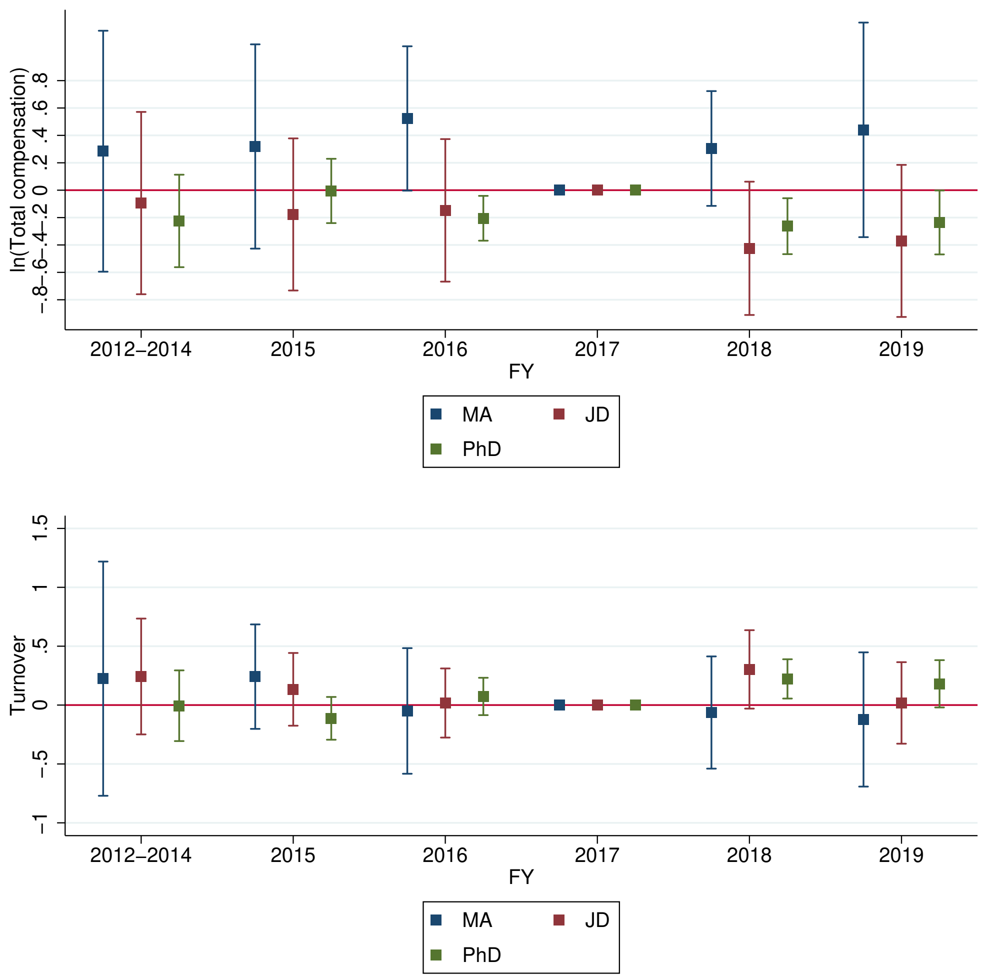


Notes: regressions include institution and year fixed effects and institution-specific time trends. Standard errors are clustered at the institution level. Pre-treatment periods before 2014 are binned into a single coefficient.

tions based on both their productivity and demographic profiles, a sorting that makes the parallel trends assumptions unlikely to hold. A practical alternative is to revisit the R-squared decomposition by institution, year, and president fixed effects, and analyze how president-level residuals are correlated with demographic characteristics.

Presidential productivity can vary widely across tasks, due to the diverse responsibilities they

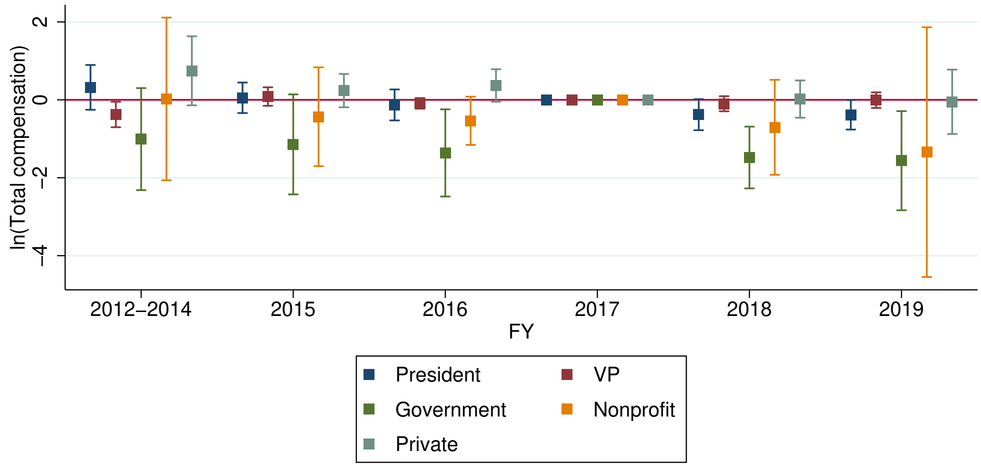
Figure 1.10: Effects of the TCJA on compensation and turnover by education

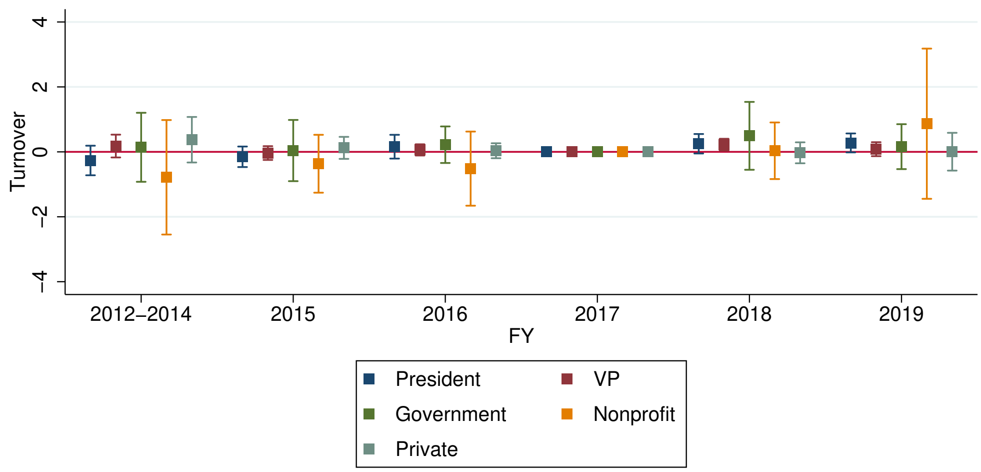


Notes: regressions include institution and year fixed effects and institution-specific time trends. Standard errors are clustered at the institution level. Pre-treatment periods before 2014 are binned into a single coefficient.

hold. I focus on the financial aspect, as presidents often prioritize this part of their role. A useful measure of institutional financial health is net income (revenues minus expenditures), which offers an first measure of whether an institution generates surplus funds—a critical foundation for any potential investments in academics, research, and other key areas.

Figure 1.11: Effects of the TCJA on compensation and turnover by experience





Notes: regressions include institution and year fixed effects and institution-specific time trends. Standard errors are clustered at the institution level. Pre-treatment periods before 2014 are binned into a single coefficient.

In first place, I estimate a model of the form

$$NetIncome_{ist} = \alpha_s + \beta_t + \gamma_i + \delta_c + e_{st} \tag{1.6}$$

where α_s is a institution fixed effect, β_t is year fixed effect, and γ_i are president fixed effects, which capture presidential productivity (specific to a given person) and are assumed to be constant and portable across institutions. However, these fixed effects are not necessarily identified in the base two way fixed effect model. To make direct comparisons across these fixed effects, every single president-institution pair needs to be linked directly or indirectly across the data. This is not the case in this sample, as limited presidential mobility across institutions divide the sample into different connected (or mobility) sets, defined as networks of presidents or institutions where each president-institution pair is directly or indirectly linked, allowing the entire group's fixed effects to be identified relative to each other.[9] As the sample is divided into multiple connected sets, I control for δ_c, connected set fixed effects, to allow for comparisons across connected sets.

After estimating these president fixed effects, I estimate how presidential characteristics are correlated with the standardized presidential fixed effects for surpluses, in terms of standard deviations of unobserved productivity.

I estimate

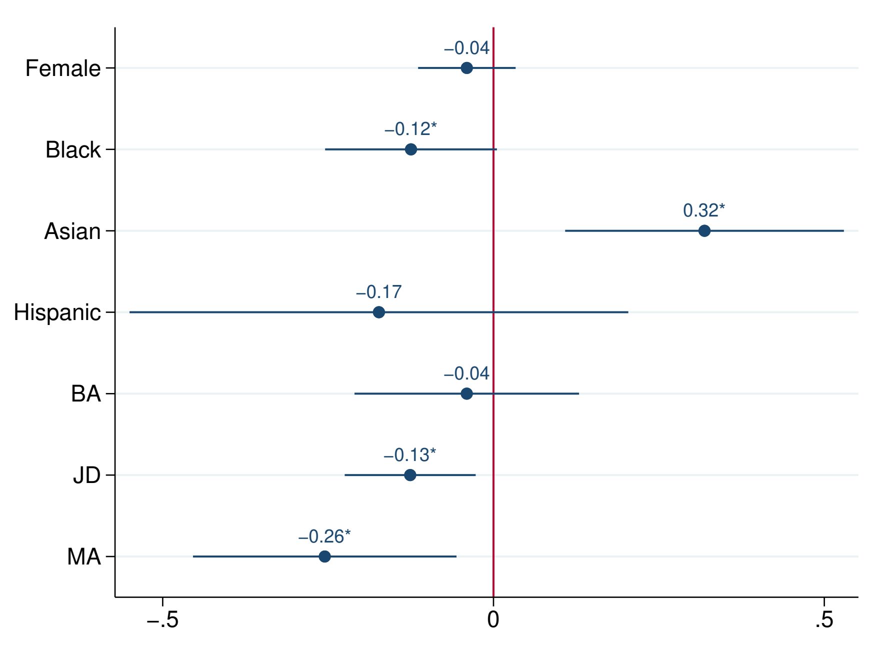
$$FEi = \alpha \cdot Demographics_i + \beta \cdot Education_i + \delta_c + e_i \tag{1.7}$$

Figure 1.12 presents the results of this model. Black presidents are associated with 0.12 lower standard deviations in productivity, while Asian presidents are associated with 0.32 higher standard deviations in productivity, compared to a White president baseline. Presidents holding a JD are associated with 0.13 lower standard deviations, while presidents holding a MA are associated with 0.26 lower standard deviations, compared to a baseline of presidents with PhDs. The group trends are likely to reflect sorting, as Black presidents are less likely to preside over the largest and wealthiest institutions which are more likely to produce surpluses. Educational trends are consistent with selection as well, as presidents holding JDs or MAs are more likely to have previous external experience, limiting their experience and effectiveness in managing educational institutions.

To address the correlation between field of study in their highest degree and president fixed

9. See Abowd, Kramarz, and Margolis (1999) for a canonical explanation in a worker and firm fixed effect setting, and Bartanen and Husain (2022) for an application to school principals.

Figure 1.12: Correlation of individual presidential fixed effects with demographics and education



Notes: coefficients from a regression of individual presidents' standardized fixed effects on covariates. President fixed effects are the president-level residuals of a decomposition of net income on institution, year, and president fixed effects.
* p<0.1, ** p<0.05, *** p<0.01

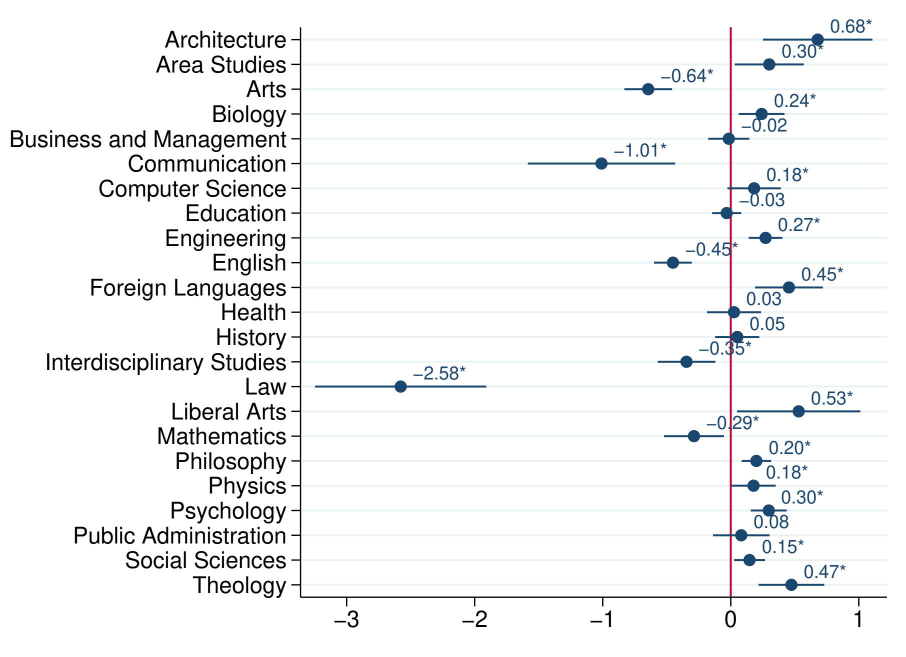
effects, I estimate

$$FEi = \alpha \cdot Demographics_i + \beta \cdot Field_i + \delta_c + e_i \tag{1.8}$$

Figure 1.13 presents the results of this model. On average, most fields are weakly positively related with productivity, with extreme negative outliers being Arts, Communication and Law - reflecting a combination of sorting issues, as presidents with this background tend to manage relatively small and troubled institutions in the first place. Significantly, Business and Management graduates have coefficients close to zero, in contrast to Acemoglu (2023) and Muñoz and Otero (2024), who find a positive relationship between business training and executive quality.

I finally turn to the correlation with presidents' previous experience, using previous experience

Figure 1.13: Correlation of individual presidential fixed effects with field of study



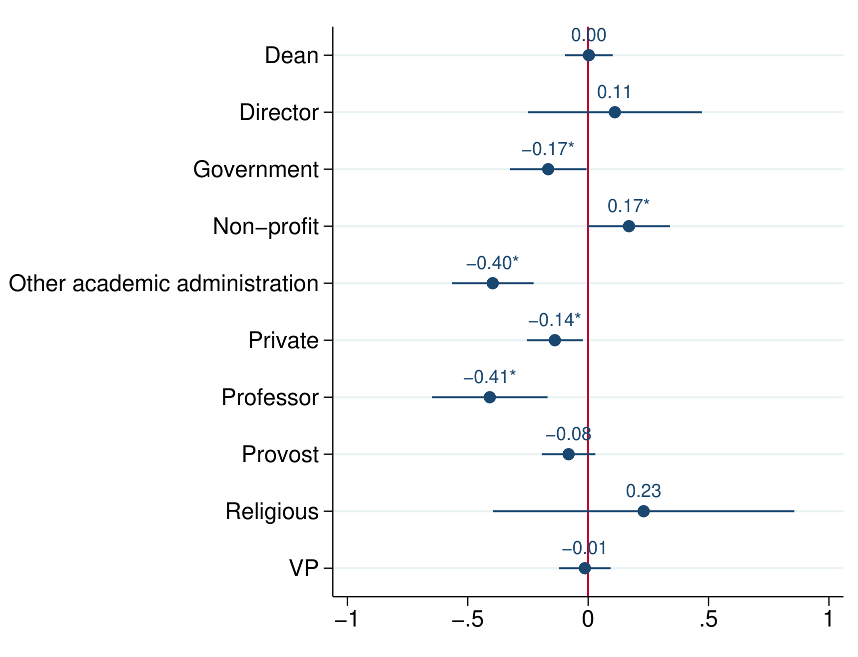
Notes: coefficients from a regression of individual presidents' standardized fixed effects on covariates. President fixed effects are the president-level residuals of a decomposition of net income on institution, year, and president fixed effects.
* p<0.1, ** p<0.05, *** p<0.01

as a college as a president as a baseline. I estimate:

$$FEi = \alpha \cdot Demographics_i + \beta \cdot Experience_i + \delta_c + e_i \tag{1.9}$$

Figure 1.14 presents the results of this model. Significantly, the returns to experience of previously having been a college president (the base category) are high, as no other category, except former non-profit directors, outperform them. Significantly, backgrounds from the public and private sector significantly underperform, being associated with 0.17 and 0.14 lower standard deviations in productivity. The returns to experience in upper administration are also evident, as former vice-presidents, deans, directors, or provosts do not underperform former presidents - but former professors or other administrators significantly underperform.

Figure 1.14: Correlation of individual presidential fixed effects with experience



Notes: coefficients from a regression of individual presidents' standardized fixed effects on covariates. President fixed effects are the president-level residuals of a decomposition of net income on institution, year, and president fixed effects.
* p<0.1, ** p<0.05, *** p<0.01

1.6 Conclusion

In this paper, I study how college presidents and their institutions respond to the financial incentives created by the 2017 Tax Cuts and Jobs Act (TCJA), which introduced a 21% excise tax on nonprofit employee compensation over \$1 million, directly impacting highly compensated university presidents. My findings indicate that the TCJA effectively reduced president compensation by an average of 26 percentage points, while increasing turnover rates by 19 percentage points, resulting in a calculated tax elasticity of compensation of -1.38. This reduction in compensation is largely due to cuts in non-taxable and other forms of compensation, instead of base salaries.

In response to the reduced compensation, college presidents appear to shift their behavior. Following the implementation of the TCJA, presidents increased net income by 30 percentage points and investment returns by 23 percentage points, and directed an additional 9 percentage points toward academic support expenditures. These changes highlight a tendency for presidents to prioritize outcomes that are easily observable and likely to be appreciated by boards, suggesting that presidents focus on generating results that are readily measurable and that align with board expectations.

The TCJA's impact also varies depending on president characteristics, with female presidents experiencing a larger reduction in compensation (around 40%) compared to their male counterparts. Moreover, characteristics such as educational background and prior industry experience are linked to differences in presidential productivity. Specifically, presidents without a PhD or with backgrounds in government or the private sector generally exhibit lower performance, suggesting that the returns to experience within academia are likely to be substantial.

Several limitations should be acknowledged. First, the institutions affected by the TCJA excise tax represent a narrow segment of the higher education sector. The treatment group consists primarily of large, wealthy, and selective private nonprofit colleges with distinct governance structures, financial capacities, and strategic environments. As such, the behavioral responses documented here, particularly the restructuring of compensation and the shift toward board-legible financial outcomes, may not generalize to public colleges or smaller, less well-resourced nonprofits, where leadership incentives and constraints differ significantly.

Second, the analysis is limited by the available data. Compensation information from IRS 990 filings, though detailed, is inconsistently reported across institutions, particularly in non-salary categories such as deferred, in-kind, or "other" compensation. Similarly, financial outcomes drawn from IPEDS may not be fully comparable across institutions due to heterogeneity in reporting practices. Differences in how colleges classify and allocate revenues, expenditures, and functional spending categories, such as academic support or investment income, can introduce measurement error. Finally, while the TCJA provides a plausibly exogenous shock to compensation, the analysis does not allow for direct identification of the mechanisms through which presidents influence institutional performance. The observed shifts are consistent with incentive effects, but the exact mechanisms are not observable in this setting.

The findings of this paper suggest the need for stronger alignment between presidential compensation and institutional mission in nonprofit higher education. At the federal level, policymakers should consider expanding the disclosure requirements in IRS Form 990 filings to include a Compensation Discussion and Analysis (CDA), modeled on SEC requirements for publicly traded firms. This addition would require nonprofit colleges and universities to explain how executive compensation is determined, what performance metrics are used, and how these align with institutional goals related to access, equity, and student success. Federal agencies could also consider mandating the public disclosure of key performance indicators (KPIs) linked to incentive pay, or even requiring publication of executive contracts, provided legal constraints allow this. If the SEC mandates this level of compensation transparency for private firms to protect shareholder interests, nonprofit colleges receiving billions in public subsidies and student aid should be held to at least the same standard to protect the public interest. These transparency measures would allow stakeholders to assess whether leadership compensation is delivering a public return on public subsidies, particularly given the tax-exempt status and substantial federal aid received by many private nonprofit institutions.

At the institutional level, boards of trustees, though harder to regulate directly, should be strongly encouraged to link performance-based compensation to measurable student outcomes. Rather than relying exclusively on financial indicators such as fundraising totals or net revenue, boards could adopt incentive frameworks that reward improvements in first-generation enrollment, graduation rates, net price for Pell recipients, or other mission-aligned metrics already tracked through federal sources such as IPEDS. Accrediting agencies and higher education associations may play a critical role in promoting such standards, helping boards re-center leadership evaluation around long-term student success and institutional equity. Given the evidence in this paper that presidents respond to incentive structures and board priorities, these modest reforms could help redirect presidential focus toward the educational and civic missions at the heart of nonprofit higher education.

These findings open avenues for further research on how presidents answer to their boards, and how institutional objectives are formed in this interaction. Future studies can also explore the dynamics of decision-making within university leadership teams, including the responses of vice presidents and other high-ranking officials to similar incentives. Additionally, examining the role of bargaining and potential discrimination in the selection and compensation of college presidents could yield insights into how leadership characteristics shape institutional performance and equity across executive roles.

2 Cosmetology Gets a Trim: The Impact of Reducing Licensing Hours on Colleges and Students

2.1 Introduction

In the United States, licenses are required for entry into many different occupations, such as cosmetology, massage therapy, nursing, teaching, construction contracting, medicine, and law. Requirements vary by state and by occupation: common elements include a particular level of schooling, passing an examination, a minimum number of hours of training, on-the-job experience, and/or continuing education. In this paper, we consider the impact of requirements for minimum training hours on students and institutions, with a particular focus on cosmetology (commonly known as hairstyling or beauty) programs – the field that requires the highest number of training hours in the largest number of states. We ask how and whether these requirements pay off for students and whether they influence institutional behavior.

There are many different types of licensing requirements across fields and states, but training or instructional hours requirements are the most salient for higher education policy and practice. Institutions offering programs in licensed fields must ensure that they provide sufficient hours to meet state-mandated minimums to allow students to practice in that field upon graduation. These types of requirements have important implications for students. All else equal, students in states with higher training hour requirements will need to stay in school longer than their counterparts. They will not only incur more direct costs in terms of tuition and fees, but they will also incur larger indirect costs: they need more time to complete their credential and must delay the start of their career. These additional costs may be reasonable if higher required training hours translate to higher earnings post-graduation, but it is not at all clear that this is the case. Lowering training hours should lower the cost to students and potentially increase demand for training, but there may be wage penalties if the education does not provide students the skills they need to be successful and practice their trade safely.

We assess the relationship between training hours requirements, tuition, and student outcomes in the field of cosmetology. Cosmetology licensing requirements have been the source of recent debates and policy movement at both the federal and state level. In addition to the state-level reductions in required hours we examine here [1], new federal regulations will require that any programs participating in federal student aid programs not exceed the minimum number of hours training hours required by the state [2]. The law, part of the 2023 Gainful Employment regulations, is currently being challenged in a lawsuit by the American Association of Cosmetology Schools (AACS). [3]

Each year, roughly 200,000 students enroll in more than 1,000 cosmetology schools that are eligible for federal student aid programs [4]. Over three-quarters of them are for-profit institutions. [5] Students enrolled in cosmetology programs are disproportionately women, low-income students, students of color[6], and parents [7], so policies affecting the sector have critical implications for equity. News reports suggest that state cosmetology boards may be requiring an excessive number of training hours in some states [8], and data on the debt and earnings of cosmetology graduates suggest disappointing outcomes for students in this field [9]. A better understanding of the role that required licensing hours play in determining student outcomes is essential for developing higher education policy that is both efficient and equitable. Using data on licensing hours for all 50 states

1. We describe these changes further in Sections 2 and 3.
2. The restriction is part of the 2023 Gainful Employment Program Hours regulations (34 CFR 668.14(b)(26)). Previously, programs could require hours up to 150% higher than those required by state licensing authorities.
3. Spitalniak (2024).
4. These figures from Fast, Granville, Moutrie (2022) count only Title IV-eligible institutions. Many hundreds, if not thousands, more schools operate without access to student aid programs and therefore outside of the Department of Education's purview, making them (and their students) difficult to count. Cellini and Goldin (2014) report that about 136 non-Title IV cosmetology schools operate in Florida, while Cellini and Blanchard (2022) find that more than 700 cosmetology and barber schools operate in the state of Texas. Non-Title IV programs are captured in our analyses of ACS data, but not Department of Education data in College Scorecard
5. Fast, Granville, and Moultrie (2022) document that 77% of Title IV cosmetology schools are for-profit.
6. Fast, Granville, and Moultrie (2022).
7. Obatuase, Cheche, and Fishman (2024).
8. Kolodner & Butrymowicz (2018).
9. See for example, Cellini and Blanchard (2022). We review the literature in detail in Section 2.

and the District of Columbia, we first undertake a descriptive analysis to assess the correlation between licensing hours and both student debt and earnings. We then implement a difference-in-difference design based on six states where licensing requirements were lowered between 2011 and 2017.

Our descriptive analyses suggest that higher licensing hours requirements are associated with higher levels of student debt, but the additional hours and debt that students do not appear to be associated with higher earnings. Our difference-in-difference results based on reductions in hours in several states, show a clear increase in the number of program completions in the four years after a state lowers the required hours. We find that the number of cosmetology certificates awarded more than doubles for programs in states that were subject to a trim in licensing hours. We also find a decline in tuition and fees by about 14% in response to a cut, a result that appears to be driven by among smaller institutions. Our results for student debt and earnings are less precise, but we find suggestive evidence of lower debt in the post-policy years and we observe no decline in earnings. While there we see no impacts on overall enrollment, we see a sizable increase in the enrollment Hispanic and Latino students. Taken together, our results suggest that cosmetology students benefit from the trimming of mandated licensing hours. Section 2 provides background on cosmetology licensing, Section 3 describes our data sources. In Section 4, we outline our dynamic difference-in-differences design. Section 5 considers the correlation between licensing hours, debt, and earnings for all states. Section 6 presents our causal results on the impacts of reductions in licensing hours. Section 7 explores heterogenous effects by college and student characteristics. Section 8 provides robustness checks and Section 9 concludes.

2.2 Background

Occupational licensing has been studied extensively by economists, going as far back as Adam Smith [10]. Theoretical arguments about the efficiency of licensing center around two primary premises. The first is that licensing may ensure quality and protect consumer safety in markets

10. As cited by Blair (2022).

exhibiting asymmetric information or negative externalities. The second is that licensing may generate barriers to entry, restricting worker supply, and putting upward pressure on wages, potentially resulting in welfare losses in competitive markets.

Empirical work has generated evidence on both of these premises. Overall, the research demonstrates that licensing has had little effect on health and safety but does increase wages for licensed workers [11]. For example, Kleiner and Krueger (2013) estimate the wage premium for licensed occupations to be around 18%. More recent estimates by Gittleman, Klee, and Kleiner (2018) suggest that the premium may be lower—around 7.5%. They also find that licensed workers are more likely to be employed and have employer-provided health insurance than those in unlicensed occupations. Nunn (2018) finds that licensed workers have longer job tenure and lower rates of part-time work, but face higher wage inequality than workers in unlicensed sectors. Blair and Chung (2019)use a boundary discontinuity design to show that licensing drives large reductions in labor supply: between 17 and 27%. In other work, they explore the potential for licensing to reduce gender and racial wage gaps, as it may allow firms to rely less on observable characteristics in determining wages Blair and Chung (2021). Empirically testing their model, they confirm that the earnings premium for licensing is higher for Black men, Black women, and White women, relative to White men Blair and Chung (2024) [12].

Many studies have focused on specific fields that require licensing. For example, Kleiner and Park (2010) and Dillender et al. (2024) study the easing of licensing requirements for health care assistants, and they find that the assistants' earnings increase with licensure. More relevant for this report, Timmons and Thornton (2019) study changes to licensing laws for barbers in Alabama and find that de-licensing decreased barbers' average annual earnings and increased the number of cosmetologists in the state. In a study of two related licensed occupations, Deyo (2022) considers makeup artists and shampooers. He compares customer ratings on Yelp for these professions to those of unlicensed pet groomers. He finds that the impact of more rigorous education and experience requirements for licensure has a counterintuitive negative relationship to quality (as

11. For a helpful summary, see House (2015).
12. A helpful summary of this work is contained in Blair (2022)

measured by ratings), but it is not economically meaningful. Adams, Jackson, and Ekelund (2002) study cosmetology specifically, using an instrumental variables strategy to assess whether licensing requirements have a causal impact on the prices of haircuts and a measure of the quantity of cosmetology services consumed in a state. They find that licensing reduces quantity and raises prices for consumers, while generating no improvement in quality.

In the economics of education, a few papers have examined cosmetology programs specifically. Cellini and Goldin (2014) document the large number of for-profit cosmetology schools that operate without access to federal student aid programs. They find that these programs have similar pass rates on licensing exams and charge lower tuition than cosmetology schools that participate in federal student aid programs in Florida. Cellini and Onwukwe (2022) build on this work to look at cosmetology schools in Texas and find that the vast majority of cosmetology schools in Texas do not participate in federal student aid programs. Grosz (2022) documents substantial growth in cosmetology awards in the California Community College system in the late 1990s and early 2000s, despite flat labor market demand for cosmetologists.

In a policy report,Menjou, Bednarczuk, and Hunter (2021) of the Institute for Justice, document the variation in licensing requirements and student costs across states. Fast, Granville, and Moultrie (2022) of the Century Foundation build on this work to document the size, scope, student outcomes, and recent policies affecting Title IV-participating cosmetology programs. They contend that high state licensure requirements contribute to the high cost of cosmetology programs and describe how students perform unpaid work while at the same time paying tuition that benefits cosmetology schools. A key question in policy debates surrounding cosmetology programs is whether underreporting of tipped income can explain the low earnings of cosmetology program graduates. Cellini and Blanchard (2022) explore this question, estimating that earnings are underreported by only about 8

Public debate over cosmetology licensing has been ongoing since at least 2012 when a high-profile court case questioned whether Jestina Clayton, a Utah resident who started an African-style hair braiding business, needed a cosmetology license that would require 2,000 hours of schooling (which was unlikely to cover hair braiding) and cost roughly $16,000 in tuition. [13] The federal judge sided with Ms. Clayton, finding that Utah's cosmetology licensing requirement was "irrational and a violation of her constitutionally protected rights." [14]

In 2018, the New York Times pointed a spotlight on the high debt incurred by cosmetology students due to high licensing hours requirements in some states. [15] The article noted that these programs required students to pay tuition for their hours working in salons—often in excess of 2,000 hours—while at the same time generating revenue from paying customers. In a memorable example in the Times report: Iowa, at the time, required 2,100 hours to become a cosmetologist—more than a full year of 40-hour workweeks. In contrast, emergency medical technicians in Iowa required just 132 hours. [16]

Added to the public scrutiny has been interest from the federal government in holding low-performing institutions accountable for student outcomes. The recent Gainful Employment regulations will likely result in a high proportion of cosmetology programs losing access to federal student aid, based on the high debt and low earnings of graduates.[17] And the Department of Education recently granted debt relief to 28,000 cosmetology students at Marinello Beauty Schools on the grounds that, among other things, the schools "failed to train students in key elements of a cosmetology program, such as how to cut hair." [18]

In this paper, we corroborate some of the patterns licensing hours reported in the media and explore changes that have been made by state cosmetology boards to licensing hours requirements in the last decade. We assess the causal impacts of these changes on institutions and students. Our study is among the first to assess the impact of licensing hours on tuition, enrollment, and student debt and one of only a handful to consider cosmetology licensing, specifically. Our study adds to the literature on licensing and the value of postsecondary credentials. It also generates new

13. See for example, New York Times and National Public Radio. The case was Clayton v. Steinagel, 885 F. Supp. 2d 1212 (D. Utah 2012).

14. Clayton, 855 F. Supp. 2d at 1215-1216.

15. Kolodner & Butrymowicz (2018).

16. https://www.nytimes.com/2018/12/26/business/cosmetology-school-debt-iowa.html.

17. https://apnews.com/article/private-college-debt-gainful-employment-1aa8f695f75d20f2cc6a5d18654808f6

18. https://www.ed.gov/news/press-releases/education-department-approves-238-million-group-discharge-28000-marinello-schools-beauty-borrowers-based-borrower-defense-findings

evidence to inform ongoing policy debates on accountability in higher education.

2.3 Data

We combine state-level cosmetology licensing data with data from the College Scorecard, the Integrated Postsecondary Education Data System (IPEDS), National Conference of State Legislatures (NCSL), and the American Community Survey (ACS).

To identify states that changed licensing hours, we collected our own state licensing data from state legislatures and cosmetology board websites. We reviewed web-based resources for all 50 states and the District of Columbia to identify changes in required hours for cosmetology licensing between 2010 and 2020. We cross-checked our data with point-in-time reports of licensing hours in a report published by the Federal Trade Commission and on third-party websites. [19]

Nine states lowered the required hours for cosmetology licenses in the time period we observe: Utah in March of 2013, Wisconsin in April of 2014, Montana and Nevada in 2015, Colorado and Kentucky in 2017, Idaho and Nebraska in 2018, and South Dakota in 2019. [20] However, to ensure sufficient post-treatment years to measure our outcomes and avoid capturing changes related to the COVID-19 pandemic, we focus our difference-in-difference analyses to policy changes before 2018 and measure outcomes until 2019. A limitation of our approach is that the six remaining states are all western or midwestern, so our results may not generalize to other states or parts of the country.

Table 1 describes the timing, nature, and magnitude of each change. The six states reduced their mandated hours by between 200 (Nevada) and 500 (Montana) hours, with the modal change from 1800 to 1500 hours. Our treatment states have slightly lower tuition and fewer completions than other states, as shown in Appendix Table A2. We have no reason to believe that licensing changes occurred for other occupations simultaneously, as cosmetology is typically governed by its own state board composed of industry stakeholders. Nonetheless, we test for this possibility by

19. https://www.ftc.gov/system/files/documents/public$_{c}omments/2017/07/00020 - 141087.pdfandBeautySchool.org.$

20. We plan to add Idaho, Nebraska, and South Dakota in future analyses when more post-years are available.

running a placebo test for massage therapy programs in Section 8.

Table 2.1: Cosmetology licensing changes, treated states

State	Year of Change	Hours Changed	Change
Utah	2013	2000 to 1600	-400
Wisconsin	2014	1800 to 1550	-250
Montana	2015	2000 to 1500	-500
Nevada	2015	1800 to 1600	-200
Colorado	2017	1800 to 1500	-300
Kentucky	2017	1800 to 1500	-300
Idaho*	2018	2000 to 1600	-400
Nebraska*	2018	2100 to 1800	-300
South Dakota*	2019	2100 to 1500	-600

Notes: Author's data collected from state cosmetology board and legislature websites 2010-2020. See text for details. States with * are not included in our difference-in-difference design, as we do not have sufficient post-policy data.

For our descriptive analysis, we supplement our state cosmetology data with more general data on state licensing requirements from the National Conference of State Legislatures (NCSL). NCSL collects data on about 30 occupations that are commonly licensed in the United States and require less than a bachelor's degree [21]. The data include whether or not each state (and the District of Columbia) requires a license, an exam, and minimum training hours, or other requirements, as of 2020.

Data on tuition, enrollment, and certificate completion comes from the IPEDS. Wherever possible we use program-level data at the 4-digit Classification of Instructional Program (CIP) code-level for code 1204 for "cosmetology and related personal grooming services." [22] Completions and tuition and fees are listed at the program level. Tuition is measured as sticker price tuition and fees for the complete program. Our measure of completion (or awards) is based on the number of students completing the relevant program in the academic year. Descriptive statistics for the IPEDS data are reported in Table 2. There are between 1,300 and 1,600 cosmetology programs in our data

21. Interviews with NCSL staff suggest that the 30 occupations were a convenience sample based on occupations with readily available data.
22. In some cases, we aggregate up from 6-digit CIP codes to 4-digit, weighting by the number of students in each 6-digit program.

in any given year from 2011-2019. Median tuition was around $15,000 per year in 2019 with a median of 17 awards given per program, down from 31 in 2011.

Table 2.2: Descriptive statistics, IPEDS program-level data

		Tuition & Fees		Completions	
Year	Programs	Mean	Median	Mean	Median
2010	1,315	11,723	11,995	47	31
2011	1,403	12,200	12,600	49	33
2012	1,479	12,441	13,100	46	30
2013	1,511	12,703	13,498	41	29
2014	1,576	13,078	13,864	39	25
2015	1,473	13,135	13,800	34	22
2016	1,477	13,472	14,300	33	22
2017	1,501	13,847	14,475	32	21
2018	1,537	14,139	14,860	30	20
2019	1,537	14,435	15,000	26	17

Notes: Author's tabulations of program-level data for CIP code = 1204 (cosmetology).

Program-level data is not available for enrollment, so we identify "cosmetology schools" based on any institution with cosmetology (code 1204) as its largest degree program, by enrollment. The majority of these institutions' names include the word "cosmetology," "beauty," or "salon," indicating that students at these institutions were most likely cosmetology students and therefore impacted by the changes in licensing hours. We use IPEDS full-year enrollment counts, as cosmetology programs often begin off-cycle, in the spring or summer. [23]

To measure student debt, we turn to the College Scorecard data. We append ten rounds of the institution-level data, keeping only institutions with CIP code 1204 listed as their largest program.[24] As in the IPEDS, this restriction creates an unbalanced panel of about 1,900 institutions, and we observe each institution for seven years on average. Our measure of debt—median original

23. Academic years run from July to June in the IPEDS, so our full-year count for 2018-19 is based on enrollments throughout the year running from July 2018-June 2019. We refer to this year as the later year (2019) in our analyses and discussion.

24. Program-level scorecard data is not available until the 14/15-15/16 pooled cohort.

amount of the loan principal upon entering repayment—is pooled across two cohorts and is missing for many small institutions. The pooled nature of the Scorecard data makes the analysis and the assignment of “pre” and “post” years challenging, but since most of these programs are only about one year long, we assume that any cohort that completes a program in a calendar year post policy change would be held to the new, lower hour requirements. We adjust debt values for inflation to bring all values to 2010 dollars. [25] We note that only programs participating in federal student aid programs are counted in IPEDS and the Scorecard [26] and only federal student debt is included in our debt measure, so our analysis represents a lower bound if students take on private debt to cover college expenses. Table 3 reports descriptive statistics for our institution-level College Scorecard data. The vast majority (more than 95%) of our sample are for-profit institutions. By 2019, about 9% of cosmetology schools are in treated states and years in the Scorecard data.

Table 2.3: Descriptive statistics, institution-level College Scorecard data

Year	All	Percent For-Profit	Treated	Percent Treated
2011	1,278	98%	0	0%
2012	1,378	98%	0	0%
2013	1,465	98%	33	2%
2014	1,472	98%	61	4%
2015	1,505	98%	77	5%
2016	1,465	98%	73	5%
2017	1,296	97%	113	9%
2018	1,259	98%	110	9%
2019	1,229	97%	106	9%
2020	1,208	97%	105	9%

Notes: Author’s tabulations of program-level data for CIP code = 1204 (cosmetology).

To measure the earnings of cosmetologists, we draw on data from the Census Bureau’s American Community Survey (ACS) by occupation and state. [27] We identify cosmetologists using the

25. For each of the rounds of Scorecard data, we take the CPI value from the second year to make this adjustment. For example, for the 2010/11 round, we use 2011 CPIs to make the inflation adjustment.

26. See Cellini and Goldin (2014) for counts of cosmetology programs not participating in Title IV programs.

27. The College Scorecard has data on earnings, but it is missing for a large number of institutions and–due to the time lag in measurement–we can only observe changes for the earliest state (Utah), so we rely on the ACS for earnings data instead, but show results for the Scorecard in Appendix Figure 1 as a robustness check.

NAICS code (81211) that most closely matches the 4-digit CIP code for cosmetology (1204). We use hourly wages to capture changes in the cosmetology labor market, as it most accurately reflects cosmetologists' productivity and/or quality, distinct from hours worked. [28] We have about 14 years of ACS wage data, averaging about 6 years in the pre-policy period and 8 years post. In our exploration of mechanisms, we further examine changes in employment and self-employment.

2.4 Methods

To assess the causal impact of changes in state licensure requirements for cosmetologists, we estimate difference-in-difference design with an event study framework following Callaway and Sant'Anna (2021) to address the staggered timing of changes in licensing hours. The event study specification is as follows:

$$y_{pstj} = \alpha + \sum_{j=-5}^{-1} \gamma_j D_{st} I(j = t - T^*) + \sum_{j=0}^{5} \beta_j D_{st} I(j = t - T^*) + \mu_p + \sigma_t + e_{pstj}$$

where y_{pstj} represents the outcome of interest, such as tuition, enrollment, completion, debt, and wages. We estimate effects on completions and enrollment in levels as well as natural logs to adjust for skewness due to a large number of very small cosmetology programs. [29] Subscripts p, s, and t, represent programs, states, and calendar year, respectively. The binary treatment variable, D_{st}, equals one if the state has introduced licensing reforms and zero otherwise. The index, j, indicates time elapsed since the policy change. $I(j = t - T^*$ represents event-year dummies that represent the years before and after each individual state implemented its reform. Our coefficients of interest, β_j, represent the impact of changes in licensure hour requirements in each post-change year on the outcomes, allowing for heterogeneous effects over time. The parameters, γ_j ,indicate pre-treatment effects, allowing for tests of parallel pre-trends. We also include program-by-institution fixed effects (μ_p) to control for time-invariant heterogeneity across programs, and

28. We find no effect on hours worked (see Appendix Figure A5).

29. Models based on the natural log of tuition, debt, wages, and cost of books/supplies, and for each of our heterogeneity analyses yielded similar results to those for levels. We omit them for brevity (available on request).

year fixed effects (σ_t) to control for common changes over time. In our analyses of institution-level IPEDS and Scorecard data (for enrollment and debt, respectively), we replace program fixed effects with institution fixed effects.

Our approach addresses concerns that the staggered timing of state licensing changes may cause treatment effects to vary over time and across states in standard difference-in-differences designs. In the standard approach, when already-treated states act as controls in some two-by-two comparisons and as treated in others, double-counting biases the aggregation of average treatment effects on the treated (ATTs), and some of the ATTs may have negative weights.

We use Callaway and Sant'Anna's approach (2021) to mitigate problems of heterogeneous treatment effects and dynamic effects under staggered timing. The Callaway and Sant'Anna method calculates ATT effects for all possible two-by-two difference-in-difference comparisons between groups of units treated at different points in time. The individual ATTs are estimated by sub-setting the data to only contain observations belonging to either the never-treated group or a given treatment cohort at a given time. It then uses a standard two-way fixed effects design to recover the ATT for that given treatment cohort and time period. These ATTs can be aggregated into further parameters of interest, such as by time period, treatment group, or an overall ATT. We aggregate the ATTs by year relative to the licensing hour reduction to approximate an event study design, using the doubly-robust inverse probability weights based on Sant'Anna and Zhao (2020). [30]

The identifying assumption necessary to support our difference in difference design is that, absent the changes to licensing hour requirements, program enrollment, completion, debt levels, and tuition would have followed the same trajectory for programs in states that changed requirements and in states that did not. To test the plausibility of this assumption we test for parallel pre-trends for states with and without changes, based on parameters, γ_j, using our event study approach. While we cannot test directly whether the other policy changes affecting outcomes may have taken place in the same year, we have no reason to believe that the timing of changes to licensing hours

30. This weighting allows the most robust outcomes to any misspecifications (Sant'Anna and Zhao 2020).

would be correlated with changes to other state or federal higher education policies more generally, as licensing boards are separate from higher education systems and cosmetology is just one of many hundreds of higher education programs in any given state. It is, however, possible that these states had high-profile legal battles (such as that described above in Utah) or faced other types of scrutiny in the lead-up to the policy change. If these changes were anticipated by students or institutions, we would likely see differential pre-trends in the years leading up to the policy change and we would expect our estimates to be attenuated. We investigate pre-trends and other identifying assumptions further below.

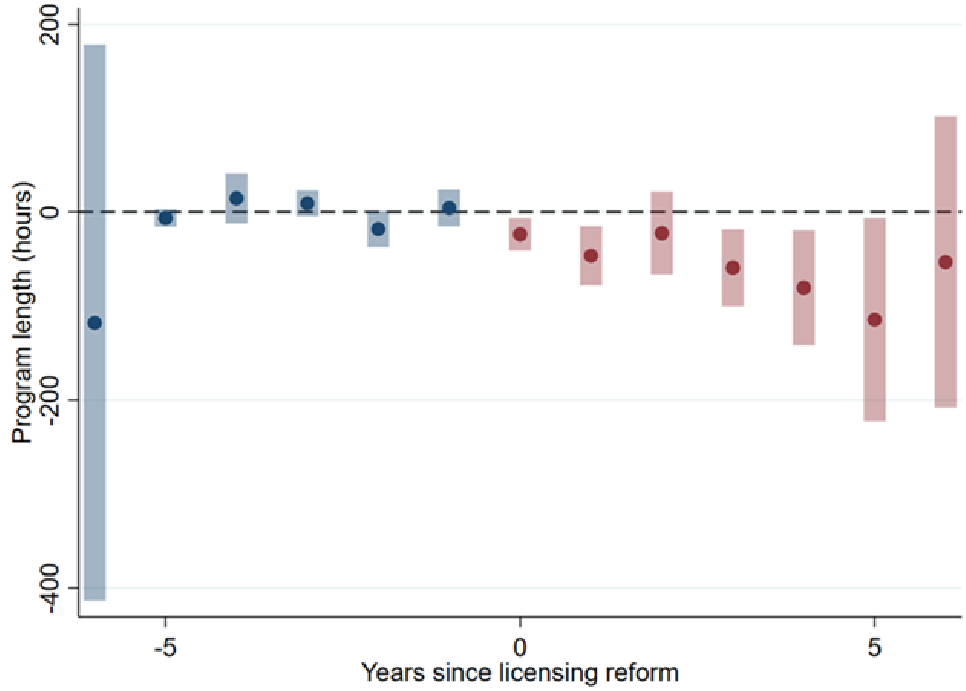
To test whether changes in licensing hours translate to changes in program hours in the first stage, we run our model in equation (1) with program hours from the IPEDS data as the outcome. The results of this analysis are presented in Figure 1. Pre-trends are not significantly different from zero and all are close to zero in magnitude. In the post-policy period, the decline in program hours is evident in nearly all post-reform years, averaging about a 100-hour decline by year five.

2.5 Descriptive analyses

To understand how cosmetology compares to other fields of study, our previous work documents that among all fields requiring occupational licensing in the U.S. as of 2020, cosmetology had the highest mean and median hours at 1,550 and 1,500, respectively (Acevedo, Blanchard, and Cellini 2022, Table 1, reproduced in Appendix Table 1A). Across states, the minimum number of hours required for a cosmetology license is 500 and the maximum is 2,100. All states and the District of Columbia have minimum required hours. Cosmetology contrasts with other occupations that show more variation in the number of states that require licensing and in training hours. For example, 32 states require licenses for electricians with just 7 of them requiring minimum training hours. Plumbers have high mean hours at 1,541 and a maximum of 8,048, but median hours are zero, since more than half of the states that require licensing for these occupations require no training hours.

Following our previous descriptive work, we consider the correlation between required training

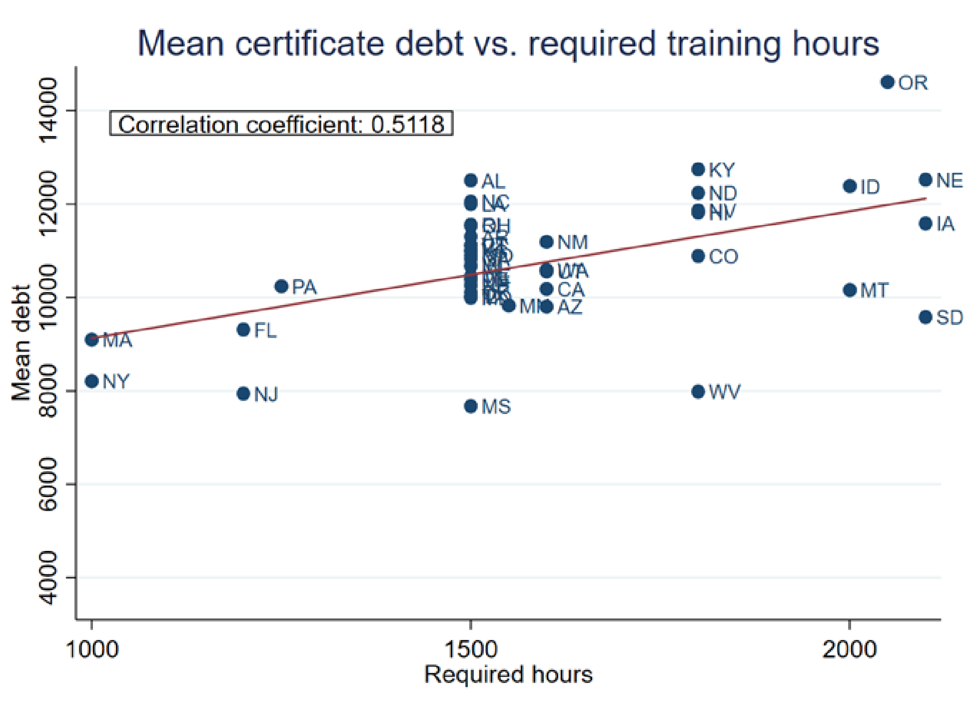
Figure 2.1: Impact of reduction of licensing hours on program length (in hours)



Notes: IPEDS program-level data.

hours and student outcomes for cosmetology programs in Figures 2 and 3. Figure 2 shows the correlation between mean licensing hours for cosmetology and median debt at the state level. The figure shows a strong upward sloping relationship. Higher licensing hours in a state are associated with higher median debt, with a correlation coefficient of 0.5118. On the low end of licensing hours and debt are Massachusetts and New York, while Oregon, Nebraska, and Iowa are among the states with the highest hours and debt. In Figure 3, we examine whether the higher debt associated with cosmetology licensing hours might generate higher wages, using ACS data by state. Licensing hours do not appear to be correlated with wages. The line of best fit between hours and wages is flat—around just $20 per hour—with a correlation coefficient close to zero, just -0.0047. While we caution that these estimates are not causal, the pattern suggests that high licensing hours requirements in this field may raise debt without generating additional productivity gains. We explore whether the relationship between licensing hours and debt and wages are causal in our differences-in-differences analyses.

Figure 2.2: State-level correlation between licensing hours and mean debt, cosmetology certificate programs

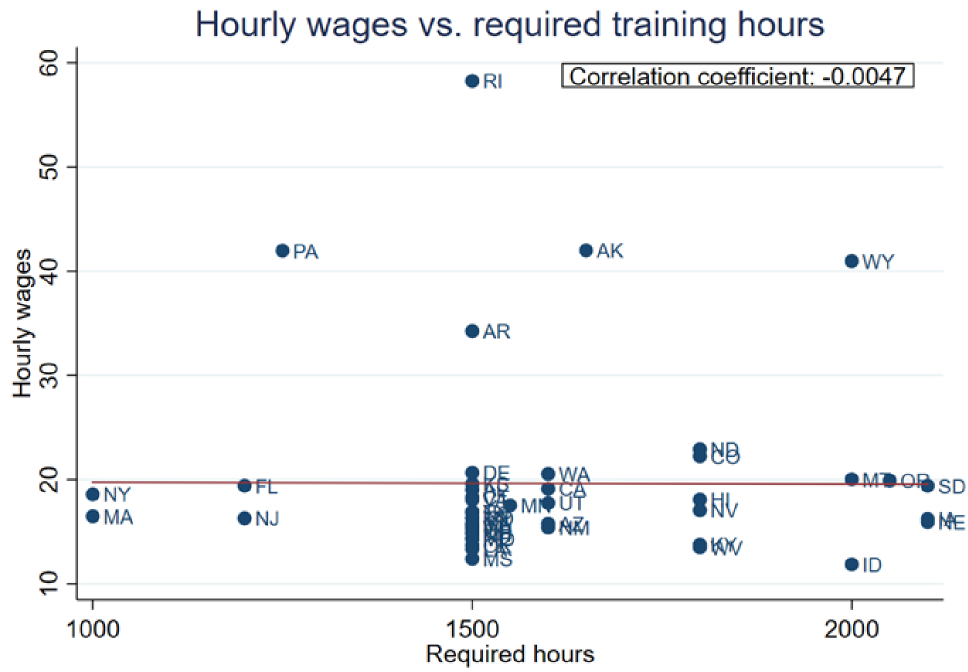


Notes: IPEDS program-level data.

2.6 Results

To explore the causal impact of licensing hours on outcomes we draw on reductions to licensing hours within states and over time, as estimated by equation (1) and described in Section 4. We display the results of the event study analysis on the total number of program completions in Figure 4A. Importantly, there appear to be no significant differences between treated state-years and controls in the six years pre-period and estimates are all very close to zero. In the post-policy period, we see an increase in awards, as predicted. Point estimates are positive and significant,

Figure 2.3: State-level correlation between licensing hours and hourly wages, cosmetology certificate programs

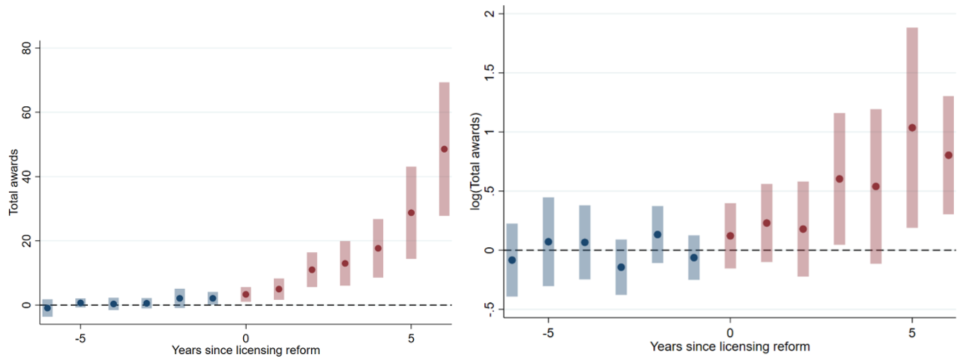


Notes: IPEDS program-level data.

rising to about 30 to 50 more awards granted per-program in states with lower hours in years five and six. The year-five increase represents a doubling of awards over the 2010 control group mean of 30. We show results for the natural log of completions in Figure 4B. Results are similar, showing effect sizes above 50 log points (or 65 percent) for years 3 through 6 after the change.

The impact of reduced licensing hours on required tuition and fees is shown in Figure 5. Here, we again see pre-trends that are very close to zero, with just one significant difference in the second year before deregulation. We see significant changes in tuition three and four years after states lowered required training hours. Point estimates suggest effects a decline of about $1,000 in year four—or about a 14% decrease relative to the 2010 mean of $7,176. Point estimates remain similar in year five, but less precise as our sample shrinks. Lower tuition likely reflects the lower

Figure 2.4: Difference-in-difference estimates of the effect of reductions in licensing hours on cosmetology program completions



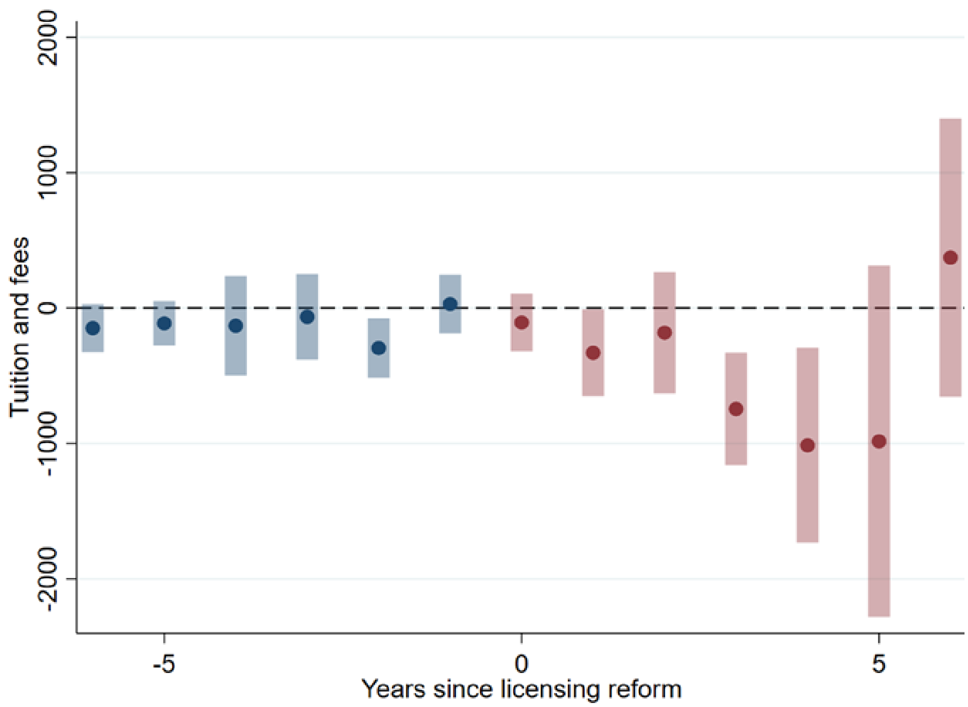
Notes: IPEDS program-level data.

hours needed for completion in the post-policy period. By year six, although imprecise, the point estimate returns to positive, suggesting that tuition changes may be temporary.

We also consider student enrollment as an outcome, using the IPEDS institution-level data, rather than program-level, in Figure 6. Our results are noisier, perhaps in part due to the lack of precision in the data and we find noisy zero estimates on total enrollment in Panel A. The results of the natural log of enrollment are stronger in Panel B, showing an upward trend in log enrollment, of roughly 0.2 to 0.5 log points (or 22 to 65%) in years 3-5. This difference may be attributed to extreme heterogeneity in institution size. Some very small institutions with zero enrollment are dropped in the log specification and larger institutions may expand enrollment more. We explore enrollment heterogeneity by institution size and student demographics in Section 8 below.

We next use the College Scorecard institution-level data to explore the effect of reductions in licensing hours on debt. In Figure 7 we run the specification in equation (1) on institution-level data for schools with cosmetology as their largest program. First, we observe a positive pre-trend in year j=-4 prior to the policy change for the treated states, suggesting that debt was higher in states that changed. The post-policy period shows no significant effects and standard errors are large,

Figure 2.5: Difference-in-difference estimates of the effect of reductions in licensing hours on tuition and fees

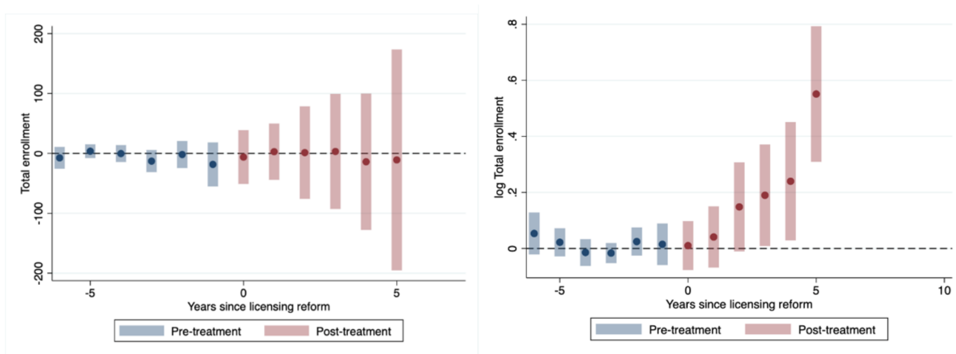


Notes: IPEDS program-level data.

although point estimates are consistently negative, suggesting reductions in debt of around $300-500 in years 1-4 post-policy. Although inconclusive, the direction of the estimates is consistent with expectations based on our findings for tuition: with tuition and supply fees declining, students should need to take on less debt.

We turn to the ACS data to study effects of the reduction in licensing hours on cosmetologists' wages in Figure 8. In the post-reform period, we see little impact on wages. The coefficients for years 3 and 5 post-policy reveal small but significant increases, yet the year 6 coefficient indicates a small negative effect. We view these results as evidence that wages do not substantially decrease, as might be expected from the cut in licensing hours. If the productivity or quality of cosmetologists were declining with more lax standards, we would expect to see large negative impacts. The

Figure 2.6: Difference-in-difference estimates of the effect of reductions in licensing hours on enrollment



Notes: IPEDS program-level data.

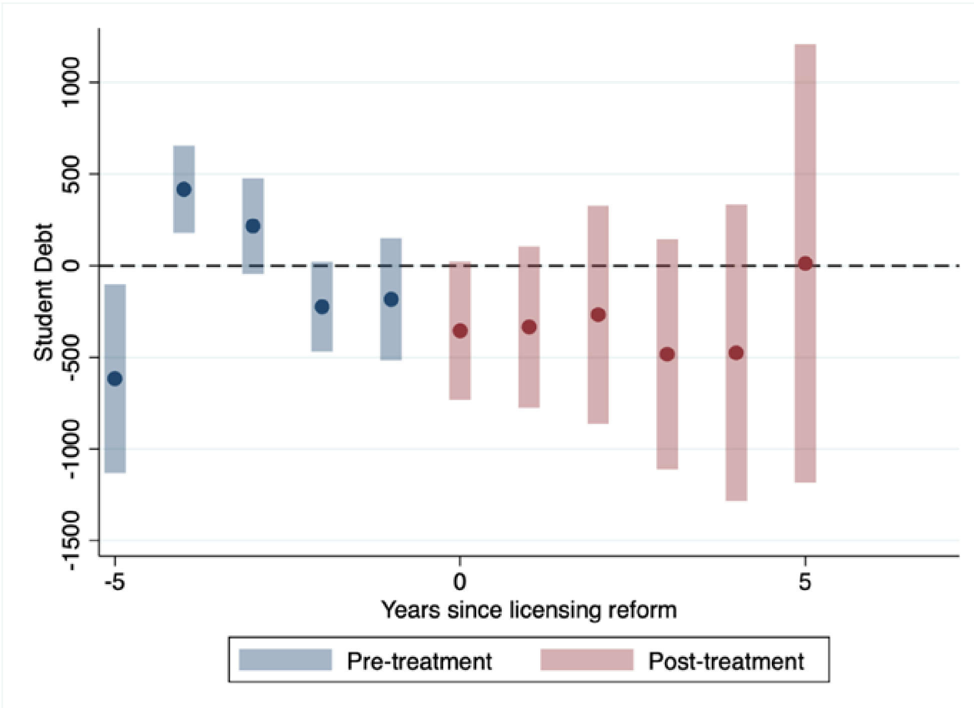
increased supply of cosmetologists as program completions rise would also put downward pressure on wages, but we find no evidence that this is the case—at least in the first 5 years after the policy change. We explore these results further with several robustness checks in Section 8.

2.7 Heterogeneity analyses

Our data allow for some disaggregation by institution and student characteristics to assess heterogeneous impacts. We first consider differences by institution size. Figure 9 shows results for completions among the set of larger (above median enrollment) and smaller cosmetology schools. In panel A, larger institutions appear to drive the positive impacts on awards we see in the full sample. Smaller institutions see smaller increases, as might be expected, as completions are measured in levels.

In Figure 10, we assess differences in the tuition adjustment for large and small institutions and our results flip: institutions above the median size show more limited declines in tuition and fees, while smaller institutions seem to adjust sharply, potentially driving the impacts in the full sample. Small institutions show reductions of about \$3,000 in tuition five years after the policy change (about a 25% reduction relative to the 2010 mean). Taken together, our results by institution size

Figure 2.7: Difference-in-difference estimates of the effect of reductions in licensing hours on student debt

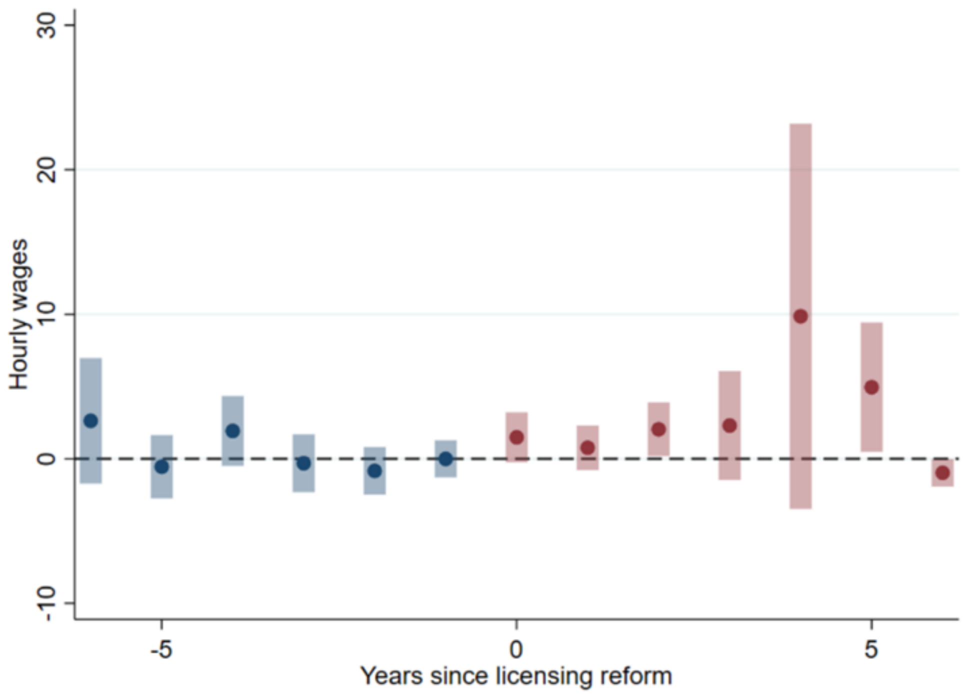


Notes: IPEDS program-level data.

suggest that larger institutions—with higher awards and fewer tuition cuts—may suffer less harm from licensing changes than smaller institutions.

Turning to student demographics, the IPEDS data allows us to look at enrollment by race and ethnicity, as well as by gender. While the impact of licensure reductions on total enrollment was null in Figure 6, in Figure 11 we observe some changes in enrollment for students for some subgroups. In Panel A of Figure 11, the coefficients on enrollment of Black students are fairly large and positive, although imprecise, two and three years after the policy change. In Panel B, enrollment among Hispanic and Latino students shows sizable significant positive impacts. Our estimates suggest that licensing hour reductions increase the enrollment of Hispanic and Latino students by up to 40 students per institution in year five. Results for female enrollment (shown

Figure 2.8: Difference-in-difference estimates of the effect of reductions in licensing hours on hourly wages of cosmetologists

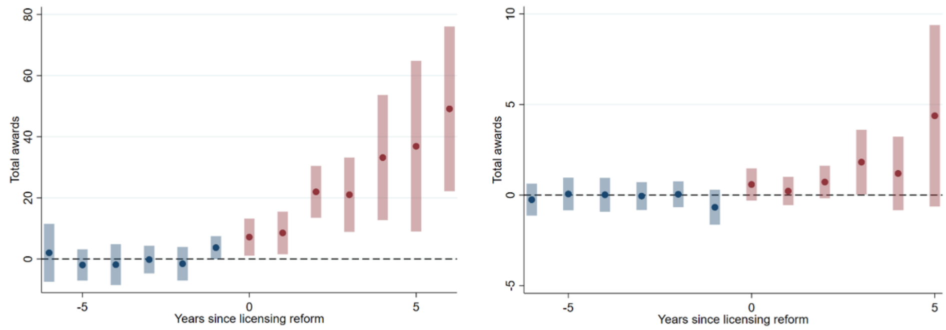


Notes: IPEDS program-level data.

in Appendix Figure A1) are nearly identical to results for the full sample, showing no discernable effects of the change. The similarity is expected since women comprise about 90% of enrollment in cosmetology programs.

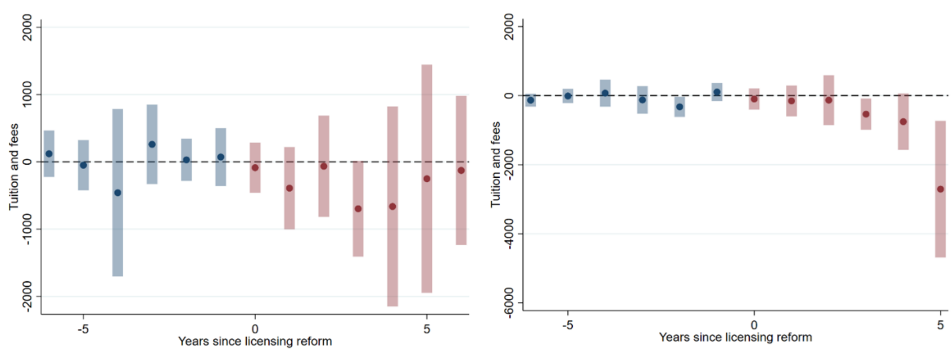
While we prefer our event study analyses, we report average effects pooled across all post-policy years for each of our primary outcomes in Table 4. Only the effects on completions remain significant when considering average effects. Tuition and fees and debt are both negative, as expected, and remarkably similar in magnitude, despite large standard errors.

Figure 2.9: Difference-in-difference estimates of the effect of reductions in licensing hours on completions, by institution size



Notes: IPEDS program-level data.

Figure 2.10: Difference-in-difference estimates of the effect of reductions in licensing hours on tuition and fees, by institution size

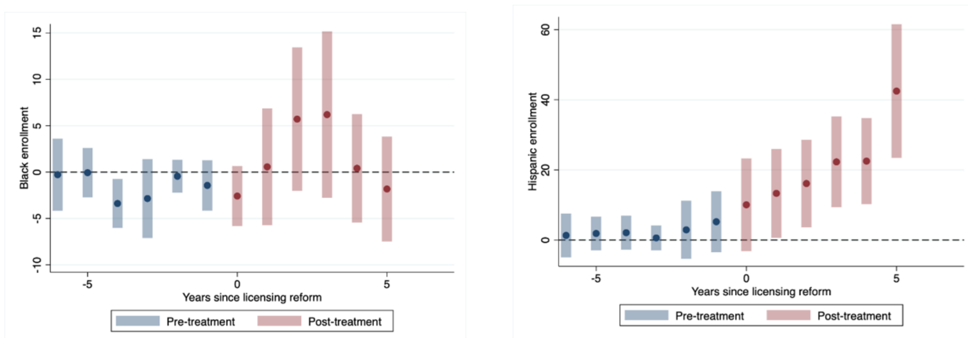


Notes: IPEDS program-level data.

2.8 Robustness checks and placebo tests

We run several robustness checks on our main results. First, we limit the sample to the longest cosmetology programs (over one year long) that should be most affected by the change in hours. Results are qualitatively similar, but less precise with the smaller sample. We also limit our sample

Figure 2.11: Difference-in-difference estimates of the effect of reductions in licensing hours on completions, by student demographics



Notes: IPEDS program-level data.

Table 2.4: Average treatment effects: impact of reducing cosmetology licensing hours

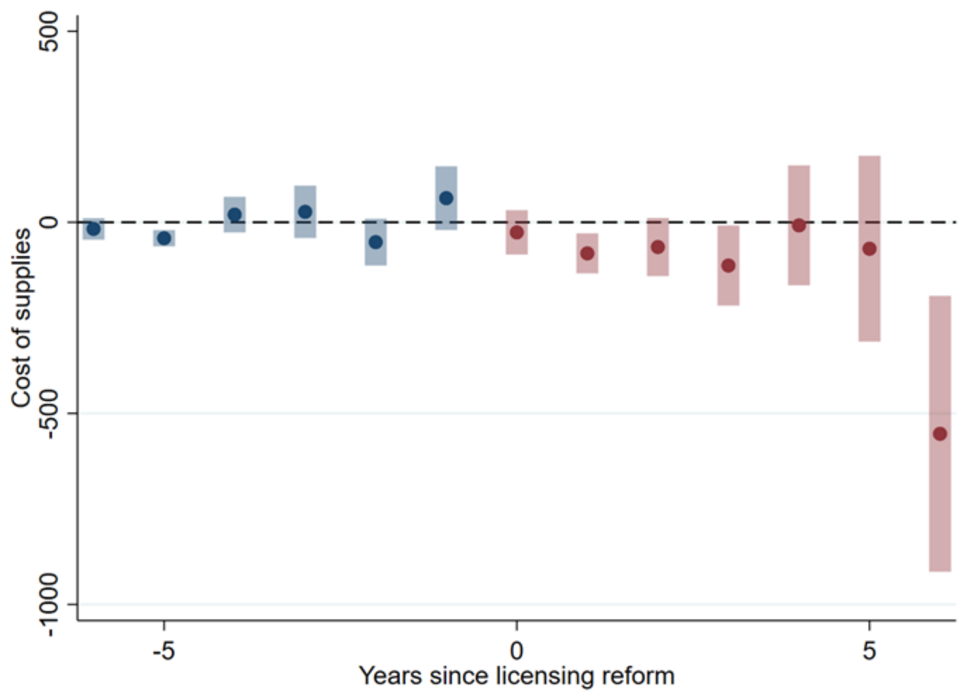
	Awards	Log Awards	Tuition & Fees ($)	Debt ($)	Enrollment	Log Enrollment
ATT	7.211***	0.212**	-316.3	-290.7	-0.605	0.055
	(1.803)	(0.072)	(176.1)	(171.7)	(21.48)	(0.040)

Notes: *** p<0.01, ** p<0.05, * p<0.1. Estimates are based on the average of all post-treatment years and separate regressions for each outcome, with controls as described for equation (1).

to for-profit institutions (which constitute more than 95% of our Scorecard sample, as shown in Table 2) and we find nearly identical results. We also run all models on the natural log of each outcome and find similar results.

To further explore the impacts of licensing hours on student costs and institutional behavior, we ask whether the cost of supplies might be impacted by licensing changes. If supplies must be purchased for each semester or get used up (e.g., shampoo, hair color) then students should have lower supply costs. On the other hand, institutions may increase the costs of supplies (e.g., scissors, books) to compensate for lost tuition revenue. In Figure 12, we observe a small reduction in the cost of supplies in year 1 and 3 post-policy and a larger drop in year 6. It is possible that the drop in year 6 mediates the impact of the rising tuition we observe in the same year. In general, the pattern aligns with the tuition results in showing lower costs for students in the years immediately following the reduction in hours.

Figure 2.12: Difference-in-difference estimates of the effect of reductions in licensing hours on cost of supplies



Notes: IPEDS program-level data.

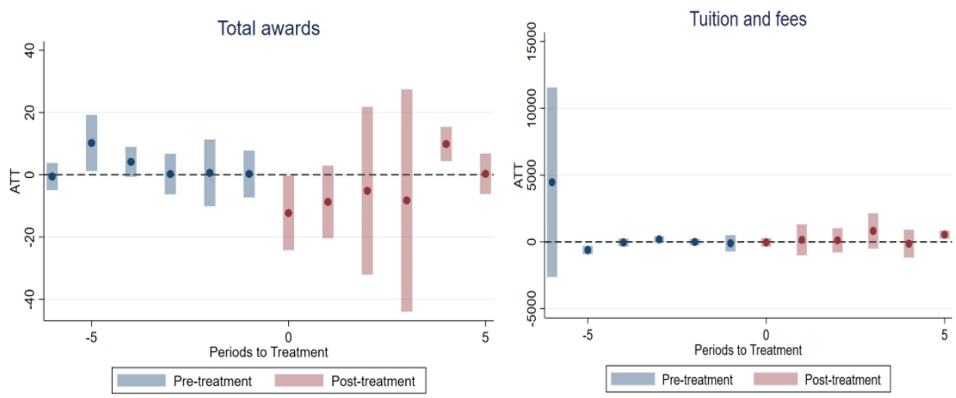
We next assess whether the licensing changes shifted the composition of programs in our data, which would indicate changes to the market and could potentially drive other outcomes. We proxy for program entry and exit based their appearance in the IPEDS program-level data. If a program appears in year t, but not in t-1 or t+1, we code it as entering or exiting, respectively. As shown in Appendix Figure A2, we observe no significant impacts on entry or exit in any year. Not only is composition not driving our results, but the licensing changes do not appear to have significant impacts on the openings and closings of cosmetology programs generally.

To better understand our wage estimates in the ACS, we draw on College Scorecard earnings data and explore employment and hours worked in the ACS. We first run our difference-in-difference specification on the College Scorecard earnings data in Appendix Figure A3. The Scorecard includes data on graduates' earnings six years after beginning their cosmetology program, but only for combined two-year cohorts. Due to these limitations, we can only observe post-treatment years for the state with the earliest reductions in licensing hours—Utah in 2013. We find roughly parallel pre-trends and a statistically significant positive impact on earnings in the first year after implementation. Although this estimate is specific to this state, the results support the findings of our ACS earnings analyses: that cosmetologists' earnings did not decline and may have increased in the first few years after the policy change.

We next turn back to the ACS to consider other labor market impacts of the change in cosmetology licensing that might mask or drive changes in wages. In Appendix Figure A4, Panel A, we see no discernable pattern in employment of cosmetologists after the policy change. In Panel B, we see no impact on the percentage of cosmetologists self-employed. Appendix Figure A5 considers changes to the hours worked by cosmetologists. We find no impacts in years 1-4, but sizable negative and positive effects in year 5 and 6, respectively. We consider these results inconclusive. Like the College Scorecard data, the year 5 and 6 estimates in the ACS are likely driven by changes in a single state (i.e., Utah) that has sufficient post-policy data for identification.

Finally, we conduct a placebo test on massage therapy programs. Massage therapy programs tend to have similar student demographics, similar program lengths, and a similar mix of for-profit and not-for-profit institutions, yet they should not be affected by changes to cosmetology licensing hours. We draw on the program-level IPEDS data to run our test on our two main outcomes with significant effects: completions and tuition. Figure 13 shows the results of our placebo tests. The results for completions in Panel A are noisy, with one post-policy coefficient positive, but small in magnitude. Tuition and fees for massage programs in Panel B, show a tightly-estimated zero in the post-period, making us more confident that the changes we observe for cosmetology programs are indeed effects of the policy change.

Figure 2.13: Difference-in-difference estimates of the effect of reductions in cosmetology licensing hours on massage therapy completions and tuition and fees



Notes: IPEDS program-level data.

2.9 Discussion and conclusion

This paper explores the role of minimum training hours requirements for cosmetology licensure on students and institutions. We implement a difference-in-differences design based on six states that lowered the number of hours required for licensing between 2011 and 2017.

Our difference-in-difference analysis reveals striking patterns and suggests that lowering required training hours requirements has significant benefits for students. Lowering hours results in more program completions, likely due to the lower time cost of certificate completion. We also find that cutting licensing hours lowers tuition, so students also benefit from a reduction in out-of-pocket monetary costs.

Our results on total enrollment are suggestive of positive effects and we see large positive enrollment effects for Hispanic/Latino students, in particular. While more research is needed to understand reasons for this pattern, it may be that Latino students are more sensitive to the lower time cost of education, as ethnographic research has documented that Latina students are sensitive to family obligations in the college choice process (Dache-Gerbino, Kiyama, and Sapp 2018).

Moreover, Hispanic/Latino students may be disproportionately recruited into vocational proprietary institutions (Dache-Gerbino, Kiyama, and Sapp 2018) and targeted by advertising (Cellini and Chaudhary 2022), which may make the hours changes more salient for this group. Our results on student debt and wages are noisy. However, we find suggestive evidence that student debt declines, with average treatment effects similar in magnitude to the declines in tuition. We would expect that the reduction in licensing hours would cause wages of cosmetologists to decline as more workers enter the market. Moreover, if additional training hours improve the quality or productivity of cosmetologists, then cuts to training hours should lower productivity or quality. In the first five years after the change, we observe no reduction in hourly wages for cosmetologists, and perhaps an increase. We find a small negative effect in year 6, which may be driven by one state. We find no evidence that hours worked, employment, or self-employment are impacted. Better data is needed to more accurately assess the impacts of licensing hour changes on these outcomes.

It is not clear how institutions fare with the change in hours. Institutions reduce their tuition for cosmetology programs in the short run and do not appear to compensate for those reductions by increasing the costs of books and supplies. However, we find some evidence that reductions dissipate in the longer run. Larger institutions generate more completions than smaller institutions, while holding tuition relatively stable. Smaller institutions lower tuition more while seeing less growth in completion. More research needs to be done to fully understand impacts on institutions, but our analysis suggests greater benefits—or at least less harm—for larger institutions, relative to smaller.

More research is needed to understand longer-run impacts of licensing hour reductions in cosmetology. It is possible that reductions in licensing hours could reduce salon safety. Still, we note that even with the reductions in required training hours that we document, the hours required to become a licensed cosmetologist remain higher than in many fields for which safety may be a bigger concern, such as emergency medical technicians and electricians. Data limitations kept us from assessing the causal impacts on safety, but we suggest this as a fruitful avenue for future research. More research is needed to explore the causal relationship between licensing hours and student outcomes in other fields, states, and time periods.

We recommend that policymakers concerned about student outcomes in higher education carefully consider the role of state-mandated training hour requirements in cosmetology. We find clear evidence that students benefit from lower hours requirements. These benefits may ultimately generate welfare gains if earnings stay stable in the long run and safety is not compromised. By lowering the time and monetary cost of certification and bringing down student debt without generating declines in wages, cuts to required hours may also help cosmetology programs pass new accountability metrics based on debt-to-earnings ratios of graduates. If increasing credential completion and lowering tuition are goals of state and federal policymakers, then trimming cosmetology licensing hours appears to be a promising strategy.

3 Short-term impacts of emergency financial aid on the academic outcomes of college students

3.1 Introduction

The role of financial aid in alleviating credit constraints and ensuring access to higher education is well documented across the literature (Dynarski, Page, and Scott-Clayton (2023)). However, most financial aid is focused on meeting the direct costs of higher education and ignoring non-tuition expenses, such as housing, food, or transportation. Even if financial aid is structured as unconditional grants, such as Pell Grants or various state-level grants, the implicit assumption behind them is that they allow students to meet these non-tuition expenses adequately. However, in the presence of negative income shocks, students may no longer able to meet these expenses, inducing entry into the labor market and reducing academic effort, leading to reduced academic achievement.

Emergency financial aid administered by individual colleges seeks to help students smooth negative income shocks by providing in-kind or direct monetary assistance, but evidence on the effectiveness of these programs is scant. However, the emergency response to the COVID-19 pandemic by the US federal government, authorized by the 2020 CARES Act, included funding directed to ensure that college students retained access to food, housing, technology, and childcare, providing a large-scale experiment on the effects of emergency financial aid. While this funding and is also administered by individual colleges, it was designed as a direct cash transfer, allowing students to prioritize their needs as they saw fit.

I examine the short-term effects on academic and labor market outcomes of the US federal government emergency aid to college students using administrative data from a large public university system and its aid assignment rule. The Higher Education Emergency Relief Fund (HEERF), part of the CARES Act, directed colleges and universities to transfer at least 50% of their assigned funding to students, providing them with a range of discretion in designing their emergency aid programs beyond federal eligibility constraints. The university instituted a system of universal base grants for all students who filed the FAFSA, plus a system of cumulative bonuses based on strict cut-offs on expected family contribution (EFC) as a measure of economic need which were not known ahead of time by students. This program design allows the use of a regression discontinuity design to estimate the effect of an additional level of grant aid.

I evaluate the impact of this policy using a regression discontinuity analysis, leveraging the discontinuities in aid generated by expected family contribution (EFC) levels to estimate the effects of changes in aid category, equivalent to an additional \$100 on average, in short-term academic outcomes. This approach has the advantage of providing unbiased estimates of treatment effects, at the cost of restricting the policy effects that can be identified. This regression discontinuity analysis finds no meaningful academic impacts at the margin of grant categories. These results are robust to a variety of functional form choices, estimation methods, and bandwidth choices, further confirming this null result. These results are not necessarily surprising, given the relatively small changes in aid at the thresholds.

This is one of the first papers to analyze the effect of large-scale emergency financial aid. The closest related study is the work by Evans et al. (2019) who find no statistically significant results of access to emergency financial aid in degree completion or academic performance as part of a larger scale RCT involving counseling and financial aid in a Texas community college. However, the estimates on enrollment may be compared favorably to the effects of larger grant programs, raising the case for a very favorable cost/benefit ratio for emergency interventions. For example, the meta-analysis by Nguyen, Kramer, and Evans (2019) finds that access to financial aid increases persistence and completion by 2-3 percentage points, totaling on average 1.5-2 percentage points per \$1.000 of grant aid. Additionally, its effects can also be related to the existing literature on the negative effects of related administrative frictions. Since this emergency aid was directly deposited to eligible students without application requirements, and did not crowd out previously received financial aid, some of the estimated effects may be attributed to this feature of this policy. Furthermore, this paper contributes to a large and growing literature on the effects of social policy mechanisms related to direct cash transfers during the pandemic, such as Parolin et al. (2022) on the effects of the CARES Act on poverty rates, or the effects of the Child Tax Credit on food insecurity (Parolin et al. (2021)) or on employment (Ananat et al. (2022)). Given the proportion of college students that are eligible for other forms of social assistance beyond educational financial aid, it is worth analyzing the role that colleges and financial aid programs may have in linking students to additional resources that can provide access to additional inputs that ensure persistence and graduation.

My results inform two important dimensions of the design of emergency financial aid. In first place, these results points towards the trade-off between universal versus targeted aid. As the effects of this policy were concentrated mainly in higher-need students, it may be possible to achieve a better cost-benefit ratio if aid is targeted to these students, instead of the null effects of relatively small amounts found in previous literature, a null result corroborated in this large-scale program by the regression discontinuity approach. In second place, these effects point to the importance of implementation, such as timing and friction minimization, given the favorable cost-benefit ratios implied by the individual fixed effects model compared to the effects per dollar on persistence found in other studies.

3.2 Related literature

Public involvement in higher education can be justified by the existence of individual, sectoral, and social returns to investments in higher education that are not realized due to frictions such as liquidity constraints or behavioral factors (Dynarski, Page, and Scott-Clayton (2023)). Furthermore, financial aid is geared towards ensuring aspects beyond access, such as persistence and completion. In the case of persistence, financial aid allows students to smooth negative income shocks without inducing entry into the labor market to compensate for lost income, along with an associated reduction in credit hours or even a full exit from college. Most available evidence points towards financial aid, both as loans and grants, as being effective in ensuring persistence and graduation. In the case of grants, Denning, Marx, and Turner (2019) leverage discontinuities in eligibility for the Pell Grant using Texas administrative data, finding that increases in grant amounts led to increased completion rates and higher earnings. However, in the case of community colleges, Anderson and Goldrick-Rab (2018) find that randomly assigned additional aid at two-year colleges in Wisconsin fails to increase college attainment, pointing to the role of complexity also highlighted by Page and Scott-Clayton (2016).

Most of the financial aid programs under review are monetary transfers aimed towards paying for direct educational expenditures, such as tuition, with cost of living allowances varying widely and being lower for less selective institutions (Libassi and Mabel 2022). However, the cost of living, broadly construed, also covers educational inputs such as food and housing that are directly affected in the case of a negative income shock. However, evidence on the effectiveness of emergency financial aid, aimed at stabilizing these inputs, is scant. One of the first studies on this issue was carried out by Geckeler (2008), who researched the effects of emergency financial aid in 37 community colleges, and characterized recipients as more likely to be older, parents, first-year students, enrolled full time, in vocational study, and recipients of other financial aid. Kruger et al. (2016) describe institutional emergency aid programs, highlighting their widespread existence, especially as in-kind programs such as food pantries and vouchers, a need for additional resources, and the relative obscurity and lack of information about these programs among students. To date, the only causal analysis of the effects of emergency financial aid is Evans et al. (2019). By leveraging a randomized control trial of the Stay the Course Fund, implemented in the Tarrant County Community College in Texas, they implement an encouragement design into a program consisting of both counseling and financial aid or only financial aid. The authors find no statistically significant results of access to emergency financial aid in degree completion or academic performance but do find positive effects for the full intervention.

Given the positive effects of pandemic social programs on issues such as nutrition and housing, it is possible to expect links with academic outcomes, as the crucial role that inputs such as nutrition (Anderson and Goldrick-Rab 2018, Lundborg, Rooth, and Alex-Petersen 2022, among others) and housing (Chetty, Hendren, and Katz 2016, among others) is well documented in K-12 education. However, evidence on the effect of specific improvements in these inputs in higher education is scant, as financial aid for higher education is usually distributed directly as funding which students are free to spend according to their priorities. However, there is also an emerging literature detailing the positive effects of pandemic-driven direct cash transfers on consumption, poverty, and welfare, leading to positive expected effects on education as well. With respect to the effects of cash assistance on poverty, Parolin et al. (2022) quantify that the CARES Act response reduced the poverty rate by around 5.6 percentage points in April 2020, with families and more vulnerable populations gaining more benefits from transfers. Parolin et al. (2021) find that the subsequent expansion and reorganization into monthly payments of the Child Tax Credit (CTC) reduced food insecurity by 7.5 percentage points, or 25 percent, with effects concentrated on households earning less than $35.000, while Ananat et al. (2022) find that this policy did not have negative effects on employment.

3.3 Institutional details and sample

The setting under study is a large public university system in the United States Northeast region, with more than ten affiliated 4-year and 2-year colleges. This large public university system provides administrative data at the student-term level on enrollment and academic outcomes. I focus on short-term outcomes for the Fall 2020 semester: enrollment, number of credits attempted and passed, and GPA for the semester. The University also provided administrative student-year data related to FAFSA applications such as EFC levels and Pell Grant awards and disbursements, as well as additional information on further financial aid, such as state-level grants and other loans. I restrict my sample to full-time students in Spring 2020 enrolled in 4-year programs who filed a FAFSA and have information on their expected family contribution (EFC), leading to a sample of 62.082 students. I focus on students enrolled in four-year programs to be able to compare the programs' effectiveness with the literature on similar estimates of the effectiveness of financial aid, which focuses on four-year programs.

The University accessed the previously described HEERF emergency funds in late April 2020, receiving approximately \$ 237 million dollars across all campuses. Of this amount, \$118 million were directly allocated to students as emergency financial aid. Individual grant amounts vary across campuses, even for individuals with the same level of need, as the allocation rule used by the Department of Education is based on the share of Pell Grant recipients and total number of enrollments. As a result, the unconditional grant base rate varies across colleges, from \$356 to \$484. For a student to be eligible to receive a grant, they must be eligible to receive any form of Title IV financial aid. This eligibility requirement is automatically fulfilled if a student has filed the FAFSA or is eligible to do so. As a result, a large majority of students received at least the base rate corresponding to their college. Most funds had been distributed at the end of May 2020, after the end of the term, so effects on Spring 2020 performance are negligible. This process was automatic, as all students who had filed the FAFSA received their grants automatically via direct deposit in their bank accounts, regardless of their attendance status. Students who were ineligible to file the FAFSA and receive this grant were encouraged to seek private emergency grants.

The funds, according to the University, were intended to 'cover expenses such as food, housing, course materials, technology, health care, and child-care expenses' during the pandemic emergency. In theory, these additional resources ought to relax students' budget constraints, allowing them to focus on degree completion and academic performance instead of working for wages or caring for family members. In practice, if the grant is small enough compared to outside sources of funding, such as student loans or earnings, it may fail to affect academic outcomes.

Although all students who filed a FAFSA received at least an amount equivalent to the base rate, students facing additional expected need received additional resources. This expected need is proxied by expected family contribution (EFC) levels. The assignment of additional funding on top of the college-dependent universal base grant follows a discontinuous assignment rule across the entire system, as follows:

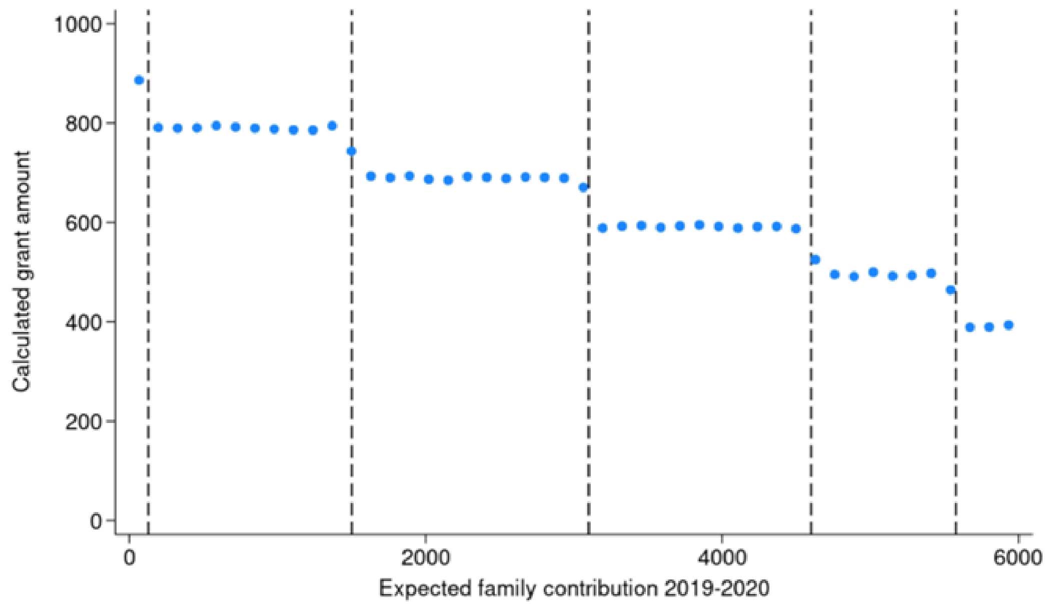
- EFC greater than \$ 5576: college-dependent base rate.

- EFC between \$4600 and \$5576: 25% bonus.
- EFC between \$3100 and \$4600: 50% bonus.
- EFC between \$1500 and \$ 3100 : 75% bonus.
- EFC between 0 and \$ 1500: 100% bonus.
- EFC of zero: 125% bonus.

Base rates and thresholds are defined differently for students enrolled in less than full-time status and students with dependents. As a result, this analysis focuses on full-time students with no dependents, as part-time students received proportionally less grant aid, weakening the case for the effect of aid on their academic outcomes. In terms of sample sizes, this restriction leads to dropping the 16% of students in four-year programs who were enrolled part-time in Spring 2020, as well as the 2% of students who claimed dependents in their FAFSA during this term. Figure 1 plots the calculated amount of grant aid for individuals depending on their EFC levels across all colleges. The observed variation within treatment categories is due to the fact that grant base rates vary across colleges - for example, a student at a college with a base rate of \$356 receiving the maximum Pell Grant for spring 2020 would receive a grant of \$801 , while a student at a college with a base rate of \$484 would receive \$1089, as both are receiving a bonus of 125% on top of their respective base rates. Furthermore, the average jump in aid between categories is in the range of \$100 - \$150, reflecting an increase in aid between adjacent categories in the range of an additional 10 - 20 % in funding.

A limitation of the available administrative data is that it is not possible to observe directly the grant amount that each student effectively received, as this grant is recorded as an outside grant, under the broader category of institutional aid. However, the administrative measure of outside grants closely mirrors the calculated grant amount presented previously. Figure 2 plots the effectively observed amount of grant aid depending on their EFC levels, reflecting the implementation of this policy.

Figure 3.1: Grant design by EFC levels



Notes: assigned institutional aid by 2019-2020 EFC levels and bonus thresholds, for all undergraduate full-time students.

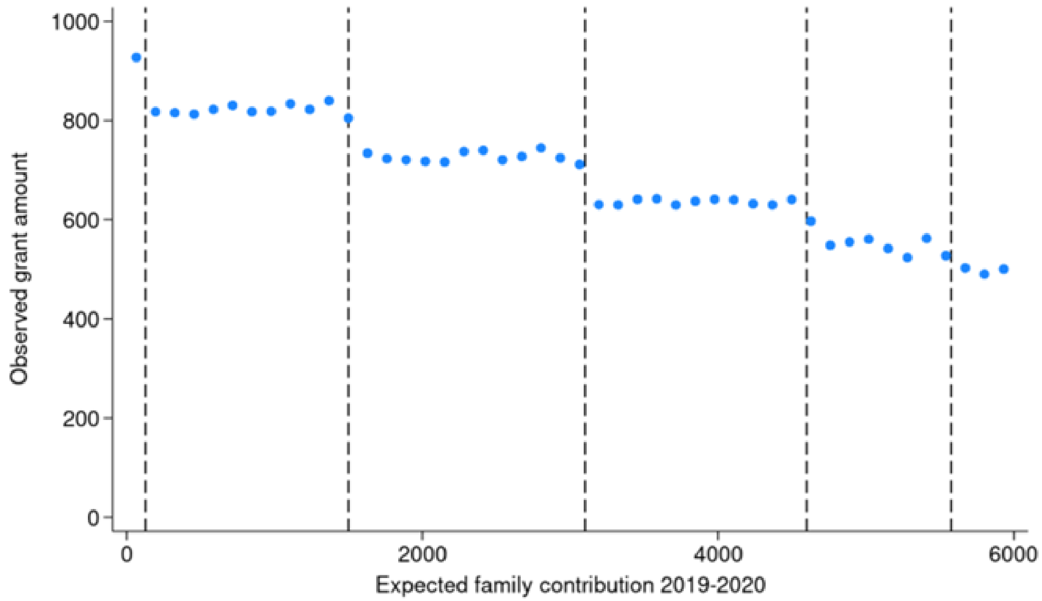
I focus on usual outcomes related to academic performance: persistence, defined as continued enrollment for those students who received the grant and are not recorded as graduating during this term; number of credits attempted and passed, and semester GPA. For reference, Table 1 presents descriptive statistics on group size, mean grants, and mean outcomes by treatment level. While grant aid levels are consistent with the assignment rule and the previously described aid data, there are no immediately significant differences in outcomes across treatment levels.

3.4 Identification

3.4.1 Regression discontinuity across treatment levels

Estimating the overall effect of this policy on short-term academic outcomes is challenging, as its universal nature provides no natural untreated group. However, it is possible to leverage the

Figure 3.2: Observed grant aid by EFC levels



Notes: effectively observed institutional aid by 2019-2020 EFC levels and bonus thresholds, for all undergraduate full-time students.

Table 3.1: Descriptive statistics by treatment category

	Base grant	25% bonus	50% bonus	75% bonus	100% bonus	125% bonus
Persistence - Fall 2020	0.79	0.78	0.79	0.78	0.79	0.80
Credits attempted - Fall 2020	11.05	10.49	10.64	10.53	10.71	10.71
GPA - Fall 2020	10.63	9.95	10.10	9.96	10.13	10.09
GPA - Fall 2020	2.76	2.63	2.64	2.59	2.63	2.63
Persistence - Spring 2021	0.60	0.57	0.59	0.59	0.60	0.62
Credits attempted - Spring 2021	9.61	8.87	9.02	8.74	9.04	9.12
GPA - Spring 2021	9.26	8.56	8.62	8.33	8.58	8.62
GPA - Spring 2021	2.44	2.24	2.26	2.20	2.23	2.24
Age	21.43	22.08	21.94	22.02	21.98	22.36
Female	55.19%	59.17%	57.91%	58.14%	56.88%	58.95%
White	41.43%	23.49%	22.42%	19.22%	16.79%	13.79%
Black	17.49%	25.94%	26.21%	25.41%	22.28%	18.78%
Hispanic	19.75%	29.95%	30.29%	31.19%	29.57%	33.18%
Asian	21.33%	20.62%	21.08%	24.17%	31.36%	34.25%
Observations	13678	1222	2695	4361	7344	32782

Notes: Descriptive statistics by treatment category for Fall 2020 financial aid information, academic outcomes, labor market outcomes, and demographic characteristics.

discontinuities in the assignment rule, described above, to estimate the effect of changes in aid generosity in short term academic outcomes. These discontinuities suggest the use of a regression discontinuity identification strategy, as students at different side of each threshold are likely to present similar observable and unobservable characteristics, except for the level of aid they receive. A concern with regression discontinuity designs is that the treatment may incentivize individuals to manipulate their assignment, if it is known in advance. However, this concern is unlikely to be present in this setting for two main reasons. In first place, expected family contributions (EFC) were calculated for the 2019-2020 academic year, prior to the onset of the pandemic, providing no opportunity for students to manipulate their reported levels of economic need. Furthermore, the possibility of income verification when filing the FAFSA might lessen the risk of manipulation. In second place, the aid thresholds were not known in advance to students, further limiting the odds of manipulation. The manipulation tests proposed by Cattaneo et al. (2018), reported in table 2, fail to find evidence of manipulation at any of the four thresholds under consideration.

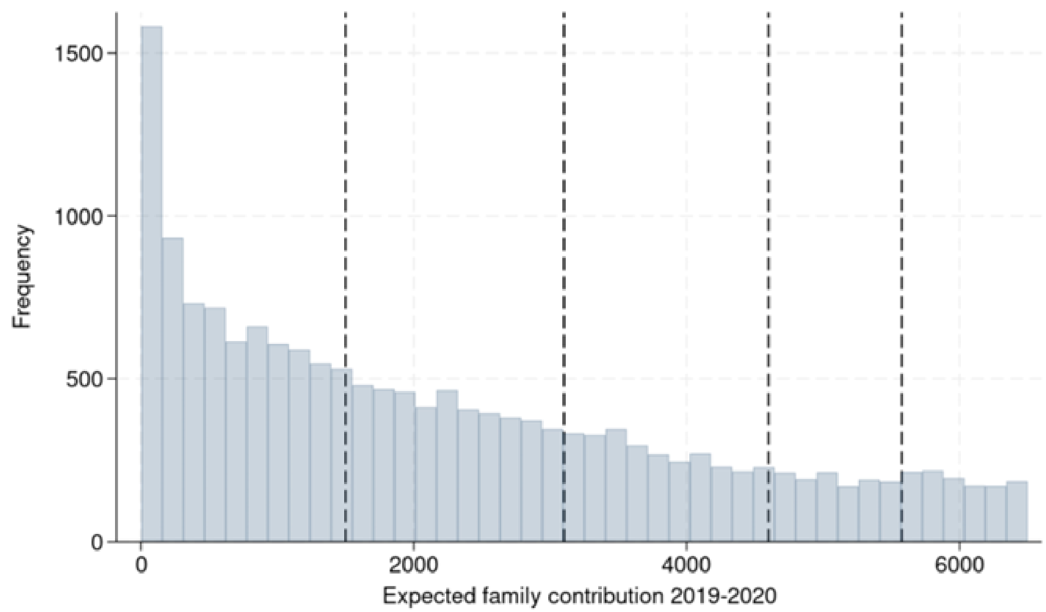
Table 3.2: Covariate balance across treatment thresholds

	Age	Female	White	Black	Hispanic	Asian
25% bonus	0.104	-0.0492	-0.0368	-0.0858	0.0990	0.0235
	(0.528)	(0.0767)	(0.0658)	(0.0664)	(0.0720)	(0.0653)
Observations	685	686	686	686	686	686
50% bonus	0.471	0.0233	0.00418	0.0833	-0.0406	-0.0469
	(0.612)	(0.0700)	(0.0551)	(0.0648)	(0.0644)	(0.0602)
Observations	771	771	771	771	771	771
75% bonus	0.202	0.0370	0.0125	0.0345	-0.0253	-0.0218
	(0.341)	(0.0437)	(0.0361)	(0.0372)	(0.0412)	(0.0377)
Observations	2110	2110	2110	2110	2110	2110
100% bonus	0.151	0.0160	0.0330	0.00937	0.00880	-0.0511
	(0.324)	(0.0434)	(0.0331)	(0.0358)	(0.0412)	(0.0392)
Observations	2095	2095	2095	2095	2095	2095

Notes: treatment effects of belonging to the given aid category with respect to the next lower one on available covariates, estimated using local linear regression with a linear distance term. Bandwidths are calculated using the Calonico et al. (2014) optimal bandwidth selection procedure. All regressions include controls for college fixed effects. Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Additionally, Figure 3 presents the distribution of the running variable along the thresholds, presenting no evidence of sorting or bunching around the relevant thresholds. Table 3 presents the coefficients of the respective jumps across the thresholds for all available covariates, finding no statistically significant differences in observable socio-demographic characteristics across any cut-off.

Figure 3.3: Observed grant aid by EFC levels



Notes: histogram of 2019-2020 EFC levels for all students in the sample, excluding students with zero EFC and students with EFC above $6000, along with bonus thresholds.

The existence of multiple thresholds for this financial aid policy requires the extension of a standard regression discontinuity analysis. In first place, it is not possible to evaluate the impact of a 125% bonus with respect to a 100% bonus, since the EFC requirement for the largest bonus is bunched at zero, making it challenging to construct an adequate bandwidth for the control group. As a result, it is only possible to evaluate the local impact of the 25% - 100% bonuses with respect to the groups receiving the next-lowest bonus. Additionally, the presence of multiple thresholds raises further challenges. Following Cattaneo et al. (2016), each cutoff is analyzed separately, as the usual normalizing-and-pooling approach would provide a weighted average of the local treatment effect for each cutoff which presents interpretation challenges. In this setting, the applicability of the estimated pooled treatment effect is unclear, due to heterogeneity in the additional levels of aid for all groups, as well as their differing EFC levels; and it would not necessarily clear for which groups the estimated local treatment effect is local to.

3.4.2 Regression discontinuity results

In first place, I present descriptive plots for the behavior of the outcomes across all thresholds. Figure 4 presents changes in measured grant aid across all bonus levels, finding that the average change in aid is in the order of $100 and is statistically significant across all bonus thresholds. In this setting, heterogeneity is related the role that different college base rates play in the determination of the size of the bonus. Figure 5 presents changes in enrollment across all bonus levels across thresholds, finding no readily apparent changes in enrollment across any cut-off, as well as relatively high levels of variance across EFC levels near the thresholds, in the range of approximately 20 percentage points.

Figure 6 presents changes in attempted credits, with no apparent changes in this outcome across any of the four cut-offs. However, the variance of attempted credits appears to fall across thresholds as aid levels increase.

Figure 7 plots changes in credits passed across all thresholds. As before, there are no observable jumps in this outcome for any of the aid cut-offs.

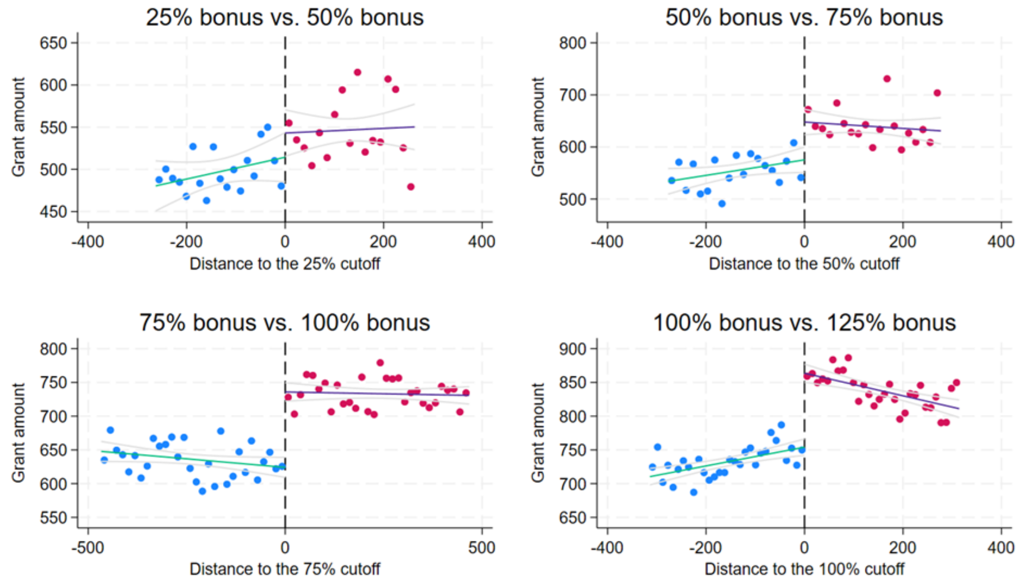
Finally, Figure 8 presents changes in GPA for all grant aid bonus cut-offs, finding no observable effects around any of the thresholds.

After this descriptive evidence, I estimate the following model for all thresholds:

$$y_i = \beta_0 + \beta_1 T_i + \beta_2 X_i T_i + \gamma C_i + \mu_c + e_i c$$

where T_i is defined as being to the left of the respective threshold - and receiving the larger

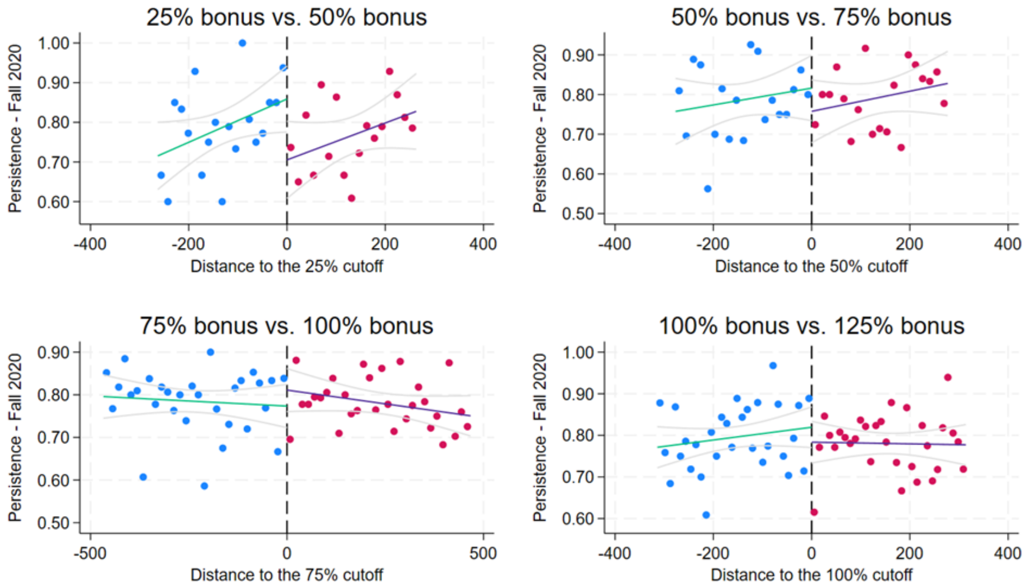
Figure 3.4: Regression discontinuity plot, observed grant amount



Notes: measured grant aid for eligible full-time undergraduate students, by $ 25 bins of re-centered EFC with respect to bonus thresholds, along with linear fit and 95% confidence intervals.

bonus, X_i is the distance to each threshold, C_i is a set of demographic controls intended to increase precision, and μ_c is a college fixed effect, reflecting the effect of varying base rates by college. If standard assumptions hold, β_1 is an estimate of the causal effect of an additional 25% bonus with respect to the previous aid category. I use the Calonico et al (2014) optimal bandwidths for the OLS and robust RD specification. Table 4 presents the treatment effects of each change in grant aid estimated using a local linear regression. I find no statistically significant effect of most bonuses, a result consistent with the estimated jump in effective additional aid at each threshold. While the 50%, 75% and 100% bonuses have no effect on any of the outcomes across all specifications, the 25% bonus appears to have a negative effect of 0.17 percentage points on persistence and a positive effect of 0.58 points on Fall 2020 GPA. However, these results are only weakly statistically significant and depend on this choice of estimator, as will be discussed below.

Figure 3.5: Regression discontinuity plot, persistence through Fall 2020



Notes: Fall 2020 enrollment for eligible full-time undergraduate students, by $ 25 bins of re-centered EFC with respect to bonus thresholds, along with linear fit and 95% confidence intervals.

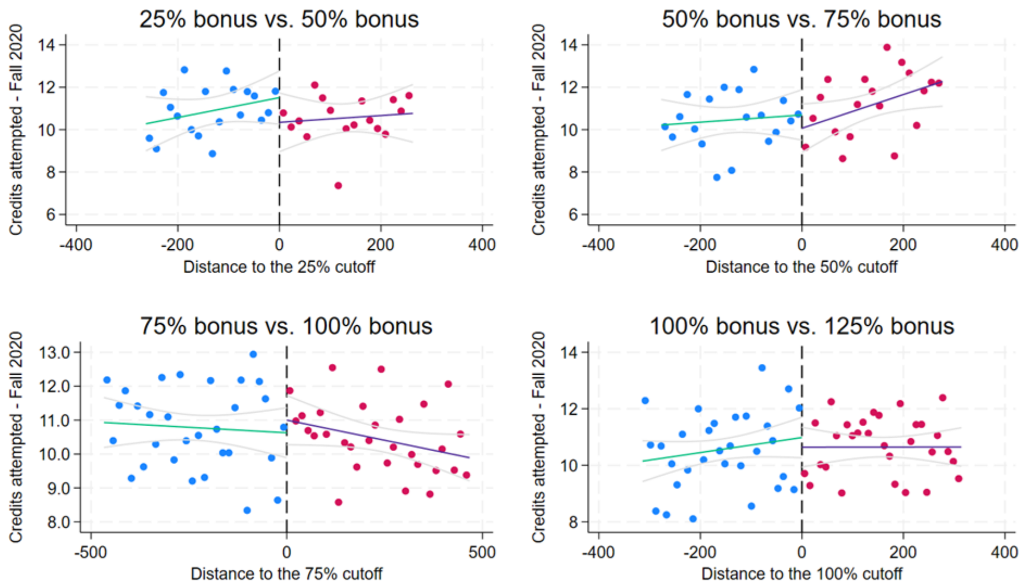
Table 5 presents the treatment effects of each change in grant aid estimated using the Calonico et al (2016) estimator. Consistent with the effects found in using a local OLS, there are no statistically significant effects of any bonus on the academic outcomes under consideration. [1]

3.5 Conclusions

This paper provides one of the first evaluations of the effectiveness of large-scale emergency financial aid, examining the trade-offs involved in designing universal versus targeted aid packages. I leverage the implementation of the Higher Education Emergency Relief Fund (HEERF), using administrative data and its associated assignment rule from a large public college system. My

1. All the above results follow a similar pattern for Spring 2021 outcomes and are robust to a range of bandwidth choices. Results are available upon request.

Figure 3.6: Regression discontinuity plot, credits attempted in Fall 2020

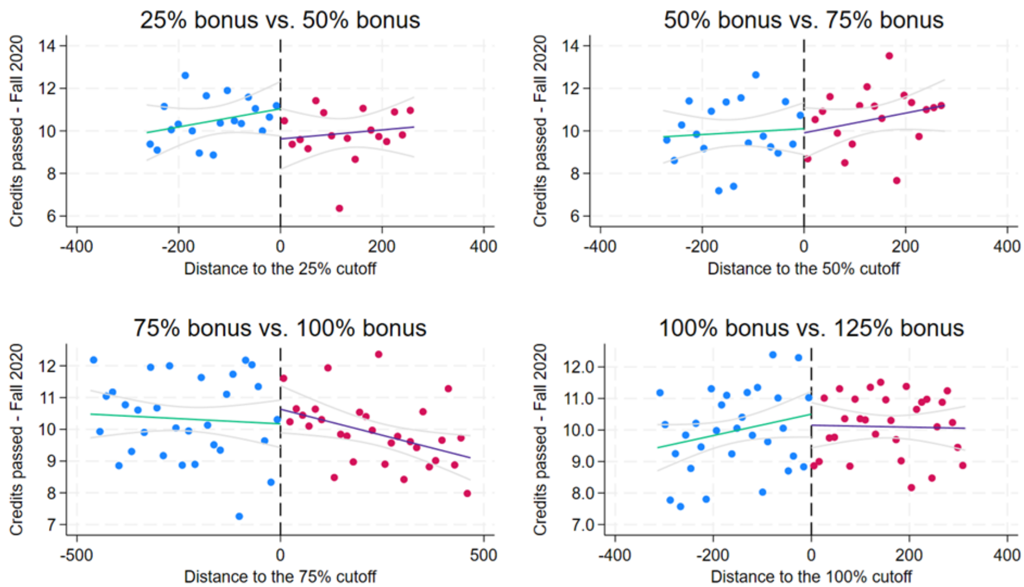


Notes: Fall 2020 attempted credits for eligible full-time undergraduate students, by $ 25 bins of re-centered EFC with respect to bonus thresholds, along with linear fit and 95% confidence intervals.

regression discontinuity analysis suggests that the marginal effects of additional aid are null, a result that holds across a range of academic outcomes and aid levels. This result is comparable to the null result of emergency financial aid of similar magnitudes (approximately $100-150) found by Evans et al. (2019).

Although the regression discontinuity estimates are local to small differences in grant size, broader evidence from the literature suggests that emergency aid is most impactful when targeted toward students with the greatest financial need. In particular, students with zero or very low expected family contributions (EFC) are more likely to face binding financial constraints and less likely to have alternative sources of support. Concentrating larger grants on these students increases the likelihood of influencing persistence and academic success, and may improve the overall cost-effectiveness of emergency aid programs. Policymakers should therefore consider al-

Figure 3.7: Regression discontinuity plot, credits passed in Fall 2020



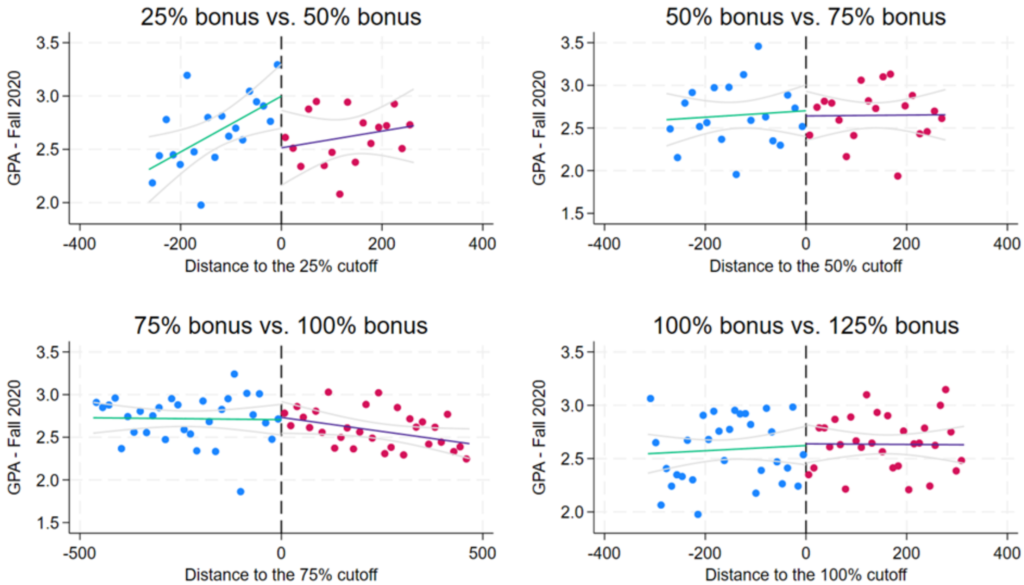
Notes: Fall 2020 credits passed for eligible full-time undergraduate students, by \$ 25 bins of re-centered EFC with respect to bonus thresholds, along with linear fit and 95% confidence intervals.

locating a greater share of emergency funds to the lowest-income students, even if this requires modest reductions in aid for others with moderate need.

In addition, the results suggest that overly fine-grained eligibility structures may reduce both effectiveness and fairness. Small differences in EFC led to marginal differences in grant amounts—typically around \$100—which were not sufficient to shift academic outcomes. Future emergency aid programs should adopt simpler eligibility designs, such as a universal base grant paired with a single, larger bonus grant for high-need students. This approach preserves a sense of fairness while freeing up resources to support those most likely to benefit. It also simplifies implementation and increases transparency for students, while strengthening the evaluability of the program by producing clearer treatment contrasts.

This study has several limitations. First, the regression discontinuity design identifies local av-

Figure 3.8: Regression discontinuity plot, Fall 2020 GPA



Notes: Fall 2020 GPA for eligible full-time undergraduate students, by $ 25 bins of re-centered EFC with respect to bonus thresholds, along with linear fit and 95% confidence intervals.

erage treatment effects near modest changes in aid amounts—typically \$100–\$150—which may be too small to meaningfully influence student behavior and limit the external validity of the findings. Results may not generalize to students with higher levels of need, to other institutional contexts, or to programs offering larger or more conditional forms of aid. Second, the analysis focuses on short-term academic outcomes and does not capture longer-run effects such as persistence, re-enrollment, or financial well-being. Finally, the data do not include information on how students used the aid, limiting the ability to explore behavioral mechanisms underlying the observed null effects.

Future research is needed on the effects of this program on students outside the current sample, such as those enrolled in associates' degrees or graduate degrees, as they may be expected to be particularly sensitive to financial aid. Additionally, future research can provide additional evidence

	Grant aid	Persistence	Attempted credits	Passed credits	GPA
25% bonus	46.07*	-0.172**	-1.470	-1.760	0.582*
	(19.42)	(0.0650)	(0.926)	(0.957)	(0.234)
Observations	685	685	685	685	685
Estimated bandwidth	3304.6	3304.6	3304.6	3304.6	3304.6
50% bonus	76.80***	-0.0481	-0.521	-0.0933	0.0651
	(15.82)	(0.0573)	(0.832)	(0.856)	(0.212)
Observations	771	771	771	771	771
Estimated bandwidth	279.3	279.3	279.3	279.3	279.3
75% bonus	108.5***	0.0482	0.466	0.519	0.0398
	(9.191)	(0.0360)	(0.511)	(0.519)	(0.126)
Observations	2110	2110	2110	2110	2110
Estimated bandwidth	473.2	473.2	473.2	473.2	473.2
100% bonus	109.1***	-0.0297	-0.237	-0.237	0.0329
	(8.518)	(0.0349)	(0.505)	(0.515)	(0.129)
Observations	2095	2095	2095	2095	2095
Estimated bandwidth	318.6	318.6	318.6	318.6	318.6

Notes: treatment effects of belonging to the given aid category with respect to the next lower one on the Fall 2020 outcomes, estimated using local linear regression with a linear distance term. Bandwidths are calculated using the Calonico et al. (2014) optimal bandwidth selection procedure. All regressions include controls for age, gender, ethnicity, and college fixed effects. Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

on the mechanisms driving the effects of emergency financial aid, such as the role of increased access to food, housing, and other educational inputs, in achievement in higher education, as well as generalizing its effects to other contexts.

	Grant aid	Persistence	Attempted credits	Passed credits	GPA
25% bonus	48.40***	-0.0301	-0.368	-0.544	0.0797
	(8.578)	(0.0284)	(0.407)	(0.419)	(0.104)
Observations	14895	14895	14895	14895	14895
Estimated bandwidth	3304.6	3304.6	3304.6	3304.6	3304.6
50% bonus	75.76***	-0.0304	-0.396	-0.0204	0.0267
	(15.70)	(0.0602)	(0.882)	(0.901)	(0.224)
Observations	3916	3916	3916	3916	3916
Estimated bandwidth	279.3	279.3	279.3	279.3	279.3
75% bonus	103.8***	0.0318	0.558	0.513	0.0102
	(8.796)	(0.0384)	(0.549)	(0.560)	(0.134)
Observations	7054	7054	7054	7054	7054
Estimated bandwidth	473.2	473.2	473.2	473.2	473.2
100% bonus	113.3***	-0.0289	-0.349	-0.247	0.0817
	(8.081)	(0.0377)	(0.536)	(0.553)	(0.141)
Observations	11703	11703	11703	11703	11703
Estimated bandwidth	318.6	318.6	318.6	318.6	318.6

Notes: treatment effects of belonging to the given aid category with respect to the next lower one on the Fall 2020 outcomes, estimated using the Calonico et al. (2014) estimator. Bandwidths are calculated using the Calonico et al. (2014) optimal bandwidth selection procedure. All regressions include controls for age, gender, ethnicity, and college fixed effects. Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

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Table A-1: Field of study by highest degree

PhD		MA		JD		BA	
Unavailable	17.78%	Business	31.18%	Law	100%	Unavailable	86.96%
Education	16.09%	Health	10.75%			Social Sciences	4.35%
Social Sciences	11.22%	Education	8.60%			Business	3.26%
Philosophy	9.24%	Unavailable	8.60%			Visual Arts	1.09%
Psychology	5.46%	Visual Arts	6.45%			English	1.09%
English	5.26%	Public Administration	6.45%			Liberal Arts	1.09%
Engineering	3.97%	Theology	5.38%			Law	1.09%
History	3.87%	Social Sciences	5.38%			Health	1.09%
Theology	3.48%	Psychology	4.30%				
Visual Arts	2.88%	Architecture	2.15%				
Physics	2.68%	Engineering	2.15%				
Biology	2.68%	Liberal Arts	2.15%				
Business	2.58%	Biology	1.08%				
Health	2.48%	Communication	1.08%				
Foreign Languages	2.09%	Philosophy	1.08%				
Mathematics	1.79%	History	1.08%				
Interdisciplinary Studies	1.29%	Foreign Languages	1.08%				
Public Administration	1.19%	English	1.08%				
Communication	1.09%						
Area Studies	0.89%						
Computer Science	0.70%						
Liberal Arts	0.30%						
Law	0.20%						
Fitness Studies	0.20%						
Architecture	0.20%						
Social Skills	0.20%						
Law Enforcement	0.10%						
Human Sciences	0.10%						
N	1007		93		136		92

Table A-2: Institution, highest degree received

PhD	Harvard University	56	5.09%
	University of Pennsylvania	36	3.27%
	Yale University	35	3.18%
	Princeton University	30	2.73%
	University of Chicago	28	2.55%
	Stanford University	20	1.82%
	Fordham University	18	1.64%
	Columbia University	18	1.64%
	University of Wisconsin-Madison	17	1.55%
	Northwestern University	16	1.45%
	All others	621	56.45%
	Unavailable	205	18.64%
JD	Harvard University	19	13.97%
	Yale University	10	7.35%
	University of Virginia	6	4.41%
	Columbia University	5	3.68%
	University of Chicago	4	2.94%
	University of Florida	3	2.21%
	Pepperdine University	3	2.21%
	Hamline University	3	2.21%
	University of Pennsylvania	3	2.21%
	Duke University	3	2.21%
	All others	53	38.97%
	Unavailable	24	17.65%
MA	Harvard University	8	8.60%
	University of Iowa	4	4.30%
	Yale University	4	4.30%
	Drexel University	3	3.23%
	University of Evansville	2	2.15%
	University of Illinois Springfield	2	2.15%
	Howard University	2	2.15%
	New York University	2	2.15%
	Columbia University	2	2.15%
	Boston College	2	2.15%
	All others	38	40.86%
	Unavailable	24	25.81%

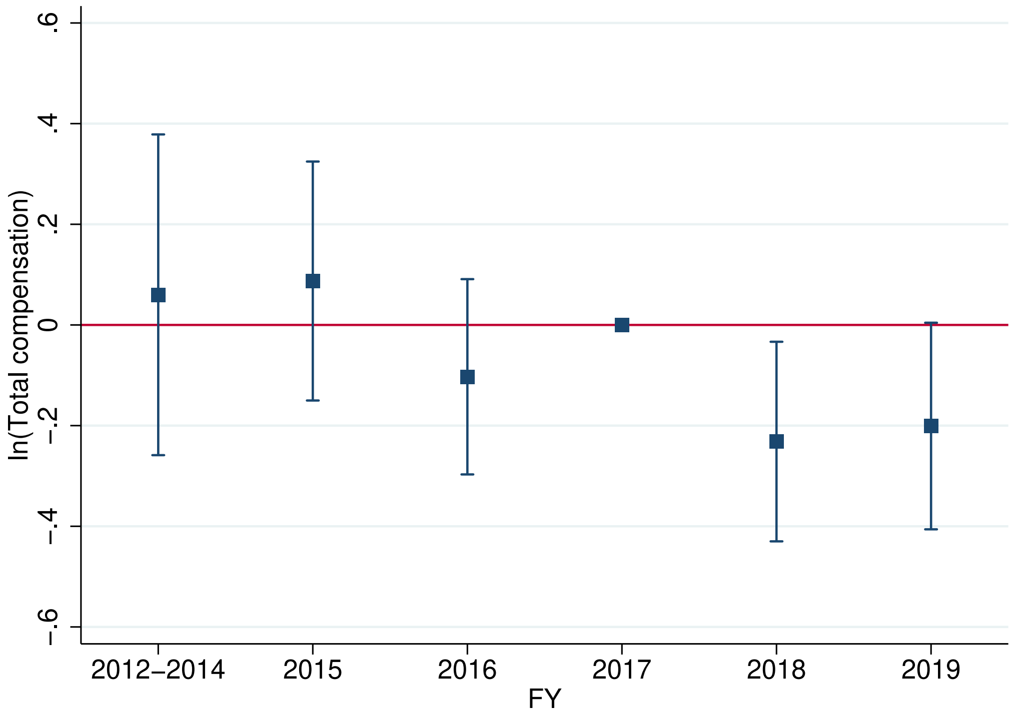
Table A-3: Position before becoming presidents

Academia		994		74.85%
	President	254	25.55%	
	VP	285	28.67%	
	Provost	179	18.01%	
	Dean	181	18.21%	
	Director	12	1.21%	
	Chair	18	1.81%	
	Professor	16	1.61%	
	Other academic administration	49	4.93%	
Government		48		3.61%
Non-profit		50		3.77%
Private		133		10.02%
Religious		25		1.88%
Unavailable		78		5.87%
		1328		

Table A-4: Pre-trends test, joint test that all pre-treatment coefficients equal 0

	p-value, joint test of leads = 0
ln(Total compensation)	0.068
Turnover	0.136
ln(Base salary)	0.757
ln(Bonuses)	0.909
ln(Non-taxable compensation)	0.276
ln(Other compensation)	0.737

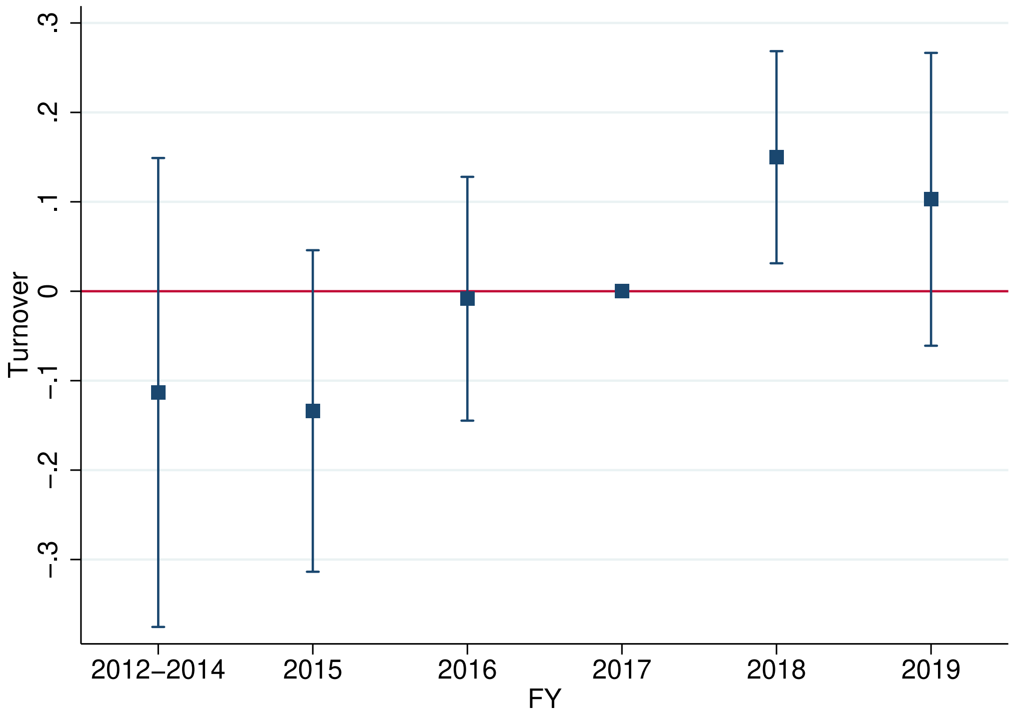
Figure A-1: Effects of the TCJA on compensation, president FE



Notes: regressions include president, institution and year fixed effects, and institution-specific time trends. Standard errors are clustered at the institution level. Pre-treatment periods before 2014 are binned into a single coefficient.

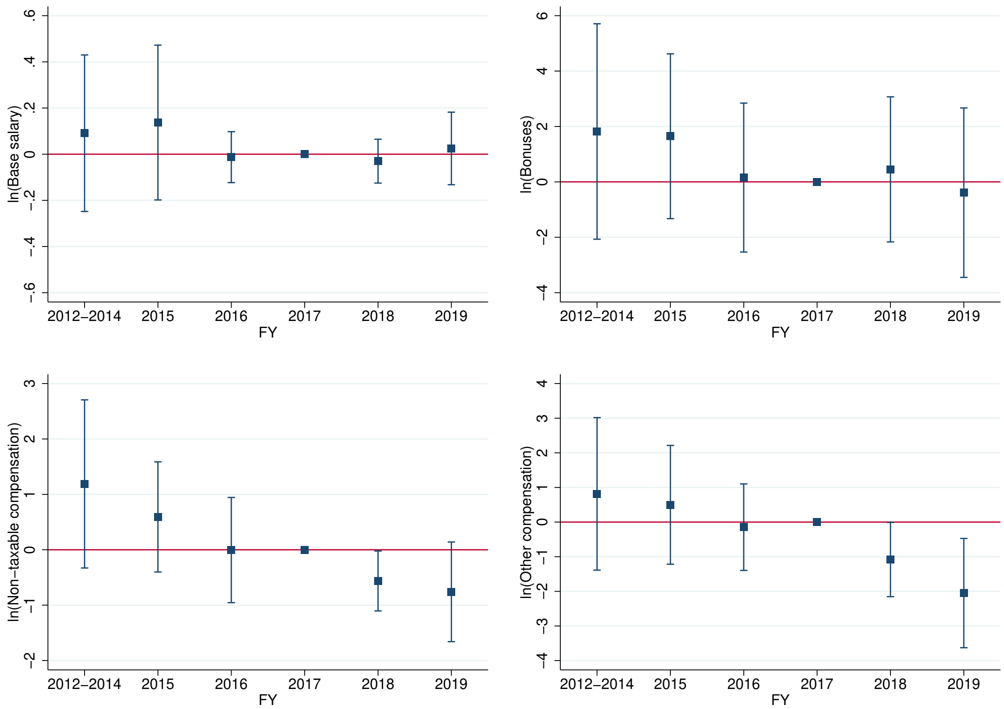
(back)

Figure A-2: Effects of the TCJA on turnover, president FE



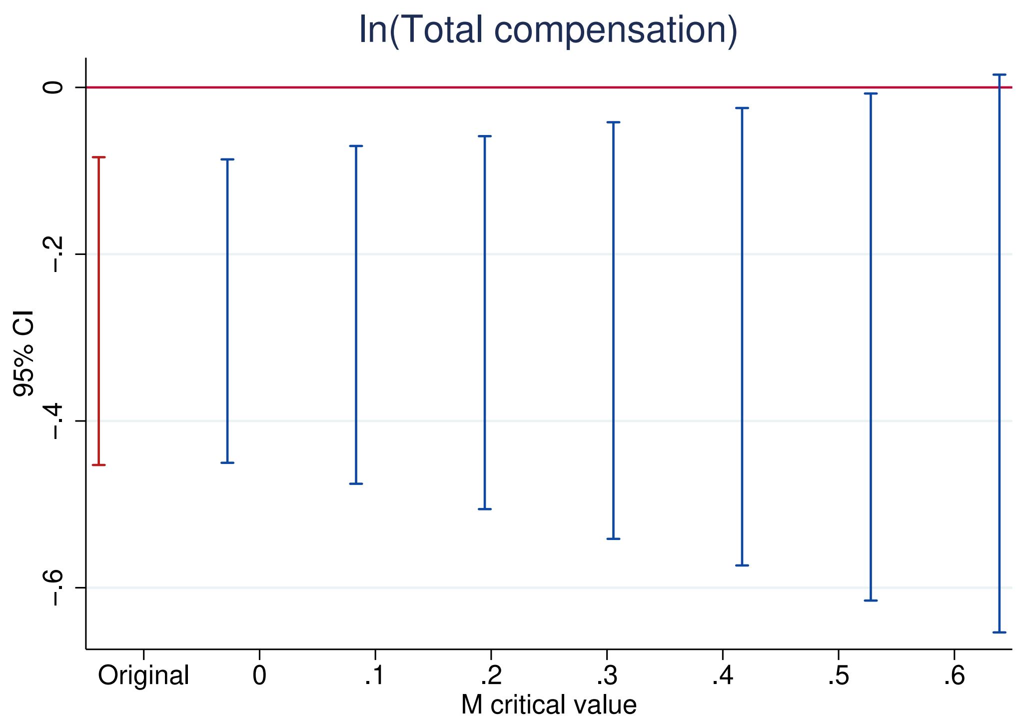
Notes: regressions include president, institution and year fixed effects, and institution-specific time trends. Standard errors are clustered at the institution level. Pre-treatment periods before 2014 are binned into a single coefficient.

Figure A-3: Effects of the TCJA on compensation by components, president FE



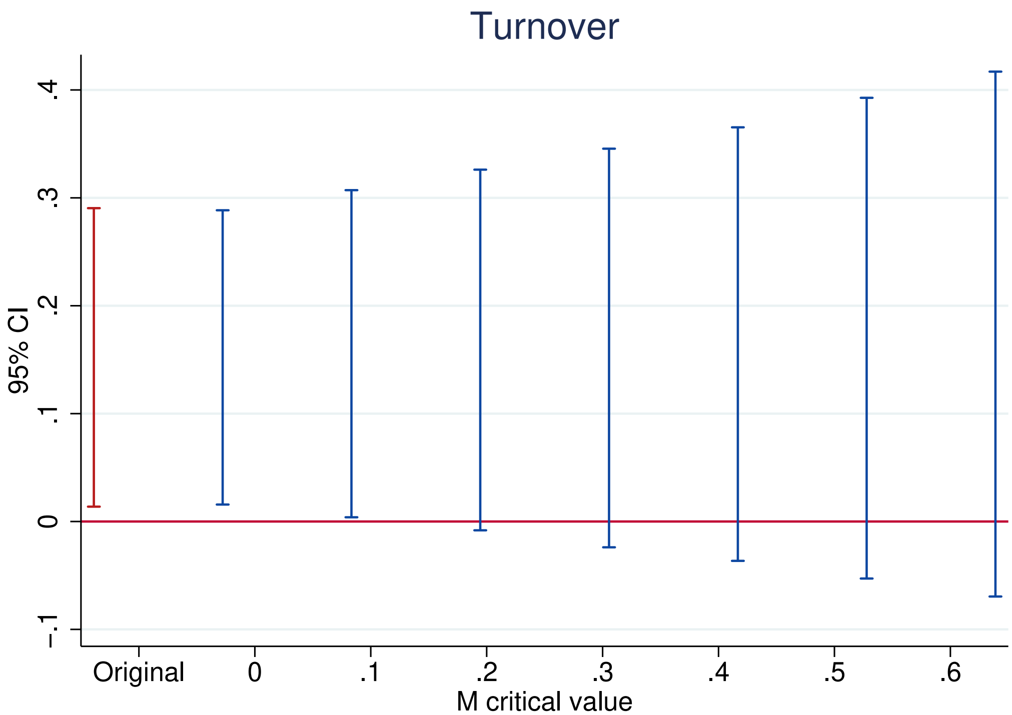
Notes: regressions include president, institution and year fixed effects, and institution-specific time trends. Standard errors are clustered at the institution level. Pre-treatment periods before 2014 are binned into a single coefficient.

Figure A-4: Rambachan and Roth (2023) sensitivity analysis for parallel trend violations, compensation



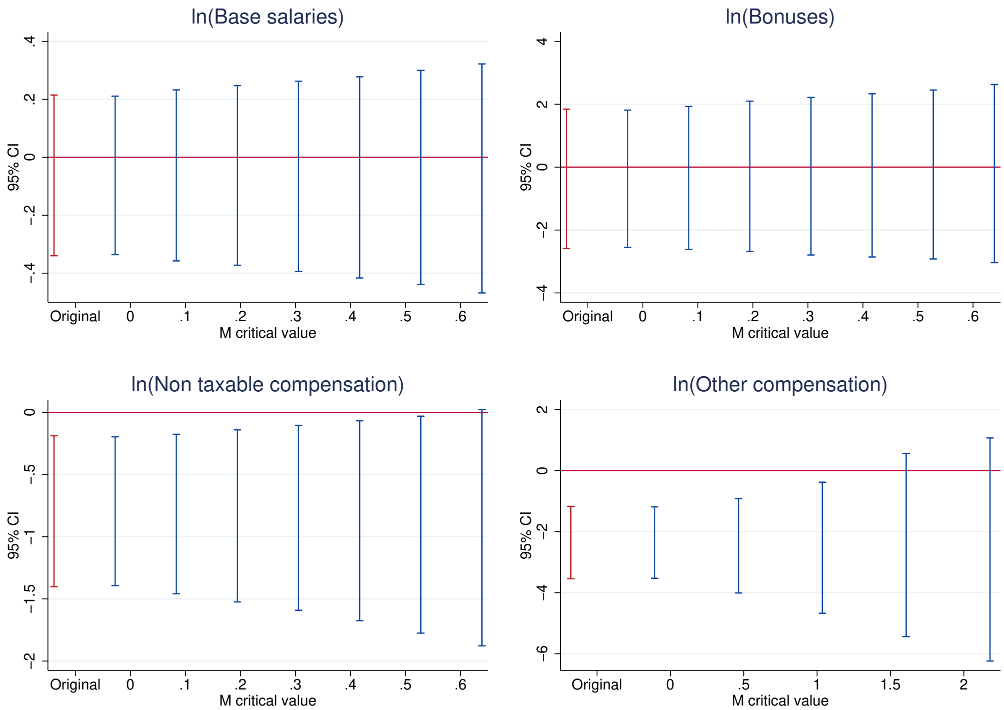
Notes: Rambachan and Roth (2023) sensitivity analysis, presenting how the treatment effect across all post-treatment periods varies by critical values of M, the maximum pre-treatment parallel trends violations.

Figure A-5: Rambachan and Roth (2023) sensitivity analysis for parallel trend violations, turnover



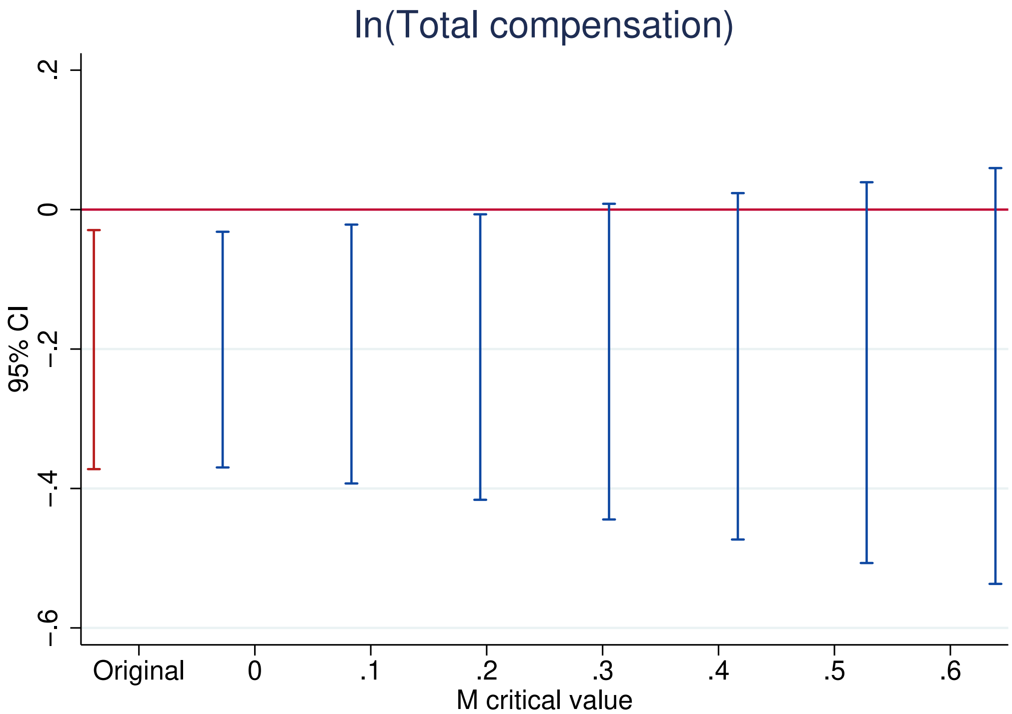
Notes: Rambachan and Roth (2023) sensitivity analysis, presenting how the treatment effect across all post-treatment periods varies by critical values of M, the maximum pre-treatment parallel trends violations.

Figure A-6: Rambachan and Roth (2023) sensitivity analysis for parallel trend violations, components of compensation



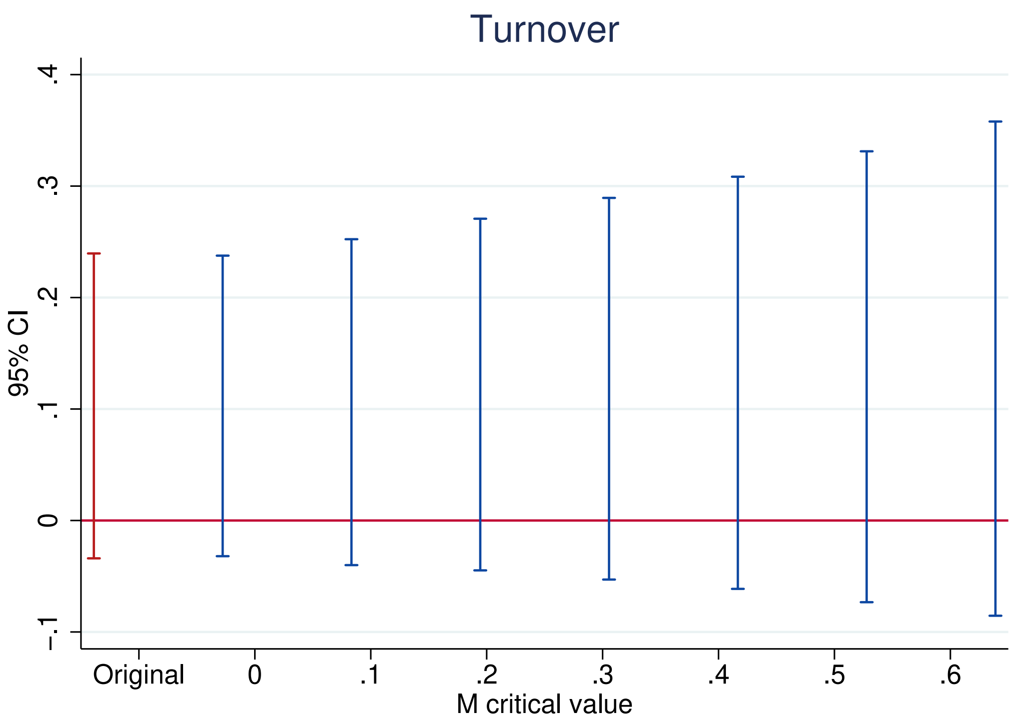
Notes: Rambachan and Roth (2023) sensitivity analysis, presenting how the treatment effect across all post-treatment periods varies by critical values of M, the maximum pre-treatment parallel trends violations.

Figure A-7: Rambachan and Roth (2023) sensitivity analysis for parallel trend violations, compensation with president FE



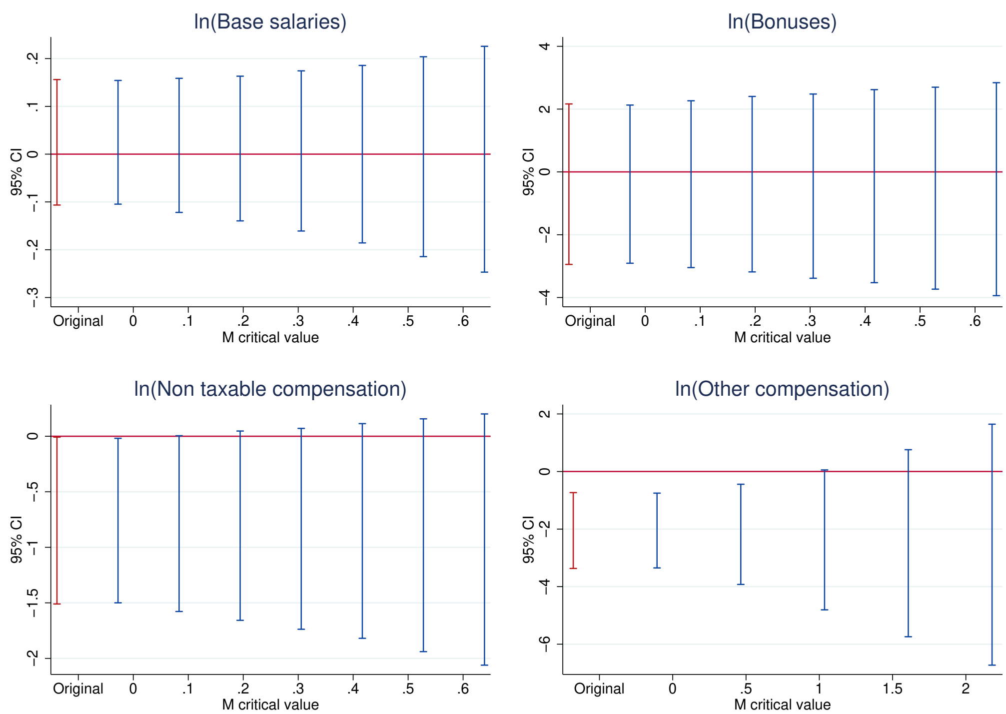
Notes: Rambachan and Roth (2023) sensitivity analysis, presenting how the treatment effect across all post-treatment periods varies by critical values of M, the maximum pre-treatment parallel trends violations.

Figure A-8: Rambachan and Roth (2023) sensitivity analysis for parallel trend violations, turnover with president FE



Notes: Rambachan and Roth (2023) sensitivity analysis, presenting how the treatment effect across all post-treatment periods varies by critical values of M, the maximum pre-treatment parallel trends violations.

Figure A-9: Rambachan and Roth (2023) sensitivity analysis for parallel trend violations, components of compensation with president FE



Notes: Rambachan and Roth (2023) sensitivity analysis, presenting how the treatment effect across all post-treatment periods varies by critical values of M, the maximum pre-treatment parallel trends violations.

Figure A-10: Effects of the TCJA on compensation, TWFE

Notes: coefficients from a two-way fixed effect regression on a binary treatment indicator including institution and year fixed effects, and institution-specific time trends. Standard errors are clustered at the institution level. * p<0.1, ** p<0.05, *** p<0.01

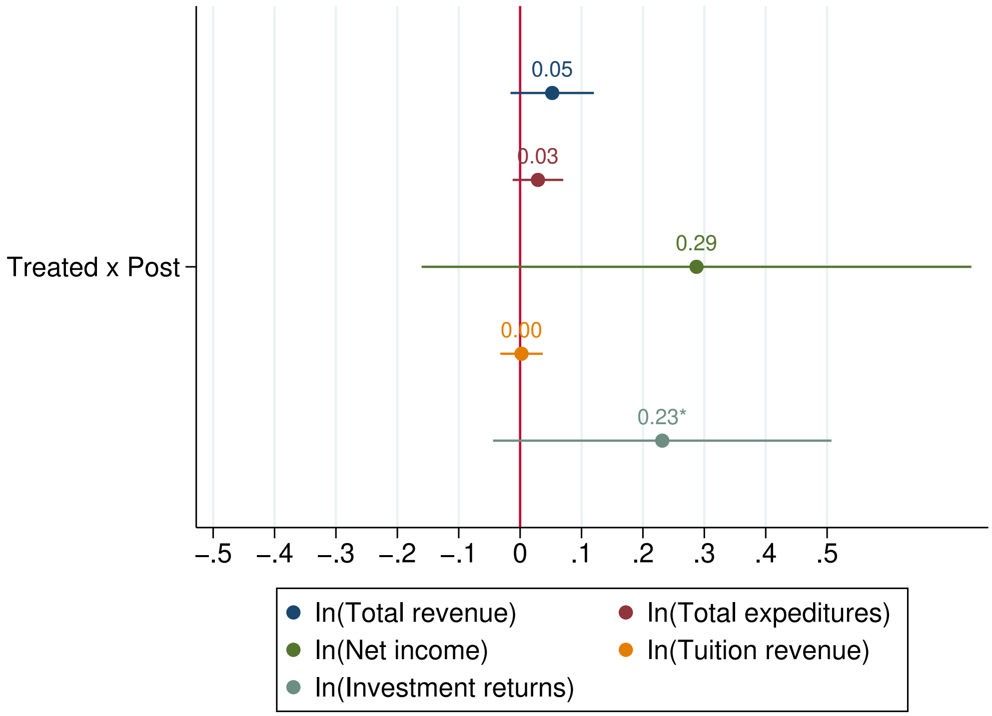
Figure A-11: Effects of the TCJA on compensation, TWFE and president FE

Notes: coefficients from a two-way fixed effect regression on a binary treatment indicator including president, institution and year fixed effects, and institution-specific time trends. Standard errors are clustered at the institution level. * p<0.1, ** p<0.05, *** p<0.01

Table A-5: Pre-trends test, joint test that all pre-treatment coefficients equal 0

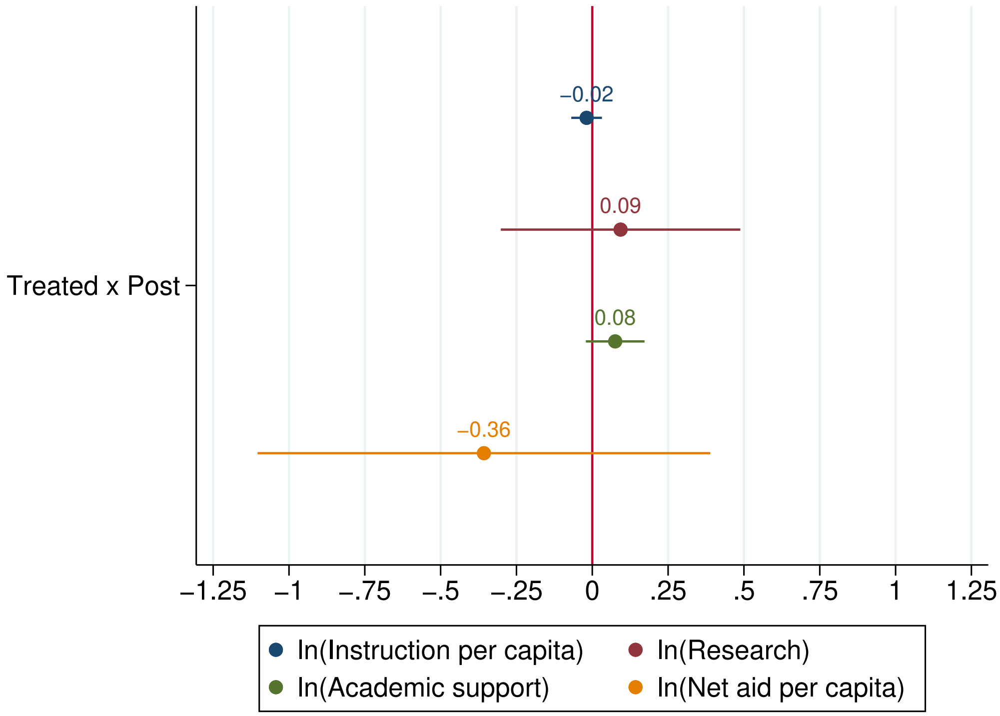
	p-value, joint test of leads = 0
ln(Total revenue)	0.129
ln(Total expenditures)	0.376
ln(Net income)	0.465
ln(Tuition revenue)	0.314
ln(Investment returns)	0.439
ln(Instructional expenditure per capita)	0.432
ln(Research expenditure)	0.648
ln(Academic support expenditure)	0.463
ln(Net aid per capita)	0.357

Figure A-12: Effects of the TCJA on assets and revenue, president FE



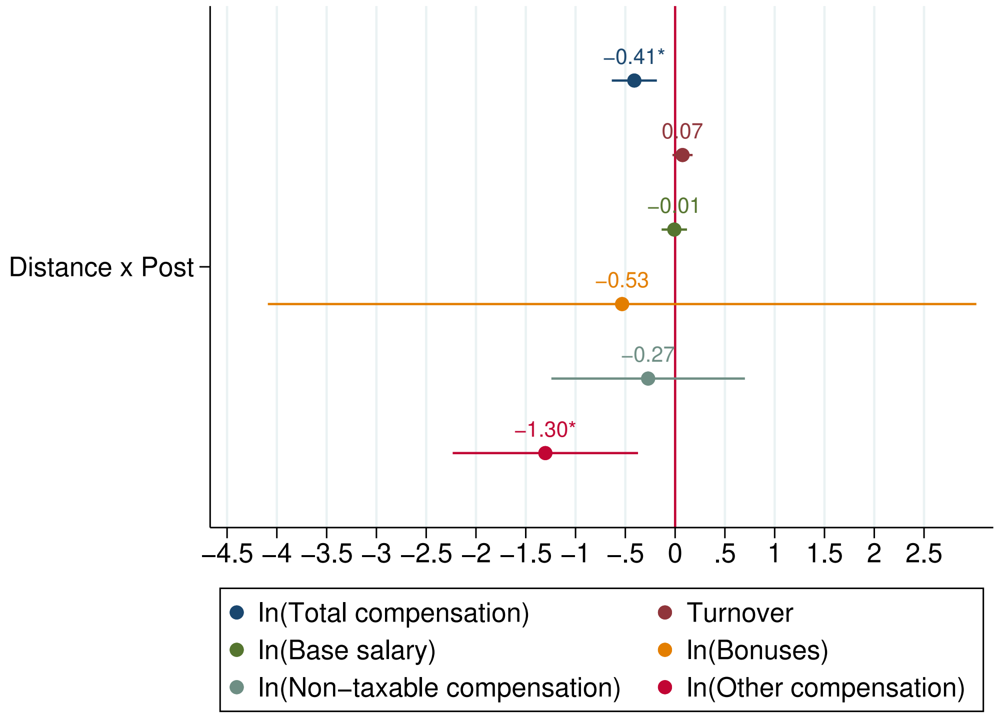
Notes: coefficients from a two-way fixed effect regression on a binary treatment indicator including institution and year fixed effects, and institution-specific time trends. Standard errors are clustered at the institution level. * p<0.1, ** p<0.05, *** p<0.01

Figure A-13: Effects of the TCJA on expenditures, president FE



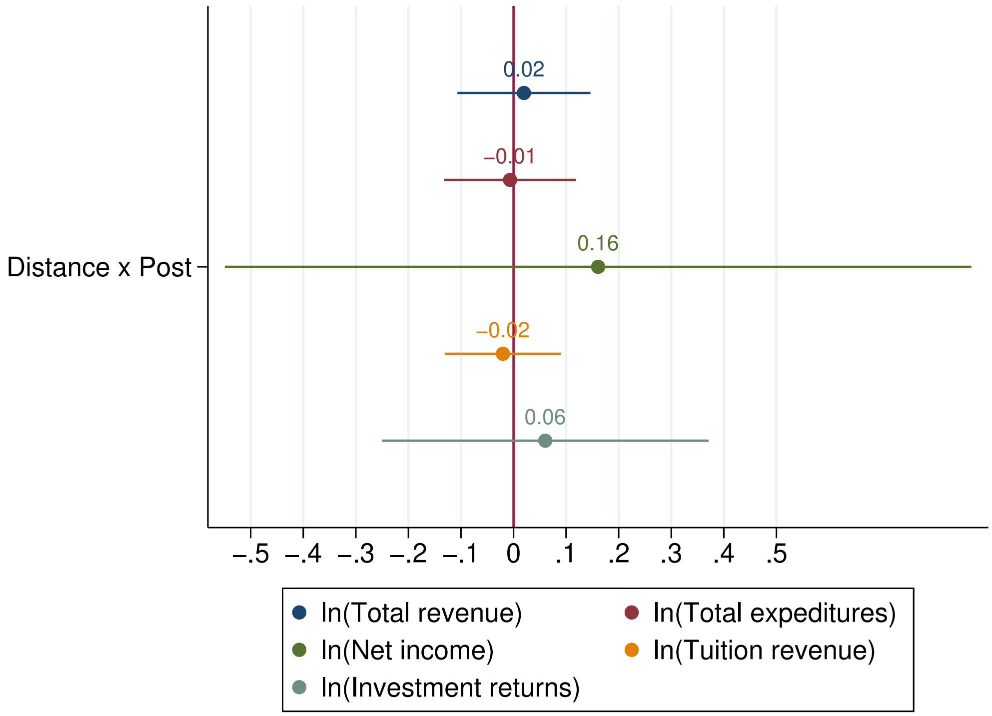
Notes: coefficients from a two-way fixed effect regression on a binary treatment indicator including institution and year fixed effects, and institution-specific time trends. Standard errors are clustered at the institution level. * p<0.1, ** p<0.05, *** p<0.01

Figure A-14: Effects of the TCJA on compensation, continuous treatment and president FE



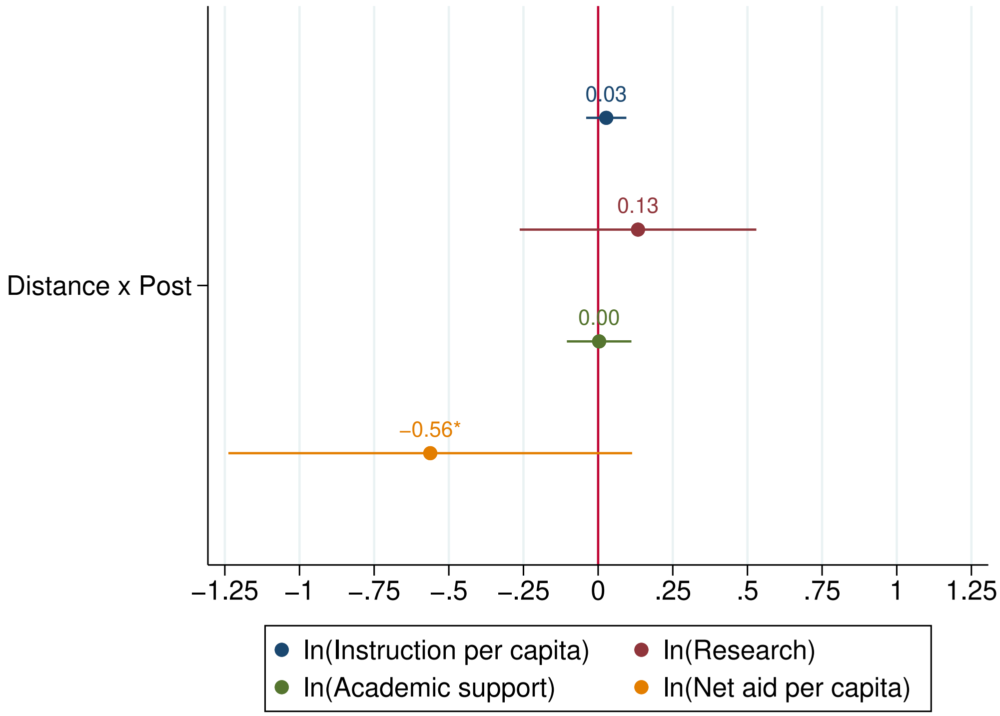
Notes: coefficients from a two-way fixed effect regression including president, institution and year fixed effects, and institution-specific time trends. Standard errors are clustered at the institution level. * p<0.1, ** p<0.05, *** p<0.01

Figure A-15: Effects of the TCJA on assets and revenue, continuous treatment and president FE



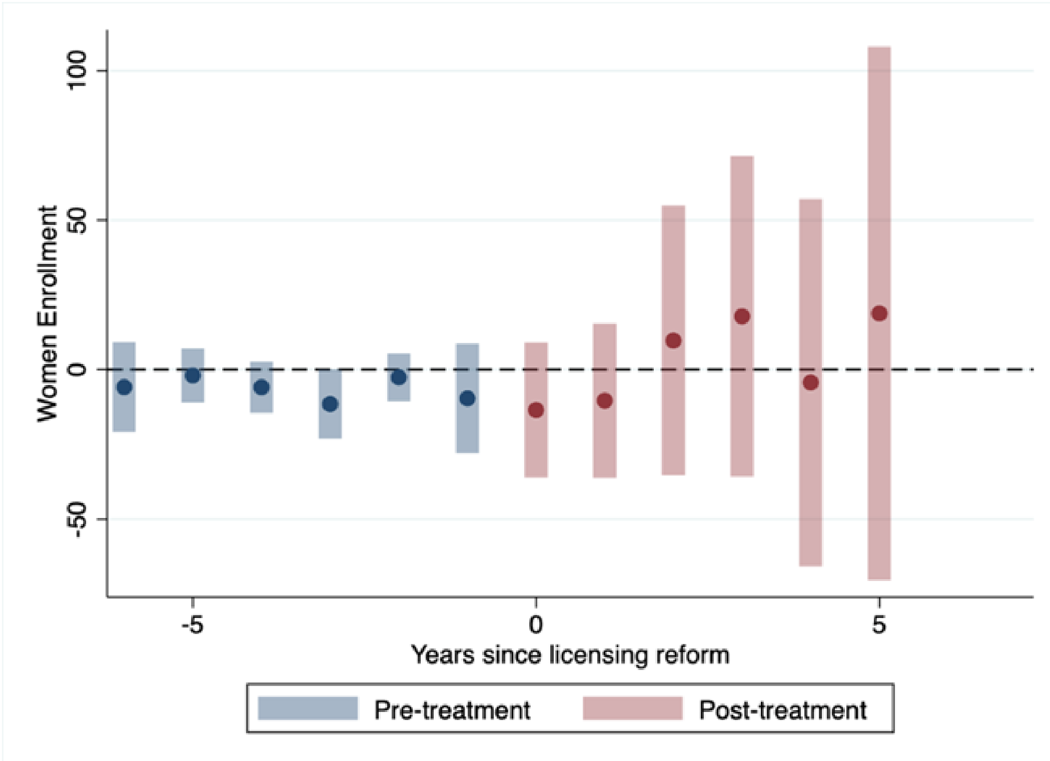
Notes: coefficients from a two-way fixed effect regression including president, institution and year fixed effects, and institution-specific time trends. Standard errors are clustered at the institution level. * p<0.1, ** p<0.05, *** p<0.01

Figure A-16: Effects of the TCJA on expenditures, continuous treatment and president FE



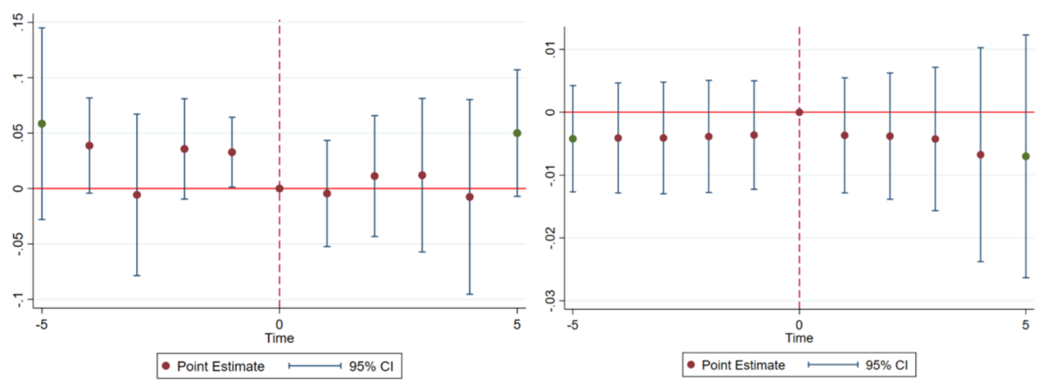
Notes: coefficients from a two-way fixed effect regression including president, institution and year fixed effects, and institution-specific time trends. Standard errors are clustered at the institution level. * p<0.1, ** p<0.05, *** p<0.01

Figure B-1: Impact of licensing hour reductions on female enrollment



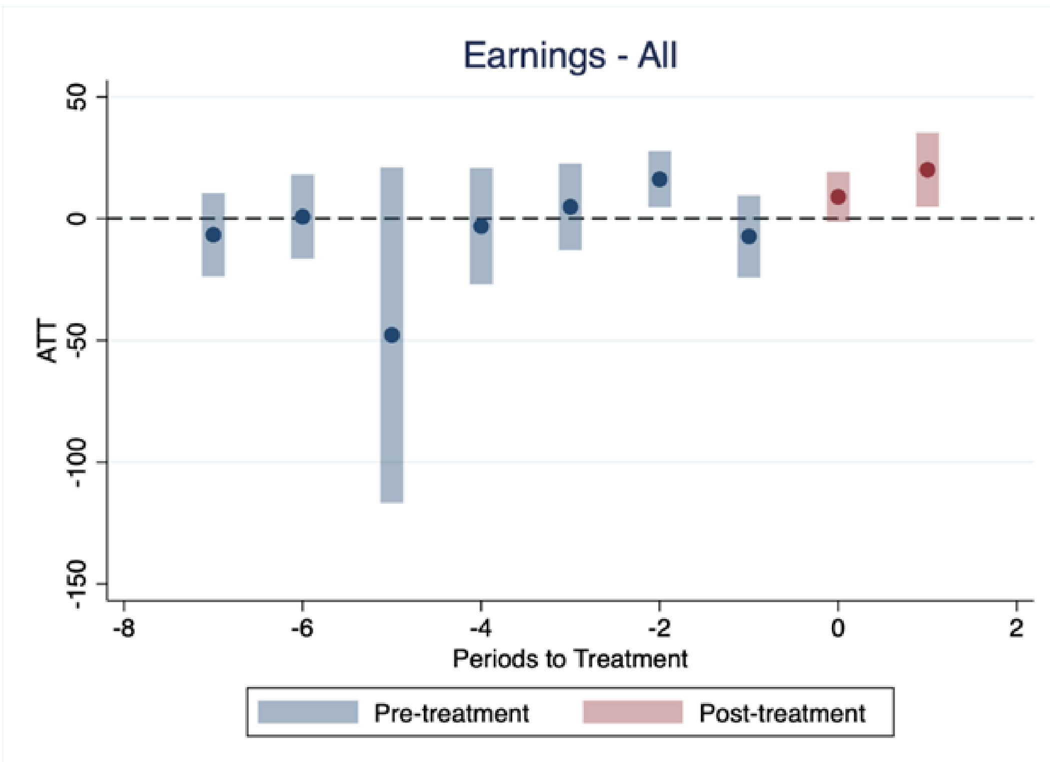
Notes: IPEDS program-level data.

Figure B-2: Robustness check: program entry and exit



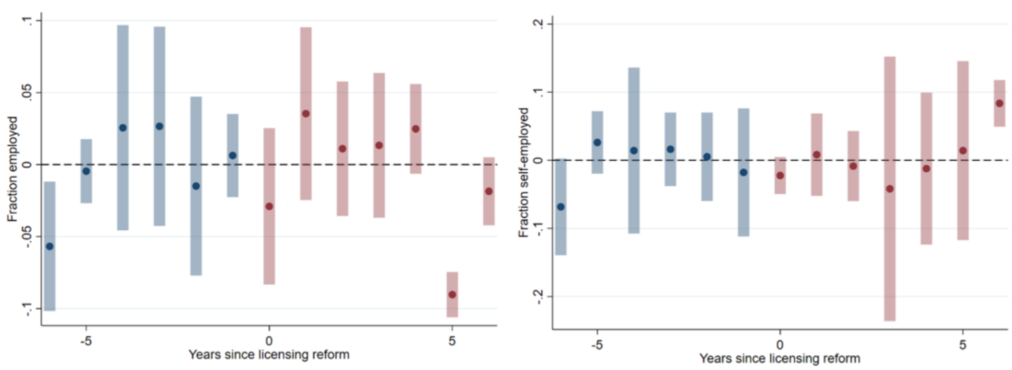
Notes: IPEDS program-level data.

Figure B-3: Robustness check: difference-in-difference estimates of the impact of licensing hour reductions in Utah, College Scorecard data



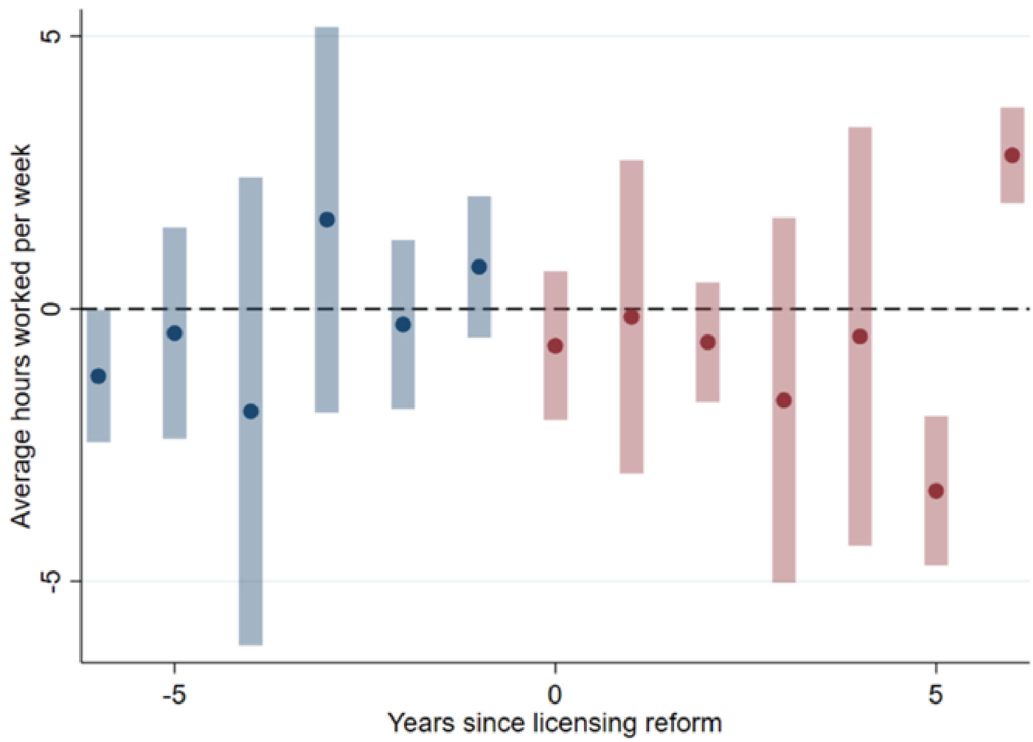
Notes: College Scorecard institution-level data. Only one state policy change included (Utah in 2013). Earnings are measured six years after entry.

Figure B-4: Impact of licensing hour reductions on employment and self-employment of cosmetologists



Notes: ACS state-level data for cosmetologists.

Figure B-5: Impact of licensing hour reductions on hours worked for cosmetologists



Notes: ACS state-level data for cosmetologists.

Table B-1: Licensing and training hours by occupation (from Acevedo, Blanchard, Cellini 2022, Table 1)

	Mean	Median	Minimum	Maximum	Number of states requiring training hours	Number of states requiring licensing
Barber	1417	1500	1000	2100	50	51
Certified Nursing Assistant	98	90	75	180	51	51
Cosmetologist	1550	1500	500	2100	51	51
Dental Hygienist	1600	1600	1600	1600	1	51
EMT	154	160	0	315	25	24
Electrician	98	0	0	1000	7	32
General Contractor	2	0	0	60	4	22
HVAC Contractor	28	0	0	1000	3	32
Home Inspector	43	0	0	180	24	30
Licensed Practical Nurse	0	0	0	0	0	51
Manicurist	377	350	0	750	50	50
Massage Therapist	500	500	0	1000	46	46
Occupational Therapist Assistant	0	0	0	0	0	46
Pharmacy Technician	22	0	0	480	4	36
Physical Therapy Assistant	0	0	0	0	0	51
Pipe-Fitter	1471	0	0	8048	14	35
Plumber	1541	0	0	8048	13	34
Pre-School Teacher	0	0	0	0	0	51
Radiologic Technologist	185	0	0	1850	1	42
Real Estate Agent	73	60	0	180	44	46
Real Estate Appraiser	19	20	0	60	28	51
Respiratory Therapist	0	0	0	0	0	50
Security-Fire Alarm Installer	26	0	0	576	13	16
Skin Care Specialist	621	600	260	1000	50	50
Transit City or School Bus Driver	1	0	0	26	2	51
Veterinary Technician	0	0	0	0	0	37

Notes: Mean, median, minimum, and maximum required training hours are calculated for all states that have licensing requirements for the occupation, including those that have no training hours requirement (coded as 0 hours), but have other requirements, such as an examination. The number of states requiring non-zero training hours are listed in column (5) and the number of states requiring any license are in column (6). Licenses are coded in the NCSL data and include variations in definitions across occupations and states. Values not coded in hours were coded as missing.

Table B-2: Descriptive statistics for treated and control states

	Tuition and Fees		Completions		Earnings	
	Treated	Control	Treated	Control	Treated	Control
2010	7,176	8,525	14	31	19,939	21,106
2011	7,323	9,235	15	33	12,837	13,802
2012	7,720	9,645	13	31	13,007	15,201
2013	8,053	9,944	15	29	13,031	16,745
2014	8,802	10,327	18	28	13,332	18,268
2015	8,984	10,413	18	25	13,715	18,179
2016	9,329	10,817	18	25	17,460	19,459
2017	10,742	11,385	22	26	19,386	19,401
2018	11,875	11,800	24	26	23,688	20,581
2019	16,502	16,519	26	25	22,194	23,048

Notes: Tuition and completion data from IPEDS, earnings data from ACS.