Investing in the Workforce: The Impact of Performance-Based Funding on Student Earnings Outcomes[1]

Robert Kelchen
Professor, Department of Educational Leadership and Policy Studies
University of Tennessee, Knoxville

Justin Ortagus
Associate Professor of Higher Education Administration and Policy
University of Florida

Kelly Rosinger
Associate Professor, Department of Education Policy Studies and School of Public Policy (by courtesy)
Pennsylvania State University

Alex Cassell
PhD Student
Pennsylvania State University

Accepted version—January 2023

The Version of Record of this manuscript has been published on February 10, 2023 and is available in *The Journal of Higher Education.* https://doi.org/10.1080/00221546.2023.2171201.

Abstract: A growing number of states use performance-based funding (PBF) systems to tie appropriations to student outcomes. Yet while many studies have examined the effects of PBF on enrollment and completion outcomes, no research has considered whether PBF affects post-college outcomes. This is of particular importance as more states directly incentivize colleges to improve student earnings and have students major in high-demand fields. We used the first detailed longitudinal dataset of state PBF policies to examine the effects of PBF on student earnings and found some modest positive effects concentrated among students who were already enrolled in college when PBF was first implemented. However, these small increases could also reflect other trends occurring prior to the implementation of PBF and analyses show this increase was not sustained among later cohorts.

[1] Our research design is preregistered through the Open Science Framework at https://osf.io/5ehdg. We are grateful to Arnold Ventures, the Joyce Foundation, and the William T. Grant Foundation for their financial support of this research. We are grateful for excellent research assistance from Lynneah Brown, Karly Caples, Junghee Choi, Garam Chu, Sam Riggs, Yahya Shamekhi, and Nicholas Voorhees. We appreciate the feedback and suggestions during data collection and analysis from our advisory board members: Alisa Hicklin Fryar, Tiffany Jones, and David Tandberg.

Many students attend college to get a well-paying job upon completion. In 2019, 84% of first-time, full-time students at four-year universities indicated that getting a better job was a very important factor in deciding to attend college. Seventy-three percent of students, and 88% of students attending Historically Black Colleges and Universities, responded that making more money was a very important factor in attending (Stolzenberg et al., 2020). As total outstanding student debt passes $1.5 trillion (Federal Reserve Bank of New York, 2021), students and their families will keep emphasizing labor market outcomes in choosing colleges.

Dating to 1862's Morrill Act, fostering economic growth has been key to the missions of public higher education institutions (Liu, 2015). This has traditionally led to strong support for public higher education, but there has been growing skepticism of the value proposition recently due to concerns about the return on investment and state taxpayer subsidies going to programs considered less economically valuable (Kelchen, 2018b). Perceptions of higher education became less favorable during the 2010s, with a growing partisan divide that could jeopardize future state funding (Cantwell & Taylor, 2020).

States responded to these concerns by requiring public colleges to meet student success metrics in order to receive funding. These performance-based funding (PBF) systems have been adopted by states across the political spectrum to try to hold colleges accountable for their outcomes (Kelchen, 2018b). Thirty-two states used PBF to allocate at least some funding to public higher education in 2020, and 41 states have done so since 1997 (Rosinger et al., 2022).

Considerable research has examined the effects of PBF on student success (see Ortagus et al. (2020) for a review). These studies have generally found null or modest positive effects of PBF on outcomes such as retention and degree completions along with unintended outcomes such as increased selectivity and reduced diversity. States responded by adopting equity-oriented

metrics that incentivize colleges to successfully serve students from historically underrepresented communities such as minoritized students and Pell Grant recipients.

A growing number of states have added PBF components based on students' labor market outcomes, such as alumni earnings or the number of graduates in STEM, health, and other high-priority fields (Rosinger et al., 2022). In 2020, Wisconsin public universities had a workforce incentive to graduate students in STEM and health majors, while technical colleges were funded partially based on job placement rates and the number of graduates in high-demand fields like health care, accounting, and truck driving. Texas primarily funded its technical colleges on a returned value formula based on former students' wages and contributions to the tax base.

Table 1 shows that 17 of the 22 states with funded PBF systems in the four-year sector and 20 of the 29 funded two-year PBF systems in 2020 had workforce PBF metrics (data from Ortagus et al., 2021). These metrics are nearly as common as equity provisions (20 states in the four-year sector and 22 states in the two-year sector) and proliferated in the early 2010s during a wave of PBF adoption. But some states operated workforce metrics during the 1990s and 2000s (Serban & Burke, 1998), which allows us to analyze the long-term effects of PBF and workforce metrics on labor market outcomes.

[Table 1]

There is currently no research on the effects of PBF on the earnings of former students, and we hypothesize differential effects between four-year and two-year institutions based on prior research. Although the effects of PBF on completion have been modest across sectors, research has found an increase in selectivity and a shift to higher-paying STEM degrees in the four-year sector (Li, 2020; Ortagus et al., 2020). In the two-year sector, PBF has induced a shift

to shorter-term certificates that often have lower labor market returns (Hillman et al., 2015, 2018; Li & Kennedy, 2018; Li & Ortagus, 2019). Additionally, the presence of economic returns to some college but no degree (Webber, 2016) makes it possible that PBF could have some effects on the returns of students who did not complete a credential. As such, we expect PBF to produce modest increases in earnings at four-year institutions and modest decreases at two-year institutions.

There is also the likelihood of differential effects of PBF based on student characteristics, although the direction of the effects is unclear. Research shows PBF systems have the potential to widen gaps between traditionally advantaged and disadvantaged student groups. If colleges face incentives to increase graduates' earnings, they may focus on certain subgroups of students who are already well-served by higher education. Alternatively, institutions could focus on outcomes of students from lower-income families to both close equity gaps and receive more state funding. Additionally, PBF policy design features, including whether states explicitly incentivize workforce or equity metrics, likely shape the effectiveness of these policies in improving earnings outcomes and reducing equity gaps.

This paper leverages the first comprehensive longitudinal dataset of state PBF policy details, such as the percent of funds tied to student outcomes and the existence of equity provisions or workforce metrics, to examine the impact of PBF on student earnings outcomes. Our research questions are the following:

1. To what extent does the presence of a funded PBF policy affect student earnings outcomes?
2. To what extent do variations in state commitments to PBF affect student earnings outcomes?

3. To what extent does the presence of workforce or equity metrics in state PBF policies affect student earnings outcomes?
4. Do the effects of PBF policy design vary between all students and historically underrepresented groups of students (independent students and students from low-income families)?

Theoretical Framework and Literature Review

We draw on agency theory to examine the impact of PBF on students' labor market outcomes. Agency theory describes the incentive-based relationship between the principal, such as a state government, and their agents, such as public colleges and universities, in which the principal provides financial resources to the agent for producing desired outcomes (Jensen & Meckling, 1976). Under PBF, state governments incentivize colleges to focus on improving particular outcomes, typically students' progression toward a degree or degree completions (Ortagus et al., 2020). PBF policies reflect an effort to align institutional behavior and activities with state priorities, often relating to workforce and economic development goals (Kelchen, 2018b).

Performance funding is an increasingly popular accountability strategy across public domains, but it has often failed to achieve desired outcomes and frequently generates unintended consequences. Public administration scholars have noted that reform efforts often fail due to limited institutional capacity and ability to redirect resources to improve on specific metrics, lack of clarity on metrics, and the extent to which the institution/agent has control over the outcomes on which they are evaluated (Andrews & Moynihan, 2002; Thompson, 1999). The issue of institutional capacity has been shown as a particular issue in the performance funding literature, as colleges often do not have the data systems or people in place to make major changes.

Additionally, colleges also have the ability to push back against policies that they do not feel meet their mission (e.g., Dougherty et al., 2016). Thus, we anticipate we may find limited effects of PBF on student earnings outcomes.

Prior literature on PBF often focuses on the impact of PBF adoption on institutional outcomes that are incentivized by the formulas, such as retention or degree completion (Ortagus et al., 2020) and frequently finds little to modest positive effects of PBF on these outcomes. Early descriptive work typically found no relationship between PBF policies and student outcomes (e.g., Shin & Milton, 2004). Rabovsky (2012) leveraged national data and found limited or negative changes in retention rates, graduation rates, and bachelor's degrees produced after states implemented a PBF system. Additional descriptive studies have shown that PBF policies were not associated with retention or graduation rates (e.g., Favero & Rutherford, 2019).

Numerous quasi-experimental studies using difference-in-differences techniques have examined the impact of PBF adoption on the intended outcomes of retention and degree completion. Previous literature has shown that PBF adoption was unrelated to associate degree production at community colleges in Ohio (Hillman et al., 2018), Washington (Hillman et al., 2015), and Tennessee (Hillman et al., 2018; Li & Ortagus, 2019). Prior work has also focused on the impact of PBF policies nationally at public four-year universities, finding that PBF adoption was unrelated to bachelor's degree production (Hillman et al., 2014). Additional quasi-experimental work has explored the impact of PBF adoption at both two- and four-year institutions. Ward and Ost (2021) studied the effects of PBF adoption in Ohio and Tennessee and reported that PBF had no impact on total degree completions, first-to-second year retention, and

six-year graduation rates. Another recent study in those states found that PBF led to fewer associate degrees but no change in bachelor's degree production (Hillman et al., 2018).

While the overall effect of PBF on earnings may be limited, there is reason to think that other unintended institutional responses to PBF—in particular, increased selectivity and decreased access at four-year institutions and a focus on shorter-term credentials in the two-year sector—could indicate small positive or negative effects on earnings, respectively. A growing body of literature on PBF policies has explored the potential for unintended impacts of PBF adoption among underserved students. Previous work has shown that community colleges may be gaming the PBF system by increasing their production of short-term certificates at the expense of associate degree production (Hillman et al., 2015, 2018; Li & Kennedy, 2018; Li & Ortagus, 2019). Given institutional responses that may move students into shorter-term certificate programs rather than associate degree pathways, we anticipate we may see a decrease in earnings at community colleges.

Responses at four-year institutions could similarly have implications for earnings. In a quasi-experimental study focused on Indiana public universities, Umbricht et al. (2017) reported that PBF adoption limited access to higher education for racially minoritized and low-income students. Additional work examining Indiana public universities using the same approach showed that PBF led to decreases in admission rates and underrepresented student enrollment (Birdsall, 2018). A recent national study affirms prior work suggesting that PBF policies can restrict access to higher education for underserved students, as the authors found that PBF-participating institutions enroll more students with higher standardized test scores yet enroll fewer first-generation students (Gándara & Rutherford, 2020). However, some research has found that equity provisions in PBF policies had a positive impact on Black student enrollment

(Kelchen, 2018a) as well as Hispanic and low-income student enrollment (Gándara & Rutherford, 2018). If four-year institutions respond to PBF by becoming more selective, earnings could increase since institutional selectivity has been linked to higher earnings in some prior research ().

Some particular design features of PBF systems, such as the share of funds at stake under PBF policies, may also yield positive impacts. Prior reform efforts are frequently abandoned or are not viewed as serious efforts to improve service delivery (e.g., Thompson, 1999). Similar to other reforms, PBF has undergone periods of discontinuations and instability, with states adopting, abandoning, and readopting PBF (Rosinger et al., 2022). We examine how the share of funds tied to performance shapes student outcomes, anticipating that higher-dosage policies may be seen by institutions as more serious reform efforts. As a result, campus leaders may have a greater incentive to make changes that improve student outcomes.

State-supported higher education institutions are constantly competing for resources with other state priorities (Weerts & Ronca, 2012). States vary considerably in their levels of funding for public higher education and the methods they use to allocate funding to students and colleges (Laderman & Weeden, 2020; Lingo et al., 2021). Importantly, state appropriations are linked to higher rates of postsecondary enrollment and completion (Cummings et al., 2021). Justifications for states funding higher education are also connected with broader goals of maximizing public and private returns on investment, such as graduate earnings (Toutkoushian & Paulsen, 2016).

Although appropriations vary widely across states, state funding for higher education is increasingly viewed as a method to boost economic prospects for the state through workforce development. For example, workforce linkages are incentivized through state funding policies, such as some statewide free college programs that focus on students in high-demand fields

(Rosinger et al., 2021). Another example is the rise of merit aid programs across numerous states (Baker et al, 2020), which are often used as a tool to prevent brain drain and keep college graduates in state to contribute economically (Zhang & Ness, 2010).

While PBF has traditionally focused on degree completion outcomes, states have increasingly incorporated workforce outcomes, such as student earnings, job placement rates, or degree completion in specific fields deemed high-demand or high-value, as funding metrics to more explicitly link to state economic development goals (Rosinger et al., 2022). As a result, public institutions that are subject to PBF policies that include workforce metrics may be more likely to organize institutional behavior and activities toward improving students' labor market outcomes. For example, institutions may direct more resources toward career services or establish closer links with local and state industries in order to position graduates to better compete for jobs.

Incentives to prioritize degree production in high-demand fields, which frequently include STEM and health fields, may result in higher earnings since STEM graduates tend to earn more than graduates in other fields on average (Melguizo & Wolniak, 2012). As a result, we anticipate PBF systems with targeted workforce metrics may have a positive impact on student earnings outcomes, especially as Li (2020) found that introducing targeted STEM incentives in PBF formulas increased the total number and relative share of STEM bachelor's degrees among PBF-adopting institutions.

Students' labor market outcomes present an interesting empirical test of the effects of PBF, as until recently, these specific outcomes were not explicitly incentivized in most PBF systems. Yet PBF policies that focus on earnings may further exacerbate longstanding gaps in post-college income by race, family income, and parental education due to labor market

discrimination that would be outside of a college's control (Gaddis, 2015). To date, we have little evidence regarding how PBF policies and their design features shape student earnings outcomes.

Sample, Data, and Methods

To answer our research questions, we combined the first comprehensive longitudinal dataset of state performance funding policy details with data from federal sources on student post-college outcomes and institutional characteristics. We explain our sample, data, and methods below.

Sample

The analytic sample for this paper consisted of degree-granting two-year and four-year public colleges and universities with available data between Fiscal Years 1997 and 2009 (to align with the availability of earnings data). We classified colleges into two-year and four-year institutions based on 2018 Carnegie basic classifications, using earlier classifications if data were missing. Colleges with basic code 23 (baccalaureate/associate institutions) were coded as two-year institutions because they primarily offered associate degrees. We excluded special-focus institutions, graduate-only universities, and military academies. This resulted in 552 four-year and 1,091 two-year colleges, with not all colleges being observed in all years due to mergers, consolidations, and missing data.

Data

Our data come from the InformEd States project's four-year initiative to create the first detailed longitudinal dataset of state PBF policy details (Ortagus et al., 2021). For details on the data collection protocol, see Kelchen et al. (2019). The dataset includes information on whether a state approved a PBF policy for a system or sector in a given year, whether the policy was actually funded, and the percent of the state's general fund budget for higher education tied to

student outcomes. The dataset also includes information on whether there were equity incentives for particular groups of students (such as low-income and racially minoritized) and for incentives to encourage more students to complete credentials in STEM, health, and other high-demand fields. Table 2 contains descriptive statistics for the dataset for two-year and four-year colleges, with separate columns for colleges that were ever subject to a funded PBF system during the period of our panel (Fiscal Years 1997 through 2009) and those that never had funds tied to student outcomes.

[Table 2]

Our first treatment variable was a binary variable indicating whether an institution was subject to a funded PBF system in a given year. To examine the extent to which the share of funds tied to performance shaped student earnings outcomes, our second treatment variable was a continuous variable indicating the dosage of the PBF policy—the percent of state general fund appropriations tied to performance—in a sector within that year. When some colleges within a sector were subject to PBF and others were not, we assigned those two groups different percentages. For example, when the Pennsylvania State System of Higher Education (PASSHE) universities operated under PBF and other public universities in the state did not, the PASSHE universities were all coded as having funding tied to outcomes and non-PASSHE universities were coded as having no funds tied to outcomes.

Our final set of treatment variables consisted of indicators providing details on specific PBF incentives. The first variable was whether a state funded an explicit workforce metric, such as employment metrics, whether students worked in high-demand fields (primarily highly-paid STEM and health fields), and the earnings of former students. Most workforce metrics explicitly mentioned employment rates or high-demand fields, while some (particularly in the two-year

sector) explicitly mentioned earnings. In other cases, language was vague around workforce alignment without providing details. The second variable was whether a state had a funded equity premium in a given year across racially minoritized students, students from low-income families, adult students, and academically underprepared students. Due to the lack of clarity in state documents and several states allowing colleges to choose which metrics were included in PBF, we were unable to examine the dosage of workforce or equity metrics (see Rosinger et al., 2022 for a detailed overview of PBF design).

The outcomes of interest were the earnings of students who received federal financial aid six and eight years after entering college, including both completers and non-completers. This excluded students who were still enrolled at the time of measurement or were not observed as having any earnings. Six-year earnings data were available for cohorts starting college between 1997 and 2009, with earnings being measured between calendar years 2003 and 2015. This included mean, median, and the 25th and 75th percentiles of earnings as well as subgroup means by family income tercile (less than $30,000 per year, $30,000-$75,000 per year, and more than $75,000 per year). Eight-year earnings data were available for the 1997 to 2007 cohorts and measured students between calendar years 2005 and 2015. Only mean, median, and the 25th and 75th percentiles of earnings were available.

The College Scorecard provides two-year pooled cohorts for student earnings outcomes. In early years of the dataset, each cohort appears in only one observation. For six-year and eight-year earnings, the 1997/98, 1999/2000, 2001/02, and 2003/04 cohorts all do not overlap; in these cases, we used the same value for both cohorts (such as 2001 and 2002). Cohorts overlap in later years of the dataset; this affects 2005/06, 2006/07, 2007/08 (six-year earnings only), and 2008/09 (six-year earnings only). To estimate the 2006 cohort, we averaged the 2005/06 and 2006/07

values. As a robustness check, we also estimated models based on the first year in which the cohort was observed. For this, we assigned the 2006 cohort only to the 2006/07 observation. The results are essentially identical and are not presented here. We then adjusted all of these values into 2020 dollars using the Consumer Price Index. The number of observations differed somewhat across six-year and eight-year earnings due to data availability in the College Scorecard.

We included institutional characteristics that likely shape characteristics of the student body and post-college outcomes independent of PBF policies. We included institutional pricing, financial resources, and size measures that could affect how institutions respond to PBF incentives and could otherwise confound our estimates of the impact of PBF. These variables were in-state tuition and fees (logged), average amount of grant aid per student (logged), percent of students receiving aid, state appropriations per student (logged), local appropriations per student (logged), instructional expenditures per student (logged), and full-time equivalent student enrollment (logged). We adjusted all financial variables into 2020 dollars using the Consumer Price Index. We also included demographic characteristics and economic conditions of states that could shape college enrollment patterns and labor market outcomes. These measures included per-capita income (logged), unemployment rate, and population size by race and ethnicity (logged).

Methods

To estimate the effect of PBF policies on (logged) student earnings outcomes, we used two different methods for each sector with the covariates described above. The results are generally robust to models excluding covariates, although we do not present them here for the sake of brevity. Our first method is a generalized difference-in-differences (DiD) framework

with two-way fixed effects (TWFE) that allows for the treatment to take place in different time periods in different states. Notably, DiD supports continuous and other nonbinary treatment variables, which are not yet supported in event study analyses. Similar to other studies using national treatment groups (Gándara & Rutherford, 2020; Hagood, 2019), we did not construct a weighted comparison group. This is appropriate because states had already begun to adopt PBF by the beginning of the panel and research finds relatively weak links between state characteristics and PBF adoption (Li, 2017). The number of states adopting PBF at some point during the panel also makes justifying the exclusion of certain states as controls difficult. We ran each DiD model for the cohort entering college when PBF policy features were measured, as well as one year before and one year after to check the robustness of our results. However, we only present the primary results because the results were very similar.

The traditional TWFE model relies on compiling weighted average treatment effects across each fixed effect unit (here, each state and year). While this has been standard practice in research studies over the past decade, a recent body of econometrics research has critiqued TWFE for allowing certain observations to have negative treatment weights and for how staggered adoptions across units are treated (e.g., Goodman-Bacon, 2021). We responded to this literature by implementing a new two-stage estimation technique from Gardner (2021) that first uses untreated observations to identify state and year fixed effects and then estimates treatment effects after excluding state and year fixed effects in the second stage.

We began by estimating the effects of a binary variable for whether a PBF system was funded. This is similar to the binary variables in prior literature, although most of this research does not indicate whether approved but unfunded PBF systems are included. We excluded states that had PBF on the books but did not fund it in a given year from the treatment group, instead

placing them in the comparison group. This could be a conservative estimate of the effects of PBF if colleges responded to unfunded systems. For these estimates, we used both traditional TWFE and Gardner (2021) techniques to compare results across both methods before moving to the Gardner technique for most other estimates.

One notable exception is when using a continuous measure of the percentage of state funding tied to performance to examine the effects of dosage. We had to rely on traditional TWFE estimates because the did2s Stata code (Butts, 2022) to implement the Gardner technique only supports balanced panels. Because our panel is highly unbalanced (nearly half of all observations are missing for at least one year due to administrative consolidations or missing data on an outcome or covariate), we used TWFE and consider that analysis to be less definitive than others. We present continuous dosage estimates in Appendix 1 and instead focused on models using terciles of funding at stake between 1997 and 2009 that supported the Gardner estimate. For four-year universities, the bottom tercile was at or below 0.97%, the middle tercile was at or below 4.19%, and the top tercile was above 4.20%. The tercile thresholds were lower among two-year colleges (1.02% and 2.00%). The omitted category was colleges in states with no funded PBF.

Our next two analyses examined the effects of PBF based on policy characteristics, with the omitted category being colleges without funded PBF. In the first analysis, we included variables for whether a college operated under a PBF policy with workforce metrics and whether there was a funded PBF policy without workforce metrics. We then conducted a similar analysis for the presence of equity metrics. Because colleges are occasionally allowed to choose some metrics from a list of options and a lack of data in many states on the share of funding tied to each metric, we could not examine the share of state funding tied directly to workforce or equity

metrics. In each model, we used state-clustered standard errors (Cameron & Miller, 2015). This is a more conservative specification than clustering by OPEID or UnitID. Because we conducted numerous regressions, we used $p<.01$ as the statistical significance threshold instead of a formal Bonferroni correction.

We followed recommendations by Furquim et al. (2020) and recent advances in the econometrics literature (e.g., Borusyak et al., 2021; Sun & Abraham, 2020) by conducting event studies to test whether DiD results held across methods that account for pre-treatment years. As the literature has not congealed around a single event study strategy, we used different techniques regarding pre-treatment observations (Roth et al., 2022). We featured the *did_imputation* technique that imputes observations to estimate treatment effects (Borusyak et al., 2021) and the *eventstudyinteract* technique (Sun & Abraham, 2020) that estimates a weighted average treatment effect. We conducted event studies using the presence of a funded PBF system as the treatment, as these techniques do not yet allow for continuous treatment variables or multiple treatment types (such as PBF with and without workforce metrics). For ease of presentation, we placed the specifications on the same plot using the *eventplot* command (Borusyak, 2021). We used the same covariates as in the DiD models, also using state-clustered standard errors.

To satisfy pre-treatment trends requirements of event studies, we excluded states that already had funded PBF in 1997 (Arkansas, Florida, Kentucky, Missouri, and Tennessee for both sectors and Ohio two-year colleges). Our second specification used states that met the canonical event study requirement of maintaining the policy throughout the panel. This also excluded all Colorado, New Jersey, South Carolina, and Washington institutions, Michigan and South Dakota four-year universities, and Illinois two-year colleges. If a state kept PBF on the books but moved

between funded and unfunded systems during this time based on the state's budget situation in a given year (such as Kansas), we kept the state in the analysis. We showed the event plots for the canonical event study sample only because the results were similar to the no-1997 PBF policy sample.

Limitations

College Scorecard earnings data includes all students who have received federal financial aid. While there are advantages of this dataset, such as the data coming from administrative sources and including both dropouts and graduates, there are notable limitations. One is that only students who received federal financial aid are included in the data. The percentage of students covered in the dataset varies considerably across colleges, but between 60% and 70% of all students are included in both the public two-year and public four-year sectors (Council of Economic Advisers, 2015). Students excluded from the dataset likely come from two groups: students from higher-income families who did not need financial assistance and students from lower-income families who did not complete the Free Application for Federal Student Aid.

Earnings metrics will always be measured using a substantial lag following when students enter higher education. But an additional gap was created due to a decision during the Trump administration to focus solely on the earnings of graduates at the program level rather than including the earnings of all former students at the institutional level. As a result, there are no comparable six-year earnings data for two cohorts of students (2010 and 2011 enrollees for six-year earnings and 2008 and 2009 enrollees for eight-year earnings) before the data resumed with two new cohorts in the 2022 data release. Rather than interpolate for missing years, we excluded the newest cohorts. Although our analyses focus on earlier waves of PBF systems, some of the PBF systems in the 1990s and 2000s had similar metrics and dosages as systems of

the 2020s (Rosinger et al., 2022). In addition, several PBF systems incentivizing workforce outcomes, such as job placement or graduation from STEM or high-demand fields, do not explicitly measure students' earnings in the PBF formula. We rely on prior literature suggesting that both job placement and graduation from STEM or high-demand fields are positively related to students' earnings (e.g., Melguizo & Wolniak, 2012). Our results thus provide insights regarding the effects of current PBF program designs and incentives on students' earnings.

Another limitation of our analysis is that College Scorecard earning metrics are reported at the Federal Student Aid OPEID level instead of the IPEDS UnitID level. This means that some institutions' earnings outcomes are combined with other institutions due to the idiosyncratic ways that colleges entered into program participation agreements for federal financial aid with the U.S. Department of Education (Kelchen, 2019). In some cases, colleges within the same degree level (four-year and two-year) were combined into the same OPEID. This occurred with two-year systems in Louisiana and Alabama along with the Rutgers University and Texas A&M University systems in the four-year sector. In some cases, earnings data are combined across sectors, such as for the University of Connecticut and Pennsylvania State University systems. Altogether, 199 two-year colleges (at the UnitID level) and 91 four-year colleges were part of a broader OPEID. As a robustness check, we dropped these observations and the results (not shown for the sake of brevity) were essentially identical.

PBF programs may have affected the earnings of student cohorts who entered college prior to the implementation of PBF. Even though these cohorts likely did not see any changes on retention or completion, they could have still benefited from any changes that the college made in response to PBF. These could include enhancements to career services or job placement initiatives designed to improve student earnings. If this is the case, treatment effects may have

been muted somewhat as a result of prior cohorts being affected by some of the institutional responses to PBF. This also means that any observed effects from before the treated cohort could be actual treatment effects instead of being a violation of parallel trends assumptions.

Finally, in spite of growing attention paid to gaps in outcomes by race and ethnicity in state PBF systems and across higher education, we could not use earnings by race in our analysis. Since the College Scorecard is based on data for FAFSA filers and the FAFSA does not currently include a question on race or ethnicity, there is no way to ascertain students' racial backgrounds without a national student-level data system. This analysis could be possible by piecing together multiple states' data systems, but otherwise it is not feasible until a question on race/ethnicity is added to the FAFSA in 2023.

Results

In this section, we present the results of our difference-in-differences analyses followed by our event study analyses. Each table represents a separate set of regression specifications, with logged earnings (both overall and by student subgroup) being the outcome of interest.

Difference-in-differences results

We began by using a binary measure of whether a college was operating under a PBF system in a given year. Tables 4 (four-year universities) and 5 (two-year colleges) show binary results using both standard TWFE estimates and the Gardner technique across three samples: all institutions, excluding institutions subject to PBF in 1997 (the first year of the panel), and also excluding institutions that dropped PBF by 2019 to match up with the canonical event study sample.

As shown in Table 3 for four-year universities, the point estimates for the effects of PBF are generally positive and statistically insignificant. There are some positive and significant

effects on the full sample for six-year earnings using TWFE, but the coefficients are only around one percent. The coefficients are larger for the Gardner technique, but standard errors are also larger and the results are not statistically significant.

[Table 3]

Among two-year colleges (Table 4), there are some scattered positive and significant effects of PBF on earnings six years after college entry. These are most prominent in the restricted samples that exclude institutions that had PBF prior to the beginning of the panel in 1997 and among students from middle-income and higher-income families. In the Gardner models, the effects peak at about a 3.5 percent increase for these subsamples, even as median and mean earnings were unaffected. There were no overall or distributional effects on earnings eight years after college entry, but the College Scorecard did not publish earnings by family income for this time period.

[Table 4]

We next considered PBF dosage effects, with our primary focus being on dosage terciles (Table 5) so we could use the Gardner two-stage estimation technique. For four-year universities, there were no dosage effects, although the high dosage tercile was imprecisely estimated. For two-year colleges, there were also no consistent effects. In Appendix 1, we used standard TWFE regressions to examine a continuous measure of dosage. These results were precisely estimated and statistically insignificant.

[Table 5]

We then examined the effects of PBF policy design based on workforce or equity provisions. Table 6 shows the results of Gardner two-stage models with two separate treatment variables compared to the reference group of no funded PBF: PBF without workforce metrics

and PBF with workforce metrics. Among four-year universities, the coefficients were positive in PBF models without workforce provisions, although they were generally not statistically significant. In PBF models with workforce metrics, coefficients were around zero. In the two-year sector, the two PBF groups produced null effects on student earnings.

[Table 6]

The pattern of results was broadly similar when comparing the effects of PBF systems with and without equity metrics to colleges not operating under funded PBF systems (Table 7). The point estimates of PBF policies with no equity metrics were consistently positive, but the models were not statistically significant in either sector. This suggests that neither workforce nor equity provisions affect student earnings.

[Table 7]

Event study results

We also conducted event studies to examine the effects of the presence of funded PBF policies on the earnings of former students. The three panels of Figure 1 show the different outcomes for four-year students using two different event study techniques and restricting to the canonical event study sample (excluding colleges that were subject to PBF in 1997 or had their PBF system eliminated by 2019). Figure 2 shows the same analyses for the two-year sector.

The findings from the two event study models are generally well aligned with each other in the four-year sector (Figure 1). Both event studies show positive and significant effects for students who entered college as early as three to four years prior to the adoption of PBF. Normally, this would raise concerns regarding parallel trends. But higher earnings (generally of the magnitude of approximately two to three percent across subsamples and points in the earnings distribution) could be legitimate treatment effects for these cohorts. This is because a

student could enter a public university and then have PBF take effect during their third year of college. If colleges are suddenly incentivized to produce more graduates, then they could respond by helping students get as far as possible toward graduation. These students would thus be partially treated by PBF even if it was not present when they enrolled, and this helps to explain why DiD models would find more muted coefficients due to some legitimate treatment effects being differenced away. However, given that these findings may also reflect different trends in earnings prior to PBF adoption, we interpret this as suggestive evidence.

[Figure 1]

Among two-year colleges (Figure 2), a more muted version of the four-year effects played out. For most subsamples in the two-year sector, there are positive and statistically significant treatment effects of about one to three percent observed for a year or two prior to the first fully treated cohort and for the first fully treated cohort. After that, effects are generally indistinguishable from zero, although the Sun and Abraham (2020) estimates are more positive than the Boruysak et al. (2021) estimates. This also provides support for modest effects that are concentrated among students who were already enrolled in college while PBF was being implemented.

[Figure 2]

Discussion

Many states across the ideological spectrum are turning to performance-based funding systems in an effort to improve student outcomes and enhance the trust of legislators and the general public in higher education. Yet the large body of research on the effectiveness of the presence of PBF policies on student access, retention, and completion outcomes has shown at

most muted positive effects of PBF while also indicating several unintended consequences that raise equity concerns (Ortagus et al., 2020).

In this paper, we advance the body of knowledge on PBF in four main ways. First, this is the first research to use student earnings as an outcome in spite of some states' long-standing policies that incentivize colleges to graduate students in high-demand fields. Second, we used a new dataset that verified whether a college was actually subject to PBF in a given year instead of combining funded and unfunded systems. Third, we constructed an indicator for whether states tied funds to workforce or equity metrics in that year to see if targeted funding improved labor market outcomes. Finally, we conducted multiple event study models to see whether results hold across recently-developed econometric techniques.

One key finding is that the presence of a funded PBF policy may modestly improve the earnings outcomes of former students, with the most affected students being the ones who were already enrolled in college when PBF was first implemented and any positive effects fading out among later cohorts. This explains the differences between our DiD and event study results, as DiD results count pre-treatment years as having no treatment and that does not necessarily apply in this situation because colleges could have targeted students who were already enrolled in an effort to improve student outcomes. Some modest positive effects in the two-year sector may run somewhat counter to prior research finding that PBF encouraged community colleges to shift students from associate degree programs to certificate programs that were more lucrative to the college but with lower labor market payoffs for students (Hillman et al., 2015; Li & Kennedy, 2018). Either community colleges were able to improve career services programs for current students after PBF was implemented or any shifts to certificate programs did not negatively affect students' earnings.

While advocacy groups have consistently pushed for a larger share of funds to be tied to student outcomes (Miller & Morphew, 2017), our research on earlier state PBF systems did not find that increasing the amount of money at stake improved students' labor market outcomes. If anything, lower-stakes PBF systems generated better results than higher-stakes systems. This result highlights the potential importance of considering more nuanced dosage measures, which we used in this analysis.

We also examined whether student earnings varied based on the presence or absence of workforce or equity metrics. We generally found no evidence of these policy design characteristics affecting earnings outcomes. However, because it is impossible to parse out how much money is tied to equity in most states' PBF formulas, we cannot answer whether stronger equity-based formulas generate larger improvements in earnings. One key remaining question to consider is how much funding is necessary to encourage colleges to successfully serve low-income, minority, adult, and first-generation students, as some formulas with equity metrics do not provide enough bonus funds to truly incentivize colleges to serve historically underrepresented students (McKinney & Hagedorn, 2017).

One potential factor that could limit the ability of colleges to respond to PBF incentives and improve students' earnings outcomes is persistent labor market discrimination that results in lower wages and fewer employment opportunities for people of color (Fryer et al.,2013; Quillian et al., 2017, 2020). Prior research indicates that racially minoritized job candidates are less likely to be selected for interviews (Quillian et al., 2017), less likely to receive job offers (Quillian et al., 2020), and tend to be offered lower wages on average than their white peers (Fryer et al., 2013). Colleges have little control over discrimination in the labor market that may lead to lower wages among some students relative to others. As a result, impacts of efforts colleges make to

improve student earnings outcomes, such as investing in career services and building connections with local industry, may be muted due to labor market discrimination.

Consequently, colleges that serve larger numbers of students of color may be disadvantaged when it comes to being evaluated on metrics that inherently advantage some students over others. Data on student earnings outcomes disaggregated by race are not currently available through large-scale, publicly available federal postsecondary data sources; however, future analyses may consider the potentially disparate effects of PBF policies, particularly those that prioritize workforce outcomes, on the earnings of students of color and the often-underfunded institutions that enroll and graduate large numbers of students of color.

Our results raise several important questions for future research. The first is to examine labor market outcomes for the most recent wave of PBF systems. Until the U.S. Department of Education resumes publishing institution-level earnings data in the College Scorecard for multiple years, it is impossible for researchers to conduct a national analysis using the earnings of all former students. However, it is possible to focus on a subsample of states using state administrative data sources.

Qualitative research is also needed to understand any actions that colleges took in response to workforce-oriented PBF systems. While researchers have interviewed stakeholders in states with workforce provisions (e.g., Zerquera & Ziskin, 2020), these interviews have not focused on whether and how colleges react to having funding tied to student labor market outcomes. As these policies continue to proliferate, understanding institutional responses will become even more important.

Another important consideration is to better understand why states are adopting workforce-oriented PBF systems and the extent to which earnings are an impetus for doing so.

Research is particularly needed to understand whether states are concerned about the potential for increased inequality in society by using labor market outcomes as a funding mechanism. If future iterations of PBF show an increase in overall earnings (and state tax revenue) but a growing gap in earnings by race or family income, are state policymakers comfortable with that outcome? This is a key area to explore in future studies.

Finally, the recommendations for quantitative analyses of state policies are changing rapidly due to concerns raised about traditional DiD/TWFE models and a host of new event study models designed to at least partially accommodate treatment occurring in different time periods (Goodman-Bacon, 2021). We found differences in results between standard TWFE models and the Gardner (2021) models, and event study models showed a more nuanced picture of pre-treatment effects that were likely due to partial treatment. As different event study models rely on different methodological assumptions, we encourage researchers to run multiple event study commands as a robustness check until the field agrees on a single best model. Once event study commands can accommodate colleges switching between treated and untreated conditions as well as handling continuous treatment measures, it will be possible to compare all of our DiD results to event study models that use the same samples.

References

Andrews, M., & Moynihan, D. P. (2002). Why reforms do not always have to "work" to succeed: A tale of two managed competition initiatives. *Public Performance & Management Review, 25*(3), 282-297. https://doi.org/10.1080/15309576.2002.11643662.

Birdsall, C. (2018). Performance management in public higher education: Unintended consequences and the implications of organizational diversity. *Public Performance & Management Review, 41*(4), 669-695. https://doi.org/10.1080/15309576.2018.1481116.

Borusyak, K. (2021). *EVENT_PLOT: Stata module to plot the staggered-adoption diff-in-diff ("event study") estimates*. https://ideas.repec.org/c/boc/bocode/s458958.html.

Borusyak, K., Jaravel, X., & Spiess, J. (2021). *Revisiting event study designs: Robust and efficient estimation.* https://sites.google.com/view/borusyak/research.

Butts, K. (2022). *did2s*. https://github.com/kylebutts/did2s_stata.

Cameron, A. C., & Miller, D. L. (2015). A practitioner's guide to cluster-robust inference. *The Journal of Human Resources, 50*(2), 317-372. https://doi.org/ 10.3368/jhr.50.2.317.

Cantwell, B., & Taylor, B. J. (2020). Political rancor and educational inequality: Why building consensus is necessary to renew American higher education. *Change: The Magazine of Higher Learning, 52*(3), 68-72. https://doi.org/10.1080/00091383.2020.1745607/

Council of Economic Advisers (2015). *Using federal data to measure and improve the performance of U.S. institutions of higher education*. Office of the President of the United States.

Cummings, K., Laderman, S., Lee, J., Tandberg, D., & Weeden, D. (2021). *Investigating the impacts of state higher education appropriations and financial aid.* Boulder, CO: State Higher Education Executive Officers Association.

Delaney, J. A. & Doyle, W. R. (2018). Patterns and volatility in state funding for higher education, 1951-2006. *Teachers College Record, 120*(6), 1-42.

Dougherty, K. J., Jones, S. M., Lahr, H., Natow, R. S., Pheatt, L., & Reddy, V. (2016). *Performance funding for higher education.* Johns Hopkins University Press.

Favero, N., & Rutherford, A. (2019). Will the tide lift all boats? Examining the equity effects of performance funding policies in U.S. higher education. *Research in Higher Education, 61*(1), 1–25. https://doi.org/10.1177/016146811812000605.

Federal Reserve Bank of New York (2021). *Quarterly report on household debt.*

Fryer, R. G., Pager, D., & Spenkuch, J. L. (2013). Racial disparities in job finding and offered wages. *The Journal of Law and Economics*, *56*(3), 633-689. https://doi.org/10.1086/673323.

Furquim, F., Corral, D., & Hillman, N. (2020). A primer for interpreting and designing difference-in- differences studies in higher education research. In L. Perna (Ed.), *Higher education: Handbook of theory and research* (Vol. 35, pp. 1–58). Springer.

Gaddis, S. M. (2015). Discrimination in the credential society: An audit study of race and college selectivity in the labor market. *Social Forces, 93*(4), 1451-1479. https://doi.org/10.1093/sf/sou111.

Gándara, D., & Rutherford, A. (2020). Completion at the expense of access? The relationship between performance-funding policies and access to public 4-year universities. *Educational Researcher, 49*(5), 321-334. https://doi.org/10.3102/0013189X20927386.

Gándara, D., & Rutherford, A. (2018). Mitigating unintended impacts? The effects of premiums for underserved populations in performance-funding policies for higher education. *Research in Higher Education, 59*(6), 681–703. https://doi.org/ 10.1007/s11162-017-9483-x.

Gardner, J. (2021). *Two-stage differences in differences.* https://jrgcmu.github.io/2sdd_current.pdf.

Goodman-Bacon, A. (2021). Difference-in-differences with variation in treatment timing. *Journal of Econometrics, 225*(2), 254-277. https://doi.org/10.1016/j.jeconom.2021.03.014.

Hagood, L. P. (2019). The financial benefits and burdens of performance funding in higher education. *Educational Evaluation and Policy Analysis, 41*(2), 189-213. https://doi.org/10.3102/0162373719837318.

Hillman, N. W., Fryar, A. H., & Crespín-Trujillo, V. (2018). Evaluating the impact of performance funding in Ohio and Tennessee. *American Educational Research Journal, 55*(1), 144–170. https://doi.org/10.3102/0002831217732951.

Hillman, N. W., Tandberg, D. A., & Fryar, A. H. (2015). Evaluating the impacts of "new" performance funding in higher education. *Educational Evaluation and Policy Analysis, 37*(4), 501-519. https://doi.org/10.3102/0162373714560224.

Hillman, N. W., Tandberg, D. A., & Gross, J. P. K. (2014). Performance funding in higher education: Do financial incentives impact college completions? *Journal of Higher Education, 85*(6), 826–857. https://doi.org/10.1353/jhe.2014.0031.

Jensen, M.C., & Meckling, W.H. (1976). Theory of the firm: Managerial behavior, agency costs and ownership structure. *Journal of Financial Economics,* 3(4), 305-360. https://doi.org/10.1016/0304-405X(76)90026-X

Kelchen, R. (2018a). Do performance-based funding policies affect underrepresented student enrollment? *The Journal of Higher Education, 89*(5), 702–727. https://doi.org/10.1080/00221546.2018.1434282.

Kelchen, R. (2018b). *Higher education accountability.* Johns Hopkins University Press.

Kelchen, R. (2019). Merging data to facilitate analyses. *New Directions for Institutional Research, 181,* 59-72. https://doi.org/10.1002/ir.20298.

Kelchen, R., Rosinger, K. O., & Ortagus, J. (2019) How to create and use state-level policy data sets in education research. *AERA Open, 5*(3), 1-14. https://doi.org/10.1177/2332858419873619.

Laderman, S., & Weeden, D. (2020). *State higher education finance FY 2019.* State Higher Education Executive Officers Association.

Li, A. Y. (2017). Covet thy neighbor or "reverse policy diffusion"? State adoption of performance funding 2.0. *Research in Higher Education, 58*, 746-771.

Li, A. Y. (2020). Performance funding policy impacts on STEM degree attainment. *Educational Policy, 34*(2), 312-349. https://doi.org/ 10.1007/s11162-016-9444-9.

Li, A. Y., & Kennedy, A. I. (2018). Performance funding policy effects on community college outcomes: Are short-term certificates on the rise? *Community College Review, 46*(1), 3-39. https://doi.org/10.1177/0091552117743790.

Li, A. Y., & Ortagus, J. C. (2019). Raising the stakes: Impacts of the Complete College Tennessee Act on underserved student enrollment and sub-baccalaureate credentials. *Review of Higher Education, 43*(1), 295–333. https://doi.org/10.1353/rhe.2019.0097.

Liu, S. (2015). Spillovers from universities: Evidence from the land-grant program. *Journal of Urban Economics, 87*, 25-41. https://doi.org/10.1016/j.jue.2015.03.001.

McKinney, L., & Hagedorn, L. S. (2017). Performance-based funding for community colleges: Are colleges disadvantaged by serving the most disadvantaged students? *The Journal of Higher Education, 88*(2), 159-182. https://doi.org/10.1080/00221546.2016.1243948.

Melguizo, T., & Wolniak, G. C. (2012). The earnings benefits of majoring in STEM fields among high achieving minority students. *Research in Higher Education, 53*(4), 383-405. https://doi.org/ 10.1007/s11162-011-9238-z.

Miller, G. N. S., & Morphew, C. C. (2017). Merchants of optimism: Agenda-setting organizations and the framing of performance-based funding for higher education. *The Journal of Higher Education, 88*(5), 754-784. https://www.tandfonline.com/doi/abs/10.1080/00221546.2017.1313084.

Ortagus, J. C., Kelchen, R., Rosinger, K. O., & Voorhees, N. (2020). Performance-based funding in American higher education: A systematic synthesis of the intended and unintended consequences. *Educational Evaluation and Policy Analysis, 42*(4), 520-550. https://doi.org/ 10.3102/0162373720953128.

Ortagus, J., Rosinger, K., & Kelchen, R. (2021). *InformEd States performance-based funding policies dataset.* InformEd States. https://informedstates.org/data.

Quillian, L., Pager, D., Hexel, O., & Midtbøen, A. H. (2017). Meta-analysis of field experiments shows no change in racial discrimination in hiring over time. *Proceedings of the National Academy of Sciences, 114*(41), 10870-10875. https://doi.org/10.1073/pnas.1706255114.

Quillian, L., Lee, J. J., & Oliver, M. (2020). Evidence from field experiments in hiring shows substantial additional racial discrimination after the callback. *Social Forces, 99*(2), 732-759. https://doi.org/10.1093/sf/soaa026.

Rabovsky, T. M. (2012). Accountability in higher education: Exploring impacts on state budgets and institutional spending patterns. *Journal of Public Administration Research and Theory, 22*(4), 675–700. https://doi.org/10.1093/jopart/mur069.

Rosinger, K., Ortagus, J., Kelchen, R., Cassell, A., & Brown, L. (2022). New evidence on the evolution and landscape of performance funding in higher education. *The Journal of Higher Education, 93*(5), 735-768. https://doi.org/10.1080/00221546.2022.2066269.

Rosinger, K., Meyer, K., & Wang, J. (2021). Leveraging insights from behavioral science and administrative burden in free college program design: A typology. *Journal of Behavioral Public Administration, 4*(2), 1-26. https://doi.org/10.30636/jbpa.42.197.

Roth, J., Sant'Anna, P. H. C., Bilinski, A., & Poe, J. (2022). *What's trending in difference-in-differences? A synthesis of the recent econometrics literature.* https://jonathandroth.github.io/assets/files/DiD_Review_Paper.pdf.

Serban, A. M., & Burke, J. C. (1998). Meeting the performance funding challenge: A nine-state comparative analysis. *Public Productivity & Management Review, 22*(2), 157-176. https://doi.org/10.2307/3381031.

Shin, J. C., & Milton, S. (2004). The effects of performance budgeting and funding programs on graduation rate in public four-year colleges and universities. *Education Policy Analysis Archives, 12*(22), 1–26. https://doi.org/10.14507/epaa.v12n22.2004.

Stolzenberg, E. B., Aragon, M. C., Romo, E., Couch, V., McLennan, D., Eagan, M. K., & Kang, N. (2020). *The American freshman: National norms fall 2019.* Higher Education Research Institute, University of California-Los Angeles.

Sun, L., & Abraham, S. (2020). *Estimating dynamic treatment effects in event studies with heterogeneous treatment effects.* http://economics.mit.edu/files/14964.

Thompson, J. R. (1999). Devising administrative reform that works: The example of the reinvention lab program. *Public Administration Review, 59*(4), 283-293. https://doi.org/10.2307/3110111.

Umbricht, M. R., Fernandez, F., & Ortagus, J. C. (2017). An examination of the (un)intended consequences of performance funding in higher education. *Educational Policy, 31*(5), 643-673. https://doi.org/10.1177/0895904815614398.

Ward, J., & Ost, B. (2021). The effect of large-scale performance-based funding in higher education. *Education Finance and Policy, 16*(1), 92-124. https://doi.org/10.1162/edfp_a_00300.

Webber, D. A. (2016). Are college costs worth it? How ability, major, and debt affect the returns to schooling. *Economics of Education Review, 53*, 296-310. https://doi.org/10.1016/j.econedurev.2016.04.007.

Zerquera, D., & Ziskin, M. (2020). Implications of performance-based funding on equity-based missions in US higher education. *Higher Education, 80*, 1153-1174. https://doi.org/10.1007/s10734-020-00535-0.

Table 1: Trends in state PBF policy adoption over time, 1997-2020.

	Four-year universities (number of states)				Two-year colleges (number of states)			
Fiscal year	Approved PBF	Funded PBF	Funded workforce	Funded equity	Approved PBF	Funded PBF	Funded workforce	Funded equity
1997	6	5	1	2	7	6	4	3
1998	8	7	3	4	8	7	5	3
1999	6	6	1	2	8	8	5	4
2000	6	6	2	3	7	7	5	4
2001	8	8	3	5	8	8	6	5
2002	9	8	3	4	9	7	4	3
2003	6	6	2	3	5	5	3	2
2004	5	4	1	2	5	4	2	1
2005	5	5	2	2	4	4	2	1
2006	7	7	4	3	5	5	2	1
2007	8	8	4	3	5	5	2	1
2008	11	11	5	4	7	7	3	1
2009	11	10	5	6	7	4	2	1
2010	10	8	3	4	10	5	2	3
2011	10	6	4	5	11	6	2	5
2012	10	7	3	6	12	8	3	8
2013	17	14	11	9	17	13	7	11
2014	22	17	13	12	24	21	13	15
2015	22	16	15	14	25	22	17	16
2016	24	18	16	15	26	22	17	18
2017	25	19	17	17	30	25	18	19
2018	26	18	16	16	32	26	19	20
2019	26	21	19	19	32	20	21	22
2020	25	22	17	20	31	30	20	22

Source: Authors' data collection and review of state policy documents.

Notes:

(1) Not all PBF systems covered every public institution within a sector in a state.
(2) "Approved" refers to having a PBF system on the books through legislative or system documents that was eligible for funding.
(3) "Funded" means that colleges received funds tied to student outcomes in the given fiscal year.

Table 2: Summary statistics.

	Four-year universities				Two-year colleges			
	Ever PBF		Never PBF		Ever PBF		Never PBF	
	Mean	(SD)	Mean	(SD)	Mean	(SD)	Mean	(SD)
Any funded PBF	24.7	(43.1)	0	--	19.7	(39.8)	0	--
Percent PBF (if funded)	0.68	(1.63)	0	--	0.45	(1.44)	0	--
Workforce premium	9.6	(29.5)	0	--	9.2	(28.9)	0	--
Any equity premium	13.8	(34.5)	0	--	10.4	(30.5)	0	--
6-year earnings: Median	36,591	(6,377)	39,114	(7,719)	28,001	(4,780)	28,570	(5,035)
6-year earnings: 25th percentile	22,747	(4,938)	23,905	(5,345)	15,098	(3,450)	15,915	(3,628)
6-year earnings: 75th percentile	51,058	(8,239)	54,938	(10,769)	42,538	(5,992)	42,571	(6,416)
6-year earnings: Low-income	37,103	(7,198)	39,306	(8,240)	28,926	(4,117)	29,465	(4,877)
6-year earnings: Middle-income	40,020	(6,109)	41,909	(7,599)	35,729	(4,130)	36,100	(4,336)
6-year earnings: High-income	41,980	(6,225)	44,249	(8,053)	38,891	(4,704)	39,021	(4,741)
8-year earnings: Median	41,031	(7,226)	44,600	(8,888)	31,106	(4,199)	31,514	(5,717)
8-year earnings: 25th percentile	25,818	(5,652)	27,826	(6,272)	16,790	(3,784)	17,567	(4,247)
8-year earnings: 75th percentile	57,508	(9,890)	62,662	(12,375)	47,177	(6,768)	46,962	(7,471)
FTE enrollment	10,355	(9,707)	9,518	(8,757)	4,392	(10,249)	3,070	(7,306)
Undergrad share of FTE	87.6	(9.2)	88.3	(10.5)	100	--	100	--
Per-FTE instructional spending	7,743	(3,417)	8,719	(4,848)	5,418	(3,246)	6,265	(4,196)
Share of undergrads part-time	23.6	(15.1)	21.6	(16.6)	58.3	(14.4)	52.1	(18.9)
In-state tuition	5,753	(2,216)	6,162	(2,822)	2,907	(1,825)	3,349	(2,322)
Amount of state grant	2,737	(1,414)	2,968	(1,371)	1,480	(769)	1,339	(848)
Amount of institutional grant	3,322	(1,877)	3,665	(2,261)	1,371	(894)	1,342	(1,185)
Percent receiving state grant	33.8	(21.0)	37.7	(23.0)	30.2	(20.4)	31.3	(29.6)
Percent receiving institutional grant	33.7	(20.6)	27.9	(18.3)	13.9	(16.3)	14.0	(17.0)
Per-FTE state appropriations	8,379	(4,162)	10,076	(6,738)	5,123	(3,900)	6,015	(5,941)
Per-FTE local appropriations	29	(309)	45	(479)	1,878	(2,994)	1,764	(2,390)
Per-capita state income	42,115	(5,831)	46,348	(7,134)	44,293	(6,513)	41,487	(5,733)

State unemployment rate	4.9	(1.0)	4.9	(1.1)	5.1	(1.1)	4.9	(1.0)
State share of adults w/BA	16.9	(3.3)	18.6	(3.6)	17.7	(3.3)	16.7	(3.3)
State number of young students	523,876	(414,648)	843,480	(774,849)	846,122	(758,055)	396,734	(250,919)
Share of Black young adults	12.3	(10.3)	16.4	(11.9)	13.3	(8.9)	20.7	(16.8)
Share of Hispanic young adults	8.0	(9.7)	8.2	(8.1)	10.6	(10.8)	5.0	(5.9)
Share of Native young adults	1.2	(2.3)	0.6	(1.7)	1.0	(2.1)	0.6	(1.3)
Number of observations	4,284		2,804		10,710		3,089	
Number of colleges	332		220		835		256	

Sources: Authors' data collection (PBF policy variables), College Scorecard (earnings outcomes), Bureau of Labor Statistics and Census (state-level characteristics), Integrated Postsecondary Education Data System (all others)

Notes:

(1) All financial values were adjusted into 2020 dollars using the Consumer Price Index.

(2) All variables are from between 1997 and 2009, except earnings (which are for cohorts beginning college in 1997 through 2009).

Table 3: Effects of a funded PBF policy on student earnings outcomes at four-year universities.

Group/outcome of interest	Full sample		No 1997 PBF		Canonical event study	
	TWFE	Gardner	TWFE	Gardner	TWFE	Gardner
6-year earnings (log)						
Median	0.011	0.023	0.009	-0.003	0.011	-0.008
	(0.004)	(0.025)	(0.006)	(0.010)	(0.010)	(0.014)
25th percentile	0.017	0.041	0.015	0.000	0.019	-0.004
	(0.007)	(0.038)	(0.010)	(0.016)	(0.016)	(0.022)
75th percentile	0.010*	0.017	0.007	-0.002	0.009	-0.004
	(0.003)	(0.017)	(0.004)	(0.007)	(0.007)	(0.010)
Low-income	0.012	0.019	0.011	-0.006	0.016	-0.009
	(0.005)	(0.024)	(0.007)	(0.013)	(0.010)	(0.018)
Middle-income	0.013**	0.019	0.012	-0.002	0.014	-0.005
	(0.004)	(0.020)	(0.006)	(0.011)	(0.008)	(0.015)
High-income	0.010*	0.020	0.009	0.001	0.011	-0.001
	(0.003)	(0.018)	(0.004)	(0.008)	(0.007)	(0.011)
8-year earnings (log)						
Median	0.008	0.029	0.003	-0.003	0.008	-0.006
	(0.005)	(0.026)	(0.005)	(0.008)	(0.009)	(0.013)
25th percentile	0.010	0.044	0.004	-0.003	0.009	-0.007
	(0.008)	(0.040)	(0.009)	(0.012)	(0.018)	(0.019)
75th percentile	0.010	0.024	0.005	0.000	0.010	-0.002
	(0.004)	(0.019)	(0.004)	(0.006)	(0.007)	(0.010)
Max number of observations	6,482		5,857		5,088	
Max number of colleges	539		489		427	

Notes:
(1) All models include the control variables shown in Table 2, state and year fixed effects, and state-clustered standard errors. Each coefficient is the result of a separate regression.
(2) * signifies p<.01. ** signifies p<.005, and *** signifies p<.001.
(3) Control variables and PBF information are aligned with a cohort's estimated first year of entry into college.
(4) The "no 1997 PBF" sample excludes colleges that had PBF in 1997, while the canonical event study sample also excludes colleges that no longer operated under PBF in 2019.

Table 4: Effects of a funded PBF policy on student earnings outcomes at two-year colleges.

Group/outcome of interest	Full sample		No 1997 PBF		Canonical event study	
	TWFE	Gardner	TWFE	Gardner	TWFE	Gardner
6-year earnings (log)						
Median	0.003	0.000	0.005	-0.010	0.008	-0.015
	(0.006)	(0.013)	(0.008)	(0.016)	(0.016)	(0.020)
25th percentile	0.002	-0.002	0.001	-0.015	-0.001	-0.022
	(0.007)	(0.013)	(0.009)	(0.015)	(0.014)	(0.019)
75th percentile	0.002	-0.003	0.002	-0.013	0.008	-0.018
	(0.006)	(0.013)	(0.008)	(0.016)	(0.018)	(0.022)
Low-income	0.013*	0.016	0.018	0.014	0.031***	0.017
	(0.005)	(0.007)	(0.007)	(0.011)	(0.006)	(0.015)
Middle-income	0.011	0.017***	0.015	0.020	0.040**	0.037***
	(0.006)	(0.005)	(0.008)	(0.008)	(0.013)	(0.009)
High-income	0.016	0.004	0.027**	0.021	0.053**	0.029
	(0.007)	(0.016)	(0.009)	(0.022)	(0.015)	(0.035)
8-year earnings (log)						
Median	-0.003	0.033	-0.001	-0.010	-0.008	-0.018
	(0.005)	(0.033)	(0.007)	(0.011)	(0.016)	(0.015)
25th percentile	-0.002	0.018	0.003	-0.008	-0.002	-0.018
	(0.006)	(0.024)	(0.008)	(0.011)	(0.014)	(0.013)
75th percentile	-0.005	0.024	-0.005	-0.016	-0.010	-0.026
	(0.005)	(0.032)	(0.007)	(0.014)	(0.017)	(0.019)
Max number of observations	10,636		9,141		7,555	
Max number of colleges	1,007		874		739	

Notes:
(1) All models include the control variables shown in Table 2, state and year fixed effects, and state-clustered standard errors. Each coefficient is the result of a separate regression.
(2) * signifies p<.01. ** signifies p<.005, and *** signifies p<.001.
(3) Control variables and PBF information are aligned with a cohort's estimated first year of entry into college.
(4) The "no 1997 PBF" sample excludes colleges that had PBF in 1997, while the canonical event study sample also excludes colleges that no longer operated under PBF in 2019.

Table 5: Effects of PBF dosage (terciles) on earnings outcomes using Gardner two-stage models.

	Four-year universities			Two-year colleges		
Group/outcome of interest	Low	Medium	High	Low	Medium	High
6-year earnings (log)						
Median	0.014	0.000	0.057	0.016	-0.020	0.024
	(0.005)	(0.012)	(0.060)	(0.008)	(0.013)	(0.015)
25th percentile	0.028	-0.001	0.094	0.009	-0.018	0.020
	(0.012)	(0.016)	(0.093)	(0.008)	(0.017)	(0.018)
75th percentile	0.008	0.003	0.041	0.015	-0.024	0.017
	(0.004)	(0.009)	(0.041)	(0.008)	(0.014)	(0.010)
Low-income	0.016	-0.003	0.044	0.021	0.002	0.036**
	(0.006)	(0.016)	(0.056)	(0.008)	(0.007)	(0.012)
Middle-income	0.017	-0.001	0.042	0.023*	0.009	0.016
	(0.007)	(0.013)	(0.047)	(0.008)	(0.005)	(0.009)
High-income	0.013	0.000	0.048	0.033	-0.007	-0.025
	(0.006)	(0009)	(0.044)	(0.016)	(0.018)	(0.032)
8-year earnings (log)						
Median	0.016	-0.002	0.076	0.051	0.017	0.041
	(0.006)	(0.009)	(0.063)	(0.033)	(0.042)	(0.020)
25th percentile	0.024	-0.005	0.119	0.036	0.009	0.010
	(0.012)	(0.009)	(0.097)	(0.022)	(0.031)	(0.015)
75th percentile	0.016*	0.003	0.057	0.047	0.007	0.027
	(0.006)	(0.008)	(0.047)	(0.031)	(0.041)	(0.017)
Max number of observations		6,482			10,636	
Max number of colleges		539			1,007	

Notes:
(1) All models include the control variables shown in Table 2, state and year fixed effects, and state-clustered standard errors. Each coefficient is the result of a separate regression.
(2) * signifies p<.01. ** signifies p<.005, and *** signifies p<.001.
(3) Control variables and PBF information are aligned with a cohort's estimated first year of entry into college.
(4) The tercile cutoffs were 0.97% and 4.19% for four-year universities and 1.02% and 2.00% for two-year colleges. The reference group is no funded PBF policy.

Table 6: Effects of PBF workforce metrics on student earnings outcomes using Gardner two-stage models.

	Four-year universities		Two-year colleges	
Group of interest	PBF, no workforce	PBF with workforce	PBF, no workforce	PBF with workforce
6-year earnings (log)				
Median	0.061	-0.008	0.018	-0.018
	(0.040)	(0.011)	(0.009)	(0.014)
25th percentile	0.012	-0.010	0.013	-0.017
	(0.062)	(0.014)	(0.010)	(0.016)
75th percentile	0.040	-0.002	0.016	-0.022
	(0.028)	(0.009)	(0.007)	(0.014)
Low-income	0.055	-0.010	0.022	0.007
	(0.036)	(0.015)	(0.010)	(0.009)
Middle-income	0.050	-0.006	0.017	0.016
	(0.030)	(0.013)	(0.007)	(0.008)
High-income	0.048	-0.002	0.007	-0.002
	(0.031)	(0.008)	(0.020)	(0.022)
8-year earnings (log)				
Median	0.068	-0.005	0.061	0.007
	(0.044)	(0.008)	(0.032)	(0.028)
25th percentile	0.107	-0.011	0.032	0.005
	(0.067)	(0.009)	(0.026)	(0.022)
75th percentile	0.053	0.000	0.053	-0.003
	(0.032)	(0.007)	(0.031)	(0.028)
Max number of observations	6,482		10,636	
Max number of colleges	539		1,007	

Notes:
(1) All models include the control variables shown in Table 2, state and year fixed effects, and state-clustered standard errors. Each coefficient is the result of a separate regression.
(2) * signifies p<.01. ** signifies p<.005, and *** signifies p<.001.
(3) Control variables and PBF information are aligned with a cohort's estimated first year of entry into college.
(4) The reference group is no funded PBF policy.

Table 7: Effects of PBF equity metrics on student earnings outcomes using Gardner two-stage models.

	Four-year universities		Two-year colleges	
Group of interest	PBF, no equity	PBF with equity	PBF, no equity	PBF with equity
6-year earnings (log)				
Median	0.063*	-0.004	0.018	-0.003
	(0.023)	(0.035)	(0.051)	(0.017)
25th percentile	0.097	-0.001	0.007	0.001
	(0.042)	(0.037)	(0.038)	(0.017)
75th percentile	0.044	0.000	0.017	-0.006
	(0.031)	(0.039)	(0.045)	(0.020)
Low-income	0.051	-0.011	0.020	-0.002
	(0.029)	(0.032)	(0.027)	(0.031)
Middle-income	0.052	-0.005	0.018	0.027*
	(0.023)	(0.034)	(0.025)	(0.010)
High-income	0.052	-0.002	0.010	0.008
	(0.023)	(0.048)	(0.041)	(0.025)
8-year earnings (log)				
Median	0.060	-0.002	0.041	0.011
	(0.024)	(0.037)	(0.088)	(0.038)
25th percentile	0.091	-0.004	0.020	0.013
	(0.040)	(0.038)	(0.082)	(0.035)
75th percentile	0.046	0.003	0.031	0.000
	(0.031)	(0.043)	(0.079)	(0.034)
Max number of observations	6,482		10,636	
Max number of colleges	539		1,007	

Notes:
(1) All models include the control variables shown in Table 2, state and year fixed effects, and state-clustered standard errors. Each coefficient is the result of a separate regression.
(2) * signifies p<.01. ** signifies p<.005, and *** signifies p<.001.
(3) Control variables and PBF information are aligned with a cohort's estimated first year of entry into college.
(4) The reference group is no funded PBF policy.

Figure 1a: Earnings event studies, 4-years

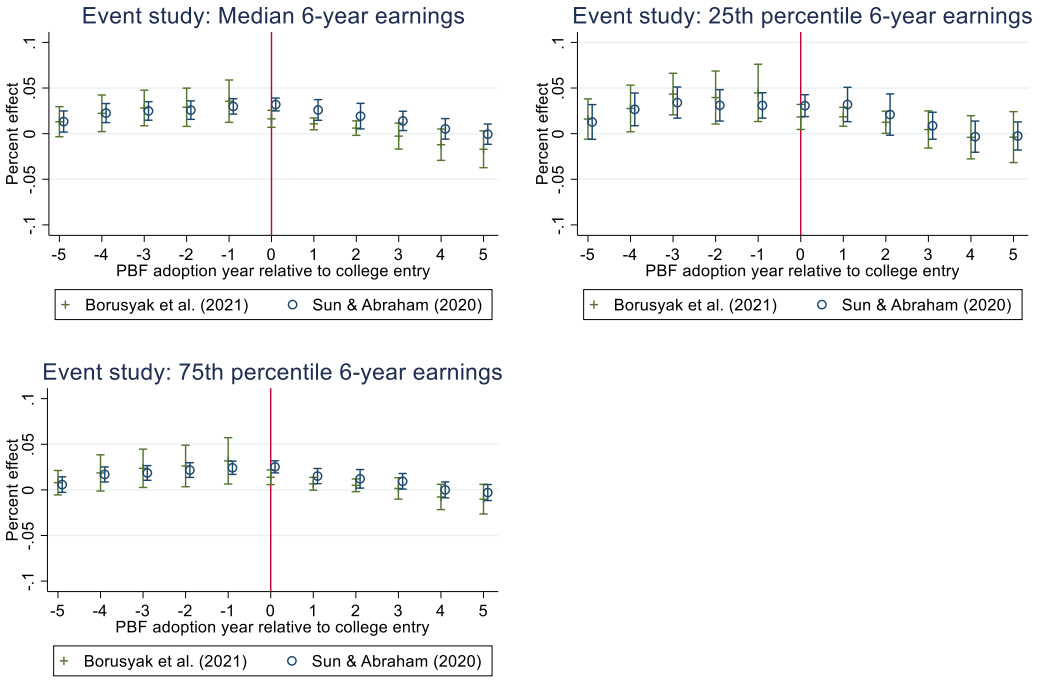


Figure 1b: Earnings event studies, 4-years

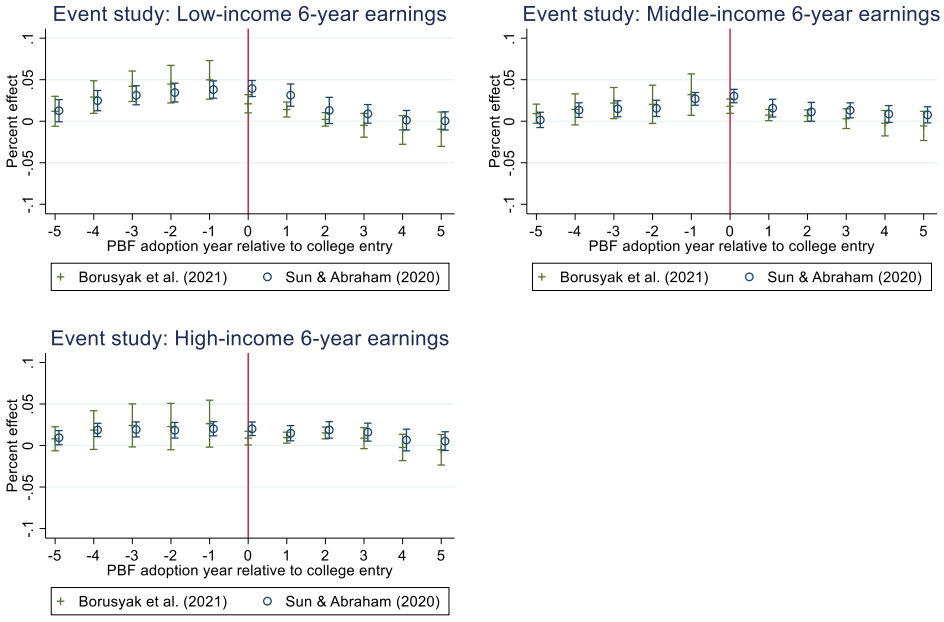


Figure 1c: Earnings event studies, 4-years

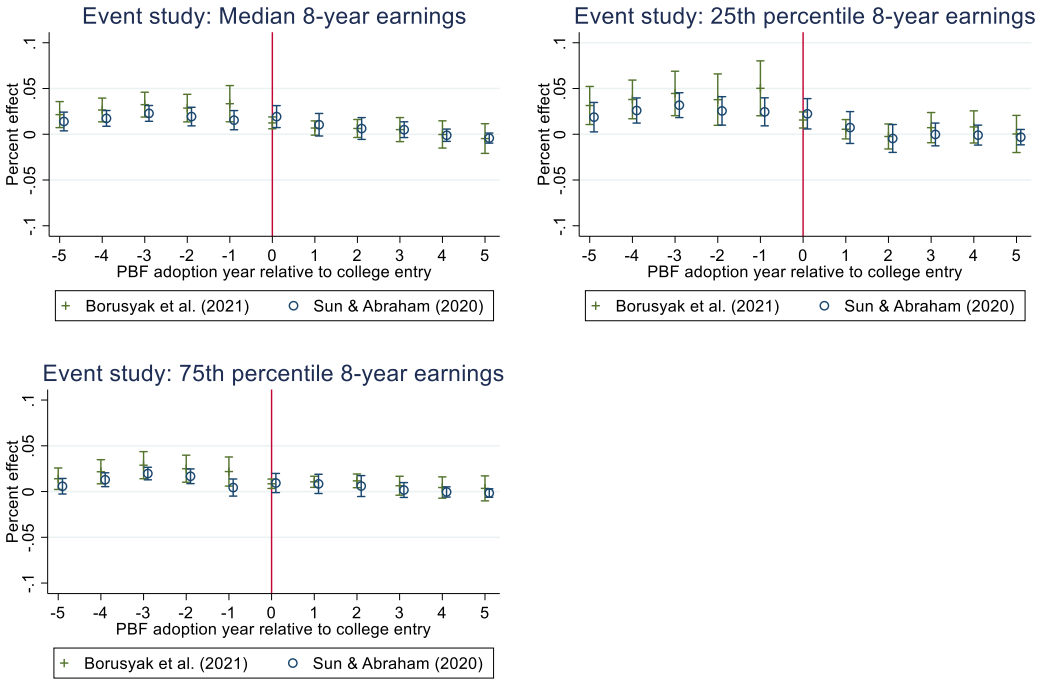
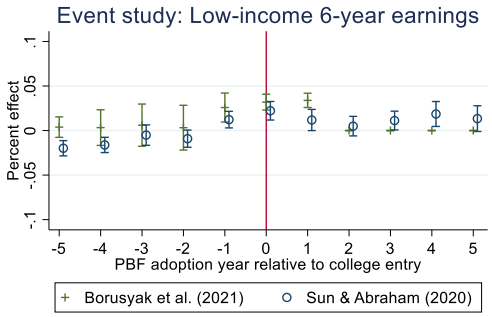
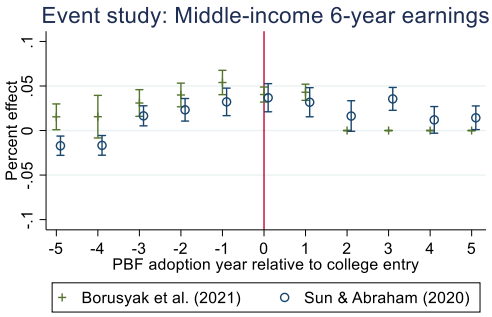


Figure 2a: Earnings event studies, 2-years

Figure 2b: Earnings event studies, 2-years





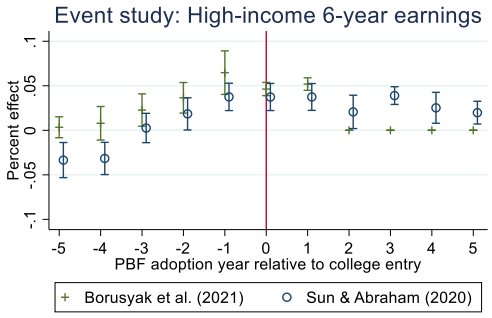


Figure 2c: Earnings event studies, 2-years