Monopsony Power in Higher Education: A Tale of Two Tracks

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Current Version: October 2021

Abstract:

This paper estimates the degree of monopsony power in the U.S. higher education labor market. It does so by using school-specific labor demand instruments to directly estimate the residual labor supply curves of full-time faculty to individual four-year colleges and universities. The results indicate that schools have significant monopsony power over their tenure track faculty but face perfectly elastic residual labor supply curves for non-tenure track faculty. There is some evidence in favor of each of the three sources of monopsony power most often discussed in the literature—employer concentration, search frictions/job switching costs, and differentiated jobs. The results also suggest that the expansion of student enrollment in the presence of this monopsony power over the tenure track faculty may have been a major contributing factor to the rising use of non-tenure track faculty, explaining perhaps as much as three-quarters of its rise over the last 20 years.

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We would like to thank Pengyu Ren, Nicole Bei Luo and Rebecca Goldgof for their outstanding research assistance and Orley Ashenfelter, Jose Azar, Emir Kamenica, Pete Klenow, Ilyana Kuziemko, Neale Mahoney, Magne Mogstad, Simon Mongey, Canice Prendergast, Robert Topel, Seth Zimmerman, seminar participants at the Bank of Spain, the University of Chicago, Clemson, East Anglia, Federal Reserve Bank of Richmond, Microsoft Research, Northwestern, Princeton, and the NBER labor and education groups. We thank the Initiative on Global Markets at the University of Chicago's Booth School for financial assistance.

1. Introduction

Rising income inequality and the decline in labor's share of income in the United States, coupled with a rise in ownership concentration in most industries, have put the issues of monopsony power and the degree of workers' bargaining power in the spotlight for policy makers, economic researchers, and even the popular press.[2]

The economics literature has tested for the existence of monopsony power in a variety of settings and with a variety of approaches. These have typically relied on more indirect methods than estimating the residual supply curve facing an individual firm.[3] This is because the data needed to identify this definitional hallmark of classical monopsony power includes firm-level labor demand shifters that are not easy to find.[4]

In this paper, we directly estimate firm-level labor supply curves to test for monopsony power in the college and university full-time faculty labor market.

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[2] See the discussions in Council of Economic Advisers (2016), U.S. Department of Treasury (2016), Autor et al. (2020), Stansbury and Summers (2020), Manning (2021), Posner (2021) or White House Executive Order (2021).

[3] Boal and Ransom (1997) and Ashenfelter et al. (2010) survey some of the classical monopsony literature. Types of tests for monopsony include those relating labor market outcomes to employer concentration like Abel et al. (2018), Azar et al. (2019, forthcoming), Benmelech et al. (2020), and Prager and Schmitt (2021); exploring pass-through of rents to workers as in Kline et al. (2017) or Lamadon et al. (2019); comparing wages to workers' estimated marginal revenue products like Hershbein et al. (2019), Isen (2013), Scully (1974), Somppi et al. (1985), and Zimbalist (1992); and looking at the employment effects of minimum wage and labor market policies as in Card and Krueger (1995, 2000), Dube et al. (2007), Manning (1996), and Naidu et al. (2016). A different form of modelling monopsony comes out of search-theoretic models of the labor market and uses data on labor flows. This "dynamic monopsony" approach is explored extensively in Bhaskar et al. (2002) and Manning (2003), and surveyed in Manning (2011) and can answer a broader set of monopsony questions relating to search frictions, workers' outside options, and bargaining power (among others).

[4] Some prominent exceptions that have directly estimated firm level labor supply curves include the work of Hirsch and Schumacher (1995), Matsudaira (2014), Garin and Silverio (2018), Staiger et al. (2010) and Sullivan (1989) on healthcare workers, Falch (2010) on teachers, and Boal (1995) on coal miners. Most of these studies exploit a natural experiment that changed wages differentially across firms. Interesting direct experimental evidence where researchers vary the wage and estimate the residual labor supply elasticity includes Dal Bo et al. (2013) and Dube et al. (2020).

Our methodology uses school-level instruments for labor demand derived from lagged application patterns at that school to identify the residual labor supply.[5]

The existence of monopsony power in the higher education industry has been the subject of intense academic and even courtroom debate in previous years, but usually relied on methods like comparing seniority wage premia in universities to those in other industries or other indirect approaches to the question.[6] It is a distinct setting from much of the existing labor literature because it potentially entails monopsony in a context that is both highly skilled and, arguably, a national rather than local market.

Our results indicate that schools have significant monopsony power over their tenure track faculty; the tenure track labor supply curve to the school is not flat. For non-tenure track faculty, however, there is not significant evidence of monopsony. In hiring such faculty, schools appear to be pure wage-takers.

The literature has generally concentrated on three major sources of monopsony power for employers: employer concentration, search frictions/job switching costs, and job differentiation. We will present suggestive evidence about the importance of each of them in the academic labor market. There is some evidence consistent with each story.

We also document that monopsony may have played a significant role in universities' increasing use of adjunct faculty over the past couple of decades. As enrollments have increased, the desire to avoid raising wages among tenure-track faculty may explain as much as three-quarters of the shift to adjuncts.

In Section I of the paper, we explain the concept behind our basic empirical specification. Section II overviews the higher education market and the data. Section III presents our basic results. Section IV further explores the validity of our instrumentation strategy. Section V addresses the potential existence of

[5] We use "school," "college," "university," and "institution" interchangeably.
[6] See, for example, Ransom (1993), Hallock (1995), and Monks and Robinson (2001).

wage differences across individual faculty and how compositional effects might influence the results. Section VI investigates the potential sources of monopsony power, and Section VII discusses monopsony and the shift toward non-tenure-track faculty in the last 15 years. Section VIII concludes.

I. Estimating Monopsony Power in Higher Education

Colleges and universities provide an interesting test case for labor market monopsony. The data allow us to test for monopsony's existence and size, and it remains a policy-relevant industry in its own right. The Department of Justice recently argued that university conduct relating to faculty hiring can be considered anti-competitive and violating of antitrust laws (U.S. Department of Justice, 2019).

Before we get to empirical tests, several elements are suggestive of potential employer market power at colleges and universities. First, the popular and trade press have argued at length that schools have exploited their faculty, especially adjunct and contingent faculty, with low wages and poor working conditions.[7] Second, it is plausible that faculty face significant switching costs that rise with tenure at the school. Third, in the economic sense, schools are definitely not homogeneous employers in terms of geography, size, prestige, and other dimensions.

Rather than rely on indirect methods, our basic test of monopsony power centers on estimating the residual labor supply curve facing each institution. A firm with monopsony power is not a wage-taker and can cut wages below competitive levels without losing all its workers. Hiring workers drives up wages for infra-marginal workers. This basic idea suggests a straightforward way to measure monopsony (with appropriate data): estimate the inverse labor supply curve facing the individual institution and test if it is upward sloping.

[7] See, for example, O'Shaughnessy (2012), Hoeller (2014), Fredrickson (2015), Bodenheimer (2018), Childress (2019), and Chronicle of Higher Education (2018).

We will use a constant elasticity functional form for residual labor supply (a first-order approximation to a general residual labor supply curve):

$$\ln(W_{it}) = \mu \ln(L_{it}) + School_i + Year_t + \epsilon_{it} \ , \qquad (1)$$

where W_{it} is the wage paid by university i in period (year) t, and L_{it} is the amount of labor (number of faculty) i employs in t. The specification also includes school and year fixed effects.

The coefficient μ is the inverse of the labor supply elasticity facing the individual school. In a competitive labor market, $\mu = 0$ (firm level labor supply is infinite), and the firm can hire or fire without changing the wage it pays. If $\mu > 0$, then the firm faces an upward sloping residual labor supply and has monopsony power.[8] This creates an incentive to reduce employment relative to the competitive market and pay workers less than the marginal product of their labor. For a cost-minimizing firm in the standard model, μ would be the size of this wedge between the two, analogous to the Lerner index in the product market monopoly context.[9]

The basic problem with estimating equation (1) has always come from the data. First, it requires employer-level wage and labor quantity data. Second, it requires employer-level instruments for labor demand to get around the standard supply-vs.-demand identification problem. This is the factor market analog to using firm-level cost shifters to estimate residual demand curves in

[8] We estimate the inverse labor supply curve rather than a conventional "forward" labor supply curve for two reasons. First, measurement error or other biases toward zero will not be misinterpreted as evidence for monopsony. Second, it allows a straightforward statistical test of competitive labor markets against a null of 0 rather than against a null of infinity.
[9] Note that identifying the existence of monopsony power in the form of $\mu > 0$ does not require the school to be profit maximizing/cost-minimizing. The degree to which schools exploit their market power or actually pay professors less than their marginal product does, though. Because the schools included in our sample are not-for-profit enterprises, it is unclear what their objective function is, and most of our results do not take a position on the matter. The exception is in Section VII, where we discuss monopsonistic schools' responses to expanding enrollments. There, we explicitly consider cost-minimizing behaviors.

an industry, as in Baker and Bresnahan (1988), for example. We believe that the data on individual universities can meet these criteria.

II. Data

Our primary data come from the Integrated Postsecondary Education Data System (IPEDS) of the Department of Education's National Center for Education Statistics. All schools eligible for financial aid under Title IV of the Higher Education Act of 1965 (e.g., Pell Grants, Stafford Loans) must provide detailed statistical information annually on their students, faculty, employees, and institution. It amounts to something like a census of colleges and universities and, being mandatory, has a compliance rate of close to 100% (though some individual data elements are often missing).[10]

IPEDS includes information about applications to the school. We use these when designing our instrument for school-specific labor demand, as described further below.

We examine four-year, not-for-profit colleges and universities in the 50 U.S. states plus D.C. We exclude two-year schools, for-profit schools, and schools in U.S. territories. Given our reliance on lagged applications as a demand instrument, we also drop school-years missing such data which, in practice, includes the open enrollment schools. IPEDS' applications data start in the 2001-2002 academic year, so our sample spans the 15-year period from 2002-2003 to 2016-2017. We end up with 1755 institutions in our sample having enough information to be included in at least one of our empirical specifications.

On the labor side, we use data from the human resources component of IPEDS on the number of full-time instructional faculty of various ranks and their average wage. To obtain a wage comparable across faculty, we divide the

[10] More details on the IPEDS data methodology can be found in Ginder et al. (2018) or at the online guide to the IPEDS survey components (https://nces.ed.gov/ipeds/use-the-data/survey-components).

reported annual pay by the stated length of the contract to compute a monthly salary. We do not know anything about summer money available to particular faculty nor non-salary fringe benefits.[11] In some specifications, we will use information broken out by gender, institution type, academic rank, and so on.

Schools differ in their classifications of what they call a tenured rank (e.g., associate professors). They also differ somewhat over what constitutes the tenure track and how they title non-tenure track faculty (e.g., 'instructors' vs. 'lecturers'). What we refer to as 'tenure track' faculty comprise three categories of instructional faculty in IPEDS: full professors, associate professors, and assistant professors. When we refer to 'non-tenure track' or 'adjunct' faculty, we also include three categories: instructors, lecturers, and non-ranked faculty.

Importantly, we only have data on full-time faculty. IPEDS includes the total headcount of part-time faculty, but not their wages so we cannot estimate a supply curve for them. This is most potentially relevant for non-tenure track faculty, where most are part-time. Using the data on full-time faculty will still give a valid estimate if demand and wages of the full-time and part-time faculty are strictly proportional. The data from IPEDS in Figure 1 shows that the aggregate full-time share of non-tenure-track faculty has been virtually constant at around 30% despite the expansion of non-tenure-track faculty over time so the proportionality assumption may be appropriate.

III. Basic Results

A. OLS

[11] The IPEDS data do include some institution-level information on benefits-to-salary ratios. We compiled a consistent measure from 2003-2011, and the aggregate ratio is stable at around 0.22. Two-thirds of the schools saw changes in their benefit share of 0.025 or less over the entire period, and almost 95% of schools saw the ratio change by less than 0.05. As a result, we will just assume benefits are a constant fraction of salary. Given the log-linear specification, our estimated elasticity is therefore the benefits-inclusive full elasticity of labor supply.

We start by presenting the reduced-form relationship between the log of the average monthly wage and the logged quantity of labor. The results are in Table 1. Each regression includes school and year fixed effects. We weight observations by the school's total faculty headcount averaged over the full sample and we cluster the standard errors by school. Column (1) shows the relationship for non-tenure track faculty and column (2) for tenure track faculty.[12] In both cases, the coefficient is close to zero and not statistically significant. Given the inverse elasticity form, if these were the school-level labor supply curves, we could not reject that schools are wage takers.

The issue, of course, is that the OLS regression does not identify the labor supply curve facing a school because supply and demand can move simultaneously. For example, anything that increases the appeal of a school to both faculty (on the labor supply side) and students (on the labor demand side) will impart a negative bias on the estimates of μ in (1). To get around this problem, we need to instrument for school-specific labor demand.

B. Instrumenting for Institution-Level Labor Demand

An instrument allows us to use only the variation in wages that is orthogonal to labor supply shocks (i.e., comes from labor demand changes) to identify the slope of the labor supply curve.

Labor demand, like any factor demand, is derived from the demand for firms' final products. Thus a college's labor demand curve should shift with changes in students' demand for education at that school. Student enrollments obviously reflect student demand, but probably fail the exclusion restriction as an instrument because the school simultaneously controls admissions decisions, which could be influenced by shifts in faculty labor supply.

[12] We are implicitly treating the two types of labor—tenure track and non-tenure track—as separate supply markets. Consistent with this assumption, we show in Appendix 1B that for the data we have on individuals in the University of California system, within-system mobility between tenure and non-tenure tracks for individual faculty members is extremely rare.

So instead of enrollments, we use a different direct measure of student demand: the number of undergraduate applications. Variations in applications reflect differences in students' desires to attend particular universities whether in the cross section or over time, but are not as dependent on choices of the university itself as is the number of students enrolled (we will analyze the viability of this instrument in further detail below). Because admitted applicants do not immediately show up as students when they apply, we use the lag of the logged number of applications as the instrument. Lagged applications are strongly correlated with following-year enrollments in our data.[13]

The top panel of Table 2 shows the results of the first stage regression of faculty headcounts on lagged applications separately for non-tenure track and tenure track faculty. In each case, the lagged number of applications strongly predicts the quantity of both tenure track and non-tenure track faculty. The *F*-statistics for instrument relevance do not raise weak-instrument concerns.

Using this school-specific labor demand shifter as an instrument, we then estimate the schools' residual labor supply curves using two-stage least squares in the lower panel of Table 2. The labor supply elasticity is the inverse of the coefficient.

Column (1)'s coefficient is small and not significantly different from zero, consistent with schools being wage-takers for non-tenure track faculty (the point estimate implies the school-specific labor supply elasticity is 28.6). Column (2), however, indicates significant market power in the market for tenure track faculty. An increase in labor demand at the school drives up the

[13] But not one-for-one as there has also been a secular increase in the number of applications per student (DeSilver, 2019). Total applications per school rose close to 75% over our sample, while total enrollment rose about 20%.

wage the school pays. The estimated coefficient in this specification implies a school-specific labor supply elasticity for tenure track faculty of around 5.[14]

IV. Instrument Validity

Of course, the critical issue is whether lagged applications actually reflect labor demand movements that are orthogonal to labor supply shifts. We will examine the evidence for this contention.

Note first that a general, unobservable improvement to the desirability of a school for both students and faculty should bias the estimated labor supply coefficient downward—i.e., toward finding no evidence of monopsony. The improvement should increase applications but should reduce the wage required to retain faculty. Likewise, measurement error in the data would bias the coefficient toward zero, again toward finding no evidence of monopsony.

The problem case would arise from unobservables that make the higher-demand environment *less* desirable for faculty—higher wages as a compensating differential; this is an inward shift in the school's residual labor supply. One example of such a shift would be if an increased number of students raised each faculty member's workload.

The data do not support this specific mechanism, though. There is an extremely small coefficient of school applications in a regression explaining student-faculty ratios, and including the student-faculty ratio in the labor supply curve as a separate control shows no impact. Further, any 'overcrowding' explanation would need to line up with the heterogeneity in estimated monopsony power that we document below. For example, it would have to exist for tenure track faculty but not non-tenure track faculty, be larger

[14] With the exception of our analysis of the shift to non-tenure track faculty in Section VII, our analyses do not need to assume schools are cost-minimizing or profit-maximizing. An upward-sloping residual labor supply curve implies schools have monopsony power but does not document the extent to which they exploit that market power by holding wages below marginal product.

for full professors than associate professors and even larger compared to assistant professors, it would need to be greater for high “prestige” schools (defined below) than others, and so on.

A. Instrument Lags

First we consider the argument about using lagged applications by examining the nature of the timing of student applications and faculty hiring and by adding multiple lags and leads of the applications to the first-stage regression as presented in Table 3.

The pattern is broadly consistent with our lagged instrument strategy. There is a large and significant impact of past and current applications on faculty headcounts. On the other hand, the relationship between future applications and current faculty headcounts is much smaller (as small as roughly one-fifth the size) and jointly not significantly different from zero. We will use the single-lagged applications instrument because it preserves the most data in the sample--IPEDS applications data begin in 2002, so every additional lag term forces us to drop one year of data from the regressions.

B. What Drives Application Changes?

Our basic idea is that a combination of factors drive applications to a particular school. Some are observable and predictable components like the size of the local population of prime college age, the appeal of a school to foreign students whose demand for U.S. schools rose significantly over the sample, or the poverty rate among potential applicants (an indicator of extra sensitivity to education costs). Then there are idiosyncratic drivers of applications like changes to the appeal of the location of the school, an unexpectedly successful sports team, an alum becoming a celebrity, and so on.[15]

We explore the roles of these factors in predicting applications in Table 4. In column (1), we regress the logged number of applicants to the school on lags of observable factors: the share of the state population aged 15-19 three years prior, the logged number of foreign students at the school the prior year, and the state poverty rate in the prior year. Each of these factors has the predicted sign on applications, with foreign students and demographics being statistically significant and state poverty only marginally so.

In columns (2) and (3), we then use only those observable/predictable components in our first stage regression and they predict hiring.

In columns (4) and (5), we decompose lagged applications into two parts: the observable/predicted component using the fitted values from the applications regression in (1), and the residuals from that regression as the unobservable/unpredicted component. We repeat the first-stage regression including the two separately. Both are correlated with faculty hiring, but increases in applications that come from the predictable components have a larger estimated coefficient than from the unpredictable component.

In Table 5, we then repeat the IV estimation using these decomposed instruments. Columns (1) and (2) use only the observable factors as instruments for labor demand (again, the share of the state's population that was aged 15-19 three years prior, the logged number of foreign students at the school in the year before, and the state poverty rate in the prior year). The results confirm the existence of monopsony over tenure track faculty. Here the magnitudes are even larger than in Table 2. The estimated residual labor supply elasticity is 1.94. The results here also indicate a borderline significant

[15] Of course, broad trends driving applications like countrywide demographics or the nationwide increase in the number of schools the average student applies to will be absorbed into the year dummies in our framework.

coefficient for non-tenure track faculty as well (albeit of modest size with an implied school-level supply elasticity of almost 8).

In columns (3) and (4), we take the residuals from the applications regression and use only the unobservable/unpredictable component of applications as the instrument for labor demand. Here again there is evidence of schools having monopsony power over tenure-track faculty. The coefficients are smaller than in columns (1) and (2), but they show the same pattern.

C. Instruments on a Placebo Group

In Table 6, we consider a placebo-type check on the applications instrument based on the notion that if the cost of expanding enrollment is particularly high at a school, labor demand and hiring there should respond less to changes in student applications, and so application growth will not be as informative as an instrument.

Though we do not directly observe a school's cost of expansion, we do have data from IPEDS on universities' educational and instructional costs. Specifically, we use the measure of spending per student used in Jacob et al. (2018) as a proxy for schools' costs of expanding enrollment. We rank schools by this measure and look at how applications affect labor demand at the highest-cost decile schools in columns (1)-(4). Basically, we are estimating the first stage regression for high-cost schools separately.

The results show that the instrument does indeed fail for the high-cost schools. Columns (1) and (3) for high-cost schools, indicate that higher applications have an insignificant and close to zero impact on hiring. In (2) and (4), however, we again see the robust relationship between applications and hiring in the regular schools.

V. Wage Differences, Quality Upgrading and Composition Changes

Our empirical model to this point has assumed a classical monopsony structure: firms face upward sloping residual labor supply, and additional hiring raises the wage paid to not just the marginal employee, but inframarginal employees as well. We assume this in part for analytical convenience because IPEDS only reports group-level average wages (e.g., all associate professors at a school) and we do not have different wages by field or by individual. In this section we consider whether this averaging might bias our results or their interpretation.

First, note that the existence of wage differences/discrimination across faculty members or fields is not an inherent problem for the findings. Indeed, to first order, it only makes it harder to find evidence of classical monopsony if schools don't raise average wages for everyone and simply pay marginal faculty hires more.[16]

Second, as a mechanical matter, so long as wage differences across faculty are proportional (e.g., the levels differ across people at a school but everyone's wages go up by 5%), the monopsony estimates in our specification and results would remain the same (barring compositional changes in who the faculty are). National annual wage data by field reported by *Inside Higher Ed* and *Higher Ed Jobs* suggest that by field, at least, this is a reasonable assumption.[17]

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[16] Note that market power is a necessary condition for price discrimination, so this type of wage setting would still result from monopsony. It just would not be detected as such by our empirical test.

[17] *Inside Higher Ed* (Jaschik, 2007) and *Higher Ed Jobs* (2017) report average salaries for 31 different academic fields and four ranks in 2006-7 and 2016-17, respectively. Though the levels differed quite a lot across fields—in 2016-17 new assistant professors in Law earned $105,243 versus in the Visual and Performing Arts, they earned $57,858, for example—the annual growth rates of salaries were quite similar across fields for the decade. For instance, new assistant professors saw average annual salary growth across all fields of 2.4 percent for the decade, and all but two of the fields (Communications Technologies and Liberal Arts—General Humanities) had annual growth rates within 0.5 percent of that. The other measures showed the same: other (not-new) assistant professors, associate and full professors averaged 2.2, 1.9 and 2.0 percent annual growth, respectively, and had all but 3, 1, and 0 fields within 0.5 percent of that.

The more problematic scenario for our findings would be if the faculty labor market is actually a collection of vertically differentiated yet perfectly competitive labor markets where workers differ in their marginal products and the composition of the faculty changes over time (in a way that is also correlated with student demand). In this sense, if new hires are systematically higher quality and paid more than the existing faculty, the quality upgrading could be mistaken for higher average wages and, therefore, monopsony.

Two pieces of evidence suggest that the alternative hypothesis of composition/quality shifting cannot explain the results.

First, if the coefficient is just picking up wage differences between new and existing faculty, then a greater net hiring rate of new faculty as a share of existing faculty, by creating larger composition shifts, should be correlated with our estimated monopsony power. Columns (1) and (2) of Table 7 interact the labor quantity with the share of net new faculty hires as a share of the existing stock of faculty in the previous year. There is no evidence that having a higher rate of compositional change is correlated with the estimated degree of monopsony. The interaction term is tiny, insignificant and has the wrong sign for tenure-track faculty.

Second, for a specific subset of schools, we obtained public records with individual level salary data for faculty at the nine campuses of the University of California system (Berkeley, Davis, Irvine, Los Angeles, Merced, Riverside, Santa Barbara, Santa Cruz, and San Diego) and can check directly whether composition changes are driving the changes in average salary. This panel is available from the University of California Office of the President (2019), and we use it to decompose average salary growth by academic rank into the part coming from rising pay for faculty already at the school versus that from new hires with higher salaries than the average continuing faculty (or departing faculty with lower salaries).

We present the results in Appendix 1-A. During this period at these public universities, average annual tenure-track salary growth was between 3.4 and 4.0%, depending on rank. None of that average increase came from new faculty coming in at higher salaries or faculty leaving with lower than average salaries. Indeed, the impact of net entry on average salaries was slightly negative. Consistent with our assumption in our nationwide regressions above, average wage changes in these schools are driven by the wage changes of the existing faculty not compositional shifts from changing faculty.[18]

VI. Sources of Monopsony Power

Here we analyze the potential roles of three of the most commonly discussed sources of monopsony from the literature in shaping our findings for the academic labor market.

A. Labor Market Concentration: Local vs. National Markets

Perhaps the most common argument regarding labor market power is that it derives from concentration. Indeed, the previous literature has often used evidence of a wage-concentration relationship as evidence of monopsony.[19] This argument relies on the assumption, among others, of local labor markets. Our data let us test this assumption in the market for faculty.

In Table 8, we compute each school's share of the academic labor market at three levels of aggregation: the school's share of total academic employment (in either tenure track or non-tenure track faculty as applicable) in that school's commuting zone, its share in the state, and its share in the nation. The national share is really just a measure of the size of the school's faculty.

[18] The fact that departing and entering faculty do not account for the wage changes also suggests there may not be the kind of strong adverse selection in the secondary market here that can be a source of monopsony power, as outlined in Acemoglu and Pischke (1998).
[19] See Azar et al. (2019) or Berger et al. (2019) for significant examinations of concentration and monopsony power in the labor market as well as a more skeptical take of Hershbein et al. (2019).

In columns (1) and (2), we interact the quantity term with each of these measures. The results show that the share measure matters only for tenure track faculty and only at the national level. Thus the extent of a college's monopsony power is related to its size, but not to any measure of its local market share. This is a cautionary example about relying on local market concentration as an indicator of monopsony in markets where competition takes place nationally.

In columns (3) and (4) we repeat the regression with just the national share/size interaction alone. It shows coefficients of very similar magnitude.[20] The quantitative implication of the estimates is that monopsony power is concentrated in the largest of schools. The median-sized school (which employs 0.033% of tenure track faculty nationwide) faces a residual labor supply elasticity of more than 7. At the 75th percentile (employing around 0.073% of faculty nationwide), the school's elasticity is about 4, and at the 90th percentile (0.174% of national faculty) it is 1.8.

B. Switching Costs and Search Frictions

A second common source of monopsony power discussed in the existing literature arises from employee search frictions and job switching costs. The best way to estimate elements of such dynamic labor market models (which also allows examination of some broader issues beyond the classical monopsony case) is with job flows data. We do not have that type of data, but we can look at situations where faculty plausibly face higher switching costs and determine whether our monopsony power estimates are larger there.

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[20] An alternative explanation for this finding could be that there are economies of scale that operate at the school level thus raising faculty's marginal products and wages even in a perfectly competitive labor market. The fact that our specifications include university fixed effects partially alleviates this concern because cross-sectional differences in universities' scale (which are substantial) are already absorbed in the school fixed effects. Furthermore, any scale effect would need to be absent for non-tenure track faculty and, as we will see below, be systematically different for full professors than assistant professors, and also be largest for schools with high prestige.

First, in columns (1)-(3) of Table 9, using the information in the IPEDS data, we can break up the tenure track faculty by rank into professors, associate professors, and assistant professors. Tenured professors, presumably, have the highest switching costs. Lower-ranked faculty have usually been at an institution for a shorter duration, are less likely to have tenure, and often come prepared to move on at the conclusion of their contract.

The data show the expected pattern. There is monopsony power over each rank, but the elasticity of labor supply to the school is most inelastic for the full professors, then associates, then assistants. The implied residual labor supply elasticities are around 1.8 for full professors, 3.0 for associate professors, and 7.7 for assistant professors.

A second circumstance with higher job switching costs might arise from professors in fields that typically require them to establish large physical laboratories that are costly to move. Although the IPEDS does not have faculty data by field, it does count the share of undergraduate degrees granted by field. Taking the share of undergraduate student degrees in lab-based sciences and engineering as a proxy for the share of faculty in those high-setup-cost fields, we interact it with the faculty headcount to see if having more lab-based fields raises estimated monopsony power at a school.[21] We present these results in columns (4) and (5) of Table 9.

The results again show no monopsony for non-tenure track faculty and monopsony for tenure track faculty. The interaction term suggests that having a higher share in lab-based fields does correspond to modestly greater monopsony power. The university labor supply elasticity for a school at the median share of undergraduate degrees in lab fields (share of .08) is 4.84. One

[21] We count as lab-based fields all Classification of Instructional Program (CIP) codes in Engineering and Engineering Technologies (14 and 15), Biological and Biomedical Sciences (26 and 30.0101 and 30.1001), Physical Sciences (40) and Science Technologies/Technicians (41).

standard deviation higher (share of .20) corresponds to a labor supply elasticity of 3.8.

A third situation highlighted in the literature where there might be higher switching costs relates to gender and the argument that women may have larger adjustment costs or put a higher value on stable work arrangements. This is alleged to give employers more monopsony power over them.[22]

IPEDS data report average salary and headcount information separately for men and for women, so we repeat our basic results and test for different monopsony power by gender in columns (6) and (7) but we find no significant difference in the school-specific labor supply elasticities of men and women tenure track faculty.[23]

A final way to examine search frictions and switching costs as a source of monopsony power comes from considering whether schools' monopsony power is persistent. Search frictions and job switching costs should be less important when workers have sufficient time to adjust. In Table 10, we take the long difference in log wages across our full sample for each school—the 2017 value minus the 2003 value—and regress it on the long difference of the log number of faculty, using the long difference in the log number of lagged applications as the instrument.

The results suggest persistent monopsony power for the schools. Indeed, the coefficients are a bit larger than in the benchmark specifications. There is even a small positive coefficient on the non-tenure track faculty in addition to

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[22] This issue arose in the original monopsony discussion of Robinson (1969) as well as in more recent work such as Barth and Dale-Olsen (2009), Hirsch et al. (2010), Ransom and Oaxaca (2010), and Mas and Pallais (2017). It has even been discussed specifically in the context of academic labor markets in Hoffman (1976), Ginther and Hays (1999), and Monks and Robinson (2000).

[23] The inverse labor supply of male non-tenure track faculty is significantly different from that of female non-tenure track faculty, but both are very small, and neither is significantly different from zero.

the large positive coefficient on tenure track faculty. This is hard to reconcile with search and switching costs being the primary source of monopsony, as we would expect their importance to lessen over time and the labor supply elasticity to become larger (inverse elasticity to become smaller). Monopsony here instead seems to involve more permanent sources, like the concentration argument above or, as described below, differentiated jobs.

Taken together (with the caveat that the results are based only on proxies), there is some evidence for search and switching costs being a source of monopsony power in higher education, but present in, perhaps, limited settings.

C. Job Differentiation and School Prestige

The final source of monopsony power from the existing literature that we examine is job differentiation, analogous to differentiated products in the goods context. As discussed in Card et al. (2018) or Azar, Berry and Marinescu (2019), the idea is that if workers value jobs across different employers differently, it can give employers monopsony wage-setting ability.

Our basic approach to testing for job differentiation is to test whether observable characteristics of schools or jobs are related to the estimated degree of market power. What we have in mind is that schools with higher levels of prestige, for example, might have greater monopsony power over their faculty through a differentiation channel, much like "luxury" brands might face less elastic demand and charge higher markups in a product market context (see for example Berry, Levinsohn, and Pakes, 1995, for automobiles). Lacking a direct measure of prestige, we use a few proxies.

First, in columns (1) and (2) of Table 11 we use the *US News* ranking of national universities (a common measure of prestige) as of the year 2000 just before our sample began. We re-estimate our basic model while adding interaction terms with an indicator for the school being in the top 50 ranked research universities as well as a dummy if the school is listed in national university tiers 2-4 (at that time, *U.S. News* did not rank schools within those additional tiers).[24]

The results show a strong association of prestige with monopsony power for tenure track faculty. Relative to all other schools, there is significant monopsony power at the *U.S.News* ranked universities. For tier 1 schools the point estimate is actually greater than 1, though not significantly so. Tier 2-4 schools also have significant monopsony power with a labor supply elasticity around 2.

Next, in columns (3) and (4) we allow the coefficient to vary for schools whose undergraduate students have median test scores in the top quartile of schools on either the SAT or ACT.[25] Again, there is no significant relationship for non-tenure track faculty at any schools but strong evidence of monopsony power over the tenure track faculty at schools with high student test scores.

In columns (5) and (6) we measure schools' prestige by the relative salary of full professors in the first year in our sample where the highest paying school would be at the 100th percentile. We find again that this proxy for prestige is associated with greater monopsony power.

These three sets of empirical patterns indicate that across multiple facets of what might be considered prestige, schools that are highly ranked in these dimensions have greater estimated market power. This is consistent with faculty viewing jobs at prestigious schools as meaningfully differentiated from alternative positions.

[24] In a previous draft we used the Carnegie classifications of research universities (R1, R2 and R3) as indicators of prestige. The results show the same pattern as these with the *US News* rankings: the higher the prestige category of the universities, the larger the estimated monopsony power.

[25] Technically we take the midpoint between the reported 25th and 75th percentile test scores as the measure of median test scores.

VII. Monopsony and the Rise of Non-Tenure-Track Faculty

The results above suggest that schools have significant and durable monopsony power over their tenure track faculty, especially so at larger and more prestigious schools, but that they are price-takers for non-tenure track faculty.

That asymmetry has an interesting implication regarding substitution toward adjunct faculty as universities grow. There has been a well-documented rise in the share of non-tenure track faculty at American universities during our sample (see American Association of University Professors, 2017). Our data show the same thing. Figure 2 shows the year dummies from a regression of the share of non-tenure track faculty among total full-time faculty on school and year dummies. The mean share in the sample started at around 13% in 2003 and rose 44% over the sample period.[26]

In the popular press, the standard discussion of this rise centers on the bargaining power of universities and their ability to exploit adjunct faculty.[27] Indeed, this has been the rallying point of efforts of adjunct faculty at several universities to unionize in order to improve their bargaining power. However, pay could be low for adjunct faculty without the presence of university-level market power in the traditional monopsony sense. Low equilibrium wages for adjunct faculty may arise from the *aggregate* labor supply curve of adjunct faculty being shifted out enough relative to aggregate demand to result in a low equilibrium wage. In that case, equilibrium market wages would be low because of supply-and-demand conditions in a competitive aggregate market, rather than because individual universities are holding back on hiring more adjuncts to avoid raising their wages. Indeed, our results suggest that schools

[26] These numbers refer specifically to full-time adjunct faculty but, as documented in Figure 1, the share of full-time adjunct faculty has remained a constant share of total adjunct faculty over time and so this result is true for part-time faculty as well.
[27] See, for example, Fredrickson (2015) or AFT Higher Education (2002).

are price-takers and can hire as many adjunct faculty as they want at the market wage.

We also know that there has been a dramatic expansion of both enrollments and applications to schools over our sample period (resulting from demographic changes, a rise in international demand, and increasing returns to education in the economy, among other reasons). We see this in our data. Figure 3 plots the year dummies from regressions of log applications and log enrollments on institution and year fixed effects.

If schools are cost-minimizing, tenure track and non-tenure track faculty are at least partial substitutes, and schools have monopsony power over one and not the other, we would expect expanding universities to shift their labor mix toward the competitively supplied input as the monopsonistic input becomes increasingly expensive. The rate of this shift depends on the slope of the residual labor supply curve for tenure track faculty as well as the production function for the school (which implies the degree of substitutability and the extent to which input use responds to scale increases).

There is not a straightforward way to estimate the production function for a university without some heroic assumptions, but if we assume that schools combine the two types of faculty labor in a Cobb-Douglas fashion to produce an output measured by the number of students, we can estimate the implied change that expansion should have on the non-tenure-versus-tenure-track ratio.

Appendix 2 goes through the mechanics for the case of isoelastic labor supply elasticities like the ones we estimate above. If the inverse labor supply elasticity of non-tenure track faculty is 0, the inverse elasticity of tenure track faculty is μ, and the output elasticity for non-tenure track labor in the production function is α, then the elasticity of the ratio of non-tenure track to tenure track labor with respect to scale will be $1/(\alpha + \mu^{-1})$.

We can estimate α using the expenditure share for non-tenure track labor, which is 0.08 in the first year of our sample. In our baseline case, μ = 0.198, so applying the formula above implies a scale elasticity of the ratio of non-tenure track to tenure track faculty of 0.195. In Figure 3, we see that enrollments (our measure of scale) rose 21.3% from 2002 to 2017. A scale expansion of that size should therefore raise the non-tenure track share by 0.042, or about three-quarters of the increase in non-tenure track faculty documented in Figure 2.[28]

One additional piece of suggestive evidence relating tenure track monopsony to the rise of non-tenure track faculty is to compare the rise across schools that our results suggest have more or less monopsony power. Figures 4, 5, and 6 show the year dummies from regressions of the share of faculty at a school that is non-tenure track on school and year dummies. Each figure compares these estimates for sets of schools that we found above exhibit higher and lower monopsony power. In every case, we see a larger increase in the share of non-tenure track faculty at schools with more estimated labor market power. Specifically, larger schools (Figure 4), tier 1-4 ranked *U.S.News* schools (Figure 5), higher student test score schools (Figure 6), and higher initial full professor salary schools (Figure 7) each saw larger growth in their use of non-tenure track faculty. Each of these is consistent with monopsony power being related to a shift away from tenure track faculty over the period.

VIII. Conclusion

We have applied a method for testing for the presence and measuring the amount of monopsony power of colleges and universities in the U.S. market for faculty. We use direct estimation of the residual supply curve facing each institution using labor-demand-shifting instruments. The results document

[28] Even if we count all of the part-time faculty as non-tenure track faculty and assume they work half-time at the same FTE wage as other non-tenure track faculty (this would more than double the estimated size of the expenditure share on non-tenure track faculty and their overall size growth), tenure track monopsony would still explain almost 40% of the observed increase in non-tenure track faculty.

that schools have no significant monopsony power over adjunct faculty, but do hold substantial and lasting monopsony power over tenure track faculty.

We develop evidence that suggests the wage changes that accompany labor demand shifts do not reflect compositional changes arising from new hires being paid more, but rather come from growth in the wages of inframarginal faculty, consistent with the standard monopsony model.

As to the potential sources of schools' monopsony power, we consider the three most often discussed in the literature: concentrated labor markets, search and switching costs, and job differentiation (here along dimensions corresponding to a school's prestige). We find some evidence consistent with each story.

We further explore whether the presence of market power over tenure track faculty might in part be responsible for the growth in the use of adjunct faculty in recent decades. The upward-sloping residual supply of tenure track faculty could cause colleges to substitute towards non-tenure track faculty as they expand. We find several pieces of evidence that indicate this has in fact happened in the market and might explain as much as three-quarters of the rise.

In sum, portions of the labor market in U.S. higher education do exhibit nontrivial monopsony power and that has shaped labor market outcomes not just at particular schools, but has also influenced broader trends in the industry.

Table 1. Estimating Inverse Labor Supply without Instruments: OLS

Dep Variable: $\ln(W_{it})$	(1) Not Tenure Track	(2) Tenure Track
$\ln(L_{it})$	-0.012	-0.005
	(0.008)	(0.019)
Observations	20,727	21,996
R^2	0.849	0.969
Year FE	Yes	Yes
Institution FE	Yes	Yes

Notes: The data are taken from IPEDS with the definitions given in the paper. The sample spans the 2002-03 to 2016-17 academic years. Regressions cover the group listed at the top of the column. Standard errors are shown in the parentheses and clustered at the school level. The regressions are weighted by the total faculty head count for the school across the full sample.

Table 2. Estimating Inverse Labor Supply with Instruments: 1st Stage and IV Regression

1st Stage: Dep Variable: $\ln(L_{it})$	(1) Not Tenure Track	(2) Tenure Track
$\ln(\text{Applications}_{it-1})$	0.236	0.083
	(0.041)	(0.015)
Observations	20,727	21,996
R^2	0.919	0.991
Year FE	Yes	Yes
Institution FE	Yes	Yes
2nd Stage: Dep Variable: $\ln(W_{it})$	**Not Tenure Track**	**Tenure Track**
$\ln(L_{it})$	0.035	0.198
	(0.031)	(0.081)
Observations	20,727	21,996
Year FE	Yes	Yes
Institution FE	Yes	Yes
1st Stage F Statistic	33.7	32.0
Labor Supply Elasticity	28.6	5.1

Notes: The data are taken from IPEDS with the definitions given in the paper. The sample spans the 2002-03 to 2016-17 academic years. Regressions cover the group listed at the top of the column. Standard errors are shown in the parentheses and clustered at the school level. The regressions are weighted by the total faculty head count for the school across the full sample. First-stage regressions are in the top panel, second-stage in the bottom panel.

Table 3. Lag Structure of the Instrument: First Stage

Dep Variable: $\ln(L_{it})$	(1) Not Tenure Track	(2) Tenure Track
$\ln(\text{Applications}_{it-2})$	0.062 (0.029)	0.033 (0.007)
$\ln(\text{Applications}_{it-1})$	0.076 (0.027)	0.022 (0.005)
$\ln(\text{Applications}_{it})$	0.075 (0.020)	0.022 (0.005)
$\ln(\text{Applications}_{it+1})$	0.027 (0.021)	0.007 (0.004)
$\ln(\text{Applications}_{it+2})$	0.061 (0.029)	0.005 (0.007)
Observations	15,872	16,786
R^2	0.934	0.994
Year FE	Yes	Yes
Institution FE	Yes	Yes
F Stat for lagged terms (p-value)	5.1 (0.006)	19.9 (0.000)
F-stat for lead terms (p-value)	2.7 (0.068)	1.5 (0.230)

Notes: The data are taken from IPEDS with the definitions given in the paper. The sample for these particular regressions spans the 2003-04 to 2014-15 academic years. Regressions cover the group listed at the top of the column. Standard errors are shown in the parentheses and clustered at the school level. The regressions are weighted by the total faculty head count for the school across the full sample.

Table 4. Alternative Instruments for Faculty Labor Demand: First Stage

Dep Variable: $\ln(L_{it})$, except in (1)	(1) $\ln(Applications_{it})$	(2) Not Tenure Track	(3) Tenure Track	(4) Not Tenure Track	(5) Tenure Track
$Demographics_{it-3}$	7.767	5.468	3.067		
	(2.381)	(8.749)	(1.110)		
$Ln(Intl\ Students_{it-1})$	0.042	0.076	0.016		
	(0.008)	(0.020)	(0.005)		
State Poverty $Rate_{it-1}$	-0.327	0.540	-0.294		
	(0.218)	(0.575)	(0.118)		
Observable Factors				1.384	0.401
				(0.399)	(0.092)
Unobservable Factors				0.232	0.080
				(0.043)	(0.015)
Observations	20,923	19,119	20,318	18,987	20,166
R^2	0.969	0.917	0.991	0.918	0.991
Year FE	Yes	Yes	Yes	Yes	Yes
Institution FE	Yes	Yes	Yes	Yes	Yes

Notes: The data are taken from IPEDS with the definitions given in the paper. The sample spans the 2002-03 to 2016-17 academic years. Regressions cover the group listed at the top of the column. Standard errors are shown in the parentheses and clustered at the school level. The regressions are weighted by the total faculty head count for the school across the full sample. The observable factors are the variables listed in the column (1) regression. The unobservable factors are the residuals from the regression in column (1).

Table 5. Alternative Instruments for Faculty Labor Demand: Second Stage

Dep Variable: $\ln(W_{it})$	(1) Not Tenure Track	(2) Tenure Track	(3) Not Tenure Track	(4) Tenure Track
$\ln(L_{it})$	0.131	0.515	0.036	0.217
	(0.068)	(0.168)	(0.035)	(0.087)
Observations	19,119	20,318	18,897	20,166
Year FE	Yes	Yes	Yes	Yes
Institution FE	Yes	Yes	Yes	Yes
Instrument	Observable Factors	Observable Factors	Unobservable Factors	Unobservable Factors
First Stage F Stat	11.5	17.9	28.2	29.2
L Supply Elasticity	7.6	1.9	27.8	4.6

Notes: The data are taken from IPEDS with the definitions given in the paper. The sample spans the 2002-03 to 2016-17 academic years. Regressions cover the group listed at the top of the column. Standard errors are shown in the parentheses and clustered at the school level. The regressions are weighted by the total faculty head count for the school across the full sample. The instruments in (1) and (2) are the observable variables in the applications regression, and in (3) and (4) are the residuals from the applications regression as described in the text.

Table 6. First Stage Regressions in Schools Where Expansion is Costly

1st Stage:	(1)	(2)	(3)	(4)
Dep Variable: $\ln(L_{it})$	Not Tenure Track	Not Tenure Track	Tenure Track	Tenure Track
Sample	Top 10% Cost	Bottom 90% Cost	Top 10% Cost	Bottom 90% Cost
$\ln(\text{Applications}_{it-1})$	0.064	0.262	0.008	0.093
	(0.085)	(0.045)	(0.038)	(0.016)
Observations	1,836	18,237	1,847	19,459
R^2	0.925	0.915	0.987	0.991
Year FE	Yes	Yes	Yes	Yes
Institution FE	Yes	Yes	Yes	Yes

Notes: The data are taken from IPEDS with the definitions given in the paper. The sample spans the 2002-03 to 2016-17 academic years. Regressions cover the group listed at the top of the column. Standard errors are shown in the parentheses and clustered at the school level. The regressions are weighted by the total faculty head count for the school across the full sample. Columns (1) and (3) restrict the sample to schools in the highest decile of spending per student as defined in the text. Columns (2) and (4) restrict the sample to schools with lower cost per student as defined in the text.

Table 7. Inverse Elasticity of Labor Supply: Wage Differentiation

Dep Variable: $\ln(W_{it})$	(1) Not Tenure Track	(2) Tenure Track
$\ln(L_{it})$	0.036	0.199
	(0.032)	(0.082)
$\ln(L_{it})$ x Flow $Rate_{it}$	0.003	-0.019
	(0.002)	(0.011)
Flow $Rate_{it}$	-0.014	0.046
	(0.011)	(0.047)
Observations	20,146	21,879
Year FE	Yes	Yes
Institution FE	Yes	Yes
Academic Rank	NT	TT
First stage F Stat	16.8	15.4

Notes: The data are taken from IPEDS with the definitions given in the paper. The sample spans the 2002-03 to 2016-17 academic years. Regressions cover the group listed at the top of the column. Standard errors are shown in the parentheses and clustered at the school level. The regressions are weighted by the total faculty head count for the school across the full sample. The instrument is the lagged level of log applications and interactions as relevant in the column. 'Flow Rate' measures the net hiring rate as a share of the stock from the preceding year.

Table 8. Inverse Elasticity of Labor Supply: Size and Market Share

	(1)	(2)	(3)	(4)
Dep Variable: $\ln(W_{it})$	Not Tenure Track	Tenure Track	Not Tenure Track	Tenure Track
$\ln(L_{it})$	0.034	0.077	0.035	0.042
	(0.034)	(0.087)	(0.034)	(0.087)
$\ln(L_{it})$ x National %	-0.057	3.015	0.010	2.935
	(0.164)	(1.103)	(0.151)	(0.975)
$\ln(L_{it})$ x State %	0.003	0.003		
	(0.002)	(0.010)		
$\ln(L_{it})$ x C-Zone %	-0.000	-0.002		
	(0.000)	(0.001)		
Observations	19,057	21,341	19,083	21,409
Year FE	Yes	Yes	Yes	Yes
Institution FE	Yes	Yes	Yes	Yes
Academic Rank	NT	TT	NT	TT
First stage F Stat	7.8	5.7	15.3	11.3

Notes: The data are taken from IPEDS with the definitions given in the paper. The sample spans the 2002-03 to 2016-17 academic years. Regressions cover the group listed at the top of the column. Standard errors are shown in the parentheses and clustered at the school level. The regressions are weighted by the total faculty head count for the school across the full sample. The instrument is the lagged level of ln(Applications) and interactions as relevant for the column. National % is the percent of the national non-tenure track or tenure track labor market accounted for by the school in 2003. State % is the percent of the state's academic labor market accounted for by the school in 2003. C-Zone % is the same but for the school's commuting zone.

Table 9: Search Frictions/Job Switching Costs and Monopsony

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dep Variable: $\ln(W_{it})$	Professor	Associate Prof.	Assistant Prof.	Non-Ten. Track	Ten. Track	Non-Ten. Track	Ten. Track
$\ln(L_{it})$	0.548	0.337	0.130	0.015	0.167	0.023	0.181
	(0.161)	(0.078)	(0.066)	(0.035)	(0.079)	(0.031)	(0.083)
$\ln(L_{it})$ x Lab Fields Share				0.152	0.493		
				(0.068)	(0.079)		
Lab Fields Share				-0.470	-2.562		
				(0.236)	(0.442)		
$\ln(L_{it})$ x Male						0.022	0.008
						(0.003)	(0.008)
Observations	21,751	21,504	21,418	20,567	21,888	38,670	43,693
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Institution FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
First stage F Stat	15.2	30.0	25.4	16.6	16.6	16.9	15.7

Notes: The data are taken from IPEDS with the definitions given in the paper. The sample spans the 2002-03 to 2016-17 academic years. Regressions cover the group listed at the top of the column. Standard errors are shown in the parentheses and clustered at the school level. The regressions are weighted by the total faculty head count for the school across the full sample. The instrument is the lagged level of ln(Applications) and interactions as relevant for the column. 'Lab Fields Share' is the share of undergraduate degrees granted in laboratory science and engineering fields as described in the text. Columns (6) and (7) combine for men and women for the same institution (hence the doubling of the number of observations) and include a dummy variable for men.

Table 10. Inverse Elasticity of Labor Supply: Long Differencing

Dep Variable: $\ln(w_{17}) - \ln(w_{03})$	(1) Non-Tenure Track	(2) Tenure Track
$\ln(L_{17}) - \ln(L_{03})$	0.085	0.306
	(0.034)	(0.073)
Observations	1,119	1,291
Year FE	No	No
Institution FE	No	No
First stage F Stat	27.2	26.0
Instruments	$\ln(Apps_{16})$-$\ln(Apps_{02})$	$\ln(Apps_{16})$-$\ln(Apps_{02})$

Notes: The data are taken from IPEDS with the definitions given in the paper. The sample spans the 2002-03 to 2016-17 academic years. Regressions cover the group listed at the top of the column. Standard errors are shown in the parentheses and clustered at the school level. The regressions are weighted by the total faculty head count for the school across the full sample. The dependent variable is the long-difference in log wages and the instrument is the lagged long-difference in log applications as described in the text.

Table 11: Inverse Elasticity of Labor Supply: Institutional Prestige and Market Power

Dep Variable: $\ln(W_{it})$	(1) Non-Ten. Track	(2) Tenure Track	(3) Non-Ten. Track	(4) Tenure Track	(5) Non-Ten. Track	(6) Tenure Track
$\ln(L_{it})$	-0.003	0.079	0.023	0.145	-0.075	-0.246
	(0.034)	(0.071)	(0.033)	(0.076)	(0.064)	(0.068)
$\ln(L_{it})$ x USN Top Tier	0.088	1.165				
	(0.051)	(0.250)				
$\ln(L_{it})$ x USN Tier 2-4	0.096	0.509				
	(0.029)	(0.090)				
$\ln(L_{it})$ x Test Score Q4			0.060	0.592		
			(0.031)	(0.100)		
$\ln(L_{it})$ x Salary Pctile (1-100)					0.002	0.009
					(0.001)	(0.002)
Observations	20,727	21,996	20,727	21,996	19,174	21,206
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Institution FE	Yes	Yes	Yes	Yes	Yes	Yes
1^{st} stg F-Stat	13.7	12.4	18.6	19.2	12.7	18.9

Notes: The data are taken from IPEDS with the definitions given in the paper. The sample spans the 2002-03 to 2016-17 academic years. Regressions cover the group listed at the top of the column. Standard errors are shown in the parentheses and clustered at the school level. The regressions are weighted by the total faculty head count for the school across the full sample. The instrument is the lagged level of log Applications and interactions as relevant in the column. 'Test Score Q4' is a dummy variable for whether the school's median SAT score or ACT score is in the top quartile of schools in IPEDS at the start of our sample. 'Salary Pctile' is the percentile ranking of the university salary level for full professors at the start of our sample.

Figure 1:

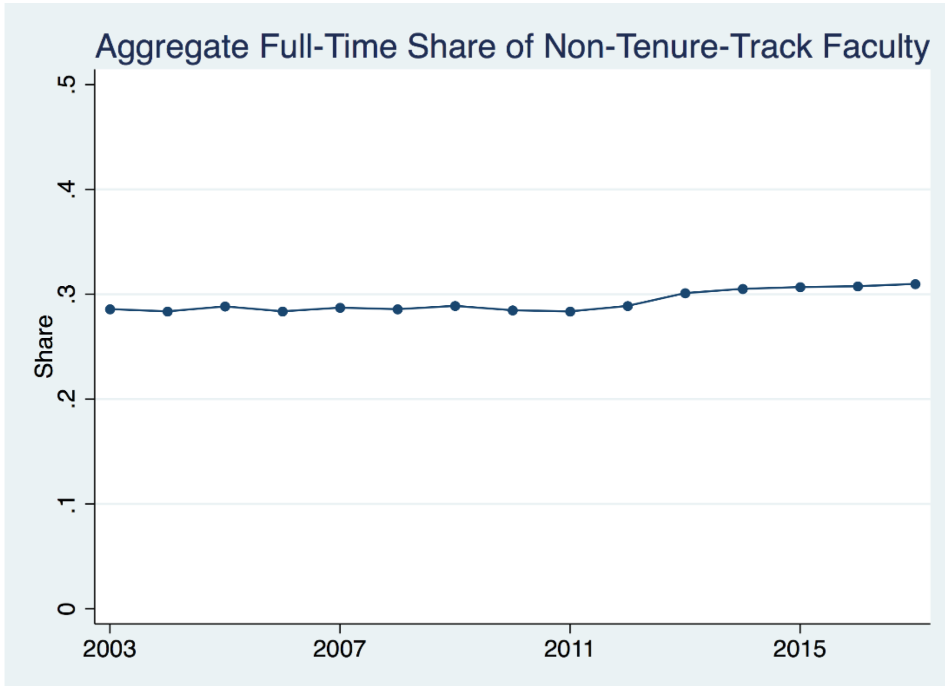


Figure 2:

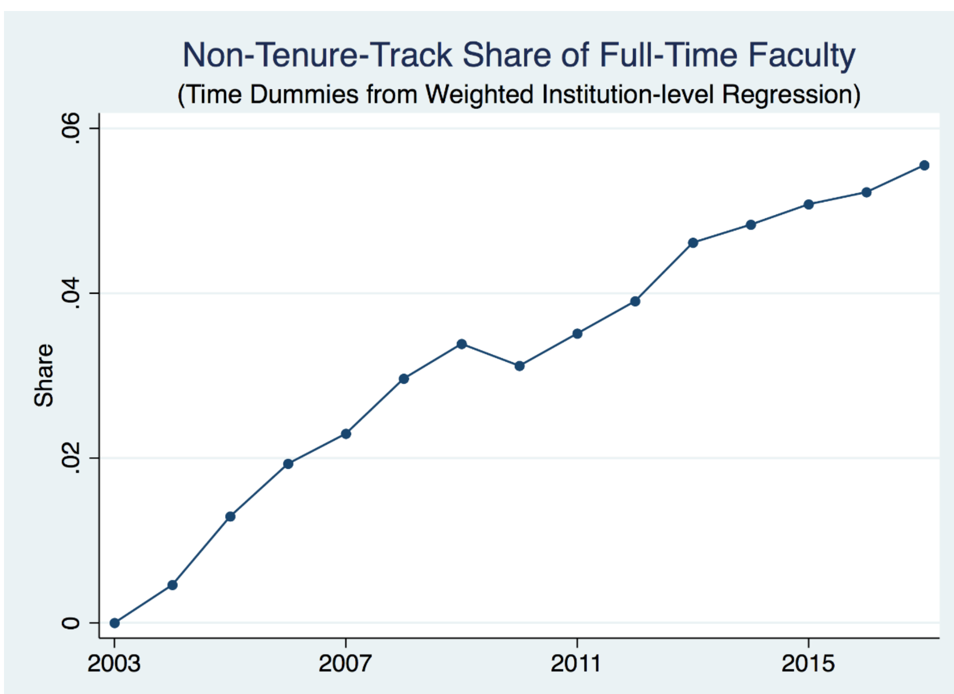


Figure 3:

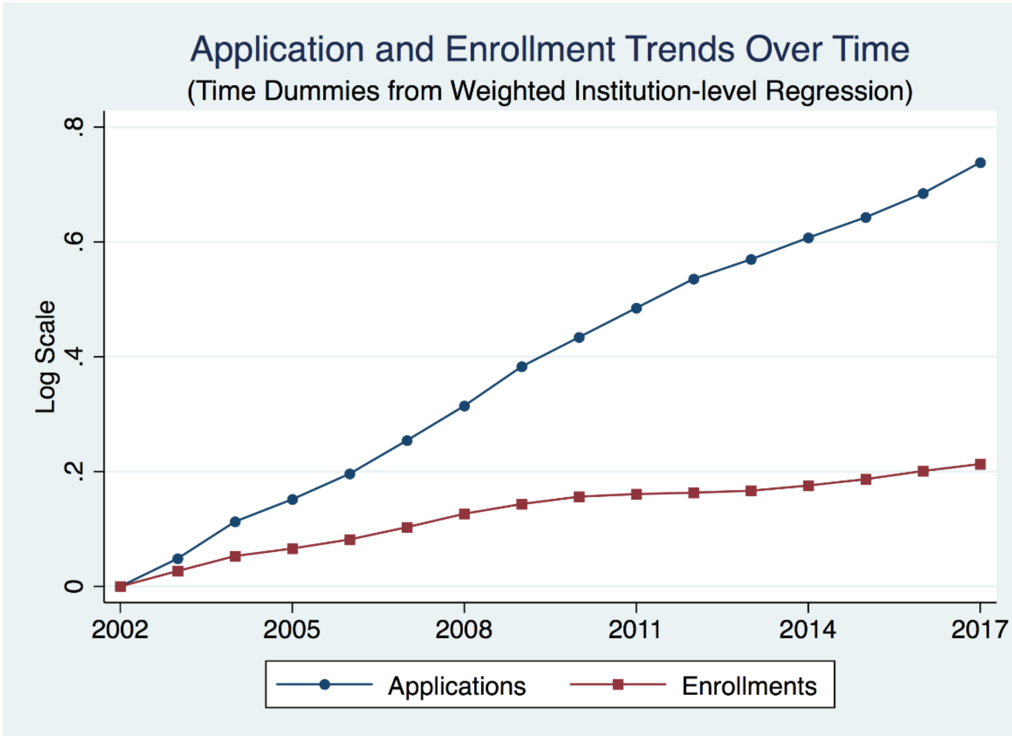

Application and Enrollment Trends Over Time
(Time Dummies from Weighted Institution-level Regression)
Log Scale
0
.2
.4
.6
.8
2002
2005
2008
2011
2014
2017
Applications
Enrollments

Figure 4:

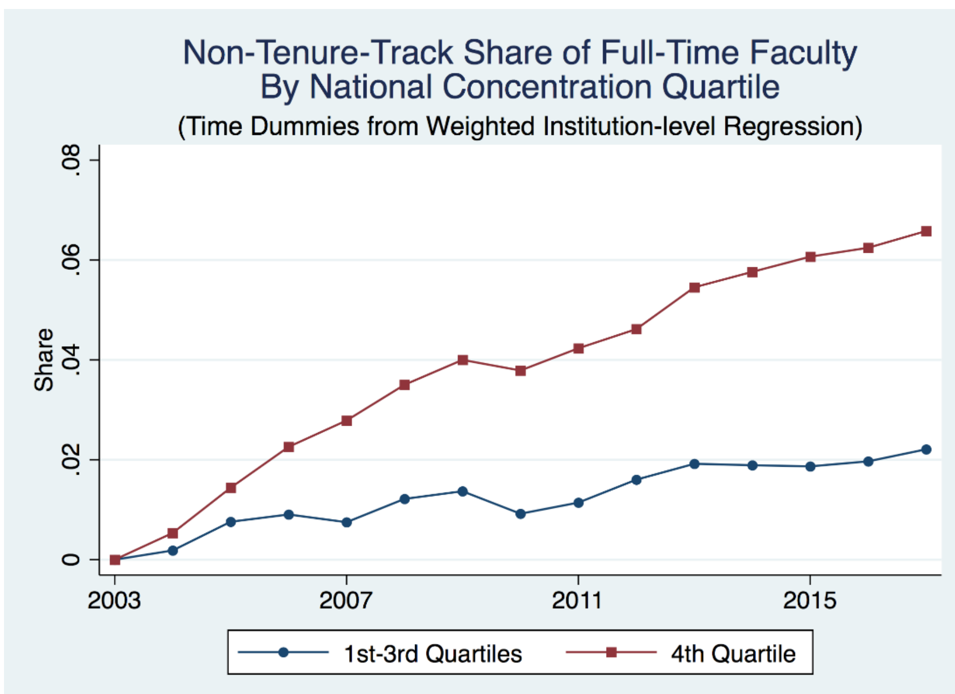

Non-Tenure-Track Share of Full-Time Faculty
By National Concentration Quartile
(Time Dummies from Weighted Institution-level Regression)
Share
0
.02
.04
.06
.08
2003
2007
2011
2015
1st-3rd Quartiles
4th Quartile

Figure 5:

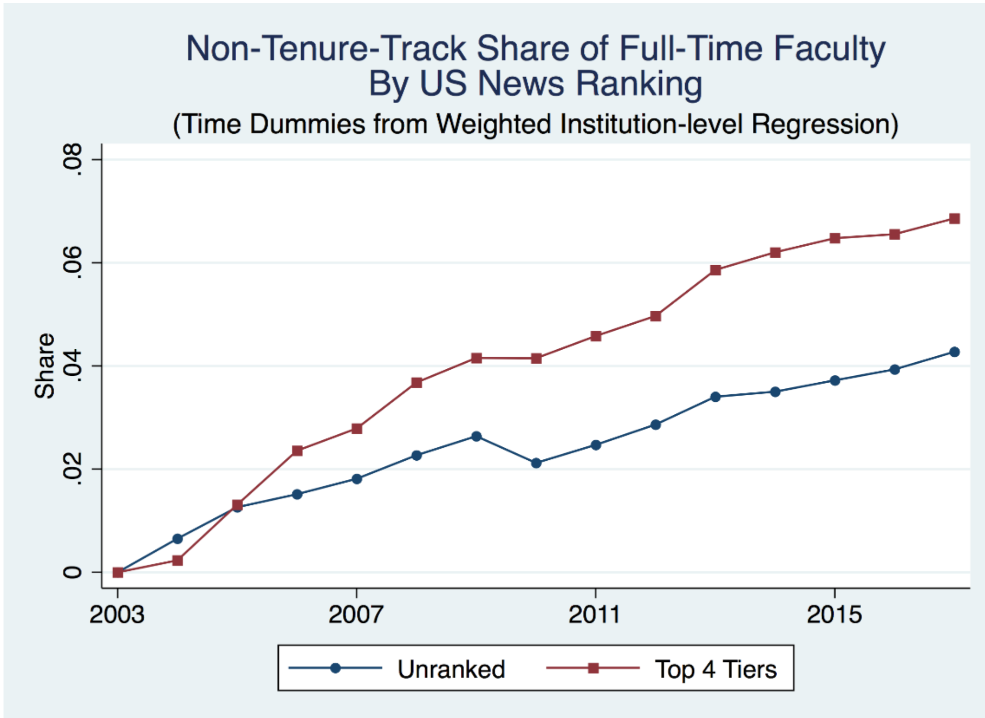


Figure 6:

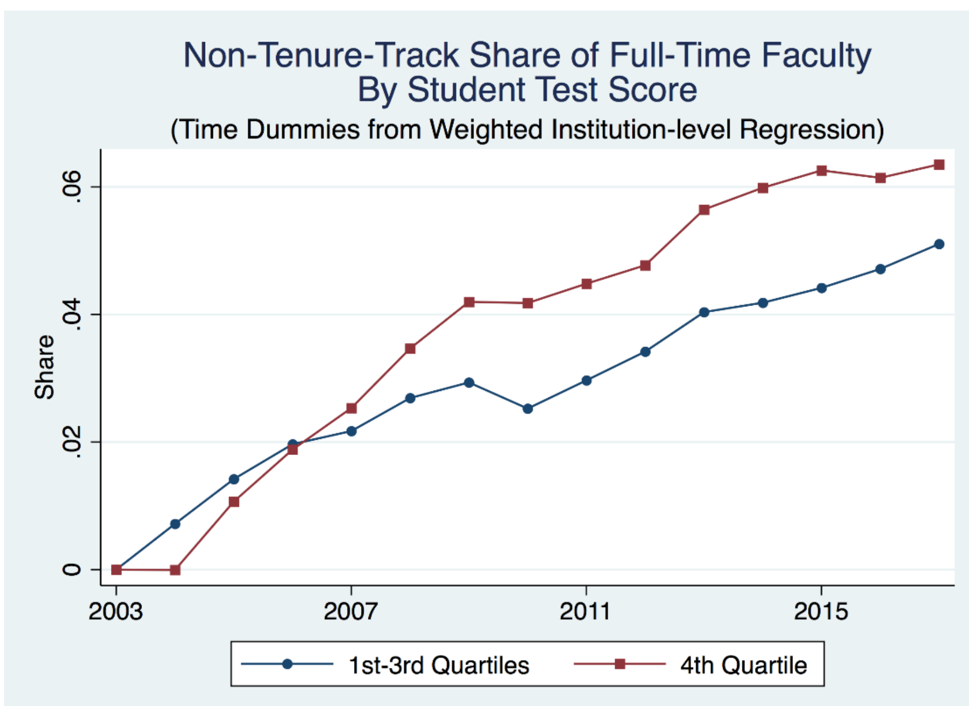
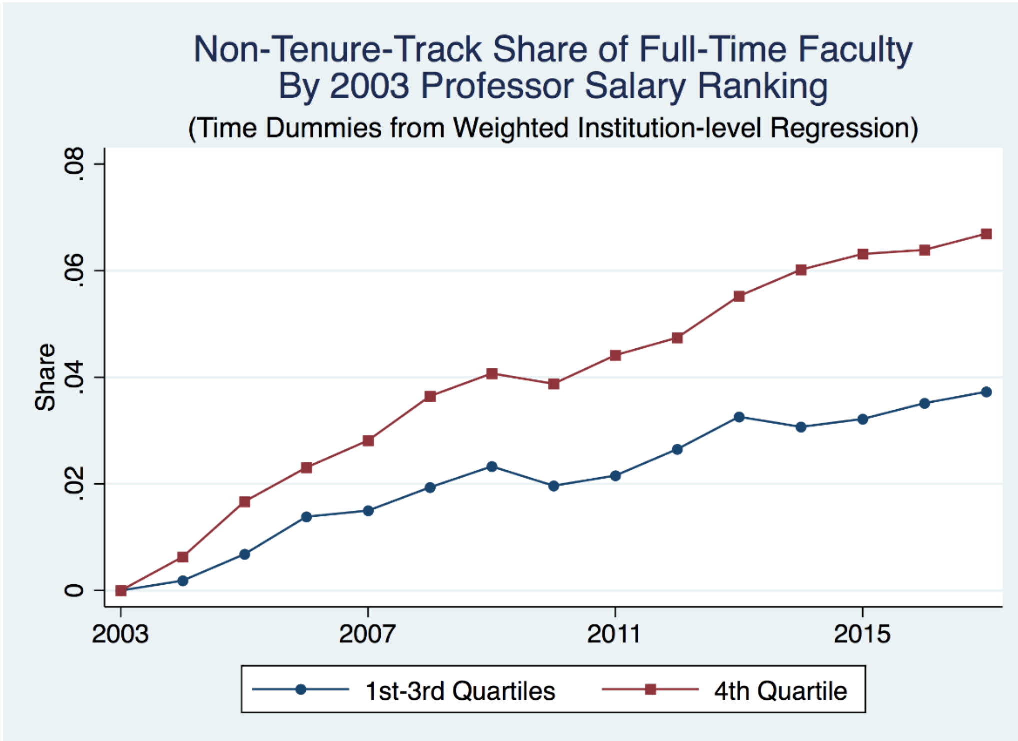


Figure 7:



Non-Tenure-Track Share of Full-Time Faculty
By 2003 Professor Salary Ranking
(Time Dummies from Weighted Institution-level Regression)
Share
0
.02
.04
.06
.08
2003
2007
2011
2015
1st-3rd Quartiles
4th Quartile

Appendix 1: Data on Individual Faculty in the University of California System

A. Average Salary Change Decomposition, New vs Continuing Faculty

To test whether wage differences for new versus existing faculty might be contributing to our findings of monopsony, we compiled data from University of California Office of the President (2019) on the individual salaries of faculty at the University of California system's nine campuses.

Salary data are available from 2010-2018, but inspection of the data makes clear that reported salary amounts for arriving or departing faculty often cover only partial years. Unfortunately, the fraction of an annual pay rate that these first or last observations cover is unspecified, so normalizing to annual pay would not be straightforward. Instead, we dropped observations for 2010 and 2018 as well as the first or last observations of faculty entering or leaving the panel in the interim period. This leaves us with the ability to decompose average annual salary growth for the six years from 2012-2017.[29]

With some slight abuse of notation, let C denote the set and number of continuing faculty present in both periods $t-1$ and t, N be the set and number of new faculty (those employed in t but not $t-1$), and X be the set and number of exiting faculty (those employed in $t-1$ but not t). Label as s_{it} the salary of faculty member i in period t. Then we can express the change in the average salary across faculty from $t-1$ to t as:[30]

$$= \bar{s}_{Ct} - \bar{s}_{Ct-1} + \frac{N}{C+N}(\bar{s}_{Nt} - \bar{s}_{Ct}) + \frac{X}{C+X}(\bar{s}_{Ct-1} - \bar{s}_{Xt-1})$$

In the appendix table below, we compute this decomposition for the 54 school-year observations by faculty rank.

For each faculty rank, hires and departures contributed little to the average salary increase. Indeed, in each case the within (continuing faculty) component is larger than the overall average change. In other words, average salaries of only the continuing faculty grew slightly more than overall average

[29] If we include the partial year salary reports, the composition results described below only become stronger, because the (artificially) low reported salaries of new faculty tend to reduce computed average wage growth rather than raise it.
[30] For simplicity this assumes the size/share for the different groups doesn't change over time. In reality there are some small changes, so average salaries move slightly with the change in the weighting. However, these terms were miniscule, so in our tables we report only the three main terms.

salaries. The combined compositional effects of entry and exit are slightly negative. There is no sign that average wage growth is reflecting a substantial influence of new faculty being hired on at higher wages than incumbent professors. Hence the wage increases that accompany hiring in response to demand shocks seem to reflect substantial inframarginal wage growth.

Appendix Table A. Decomposition of Average Annual Salary Change

	Professor	Associate Prof	Assistant Prof
Total Average Salary Change	0.039	0.040	0.034
Within/Continuers	0.049	0.043	0.036
Entering Faculty	-0.010	0.004	0.004
Exiting Faculty	0.000	-0.006	-0.004

Notes: The table reports the average for each statistic using the individual faculty salary data from UCOP (2019) according to the method described in the text. There are 54 school-years of data from the 9 schools over the 2012-2017 period.

B. Mobility Between Tenure Track and Non-Tenure Track Faculty

If the tenure and non-tenure track labor markets are not independent (i.e., if people choose to switch between them depending on the relative wages, for example), a profit-maximizing monopsonist would take that interdependence into account when setting wages and it is not correct to estimate separate residual labor supply curves for different types of faculty.

On one hand, most academic fields draw non-tenure track faculty from the same Ph.D. programs as tenure track faculty so they look like they could be substitutes. On the other hand, however, wide differences in pay between the tenure track and non-tenure track faculty (tenure track faculty's average monthly pay is 45% higher than full-time adjunct faculty's and that is similarly true at every quartile of the distribution, as well) suggest they may be rather different jobs.

We can also look in more detail at the University of California system using data on individual faculty over time to understand the degree of mobility between the tracks.

In Appendix Table B, we compose the transition matrix for tenure track faculty and non-tenure track faculty according to our same definitions of faculty roles. They show the number and share of people that started the period in a faculty position in the University of California system on the track given by the row that were, at the end of the period, in a faculty position as given by the column. The panel splits evenly into two periods. The upper panel looks at faculty in 2010 and where they ended up in 2013. The lower panel is from 2014 to 2017.[31]

The data show there is virtually no mobility for individual faculty members across tracks within the system.[32] Individual faculty members either stay on the track they started with or exit the University of California system.

Appendix Table B: Transition Matrix of Tenure and Non-Tenure Track Jobs

Period: 2010-13	Tenure Track (2013)	Non-Tenure Track (2013)	Missing/Exit (2013)
Tenure Track (2010)	6,705 (88.3%)	8 (0.1%)	882 (11.6%)
Non-Tenure Track (2010)	14 (0.3%)	2,744 (54.9%)	2,237 (44.8%)

Period: 2014-17	Tenure Track (2017)	Non-Tenure Track (2017)	Missing/Exit (2017)
Tenure Track (2014)	6,857 (90.6%)	9 (0.1%)	704 (9.3%)
Non-Tenure Track (2014)	9 (0.2%)	3,128 (58.8%)	2,185 (41.1%)

Notes: The two panels of the table report the number of individual faculty with a faculty position at the start of the period as defined in the left column that ended the period three years later in the position as defined in the top row using the data on individual faculty in UCOP (2019) as described in the text. The non-tenure track faculty positions in the table include full- and part-time positions (because the data do not distinguish them). The same basic findings resulted from restricting the non-tenure track sample to individual faculty members making at least $50,000 in the base year.

[31] We looked at annual transition matrices, as well and the share crossing tracks was even smaller than in the current table—well below one tenth of one percent in every year of the sample. We also restricted the non-tenure track faculty to having a salary of over $50,000 in the initial year to exclude part-time adjunct faculty but it made no difference to the share of faculty crossing tracks.

[32] We do not have information on switching tracks as well as institutions (e.g., from a non-tenure track job in the University of California system to a tenure track job elsewhere) but given that our estimated residual labor supply curves are at the school level, this is the relevant transition matrix for our purpose.

Appendix 2: Substitution across Faculty Labor Types with Cobb-Douglas Production

For a college with a CRS Cobb-Douglas production function in the two faculty types (tenure-track l_T and non-tenure track l_N: $q = l_N^{\alpha} l_T^{1-\alpha}$), and (inverse) residual labor supply curves that are isoelastic ($w_N = l_N^{\gamma}$ and $w_T = l_T^{\mu}$), the college's cost minimization is:

$$\min_{l_N, l_T} l_N^{\gamma+1} + l_T^{\mu+1} \quad s.t. \bar{q} = l_N^{\alpha} l_T^{1-\alpha}$$

The first-order conditions are:

$$(\gamma + 1) l_N^{\gamma} = \lambda \alpha l_N^{\alpha-1} l_T^{1-\alpha}$$

$$(\mu + 1) l_T^{\mu} = \lambda (1 - \alpha) l_N^{\alpha} l_T^{-\alpha}$$

Solving gives an expression for one type of faculty hiring in terms of the other:

$$l_N = \left[\frac{(\mu + 1)\alpha}{(\gamma + 1)(1 - \alpha)}\right]^{\frac{1}{\gamma+1}} l_T^{\frac{\mu+1}{\gamma+1}}$$

Plugging this into the production function, the relationships between the level of output, $\bar{q}$, and optimal hiring by type are:

$$l_N = C_1 \bar{q}^{\frac{\mu+1}{\alpha\mu+\gamma+1-\alpha\gamma}}$$

$$l_T = C_2 \bar{q}^{\frac{\gamma+1}{\alpha\mu+\gamma+1-\alpha\gamma}}$$

Where C_1 and C_2 are constant functions of parameters.

The elasticities of faculty hiring of each type with respect to desired output are

$$\varepsilon_{l_N,\bar{q}} = \frac{\mu + 1}{\alpha\mu + \gamma + 1 - \alpha\gamma}$$

$$\varepsilon_{l_T,\bar{q}} = \frac{\gamma + 1}{\alpha\mu + \gamma + 1 - \alpha\gamma}$$

As colleges expand, their hiring of non-tenure track faculty will be more responsive to growth than non-tenure track hiring if:

$$\varepsilon_{l_N,\bar{q}} > \varepsilon_{l_T,\bar{q}} \quad \Leftrightarrow \quad \mu > \gamma$$

That is, an expanding school substitutes away from whichever type of labor it has more monopsony power over.

The equilibrium ratio of the two types of faculty will be

$$\frac{l_N}{l_T} = \frac{C_1 \bar{q}^{\frac{\mu+1}{\alpha\mu+\gamma+1-\alpha\gamma}}}{C_2 \bar{q}^{\frac{\gamma+1}{\alpha\mu+\gamma+1-\alpha\gamma}}} = \frac{C_1}{C_2} \bar{q}^{\frac{\mu-\gamma}{\alpha\mu+\gamma+1-\alpha\gamma}}$$

and the elasticity of this ratio with respect to quantity is

$$\varepsilon_{l_N/l_T,\bar{q}} = \frac{\mu - \gamma}{\alpha\mu + \gamma + 1 - \alpha\gamma}$$

If non-tenure track labor markets are perfectly competitive, $\gamma = 0$, and this elasticity simplifies to

$$\varepsilon_{l_N/l_T,\bar{q}} = \frac{\mu}{\alpha\mu + 1} = \frac{1}{\alpha + 1/\mu}$$

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