Educational Evaluation and Policy Analysis
Month 201X, Vol. XX, No. X, pp. 1–26
DOI: 10.3102/0162373717709968

The Costs and Consequences of Excess Credit Hours Policies

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The growth of the public discourse on college completion and student debt has pushed policymakers and institutional leaders to implement a variety of policies aimed at incentivizing student completion. This article examines state-adopted excess credit hour (ECH) policies on student completion and median debt outcomes. Using a quasi-experimental approach, we find little evidence that ECH policies positively affect student completion. However, we find statistically large estimates that adoption of ECH policies increase median student debt. Students from marginalized backgrounds (i.e., first-generation and low-income) appear to be most adversely affected by ECH policies. As states face constant pressures for resources, the adoption of tuition-based surcharges does not significantly alter student course-taking behaviors, rather shifts the cost burdens from the state to the individual student for perceived inefficiencies in students' course-taking behaviors.

Keywords: *econometric analysis, economics of education, educational policy, finance, higher education, policy, policy analysis, postsecondary education, quasi-experimental analysis, secondary data analysis*

Introduction

The price of attending a public college has increased markedly over the past several decades. Between 1985 and 2015, published tuition and fees at a typical 4-year public college increased from US$2,942 to US$9,500 in inflation-adjusted dollars—a 223% increase that far outpaced the small gains in real household income for most U.S. families (Ma, Baum, Pender, & Welch, 2016). Although grant aid has also increased during this period, the real average net price of attending a 4-year public college (defined as the total cost of attendance less all grant aid) rose from US$11,662 to US$13,419 between 2000 and 2012, a 15% increase (authors' calculation using the National Postsecondary Student Aid Study). Approximately, 68% of students of bachelor degree completers who borrowed student loans had US$30,100 in debt in 2015, up more than 20% since 2006 (Cochrane & Cheng, 2016).[1]

One factor that is likely contributing to rising debt burdens is that students are taking longer to complete a bachelor's degree than in prior decades. Not only must students pay tuition for the additional semesters, they must also pay for the living expenses that make up a majority of the cost of attendance at 4-year public colleges (Kelchen, Goldrick-Rab, & Hosch, 2017). In the 1972 cohort of high school graduates, 53% of BA completers obtained a college degree within 4 years of finishing high school. The percentage of completers within 4 years of high school graduation had dropped to 39% in the 1992 cohort (Bound, Lovenheim, & Turner, 2012) and was only 42% in the 2004 cohort (authors'

calculation using the Education Longitudinal Study). Among completers, the average time-to-degree increased from 4.34 years in the 1972 cohort, to 4.56 years in the 1992 cohort, and then to 4.83 years in the 2004 cohort (Adelman, 2004; author's calculation).

This trend in delayed degree completion is predominately seen in public universities rather than private, not-for-profit institutions (Bowen, Chingos, & McPherson, 2009). In fact, time-to-degree at highly selective private universities decreased from 4.31 to 4.20 years from the 1972 to the 1992 cohort (Bound et al., 2012). This decline for private 4-year institutions is juxtaposed with an increase in time-to-degree at top 50 public 4-year institutions (4.49–4.66 years) and an even larger increase (4.49–4.93 years) at non–top 50 public 4-year institutions over this 20-year period (Bound et al., 2012). In the 2004 cohort of high school graduates, 59% of students who began at private nonprofit colleges graduated within 4 years, compared with just 41% of those who started at public colleges (authors' calculation using The Education Longitudinal Study [ELS] data).

The average time-to-degree is longer for students from lower income families, who can little afford additional tuition and living expenses. Among students who graduated from high school in 2004, 32% of bachelor's degree recipients in the lowest socioeconomic status quartile finished their degree on time, compared with 48% of students in the highest quartile (authors' calculation using ELS data). This contributed to a 6-month differential in the average time-to-degree (62.1 months vs. 56.1 months). The increase in time-to-degree is concurrent with an increase in total credits obtained. Baccalaureate completers in the 1972 cohort accumulated 130.1 credits on average, whereas those in the 1992 cohort completed an average of 138.4 credits, and students in the 2004 cohort completed 138.6 credits (Adelman, 2004; authors' calculation using ELS data).[2] A similar differentiation between public and private 4-year institutions can be seen regarding credits accumulated. Among 1992 high school graduates, those completing a baccalaureate degree at public 4-year universities accumulated 133.1 credits on average, compared with the 129.3 credits of their peers at private, non-for-profit 4-year institutions (McCormick, 1999; Radford & Horn, 2012). Public college graduates in the 2004 cohort completed 140.8 credits, compared with 129.5 credits for private college graduates (authors' calculations using ELS data).

Research about the factors affecting time-to-degree has pointed to several important considerations. Bound et al. (2012) found that the recent increase in time-to-degree at public universities is driven by a decline in institutional resources, lessening the ability of colleges to graduate students on time, not by changes in student characteristics such as college preparedness or demographic factors. In contrast, Kurlaender, Jackson, Howell, and Grodsky (2014) found that course scarcity did not delay graduation at the University of California–Davis.

There are also factors outside the higher education system that may affect time-to-degree. Evidence from Italian and European universities indicate that labor market quality is also associated with the time it takes to obtain a bachelor's degree (Aina, Baici, & Casalone, 2011; Brunello & Winter-Ebmer, 2003). In the German university system, increased time spent in part-time work outside the university is related to a lengthening time-to-degree (Behr & Theune, 2016). Triventi (2014) distinguished between low- and high-intensity employment, and found that only the latter delayed academic progression in Europe. Bound et al. (2012) also pointed to employment hours crowding out educational time in the U.S. system, a consideration that has likely become more important as students work to fund an ever-more-expensive degree.

Both institutions and states have long developed policies and programs aimed at increasing student completion and reducing time-to-degree. For example, Temple University implemented a "Fly in Four" campaign, offering to pay for remaining credits after 4 years for students who have met numerous checkpoints. Scott-Clayton (2011) showed that West Virginia's merit aid program, which required students to complete 30 credits per year, significantly increased 4-year graduation rates. Bettinger and Baker (2014) articulated the potential benefits of the technology-aided advising as a way to reduce unneeded credits. Finally, Tinto (2010) and Calcagno, Crosta, Bailey, and Jenkins (2007) both documented the impact of structured degree pathways on timely bachelor's degree completion.

As a mechanism to combat increases in time-to-degree, several states—including Arizona, Florida, North Carolina, and Texas—have legislated policies designed to discourage students at public colleges and universities from taking too many credits. These excess credit hour (ECH) policies assess a tuition surcharge for credits taken beyond a predetermined cutoff, usually between 115% and 130% of the degree's usual required credits. At their core, ECH policies are designed to incentivize student completion by increasing student costs associated with untimely progress toward a bachelor's degree. The implementation of the ECH policies is not a new phenomenon—North Carolina implemented the first ECH policy in Fall 1992—however, states have become increasingly interested in adopting policies aimed at increasing student completion and minimizing state investment (Complete College America, 2011).

Policies designed to curb ECHs could also have the effect of curbing student loan debt if students are able to finish in fewer semesters and pay less in tuition and living expenses. However, these policies could actually increase debt burdens for two groups of students. The first group includes those who could be induced to finish in fewer semesters but would have to borrow more money to substitute for employment income. The second group comprises those students who have to pay the higher per-credit price under ECH policies because they were unable or unwilling to complete their degree within the specified number of credits. As a result, it is entirely possible that an ECH policy is associated with rising student debt.

Using a generalized difference-in-difference (GDD) approach, this study examines the institution-level effect of ECH policy adoption on the intended (degree completion and time-to-degree) and the possibly unintended (student debt) outcomes. Previous evidence of the direct and indirect impact of ECH policy on student enrollment behaviors and outcomes is scarce. Our analysis of institution-level responses to ECH policies leads us to ask the following research questions:

Research Question 1: To what extent are degree completion and time-to-degree measures affected by the adoption of an ECH policy?

Research Question 2: To what extent is the presence of an ECH policy associated with student debt burdens?

Research Question 3: How do underrepresented and/or low-income students respond to the adoption of ECH policies?

Results from our study have failed to substantiate any positive impact of ECH policies on degree production or time-to-degree. Instead, our results demonstrate that the adoption of ECH policies significantly increases median student debt. Results from our study appear to demonstrate that ECH policies do not significantly alter student course-taking behaviors, but rather shift the cost burdens from the state to the individual student for actual or perceived inefficiencies in students' course-taking behaviors.

Literature Review

Effects of Costs on Completion and Time-to-Degree

Emerging literature examines the impact of tuition policies on time-to-degree internationally. This literature stems from the introduction of tuition and fees within institutions that have traditionally been tuition free or low tuition compared with U.S. institutions. Garibaldi, Giavazzi, Ichino, and Rettore (2012) exploited discontinuities in the formula for tuition at Bocconi University to find that a 1,000-euro increase in tuition during the final regular year leads to a 5.2% decrease in the probability of late graduation. Students, they argued, view final-year tuition as a predictor for tuition in potential extra years of study and adjust their efforts to graduate on time when faced with a tuition hike. Bruckmeier, Fischer, and Wigger (2015) used a difference-in-difference approach to examine the natural experiment of tuition fee introduction at German public universities in seven out of 16 states in 2007. They found that the introduction of even modest tuition fees—public universities were previously free—significantly reduced time-to-degree in states that adopted them. Gunnes, Kirkebøen, and Rønning (2013) studied the effects of a Norwegian policy that provided an incentive of approximately US$3,000 for on-time graduation from 1990 to 1995. Evidence from their difference-in-difference design suggests that the incentive

reduced the average graduation delay by 0.23 semesters per year treated. A randomized experiment from the Netherlands showed that financial rewards for on-time completion of first-year requirements increased total credits completed in the first year only for high-ability students; low-ability students were made worse off by the incentive, possibly because the external reward crowded out their intrinsic motivation (Leuven, Oosterbeek, & van der Klaauw, 2010).

Domestically, scholars have focused primarily on increases in tuition and fees as well as the role of need-based and merit aid programs in students' time-to-degree and completion. Hemelt and Marcotte (2011) found that for every US$100 increase in tuition and fees, institutions experienced a significant decrease in enrollment. This effect was particularly concentrated within doctoral institutions. They also found that similar increases to in-state tuition and fees significantly reduced student credit hours completed. In addition, there exists limited evidence on the effects of tuition surcharges affecting total credits accumulation, increasing persistence, or timely degree completion (Hemelt & Stange, 2016). Other evidence from the end of the 20th century suggests that student persistence is less affected by tuition increases than by changes in financial aid (Paulsen & St John, 1997; St John, 1990). One study of a potential increase in the tuition cap at a Wyoming community college found modest impacts on credit-taking behaviors (Davis et al., 2015).

Recently, scholars have focused increasing attention on the role of financial aid in promoting student persistence and completion. The significant focus on the effects of financial aid on completion has led to mixed results. Evidence from a merit-based grant program in West Virginia shows that financial aid packages with yearly renewal tied to timely credit accumulation decreased time-to-degree and increased degree attainment (Scott-Clayton, 2011). In addition, Angrist, Lang, and Oreopoulos (2009) found that performance-based scholarships have a positive effect on student postsecondary grade point averages (GPAs) and persistence toward a degree. Evidence from need-based grant programs in Florida and Wisconsin shows that the aid packages increased recipients' likelihood of completion and rate of credit accumulation (Castleman & Long, 2016; Goldrick-Rab, Kelchen, Harris, & Benson, 2016). Studies that have examined grant-in-aid programs without required academic performance metric have demonstrated even less conclusive evidence regarding positive effects on continued student persistence or degree completion (Bettinger, 2004; Glocker, 2011).[3] Prior work by DesJardins, McCall, Ott, and Kim (2010) has found little evidence that the Gates Millennium Scholarship increased retention and course-taking patterns for academically qualified and low-income students.

Prior Work on Policy Adoption and Student Debt

Prior research has affirmed the role of statewide tuition policies, specifically in the funding and support of higher education, in influencing student debt levels (Mortenson, 1998; Zumeta, 2004). Early work by Hartman (1972) warned that a move away from state-subsidized tuition at public universities toward loan financing would have negative consequences for low-income families. Of course, such a shift has occurred, and the effect of changing state funding on student debt has been documented (Heller, 2006). Monks (2001) found that need-blind admission policies, loan limits, and major choice have a larger impact on student debt levels than an institution's costs of attendance. Changes in both federal and state policies have produced significant increases in the prevalence and levels of student debt. Specifically, between 1996 and 2012, students at public 4-year institutions have increased their borrowing rate from 38% to 50% (Horn & Paslov, 2014).

Prior Literature on ECH Policies

Evidence about the effects of lengthening time-to-degree has indicated several areas of concern. On a personal level, increases in the time taken to complete a baccalaureate degree may delay entry into the labor market and diminish lifetime earnings (Monks, 1997; Taniguchi, 2005). Aina and Pastore (2012), examining the Italian university system, suggested that delayed graduation signals educational underperformance in the labor market, dampening late graduates' wage opportunities. Findings from

Brodaty, Gary-Bobo, and Prieto (2008) support this, with a 1-year delay in graduation causing a 9% decrease in average wage and a 20% decline in the probability of employment in the first five postgraduation years in France. Furthermore, evidence from the United States suggests that students who progress slowly in the beginning of their studies have a lower probability of graduating at all (Attewell, Heil, & Reisel, 2012). These effects are compounded by the fact that lengthening the time of university study plagues students of low socioeconomic status and minority students disproportionately (Bowen et al., 2009), implying that the costs of delayed graduation are borne by the most vulnerable students.

Whereas the individual costs are substantial, the institutional and state-level costs may be greater—particularly for public institutions (Turner, 2004). Given that each credit completed is subsidized either through state appropriations or institutional grants and aid, the costs directly to the student are only a fraction of the total cost as they do not include the public subsidies provided (Bowen et al., 2009). One study of an unnamed state's community college system estimated the costs of ECHs to be at least US$6 million per year, not including failed and dropped credits (Zeidenberg, 2015). Bound and Turner (2007) found that the supply of higher education is relatively inelastic; demand shocks produced by an increase in the college-age population lead to a lower rate of completion. Following this reasoning, an analogous demand shock in the form of late-graduating students failing to normally progress through the system is likely to crowd out the supply of higher education by occupying course seats that would otherwise be available to incoming or returning students.

Contribution

Our research explores the effect of ECH policies on both intended and unintended institution-level outcomes. ECH policies have gained traction within state legislative bodies and state higher education systems as pressures related to efficiency, affordability, and completion mount. Our study contributes to higher education policy literature in a number of ways. First, this study represents the first known systematic evaluation of ECH policies on degree production, 4- and 6-year graduation rates, and student debt. Whereas a few international studies have considered single policy adoptions (Bruckmeier et al., 2015; Garibaldi et al., 2012), this study examines the systematic adoption of ECH policies in the United States through a quasi-experimental approach. Second, our study extends the literature base on policy-adoption methods, accounting for variations in policy shock implementation. Whereas traditional difference-in-difference methods focus on a single policy adoption, our approach builds on prior work (Heckman, Ichimura, & Todd, 1997) to provide a more flexible estimation parameter. Finally, our findings add to the scarce literature on the unintended effects of completion programs that create incentives for students through tuition-based surcharges.

Data and Research Design

We constructed a panel data set from 2000 to 2013 using data from the U.S. Department of Education's Integrated Postsecondary Education Data System (IPEDS), the College Scorecard, and state-level factors extracted from the Bureau of Economic Analysis, Bureau of Labor Statistics, and Carl Klarner and the National Conference of State Legislatures. The primary goal of this study was to estimate the impact of adopting ECH policies on graduation rates and student median debt. To do this, we included all public 4-year institutions[4] (n = 506) over our 14-year (2000–2013) panel—a total of 6,912 observations.

Variables

All measures included within our analysis were log transformed except for percentage measures of dummy coded indicators (e.g., Democratic Governor). The decision to log transform was made to reduce our model's sensitivity to variations in institutional size and to allow our estimands to be interpreted as a percentage change (Cameron & Trivedi, 2005). However, Meyer (1995) has argued that difference-in-differences (DiD) estimations can be sensitive to the selected functional form. Specifically, logged nonlinear transformations within a DiD specification can change signs when applied to a dependent variable. To account for this potential

TABLE 1

State-Adopted Excess Credit Hour Policies

State	Implementation year (academic year)	Excess credit hour threshold	Fee amount	Institutions subjected to the policy
Arizona	Fall 2007	145 credits	120% of tuition rate	Public 4-year universities
Florida	Fall 2009	110% of program of study	200% of tuition rate	Public 4-year and 2-year institutions
Massachusetts	Fall 1999	118% of program of study	Out-of-state tuition rate	Public 4-year and 2-year institutions
Nevada	Fall 2014	150% of program of study	150% of tuition rate	Public 4-year and 2-year institutions
North Carolina	Fall 1992	140 credits	125% of tuition rate	The University of North Carolina System Institutions
Texas	Fall 1999	45 credits beyond program of study	Out-of-state tuition rate	Public 4-year and 2-year institutions
Utah	Fall 2013	125% of program of study	200% of tuition rate	Public 4-year and 2-year institutions
Virginia	Fall 2006	125% of program of study	200% of tuition rate	Public 4-year institutions
Wisconsin	Fall 2004	165 credits, or 30 more than required program of study	200% of tuition rate	Public 4-year and 2-year institutions

Source. Excess credit hour policy adopting states and years were generated by reviewing state websites and legislative documents, policy databases from the Education Commission of the States and the National Council of State Legislatures, and other reliable secondary sources.

limitation, we ran model specifications that included our dependent variable nontransformed to test whether our specifications were sensitive to functional form. Our functional form tests indicate that our point estimates were not dependent on a functional form choice. We then chose to follow the recommendation of Wooldridge (2009) and log transformed our dependent variables for efficiency and ease of interpretation. An additional concern with our empirical design was the potential presence of various composition effects. To test for the possible presence of these effects, we reestimated our main-effects tables to include institution-by-year interaction terms. After employing these checks for composition effects, we obtained virtually the same results in both magnitude and directionality.

Independent Variable(s). Within this study, we were primarily interested in analyzing the effect of ECH policy adoption on a variety of direct and indirect outcomes. Because there is no single comprehensive list of the ECH adopters, we developed one by examining state legislative records, institutional websites, and a variety of policy reports to generate our ECH adoption variable.[5] This included contacting administrators from institutions within adopting states to confirm not only the timing of adoption but also the mechanisms and provisions of the ECH policy. For our study, ECH policies must have met the following requirements: (a) a state adopted the policy, (b) a threshold exists based on total credits completed, and (c) the policy affects public 4-year institutions. Table 1 provides an overview of state adopted excess credit hour policies.

Dependent Variable(s). Because the primary stated aim surrounding the adoption of ECH policies is to incentivize students to complete their baccalaureate degree within 4 years, our first set of outcomes addressed the intended outcomes of facilitating completion for students. To do so, we captured three different measures of degree

completion and graduation. First, we estimated the impact of ECH policies on total degrees produced. Second, we examined both the 4-year and 6-year graduation rates, which serve as proxies for time-to-degree.[6] Taken in combination, our three completion measures will illustrate the effect of ECH polices on the ability to incentivize changes in student course-taking patterns. For example, no significant effect on total degrees produced but an increase in the 4-year graduation rate would signal that an institution is not producing more degrees, but rather a larger proportion of degree completers are graduating within 4 years.

In addition to analyzing the impact of ECH policies on intended completion outcomes, we also postulated that implementation of additional fiscal charges may have an unintended consequence of increased student debt. Using data from the U.S. Department of Education's College Scorecard, we examined median student debt, median student debt of graduates, and median student debt for a variety of subgroups. With our data set, we focused primarily on changes in student debt for low-, middle-, and high-income students. A number of scholars (Avery & Kane, 2004; Grodsky & Jones, 2007; Horn, Xianglei, & Chapman, 2003) have documented that students from low-income and other disadvantaged backgrounds exhibit difficulties in estimating costs associated with tuition and fees.

Covariates. As discussed within our "Analytical Strategy" section, we used a GDD technique to estimate the effects of ECH adoption. We selected covariates based on prior studies that have found statistically significant relationships among the number of degrees produced, completion rates, or levels of student debt. Our institution-level covariates included the percentage of undergraduate students receiving federal grant aid and the percentage of undergraduates who are underrepresented minority students, factors that are associated with lower completion rates and higher debt burdens (Bahr, Toth, Thirolf, & Masse, 2013; Hillman, 2015; Ishitani, 2006; Titus, 2006).[7] We also controlled for factors such as total undergraduate enrollment, published in-state tuition and fees, published out-of-state tuition and fees, and the percentage of degrees produced by broad academic subject area, which are also significant predictors of our outcomes of interest (Chen & Wiederspan, 2014; DesJardins, Kim, & Rzonca, 2003).

In addition to controlling for institutional factors affecting our outcome measures, we also included state-level economic and political conditions because state legislatures and budgets ultimately direct the funding and tuition policies of public universities. To this end, we included measures of party control (i.e., if the governor was a Democrat and if the state legislative body was under Democratic control) and overall economic conditions in each state (i.e., poverty rate and adjusted per capita income) that have been shown to affect public universities (Doyle, 2012; Humphreys, 2000; McLendon, Hearn, & Mokher, 2009; Weerts & Ronca, 2012).

Analytical Strategy

To estimate the overall impact of the adoption of ECH policies, we borrowed similar methodological approaches from S. Dynarski (2000), Hillman, Tandberg, and Gross (2014), and Zhang and Ness (2010), who each used a combination of the traditional ordinary least squares (OLS) fixed-effects regression parameters and the DiD approach to study institutional and student responses to state-level policy adoption. In an environment where there is a single policy shock at a single time period, we specified Equation 1 as follows:

$$\begin{aligned} Y_{it} = {} & \alpha + \beta_1 ECH_s + \beta_2 Post_t + \\ & \beta_3 \left(ECH_s \times Post_t \right) + W_{it} + \\ & V_{st} + \lambda_i + \delta_t + \varepsilon_{st}, \end{aligned} \tag{1}$$

where ECH_s is an indicator valued at 1 if an institution was located within a state that adopted an ECH policy at any time during the analytical sample and zero otherwise, $Post_t$ has a value of 1 when the time period is after the adopting year—for example, if we were evaluating Florida's adoption, this would be 2009 and afterward—and zero otherwise. $ECH_s \times Post_t$ is the DiD coefficient that represents the estimate of causal effects of ECH adoption on outcome Y for institution i during time t.

Although a traditional DiD approach is most efficient in estimating the impact of a single policy shock, the adoption of ECH policies across multiple states has not occurred during a single

period. To account for the varying adoption periods, we implemented a generalized DiD model. Building on the logic provided by Belasco, Rosinger, and Hearn (2014), we specify Equation 2 as our GDD:

$$Y_{it} = \alpha + \beta_1 ECH_{it} + W_{it} + V_{st} + \lambda_i + \delta_t + \varepsilon_{st}, \qquad (2)$$

where ECH_{it} is a dichotomous indicator equal to 1 during the year of first-year post–state adoption of an ECH policy and afterward and 0 prior to adoption or if an institution is located within a state that has not adopted an ECH policy. W_{it} is a vector of institution-level continuous covariates that affect our dependent variable (as discussed previously). V_{st} is a vector of state-level factors that not only affect the postsecondary environment but also directly affect our dependent variable. δt is the year fixed-effect, λ_i is the institutional fixed-effect, and ε_{st} is the state-clustered robust standard error. The decision to cluster at the state level is an attempt to relax assumptions about heteroskedasticity and serial correlation between institutions within states.

The causal estimates from Equation 1 are approximated within Equation 2 through two-way fixed effects (Bertrand, Duflo, & Mullainathan, 2004). To this end, the first difference between institutions in adopting and nonadopting states is accounted for within our institution fixed effects (λ_i), and the second difference between pre- and post–time periods is accounted for within our year fixed effects (δt). The resulting coefficient on $\beta_1 ECH_{it}$ is our causal GDD estimate.

Robustness Checks

The difficulty in policy-based research is approximating what *would* have happened for institutions within adopting states had the state never adopted an ECH policy. To account for this, we implemented a variety of robustness checks to present plausible scenarios of what *could* have occurred in the absence of an ECH policy.

Control Groups. Within our difference-in-difference framework, it is important that the treated group (i.e., institutions in adopting states) and the comparison group (i.e., institutions in nonadopting states) followed parallel (or similar) trends prior to the introduction of the policy (Blundell & Costa Dias, 2000). Because this is difficult to accomplish, we utilized three different control groups to illustrate that our results are not sensitive to the varying preadopting trends associated with any one group of institutions in nonadopting states.

Group 1, National Comparison: Our analysis began with the most general control group—institutions within states that have not adopted an ECH policy—and then narrowed to states that share regional commonalities. Within the national comparison, we included public 4-year institutions located within 41 nonadopting states. As a robustness check, we included states that adopted an ECH policy after the time frame of our analytical sample as a control and achieved similar results.

Group 2, Regional Association: In addition to using our nonadopting states as a control group, second, we limited our sample to regions where states have adopted ECH policies. We also limited our sample to regions with ECH policies to account for any region-based tuition reciprocity agreements (Cornwell, Mustard, & Sridhar, 2006; DesJardins, Ahlburg, & McCall, 2006) or other policy-relevant set of counterfactuals. Including all regions in which ECH policies have been adopted yielded a comparison group of 217 distinct nonadopting institutions from 24 unique states.[8] In total, this comparison included 31 states, 318 unique institutions, and a total of 4,449 observations.

Group 3, Border States: As is common with difference-in-differences approaches, we utilized a second set of geographic boundaries to specify our third control group (S. Dynarski, 2000; Flores, 2010; Zhang & Ness, 2010). Specifically, we leveraged geographic proximity and included states that share a physical border with adopting states. These "border states" share similar demographics and regional characteristics with adopting states. The nonadopting border states group consists of 23 different states, 246 unique institutions, and a total

TABLE 2

*Mean (*SD*) of Key Outcomes and Covariates*

	Overall sample	ECH adopters	Nonadopters: National	Nonadopters: Region	Nonadopters: Border states
Dependent variables (outcomes)					
Total bachelor degrees	3947.21 (3950.58)	5167.77 (4749.94)	3648.83 (3668.4)	3377.84 (3403.43)	4036.75 (3651.29)
6-year graduation rate (%)	0.46 (0.16)	0.49 (0.16)	0.45 (0.16)	0.41 (0.15)	0.45 (0.17)
Median student debt (constant dollars)	14348.72 (3199.96)	14330.55 (2828.68)	14353.16 (3284.55)	13981.85 (3230.43)	13929.53 (3177.86)
Low-income student debt (constant dollars)	14466.99 (3404.09)	14361.48 (3032.25)	14492.17 (3486.76)	14118.45 (3511.71)	14139.31 (3456.96)
Middle-income student debt (constant dollars)	14206.12 (3573.52)	14291.99 (3260.85)	14185.13 (3645.83)	13657.14 (3535.39)	13710.56 (3512.32)
High-income student debt (constant dollars)	12235.03 (3658.88)	12263.62 (3517.38)	12227.96 (3693.32)	11516.48 (3595.74)	11860.72 (3478.22)
Institutional covariates (controls)					
Total undergraduate enrollment	11769.93 (9296.82)	14961.99 (11480.76)	10979.1 (8488.79)	10832.92 (8198)	11829.79 (8323.3)
In-state undergraduate students (%)	81.13 (20.14)	81.39 (20.64)	81.07 (20.02)	79.85 (20.46)	81.71 (20.18)
Undergraduates receiving federal grants (%)	35.26 (16.68)	31.83 (17.47)	36.1 (16.37)	37.38 (17.44)	36.56 (17.22)
Undergraduates receiving institutional aid (%)	35.39 (20.51)	30.68 (18.09)	36.55 (20.9)	36.56 (21.04)	34.13 (20.93)
In-state tuition and fees (constant dollars)	6651.92 (2942.48)	5943.22 (2774.46)	6845.58 (2957.61)	6336.22 (2811.87)	6461.83 (2734.12)
Out-of-state tuition and fees (constant dollars)	14367.22 (5340.86)	14772.81 (5671.17)	14256.42 (5242.15)	14260.13 (5530.62)	14188.8 (5398.97)
Undergraduates students: Asian (%)	5.07 (7.32)	4.27 (4.25)	5.27 (7.88)	3.25 (4.19)	6.07 (8.54)
Undergraduates students: Black (%)	13.14 (21.6)	14.11 (23.48)	12.91 (21.11)	15.66 (23.24)	14.97 (22.34)
Undergraduates students: Hispanic (%)	6.38 (11.14)	10.58 (17.59)	5.35 (8.57)	3.8 (7.41)	6.83 (10.3)
Undergraduates students: Other (%)	6.69 (7.16)	5.34 (6.96)	7.01 (7.17)	5.29 (5.29)	7.43 (7.57)
State-level covariates (controls)					
Governor Democrat	0.49 (0.5)	0.38 (0.48)	0.52 (0.5)	0.46 (0.5)	0.52 (0.5)
State legislative Democrat (%)	50.5 (14.37)	48.99 (15.82)	50.87 (13.97)	51.29 (13.21)	56.4 (11.26)
Per capita income (constant dollars)	50352.23 (7925.75)	51025.07 (7209.4)	50187.74 (8083.3)	46076.83 (7061.16)	50396.15 (8616.72)
Poverty rate (%)	13.05 (3.15)	13.14 (3.16)	13.03 (3.15)	14.06 (3.29)	13.77 (3.19)
Observations and groups					
No. of states	50	8	42	24	23
No. of institutions	514	101	413	217	246
Total observations	5,193	1,413	5,780	3,036	3,442

Note. ECH = excess credit hour.

of 3,442 nonadopting observations. Table 2 provides an overview of descriptive statistics for ECH adopters and our three comparison groups.

Timing. First, we utilized falsification tests (Cook & Campbell, 1986) to overcome a major concern with quasi-experimental approaches, untangling the policy effect from a potential corresponding time effect. To this end, we artificially created the adoption of an ECH policy a number of years prior to the actual adoption. Significant results prior to the actual adoption signaled that the estimated impact on our dependent variable was not a product of the ECH policy adoption, but rather a time effect that happened to coincide with the adoption. In addition, we also added an institution-specific trend to the set of covariates (Angrist

& Pischke, 2009; Belasco et al., 2014). This inclusion controls for the potential that institutions within adopting states may have experienced differences in the outcomes of interest prior to adopting an ECH policy. Accordingly, institutional trend variables were created by regressing dummy time variables for the years prior to adoption on each of the dependent variables and by multiplying the resulting coefficient by year to create a unique institutional trend variable. After running model specifications with our preadoption trend variable, we found similar results.

Limitations

Before the presentation of our results, we acknowledge the limitations of our empirical results and generated estimates. First, our data were extracted from both IPEDS and the College Scorecard. The National Center for Education Statistics (NCES), while collecting similar measures, provides institutions with different technical guidance on reporting aggregates at the campus level (Jaquette & Parra, 2014).[9] To account for this, we ran model specifications to exclude parent institutions that may have reported multiple child campuses in a single year. For our analysis on student debt—as reported by the College Scorecard and subject to the parent–child report—we also ran model specifications where our data from IPEDS were aggregated from the "child level" to the "parent level" prior to merging with our College Scorecard data. These results indicated that our point estimates were not sensitive to this reporting difference. In addition, we clustered our standard errors at the state level to produce more conservative tests of significance.

Results

Descriptive Statistics

Table 2 provides descriptive statistics of the variables used within our difference-in-difference specification to estimate the impact of ECH adoption. This table compares institutions within adopting and nonadopting states. In general, institutions within adopting states produced significantly more bachelor's degrees and have notably higher 6-year graduation rates, on average, than institutions within nonadopting states. In addition, ECH institutions also possess slightly lower levels of median student debt across income categories. Institutions within adopting states appear to have larger average undergraduate enrollments, a smaller percentage of in-state student enrollment, and a higher out-of-state tuition and fee level. In addition, institutions within adopting states appear to be less diverse, both economically and in their racial/ethnic composition. At the state level, adopting states were less Democratic—both in gubernatorial and legislative control—and slightly more affluent.

Figure 1 provides three panels that illustrate the pre- and posttrends for institutions both within adopting states and our control institutions. The first panel provides the trends on the 4-year graduation rate. As is evident, regardless of the adoption year, institutions in adopting and nonadopting states followed similar trends both before and after adoption. We do not find visual evidence of changes, either positive or negative, in the trajectory of institutions. Our second panel examines similar trends on the 6-year graduation rate. Again, we fail to find visual evidence that the implementation of an ECH policy altered 6-year graduation rates. Unlike both our graduation rate measures, we do see visual evidence that the median student debt gap between adopting institutions and nonadopting institutions begins to close post adoption. The most notable visual impact of ECH adoption on median student debt is shown in Virginia and Wisconsin, whose preadoption trend was below that of the nonadopting controls, but by the end of our analytical sample, the median student debt had surpassed institutions within nonadopting states.

Impact of ECH Policies on Completion

In Table 3, we present results from our fitting Equation 2 on our analytical sample, focusing on the impact of ECH policy adoption on total number of degrees awarded, 4-year graduation rate, and 6-year graduation rate. Each outcome is disaggregated by our comparison groups outlined previously: (a) national, (b) regional, and (c) border states. Across these three groups, we attempt to illustrate consistently emerging patterns not influenced by the selection of a particular control group. Each control group provides its own strength, and it is not our goal to generate the "ideal" control group but rather to examine the

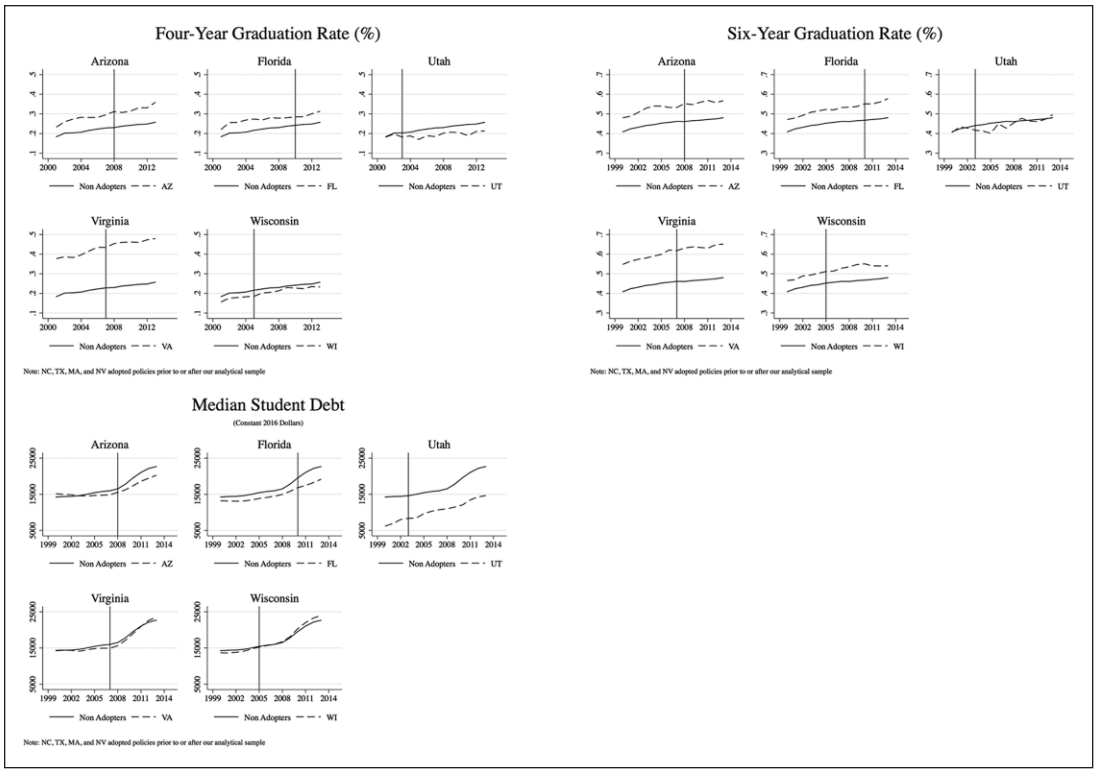

FIGURE 1. *Pre- and posttrends of adopting and nonadopting institutions on outcomes.*

extent to which our estimation patterns are consistent across multiple counterfactuals.[10] To account for the delayed effect of our outcome variable, we lagged our ECH policy adoption indictor 4 and 6 years. For each of total bachelor's degrees (logged), 4-year graduation rate (percent), and 6-year graduation rate (percent), we fail to find any evidence of a statistically significant relationship between ECH adoption and completion. Across our various control groups, our point estimates are robust and stable.

Although we failed to find a significant effect on an institution's overall degree production and graduation rate, prior research indicates that various subgroups may respond differently to the presence of the fiscal incentives—or disincentives. To this end, Table 4 disaggregates the ECH adoption effects on degree production as well as 4-year and 6-year graduation rates by race/ethnicity.[11] Similar to the estimates for our overall completion outcomes, disaggregated results indicate no significant change in degree production and 4-year graduation rate, post adoption, for any racial/ethnic subgroup. Despite no statistical impact in degree production and 4-year graduation rate, the 6-year graduation rate for Hispanic/Latino students appears to increase significantly post adoption—between 2.5% and 3.4%. In contrast to the positive effects for Hispanic/Latino students, we find marginal significant evidence that the 6-year graduation rate for African American/Black students significantly decreased 6 years after ECH adoption—between 3.5% and 4.2%. The prior work of Nora, Barlow, and Crisp (2006) has provided insight into the differing responses to financial aid programs and/or financing of postsecondary education for students from variable racial/ethnic subgroups.

Impact of ECH Policies on Student Debt

Tables 5 through 8 display results from our GDDs analyses, where each table provides point estimates for the changes in median student debt by a variety of subgroups and income levels. As with our analyses of degree production and

TABLE 3

ECH, Total BA Degrees, Graduation Rate

	Total bachelor degrees (logged)			Four-year graduation rate (%)			Six-year graduation rate (%)		
	National	Region	Border	National	Region	Border	National	Region	Border
ECH adoption	0.001	0.009	0.006	−0.003	0.001	−0.008	−0.004	−0.003	−0.008
	(0.009)	(0.014)	(0.009)	(0.004)	(0.004)	(0.004)	(0.005)	(0.006)	(0.005)
Number of observations	3,519	2,279	2,553	4,321	2,781	3,145	3,521	2,281	2,555
Number of groups	470	303	340	470	303	340	470	303	340
R^2 (within)	.466	.467	.485	.1550	.143	.220	.1170	.120	.149
State-level time-varying covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Institution-level time-varying covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Institution fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note. State clustered robust standard errors in parentheses. ECH adoption and model covariates are lagged 6 years for total BA degree production and 6-year graduation rate and 4 years for 4-year graduation rate. ECH = excess credit hour.
$^{*}p < .05$. $^{**}p < .01$. $^{***}p < .001$.

TABLE 4

ECH, BA Degree Production, and Graduation Rate: Disaggregated

	Total bachelor degrees (logged)			Four-year graduation rate (%)			Six-year graduation rate (%)		
	National	Region	Border	National	Region	Border	National	Region	Border
Overall student graduation rate	0.001	0.009	0.006	−0.003	0.001	−0.008	−0.004	−0.003	−0.008
	(0.009)	(0.014)	(0.009)	(0.004)	(0.004)	(0.004)	(0.005)	(0.006)	(0.005)
Caucasian/White	0.008	0.023	0.010	−0.008	0.005	−0.011	−0.008	−0.004	−0.011
	(0.013)	(0.020)	(0.014)	(0.006)	(0.005)	(0.008)	(0.006)	(0.007)	(0.007)
African American/Black	0.112	0.141	0.130	−0.000	0.014†	−0.003	−0.035*	−0.020	−0.042**
	(0.102)	(0.102)	(0.101)	(0.009)	(0.008)	(0.010)	(0.014)	(0.012)	(0.013)
Hispanic/Latino	0.038	0.076*	0.063	−0.009	−0.000	−0.012	0.025*	0.032**	0.034**
	(0.035)	(0.033)	(0.041)	(0.011)	(0.011)	(0.011)	(0.010)	(0.009)	(0.010)
State-level time-varying covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Institution-level time-varying covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Institution fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note. State clustered robust standard errors in parentheses. ECH = excess credit hour.
$^{\dagger}p < .10$. $^{*}p < .05$. $^{**}p < .01$. $^{***}p < .001$.

TABLE 5

ECH and Overall Debt

	Median student debt (logged)			Median student debt (logged)			Median student debt (logged)			Median student debt (logged)		
	National	Region	Border	National	Region	Border	National	Region	Border	National	Region	Border
ECH adoption	0.005	−0.008	0.007									
	(0.028)	(0.030)	(0.027)									
ECH adoption (3-year lag)				0.049	0.044	0.052*						
				(0.026)	(0.029)	(0.025)						
ECH adoption (4-year lag)							0.065***	0.072**	0.057**			
							(0.016)	(0.022)	(0.016)			
ECH adoption (5-year lag)										0.057**	0.063**	0.049**
										(0.017)	(0.021)	(0.017)
Number of observations	6,340	4,097	4,594	445	300	315	441	296	312	441	296	312
Number of groups	477	308	346	4,784	3,209	3,383	4,185	2,813	2,969	3,776	2,541	2,683
R^2 (within)	.772	.767	.775	.779	.781	.788	.765	.769	.774	.749	.751	.759
State-level time-varying covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Institution-level time-varying covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Institution fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note. State clustered robust standard errors in parentheses. ECH = excess credit hour.
$^{\dagger}p < .10$. $^{*}p < .05$. $^{**}p < .01$. $^{***}p < .001$.

TABLE 6

ECH and Debt by Income

	Low-income student debt (logged)			Middle-income student debt (logged)			High-income student debt (logged)		
	National	Region	Border	National	Region	Border	National	Region	Border
ECH adoption (4-year lag)	0.048**	0.051*	0.041*	0.056***	0.055**	0.053**	0.041	0.039	0.033
	(0.016)	(0.022)	(0.016)	(0.014)	(0.018)	(0.015)	(0.025)	(0.028)	(0.024)
Number of observations	470	302	340	473	304	343	470	304	340
Number of groups	4,317	2,802	3,121	4,445	2,876	3,220	4,374	2,842	3,171
R^2 (within)	.650	.662	.675	.802	.812	.814	.821	.833	.829
State-level time-varying covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Institution-level time-varying covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Institution fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note. State clustered robust standard errors in parentheses. ECH = excess credit hour.
$^{\dagger}p < .10$. $^{*}p < .05$. $^{**}p < .01$. $^{***}p < .001$.

TABLE 7

ECH and Debt by Debt Level

	10th percentile (logged)			25th percentile (logged)			75th percentile (logged)			90th percentile (logged)		
	National	Region	Border	National	Region	Border	National	Region	Border	National	Region	Border
ECH adoption (4-year lag)	0.045	0.071	0.042	0.063**	0.071*	0.056*	0.053**	0.066**	0.049**	0.053**	0.063**	0.049**
	(0.053)	(0.051)	(0.050)	(0.023)	(0.027)	(0.022)	(0.015)	(0.019)	(0.014)	(0.015)	(0.017)	(0.014)
Number of groups	473	304	343	473	304	343	473	304	343	473	304	343
Number of observations	4,456	2,882	3,231	4,456	2,882	3,231	4,456	2,882	3,231	4,456	2,882	3,231
R^2 (within)	.782	.777	.779	.760	.774	.761	.859	.849	.861	.815	.800	.820
State-level time-varying covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Institution-level time-varying covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Institution fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note. ECH = excess credit hour.

TABLE 8

ECH and Debt by Program and Institutional Type

	Median student debt (logged)			Low-income student (logged)			Middle-income student (logged)			High-income student (logged)		
	National	Region	Border	National	Region	Border	National	Region	Border	National	Region	Border
ECH adoption	0.105***	0.107***	0.095***	0.118***	0.124***	0.106***	0.084***	0.079***	0.076***	0.145***	0.123***	0.138***
(4-year lag)	(0.016)	(0.023)	(0.021)	(0.017)	(0.028)	(0.021)	(0.015)	(0.020)	(0.020)	(0.020)	(0.025)	(0.028)
ECH × master	−0.017	−0.011	−0.016	−0.048**	−0.051**	−0.046**	−0.011	−0.007	−0.007	−0.083***	−0.063**	−0.083***
	(0.012)	(0.017)	(0.014)	(0.015)	(0.017)	(0.016)	(0.015)	(0.021)	(0.016)	(0.019)	(0.019)	(0.020)
ECH × doctoral	−0.080*	−0.078†	−0.076*	−0.108*	−0.115*	−0.100*	−0.059†	−0.057	−0.052	−0.150***	−0.129***	−0.149***
	(0.036)	(0.039)	(0.034)	(0.040)	(0.042)	(0.040)	(0.031)	(0.034)	(0.032)	(0.036)	(0.034)	(0.034)
Number of groups	470	301	340	470	301	340	470	301	340	470	301	340
Number of observations	4,432	2,858	3,207	4,432	2,858	3,207	4,432	2,858	3,207	4,432	2,858	3,207
R^2 (within)	.765	.765	.776	.644	.654	.664	.790	.797	.797	.814	.822	.821
State-level time-varying covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Institution-level time-varying covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Institution fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note. State clustered robust standard errors in parentheses. ECH = excess credit hour.
†$p < .10$. *$p < .05$. **$p < .01$. ***$p < .001$.

completion, each of these analyses includes both state- and institution-level time-varying covariates as well as institution and year fixed effects. First, Table 5 provides the impact of ECH adoption on the overall median student debt by various adoption lags. Consistent with the policy implementation effects—typically ECH policies do not immediately affect students who were already enrolled when the policy was enacted[12]—we see a consistent significant effect of ECH policies on median student debt beginning at 4 years post adoption. We estimate that 4 years post adoption, ECH adoption significantly increased median student debt. The estimated effect was robust across each of our three nonadopting control groups. Institutions within adopting states experienced an increase of from 5.7% (nonadopting border state controls) to 7.2% (nonadopting regional state controls) in median student debt 4 years after adoption. This significant effect continued 5 years post adoption with similar significance and magnitude levels.

If we focus on the marginal costs associated with ECH surcharges, the estimated point effects are plausible. Based on our calculations, the typical institution charges approximately US$300 per credit. Based on the latest complete wave of the nationally representative Beginning Postsecondary Students (BPS 04/09) Longitudinal Study, a typical undergraduate completed a degree with an excess of 30 credits. If 50% of those credits were subjected to the ECH surcharge (typically an additional 100%), it would generate a total of US$4,500 in additional tuition costs. If students only financed 50% of this additional cost through student loans, it would represent an increase of about 10% for many graduates with debt. Given our previous results that ECH policies do not significantly alter timely completion or degree production, it seems plausible that our results would have similar magnitudes.

Table 5 establishes the timing effects of ECH adoption on student debt. The remaining tables (Table 6 through Table 8) provide the impact of the 4-year lagged effect of ECH adoption on varying subgroups of students. Table 6 provides the point estimates of the policy adoption effect by student income levels. Overall, we found that the impact of ECH policy was concentrated within low- and middle-income students. Students within the high-income subgroup (nominal family incomes of greater than US$75,000) did not experience any significant increase in their median debt post adoption. Low-income students (nominal family incomes less than US$30,000) experienced between a 4.1% and 5.1% increase in their median student debt 4 years post adoption, whereas middle-income students (nominal family incomes of between US$30,000 and US$75,000) experienced the largest increase in their median debt of between 5.3% and 5.6%. Given the income thresholds for these groups, it is possible that access to Pell Grants mediated the increase in student debt slightly. The findings that high-income students are not as affected by ECH policies as more marginalized students is further supported within Table 7, which provides the postadoption response to ECH policy by student debt level. Post ECH adoption, we find no significant evidence that cumulative loan debt for the 10th percentile was affected. However, we find systematic increases in debt levels across each of three control groups at the 25th, 75th, and 90th percentiles of cumulative loan debt levels. For students borrowing the most (90th percentile), we see significant increases between 4.9% and 6.3%. At the 25th percentile of median student debt, ECH adoption appears to produce the largest percentage increase between 5.6% and 7.1%. The stability of our estimates across each control group engenders confidence in our results that the debt of low-income students appears to be most affected by ECH policy adoption.

Our study also examines the impact of ECH policies on student debt for first-generation, Pell Grant recipients, and noncompleters. These results can be found in the online appendix (available at http://epa.sagepub.com/supplemental) and support our prior findings suggesting that the most marginalized students experience the largest increases in student debt post ECH adoption. Specifically, Pell Grant recipients experienced a 6.0% increase, first-generation college students a 7.2% increase, and noncompleters a 7.7% increase in their median student debt levels. Each of these is larger than the 5.7% increase in overall median student debt.

Impact of ECH Policies on Student Debt by Institution Type

Despite finding consistent evidence that ECH adoption increases student debt, particularly for those who borrow the most and come from low- or middle-income backgrounds, there exists the potential for different effects by institutional types. Table 8 provides an extension of our analysis that interacts institutional types (doctoral, master's, or bachelor's[13]) with ECH adoption. Overall, it appears that students enrolled in bachelor's degree–granting institutions who were under an ECH policy were most affected by the policy. Specifically, the median student debt for students enrolled at bachelor's institutions showed increases ranging between 9.5% and 10.7%, whereas the median student debt at master's degree–granting institutions experienced an increase of 7.9% to 9.6%,[14] and doctoral degree–granting institutions experienced only a 2% to 3% increase in student debt post ECH adoption.

Across income levels, attending a doctoral institution appears to significantly mediate the effects of ECH policies on increases in median debt. This is particularly true for students possessing high nominal family incomes, as their increase in debt is near zero compared with high-income students attending bachelor's-only–granting (increase in median student debt of 12%–15%) and master's degree–granting (increase in median student debt of 6%) institutions. Within doctoral institutions, students from middle nominal family incomes still experienced the largest impact in student debt post ECH adoption (2.2% compared with less than 1% for students from low and high nominal family incomes). At bachelor's institutions, students from high nominal family incomes experienced the largest increase in median debt post ECH adoption at 13.8% compared with students from middle nominal family incomes (7.6%) and students from low nominal family incomes (10.6%). These results indicate varying policy impacts on debt, based on the institution type attended. The access to additional financial resources and larger institutional grants at doctoral institutions (Doyle, 2010a) may explain the overall reduced effects on debt.

ECH Adoption and Time Trends

The difficulty in any quasi-experimental design lies in identifying the counterfactual in the absence of policy adoption. The use of a GDD design allows this study to approximate the impact of ECH adoption on institutions in adopting states, using nonadopting institutions and variations in adoption year as controls. This approach produces estimates of what *could* have occurred within our outcomes if the state had not adopted an ECH policy. This counterfactual approach assumes that treatment and control units follow similar (or parallel) prepolicy patterns, and that the resulting variations in the outcome can be attributed to policy adoption. A chief concern around any DiD approach is distinguishing the policy impact from the time impact. To illustrate the lack of preadoption significance and to highlight potential time trends in our primary outcomes, the following uses leading and lagging adoption terms. We specifically focused on the year prior to adoption, and post adoption with unaffected students, to see whether institutions within adopting states experienced significant changes in their primary outcomes.

Figure 2 illustrates the pre- and postadoption trend for median student debt. As seen in Figure 2, there were no significant changes prior to the adoption ECH. This nonsignificance continued until Year 4 (the first year of full impact on students). We see a marginally significant effect of ECH adoption on student debt 3 years post adoption, which makes sense as about 75% of students would fall under the surcharge policy.[15] Four years after adopting the ECH, institutions experienced their first statistically significant increase (7%) in median student debt. This significant impact persisted into Years 5 and 6 post adoption. This display provides evidence that the prior-adoption parallel trend assumptions have been met, and that the estimates produced in Tables 3 and 4 are robust and attributable to the effect of ECH policy adoption.

Discussion

The adoption of state postsecondary policies aimed at increasing student completion and success is not a new topic within the empirical

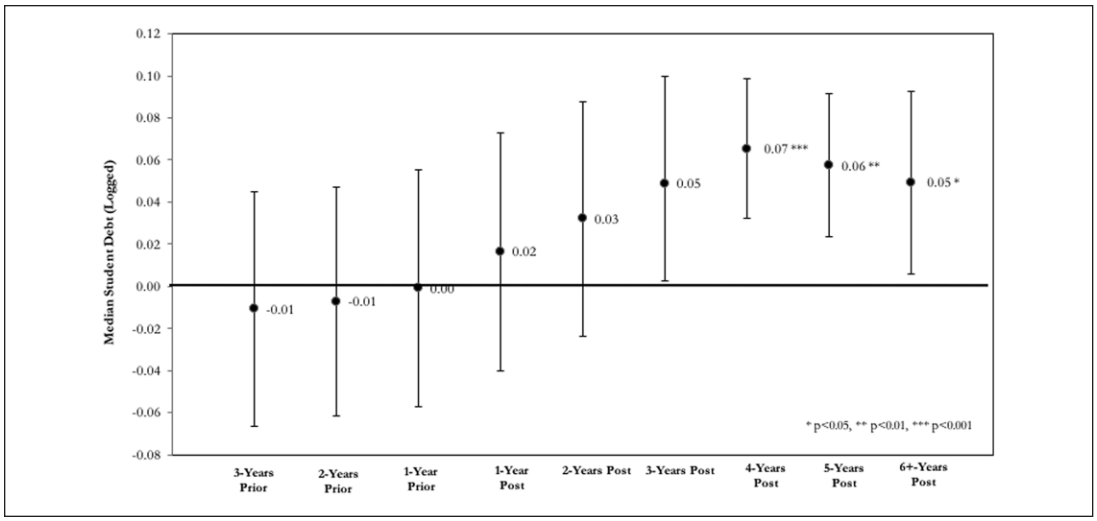

FIGURE 2. *Pre- and postadoption time trend on median student debt.*

literature. However, few have examined the impact of tuition surcharge policies on student completion and debt outcomes. The use of ECH policies is becoming an increasingly popular policy strategy for state policymakers and institutional leaders seeking to create an environment that incentivizes efficient student completion while minimizing state subsidies (Office of Program Policy Analysis and Government Accountability, 2006). State policies focusing on increasing student performance while reducing state subsidies have received considerably less attention than state-adopted merit aid policies (Doyle, 2010b; S. Dynarski, 2002; Monks, 2009) or state performance funding (Hillman, Tandberg, & Fryar, 2015; Hillman et al., 2014). Results from this study begin to answer the broader question of whether tuition-based surcharges directly affect student course-taking behaviors, much as the impact of changes in tuition levels has been shown, or if there are unintended consequences affecting student debt. Using institutional data to leverage a natural policy adoption experiment, our study makes additional contributions regarding the use of a GDDs approach.

Departing from prior findings on the effects of tuition increases on course-taking behaviors and time-to-degree (Bruckmeier et al., 2015; Garibaldi et al., 2012), our results yield no systematic evidence that excess tuition surcharges affect student course-taking behaviors or provide incentive for degree completion. This is despite, on average, the tuition surcharges of ECH policies being as much as 30 times bigger than the US$100 tuition increases reported in prior work to facilitate student completion and affect student course-taking behaviors (Hemelt & Marcotte, 2011). We postulate that this is due to the lack of information students have about ECH policies. Compared with tuition increases, which are publicly debated and widely known to students (Grodsky & Jones, 2007), ECH policies are often less familiar. The tuition surcharge only affects students once they cross the predetermined threshold, and at that point, it is too late for significant modifications to course plans. In addition, to elicit reduction in time-to-degree and streamline course-taking behaviors, students must engage in course planning early on in their program of study (Adelman, 2006). The potential lack of information about ECH policies and the delayed nature of their impact do not facilitate incentives for students early in their program of study.

In summary, we did not find systematic evidence of changes in degree production or the 4-year graduation rate, we did note differing effects for African American and Hispanic student completion rates post implementation. Although initially surprising, these results appear to be supported by the prior literature. Specifically, Gross, Torres, and Zerquera (2013) found that financial aid subsidies appear to significantly increase Hispanic/Latino student persistence and completion more than other racial/ethnic

subgroups. Kim, DesJardins, and McCall (2009) also demonstrated variations in student responses to fiscal policies by race/ethnicity. Because our results demonstrate a positive effect of ECH on the 6-year graduation rate of Hispanic/Latino students, we find them to be complementary to the existing literature.

Our results highlighted the potential of ECH policies to significantly increase student debt. We feel that our results, when combined with the lack of impact on student degree completion and graduation, further strengthen our conclusion that either ECH may most likely be unknown to incoming students until they reach (or increase in proximity to) the excess credit threshold. Scholars have long noted the presence of information asymmetry surrounding state-adopted policies (Lin, Cai, & Li, 1998). Pusser and Doane (2001) explicitly stated that students are particularly vulnerable to information asymmetries when policies do not immediately affect their day-to-day activities. Qualitative studies on students' knowledge and perception of ECH policies upon entering college would be a valuable addition to the literature.

It is possible that the surcharges associated with these policies are not large enough to modify student behaviors, prior research has produced findings that demonstrate large student responses to nominal changes in prices (Bryan & Whipple, 1995; Savoca, 1990)—leading us to believe that the surcharges are large enough to potentially induce behavioral changes. Our heterogeneous effects articulate the lack of significant effect on the median student debt of low (or zero-amount) student loan borrowers as well as high-income students. The estimates that unintended consequences of these policies increase debt for those already borrowing significant amounts of student loans as well as those from low- and middle-income families provide significant concern. As these populations of students traditionally have lower levels of educational capital (Bailey & Dynarski, 2011; Bastedo & Jaquette, 2011), it is not surprising that their responses to nuances in tuition surcharge policies would be more affected.

What is not clear within our analysis is the role of loans within this process. Our findings could potentially demonstrate the perverse impact of ECH policies on low- and middle-income students; it is also possible that students not utilizing loans (or limited loans) may feel the immediate impact of these surcharges whereas borrowing students have a delayed impact. In addition, there may be simultaneity between the surcharges related to ECH and the exhaustion of federal loan eligibility by students. Our current data would not untangle the impact of loans; however, this is an area ripe for future research.

Our findings also provide interesting insights into the potential differential effects of tuition surcharges on students by institutional type. The results appear to suggest that doctoral institutions mitigate the potential effects of ECH policies of median student debt—this is particularly true for low-income and affluent students. Desrochers and Hurlburt (2016) reported that public 4-year research universities—analogous to doctoral institutions within our sample—spend more on scholarships and institutional aid and have significantly more operating revenues than public 4-year master's and bachelor's institutions. It stands to reason that this increase in institutional aid would reduce student debt levels. In addition, the preponderance of the no-loan programs—aimed at supporting low-income students—are situated at doctoral institutions (Hillman, 2013), supporting our results. A recent Brookings Institution report (Looney & Yannelis, 2015) found that selective institutions, and institutions with a large proportion of graduate degree programs, experience the lowest increase in the number of students engaging in student loan borrowing. In addition, the profiles of student loan borrowers indicate that they are typically more affluent at the most selective institutions.

Given the popularity of postsecondary policies aimed at increasing student completion and the discourse on college affordability, continued work on the effect of ECH and similar policies provides opportunities for additional contributions. Limitations aside, this study provides additional insights into the ineffectiveness and the cost-shifting mechanism associated with the implementation of ECH policies. The insights from this study extend beyond ECH policies and highlight the broader effects of tuition-based surcharges on student course-taking behaviors and completion. This study contributes to the empirical literature on state policies

aimed at increasing student completion and minimizing student debts. It is our hope that the findings of our study shed light for state policymakers considering future ECH policy adoption, while encouraging state policymakers who have already adopted ECH policies to restructure, or outright reconsider, the ways in which current policies penalize students for exceeding required credit hours.

Acknowledgment

We thank University of Florida doctoral students, Joseph Uong and Jiayao Wu, for their assistance in collecting state policy documents on excess credit hour policies. Additionally, we are extremely grateful to the editor and anonymous reviewers for their thoughtful recommendations.

Declaration of Conflicting Interests

The author(s) declared no potential conflicts of interest with respect to the research, authorship, and/or publication of this article.

Funding

The author(s) received no financial support for the research, authorship, and/or publication of this article.

Notes

1. This number combines graduates of public and private nonprofit colleges, but National Postsecondary Student Aid Survey (NPSAS) data show little difference between public and private college graduates in previous years.

2. Typically, a bachelor's degree requires a student to complete at least 120 credits.

3. The largest grant-in-aid would be the federally funded Pell Grant program. Research on the effects of the Pell Grant program has focused predominately on the effects of the awards on postsecondary enrollment. Studies that have examined the impact of Pell Grant participation on graduation rates have produced inclusive results.

4. Given that a number of community colleges have begun to offer baccalaureate degrees, we limited our sample to institutions identified as public 4-year institutions within the Integrated Postsecondary Education Data System (IPEDS) data and those whose bachelor's or higher degrees awarded were greater than or equal to 51% of total degrees.

5. Based on the authors' review of legislative reports and policy documents, institutions do not possess the ability to establish their own thresholds and/or modify the surcharge for those exceeding the established threshold. Most adopting states distinguish between public 2- and 4-year institutions, and therefore, we limited our sample to public 4-year institutions.

6. The average number of months or semesters needed to complete a bachelor's degree is not systematically captured within National Center for Education Statistics (NCES) data sets.

7. The percentage of Pell Grant recipients was not available for the entire time period of our panel, but in years for which data on both the percentage of federal grant and Pell Grant recipients were available, the correlation was above .95.

8. We utilize the Bureau of Economic Analysis (BEA) regional categorization. The regions excluded from our sample, those not possessing an adopting state within our analytical time period, are (a) Mid East (DE, DC, MD, NJ, NY, PA), (b) Plains (IA, KS, MN, MO, NE, ND, SD), and (c) Far West (AK, CA, HI, NV, OR, WA).

9. Jaquette and Para (2016) called this limitation "parent–child reporting" that "occurs when an institution reports some IPEDS data at a disaggregated level (e.g., campus-level) and other IPEDS data at a more aggregated level (e.g., Title IV institution-level)" (p. 632).

10. We borrow from Hillman, Tandberg, and Gross (2014) when discussing robustness across multiple comparison groups: (a) one comparison group yields a statistically significant result and equals "limited" evidence, (b) two groups equal stronger evidence, and (c) all three comparison groups yield statistically significant results, which is the strongest evidence possible. Alternatively, if our estimates have no significant patterns across all three comparison groups, then we conclude that the policy had null effects on that particular outcome.

11. We are limited by IPEDS data and, therefore, are unable to estimate completion effects by socioeconomic status.

12. In reviewing each of the state-adopted excess credit hour (ECH) policies, state legislation did not affect students currently enrolled within a postsecondary institution. However, incoming students and students entering in-state institutions post legislation were subjected to ECH surcharges.

13. Institutions granting only bachelor's degrees serve as our reference group. This was done purposefully to examine increase in prestige on the policy effect.

14. Our significance tests found that master's degree granting institutions did not act significantly different on median student debt from bachelor's degree granting institutions; however, we included their coefficient in interpreting our interaction term.

15. Noncompleting students are also considered within this 3-year lagged impact and could be driving the statistically significant results.

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Manuscript received November 29, 2016
First revision received March 2, 2017
Second revision received April 12, 2017
Accepted April 21, 2017