

# A New Look at Coordination of Financial Aid at America's Elite Colleges

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## **Abstract**

Since 2002, members of a group of elite American colleges and universities have used a limited exemption to antitrust laws to coordinate financial aid packages and policies. The colleges claim that coordination reduces the need to compete on scholarships for students who do not need them, freeing up resources for students who do. Now, other colleges are asking Congress for inclusion in this exemption. This paper uses a differences-in-differences model to compare student borrowing at schools that used the exemption and similar schools that did not, before and after the period of coordination. The results suggest a decrease in student borrowing at schools that coordinated relative to schools that did not. This decrease was large, generated benefits primarily enjoyed by low-income students, and occurred in the long-run.

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# 1 Introduction

Since 2002, members of a group of elite American colleges and universities, called the 568 Presidents Group, have met annually to set and coordinate financial aid packages and policies. Using a limited exemption to antitrust laws, the colleges claim that coordination allows them to make college more affordable and more accessible. To my knowledge, no researcher has conducted a long-run analysis of the causal impact of these meetings on various affordability measures.

The exemption defines several ways in which need-blind colleges, which admit students without regard to financial need, may coordinate award policies and exchange information about admitted students. This paper addresses the potential impacts of a provision which allows colleges to use common principles of analysis when determining need. This provision allows college to jointly define an aid methodology that serves needier students on average, relative to the methodology in place. The 568 Presidents Group used the exemption to create a set of common standards of need analysis called the Consensus Approach.<sup>1</sup>

In a 2017 report, the National Association of Independent Colleges and Universities, asked Congress to expand the antitrust exemption to all private nonprofit colleges. The NAICU, which lobbies on behalf of 1,000 institutions, claims that allowing colleges to talk to each other about financial aid policies could increase low-income student enrollment, increase college affordability, and decrease student borrowing.<sup>2</sup> If the NAICU is right, then an antitrust exemption may be an attractive public policy tool. An exemption could be a zero-cost option to help address rising tuition levels and a student loan crisis. It is conceivable that, as the NAICU suggests, regular exchange of information would allow schools to develop financial aid policies that prioritize low-income students relative to the standard policies in place. The Consensus Approach may achieve this result by modifying how certain household income sources and assets are used to determine expected family contributions.

At first look, this scenario is not consistent with the common intuition of antitrust economics, which holds that price coordination allows producers (institutions) to increase prices (tuition) and reduce consumer (student) welfare. At the same time, the common intuition of antitrust analysis is difficult to apply to higher education. The colleges and universities

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1. Another provision allows colleges to engage in a coordinated reduction or elimination of merit aid programs. Theoretically, the 568 Presidents Group can use coordination to serve needier students by shifting merit aid resources into need-based aid programs. According to the U.S. Government Accountability Office (GAO), this provision has not been adopted by member schools. *See* U.S. Government Accountability Office, “School’s Use of the Antitrust Exemption Has Not Significantly Affected College Affordability or Likelihood of Student Enrollment to Date,” no. GAO-06-963 (September 2006): 11, <https://www.gao.gov/products/GAO-06-963>.

2. National Association of Independent Colleges and Universities, *NAICU - Anti-Trust*, accessed March 30, 2019, <https://www.naicu.edu/policy-advocacy/issue-brief-index/student-aid/anti-trust>.

requesting an exemption are nonprofits with an objective to promote diversity and equitable access to higher education. Purely profit-seeking motives may not be able to explain behavior in this market.

Other scholars argue that price coordination in higher education may in fact be perfectly consistent with standard antitrust analysis. Salop and White (1991) suggest that coordination may generate positive externalities by producing a more diverse class, though they note that colleges would have to demonstrate that the quality of education depends on the mix of students at the college. They suggest their argument is consistent with antitrust analysis in that it “directly links the price fixing to improved quantity and quality of the educational product sold by colleges.”<sup>3</sup> Some research has identified a trade off between certain types of aid programs and low-income enrollment. For example, Ehrenberg et al. (2005)<sup>4</sup> found that an increase in institutionally funded merit aid is associated with a decrease in enrollment of low-income students.

In any case, the public policy debate should not rely on contested theory and intuition. This situation calls for a quasi-experimental study. It is therefore appropriate to examine what happened in the past when American colleges and universities talked to each other about tuition and financial aid.

There are two distinct periods of price coordination in higher education in the United States. The first period began in the 1950s when members of the Ivy League agreed to end athletic scholarships. The schools, calling themselves the Overlap Group, continued to meet annually, reaching agreements on family contributions such that students admitted to and seeking financial aid at more than one member school would pay the same price at any other member school.<sup>5</sup> Over time, the Overlap Group grew to include 23 member schools, comprised of the Ivy League schools, MIT, and 14 other highly-selective schools including Tufts.<sup>6</sup>

The Overlap Group defined its process and purpose in the Manual of the Council of Ivy League Presidents. First, colleges agreed to award aid on the basis of demonstrated financial need, not academic merit or athletic ability. Second, colleges agreed to develop and follow

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3. Steven C. Salop and Lawrence J. White, “Policy Watch: Antitrust Goes to College,” *The Journal of Economic Perspectives* 5, no. 3 (1991): 199, ISSN: 0895-3309, <https://www.jstor.org/stable/1942804>.

4. Ronald G. Ehrenberg, Liang Zhang, and Jared M. Levin, “Crafting a Class: The Trade-Off between Merit Scholarships and Enrolling Lower-Income Students,” *Review of Higher Education* 29, no. 2 (2006): 195–211, ISSN: 0162-5748.

5. Dennis W. Carlton, Gustavo E. Bamberger, and Roy J. Epstein, “Antitrust and Higher Education: Was There a Conspiracy to Restrict Financial Aid?,” *The RAND Journal of Economics* 26, no. 1 (1995): 132-133, ISSN: 0741-6261, doi:10.2307/2556039, <https://www.jstor.org/stable/2556039>.

6. Anthony Depalma, “Ivy Universities Deny Price-Fixing But Agree to Avoid It in the Future,” *The New York Times*, May 1991, chap. U.S. ISSN: 0362-4331, <https://www.nytimes.com/1991/05/23/us/ivy-universities-deny-price-fixing-but-agree-to-avoid-it-in-the-future.html>.

a common policy for calculating expected family contributions. Third, colleges agreed to adjust individual aid packages on a case-by-case basis to reduce any remaining variation in financial aid awards. The colleges said that these agreements were meant “to provide financial assistance to students who without such aid would be unable to attend that institution.”<sup>7</sup> Presumably, the savings from eliminating merit and athletic scholarships would be shifted entirely to a need-based aid program serving relatively less wealthy students.

On behalf of MIT, Carlton et al. (1995)<sup>8</sup> compared average price per student in the Overlap Group and a control group, finding no significant difference between the two groups. While the study does not fully answer whether price coordination helps needy students, it does suggest that college revenues do not increase or decrease as a result of it and suggests that antitrust regulators should take that into account.

In the early 1990s, the Department of Justice sued the Ivies and MIT alleging that the agreements violated the Sherman Antitrust Act, causing a temporary cessation of the meetings.<sup>9</sup> Hoxby (2000) used a difference-in-differences approach to examine the effect of the temporary cessation of meetings. She found that ending the meetings led to “aid that was less progressive with respect to parents’ income and slightly more sensitive to merit.”<sup>10</sup> Her paper therefore provides some evidence to support the stated purpose of the meetings. My paper applies a similar methodology in order to assess the effects of the 568 Presidents Group, which designed and implemented its own system of coordination after the period considered by Hoxby.

A District Court ruled in favor of the Justice Department. On appeal, the Third Circuit Court of Appeals reversed the judgement and returned the case to the District Court in order to consider the non-economic social benefits that could arise from coordination.<sup>11</sup> In 1992, in recognition of the possible benefits of price coordination in higher education, Congress passed a temporary exemption.<sup>12</sup> The exemption was limited by design and applied only to schools that admitted students on a need-blind basis. The need-blind requirement prevented some former members of the Overlap Group from using the exemption, such as Tufts which is not need-blind. In addition to allowing colleges to develop a set of common standards of

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7. *United States v. Brown University*, September 1992, 288.

8. Carlton, Bamberger, and Epstein, “Antitrust and Higher Education.”

9. Depalma, “Ivy Universities Deny Price-Fixing But Agree to Avoid It in the Future.”

10. Caroline M Hoxby, *Benevolent Colluders? The Effects of Antitrust Action on College Financial Aid and Tuition*, Working Paper 7754 (National Bureau of Economic Research, June 2000), 41, doi:10.3386/w7754, <http://www.nber.org/papers/w7754>.

11. U.S. Government Accountability Office, “School’s Use of the Antitrust Exemption Has Not Significantly Affected College Affordability or Likelihood of Student Enrollment to Date,” 7.

12. U.S. House of Representatives Committee on the Judiciary, *Need-Based Educational Aid Act of 2015*, technical report 114-224 (114th Congress, July 2015), <https://www.congress.gov/congressional-report/114th-congress/house-report/224/1>.

need analysis, the exemption explicitly permitted agreements to award aid only on the basis of need. Since its initial passage, the exemption has been reauthorized and revised in minor ways.

In the 2002-2003 academic year, 28 schools, some of which were part of the Overlap Group, started the 568 Presidents Group and used the exemption to create the Consensus Approach.<sup>13</sup> The formation of the this group marks the beginning of the second era of price coordination and is the focus of this paper. The Consensus Approach modified the College Board’s Institutional Methodology, a common need analysis methodology used to calculate family contributions. Like the Overlap Group, the 568 Presidents Group stated that their methodology would increase college affordability, relative to the methodology in place, by more effectively targeting need-based aid.<sup>14</sup> For example, the Institutional Methodology uses the full value of a family’s home in determining assets available to pay for college, while the Consensus Approach places an income-based cap on the amount of home equity that can be considered.<sup>15</sup> This modification is amongst several that may be used by the 568 Presidents Group to shift scarce financial aid resources to students found to be relatively more needy.

In 2006, the U.S. Government Accountability Office studied the Consensus Approach using a difference-in-differences approach and found no significant impact on affordability. The study was imperfect, as the GAO was only able to perform its analysis on a single year of implementation. It suggested that the “effects of using the Consensus Approach could be gradual, rather than immediate, and therefore may not be captured until later years.”<sup>16</sup> The report found that while some students received more need-based aid than before, the change was offset by an overall decrease in merit aid. Low-income students were found to receive greater amounts of total aid, with the increase likely in the form of loans that students would have to pay back. This result led the GAO to suggest that coordination may have induced more student borrowing due to aid given in the form of loans.<sup>17</sup> The GAO did not explicitly measure changes in student borrowing, which my paper does measure.

Karikari and Dezhbakhsh (2019) adapted the GAO’s empirical model and included public universities in their control group. They found that the likelihood of enrollment of low-income students increased at colleges using the Consensus Approach compared to those that did not.<sup>18</sup> Like the GAO, the paper also relied on a single year of post-coordination data,

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13. U.S. Government Accountability Office, “School’s Use of the Antitrust Exemption Has Not Significantly Affected College Affordability or Likelihood of Student Enrollment to Date,” 4.

14. *Ibid.*, 12.

15. *Ibid.*, 13.

16. *Ibid.*, 6.

17. *Ibid.*, 5.

18. John A. Karikari and Hashem Dezhbakhsh, “The United States’ Higher Education Antitrust Exemption and College Enrollment,” *Applied Economics* 51, no. 10 (2019): 19, doi:10.1080/00036846.2018.1524978.

rendering it unable to produce any results on the long-run impacts of coordination.

The research on the impacts of the Consensus Approach is scarce and inconclusive, lacking any analysis of the potential long-run impact. Given the recent proposal to expand the exemption, the Consensus Approach is a clear candidate for a research topic examining how coordination in higher education may affect college affordability.

Recognizing the need for a long-run model, I designed an empirical model that measures changes in student borrowing and college affordability from 2000 to 2015. The ideal study would measure changes in institutional grant aid at the student-level with data on household income and assets, segmented by whether the aid was based on merit or need, at colleges that used the Consensus Approach and similar colleges that did not. Unfortunately, public data on institutional grant aid is not available at the student-level and is not broken down by student merit status or need. Instead, I used student borrowing data broken down by income bracket to identify a shift in resources at coordinating schools towards low-income students, who ended up borrowing less in the post-coordination period. I then test several variables measuring institutional revenue and financial aid to support the hypothesis that colleges used the Consensus Approach to shift scarce aid resources to the neediest students.

The rest of the paper is organized as follows. In Section 2, I explain how I constructed the dataset. Section 3 explains the empirical model used in this paper. Section 4 presents the results of the empirical model. Section 5 concludes.

## 2 Data

This paper relies on institution-level data from the 1999-2000 academic year through 2014-2015. Data on certain college characteristics is drawn from the Delta Cost Project, maintained by the National Center for Education Statistics as part of the Integrated Postsecondary Education Data System. The U.S. Department of Education's College Scorecard provides data on student borrowing and other measurements of college affordability. Information regarding the coordination practices of specific institutions is drawn from the 2006 GAO study and the the 568 Presidents Group website, which publishes a regularly updated list of member institutions.

I converted all dollar figures to 2015 dollars and log-transformed my dependent variables and several independent variables in order to reduce the effect of outliers and to allow for a more intuitive and percentage-based interpretation of the results.

This study uses a difference-in-differences approach which necessitates the selection of a coordination group comprised of schools that used the Consensus Approach and a control group comprised of similar schools that did not. In the past, other papers have relied on



survey data in order to define groups. Hoxby (2000), which examined the effect of antitrust action, assigned defendants in the lawsuit to the coordination group.<sup>19</sup> GAO (2006) surveyed schools in 2006, asking whether or not they participated in coordination activities.<sup>20</sup> The coordination assignments from previous studies are not sufficient for my study of the long-run effects. I needed to identify schools that engaged in coordination on a continuing basis.

Table 1: Sample Schools

<b>Coordination</b>	<b>No Coordination</b>
Boston College	Brandeis University
Claremont McKenna College	Bryn Mawr College
Davidson College	New York University
Duke University	Princeton University
Georgetown University	Tulane University
Northwestern University	University of Rochester
Pomona College	Wash. Univ. in St. Louis
St. John’s College	
Swarthmore College	
University of Notre Dame	
Vanderbilt University	

Table 1 displays the schools assigned to the coordination and control groups in this paper. I assigned schools to the coordination group in two steps. First, I assembled a list comprised of schools that were consistent and continuing members of the 568 Presidents Group. From that list, I removed schools that were members of the original Overlap Group, having coordinated pre-exemption. The coordination group is comprised of the remaining schools. To identify consistent and continuing membership, I accessed archived versions of the 568 Presidents Group website, which are available annually dating back to 2005. Certain schools, like Brown, were only occasional members of the 568 Presidents Group, dropping in and out over the course of several years; these schools were omitted from the coordination group, because their consistent and continued engagement in price coordination could not be clearly determined. Other schools, like the Massachusetts Institute of Technology, were defendants in the original antitrust action and coordinated pre-exemption. If the need-analysis methodology of the Consensus Approach is similar to that of the Overlap Group’s

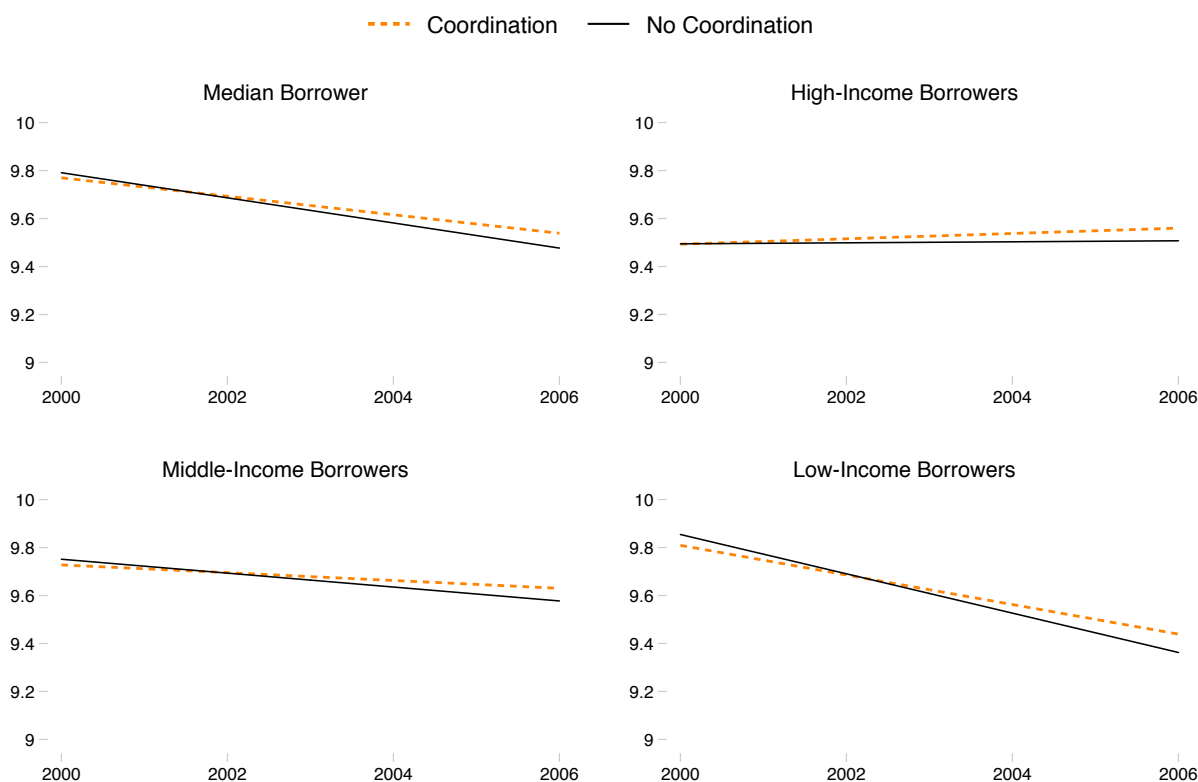
19. Hoxby, *Benevolent Colluders?*, 46.

20. U.S. Government Accountability Office, “School’s Use of the Antitrust Exemption Has Not Significantly Affected College Affordability or Likelihood of Student Enrollment to Date,” 2.

methodology, then schools like MIT may have internalized the benefits of coordination pre-exemption. MIT may have reasonably continued to use the same need-analysis methodology during the Overlap Group-era, through the antitrust action period, and into the period in which the 568 Presidents Group was formed to create the Consensus Approach. For that reason, it's difficult to assign schools like MIT to the coordination group, and I omit them.<sup>21</sup>

Schools assigned to the control group needed to meet the parallel pre-coordination trends requirement to conduct a robust difference-in-differences analysis. I used the same group of control schools used by the 2006 GAO Study. This specification came with several advantages.

Figure 1: Pre-Coordination Trends in Median Debt at Separation



Note: Figures are displayed in log of dollars.

First, I was able to avoid handpicking the schools which could create the appearance of

21. Bryn Mawr College and Princeton University were in the Overlap Group, but I do not omit them from my control group. Though Bryn Mawr and Princeton may have implemented a common need-analysis methodology in the past, they necessarily have not used the Consensus Approach because they are not members of the 568 Presidents Group. The GAO similarly included these schools in their control group. Given that they must have stopped using the Overlap Group methodology and transitioned to a non-Consensus Approach methodology, I do not omit them from the control group. In Section 4 on page 17, I note the results of estimations that omit Princeton and Bryn Mawr.

impropriety. Second, my coordination specification somewhat overlaps with the 2006 GAO paper's coordination specification. Given this overlap, I assumed that the GAO's efforts to build a comparable control group may extend to my specification. I tested this assumption and compared summary statistics on several student profile measurements.

My coordination specification and the GAO Control group matched parallel pre-coordination trends in all dependent variables examined in this paper.<sup>22</sup> Figure 1 plots the trends in median debt at separation by type of borrower in the pre-coordination period as predicted by the model in Column 4 in Section 4. It shows that the pre-coordination trends are nearly parallel and level across borrowers in the coordination and control groups.

In addition, I found several characteristics of the GAO control specification to be sufficiently similar to those of my coordination group in the pre-coordination period, such as investment revenues per student and share of students receiving aid. For other variables, such as instructional and institutional spending per student, a test showed that the two groups spent differently on average; however, the trend in spending over time is the same at coordination and control schools. Using institution-specific fixed effects should mitigate concerns about the differences between the means. Given these results, I am confident that this control specification will demonstrate a relevant counterfactual for how schools that coordinated would have behaved in the absence of the antitrust exemption.

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22. See coefficient on *Time\*Coordination* in Column 4 in the regression tables, insignificant in all tables except in Table 6a measuring median debt at separation for low-income borrowers. The coefficient is significant at the 10% level. The included time-trend variables should control for this concern.

Table 2: Summary Statistics in the Pre-Coordination Period (2000-2006)

	Coordination	No Coordination	Difference
	mean	mean	p
Median Debt at Separation	16,171	16,499	0.69
Median Debt (High Income)	14,290	14,392	0.89
Median Debt (Middle Income)	16,821	17,142	0.70
Median Debt (Low Income)	16,177	16,259	0.93
Net Tuition Revenue per Student	23,292	20,754	0.04
Pell Grant Outlays per Student	303	334	0.33
Institutional Grants per Student	10,533	12,299	0.01
Investment Revenues per Student	32,981	44,683	0.47
Net Tuition Revenue per Student	23,292	20,754	0.04
Share of Students Receiving Aid	12%	11%	0.22
Instructional Spending per Student	26,216	33,982	0.01
Inst. Support Spending per Student	11,611	8,492	0.00
Share of Revenue from Government	16%	30%	0.00
Full-Time Enrollment	7,305	10,574	0.03
Observations	77	49	126

Table 2 is a table of summary statistics that includes the dependent and explanatory variables (or their relevant transformations) examined in this paper. Readers should note that, relative to the typical American college or university, the institutions sampled are more selective, have higher investment revenues, and serve wealthier student bodies on average. This relative difference exists because the exemption only applies to institutions that admit students on a need-blind basis, which tend to be wealthy and highly selective.

My analysis is unique because it is the first study of the exemption to specifically examine the effect on student loans, a question posed but unanswered in the GAO paper. Because of my unique post specification described in Section 3, my study includes seven years of data pre-coordination and nine years of data post-coordination, which may address the concerns of previous papers which suggested that the effects of the exemption could be gradual and captured in later years.

As a potential weakness, this paper deals with an imperfect understanding of how the effects of coordination are internalized by schools that do not formally participate. Given that the existence and design of the Consensus Approach is publicly known, it is possible that schools assigned to the control group can observe and adopt its principles without ever associating with the 568 Presidents Group. This scenario, although possible, may be a violation of the exemption and antitrust laws, mitigating some of the concern of this outcome. In any case, this factor would weaken the observed impact of coordination – not intensify it.

Skeptics should keep this point in mind as they examine the results.

Finally, the results in this paper should not be construed to be predictions of how all colleges and universities would behave in the aftermath of an antitrust exemption. This paper examines a single apparatus for coordination, the Consensus Approach, and a very specific subset of institutions. Schools outside of my coordination and control group specifications may behave differently.

### 3 Empirical Model

This study uses a difference-in-differences approach with year fixed effects interacted with a coordination dummy variable. Because I identified a time-trend in the data, the following difference-in-differences approach is the preferred model. In the Results section, this preferred model is estimated in Column 5.

$$Y_{it} = \beta_t(\textit{Coordination} \times \textit{Year})_{it} + \beta_x(X_{it}) + \delta_i + \lambda_t + \varepsilon_{it} \quad (1)$$

The index  $i$  designates the institution, and  $t$  designates the academic year.  $\mathbf{X}$  is a vector of explanatory variables.  $\mathbf{Y}$  is the outcome variable of interest.

$(\textit{Coordination} \times \textit{Year})$  defines a coordination-year interaction term and the independent variable of interest. The coordination component of the interaction term is a fixed effect that absorbs the coordination assignment. The year component can absorb the effect of being in the post-coordination period. For example, the coefficient on the term interacting Coordination and 2015 indicates the predicted difference in the outcome variable at a school that used coordination and a similar school that did not in 2015.

$\lambda_t$  is a year fixed effect not interacted with the coordination assignment. The coefficient predicts the linear intercept for each data year, relative to 2000, when the coordination variable equals zero. For example, the 2015 fixed effect indicates the difference between the outcome variable at a control school in 2015 and in 2000.

$\delta_i$  is an institution-specific fixed effect. This fixed effect controls for unobservable characteristics unique to each college that stay constant over time.

In all of my analyses, I use robust standard errors. I also ran the same estimations using bootstrapped standard errors and found no significant difference in my results.

Equation 1 is particularly appropriate because the effects of coordination may only be apparent in the long-run. The coordination-year interaction allows for a granular understanding of when the effects of coordination began to appear and how the returns to coordination changed over time. This technique is also useful under the assumption that colleges had to

overcome a learning curve to identify the most efficient and effective ways to coordinate, as the GAO paper suggests.<sup>23</sup>

First, this paper seeks to estimate the causal impact of coordination on various college affordability and financial aid outcomes. Accordingly, the Results section estimates Equation 1 using measurements of student borrowing, net tuition revenue, Pell Grant outlays, and institutional grant aid. The vector of explanatory variables includes measurements of an institution's investment and endowment revenues, net tuition revenue, number of students receiving financial aid, instructional expenditures, institutional support expenditures, and reliance on government funding.

I included Investment revenues in order to control for market movements and institutionally unique investment strategies that could affect the amount of institutional resources available for financial aid programs. Net tuition revenue per student, directly affecting institutional resources, is similarly necessary when examining financial aid outlays.<sup>24</sup> Controlling for the number of students receiving financial aid helps control for variation in student bodies between the schools. Variables for instructional and institutional support expenditures, being large components of university expenses, may also vary widely amongst the schools and can influence financial aid policies. I included reliance on government funding to control for possible trends in institutional revenues sourced from the government.

I also ran the model against each dependent variable using controls for a quadratic time-trend. The quadratic time-trend variables were neither significant on their own nor significant in a joint F-test. Therefore, I dropped them from my empirical model.

A clean analysis of college affordability under a new policy can be difficult. This is because the nature of data reporting from a four-year college is such that, in the immediate aftermath of policy implementation, college affordability variables that typically sample from a cohort of all current students will also include students who were admitted and offered aid packages prior to the implementation of the new policy. The main dependent variable of interest in this study mitigates that concern. The median debt variable measures the sum of all federal loans borrowed by the median borrower at separation from the institution. Given my sample institutions, where the average completion rate is 88 percent, this variable is a reasonable measurement of borrowing by the graduating class. Variables derivative of the debt variable, such as debt by income group, are also measured at the point of separation.

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23. U.S. Government Accountability Office, "School's Use of the Antitrust Exemption Has Not Significantly Affected College Affordability or Likelihood of Student Enrollment to Date," p. 11, ("With respect to the other three activities allowed under the exemption, the schools either chose to not engage in the activities or piloted them on a limited basis").

24. In Table 7a, net tuition revenue per student is necessarily dropped as an explanatory variable because it is the dependent variable.

The Consensus Approach was first used in the 2002-2003 academic year. The first students admitted and offered aid packages under the new policy would graduate and appear in the debt variable in 2007. As such, I specify 2007 as the post-year for all models that measure debt at separation. I use 2007 as the post-year for non-separation variables as well, given it could reasonably be considered the year in which the Consensus Approach was fully implemented (when nearly all enrolled students were admitted and offered aid under the new policy). In any case, the preferred model uses coordination-year interactions that do not require the specification of a post-year. The preferred model then is particularly relevant and useful for variables not collected at separation.

## 4 Results

Tables 3a - 9b present my regression results. All tables start with a basic regression specification which is then built up to the full model. The first column includes dummy variables for coordination schools and the post-coordination period, as well as their interaction, but no other controls. Column 2 adds various explanatory variables to control for certain school characteristics, revenues, and expenditures. Column 3 adds school fixed effects. Column 4 adds separate linear time trends for coordination and control schools for the pre- and post-coordination periods. Finally, Column 5 presents my preferred specification from Equation 1.

### 4.1 The Median Borrower Fared the Same or Better Under Coordination

Table 3a shows the results of all models against a measurement of the sum of all federal loan amounts borrowed over a student's college education. It is a median reported at separation from the institution. All models indicate that that the median borrower at a coordinating school fared as well as or better than a similar student at a control school. Over the period analyzed, the level of median debt at separation was decreasing in many of the pre-coordination years before starting to increase again and returning to the level present in 2000, the base year. This post-coordination trend of increased borrowing, however, was largely experienced equally by students at both coordinating and control schools, and the trend does not appear to be associated with the exemption.

Column 1 predicted a net decrease in borrowing at coordinating schools. The insignificant coefficient on *Post* indicates that borrowing did not observably change at control schools. The significant coefficient on *Post\*Coordination* suggests that borrowing at a coordinating

school was 19 percent lower than borrowing at control schools. The full effect on a student at a coordinating school is the difference between these coefficients, indicating a net decrease in borrowing at coordinating schools. These results are consistent with Column 2, which adds a vector of explanatory variables on college revenues and expenditures, and Column 3, which adds institution-specific fixed effects.

In Column 2 and Column 3, the coefficients on *Post\*Coordination* shows that the median borrower at a coordinating school graduated with roughly 18 to 19 percent less in debt than a borrower at a control school in the post-coordination period. Over the same period, the median borrower at a control school experienced little to no change in graduating debt, as noted by the coefficient on *Post*. The net effect is that student borrowing is largely unchanged at control schools and significantly lower at control schools. Because I consider control schools the counterfactual – showing how coordination schools would have behaved had they not colluded – the results suggest that the median borrower at a coordinating school was better off and graduated with up to 20 percent less in debt than they otherwise would have.

Results in Column 4 and Column 5 are consistent with Columns 1-3, providing further evidence that the median borrower at a coordinating school borrowed as much as or less than their counterpart at a control school. Figure 2a, which plots time trends estimated in Column 4, shows a steady decrease in borrowing in the pre-coordination period in both groups. In the post-coordination period, borrowing appears to start increasing in control schools with no discernible change at coordination schools.

Column 4 shows an insignificant coefficient on *Post\*Coordination*, which is a change in intercept – interpreted as the immediate change in student loan levels in the post-coordination period as predicted by the model. The insignificant result suggests there is little evidence that points to a short-run impact in this model as there was no immediate decrease in student loans at separation due to coordination, even after setting the post-coordination period to four years after the Consensus Approach was first adopted. *Time\*Post\*Coordination* measures a change in slope – the acceleration or deceleration in student borrowing due to coordination, relative to the rate of change in the counterfactual control schools. It's highly significant at the 5 percent level. The result, consistent with Figure 2a, suggests a large long-run impact on borrowing, which is predicted to have decreased by roughly 6 percent annually relative to control schools in the post-coordination period.

Column 5, which interacts year dummies with the coordination assignment, similarly suggests evidence for a long-run impact. Table 3b shows that the coefficients on these coordination-year interaction terms, which are relative to the base-year 2000, are not significant in any year but are negative and increasing in magnitude from 2009 through 2015, the



latter years of the post-coordination period. Figure 2b plots these coefficients. Because the coefficients are negative and not significant, the model suggests that the median borrower at a coordinating school borrowed about as much and possibly less than their counterpart at a control school. The coefficients in the post-coordination years (2007 – 2015) are jointly significant at the 5 percent level. The coefficients show a steady increase in magnitude that, given more data years, may become stronger.

The evidence for a long-run impact due to coordination from Column 4 and, in part, Column 5 is not unexpected. In part, this long-run result is due to the definition of the dependent variable, which measures student debt at separation. Given that coordination began in 2003, its impacts would not appear, at least in this dependent variable, until 2007 at the earliest, which is part of the logic for setting 2007 as the start of the post-coordination period.

In addition, this long-run result is consistent with GAO (2006),<sup>25</sup> Hoxby (2000),<sup>26</sup> and Karikari and Dezhbakhsh (2019),<sup>27</sup> which all opined that the impacts of coordination could exist in the long-run, beyond the scope of the data used in previous research. The long-run result is also consistent with the findings in the GAO report, which wrote that coordinating schools experimented with several methods of coordination post-exemption before settling on the most efficient and effective means of sharing information.<sup>28</sup> The magnitude of the coordination-year interactions increasing year-to-year is also indicative of what may be somewhat of a learning curve associated with price coordination. Perhaps as schools discovered the most effective and efficient means of coordination, they were also more effective in designing financial aid allocations that kept student loan levels under control.

The coefficients on the explanatory variables are also noteworthy. An increase in investment revenues was not particularly associated with a decrease in borrowing. An earlier version of this analysis that did not use logged variables found that student borrowing had a positive relationship with institutional support spending and a negative relationship with in-

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25. U.S. Government Accountability Office, “School’s Use of the Antitrust Exemption Has Not Significantly Affected College Affordability or Likelihood of Student Enrollment to Date,” p. 6, (“The effects of using the consensus approach could be gradual, rather than immediate, and therefore may not be captured until later years”).

26. Hoxby, *Benevolent Colluders?*, p. 41, (“Additional years of data may, as they become available, give us more evidence about whether need-based aid is efficiency-enhancing”).

27. Karikari and Dezhbakhsh, “The United States’ Higher Education Antitrust Exemption and College Enrollment,” p. 1079, (“The limited data we used could mask potential effects of the consensus approach since these effects could be gradual, rather than immediate, and therefore time is needed for their full effect to be manifested and captured”).

28. U.S. Government Accountability Office, “School’s Use of the Antitrust Exemption Has Not Significantly Affected College Affordability or Likelihood of Student Enrollment to Date,” p. 11, (“With respect to the other three activities allowed under the exemption, the schools either chose to not engage in the activities or piloted them on a limited basis”).

structional spending. Using logged variables removed any observable results, which suggests that the earlier results could have been driven by outliers.

Share of revenue from government sources appears to be a decent predictor of student borrowing. Column 5 predicts that a 1 percent increase in an institution's reliance on government revenue can increase student borrowing by nearly a tenth of a percentage point. My guess is that schools that rely on government revenues are therefore reliant on a revenue source that may not be stable year-to-year, and therefore require a larger contribution from students that leads to increased borrowing. The number of students receiving aid was a fairly weak predictor. It's possible that a metric with a more direct effect on student loan levels, such as average award size, would be a stronger predictor. I ran the Column 5 estimation after omitting the explanatory variables and found no significant difference in results.

## 4.2 Benefits Concentrated Amongst Poorest Students

Having determined that the median borrower may see benefits from coordination, I identified three variables that would identify how those benefits were allocated. The Department of Education's College Scorecard provides three variables that segment median debt at separation by family income. Low-income students are grouped into a variable that measures student loans at separation for the median borrower from families that report \$0-\$30,000 in annual income. Middle-income students are grouped into a variable that measures loans at separation for the median borrower from families that report \$30,000 to \$74,999 in annual income. High-income students are grouped into a variable that measures student loans at separation for the median borrower families that report \$75,000 or more in annual income. The results suggest that borrowers in all three income brackets are at least as well-off under coordination as their control school counterparts. The benefits to coordination appear to have a negative relationship with income, and low-income borrowers appear to be the primary beneficiaries of coordination.

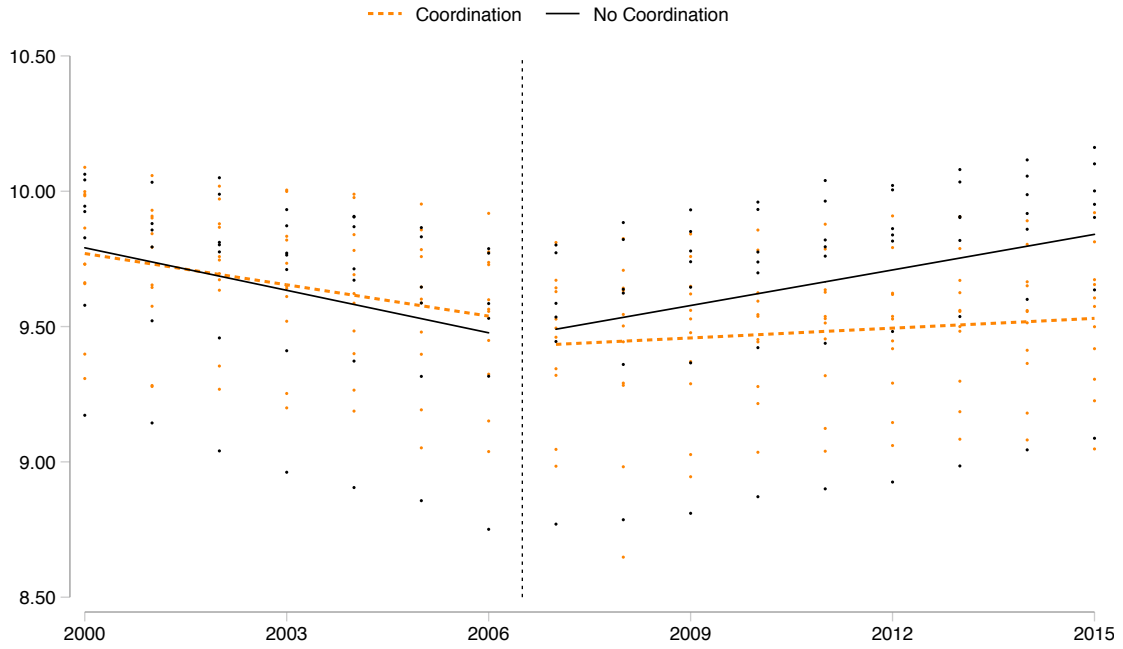
Table 4a shows the results of all models against median debt for high-income borrowers. In Columns 1-3, the coefficient on *Post\*Coordination* shows that the median high-income borrower at a coordinating school graduated with 15 to 16 percent less in debt than a similar borrower at a control school in the post-coordination period.<sup>29</sup> Over the same period, the median borrower at a control school experienced a 16 percent to 19 percent increase in graduating debt. On net, high-income borrowers at both coordinating and non-coordinating

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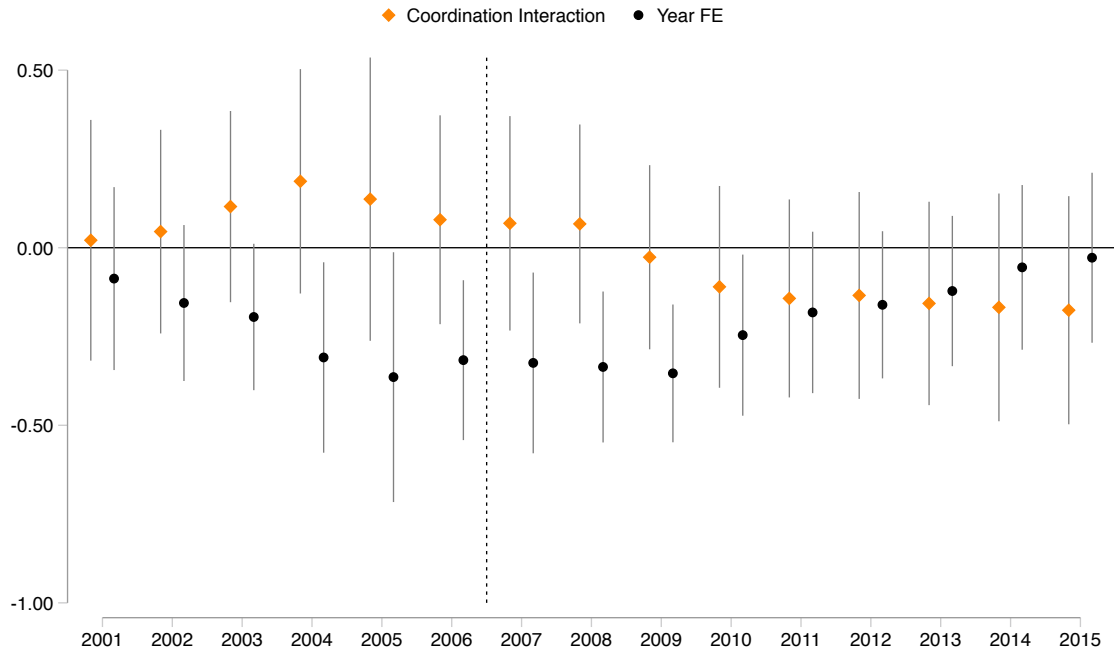
29. Omitting Princeton and Bryn Mawr, which participated in Overlap Group meetings, from the control group found that high-income borrowers in the coordination group saw a slight 3 percent increase in borrowing relative to control schools. All other estimations yielded similar interpretations, with benefits concentrated amongst low-income borrowers. This result remains consistent with results that suggest an overall transfer of financial aid resources to needy students.

Figure 2: Median Debt at Separation

(a) Difference-in-Differences Estimation from Column 4



(b) Coordination-Year Interaction Coefficients from Column 5



Note: Figures are displayed in log of dollars. The Post-Coordination period begins after 2006, four years after the Consensus Approach was first used to offer aid to admitted students.

schools experienced an increase in debt in the post-coordination period, though the increase at coordinating schools was much more modest at 1 percent to 4 percent. The high-income coordination outcome is unlike the result for the median borrower, who saw no overall increase in debt over the period analyzed. Still, because the behavior of control schools represents a counterfactual outcome, high-income borrowers benefited under a system of coordination as they only partially internalized the overall growth in student loan debt levels, according to the estimations in Columns 1-3.

Controlling for a time-trends in the data weakens this observed relationship, and suggests that high-income borrowers at coordinating schools are not necessarily better-off but also not worse-off than their control school counterparts. Column 4 predicts no immediate drop in borrowing for high-income borrowers in the post-coordination period. The model predicts that borrowing increases by 6 percent annually in both groups in the post-coordination period, and the difference in the rate of change in borrowing between coordinating schools and control schools is not statistically significant. Figure 3a shows the increase in borrowing over time.

The results in Column 5 are similar. Table 4b shows the coordination-year interaction terms from the Column 5 estimation. Figure 3b plots these coefficients. None of the coordination-year interaction terms are found to be statistically significant, nor did a joint-test find statistical significance in the post-coordination years. The results indicate an overall and equal increase in borrowing amongst high-income borrowers in both groups.

The year dummies capture a significant increase in borrowing amongst high-income students at control schools. The coefficients start to increase considerably in 2012, continuing through 2015. From 2000 to 2015, borrowing increased 45 percent for the median high-income borrower in both groups. The results demonstrate that the cost of college and student loan levels are rising rapidly overall, and the benefits of coordination are relatively small for high-income borrowers.

Coefficients on other explanatory variables are generally consistent with the median borrower prediction. Investment revenues remain a weak predictor of borrowing, as are institutional support spending and net tuition revenue. Column 4 predicts that a 1 percent increase in instructional spending per student leads to a 0.03 percent increase in borrowing by high-income borrowers. Reliance on government funding remains a strong predictor of borrowing in the high-income model. Column 4 and Column 5 predict that a 1 percent increase in an institution's reliance on government revenues leads to a roughly 0.1 percent increase in borrowing by high-income borrowers.

Across the board, coefficients on school fixed effects are lower on the high-income borrower relative to the median borrower. This result shows that relatively higher-income students

borrow less than the median borrower. This result demonstrates that high-income students, who have greater resources, are less likely to rely on federal loans to cover the cost of college. The higher coefficients on the median borrower must be caused by higher borrowing by middle and low-income students, as shown in the discussion that follows.

Table 5a shows the results of all models against median debt for middle-income borrowers, defined as having family incomes from \$30,001 to \$74,999. The results show that middle-income borrowers in both groups saw an overall increase in borrowing from 2000 to 2015, but the increase was much more modest than that experienced by high-income borrowers. The models provide evidence that the median middle-income borrower was better off under coordination relative to borrowers at the counterfactual control schools.

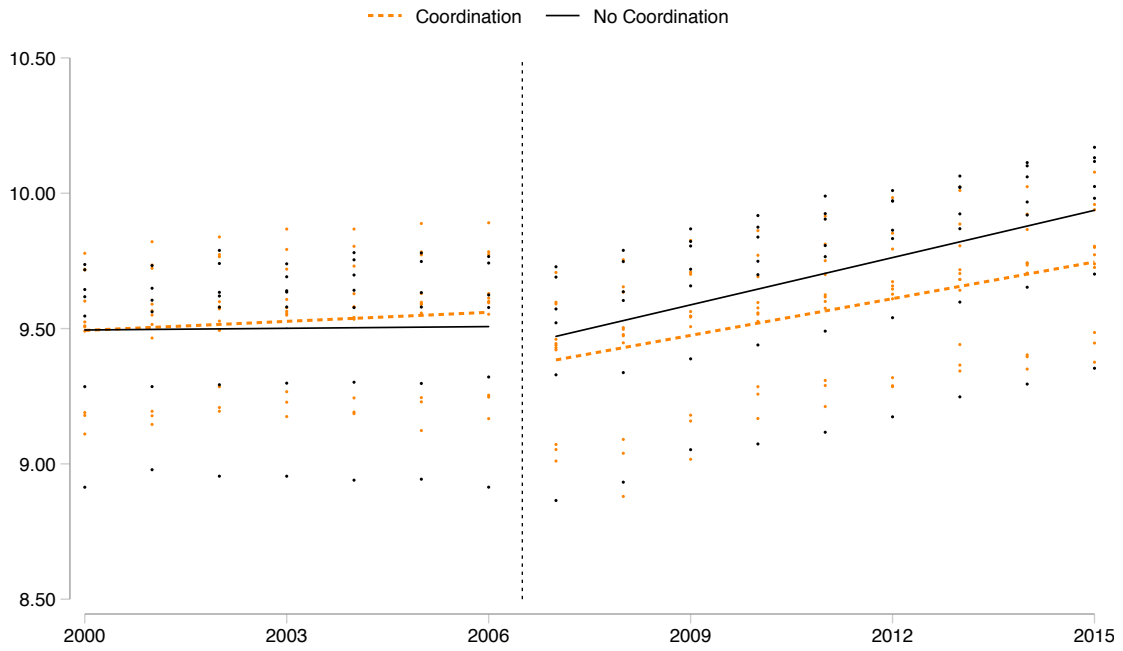
Columns 1 and 2 predict that a student at a coordinating school graduated with 16 percent less in debt than their counterpart at a control school in the post-coordination period. The coefficient in Column 1 is not significant. The coefficient in Column 2, which controls for certain institutional revenues and expenditures, is significant at the 10 percent level. Over the same period, the median middle-income borrower at control schools saw no observable increase in borrowing. Therefore, Columns 1 and 2 predict that students at coordinating schools experienced a 16 percent net decrease in borrowing in the post-coordination period, indicating that coordination may be associated with a decrease in borrowing. Adding institution-specific fixed effects in Column 3 yields similar results. Column 3 similarly predicts a roughly 14 percent net decrease in borrowing at coordinating schools, and the coefficient is significant at the 5 percent level.

Controlling for time-trends in Column 4 shows no short-run impact due to coordination but provides evidence for a substantial long-run impact. According to Column 4, borrowing at coordinating schools was 7 percent lower annually in the post-coordination period, relative to borrowing at control schools. Borrowing at control schools increased by 7 percent annually over the same period. Therefore, the estimation in Column 4 finds that middle-income borrowers saw no net increase in debt and borrowed less than their control school counterparts. The results show that, in an environment in which the cost of college was rising overall, coordination seemed to have a dampening effect, as shown in Figure 4a. Coordination allowed students at those schools to mitigate some – if not all – of those cost increases.

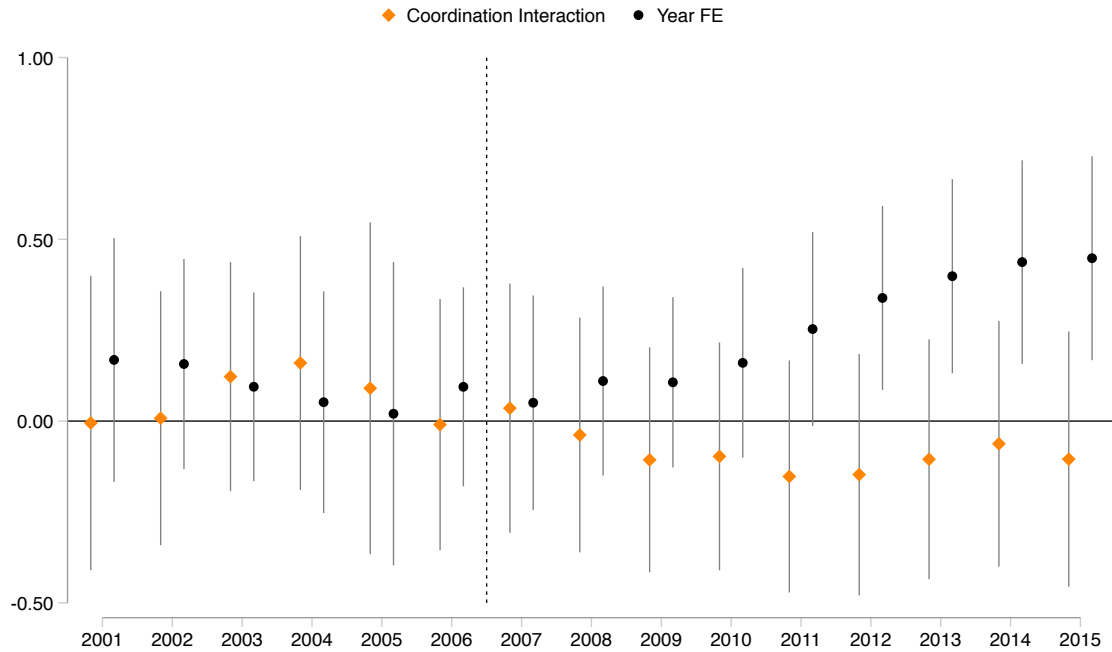
Column 5 finds little-to-no change in borrowing by middle-income borrowers in both groups. Table 5b displays the coordination-year interaction coefficients. Figure 4b plots these coefficients. The year dummies suggest that borrowing was on a slight downward trajectory in the early part of the years analyzed before returning to base year levels in later years. Starting in 2010 and continuing through 2015, the coefficients on the coordination-interaction terms are negative and generally increasing in magnitude year-to-year. These terms could

Figure 3: Median Debt at Separation (High Income)

(a) Difference-in-Differences Estimation from Column 4



(b) Coordination-Year Interaction Coefficients from Column 5



Note: Figures are displayed in log of dollars. The Post-Coordination period begins after 2006, four years after the Consensus Approach was first used to offer aid to admitted students.

suggest some evidence for a relative decrease in borrowing at coordinating schools, though the standard deviations are high and the terms are not statistically significant. In a test of joint significance, the coefficients in the post-coordination years are just outside the threshold to be considered significant at the 10 percent level.

Unlike in the median and high-income borrower models, the number of students receiving financial aid appears to be a strong predictor of borrowing for middle-income students. The models estimate that a 1 percent increase in the number of students on aid leads to a roughly 0.4 percent decrease in borrowing for middle-income students. Institutional support spending is also a strong predictor in this model, and a 1 percent increase in institutional support spending per student is associated with a roughly 0.09 percent increase in borrowing. In this model, reliance on government revenues is a relatively weak predictor.

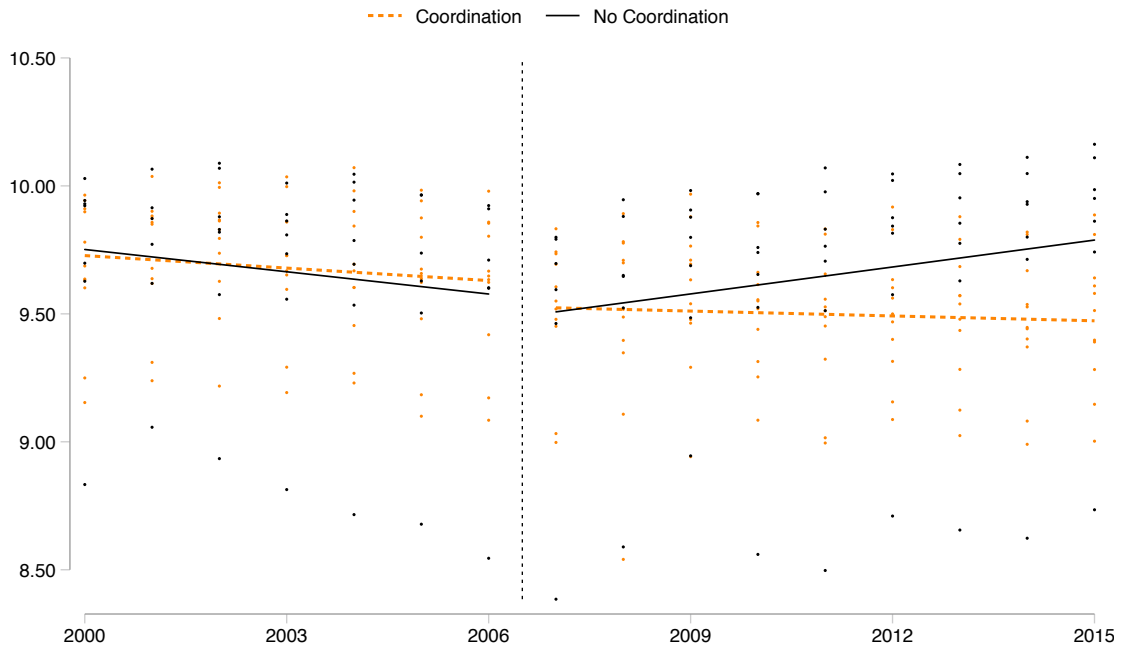
Table 6a shows the results of all models against median debt for low-income borrowers, defined as having a family income of \$30,000 or below. The results demonstrate that low-income borrowers were the primary beneficiaries of coordination and were better-off as a result.

Columns 1-3 predict with high statistical significance that low-income borrowers at coordinating schools graduated with roughly 26 percent less in loans than their counterparts at control schools in the post-coordination period. Over the same period, the models predicted that borrowing increased by up to 10 percent at control schools. The models suggest that coordination was able to reduce the impact of borrowing levels that were increasing overall and is also associated with a net reduction in student loan debt for these low-income students. That said, the coefficient on *Time\*Coordination* in Column 4 is statistically significant at the 10 percent level, which means that the coordination and control group may be moving in slightly different directions in the pre-coordination period for the dependent variable. Parallel pre-coordination trends is a necessary condition for this analysis, so Column 4 and Column 5, which control for time-trends, are especially appropriate in this case.

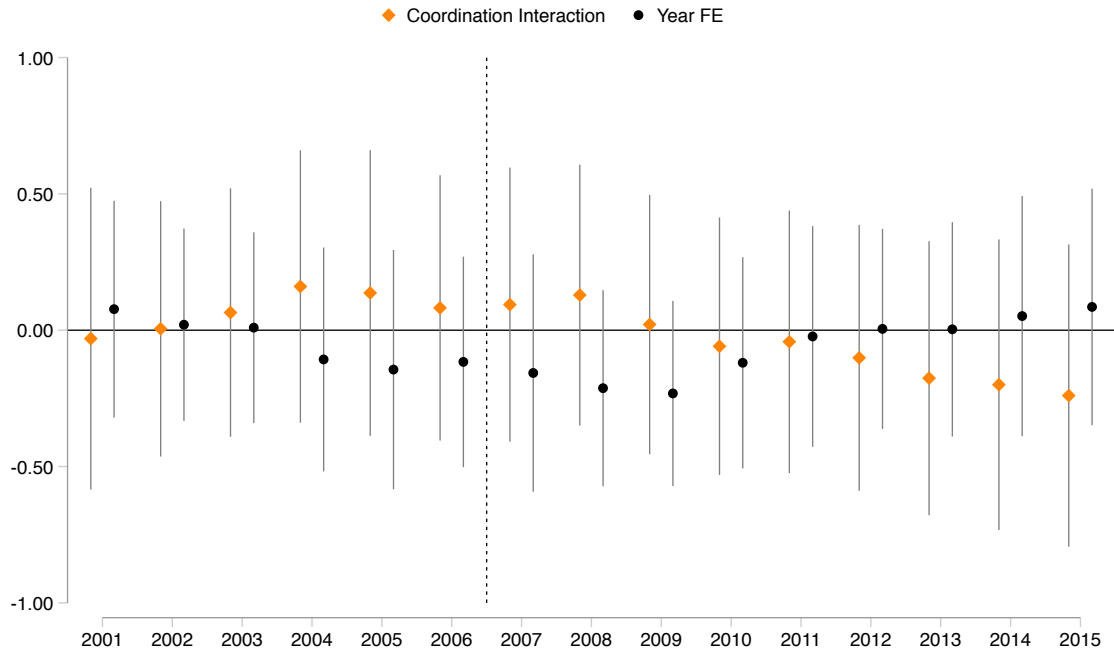
With a control for time-trends, Column 4 and Column 5 remain consistent with the results in Columns 1-3 and provide further insights into the effects of coordination on low-income borrowers. Column 4 found no short-term impact, as the coefficient on *Post\*Coordination* is not statistically significant. The coefficient on *Time\*Post\*Coordination* is significant at the 1 percent level. Figure 5a shows a large decrease in borrowing at coordinating schools in the post-coordination period. The change appears to continue an existing trend of decreasing borrowing, though the trend did not continue at control schools in the post-coordination period. The relative decrease suggests that coordination supported a slowdown in the rate of borrowing by low-income students. Relative to students at control schools, students at coordinating schools saw steady decreases in graduating debt levels amongst low-income

Figure 4: Median Debt at Separation (Middle Income)

(a) Difference-in-Differences Estimation from Column 4



(b) Coordination-Year Interaction Coefficients from Column 5



Note: Figures are displayed in log of dollars. The Post-Coordination period begins after 2006, four years after the Consensus Approach was first used to offer aid to admitted students.



students, up to 10 percent annually. Over the same post-coordination period, borrowing increased by up to 12 percent annually at control schools. This result, which suggests large benefits from coordination to low-income students, is consistent with previous results that provided evidence for a long-run impact.

Table 6b shows the coordination-year interaction coefficients for Column 5. Figure 5b plots these coefficients. The coefficients on these coordination-year interaction terms are negative in much of the post-coordination period and increasing in magnitude through 2015. By 2015, a low-income borrower at a coordinating school graduates with 40 percent less in federal loans than their counterpart at a control school, a result which is statistically significant at the 10 percent level. The year fixed-effect coefficients are negative and significant from 2002 through 2015. This result suggests that borrowing decreased at control schools too throughout the years analyzed. By the start of the post-coordination period in 2007, a low-income borrower at a control school would have borrowed 57 percent less than they would have borrowed in 2000. A year later, savings peaked at 60 percent before steadily decreasing. By 2015, a low-income borrower at a control school would have borrowed close to 30 percent less than they would have borrowed in 2000. Therefore, the total effect on low-income borrowers at coordinating schools from 2000 to 2015 is the sum of both 2015 coefficients. The median low-income borrower at a coordinating school in 2015 graduated with nearly 70 percent less in debt than a similar borrower in 2000.

The results in the low-income borrower model are particularly large and significant. They suggest that the relative debt reductions for the median borrower due to coordination were likely driven by savings amongst low-income borrowers.

The results on other explanatory variables are similar to the median middle-income borrower predictions, with few differences in significance or magnitude in coefficients. In addition, the coefficients on school fixed effects are higher in the low-income estimation than in the median and high-income estimation. This result supports my earlier finding that low-income students relied more heavily on student loans to finance their education.

Overall, these debt variables demonstrate a long-run shift in institutional resources at coordinating schools that allowed low-income students to graduate with less debt. This result is consistent with statements by the 568 Presidents Group on the purpose of coordination: to increase college affordability and accessibility for low-income students.

### **4.3 No Relative Increase in Tuition Revenues**

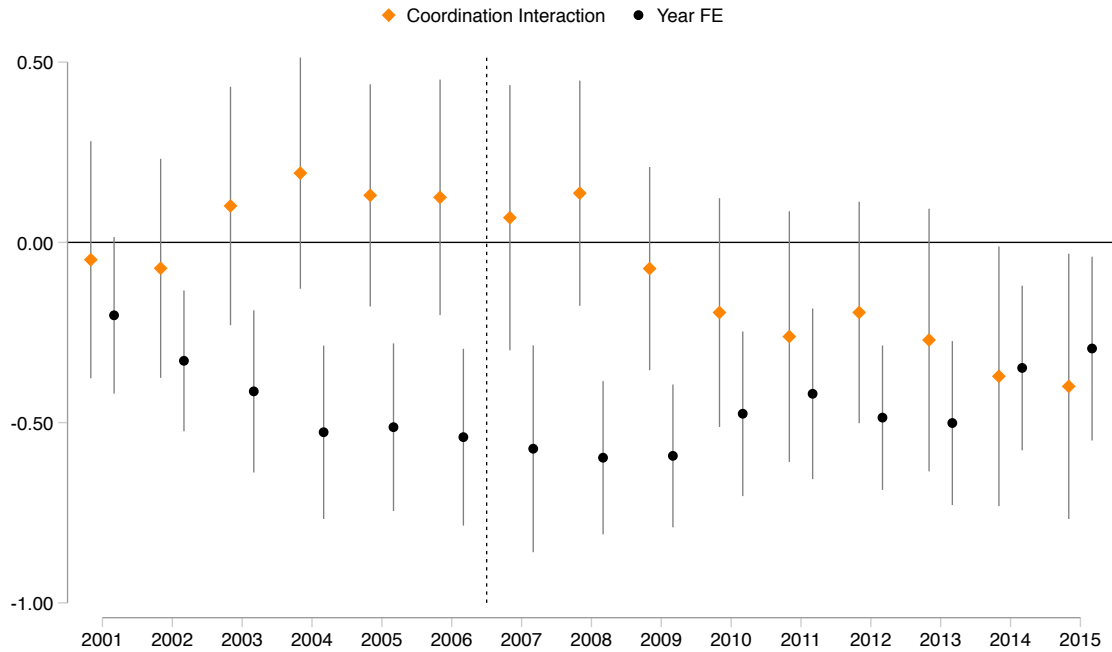
It is possible that coordination could lead to a decrease in borrowing per student without promoting student welfare in the aggregate. Consider a scenario in which schools use co-

Figure 5: Median Debt at Separation (Low Income)

(a) Difference-in-Differences Estimation from Column 4



(b) Coordination-Year Interaction Coefficients from Column 5



Note: Figures are displayed in log of dollars. The Post-Coordination period begins after 2006, four years after the Consensus Approach was first used to offer aid to admitted students.

ordination to make cuts to financial aid packages to certain high-income students without increasing funding for relatively more needy students dollar-for-dollar. This scenario would lead to an increase in net tuition revenue per student that would leave students at coordinating schools worse-off in the aggregate. My results suggest that coordination was not associated with a statistically significant change in net tuition revenue per student. Table 7a shows these results.

Columns 1-3 predicted no significant impact on net tuition revenue due to coordination. Though the schools in the coordination group started at a higher level of net tuition revenue per student on average, the different groups do not trend differently in the pre-coordination period. Column 1 does not find any overall change in revenues over time. Adding controls for certain expenses and revenues, in addition to institution-specific fixed effects, as in Column 2 and Column 3 respectively, shows a 1 percent increase in net tuition revenues per student on average in the post-coordination period. The results show that this increase affected control and coordination schools equally.

Column 4 is similarly consistent with the results from Columns 1-3. It predicts neither an immediate change in net tuition revenues nor a long-run effect due to coordination. Consistent with the result, in Figure 6a, the trends in borrowing in the post-coordination period appear parallel. Column 4, which controls for time trends, does not however, find a significant increase in net tuition revenues in the post-coordination period in either group, indicating the presence of a time-trend in the data that should be (and is) controlled for.

Similarly, Column 5 does not predict any impact on net tuition revenue per student due to coordination. Table 7b shows the coordination-year Interaction coefficients from Column 5. Figure 6b plots these coefficients. None of the coordination-year interaction terms are significant. A test of the coefficients in the post-coordination years did not find evidence for joint significance. Column 5 finds overall and equal increases in net tuition revenue across both groups, generally increasing in magnitude. From 2000 to 2015, the average school sampled increased net tuition revenue per student by about 2 percent.

Some explanatory variable results stand-out. Investment revenues, which may fluctuate in the short-run with the market, have little if any impact on net tuition revenue, which is likely determined by a more long-run set of variables. The coefficient on instructional expenditures per student is also a weak predictor, which is surprising because I expected a relationship between tuition-setting and such a large component of overall college costs. Institutional support spending is similarly weak as a predictor. Reliance on government revenue is significant and negatively correlated with modest effects. The models predict that a 1 percent increase in share of revenue from government sources leads to a 0.01 percent decrease in net tuition revenue per student. The number of students on aid is, intuitively, a strong

predictor. The models predict that a 1 percent increase in the number of students receiving aid is associated with a 0.01-0.03 percent decrease in net tuition revenue per student.

#### 4.4 No Decrease in Total Pell Grant Outlays

The previous results showed that borrowers at coordinating schools, particularly low-income borrowers, saw relative decreases in student loan levels in the post-coordination period. Without more information, this result could be due to a scenario in which coordinating schools increased grant aid for the median needy student while decreasing total enrollment of low-income students. In this scenario, low-income students in aggregate would not benefit from coordination as the impact of coordination would simply be a welfare transfer from some needy students to other equally needy students.

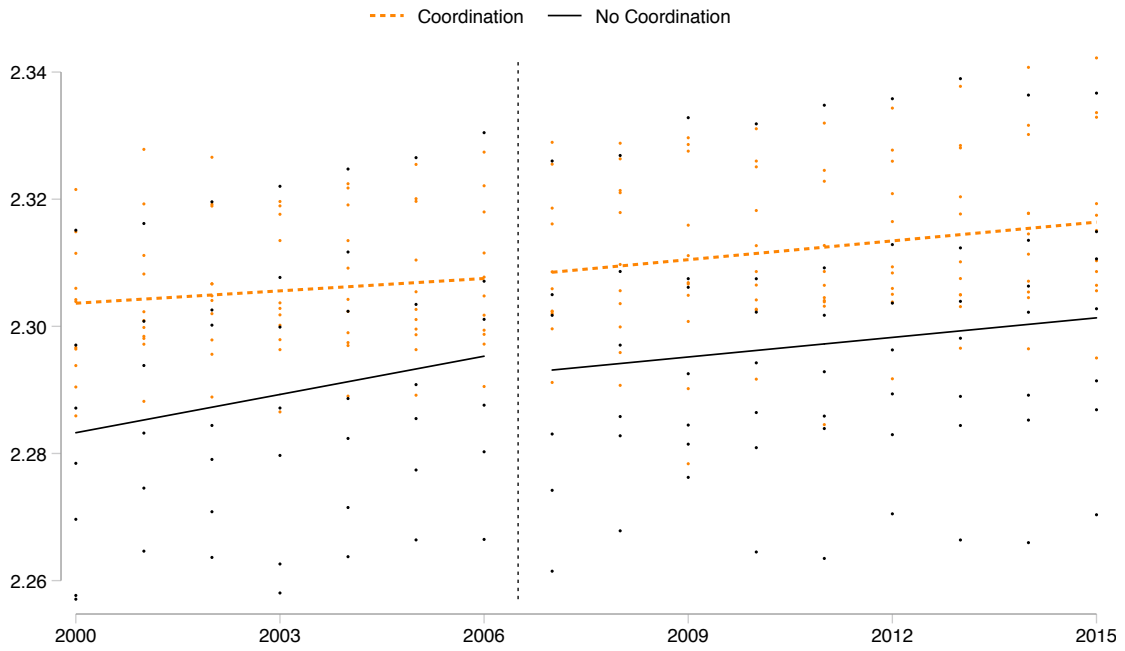
To verify this hypothesis, I used a dependent variable that measures total Pell Grant outlays, which are awarded to students with exceptional financial need, at an institution annually. If coordinating schools reduced low-income enrollment in the post-coordination period, the predicted change in total Pell Grant outlays in the post-coordination period would be negative. I do not find evidence for the existence of a relationship between coordination and Pell Grant outlays. Therefore, the previous results suggest that the gains enjoyed by the median low-income borrower were due to new institutional resources allocated to that cohort of students – not a transfer within the cohort. Table 8a shows the results of all models against total Pell Grant outlays.

Column 1 shows that Pell Grant outlays increased at schools in both groups by about 46 percent during the post-coordination period. There is no significant difference in growth trends between the two groups in the post-coordination period. After controlling for certain explanatory variables, Column 2 mitigates the effect from Column 1 and finds an increase of 29 percent in both groups, significant at the 1 percent level. Column 2 however lacks institution-specific fixed effects, which may be necessary to analyze a variable which may be determined by time-invariant characteristics that vary on the school-level. That said, adding these fixed effects only slightly increases the size of the coefficient, now a decrease of 30 percent at the 1 percent level. Column 3 lacks controls for time-trends, which an F-Test of the model showed would be necessary.

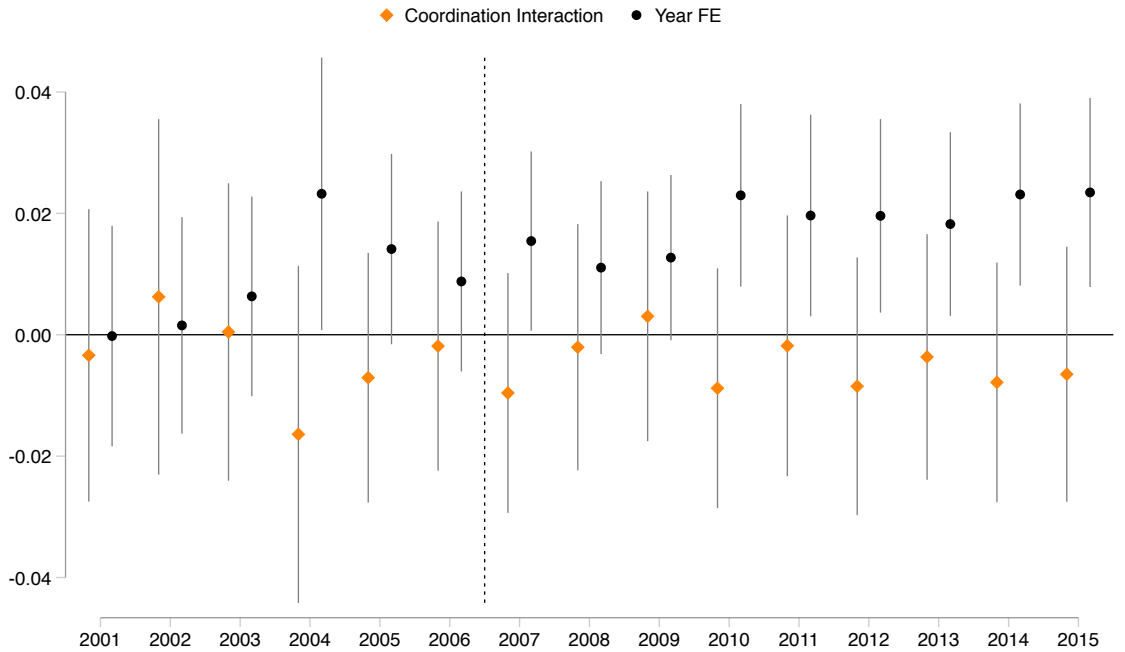
The results in Column 4 and Column 5 are consistent with Columns 1-3. Having added a control for time-trends, Column 4 finds no short-run or long-run impact on Pell Grant outlays due to coordination. Column 4 finds that total Pell Grant outlays increase by 6 percent annually in the sample schools in the post-coordination period, and this increase is observed equally in both groups, as shown in Figure 7a.

Figure 6: Net Tuition Revenue per Student

(a) Difference-in-Differences Estimation from Column 4



(b) Coordination-Year Interaction Coefficients from Column 5



Note: Figures are displayed in log of dollars. The Post-Coordination period begins after 2006, four years after the Consensus Approach was first used to offer aid to admitted students.

The results in Column 5 are similar. Table 8b shows the coordination-year interaction coefficients from Column 5. Figure 7b plots these coefficients. No coordination-year interaction term carries a significant coefficient. A test of the coefficients in the post-coordination years did not find evidence for joint significance. The year dummies are significant and positive from 2002 through 2005 and again from 2008 through 2015. Because significance was not observed in the coordination-interactions, the results suggest that both groups of schools saw a roughly equal increase in total Pell Grant outlays over the data-years. From 2000 to 2015, the average sample school increased total Pell Grant outlays by 79 percent.

Total Pell Grant outlays is highly and positively correlated with net tuition revenue per student, which is to be expected, because the Pell Grant is a source of tuition revenue. Reliance on government revenues is also a significant predictor, which is similarly expected because the Pell Grant is funded by the federal government.

#### 4.5 No Change in Total Institutional Grant Aid Outlays

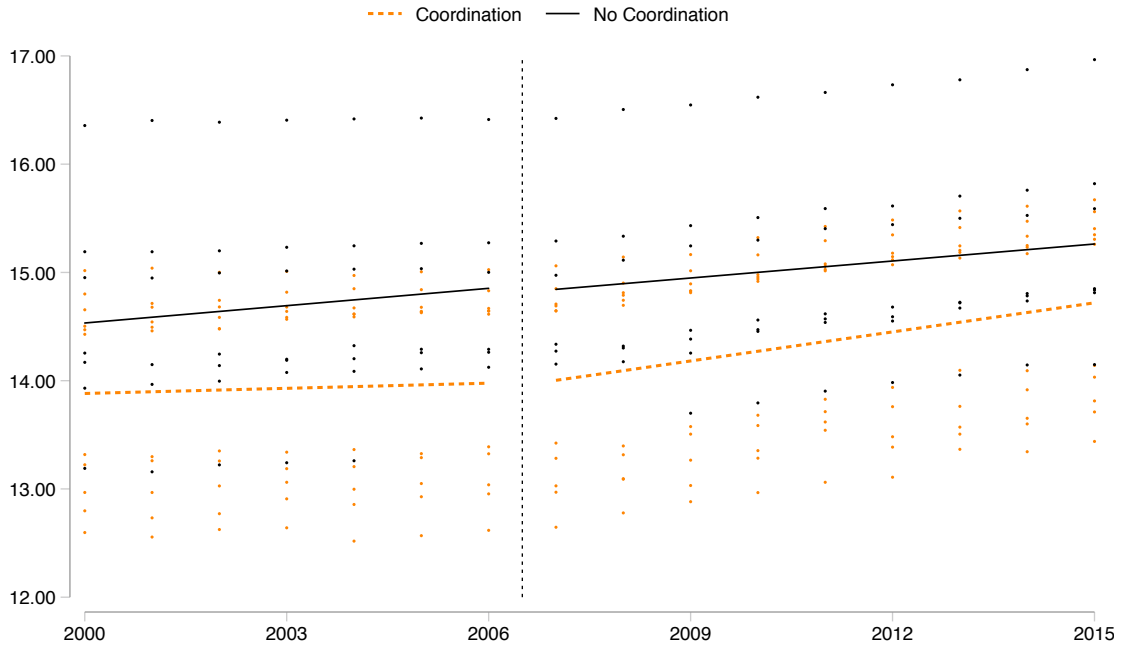
The previous results demonstrate that needy students are being given access to more financial aid resources under coordination. Where does the money come from? The Consensus Approach is not necessarily designed to be cost-saving. It is primarily a method by which schools may divert existing financial aid resources from some students to others such that the average aid recipient is lower-income or has less in assets. It follows that students that would have been considered for financial aid pre-coordination receive less or none at all post-coordination. The alternative scenario would require a shift in institutional expenditures toward financial aid – cutting spending elsewhere – to pay for larger grant awards for the neediest students.

I tested this hypothesis against a measurement of institutional grant aid and found no evidence of a relationship between total grant outlays and coordination. This result is consistent with the hypothesis that the decrease in low-income student borrowing was due to an increase in grants to low-income students, likely paid for by shifting existing institutional grant aid resources from relatively higher-income students to the neediest students. Given that the high-income borrower did not see a relative increase in borrowing due to coordination, it appears that those students who would have received financial aid – but did not – also did not proceed to take out loans in response. Table 9a shows the results of all models against total institutional grant aid.

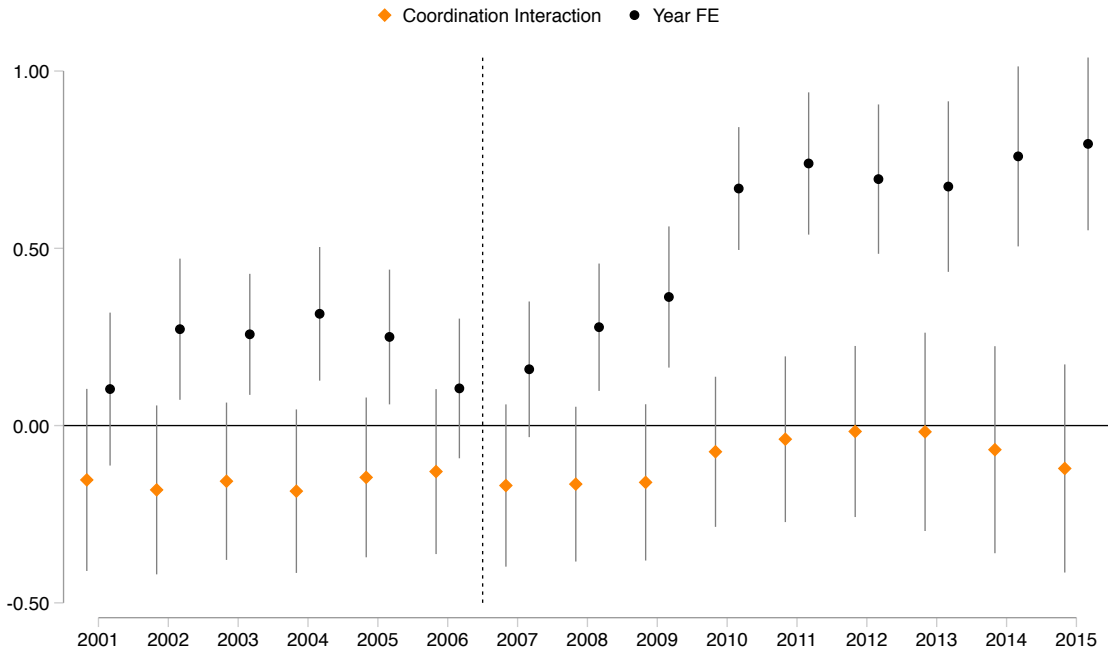
Columns 1-3 show that institutional grant aid increased equally in both groups during the post-coordination period. Column 1 finds that both groups saw an average increase of 46 percent in institutional grant aid in the post-coordination period. Adding explanatory

Figure 7: Total Pell Grant Outlays

(a) Difference-in-Differences Estimation from Column 4



(b) Coordination-Year Interaction Coefficients from Column 5



Note: Figures are displayed in log of dollars. The Post-Coordination period begins after 2006, four years after the Consensus Approach was first used to offer aid to admitted students.

variables reduces the coefficient to about 29 percent. Adding institution-specific fixed effects slightly increases the coefficient to 30 percent.

Similarly, Column 4 finds no evidence of a relationship between coordination and institutional grant aid in both the short-run and long-run. Column 4 finds that both schools equally observed a long-run increase in institutional grant outlays, as shown in Figure 8a. This long-run result may be caused by a trend of steadily increasing undergraduate enrollment in both groups.

Column 5 also finds no relationship, as none of the coordination-year interaction terms show a significant coefficient. Table 9b shows the coordination-year interaction coefficients from Column 5. Figure 8b plots these coefficients. Column 5 similarly finds a steady and equal increase in total institutional grants in both groups. A test of the coefficients in the post-coordination years did not find evidence for joint significance. From 2000 to 2015, the average sample school increased total institutional grant aid by 79 percent.

## 5 Conclusion

Coordination on student financial aid packages under the Consensus Approach led to a decrease in student borrowing at schools that coordinated relative to similar schools that did not. This decrease was large, generated benefits primarily enjoyed by low-income students, and occurred in the long-run.

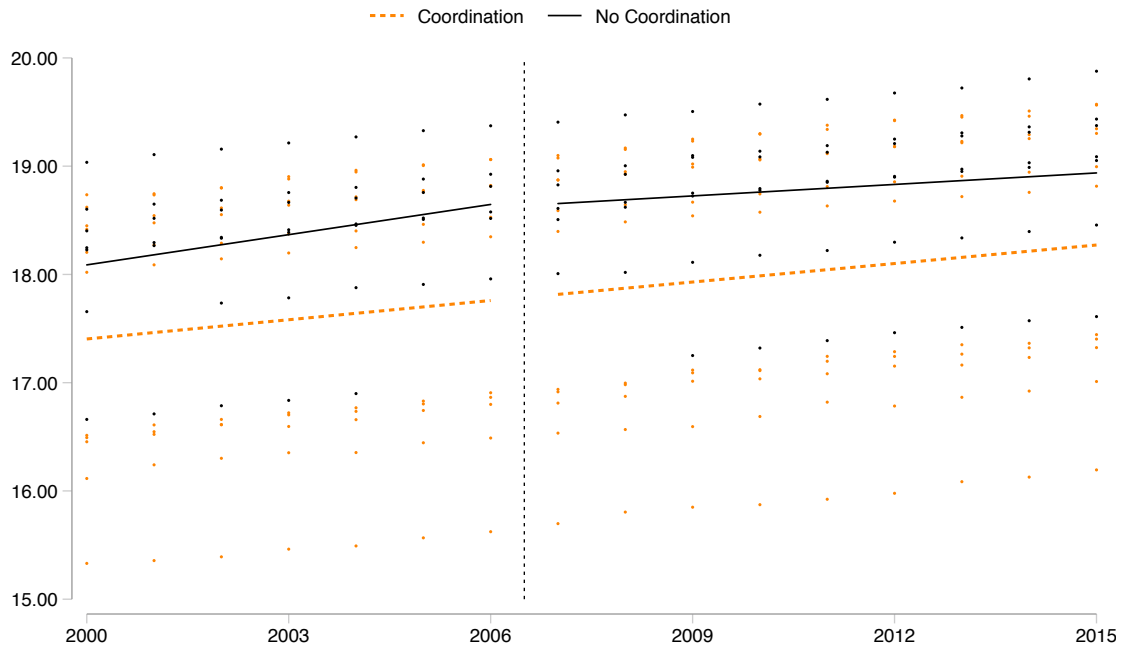
In general, the results support the claims put forward by the 568 Presidents Group, consistent with the purpose of the pre-exemption era Overlap Group. Needy students borrowed observably less in the post-coordination period at coordinating schools than in the counterfactual non-coordinating schools. The primary beneficiaries were low-income students. Given no observable effect on net tuition revenue per student, Pell Grant outlays, or total institutional grant aid, it is likely that the decrease in borrowing was due to a shift in institutional resources from relatively high-income students to students with more need. This shift was likely funded by a coordinated effort to divert financial aid resources to high-need students using the Consensus Approach, which modified the need-analysis methodologies previously in place. In addition, it appears that relatively low-need students who would have received financial aid (and received less or none at all due to coordination) did not increase their borrowing in the post-coordination period.

These effects occurred in the long-run for several reasons. First, though the exemption was first enacted in 1992, the Consensus Approach was not devised and used until the 2002-2003 academic year. Second, data that samples entire undergraduate cohorts, such as those variables estimated by previous studies, fail to accurately capture the effects of a policy

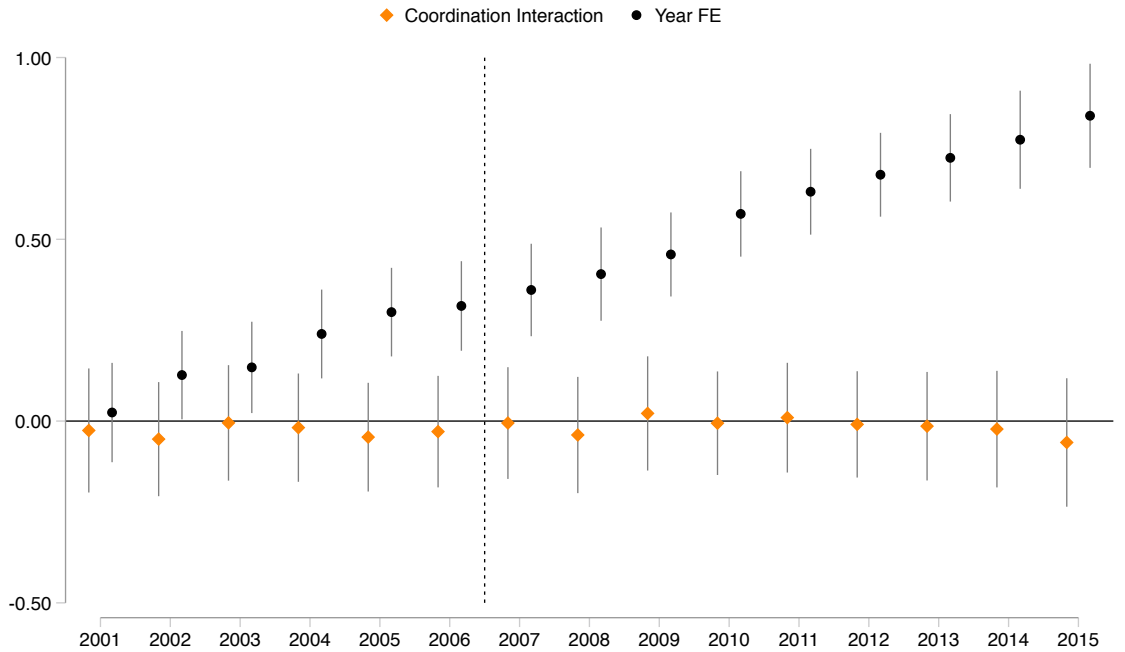


Figure 8: Total Institutional Grant Aid

(a) Difference-in-Differences Estimation from Column 4



(b) Coordination-Year Interaction Coefficients from Column 5



Note: Figures are displayed in log of dollars. The Post-Coordination period begins after 2006, four years after the Consensus Approach was first used to offer aid to admitted students.

change that affects one new class-year at a time. As a result, the effects would not be clear for several years after implementation. By relying on a debt variable measured at separation, this issue is mitigated. Third, it is possible that colleges had to overcome a learning curve before being able to most efficiently share information successfully implement coordination so as to increase college affordability. Using a coordination-year interaction fixed effect allows for a granular understanding of these changes over time.

Policymakers should consider whether the current limited exemption can be modified to work more effectively. In a 2008 white paper, the Institute for College Access and Success suggested that cooperation could increase college affordability but that the vague, confusing, and highly restrictive nature of the exemption prevents colleges from taking advantage of it. An antitrust law firm hired by TICAS said that the language of the current exemption is “too dangerous for a group of colleges to enter it. More cooperation will only get the approval of a college’s lawyers if there is a clearer, broader exemption in statute.”<sup>30</sup> TICAS also notes the lack of clear data and definition of what practices constitute the “need-blind” admissions policy that is necessary to qualify for the exemption.<sup>31</sup> Minor revisions to the legislative language of the exemption may decrease the transaction costs that limit coordination and more effectively capture the potential benefits of it.

Policymakers also have the option of pursuing a wide expansion of the exemption to all private, nonprofit schools regardless of need-blind status, as proposed by the NAICU, though the results of this paper would not yield an assessment of the potential impact. The current exemption, which is limited to schools that admit students on a need-blind basis, produces a group of coordinating schools that is not representative of the typical college in the United States. The schools currently exempt generally serve wealthier student bodies, have larger endowments, and are more selective. It is not clear that other colleges, if given an exemption, would behave similarly to the schools examined here.

Given these differences, any expansion of the exemption should be accompanied by ongoing data collection and regulatory oversight. Legislation that expands the exemption should require colleges and universities to maintain and regularly provide student-level price and financial aid data to the relevant authorities. In turn, regulators can use that data to examine the expansion of the exemption and the effect on prices.

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30. Matthew Reed and Robert Shireman, *Time to Reexamine Institutional Cooperation on Financial Aid*, technical report (Institute for College Access & Success, June 2008), 10, [https://ticas.org/sites/default/files/pub\\_files/antitrust.pdf](https://ticas.org/sites/default/files/pub_files/antitrust.pdf).

31. *Ibid.*, 8.

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# Tables

Table 3a: Log of Median Debt at Separation

	(1)	(2)	(3)	(4)	(5)
Coordination	0.00 (0.06)	0.01 (0.06)			
Post	0.02 (0.07)	0.00 (0.07)	0.02 (0.04)	0.07 (0.08)	
Post*Coordination	-0.19** (0.09)	-0.19** (0.08)	-0.18*** (0.05)	-0.13 (0.10)	
Investment Revenues per Student ( <i>millions</i> )		-0.75* (0.44)	0.05 (0.17)	0.03 (0.14)	-0.11 (0.18)
Log of Inst. Support Spending per Student		0.02 (0.03)	0.07* (0.04)	0.06 (0.04)	0.06 (0.04)
Log of Net Tuition Revenue per Student		3.19** (1.33)	1.04 (1.10)	1.34 (0.98)	1.61 (1.07)
Log of Instructional Spending per Student		-0.06 (0.09)	0.01 (0.03)	0.03 (0.02)	0.02 (0.02)
Log of Students on Aid		0.08*** (0.03)	-0.21 (0.14)	-0.20 (0.13)	-0.19 (0.13)
Log of Revenue Share from Government		0.02 (0.03)	0.00 (0.04)	0.06* (0.04)	0.09** (0.04)
Time				-0.06*** (0.02)	
Time*Coordination				0.02 (0.02)	
Time*Post				0.10*** (0.02)	
Time*Post*Coordination				-0.06** (0.02)	
Constant	9.65*** (0.06)	2.24 (2.51)			
School FE	No	No	Yes	Yes	Yes
Coordination-Year Interaction	No	No	No	No	Yes
Observations	288	283	283	283	283
R-squared	0.059	0.229	1.000	1.000	1.000

Note: See Table 3b for Coordination-Year Interaction coefficients from Column 5.

Table 3b: Coordination-Year Interaction Coefficients

	Year FE	Coord. Interaction
2001	-0.09 (0.13)	0.02 (0.17)
2002	-0.16 (0.11)	0.05 (0.15)
2003	-0.20 (0.10)	0.12 (0.14)
2004	-0.31* (0.14)	0.19 (0.16)
2005	-0.36* (0.18)	0.14 (0.20)
2006	-0.32** (0.11)	0.08 (0.15)
2007	-0.32* (0.13)	0.07 (0.15)
2008	-0.34** (0.11)	0.07 (0.14)
2009	-0.35*** (0.10)	-0.03 (0.13)
2010	-0.25* (0.12)	-0.11 (0.14)
2011	-0.18 (0.12)	-0.14 (0.14)
2012	-0.16 (0.11)	-0.13 (0.15)
2013	-0.12 (0.11)	-0.16 (0.15)
2014	-0.06 (0.12)	-0.17 (0.16)
2015	-0.03 (0.12)	-0.18 (0.16)

Table 4a: Log of Median Debt at Separation (High Income)

	(1)	(2)	(3)	(4)	(5)
Coordination	0.01 (0.06)	0.03 (0.06)			
Post	0.19*** (0.07)	0.18*** (0.07)	0.16*** (0.05)	-0.04 (0.09)	
Post*Coordination	-0.15* (0.08)	-0.16** (0.08)	-0.15*** (0.05)	-0.13 (0.10)	
Investment Revenues per Student ( <i>millions</i> )		-0.59 (0.40)	0.07 (0.18)	-0.05 (0.16)	-0.02 (0.22)
Log of Inst. Support Spending per Student		0.05** (0.02)	0.05** (0.02)	0.03 (0.02)	0.02 (0.02)
Log of Net Tuition Revenue per Student		1.79 (1.14)	1.26 (0.95)	0.15 (0.91)	0.50 (1.01)
Log of Instructional Spending per Student		0.00 (0.05)	0.05*** (0.02)	0.03* (0.02)	0.02 (0.02)
Log of Students on Aid		0.13*** (0.03)	0.10 (0.13)	-0.08 (0.11)	-0.01 (0.11)
Log of Revenue Share from Government		-0.02 (0.03)	0.06 (0.05)	0.09** (0.04)	0.12*** (0.04)
Time				-0.00 (0.02)	
Time*Coordination				0.01 (0.02)	
Time*Post				0.06** (0.02)	
Time*Post*Coordination				-0.02 (0.03)	
Constant	9.52*** (0.05)	4.03* (2.28)			
School FE	No	No	Yes	Yes	Yes
Coordination-Year Interaction	No	No	No	No	Yes
Observations	288	283	283	283	283
R-squared	0.052	0.222	1.000	1.000	1.000

Note: See Table 4b for Coordination-Year Interaction coefficients from Column 5.

Table 4b: Coordination-Year Interaction Coefficients

	Year FE	Coord. Interaction
2001	0.17 (0.17)	-0.01 (0.21)
2002	0.16 (0.15)	0.01 (0.18)
2003	0.09 (0.13)	0.12 (0.16)
2004	0.05 (0.15)	0.16 (0.18)
2005	0.02 (0.21)	0.09 (0.23)
2006	0.09 (0.14)	-0.01 (0.18)
2007	0.05 (0.15)	0.04 (0.17)
2008	0.11 (0.13)	-0.04 (0.16)
2009	0.11 (0.12)	-0.11 (0.16)
2010	0.16 (0.13)	-0.10 (0.16)
2011	0.25 (0.14)	-0.15 (0.16)
2012	0.34** (0.13)	-0.15 (0.17)
2013	0.40** (0.14)	-0.11 (0.17)
2014	0.44** (0.14)	-0.06 (0.17)
2015	0.45** (0.14)	-0.10 (0.18)



Table 5a: Log of Median Debt at Separation (Middle Income)

	(1)	(2)	(3)	(4)	(5)
Coordination	0.01 (0.07)	-0.02 (0.06)			
Post	0.00 (0.09)	-0.01 (0.07)	-0.02 (0.05)	-0.05 (0.09)	
Post*Coordination	-0.16 (0.10)	-0.16* (0.09)	-0.14** (0.06)	-0.05 (0.11)	
Investment Revenues per Student ( <i>millions</i> )		-2.08*** (0.45)	-0.52 (0.36)	-0.48 (0.35)	-0.56 (0.57)
Log of Inst. Support Spending per Student		0.08** (0.03)	0.09** (0.03)	0.08** (0.03)	0.09** (0.04)
Log of Net Tuition Revenue per Student		2.46* (1.37)	1.56 (1.30)	1.71 (1.41)	1.90 (1.63)
Log of Instructional Spending per Student		-0.02 (0.10)	0.02 (0.04)	0.03 (0.04)	0.02 (0.04)
Log of Students on Aid		0.08** (0.03)	-0.38** (0.17)	-0.43*** (0.16)	-0.42** (0.18)
Log of Revenue Share from Government		0.00 (0.04)	0.00 (0.05)	0.04 (0.05)	0.04 (0.06)
Time				-0.03 (0.03)	
Time*Coordination				0.03 (0.03)	
Time*Post				0.07** (0.03)	
Time*Post*Coordination				-0.07** (0.03)	
Constant	9.68*** (0.06)	3.09 (2.49)			
School FE	No	No	Yes	Yes	Yes
Coordination-Year Interaction	No	No	No	No	Yes
Observations	275	270	270	270	270
R-squared	0.034	0.326	1.000	1.000	1.000

Note: See Table 5b for Coordination-Year Interaction coefficients from Column 5.

Table 5b: Coordination-Year Interaction Coefficients

	Year FE	Coord. Interaction
2001	0.08 (0.20)	-0.03 (0.28)
2002	0.02 (0.18)	0.00 (0.24)
2003	0.01 (0.18)	0.06 (0.23)
2004	-0.11 (0.21)	0.16 (0.25)
2005	-0.14 (0.22)	0.14 (0.27)
2006	-0.12 (0.20)	0.08 (0.25)
2007	-0.16 (0.22)	0.09 (0.26)
2008	-0.21 (0.18)	0.13 (0.24)
2009	-0.23 (0.17)	0.02 (0.24)
2010	-0.12 (0.20)	-0.06 (0.24)
2011	-0.02 (0.21)	-0.04 (0.24)
2012	0.00 (0.19)	-0.10 (0.25)
2013	0.00 (0.20)	-0.18 (0.25)
2014	0.05 (0.22)	-0.20 (0.27)
2015	0.09 (0.22)	-0.24 (0.28)

Table 6a: Log of Median Debt at Separation (Low Income)

	(1)	(2)	(3)	(4)	(5)
Coordination	0.02 (0.07)	-0.03 (0.07)			
Post	-0.08 (0.08)	-0.10 (0.07)	-0.08 (0.05)	0.11 (0.07)	
Post*Coordination	-0.27** (0.10)	-0.27*** (0.10)	-0.26*** (0.06)	-0.13 (0.11)	
Investment Revenues per Student ( <i>millions</i> )		-1.59*** (0.42)	-0.10 (0.36)	0.10 (0.30)	-0.35 (0.40)
Log of Inst. Support Spending per Student		0.06 (0.04)	0.09* (0.05)	0.09* (0.05)	0.10** (0.05)
Log of Net Tuition Revenue per Student		3.27** (1.53)	1.09 (1.41)	2.46** (1.23)	2.66* (1.42)
Log of Instructional Spending per Student		-0.06 (0.12)	-0.00 (0.05)	0.03 (0.04)	0.03 (0.04)
Log of Students on Aid		0.08** (0.04)	-0.50** (0.19)	-0.40** (0.17)	-0.44** (0.17)
Log of Revenue Share from Government		0.00 (0.04)	-0.11* (0.06)	-0.02 (0.06)	0.01 (0.06)
Time				-0.09*** (0.02)	
Time*Coordination				0.04* (0.02)	
Time*Post				0.12*** (0.02)	
Time*Post*Coordination				-0.10*** (0.03)	
Constant	9.63*** (0.06)	1.78 (2.68)			
School FE	No	No	Yes	Yes	Yes
Coordination-Year Interaction	No	No	No	No	Yes
Observations	275	270	270	270	270
R-squared	0.109	0.285	0.999	0.999	0.999

Note: See Table 6b for Coordination-Year Interaction coefficients from Column 5.

Table 6b: Coordination-Year Interaction Coefficients

	Year FE	Coord. Interaction
2001	-0.20 (0.11)	-0.05 (0.17)
2002	-0.33** (0.10)	-0.07 (0.15)
2003	-0.41*** (0.11)	0.10 (0.17)
2004	-0.53*** (0.12)	0.19 (0.16)
2005	-0.51*** (0.12)	0.13 (0.16)
2006	-0.54*** (0.12)	0.12 (0.17)
2007	-0.57*** (0.15)	0.07 (0.19)
2008	-0.60*** (0.11)	0.14 (0.16)
2009	-0.59*** (0.10)	-0.07 (0.14)
2010	-0.48*** (0.12)	-0.19 (0.16)
2011	-0.42*** (0.12)	-0.26 (0.18)
2012	-0.49*** (0.10)	-0.19 (0.16)
2013	-0.50*** (0.12)	-0.27 (0.18)
2014	-0.35** (0.12)	-0.37* (0.18)
2015	-0.29* (0.13)	-0.40* (0.19)

Table 7a: Log of Net Tuition Revenue per Student

	(1)	(2)	(3)	(4)	(5)
Coordination	0.01*** (0.00)	0.02*** (0.00)			
Post	0.01 (0.00)	0.01* (0.00)	0.01*** (0.00)	-0.01 (0.01)	
Post*Coordination	0.00 (0.01)	-0.00 (0.00)	-0.00 (0.00)	0.01 (0.01)	
Investment Revenues per Student ( <i>millions</i> )		-0.05** (0.02)	-0.01 (0.01)	-0.01 (0.01)	-0.01 (0.02)
Log of Inst. Support Spending per Student		-0.00 (0.00)	0.00 (0.00)	-0.00 (0.00)	-0.00 (0.00)
Log of Instructional Spending per Student		0.01* (0.00)	0.01 (0.01)	0.00 (0.00)	0.00 (0.00)
Log of Students on Aid		0.01*** (0.00)	-0.02** (0.01)	-0.02*** (0.01)	-0.03*** (0.01)
Log of Revenue Share from Government		-0.01*** (0.00)	-0.00* (0.00)	-0.01** (0.00)	-0.01*** (0.00)
Time				0.00** (0.00)	
Time*Coordination				-0.00 (0.00)	
Time*Post				-0.00 (0.00)	
Time*Post*Coordination				0.00 (0.00)	
Constant	2.29*** (0.00)	2.16*** (0.04)			
School FE	No	No	Yes	Yes	Yes
Coordination-Year Interaction	No	No	No	No	Yes
Observations	287	283	283	283	283
R-squared	0.117	0.315	1.000	1.000	1.000

Note: See Table 7b for Coordination-Year Interaction coefficients from Column 5.

Table 7b: Coordination-Year Interaction Coefficients

	Year FE	Coord. Interaction
2001	-0.00 (0.01)	-0.00 (0.01)
2002	0.00 (0.01)	0.01 (0.01)
2003	0.01 (0.01)	0.00 (0.01)
2004	0.02* (0.01)	-0.02 (0.01)
2005	0.01 (0.01)	-0.01 (0.01)
2006	0.01 (0.01)	-0.00 (0.01)
2007	0.02* (0.01)	-0.01 (0.01)
2008	0.01 (0.01)	-0.00 (0.01)
2009	0.01 (0.01)	0.00 (0.01)
2010	0.02** (0.01)	-0.01 (0.01)
2011	0.02* (0.01)	-0.00 (0.01)
2012	0.02* (0.01)	-0.01 (0.01)
2013	0.02* (0.01)	-0.00 (0.01)
2014	0.02** (0.01)	-0.01 (0.01)
2015	0.02** (0.01)	-0.01 (0.01)

Table 8a: Log of Total Pell Grant Outlays

	(1)	(2)	(3)	(4)	(5)
Coordination	-0.62*** (0.17)	-0.26*** (0.07)			
Post	0.46** (0.18)	0.29*** (0.08)	0.30*** (0.05)	-0.00 (0.08)	
Post*Coordination	0.02 (0.23)	0.02 (0.10)	0.08 (0.06)	0.05 (0.09)	
Investment Revenues per Student ( <i>millions</i> )		0.04 (0.24)	0.31* (0.17)	0.12 (0.10)	0.01 (0.11)
Log of Inst. Support Spending per Student		0.01 (0.03)	0.02 (0.02)	-0.02 (0.02)	-0.01 (0.01)
Log of Net Tuition Revenue per Student		5.89*** (1.07)	0.68 (0.88)	-1.04 (0.84)	-2.00*** (0.76)
Log of Instructional Spending per Student		-0.07 (0.05)	0.08** (0.03)	0.05* (0.03)	0.02 (0.03)
Log of Students on Aid		0.88*** (0.04)	0.42*** (0.14)	0.18* (0.10)	0.12 (0.07)
Log of Revenue Share from Government		0.04 (0.03)	0.18*** (0.05)	0.21*** (0.04)	0.10*** (0.04)
Time				0.01 (0.02)	
Time*Coordination				-0.01 (0.02)	
Time*Post				0.06*** (0.02)	
Time*Post*Coordination				0.02 (0.02)	
Constant	14.51*** (0.13)	-4.06* (2.39)			
School FE	No	No	Yes	Yes	Yes
Coordination-Year Interaction	No	No	No	No	Yes
Observations	275	270	270	270	270
R-squared	0.147	0.874	1.000	1.000	1.000

Note: See Table 8b for Coordination-Year Interaction coefficients from Column 5.

Table 8b: Coordination-Year Interaction Coefficients

	Year FE	Coord. Interaction
2001	0.10 (0.11)	-0.15 (0.13)
2002	0.27** (0.10)	-0.18 (0.12)
2003	0.26** (0.09)	-0.16 (0.11)
2004	0.32** (0.10)	-0.18 (0.12)
2005	0.25* (0.10)	-0.15 (0.11)
2006	0.10 (0.10)	-0.13 (0.12)
2007	0.16 (0.10)	-0.17 (0.12)
2008	0.28** (0.09)	-0.17 (0.11)
2009	0.36*** (0.10)	-0.16 (0.11)
2010	0.67*** (0.09)	-0.07 (0.11)
2011	0.74*** (0.10)	-0.04 (0.12)
2012	0.70*** (0.11)	-0.02 (0.12)
2013	0.67*** (0.12)	-0.02 (0.14)
2014	0.76*** (0.13)	-0.07 (0.15)
2015	0.79*** (0.12)	-0.12 (0.15)



Table 9a: Log of Total Institutional Grant Aid

	(1)	(2)	(3)	(4)	(5)
Coordination	-0.71*** (0.17)	0.03 (0.05)			
Post	0.45*** (0.14)	0.32*** (0.06)	0.39*** (0.03)	-0.03 (0.03)	
Post*Coordination	0.01 (0.23)	0.02 (0.06)	0.01 (0.04)	0.04 (0.04)	
Investment Revenues per Student ( <i>millions</i> )		0.72** (0.28)	0.09 (0.10)	-0.10* (0.05)	-0.17** (0.07)
Log of Inst. Support Spending per Student		0.07 (0.06)	0.02* (0.01)	-0.00 (0.01)	-0.01 (0.01)
Log of Net Tuition Revenue per Student		0.14 (1.08)	1.88*** (0.64)	-0.24 (0.41)	-0.46 (0.44)
Log of Instructional Spending per Student		0.18** (0.09)	0.05** (0.02)	0.02 (0.01)	0.02 (0.01)
Log of Students on Aid		0.91*** (0.03)	0.46*** (0.09)	0.15*** (0.05)	0.14*** (0.05)
Log of Revenue Share from Government		0.22*** (0.03)	0.09** (0.04)	0.06** (0.02)	0.04* (0.02)
Time				0.06*** (0.01)	
Time*Coordination				-0.00 (0.01)	
Time*Post				0.00 (0.01)	
Time*Post*Coordination				-0.00 (0.01)	
Constant	18.29*** (0.10)	9.61*** (2.06)			
School FE	No	No	Yes	Yes	Yes
Coordination-Year Interaction	No	No	No	No	Yes
Observations	288	283	283	283	283
R-squared	0.140	0.947	1.000	1.000	1.000

Note: See Table 9b for Coordination-Year Interaction coefficients from Column 5.

Table 9b: Coordination-Year Interaction Coefficients

	Year FE	Coord. Interaction
2001	0.02 (0.07)	-0.03 (0.09)
2002	0.13* (0.06)	-0.05 (0.08)
2003	0.15* (0.06)	-0.01 (0.08)
2004	0.24*** (0.06)	-0.02 (0.08)
2005	0.30*** (0.06)	-0.04 (0.08)
2006	0.32*** (0.06)	-0.03 (0.08)
2007	0.36*** (0.06)	-0.01 (0.08)
2008	0.40*** (0.07)	-0.04 (0.08)
2009	0.46*** (0.06)	0.02 (0.08)
2010	0.57*** (0.06)	-0.01 (0.07)
2011	0.63*** (0.06)	0.01 (0.08)
2012	0.68*** (0.06)	-0.01 (0.07)
2013	0.72*** (0.06)	-0.01 (0.08)
2014	0.77*** (0.07)	-0.02 (0.08)
2015	0.84*** (0.07)	-0.06 (0.09)