

This PDF is a selection from a published volume from the National Bureau of Economic Research

Volume Title: Productivity in Higher Education

Volume Authors/Editors: Caroline M. Hoxby and Kevin Stange, editors

Volume Publisher: University of Chicago Press

Volume ISBNs: 978-0-226-57458-5 (cloth); 978-0-226-57461-5 (electronic)

Volume URL:

<https://www.nber.org/books-and-chapters/productivity-higher-education>

Conference Date: May 31–June 1, 2016

Publication Date: November 2019

Chapter Title: Labor Market Outcomes and Postsecondary Accountability: Are Imperfect Metrics Better Than None?

Chapter Author(s): Veronica Minaya, Judith Scott-Clayton

Chapter URL:

<https://www.nber.org/books-and-chapters/productivity-higher-education/labor-market-outcomes-and-postsecondary-accountability-are-imperfect-metrics-better-none>

Chapter pages in book: (p. 67 – 104)

Labor Market Outcomes and Postsecondary Accountability Are Imperfect Metrics Better Than None?

Veronica Minaya and Judith Scott-Clayton

3.1 Introduction

The postsecondary accountability movement is motivated by the idea that reporting and rewarding measures of institutional performance can generate both better information and stronger financial incentives to improve the decision-making processes of prospective consumers, policy makers, and institutions (Dougherty and Reddy 2013). In his 2013 State of the Union address, President Obama gave voice to this movement by calling for institutions to be “[held] accountable for cost, value, and quality,” eventually by linking measures of institutional performance to federal aid (US Department of Education 2013).

This accountability agenda is even more advanced at the state level. As of 2015, 32 states were already utilizing some form of performance or “outcomes-based” funding, with another 5 in the process of implementing it

Veronica Minaya is a senior research associate at the Community College Research Center at Teachers College, Columbia University.

Judith Scott-Clayton is associate professor of economics and education at Teachers College, Columbia University, a senior research scholar at the Community College Research Center at Teachers College, and a research associate of the National Bureau of Economic Research.

Authors are listed in alphabetical order; both contributed equally to this project. The authors gratefully acknowledge Josh Hawley and Lisa Neilson at the Ohio Education Research Center, who helped facilitate their application for the restricted data utilized herein, and Caroline Hoxby, Kevin Stange, Jeff Smith, Robert Kelchen, and NBER conference participants for valuable suggestions. Funding to obtain and clean the data used herein was provided by the Institute of Education Sciences, US Department of Education, through Grant R305C110011 to Teachers College, Columbia University. The opinions expressed are those of the authors and do not represent views of the Institute, the US Department of Education, or the Ohio Education Research Center. For acknowledgments, sources of research support, and disclosure of the authors’ material financial relationships, if any, please see <http://www.nber.org/chapters/c13876.ack>.

(National Council of State Legislatures [NCSL] 2015). While in most states the portion of state funding that is performance based remains small—typically less than 10 percent—two states (Tennessee and Ohio) now base the majority of institutional funding on performance metrics (Snyder 2015).

These accountability efforts increasingly look beyond just credit and credential completion to what some view as the most important dimension of student outcomes: postcollege labor market success. In September 2015, the Obama administration took a major step toward this goal by releasing an updated version of its College Scorecard, which for the first time provided information not just on college costs and graduation rates but also on median postcollege earnings for more than 4,000 institutions nationwide. Several states now incorporate job placement, employment, and earnings data into their performance funding formulae, at least for portions of their postsecondary sectors. And the Texas State Technical College System uses information on students' postcollege earnings as the *sole* criteria for making funding recommendations to the Texas legislature (Selingo and Van Der Werf 2016; Texas Higher Education Coordinating Board [THECB] 2013).

There is no consensus, however, on how such labor market measures should be constructed, nor is there much evidence regarding how the choice of measure may affect the resulting institutional ratings. While the College Scorecard provides earnings for all entrants 10 years after entry, states using labor market data in performance funding formulae sometimes examine outcomes for graduates less than a year after graduation. Does it matter whether employment/earnings are measured 1, 2, or 10 years postgraduation? Moreover, should schools be held accountable for all students or just those who graduate? What difference does it make whether metrics are adjusted to account for the incoming characteristics of the student population? And can labor market data be used to examine more than just earnings?

In this chapter, using administrative data from one state that links postsecondary transcripts to in-state quarterly earnings and unemployment records over more than a decade, we construct a variety of possible institution-level labor market outcome metrics. Our goal is not to identify the “best” metric but to explore how sensitive institutional ratings may be to the choice of metric, length of follow-up, and inclusion of adjustments for student characteristics, particularly in the context of real-world data limitations. We believe we are the first to use a state-level database to assess labor market outcome metrics beyond earnings, including full-time, full-year employment rates; social service sector employment; and unemployment claims. We also examine how these metrics compare with the academic-outcome-based metrics more commonly incorporated into state accountability systems. This work builds on similar efforts to analyze labor outcome metrics in the postsecondary sector using Internal Revenue Service (IRS) data (Hoxby 2015; also see chapter 2 in this volume), the College Scorecard data (Executive Office of the President [EOP] 2015), and data on four-year colleges in Texas and Canada

(Betts, Ferrall, and Finnie 2013; Cunha and Miller 2014). It also builds on research on institutional performance measurement in sectors with similar features, including job training (Heckman, Heinrich, and Smith 2002) and health care (Staiger et al. 2013).

We conclude that labor market data, even when imperfect, *can* provide valuable information distinct from students' academic outcomes. As has been found in other sectors, however, ratings are highly sensitive to the choice of outcome and length of follow-up (and, to a lesser extent, to the inclusion of compositional adjustments). The most obvious labor market outcomes—graduates' employment and earnings in the year after graduation—are unreliable predictors of institutional performance on the same metrics measured several years later. Earnings and employment alone also fail to capture other aspects of economic well-being that may be valued by both policy makers and students themselves. Consistent with Cunha and Miller (2014), our findings suggest a cautious approach: while a mix of feasible labor market metrics may be better than none, reliance on a single unadjusted earnings metric, especially if measured too early, may undermine policy makers' ongoing efforts to accurately quantify institutional performance.

The remainder of the chapter proceeds as follows. Section 3.2 provides policy context around performance accountability efforts in higher education. Section 3.3 provides a conceptual and practical overview of the challenges of using state-level labor market data for this purpose. Section 3.4 describes our data and methodology. Section 3.5 presents results, and section 3.6 provides a concluding discussion.

3.2 Policy Background

Policy goals in higher education traditionally have been measured and financed primarily using input metrics—such as student enrollment and credit hours—for many decades (SRI International 2012).¹ This stands in contrast to the job training sector, which has a more established tradition of evaluating programs based on participants' labor market outcomes going back at least to the Job Training Partnership Act of 1982 (Barnow and Smith 2015). Over the past three decades, however, there has been a push to align higher education funding with academic outputs, such as credits completed or degrees conferred, rather than inputs. Output-based accountability efforts range from purely informational reporting to higher-stakes performance-based funding (Burke 2001; Dougherty and Reddy 2013; Umbricht, Fernandez, and Ortagus 2015). The idea behind outcomes-based

1. A cost-plus approach is a traditional budgeting strategy in which public colleges and universities primarily base their projected budgetary needs on current costs, student enrollments, and inflationary increases.

accountability policies is that they generate both better information and stronger financial incentives to improve the decision-making processes of prospective consumers, policy makers, and institutions (Dougherty et al. 2014; Heckman, Heinrich, and Smith 2002; Muriel and Smith 2011).

The first wave of performance funding (PF) policies, or PF 1.0, used metrics to award bonuses over and above base state funding for higher education (Dougherty and Reddy 2013; Snyder 2011; Dougherty, Natow, and Vega 2012). These early programs eventually lost support, however, due to dissatisfaction with the reliability and validity of the chosen performance metrics, the top-down process by which they were determined, and the small amount of funding at stake (Burke and Serban 1997; Snyder 2015). More than half of PF 1.0 programs were abandoned in the early 2000s (Dougherty, Natow, and Vega 2012).

A new wave of performance funding that “no longer takes the form of a bonus but rather is part and parcel of regular state base funding for higher education” (Dougherty et al. 2014a) began spreading in the late 2000s. Ohio and Indiana both established such PF 2.0 programs in 2009, followed by Tennessee in 2010 (Dougherty et al. 2014b; Dougherty and Reddy 2013). By 2015, 32 states had a policy in place to allocate a portion of funding based on performance indicators, with 5 others in the process of transitioning (NCSL 2015). Although many states continue to use performance funding to allocate relatively small percentages of higher education funding, some states now allocate much larger percentages of funding using performance metrics (Dougherty and Reddy 2013). For example, outcomes-based funding represents about two-thirds of total state support to all higher education institutions in Ohio (Snyder 2015). This high proportion of funding is one reason why Snyder (2015) classifies Ohio and Tennessee as the two most-advanced/high-stakes funding systems, which some are calling PF 3.0 (Kelchen and Stedrak 2016).

With respect to the range of outcomes considered, 28 states currently consider the number of degrees awarded by a university, 16 use some form of course completion, 12 include retention rates, and 12 incorporate graduation rates (NCSL 2015). Many states give extra weight to outcomes for certain subgroups, such as Pell-eligible students (Burke 2002; Dougherty, Hare, and Natow 2009).

Recently, and particularly after the recession, accountability conversations have increasingly focused on the financial costs and benefits of college. Ten states now put weight on postgraduation outcomes such as job placement rates or earnings (EOP 2015; NCSL 2015; see appendix table 3A.1 for additional details). The Texas State Technical College System now uses information on students’ postcollege earnings as the *sole* criteria for making funding recommendations to the Texas legislature (THECB 2013). Other states—such as California, Virginia, and Ohio—provide interactive online

tools that can be used to explore median earnings after graduation by degree level, field, and/or institution (Nye et al. 2015).

Rigorous evidence regarding the effectiveness of PF policies is limited but discouraging. Two recent quasi-experimental studies compare trends over time in states adopting new policies to states that did not and find evidence of unintended strategic responses. Kelchen and Stedrak (2016) find suggestive evidence that colleges under PF may enroll fewer low-income students as a result, while Hillman, Tandberg, and Fryar (2015) find that two-year colleges in Washington state increased the production of short-term certificates, but not associate's degrees, when completion rates were introduced as a performance metric. A broader review of the literature by Hillman (2016) identifies 12 studies, which find mostly null or even negative results of PF policies.

As indicated by the failure of many early performance funding programs in the late 1990s, the successful design of such programs requires a close examination of the mission of institutions, the type of student body it serves, and the institution's capacity for organizational learning and change (Dougherty et al. 2014; Li 2014). Alignment with state and social priorities for higher education is crucial, as is confidence in the reliability of the chosen metrics. As more states begin to use labor market data for accountability, it is essential to understand the implications of alternative metrics as well as the potential for unintended consequences in order to avoid repeating the mistakes of earlier efforts at reform.

3.3 Conceptual and Practical Challenges to Using State Labor Market Data

As more and more states are able to track students into the labor market via state unemployment insurance (UI) databases, it opens the door to use this information for institutional accountability. Such use presents a number of important practical and conceptual challenges, however. Practical challenges derive from both mundane data limitations—limited length of follow-up, for example, or an inability to track graduates out of state—and the fundamental statistical difficulty of disentangling differences in institutions' true productivity from mere differences in the composition of their respective student populations. Even when stakeholders agree on an outcome they'd like to measure, these challenges can lead to biased estimates in practice.

Moreover, stakeholders may not always agree on what should be measured and when, even if ideal data are available. Conceptual challenges derive from both the multiple objectives that postsecondary institutions serve (e.g., improving not just labor market outcomes but also well-being more broadly; promoting degree completion but also access and persistence at other levels)

and the multiple purposes and audiences accountability data may be used for (e.g., informing enrollment decisions by students, short-term funding decisions by the state, and longer-term strategic planning by institutions). This section describes these challenges and helps motivate the variety of metrics that we create and compare in the subsequent analysis.

3.3.1 Productivity versus Student Composition

It is one thing to simply measure student outcomes and another thing entirely to assign all credit (or blame) for those outcomes to the institution. Students at highly selective institutions are likely to have higher graduation rates and better labor market outcomes at least in part because these students come in with stronger academic preparation, better family supports, and greater financial resources. Similarly, student preferences may drive differences in outcomes: students at institutions with strong math and science programs may have better outcomes because math and science majors have better outcomes in general, regardless of the strength of the institution. Finally, students who attend institutions in strong labor markets may have higher earnings than those in weaker labor markets (Hoxby 2015 distinguishes these last two types of selection bias as horizontal selection, while the first represents vertical selection). Failure to account for selection in a PF system can lead to both biased estimates of true productivity as well as adverse incentives for institutions to reduce access, as suggested by Kelchen and Stedrak (2016).

Assessing and addressing the selection or “cream-skimming” problem has been a major focus of performance measurement efforts in other sectors (Heckman, Heinrich, and Smith 2002; Muriel and Smith 2011; Staiger et al. 2013). While randomized control trials (RCTs) have been used to circumvent selection bias in the evaluation of job training programs, they are less feasible in the context of evaluating schools or hospitals. Still, these concerns have motivated a small but growing literature that uses rigorous quasi-experimental methods to measure institutions’ true causal effects or “value added.” In higher education, some studies have relied on admissions cutoff policies at a limited number of institutions (Hoekstra 2009), while others have compared students with similar qualifications who were admitted to the same set of selective schools (Dale and Krueger 2002, 2011). More recently, Hoxby (2015 and chapter 2 in this volume) uses a vast data set combining college admissions test scores, enrollment data, and income data from the US Treasury to estimate institutional value added, relying on idiosyncrasies both in how schools choose between similar students and in how students choose between similar schools to isolate plausibly causal institutional effects. For a detailed review of the selection challenge and related empirical literature in higher education, see Hoxby (2015) and the Executive Office of the President (2015).

Unfortunately, there is no guarantee that state policy makers will have

access to both the right data and the right natural experiment to undertake these types of rigorous causal analysis. The one state system currently using a “value-added” approach, the Texas technical college system, simply deducts a fixed minimum amount from observed earnings (corresponding to full-time, full-year employment at the minimum wage; see THECB 2013). Such a strategy is vulnerable to strategic “cream-skimming” behavior if colleges shift recruitment away from students with the largest potential benefits and toward those with the highest preexisting earnings potential.

A more generally feasible state strategy would be to compute institutional “fixed effects” that use regression analysis to control for any differences in student outcomes that are attributable to observable student characteristics, such as age, race/ethnicity, gender, location of residence at entry, and declared major. These regression adjustments may be less transparent and require more choices to be made by the analyst than simply presenting unadjusted student outcomes. Moreover, differences in *unobserved* student characteristics (such as ability or motivation) are likely to remain even after observed characteristics are taken into account. This may explain why state and federal tools allowing students to browse earnings by institution/program generally provide simple unadjusted means or medians rather than attempting to control for student characteristics.

In the analysis that follows, we present both unadjusted institutional mean outcomes and adjusted outcomes using an increasingly rich set of controls. Even in our richest model, however, we do not attempt to interpret the resulting institutional fixed effects as causal. Nor are we able to identify the method that most closely approximates a causal analysis. Our modest goal is to evaluate how much these choices actually matter in practice.

3.3.2 Interstate Mobility

A major practical challenge in using state UI databases to measure earnings is that such databases typically include information only for individuals who remain in state (though some states do have data-sharing agreements with border states).² Individuals who leave the state are indistinguishable from those who are in state but simply not working.

This complicates the analysis of both employment rates and earnings: without any adjustments, institutions that send many graduates out of state could be seriously disadvantaged on these outcome measures.³ For

2. In addition, UI databases do not include those who are self-employed, some student employees (e.g., work-study students), railroad workers, some agricultural workers, and federal employees. Despite coverage gaps relative to self-reported survey data, prior research has found UI data to provide comparable estimates of program impacts (Kornfeld and Bloom 1999).

3. Note that our subsequent analysis will focus on Ohio’s in-state student population, both because such home-state students are of particular interest to state policy makers and because students who migrate to Ohio for college have a particularly high likelihood of leaving the state after college.

example, using data from the National Longitudinal Survey of Youth 1997 (NLSY-97), Scott-Clayton and Wen (2017) show that attending or graduating from a four-year institution is associated with an increased likelihood of living outside of one's home state after college and that four-year students who later live outside their home state earn significantly more than their counterparts who remain in their home state. Prior research further indicates that the effects of education on mobility are causal (Bound and Holzer 2000; Malamud and Wozniak 2012; Wozniak 2010). Moreover, as mobility accumulates over time, this problem worsens the longer the follow-up period. Grogger (2012) discusses this problem in detail in the context of job training program evaluation and finds that it can seriously compromise the validity of program impact estimates. Out-of-state mobility may be less of a concern for evaluating outcomes for two-year colleges, as two-year enrollment and graduation are not associated with the likelihood of leaving the state, nor do two-year students appear to earn substantially different returns if they leave the state (Scott-Clayton and Wen 2017).⁴

In part to minimize this bias, the states that provide information on graduates' employment and earnings often do so within a relatively short period of time postgraduation (e.g., three to six months) and condition earnings metrics on at least some level of observed employment. For example, Ohio examines in-state retention (a combination of employment and subsequent educational enrollment) in the fourth quarter of the year for spring graduates. Earnings are considered only for those who have earnings above a minimum level approximating full-time employment.

Examining earnings conditional on some approximation of full-time employment has the advantage of avoiding confounds not just from out of state mobility but also from individual choices regarding labor force participation (e.g., relating to family formation or continued educational investments). Scott-Clayton and Wen (2017) show that these conditional earnings estimates are much more robust to out-of-state mobility than unconditional earnings estimates: the estimated returns to two- and four-year degrees are quite similar whether analysts condition on employment in any state or on employment only in the home state. On the other hand, these conditional measures will also miss important effects institutions may have on the likelihood of finding and maintaining stable employment.

Our solution to this is to look at graduates in four subsequent quarters in a focal year. If they show up in the data at all, we make the assumption that they are part of the in-state labor force. We then examine our measures

4. Scott-Clayton and Wen (2017) find no relationship between out-of-state mobility and estimated earnings returns to two-year college enrollment. Two-year college graduates who leave the state appear if anything to earn somewhat lower returns than graduates who remain in state, but the differences are not statistically significant.

of full-time, full-year employment; social service sector employment; and unemployment claims only for those who appear in the data in that year. For earnings, we further condition on a proxy measure of full-time, full-year employment (described in more detail in the methodology section below).

3.3.3 Timing of Outcomes Measurement

Measures of employment and earnings from relatively early in the life cycle can be not only noisy but also potentially biased measures of lifetime earnings. As discussed by Heckman, Heinrich, and Smith (2002), “In [the context of human capital programs], the short-run measurements on which performance standards are based will likely be perversely related to long-run benefits” (780). Those with the highest long-term earnings potential may have lower-than-expected earnings if measured soon after graduation if they continue to invest in additional skills/education both on and off the job. Moreover, those with the highest earnings potential may optimally spend more time after graduation searching for a good job match. Evidence suggests that the optimal time to measure individuals’ earnings is not until their early 30s to mid-40s (Haider and Solon 2006). Outcomes measured mere months after graduation may reflect mostly noise, or worse, they could be inversely correlated with outcomes over a longer period of time.

From an accountability perspective, however, long time lags also have their own conceptual and practical limitations. To be useful, accountability metrics should reflect institutional performance from a relatively recent period. In addition, the longer the lag between graduation and labor market observation, the more serious the interstate mobility problem becomes. Since the optimal time lag is far from obvious in this context, we measure labor market outcomes four years after graduation but also test variations from one year to seven years postgraduation.

3.3.4 Measuring Outcomes beyond Earnings

Even with ideal data on earnings, a fundamental critique that has been leveled against the use of earnings data for postsecondary accountability is that they fail to capture many other positive impacts of education. For example, institutions that send many graduates into teaching or social service jobs will perform worse on earnings-based metrics than those that send many graduates into finance. Even within a given industry, individuals make trade-offs between wages and other “job amenities” such that wages alone may be a poor summary of overall labor market success. In addition, policy makers (and individuals) may care more about earnings differences at the bottom of the income distribution than in the middle or at the top, but neither average nor median wage metrics will reflect this. Finally, ideally, measures of postsecondary accountability would include not just measures of labor market success but also measures of health and well-being.

State UI databases obviously cannot measure all relevant possible institutional effects. Still, even within UI databases, it is possible to construct a more diverse range of metrics to capture dimensions beyond earnings. For example, UI data can be used to look at the stability of employment over time (such as whether individuals are employed full time for the entire year or how many employers they have had in a given period). Information on industry of employment can also be used to measure employment in “social service” sectors such as teaching or government. Finally, actual unemployment claims can be examined as a measure of job loss (though in practice, UI claims data are typically held separately from quarterly wages and may require additional data permissions to merge). We describe the specific additional measures that we create in section 3.4 below.

3.3.5 Outcomes for Whom? Graduates versus Entrants

Most of the state data tools that provide earnings information by institution and program—such as in California, Florida, Virginia, and Ohio—do so for graduates only rather than looking at outcomes for all students who enter the institution. The conceptual argument for looking only at graduates is twofold: first, institutions may have limited influence over the earnings of students who drop out, and second, given the vast differences in earnings of graduates versus nongraduates, averaging across both groups may be a poor summary of either group’s typical outcomes. On the other hand, examining the earnings only of graduates may seriously distort institutions’ overall productivity if they graduate only a fraction of entrants. The federal College Scorecard is one data source that provides median earnings for all entrants, not just those who graduate.

Our resolution to this trade-off is to examine labor market outcomes for graduates only but to examine these metrics alongside graduation metrics that *are* measured for all students. This avoids the problem of interpreting labor market metrics that muddle both margins while still holding institutions accountable for both.

One limitation of this strategy is that it will not credit institutions that are particularly effective or ineffective at increasing the earnings of nongraduates relative to graduates. This might occur if an institution has a program that is so effective that students leave to take good jobs even before they graduate or if an institution’s degrees have a particularly high “sheepskin effect” component, such that the payoff to completing 99 percent of the degree is far less than 99 percent of the payoff to completing the degree. In general, however, it seems reasonable to assume that whatever the earnings payoff to graduating from a given institution, the payoff to attending but not graduating may be proportional to the fraction of the degree that was completed (indeed, empirical evidence on the returns to credits from Kane and Rouse [1995] supports this proportional-payoff hypothesis).

3.4 Empirical Methodology

3.4.1 Data and Sample

Deidentified data were provided by the Ohio Education Research Center (OERC) under a limited-use, restricted-data agreement. The OERC assembles data from multiple state agencies, including the Ohio Board of Regents (OBR) and the Ohio Department of Job and Family Services (ODFJS), into a repository known as the Ohio Longitudinal Data Archive (OLDA).⁵

From the available data, we requested elements from the Ohio Higher Education Information (HEI) system, including students' demographic characteristics, entrance and enrollment records, major choice, and certificate and degree completion from each of Ohio's higher education institutions (14 universities with 24 regional branch campuses and 23 community colleges, some of which also have multiple campuses). We also requested elements from the UI data, including quarterly earnings and unemployment claims to enable us to examine students' labor market outcomes. While the OLDA data cover more than a decade, for this project we utilize student data from 2000 to 2007 (to enable sufficient follow-up of entrants/graduates) and labor market data from 2000 to 2012. We describe some additional sample restrictions below after providing more detail about our methodology.

The data do not include any measure of students' academic ability upon admission (such as SAT/ACT scores, high school grade point average or test scores, or college entrance or placement exam scores), nor do they include financial aid application data or family income information. The data do include information on financial aid receipt for some years; however, for this project, we chose to prioritize elements that are available for all analytic cohorts. We may incorporate this information into subsequent sensitivity analyses.

3.4.2 Methods and Metrics

This section describes the outcome variables we use, the key analysis groups, and the process we employ to estimate the resulting metrics. After

5. The following acknowledgment is required to be stated on any materials produced using workforce or higher education data accessed from the OLDA: This workforce solution was funded by a grant awarded to the US Department of Labor's Employment and Training Administration. The solution was created by the Center for Human Resource Research on behalf of the Ohio Department of Job and Family Services and does not necessarily reflect the official position of the US Department of Labor. The Department of Labor makes no guarantees, warranties, or assurances of any kind, express or implied, with respect to such information, including any information on linked sites and including, but not limited to, accuracy of the information or its completeness, timeliness, usefulness, adequacy, continued availability, or ownership. This solution is copyrighted by the institution that created it. Internal use, by an organization and/or personal use by an individual for noncommercial purposes, is permissible. All other uses require the prior authorization of the copyright owner.

computing regression-adjusted “institutional fixed effects” to account for compositional differences across institutions, we standardize group-level means / fixed effects in order to be able to compare metrics that have different natural scales and then assess the resulting metrics using correlation matrices and graphical analysis.

Outcomes. We construct four labor market accountability metrics based on cohorts of bachelor of arts (BA) / bachelor of science (BS) graduates for four-year institutions and cohorts of certificate/degree completers and transfer students for two-year institutions (i.e., two-year students who transferred to a public four-year institution are grouped with those who earned a credential in the same year/institution). We focus on spring graduates only to simplify our analysis and examine outcomes in the fourth full calendar year postgraduation (so, for a spring 2002 graduate, this would be calendar year 2006). We test sensitivity to examining these outcomes earlier or later, from one year to seven years postgraduation.

To avoid contaminating our estimates with out-of-state mobility (those who move out of state are indistinguishable from those in state but not working), we limit all labor market measures to individuals who have at least some in-state earnings during the focal year. We also limit our labor market measures to those who are not enrolled during the focal year.

While our chapter is primarily focused on the potential use of labor market outcomes, we also wanted to compare these to academic outcomes that are more commonly used for accountability purposes. We created several measures, including degree completion and transfer rates, cumulative credits attempted and completed, and the ratio of credits completed to credits attempted. But because all of these measures were very highly correlated, we chose to focus on degree completion rates (or completion/transfer for the two-year sector) as a summary academic measure. Additional details on each outcome and its rationale are below:

1. *Full-time, full-year employment (proxy).* This measure is intended to capture the stable employment margin: what percentage of graduates are substantially and consistently engaged in the labor market? We do not have any measure of full-time status or hours worked, so we approximate this as employment in all four quarters of the year, with real earnings in each quarter above an amount roughly corresponding to 35 hours per week at minimum wage.⁶ As noted above, this is computed only for individuals who show up in the employment data and are not enrolled in the focal year.

2. *Annual earnings conditional on full-time, full-year employment.* This is intended to capture the intensive employment margin. This is the sum of real quarterly earnings, adjusted to constant 2013 US dollars. In practice,

6. The minimum wage for Ohio in 2013 was \$7.85 according to the US Department of Labor. Therefore, the average quarterly minimum wage for full-time employees in 2013 was approximately \$4,396.

since we cannot observe hours of work, this measure captures both variation in wages as well as variation in hours. Earnings are top-coded at the 95th percentile and only for individuals who are not enrolled in the focal year.⁷

3. *Employment in “social service” sectors.* The rationale for this measure is to address the critique that earnings are not the only positive outcome of education. This measure gives credit for potential positive social externalities of public/social sector employment and could also be a way of acknowledging that some sectors offer benefits and job protections not captured by wages alone. Since we only have industry codes in the employment data, this is only a rough proxy: we include those working in educational services (NAICS 661, including private, state, and local government schools), and the federal, state, and local government excluding state and local schools and hospitals (NAICS 999). For those who show up in the employment data (and are not enrolled) at some point within the focal year, we count them as employed in this sector if they worked at least one quarter during that year in one of these selected industries.

4. *Percent ever claiming unemployment since graduating.* This is intended to capture particularly negative employment outcomes that might carry additional weight in policy makers’ social welfare function and might not be captured by average earnings. This is computed only for those who show up in the employment data at some point within the focal year.⁸ UI claims data are only available from 2004 to 2012; therefore, this metric has only been estimated for two cohorts of graduates. As opposed to the other outcomes, this is a cumulative metric and thus is not restricted by enrollment status within the focal year.

5. *Degree completion (or transfer) rates.* For four-year first-time degree-seeking entrants, we examine BA/BS completion within six years of entry. For two-year first-time degree-seeking entrants, we include completion of any credential, including short-term certificates (less than one year), long-term certificates (more than one year), and associate’s degrees, as well as students who transferred to a four-year institution within three years of entry. We count students as completers regardless of whether they completed at their entry institution. Note, however, that the data only track students in

7. We considered using median earnings instead of average earnings to diminish the role of outliers. However, medians are more cumbersome to work with in our regression-adjusted models. In sensitivity testing not shown, we found that average earnings after top coding are very similar to medians, so we stick with averages for simplicity.

8. In addition to helping address concerns about out-of-state mobility, this also helps address another concern: individuals cannot claim UI unless they have worked enough in the past year to meet minimum eligibility criteria. This could introduce some ambiguity about whether claiming UI might actually be considered a good outcome, particularly among marginally attached workers. Our extract of the data do not contain the details necessary to precisely determine UI eligibility; however, in our data, about two thirds of those who worked at all during the year have earnings suggesting they are likely to be eligible. (In Ohio, individuals must have at least 20 weeks of work in the past year with average weekly earnings of at least \$243.)

public Ohio institutions, so students who transfer to a private institution or out of state will not be counted here.

Key analysis groups. For the labor market metrics, we use six cohorts of baccalaureate and subbaccalaureate graduates/transfers who earned their first degree or certificate (or transferred, for two-year students) between the 2000 and 2005 school years. We examine baccalaureate and subbaccalaureate institutions separately in all analyses.

For our academic metric, we use eight cohorts of first-time college students in 2000–2007, admitted as first-time undergraduates between ages 15 and 60. Students enrolled in a four-year institution whose academic intentions at first entry were to obtain a certificate or associate in arts (AA) / associate in science (AS) were excluded from this sample.

Given that in the HEI system, baccalaureate degrees awarded are recorded at the institution level and our analysis is at the *campus* level, we use the last campus of enrollment before earning the first BA/BS degree.⁹ We restrict our sample to Ohio residents.¹⁰ We further exclude students in the BA/BS sample who were enrolled in a two-year institution during their last semester of enrolment (this is not many students and simplifies our analysis).

This sample consists of 172,541 baccalaureate students from 39 four-year main and regional branch campuses and 79,255 subbaccalaureate students from 32 two-year colleges and campuses (which include community colleges, technical colleges, and state community colleges). Finally, however, we exclude from our analysis two medical institutions and some small campuses that had fewer than 100 students in the analysis sample for all outcomes. This brings the number of campuses to 30 at the BA/BS level and 28 for the two-year sample.

Computing mean outcomes. The first and simplest thing to do once outcomes are constructed is to compute mean outcomes by campus. It is also straightforward to compute them by program or program-campus; for simplicity, we focus on campus. An obvious concern, however, is that differences in outcomes across campus will reflect many factors other than institutional performance: they could reflect differences in students' fields of study, background characteristics (age, race, gender), or differences in local labor markets. This suggests the need to adjust these observed means for compositional differences, a process we describe below.

Computing regression-adjusted institutional fixed effects. The institutional "fixed effect" is simply the estimated contribution of the institution to students' outcomes after accounting for other factors via regression analysis. If no other factors are included in the regression, the fixed effect is equivalent

9. In the remainder of the analysis, we use *institution* and *campus* interchangeably to refer to campus-level estimates, unless specifically noted.

10. In the event the zip code at entry is missing, we assume individuals are residents as long as they are not otherwise identified as international students.

to the unadjusted institutional mean. Our most complete regression model (run separately for two-year and four-year institutions) is the following, run on the individual-level data (we run this without a constant in order to estimate a full set of institutional fixed effects):

$$(1) \quad y_i = instFE + majorFE + \delta X_i + ZLchrs + \gamma_c + \varepsilon_i,$$

where i indexes individuals; y_i is a labor market or academic outcome; $instFE$ is a vector of institutional fixed effects (FEs; entered as a set of dummy variables indicating the institution initially attended); $majorFE$ is a vector of discipline areas (measured upon college entry) using the two-digit Classification of Instructional Programs (CIP) major category,¹¹ X_i is a vector of individual background characteristics, including gender, race/ethnicity, age, and dummy variables for missing values in student characteristics; and $ZLchrs$ is a vector of five-digit zip code characteristics taken from the 2007–11 American Community Survey (ACS) five-year estimates that include economic and demographic characteristics.¹² Cohort fixed effects, γ_c , are also included to ensure that institutions' graduates (or entrants) are compared against others graduating (or entering) in the same year. We add these covariates in groups to help understand which appear most important. Because college major is not necessarily a fixed student characteristic but is potentially endogenously influenced by institutions, we add majors last. We note, however, that majors declared at entry are potentially less influenced by institutions than degree fields measured at graduation.

Controlling for zip-code-level characteristics is a way to account for both regional differences in family wealth / socioeconomic status (SES), which we have no other way to capture, as well as differences in local labor markets.¹³ Note that zip codes are measured at initial enrollment, not the time of actual employment. This is preferable because controlling for location at employment (which we do not have, in any case) could potentially absorb some of

11. We use the 2010 CIP list to create discipline areas. Based on the CIP list, we have the following discipline areas: arts and humanities, business, education, engineering, health, law, natural science and mathematics, services, social and behavioral sciences, and other, which includes trades and repair technicians and undeclared/interdisciplinary. Note that we exclude individuals with missing majors at entry, which are less than 2 percent of the sample.

12. Five-digit zip codes were reported on the admissions application and merged with census data. These zip code characteristics include percent unemployment; percent in labor force; median household income; per capita income; percent of people below the poverty line; median age; percent of white, African American, and other ethnicities; total population of Hispanics; total population 18 to 24 years old; total population 25 years and older; percent population with less than 9 years of schooling; percent population with 9 to 12 years of schooling; percent population with high school education; percent population with some college education; percent population with associate's degree; percent population with less than 9 years of schooling; and percent population with less than 9 years of schooling.

13. Alternatively, we could control for zip code fixed effects (and we did so in a prior version, with broadly similar results). A potential concern with zip code fixed effects, however, is that this may absorb some of the true institutional effects, particularly for institutions that attract a predominantly local population.

the real impacts of a successful education if graduates migrate to stronger labor markets in state.

For first-time college students enrolled in a two-year institution, we also add fixed effects for different categories of students' declared intent at entry (e.g., upgrade skills, train for a new career, transfer before completing, obtain an AA/AS degree). Note that for the academic metrics, we are using age at entry, while for labor market metrics we use age as reported at graduation.

Standardizing institutional means / fixed effects. Once the institutional fixed effects are estimated, an entirely separate challenge is what to do with them. It can be particularly difficult to detect patterns across metrics when the metrics are all in different natural scales. While the simplest solution might be to simply rank the institutions on each metric and compare the ranks, this is also limiting because the ranks eliminate valuable information on how far apart institutions are from each other—a small difference in ranks could represent a huge difference in institutional outcomes for some measures but not others or could represent large differences in the tails of the distribution but not in the middle.

We thus take the middle path of standardizing the institution-level fixed effects by subtracting the overall mean and dividing by the standard deviation. The result is a standardized rating metric that expresses how far above or below the mean the institution is in standard deviation units for that outcome. This allows us to more easily compare across our different metrics, but note that it produces inherently relative ratings. If policy makers were to use this standardization process in practice, it might make sense to standardize using the mean and standard deviation for an earlier cohort so that institutions could show improvement over time. Note that this standardization is performed separately for four-year and two-year institutions.

3.5 Results

3.5.1 Baccalaureate Institutions

Role of adjustments. To first explore the role of compositional adjustments, tables 3.1–3.4 present, for each of our four labor market metrics, unadjusted institution means side by side with institution fixed effects measured with increasingly rich student-level controls. For ease of comparability across models and outcomes, the institutional fixed effects are standardized to mean zero and a standard deviation of one. Note that since model 1 contains no controls, the standardized fixed effects in this column are identical to the standardized raw means. Each table also shows, near the bottom, how each set of metrics correlates with our most fully adjusted model. The pattern of correlations indicates which analytic choices are particularly consequential for the resulting ratings and which are not.

Several interesting findings emerge from these tables. In general, adjusted

Table 3.1 Institutional fixed effects: Conditional earnings, year 4 postgraduation

| Institution code | Baccalaureate institutions | | | | |
|-------------------------------|----------------------------|----------------------------------|--|--|---|
| | Raw mean (\$) | Model 1 <i>No adjustments</i> | Model 2 <i>Adjusted for student characteristics</i> | Model 3 <i>Adjusted for 2 plus zip-level controls</i> | Model 4 <i>Adjusted for 3 plus majors at entry</i> |
| camp_17 | 52,988 | 1.03 | 1.50 | 1.44 | 1.97 |
| camp_06 | 55,183 | 1.68 | 2.16 | 2.19 | 1.44 |
| camp_04 | 55,099 | 1.66 | 1.83 | 1.89 | 1.38 |
| camp_12 | 52,755 | 0.96 | 0.54 | 0.64 | 1.34 |
| camp_16 | 51,642 | 0.63 | 0.84 | 0.84 | 1.20 |
| camp_14 | 53,747 | 1.26 | 0.24 | 0.27 | 0.90 |
| camp_27 | 53,547 | 1.20 | 1.39 | 1.45 | 0.87 |
| camp_19 | 52,127 | 0.78 | 1.05 | 0.99 | 0.73 |
| camp_07 | 51,747 | 0.66 | 0.73 | 0.67 | 0.55 |
| camp_18 | 50,318 | 0.24 | 0.32 | 0.25 | 0.52 |
| camp_23 | 49,489 | -0.01 | 0.26 | 0.18 | 0.48 |
| camp_20 | 49,787 | 0.08 | -0.22 | -0.09 | 0.44 |
| camp_15 | 51,335 | 0.54 | -0.45 | -0.39 | 0.11 |
| camp_01 | 51,061 | 0.46 | 0.53 | 0.47 | 0.08 |
| camp_29 | 49,699 | 0.05 | 0.29 | 0.30 | -0.08 |
| camp_03 | 50,384 | 0.26 | 0.31 | 0.19 | -0.11 |
| camp_10 | 49,126 | -0.12 | -0.25 | -0.25 | -0.27 |
| camp_22 | 46,913 | -0.77 | -0.62 | -0.65 | -0.34 |
| camp_24 | 44,895 | -1.37 | -1.34 | -1.09 | -0.39 |
| camp_02 | 47,383 | -0.63 | -0.35 | -0.52 | -0.44 |
| camp_28 | 47,174 | -0.70 | -0.60 | -0.73 | -0.45 |
| camp_05 | 49,853 | 0.10 | -0.19 | -0.31 | -0.53 |
| camp_25 | 48,159 | -0.40 | -0.57 | -0.47 | -0.60 |
| camp_11 | 47,424 | -0.62 | -0.34 | -0.33 | -0.63 |
| camp_26 | 47,899 | -0.48 | -0.58 | -0.26 | -0.83 |
| camp_30 | 48,056 | -0.43 | -0.52 | -0.51 | -0.88 |
| camp_09 | 48,276 | -0.37 | -0.96 | -1.00 | -0.97 |
| camp_21 | 44,113 | -1.60 | -1.39 | -1.51 | -1.32 |
| camp_13 | 45,029 | -1.33 | -1.62 | -1.67 | -1.80 |
| camp_08 | 40,293 | -2.74 | -1.98 | -1.98 | -2.37 |
| Institution-level mean | 49,517 | 0.00 | 0.00 | 0.00 | 0.00 |
| Institution-level SD | 3,367 | 1.00 | 1.00 | 1.00 | 1.00 |
| Correlations between] metrics | | | | | |
| Model 1 | | 1.00 | | | |
| Model 2 | | 0.92 | 1.00 | | |
| Model 3 | | 0.93 | 0.99 | 1.00 | |
| Model 4 | | 0.91 | 0.90 | 0.91 | 1.00 |
| Observations | 66,695 | 66,695 | 66,695 | 66,695 | 66,695 |

Notes: Institutions sorted by model 4 effects. Earnings are measured for nonenrolled graduates in four consecutive quarters in the fourth full year postgraduation and are measured conditional on our proxy of full-time, full-year employment (see text for additional details), so the overall average of \$49,517 is among those employed full-time, full-year in state and not still enrolled in that year.

Table 3.2 Institutional fixed effects: Full-time, full-year employment (proxy), year 4 postgraduation

| Baccalaureate institutions | | | | | |
|------------------------------|----------|----------------------------------|--|--|---|
| Institution code | Raw mean | Model 1 <i>No adjustments</i> | Model 2 <i>Adjusted for student characteristics</i> | Model 3 <i>Adjusted for 2 plus zip-level controls</i> | Model 4 <i>Adjusted for 3 plus majors at entry</i> |
| camp_12 | 0.80 | 1.97 | 2.01 | 2.28 | 2.48 |
| camp_10 | 0.77 | 1.25 | 1.30 | 1.67 | 1.47 |
| camp_20 | 0.76 | 0.94 | 1.21 | 0.95 | 1.47 |
| camp_18 | 0.77 | 1.38 | 1.41 | 1.21 | 1.21 |
| camp_17 | 0.75 | 0.76 | 0.61 | 0.63 | 0.73 |
| camp_07 | 0.73 | 0.35 | 0.56 | 0.63 | 0.64 |
| camp_29 | 0.75 | 0.71 | 0.76 | 0.78 | 0.64 |
| camp_02 | 0.75 | 0.74 | 0.61 | 0.52 | 0.53 |
| camp_11 | 0.73 | 0.25 | 0.13 | 0.52 | 0.50 |
| camp_28 | 0.77 | 1.28 | 1.38 | 0.78 | 0.47 |
| camp_01 | 0.74 | 0.61 | 0.61 | 0.58 | 0.47 |
| camp_06 | 0.73 | 0.30 | 0.61 | 0.66 | 0.44 |
| camp_15 | 0.74 | 0.53 | 0.18 | 0.52 | 0.32 |
| camp_23 | 0.73 | 0.17 | -0.04 | -0.03 | 0.26 |
| camp_22 | 0.71 | -0.11 | 0.01 | -0.17 | 0.11 |
| camp_04 | 0.73 | 0.40 | 0.36 | 0.37 | 0.11 |
| camp_16 | 0.72 | 0.12 | -0.04 | -0.09 | -0.01 |
| camp_19 | 0.72 | -0.09 | -0.24 | -0.17 | -0.09 |
| camp_27 | 0.73 | 0.33 | 0.27 | 0.29 | -0.09 |
| camp_21 | 0.70 | -0.40 | -0.39 | -0.84 | -0.36 |
| camp_25 | 0.70 | -0.52 | -0.47 | -0.72 | -0.45 |
| camp_08 | 0.64 | -2.10 | -0.73 | -0.61 | -0.89 |
| camp_26 | 0.70 | -0.45 | -0.61 | -0.64 | -0.89 |
| camp_13 | 0.67 | -1.17 | -1.44 | -1.07 | -0.92 |
| camp_05 | 0.69 | -0.63 | -0.79 | -0.92 | -1.04 |
| camp_14 | 0.68 | -1.04 | -1.18 | -0.87 | -1.07 |
| camp_24 | 0.66 | -1.40 | -1.44 | -1.59 | -1.25 |
| camp_30 | 0.68 | -0.91 | -1.13 | -1.15 | -1.31 |
| camp_03 | 0.66 | -1.40 | -1.36 | -1.67 | -1.66 |
| camp_09 | 0.65 | -1.89 | -2.15 | -1.85 | -1.81 |
| Institution-level mean | 0.72 | 0.00 | 0.00 | 0.00 | 0.00 |
| Institution-level SD | 0.04 | 1.00 | 1.00 | 1.00 | 1.00 |
| Correlations between metrics | | | | | |
| Model 1 | | 1.00 | | | |
| Model 2 | | 0.95 | 1.00 | | |
| Model 3 | | 0.93 | 0.97 | 1.00 | |
| Model 4 | | 0.93 | 0.96 | 0.97 | 1.00 |
| Observations | 91,600 | 91,600 | 91,600 | 91,600 | 91,600 |

Notes: Full-time, full-year employment is estimated by examining four consecutive quarters in the fourth full year postgraduation and requires a graduate to earn above a minimum amount in each quarter corresponding to 35 hours per week at minimum wage. The sample is restricted to graduates who are not still enrolled and have positive earnings in at least one quarter of the focal year.

Table 3.3 Institutional fixed effects: Social sector employment (proxy), year 4 postgraduation

| Baccalaureate institutions | | | | | |
|------------------------------|----------|----------------------------------|--|--|---|
| Institution code | Raw mean | Model 1 <i>No adjustments</i> | Model 2 <i>Adjusted for student characteristics</i> | Model 3 <i>Adjusted for 2 plus zip-level controls</i> | Model 4 <i>Adjusted for 3 plus majors at entry</i> |
| camp_20 | 0.44 | 1.62 | 1.60 | 1.56 | 1.55 |
| camp_25 | 0.46 | 1.82 | 1.70 | 1.63 | 1.47 |
| camp_05 | 0.35 | 0.76 | 0.82 | 1.05 | 1.29 |
| camp_24 | 0.49 | 2.09 | 2.07 | 1.88 | 1.13 |
| camp_26 | 0.35 | 0.77 | 0.81 | 0.51 | 0.97 |
| camp_08 | 0.36 | 0.81 | 0.82 | 0.90 | 0.86 |
| camp_22 | 0.33 | 0.53 | 0.43 | 0.39 | 0.74 |
| camp_19 | 0.20 | -0.60 | -0.40 | -0.31 | 0.45 |
| camp_17 | 0.27 | -0.01 | 0.22 | 0.38 | 0.42 |
| camp_23 | 0.25 | -0.16 | 0.08 | 0.13 | 0.42 |
| camp_21 | 0.35 | 0.78 | 0.63 | 0.41 | 0.40 |
| camp_29 | 0.26 | -0.06 | 0.03 | 0.04 | 0.39 |
| camp_07 | 0.24 | -0.28 | -0.24 | -0.10 | 0.30 |
| camp_28 | 0.47 | 1.91 | 1.77 | 1.83 | 0.06 |
| camp_18 | 0.35 | 0.72 | 0.73 | 0.85 | 0.05 |
| camp_01 | 0.23 | -0.38 | -0.33 | -0.31 | 0.05 |
| camp_04 | 0.17 | -0.97 | -0.84 | -0.83 | -0.01 |
| camp_02 | 0.26 | -0.07 | 0.11 | 0.22 | -0.02 |
| camp_11 | 0.21 | -0.51 | -0.44 | -0.33 | -0.07 |
| camp_30 | 0.25 | -0.16 | -0.13 | -0.24 | -0.14 |
| camp_27 | 0.18 | -0.81 | -0.65 | -0.69 | -0.19 |
| camp_06 | 0.17 | -0.94 | -1.19 | -1.18 | -0.32 |
| camp_03 | 0.29 | 0.19 | -0.08 | -0.11 | -0.32 |
| camp_13 | 0.19 | -0.75 | -0.74 | -0.70 | -0.43 |
| camp_16 | 0.20 | -0.67 | -0.53 | -0.53 | -0.47 |
| camp_09 | 0.23 | -0.40 | -0.61 | -0.57 | -0.75 |
| camp_10 | 0.13 | -1.28 | -1.28 | -1.26 | -1.06 |
| camp_12 | 0.11 | -1.48 | -1.66 | -1.80 | -1.91 |
| camp_15 | 0.18 | -0.81 | -0.76 | -0.77 | -1.93 |
| camp_14 | 0.09 | -1.65 | -1.93 | -2.04 | -2.93 |
| Institution-level mean | 0.27 | 0.00 | 0.00 | 0.00 | 0.00 |
| Institution-level SD | 0.11 | 1.00 | 1.00 | 1.00 | 1.00 |
| Correlations between metrics | | | | | |
| Model 1 | | 1.00 | | | |
| Model 2 | | 0.99 | 1.00 | | |
| Model 3 | | 0.98 | 0.99 | 1.00 | |
| Model 4 | | 0.77 | 0.82 | 0.83 | 1.00 |
| Observations | 91,600 | 91,600 | 91,600 | 91,600 | 91,600 |

Notes: Social sector employment is estimated by examining four consecutive quarters in the fourth full year postgraduation and requires a graduate to have been employed in educational services or government in at least one of these quarters. Sample is limited to graduates who are not still enrolled and who have positive earnings in at least one quarter of the focal year.

Table 3.4 Institutional fixed effects: Cumulative UI receipt, year 4 postgraduation

| Baccalaureate institutions | | | | | |
|------------------------------|----------|----------------------------------|--|--|---|
| Institution code | Raw mean | Model 1 <i>No adjustments</i> | Model 2 <i>Adjusted for student characteristics</i> | Model 3 <i>Adjusted for 2 plus zip-level controls</i> | Model 4 <i>Adjusted for 3 plus majors at entry</i> |
| camp_17 | 0.06 | 1.03 | 1.01 | 1.04 | 1.09 |
| camp_19 | 0.08 | 0.70 | 0.81 | 0.83 | 0.91 |
| camp_23 | 0.07 | 0.74 | 0.72 | 0.77 | 0.84 |
| camp_22 | 0.08 | 0.57 | 0.58 | 0.54 | 0.68 |
| camp_20 | 0.09 | 0.49 | 0.60 | 0.65 | 0.68 |
| camp_26 | 0.09 | 0.49 | 0.56 | 0.74 | 0.68 |
| camp_04 | 0.09 | 0.42 | 0.51 | 0.58 | 0.66 |
| camp_21 | 0.08 | 0.68 | 0.69 | 0.56 | 0.57 |
| camp_28 | 0.06 | 1.03 | 0.96 | 0.72 | 0.54 |
| camp_29 | 0.09 | 0.44 | 0.45 | 0.56 | 0.54 |
| camp_18 | 0.08 | 0.64 | 0.58 | 0.58 | 0.52 |
| camp_13 | 0.10 | 0.33 | 0.36 | 0.29 | 0.45 |
| camp_12 | 0.09 | 0.42 | 0.51 | 0.40 | 0.41 |
| camp_16 | 0.10 | 0.21 | 0.18 | 0.20 | 0.27 |
| camp_24 | 0.10 | 0.25 | 0.22 | 0.27 | 0.27 |
| camp_07 | 0.12 | -0.08 | 0.09 | 0.20 | 0.25 |
| camp_01 | 0.10 | 0.21 | 0.22 | 0.22 | 0.22 |
| camp_02 | 0.10 | 0.33 | 0.20 | 0.22 | 0.13 |
| camp_05 | 0.10 | 0.23 | 0.13 | 0.18 | 0.11 |
| camp_09 | 0.12 | -0.05 | 0.13 | 0.09 | 0.09 |
| camp_11 | 0.10 | 0.18 | 0.07 | 0.02 | 0.02 |
| camp_27 | 0.11 | 0.01 | 0.00 | 0.09 | 0.02 |
| camp_30 | 0.12 | -0.03 | 0.00 | -0.03 | -0.12 |
| camp_25 | 0.11 | -0.01 | -0.02 | -0.05 | -0.12 |
| camp_06 | 0.13 | -0.34 | -0.49 | -0.44 | -0.41 |
| camp_15 | 0.16 | -0.87 | -0.78 | -0.89 | -0.89 |
| camp_10 | 0.16 | -0.81 | -1.01 | -1.09 | -1.05 |
| camp_03 | 0.16 | -0.77 | -0.92 | -1.00 | -1.26 |
| camp_08 | 0.30 | -3.49 | -3.07 | -2.81 | -2.67 |
| camp_14 | 0.27 | -2.97 | -3.29 | -3.45 | -3.44 |
| Institution-level mean | 0.11 | 0.00 | 0.00 | 0.00 | 0.00 |
| Institution-level SD | 0.05 | 1.00 | 1.00 | 1.00 | 1.00 |
| Correlations between metrics | | | | | |
| Model 1 | | 1.00 | | | |
| Model 2 | | 0.99 | 1.00 | | |
| Model 3 | | 0.98 | 0.99 | 1.00 | |
| Model 4 | | 0.97 | 0.99 | 1.00 | 1.00 |
| Observations | 35,317 | 35,317 | 35,317 | 35,317 | 35,317 |

Notes: Institutional fixed effects (FE) from models 1–4 are reverse coded so that lower rates of UI receipt correspond to more positive standardized FE. Cumulative UI receipt is measured as the percent ever receiving UI or other unemployment compensation by the end of the fourth full year postgraduation. To limit bias from out-of-state mobility, sample is limited to those with positive earnings in at least one quarter of the focal year.

Table 3.5 Correlations of adjusted institution-level metrics

| Correlations | Baccalaureate institutions | | | | |
|--------------------------------------|----------------------------|---------------|--------------|----------------|------------|
| | ba6yr | Ftemp 4yrs | Earn 4yrs | SS sec 4yrs | UI 4yrs |
| ba6yr | 1.000 | — | — | — | — |
| Ftemp 4yrs | 0.176 | 1.000 | — | — | — |
| Earn 4yrs | 0.215 | 0.497*** | 1.000 | — | — |
| SS sec 4yrs | 0.226 | -0.117 | -0.277 | 1.000 | — |
| UI 4yrs | 0.325* | 0.316* | 0.202 | 0.492*** | 1.000 |
| Avg. diff. vs. BA metric (SDs) | 0.00 | 1.01 | 1.00 | 0.97 | 0.93 |
| Avg. diff. vs. earnings metric (SDs) | 1.00 | 0.86 | 0.00 | 1.24 | 0.94 |

Note: *** $p < .01$, ** $p < .05$, * $p < .10$.

and unadjusted metrics are very highly positively correlated. Across our four metrics, the correlation between the unadjusted effects and fully adjusted effects ranges from 0.77 (for our social service employment metric) to 0.97 (for our full-time, full-year employment proxy and for our UI receipt metric). For earnings, the correlation is 0.91. Zip-code-level controls appear the least important controls across all outcomes: the correlations between models 2 and 3 are 0.97 or higher across the board. Controlling for field of study makes a particularly large difference for social sector employment.

Even high correlations, however, can mask substantial movement in institutions' ratings and rankings. For our earnings metric, for example, the average institution swung by about half of a standard deviation across the four models (i.e., from its most favorable rating to its least favorable rating), or by 5 positions in the rankings of these 30 institutions.¹⁴ One institution's rank swung by 11 positions depending on which controls were added. Rankings and ratings based on social sector employment rates were similarly volatile. Adjustments matter less for the full-time, full-year employment proxy and UI receipt metric: the average swings were only about 0.39 and 0.20 of a standard deviation, respectively (or about 3 positions in the rankings in both cases).

Correlations across metrics. Table 3.5 and figure 3.1 examine the relationship among our five different metrics using estimates from the fully adjusted model. In figure 3.1, each vertically aligned set of points represents an institution's rating on one of our five measures (standardized to mean zero and SD of one to enable comparisons across metrics). If a point lies above zero, that indicates the institution rates above average on that metric. A point at -2, on the other hand, would indicate an institution fell two standard deviations below the institutional mean for that metric. To the extent all points

14. Even just considering the first three models, which are all correlated at 0.93 or above for the earnings outcome, the average institution swung by 0.32 standard deviations or 3 positions in the rankings of these 30 institutions.

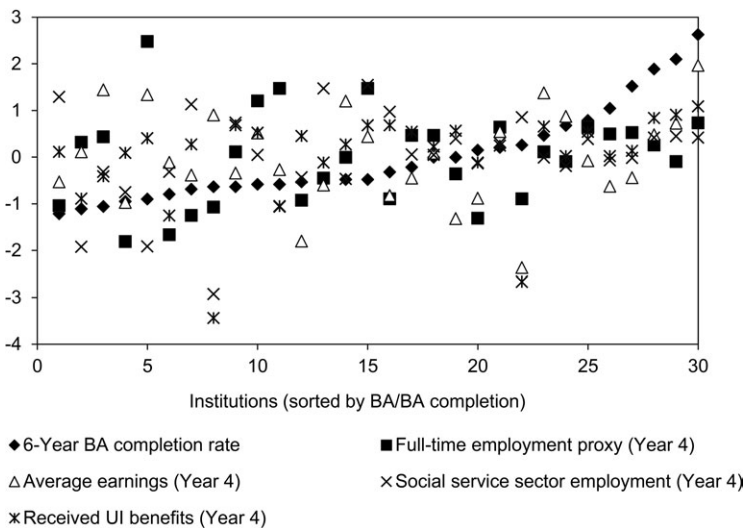


Fig. 3.1 Adjusted institution-level metrics (standardized)

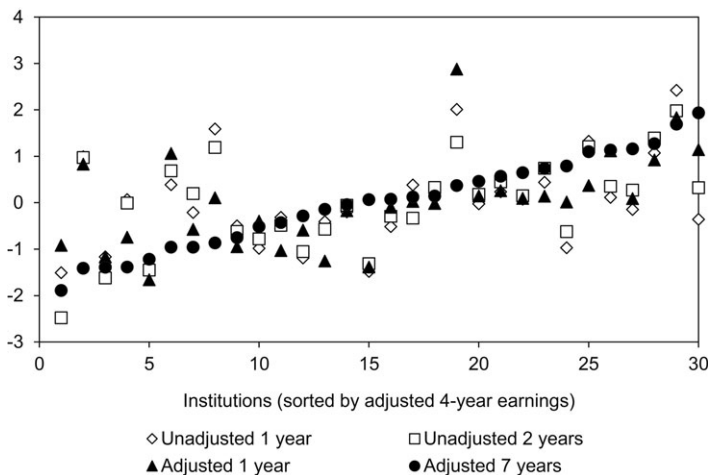


Fig. 3.2 Alternative earnings metrics (standardized)

for a given institution are very tightly clustered, that indicates consistency in the institution’s rating across metrics. If the points are very far apart vertically (i.e., for a given institution), it means that an institution’s rating could be dramatically different depending on the measure used. To help reveal patterns in the data, the graph is sorted by the degree completion metric, with the lowest-ranking institution on this metric on the left and the highest-ranking on the right. This makes it easy to identify how top institutions on this metric fare on the labor market metrics and vice versa.

Most metrics are positively correlated with each other, though not often not significantly so; they do seem to capture different information. Degree completion rates correlate positively with all other metrics, but the correlation is only significant for our (reverse-coded) UI metric, at 0.33 (the other correlations hover around 0.2). Social sector employment is negatively correlated with both employment and earnings (though not significantly so). Interestingly, the correlation between social sector employment and rates of UI receipt is almost as high as the correlation between employment and earnings (both around 0.49).

In practice, the average difference between an institution's rating on the degree completion metric and its rating on a given labor market metric ranges from 0.93 to 1.01 standard deviations; the average swing across all five metrics is a full 2 standard deviations (or 17 positions in rank). Even just among the four labor market metrics, the average swing is 1.6 standard deviations.

This seemingly large variation in ratings for a single institution is depicted visually in figure 3.1. Each institution's ratings (in standard deviation units) are plotted along a vertical line, and the institutions are sorted from left to right by degree completion rates. The graph illustrates both the general positive correlation among the metrics as well as the dispersion for each institution. The graph also highlights that the dispersion of the labor market metrics is much greater for institutions with low degree completion rates than for those with high degree completion rates.¹⁵

Correlations of metrics over time. How sensitive are these labor market metrics to different lengths of follow-up? Tables 3.6 and 3.7 explore this question from different angles. First, we examine the correlation of the same metric measured at different points in time. Table 3.6 shows that our adjusted measures are generally positively and significantly correlated over time, with the social sector employment metric having the greatest stability and the full-time employment proxy having the least. In the case of the full-time employment proxy, the one-year and seven-year metrics are barely significantly correlated ($\rho = 0.33$), suggesting these measures may be quite misleading if measured too soon after graduation.¹⁶ The full-time employment proxy also may be particularly sensitive to out-of-state mobility: the sample for which this statistic is computed (those present in the earnings data in a given year) shrinks on average by about 25 percent between the one-year and seven-year follow-up, while in contrast the sample size for our earnings metric (which is conditional on the full-time employment proxy) remains fairly stable over time.

Table 3.7 and figure 3.3 suggest that at least for the conditional earnings

15. We also examined versions of table 3.5 and figure 3.1 using the raw (unadjusted) versions of our five metrics. Overall, whether or not controls are included does not make much difference to the cross-metric correlations. The exception to this is the social sector employment measure, likely because of its sensitivity to field-of-study controls. When no controls are included, this measure is significantly negatively correlated with earnings and no longer significantly correlated with our UI metric (though the positive direction remains the same).

16. We find broadly similar patterns when we perform the same analysis with the unadjusted metrics.

Table 3.6 Correlations of adjusted LM metrics over time

| | Year 1 | Year 2 | Year 4 | Year 7 |
|--------------------------------------|----------|----------|----------|--------|
| <i>A. Full-time employment proxy</i> | | | | |
| Year 1 | 1.000 | | | |
| Year 2 | 0.713*** | 1.000 | | |
| Year 4 | 0.592*** | 0.730*** | 1.000 | |
| Year 7 | 0.331* | 0.266 | 0.337* | 1.000 |
| <i>B. Conditional earnings</i> | | | | |
| Year 1 | 1.000 | — | — | — |
| Year 2 | 0.933*** | 1.000 | — | — |
| Year 4 | 0.765*** | 0.881*** | 1.000 | — |
| Year 7 | 0.553*** | 0.672*** | 0.874*** | 1.000 |
| <i>C. Social sector employment</i> | | | | |
| Year 1 | 1.000 | — | — | — |
| Year 2 | 0.982*** | 1.000 | — | — |
| Year 4 | 0.965*** | 0.955*** | 1.000 | — |
| Year 7 | 0.957*** | 0.948*** | 0.963*** | 1.000 |
| <i>D. UI receipt (inverse)</i> | | | | |
| Year 1 | 1.000 | — | — | — |
| Year 2 | 0.828*** | 1.000 | — | — |
| Year 4 | 0.864*** | 0.886*** | 1.000 | — |
| Year 7 | 0.550*** | 0.745*** | 0.769*** | 1.000 |

Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 3.7 Correlations between adjusted and unadjusted metrics

| Metric (standardized) | Year 1 | Year 2 | Year 4 | Year 7 |
|----------------------------|--------|--------|--------|--------|
| Full-time employment proxy | 0.920 | 0.956 | 0.931 | 0.941 |
| Conditional earnings | 0.778 | 0.828 | 0.905 | 0.905 |
| Social sector employment | 0.679 | 0.701 | 0.770 | 0.693 |
| UI receipt | 0.989 | 0.978 | 0.967 | 0.965 |

Note: $p < 0.01$ for all correlations in this table.

metric, analysts may be able to choose between performing statistical adjustments and following graduates for a longer period of time, depending on which is more feasible. Just between year 1 and year 2, the correlation between the adjusted and unadjusted earnings metric grows from 0.78 to 0.83 and then to 0.91 by year 4. Figure 3.3 further illustrates this. The figure shows that unadjusted earnings after one year (the most common and feasible way to measure earnings) are only modestly correlated ($r = 0.38$) with a fully adjusted metric after seven years (which might be the most preferred measure except for the inconvenience of waiting that long). However, adjusting the earnings measure after one year is just as good as using an unadjusted measure after two years: both improve the correlation with seven-year earnings to 0.54.

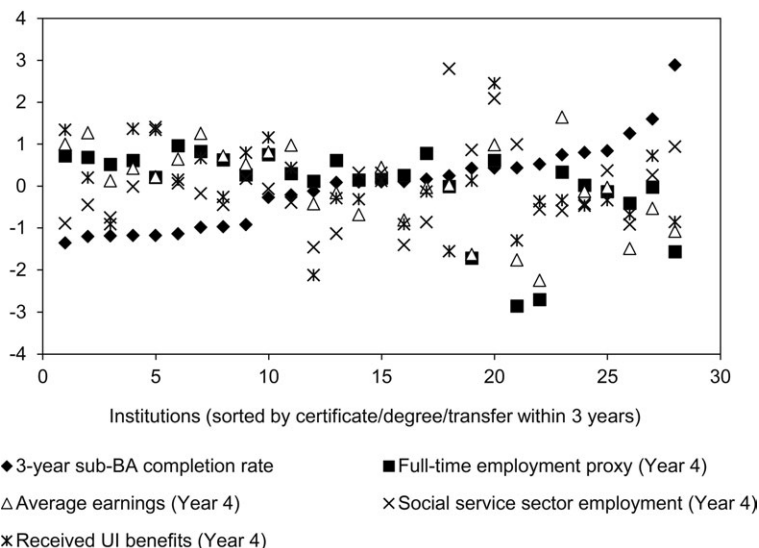


Fig. 3.3 Adjusted metrics for sub-BA institutions

3.5.2 Subbaccalaureate Institutions

Role of adjustments. We repeat the same set of analyses, but this time for subbaccalaureate institutions. Tables 3.8–3.11 present unadjusted and adjusted versions of each of our four labor market metrics, measured four years postgraduation. Again, adjusted and unadjusted versions are always positively correlated, ranging from 0.84 (for our earnings metric) to 0.95 (for our social service employment and UI metrics). The typical ratings swing across model adjustments ranges from about one-third of a standard deviation for the full-time employment proxy, UI, and social sector employment metrics to about half of a standard deviation for the earnings metric. In general, this is quite similar to what we found in the four-year sector.

Correlations across metrics. Table 3.12 and figure 3.3 examine the relationships among our five metrics using estimates from our fully adjusted model.¹⁷ In notable contrast to the four-year sector, we see here that institution-level earnings and employment are both strongly *negatively* correlated with our completion metric (around -0.53 for both metrics), while our measure of social sector employment is positively but not significantly correlated (0.18).¹⁸ The average ratings swing across these five metrics is 2.2 standard deviations.

17. We find broadly similar patterns if we use unadjusted versions of these metrics.

18. Another notable finding is that the UI metric is not as strongly correlated with the other labor market metrics in the two-year sector as compared with the four-year sector. This may be because of the issue raised earlier, that to claim UI, graduates have to have held a job for at least 20 weeks with average earnings above \$243 per week. An institution could thus do “well” on this measure either because its graduates are rarely unemployed or because they rarely work long enough to qualify for unemployment benefits. We thank Lawrence Katz for raising this point.

Table 3.8 Institutional fixed effects: Conditional earnings, year 4 postgraduation

| Subbaccalaureate institutions | | | | | |
|-------------------------------|---------------|----------------------------------|--|--|---|
| Institution code | Raw mean (\$) | Model 1 <i>No adjustments</i> | Model 2 <i>Adjusted for student characteristics</i> | Model 3 <i>Adjusted for 2 plus zip-level controls</i> | Model 4 <i>Adjusted for 3 plus majors at entry</i> |
| camp_23 | 46,940 | 0.37 | 0.65 | 0.92 | 1.65 |
| camp_08 | 49,261 | 1.07 | 1.02 | 0.94 | 1.27 |
| camp_06 | 49,592 | 1.17 | 1.21 | 1.08 | 1.26 |
| camp_07 | 49,985 | 1.29 | 1.48 | 1.52 | 1.00 |
| camp_21 | 46,722 | 0.30 | 0.61 | 0.94 | 0.98 |
| camp_13 | 49,200 | 1.05 | 0.86 | 0.69 | 0.97 |
| camp_03 | 48,919 | 0.97 | 0.98 | 0.89 | 0.81 |
| camp_20 | 48,518 | 0.85 | 0.80 | 0.85 | 0.71 |
| camp_22 | 48,642 | 0.88 | 0.78 | 0.76 | 0.64 |
| camp_02 | 47,651 | 0.58 | 0.55 | 0.62 | 0.52 |
| camp_04 | 47,800 | 0.63 | 0.93 | 0.92 | 0.44 |
| camp_12 | 47,827 | 0.64 | 0.85 | 0.77 | 0.42 |
| camp_05 | 47,354 | 0.49 | 0.43 | 0.20 | 0.22 |
| camp_19 | 47,086 | 0.41 | 0.22 | 0.17 | 0.12 |
| camp_24 | 42,631 | -0.94 | -0.90 | -0.86 | 0.02 |
| camp_26 | 47,103 | 0.42 | 0.26 | 0.27 | -0.03 |
| camp_16 | 46,797 | 0.32 | 0.27 | 0.24 | -0.04 |
| camp_14 | 45,953 | 0.07 | 0.24 | 0.32 | -0.13 |
| camp_09 | 44,329 | -0.42 | -0.65 | -0.73 | -0.16 |
| camp_27 | 45,173 | -0.17 | -0.81 | -0.88 | -0.42 |
| camp_25 | 38,307 | -2.25 | -1.81 | -1.74 | -0.54 |
| camp_17 | 45,554 | -0.05 | -0.18 | -0.13 | -0.68 |
| camp_18 | 42,365 | -1.02 | -0.97 | -1.00 | -0.81 |
| camp_10 | 43,665 | -0.63 | -0.76 | -0.70 | -1.08 |
| camp_15 | 40,392 | -1.62 | -1.65 | -1.65 | -1.49 |
| camp_28 | 40,049 | -1.72 | -1.58 | -1.59 | -1.63 |
| camp_01 | 42,902 | -0.86 | -1.04 | -1.00 | -1.77 |
| camp_11 | 39,743 | -1.82 | -1.79 | -1.84 | -2.25 |
| Institution-level mean | 45,731 | 0.00 | 0.00 | 0.00 | 0.00 |
| Institution-level SD | 3,297 | 1.00 | 1.00 | 1.00 | 1.00 |
| Correlations between metrics | | | | | |
| Model 1 | | 1.00 | | | |
| Model 2 | | 0.98 | 1.00 | | |
| Model 3 | | 0.96 | 0.99 | 1.00 | |
| Model 4 | | 0.84 | 0.88 | 0.89 | 1.00 |
| Observations | 36,596 | 36,596 | 36,596 | 36,596 | 36,596 |

Notes: Institutional fixed effects from models 1–4 are standardized to mean 0 and SD 1. Institutions sorted by model 4 effects. Earnings are measured for four consecutive quarters in the fourth full year postgraduation and are measured conditional on our proxy of full-time, full-year employment (see text for additional details), so the overall average of \$45,731 is among those employed full-time, full-year in state in that year. Sample also restricted to those not enrolled within the focal year.

Table 3.9 Institutional fixed effects: Full-time full-year employment (proxy), year 4 postgraduation

| Subbaccalaureate institutions | | | | | |
|-------------------------------|----------|----------------------------------|--|--|---|
| Institution code | Raw mean | Model 1 <i>No adjustments</i> | Model 2 <i>Adjusted for student characteristics</i> | Model 3 <i>Adjusted for 2 plus zip-level controls</i> | Model 4 <i>Adjusted for 3 plus majors at entry</i> |
| camp_01 | 0.53 | -2.22 | -2.29 | -2.36 | -2.85 |
| camp_02 | 0.69 | 0.26 | 0.27 | 0.31 | 0.27 |
| camp_03 | 0.72 | 0.76 | 0.85 | 0.96 | 0.74 |
| camp_04 | 0.71 | 0.66 | 0.69 | 0.54 | 0.19 |
| camp_05 | 0.68 | 0.13 | 0.19 | 0.28 | 0.20 |
| camp_06 | 0.67 | -0.11 | 0.33 | 0.71 | 0.82 |
| camp_07 | 0.69 | 0.21 | 0.63 | 0.97 | 0.71 |
| camp_08 | 0.70 | 0.42 | 0.35 | 0.53 | 0.68 |
| camp_09 | 0.72 | 0.71 | 0.55 | 0.25 | 0.61 |
| camp_10 | 0.59 | -1.26 | -1.41 | -1.31 | -1.57 |
| camp_11 | 0.50 | -2.77 | -2.73 | -2.69 | -2.70 |
| camp_12 | 0.71 | 0.61 | 0.69 | 0.71 | 0.61 |
| camp_13 | 0.68 | 0.11 | 0.00 | 0.13 | 0.30 |
| camp_14 | 0.71 | 0.58 | 0.58 | 0.31 | 0.02 |
| camp_15 | 0.65 | -0.31 | -0.37 | -0.45 | -0.41 |
| camp_16 | 0.70 | 0.40 | 0.35 | 0.18 | -0.14 |
| camp_17 | 0.72 | 0.71 | 0.62 | 0.51 | 0.14 |
| camp_18 | 0.70 | 0.39 | 0.35 | 0.11 | 0.25 |
| camp_19 | 0.74 | 1.06 | 0.96 | 0.65 | 0.51 |
| camp_20 | 0.72 | 0.69 | 0.66 | 0.73 | 0.62 |
| camp_21 | 0.70 | 0.39 | 0.44 | 0.54 | 0.61 |
| camp_22 | 0.73 | 0.97 | 0.91 | 1.09 | 0.96 |
| camp_23 | 0.66 | -0.23 | -0.22 | -0.19 | 0.33 |
| camp_24 | 0.64 | -0.53 | -0.56 | -0.62 | -0.01 |
| camp_25 | 0.60 | -1.10 | -0.99 | -0.86 | -0.03 |
| camp_26 | 0.74 | 1.13 | 1.02 | 1.02 | 0.78 |
| camp_27 | 0.69 | 0.31 | 0.03 | -0.18 | 0.11 |
| camp_28 | 0.55 | -1.93 | -1.92 | -1.87 | -1.73 |
| Institution-level mean | 0.67 | 0.00 | 0.00 | 0.00 | 0.00 |
| Institution-level SD | 0.06 | 1.00 | 1.00 | 1.00 | 1.00 |
| Correlations between metrics | | | | | |
| Model 1 | | 1.00 | | | |
| Model 2 | | 0.99 | 1.00 | | |
| Model 3 | | 0.96 | 0.98 | 1.00 | |
| Model 4 | | 0.90 | 0.93 | 0.95 | 1.00 |
| Observations | 53,353 | 53,353 | 53,353 | 53,353 | 53,353 |

Notes: Institutional fixed effects from models 1–4 are standardized to mean 0 and SD 1. Institutions sorted by model 4 effects. Full-time, full-year employment is estimated by examining four consecutive quarters in the fourth full year postgraduation and requires a graduate to earn above a minimum amount in each quarter corresponding to 35 hours per week at minimum wage. The sample is restricted to graduates who are not still enrolled and have positive earnings in at least one quarter of the focal year.

Table 3.10 Institutional fixed effects: Social sector employment (proxy), year 4 postgraduation

| Subbaccalaureate institutions | | | | | |
|-------------------------------|----------|----------------------------------|--|--|---|
| Institution code | Raw mean | Model 1 <i>No adjustments</i> | Model 2 <i>Adjusted for student characteristics</i> | Model 3 <i>Adjusted for 2 plus zip-level controls</i> | Model 4 <i>Adjusted for 3 plus majors at entry</i> |
| camp_01 | 0.15 | 0.58 | 0.59 | 0.59 | 0.99 |
| camp_02 | 0.14 | 0.47 | 0.41 | 0.31 | 0.18 |
| camp_03 | 0.11 | -0.29 | -0.40 | -0.51 | -0.07 |
| camp_04 | 0.11 | -0.29 | -0.17 | -0.08 | 0.31 |
| camp_05 | 0.18 | 1.55 | 1.39 | 1.34 | 1.40 |
| camp_06 | 0.15 | 0.63 | 0.04 | -0.28 | -0.18 |
| camp_07 | 0.10 | -0.64 | -1.10 | -1.23 | -0.89 |
| camp_08 | 0.11 | -0.34 | -0.38 | -0.31 | -0.45 |
| camp_09 | 0.08 | -1.07 | -1.03 | -0.98 | -1.14 |
| camp_10 | 0.17 | 1.26 | 1.26 | 1.26 | 0.94 |
| camp_11 | 0.11 | -0.42 | -0.27 | -0.28 | -0.56 |
| camp_12 | 0.12 | -0.04 | -0.01 | -0.11 | -0.02 |
| camp_13 | 0.11 | -0.26 | -0.30 | -0.23 | -0.40 |
| camp_14 | 0.09 | -0.80 | -0.66 | -0.68 | -0.43 |
| camp_15 | 0.10 | -0.72 | -0.61 | -0.53 | -0.92 |
| camp_16 | 0.12 | 0.01 | 0.01 | 0.11 | 0.37 |
| camp_17 | 0.13 | 0.09 | 0.07 | 0.09 | 0.31 |
| camp_18 | 0.08 | -1.32 | -1.13 | -1.13 | -1.41 |
| camp_19 | 0.09 | -0.91 | -0.92 | -0.83 | -0.75 |
| camp_20 | 0.11 | -0.40 | -0.38 | -0.46 | -0.45 |
| camp_21 | 0.19 | 1.72 | 1.71 | 1.84 | 2.09 |
| camp_22 | 0.13 | 0.06 | -0.04 | -0.13 | 0.07 |
| camp_23 | 0.11 | -0.40 | -0.22 | -0.13 | -0.59 |
| camp_24 | 0.24 | 3.02 | 3.09 | 3.01 | 2.80 |
| camp_25 | 0.14 | 0.39 | 0.77 | 0.91 | 0.26 |
| camp_26 | 0.07 | -1.51 | -1.49 | -1.38 | -0.86 |
| camp_27 | 0.09 | -1.02 | -1.05 | -1.06 | -1.46 |
| camp_28 | 0.15 | 0.63 | 0.82 | 0.91 | 0.86 |
| Institution-level mean | 0.12 | 0.00 | 0.00 | 0.00 | 0.00 |
| Institution-level SD | 0.04 | 1.00 | 1.00 | 1.00 | 1.00 |
| Correlations between metrics | | | | | |
| Model 1 | | 1.00 | | | |
| Model 2 | | 0.98 | 1.00 | | |
| Model 3 | | 0.96 | 0.99 | 1.00 | |
| Model 4 | | 0.95 | 0.95 | 0.95 | 1.00 |
| Observations | 53,353 | 53,353 | 53,353 | 53,353 | 53,353 |

Notes: Institutional fixed effects from models 1–4 are standardized to mean 0 and SD 1. Institutions sorted by model 4 effects. Social sector employment is estimated by examining four consecutive quarters in the fourth full year postgraduation and requires a graduate to have been employed in educational services or government in at least one of these quarters. Sample is limited to graduates who are not still enrolled and who have positive earnings in at least one quarter of the focal year.

Table 3.11 Institutional fixed effects: Cumulative UI receipt, year 4 postgraduation

| Subbaccalaureate institutions | | | | | |
|-------------------------------|----------|----------------------------------|--|--|---|
| Institution code | Raw mean | Model 1 <i>No adjustments</i> | Model 2 <i>Adjusted for student characteristics</i> | Model 3 <i>Adjusted for 2 plus zip-level controls</i> | Model 4 <i>Adjusted for 3 plus majors at entry</i> |
| camp_01 | 0.21 | -0.82 | -0.86 | -0.78 | -1.30 |
| camp_02 | 0.13 | 0.99 | 0.96 | 0.95 | 0.80 |
| camp_03 | 0.15 | 0.62 | 1.01 | 0.88 | 1.16 |
| camp_04 | 0.15 | 0.62 | 0.40 | 0.37 | 0.12 |
| camp_05 | 0.12 | 1.28 | 1.44 | 1.19 | 1.34 |
| camp_06 | 0.17 | 0.05 | 0.72 | 0.60 | 0.67 |
| camp_07 | 0.12 | 1.11 | 1.55 | 1.58 | 1.34 |
| camp_08 | 0.15 | 0.45 | 0.47 | 0.18 | 0.20 |
| camp_09 | 0.20 | -0.65 | -0.66 | -0.73 | -0.29 |
| camp_10 | 0.20 | -0.75 | -0.62 | -0.47 | -0.86 |
| camp_11 | 0.19 | -0.33 | -0.41 | -0.19 | -0.37 |
| camp_12 | 0.11 | 1.51 | 1.37 | 1.30 | 1.36 |
| camp_13 | 0.13 | 0.99 | 0.94 | 0.58 | 0.43 |
| camp_14 | 0.18 | -0.21 | -0.34 | -0.24 | -0.47 |
| camp_15 | 0.22 | -1.05 | -1.11 | -0.94 | -0.68 |
| camp_16 | 0.18 | -0.18 | -0.21 | -0.22 | -0.34 |
| camp_17 | 0.17 | -0.07 | -0.14 | -0.12 | -0.32 |
| camp_18 | 0.22 | -1.13 | -1.20 | -1.22 | -0.91 |
| camp_19 | 0.21 | -0.98 | -0.93 | -1.01 | -0.91 |
| camp_20 | 0.19 | -0.37 | -0.25 | -0.33 | -0.26 |
| camp_21 | 0.08 | 2.08 | 1.82 | 2.17 | 2.45 |
| camp_22 | 0.16 | 0.22 | 0.35 | 0.07 | 0.15 |
| camp_23 | 0.18 | -0.11 | -0.46 | -0.22 | -0.34 |
| camp_24 | 0.22 | -1.13 | -1.32 | -1.24 | -1.56 |
| camp_25 | 0.14 | 0.80 | 0.38 | 0.77 | 0.72 |
| camp_26 | 0.19 | -0.35 | -0.30 | -0.45 | -0.13 |
| camp_27 | 0.29 | -2.68 | -2.40 | -2.60 | -2.13 |
| camp_28 | 0.17 | 0.10 | -0.19 | 0.11 | 0.12 |
| Institution-level mean | 0.17 | 0.00 | 0.00 | 0.00 | 0.00 |
| Institution-level SD | 0.04 | 1.00 | 1.00 | 1.00 | 1.00 |
| Correlations between metrics | | | | | |
| Model 1 | | 1.00 | | | |
| Model 2 | | 0.97 | 1.00 | | |
| Model 3 | | 0.98 | 0.98 | 1.00 | |
| Model 4 | | 0.95 | 0.96 | 0.97 | 1.00 |
| Observations | 22,467 | 22,467 | 22,467 | 22,467 | 22,467 |

Notes: Institutional fixed effects from models 1–4 are reverse-coded so that lower rates of UI receipt correspond to more positive standardized FE. Cumulative UI receipt is measured as the percent ever receiving UI or other unemployment compensation by the end of the fourth full year postgraduation. To limit bias from out-of-state mobility, sample is limited to those with positive earnings in at least one quarter of the focal year.

Table 3.12 Correlations of Adjusted Institution-level Metrics

| Subbaccalaureate institutions | | | | | |
|--------------------------------------|---------------|---------------|-----------|----------------|------------|
| Correlations | Subba 3yrs | Ftemp 4yrs | Earn 4yrs | SS sec 4yrs | UI 4yrs |
| Subba 3yrs | 1.000 | — | — | — | — |
| AA 3yrs | 0.646*** | — | — | — | — |
| LTC 3yrs | 0.467** | — | — | — | — |
| STC 3yrs | 0.744*** | — | — | — | — |
| Trans 3yrs | 0.376** | — | — | — | — |
| Ftemp 4yrs | -0.527*** | 1.000 | — | — | — |
| Earn 4yrs | -0.533*** | 0.818*** | 1.000 | — | — |
| SS sec 4yrs | 0.183 | -0.231 | -0.026 | 1.000 | — |
| UI 4yrs | -0.330* | 0.378** | 0.488*** | 0.222 | 1.000 |
| Avg. diff. vs. BA metric (SDs) | 0.00 | 1.35 | 1.45 | 1.06 | 1.35 |
| Avg. diff. vs. earnings metric (SDs) | 1.45 | 0.47 | 0.00 | 1.20 | 0.80 |

Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

This vast difference in institutional ratings dependent on the measure is reflected graphically in figure 3.3. As shown, institutions with the highest completion/transfer rates typically have some of the lowest employment and earnings. This striking negative correlation may reflect two issues. First, our completion measure combines certificate completion, AA/AS completion, and transfer to a four-year institution within three years. If we separate these out (not shown), long certificate completion (for programs 1–2 years in length) is the most negatively correlated with subsequent employment and earnings, though associate’s degree completion is also significantly negatively correlated with earnings. Transfer rates are only slightly positively correlated with earnings four years after transfer (though these correlations are not significant). Second, although our labor market measures exclude students who are currently enrolled in the focal year, these patterns may nonetheless reflect the fact that students who graduate or transfer may still have spent significant time engaged in school in the intervening periods and thus may have accumulated less work experience than students who drop out. Yet we find that these negative correlations are still strong if we look seven years postgraduation (not shown). Overall, the negative correlations between subbaccalaureate completion rates and subsequent labor market outcomes is puzzling and provides strong motivation for considering measures beyond graduation/transfer for this sector.

Correlation of metrics over time. Table 3.13 examines the sensitivity of these metrics to the length of follow-up. It appears that labor market outcomes are much less sensitive to the length of follow-up for the two-year sector than we found for the four-year sector.¹⁹ Reasons for this could

19. This holds regardless of whether we use adjusted or unadjusted measures.

Table 3.13 Correlations of adjusted LM metrics over time

| Subbaccalaureate institutions | | | | |
|--------------------------------------|----------|----------|----------|--------|
| | Year 1 | Year 2 | Year 4 | Year 7 |
| <i>A. Full-time employment proxy</i> | | | | |
| Year 1 | 1.000 | — | — | — |
| Year 2 | 0.977*** | 1.000 | — | — |
| Year 4 | 0.920*** | 0.930*** | 1.000 | — |
| Year 7 | 0.887*** | 0.873*** | 0.890*** | 1.000 |
| <i>B. Conditional earnings</i> | | | | |
| Year 1 | 1.000 | — | — | — |
| Year 2 | 0.971*** | 1.000 | — | — |
| Year 4 | 0.960*** | 0.975*** | 1.000 | — |
| Year 7 | 0.883*** | 0.923*** | 0.933*** | 1.000 |
| <i>C. Social sector employment</i> | | | | |
| Year 1 | 1.000 | — | — | — |
| Year 2 | 0.937*** | 1.000 | — | — |
| Year 4 | 0.918*** | 0.955*** | 1.000 | — |
| Year 7 | 0.841*** | 0.856*** | 0.940*** | 1.000 |
| <i>D. UI Receipt (inverse)</i> | | | | |
| Year 1 | 1.000 | — | — | — |
| Year 2 | 0.894*** | 1.000 | — | — |
| Year 4 | 0.798*** | 0.853*** | 1.000 | — |
| Year 7 | 0.712*** | 0.779*** | 0.942*** | 1.000 |

Note: *** $p < .01$, ** $p < .05$, * $p < .10$.

include lower rates of out-of-state mobility as well as lower rates of further educational enrollment. Table 3.14 further shows that unlike in the four-year sector, it is not obvious that the role of statistical adjustments diminishes over time for this sector. The correlation between adjusted and unadjusted versions of the same metric appears more stable, sometimes even declining over time.

3.6 Discussion

While newly accessible state UI databases present great opportunities for enhancing states' ongoing efforts to measure college student outcomes, it is no straightforward task to figure out how to use these data most effectively. We draw the following conclusions from our analyses.

First, state UI databases can provide richer measures of graduates' labor market experiences beyond just earnings. While three of the four labor market metrics are positively correlated (with social sector employment the exception), they do appear to capture different aspects of postcollege labor market success, and institutions could receive markedly different ratings or rankings depending on which measure is used.

Second, metrics based on labor market outcomes result in substantially

Table 3.14 Correlations between adjusted and unadjusted metrics

| Subbaccalaureate institutions | | | | |
|-------------------------------|--------|--------|--------|--------|
| Metric (standardized) | Year 1 | Year 2 | Year 4 | Year 7 |
| Full-time employment proxy | 0.941 | 0.931 | 0.904 | 0.913 |
| Conditional earnings | 0.912 | 0.898 | 0.840 | 0.815 |
| Social sector employment | 0.928 | 0.955 | 0.947 | 0.922 |
| UI receipt | 0.950 | 0.947 | 0.947 | 0.939 |

Note: $p < 0.01$ for all correlations in this table.

different institutional ratings and rankings than those based on degree completion (or completion/transfer) alone, particularly in the two-year sector. Indeed, for two-year institutions, degree completion/transfer rates are negatively correlated with three of our four labor market outcome metrics, highlighting the risk involved in relying on academic outcomes alone.

Third, statistical adjustments generally have less consequence for ratings/rankings than the choice of outcome metric and length of follow-up. Moreover, field of study controls appear particularly important. Research in the Canadian context by Betts, Ferrall, and Finnie (2013) similarly highlights the role of field-of-study controls, and it is worth noting that data on major field of study are one of the comparative advantages of state administrative databases compared to alternative national data sources (such as IRS data). Still, overall, the effect of adjustments appears modest compared to other analytic choices. It is possible that even our fully adjusted model omits important factors that if incorporated, could more substantially change institutions' ratings. But our finding echoes a similar pattern in hospital performance measurement, in which the choice of outcome generally matters more than which patient-level controls are included (Staiger 2016).

Fourth, for earnings-based metrics in the four-year sector, statistical adjustments appear more important when outcomes are measured early. Compared against seventh-year adjusted earnings, an adjusted one-year measure performed about as well as an unadjusted two-year measure. This suggests states may be able to choose between using an adjusted measure soon after graduation or an unadjusted measure after a longer period of time, depending on which is more feasible. This trade-off is not evident for every outcome metric, however, or for the two-year sector.

Finally, when we examine the correlation of our metrics over different lengths of follow-up, we find that our conditional earnings metric is much less stable over time for the four-year sector than for the two-year sector. In the four-year sector, the correlation of seven-year earnings with earnings measured earlier ranges from 0.55 to 0.87, while in the two-year sector, the equivalent correlations range from 0.88 to 0.93. The full-time, full-year employment metric is even more unstable for four-year graduates: the cor-

relation between employment measured at one year versus seven years post-graduation is only 0.331 and only marginally significant.

Limitations. Currently we use several cohorts of entrants/graduates to estimate each institution's fixed effect. We have not examined what would happen if these effects were estimated with only one or two cohorts at a time. We have not incorporated any controls for student ability or family income, which have been used in other studies of accountability metrics. Finally, we have not examined any input-based measures of institutional quality/selectivity, such as constructed in Dillon and Smith (2015). It would be very valuable to further investigate the correlation between input- and output-based institutional ratings, as done by Betts, Ferrall, and Finnie (2013) in the Canadian context.

Conclusion. Overall, our preliminary conclusion is that labor market data, even when imperfect, can provide valuable information distinct from students' academic outcomes, particularly for the two-year sector. Institutional ratings based on labor market outcomes, however, are quite sensitive to the specific metric constructed. The simplest labor market metrics at policy makers' disposal—unadjusted employment rates and average earnings within a year after graduation—both prove to be quite unreliable compared to the same outcomes measured later. Moreover, earnings and employment on their own may fail to capture other aspects of economic well-being of value to both policy makers and students themselves. Consistent with similar types of studies conducted in other contexts (such as outcomes-based evaluations of hospital quality), the choice of metric and length of follow-up appear to matter more than compositional adjustments.

Of course, while Ohio is a large and diverse state, there is no guarantee that our results will generalize to other states or contexts. For example, Ohio has a relatively low rate of out-of-state mobility: census data show that approximately 86 percent of Ohio's 26- to 35-year-olds with at least some college education still lived in the state five years later, compared to a median of about 80 percent across all states. In states with high rates of out-of-state mobility, it's possible that compositional adjustments might be particularly important and that outcome measurements might be more sensitive to the timing of follow-up than we estimate here. In addition, it is important to note that our entire analysis is limited to public state institutions (a limitation common to most state administrative databases). Overall, our findings suggest a cautious approach: while a mix of feasible labor market metrics may be better than none, reliance on any one metric—particularly one measured early—may unintentionally undermine policy makers' ongoing efforts to accurately quantify institutional performance.

Appendix

Table 3A.1 Labor market outcome (LMO) metrics used for performance-based funding

| State | Sector using LMOs | Mandatory vs. optional | Funding linked to overall performance and role of LMOs | Employment data source | Sample for LMO metrics | LMO metrics |
|-------|---------------------------------|------------------------|---|----------------------------------|---|---|
| FL | Two- and four-year institutions | Mandatory | \$200 million linked to PBF in 2016, half new funds, half reallocated from base fund. Both universities and colleges have mandatory metrics based on LMOs, though specific metrics vary by sector, and LMOs are among several outcomes considered. | Unemployment insurance (UI) data | Graduates | Percent of bachelor's graduates employed and/or continuing their education further one year after graduation; median average full-time wages one year after graduation; job placement; average full-time wages compared to entry-level wages in local service area. |
| TX | State technical colleges | Mandatory | One hundred percent of state funding recommendations (from THECB to state legislature) are based on LMO. | UI data | Graduates and those who leave TSTCS for at least two years and show up in state workforce | THECB "value-added" measure is based on five years of postenrollment earnings minus the minimum wage. |
| KS | Two-year institutions | Optional | New state funds; LMOs are options but not mandatory and vary by institution (at least eight are currently using). | UI data | Graduates | Percent of students employed; percent employed in field; wages of students hired; starting wages. |
| AK | Two-year institutions | Optional | Five percent of funding in the 2012–13 school year, increasing by 5 percent increments until capped at 25 percent during the 2017–18 school year. The remaining 75 percent of funding will be based on enrollment and institutional needs. LMO metric is optional, not mandatory; unclear if any campus is using. | UI data | Graduates | Number of completers that obtain employment (time frame unclear). |

| | | | | | | |
|----|---|-----------|---|--------------------------------|--------------------------------------|--|
| LA | Two- and four-year institutions | Optional | Fifteen percent of base appropriations; institutions can also receive permission to raise tuition by 10 percent without legislative approval. LMOs are options but not mandatory and not clear that any institutions are currently using. | Not specified | Graduates | Employment of degree and certificate earners. |
| MN | Two-year institutions | Mandatory | Five percent of base funding is reserved until institutions meet three out of five performance goals. | Graduate follow-up survey data | Graduates | Percent employed in program-related job during the year after graduation. |
| UT | In place at two-year and four-year institutions | | For fiscal year (FY) 2015, \$1.5 million in one-time funding was allocated based on performance. | UI data | Graduates from two-year institutions | Job placement rates following graduation. |
| WI | Technical colleges | Mandatory | Ten percent of base funding were at stake during FY2014–15. The amount of performance funding increased by 10 percent increments until reaching 30 percent in FY2016–17. | Graduate follow-up survey data | Graduates | Placement rate of students in jobs related to students' programs of study. |
| TN | Two-year institutions | Mandatory | After a base amount is set aside for operational support, 100 percent of state funding is allocated based on institutional outcomes. | Not specified | Graduates | Job placement within a related field through June 30 of the year following graduation. |

Source: NCSL (2015), THECB (2013), FLBOG (2016), and state agency websites.

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