

Do Elite Universities Overpay Their Faculty?*

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No. Elite institutions offer high salaries because they hire the most valued faculty. Moreover, in contrast to the broader labor market, faculty are equally likely to move up and down the prestige ladder, and they increase their salary either way. We speculate that these facts reflect the visible nature of faculty productivity and the sporadic nature of academic job openings.

JEL codes: I23, J31, J44

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1 Introduction

We show that the labor market for tenure-stream (tenured and tenure-track) STEM faculty has several features that differ from the broader labor market. In general labor markets, there is a hierarchy of firms, with some paying more on average than others for seemingly identical workers (Bagger and Lentz, 2019; Moscarini and Postel-Vinay, 2018; Haltiwanger et al., 2018). This hierarchy is identified as firm fixed effects in AKM (Abowd et al., 1999) wage equations that allow worker and firm fixed effects. (See Kline 2024 for a review of studies finding firm effects in the broader labor market.)

In broader labor markets, workers moving to more elite or higher-paying firms see wage increases, mobility is more frequently upward than downward, and downward movement results in losses roughly equal to the gains from upward movement. At least some of these firm effects represent rents – a share of profits or return on capital captured by workers (see Card et al., 2018 for a review).

In academia, there is also a clear hierarchy of universities (measured by rankings and research emphasis). And, as others have found (Ehrenberg, 2003; Bound et al., 2019; Rippner and Toutkoushian, 2015) and we confirm, average salaries are higher at elite universities as measured by their rank or endowment. Faculty salaries and productivity are linked both within and across universities, whether measured by the number of publications, citations, or the h-index.¹

Yet in some other respects, academia is very different from other labor markets. We show that, in contrast to the broader labor market, tenure-stream STEM faculty moving up and down the hierarchy experience similar wage changes, and movements up and down are approximately equally likely.

¹Katz (1973); Smith and Choudhry (1978); Hamermesh et al. (1982); Hamermesh and Pfann (2012); Hilmer et al. (2015); Ransom et al. (2022); Kwiek (2018); Li and Koedel (2017); Faria and Mixon Jr (2021); Sandnes (2018); MacLeod and Urquiola (2021).

To ascertain whether faculty can capture some of the greater resources at elite institutions and, therefore, receive a premium (or rent) relative to their potential salary elsewhere, we estimate an AKM salary model using longitudinal data on academics in STEM fields. We find that firm effects/rents are small to nonexistent. In contrast to studies of firm fixed effects in the broader labor market, institution fixed effects in academia explain little of the earnings variance, so that prestige or endowment *per se* generates, at most, modest earnings differentials, even between the best and worst-ranked or richest and poorest institutions. Instead, salaries are higher at more elite institutions because they hire more elite and, thus, high-earning faculty. Consequently, when faculty move institutions, their salary change is unrelated to the relative prestige change.

What explains these striking differences between the academic and broader labor markets? First, the absence of university effects on salary may result from nonpecuniary compensation or amenities that increase with eliteness more rapidly in academia than in the broader market. Faculty at elite institutions are likely to have lighter teaching loads, more resources for their research, and better students (Claypool et al., 2017), which could substitute for higher salaries. Still, some authors suggest the opposite. Faculty at elite institutions experience more pressure to publish in more highly ranked journals, face more demanding students, and, if not tenured, must meet higher standards for gaining tenure. They may suffer from comparisons with successful colleagues in their department (Frank, 1985). Ehrenberg et al. (1998) found a compensating differential for a lower tenure probability even within institution-prestige tier.² We examine the relation between job satisfaction and institution rank and find no evidence to support the hypothesis that better working conditions explain why prestige rents are absent. Moreover, compensating wage differentials cannot account for the unusual mobility pattern in academia.

²They also found that institutions appearing to pay a compensating differential for low tenure rates seemingly pay a premium to their tenured faculty, which requires a more complex explanation.

Alternatively, we may fail to find prestige rents because faculty salaries depend on their outside option and indirectly its prestige, not their current job's, as suggested by the models of Postel-Vinay and Robin (2002), Cahuc et al. (2006), and Bagger et al. (2014). We follow Di Addario et al. (2023) in testing this explanation by allowing salary to depend on both the current and previous employer (their outside option). The results provide little support for this explanation, which, again, does not predict the unusual mobility patterns observed.

We suggest that the mobility and salary patterns we observe reflect that matching is important, but that jobs at specific university departments are available sporadically, making many initial matches imperfect. Salaries increase as faculty matches improve due to moves up or down the prestige ladder. Moreover, perhaps in contrast with the general labor market, moves mostly do not reflect new information; there is good information about STEM faculty productivity even when individuals first enter the assistant professor market.

2 Data

We combine data from (i) restricted-use faculty job histories for US STEM academics, (ii) publicly available university and college rankings, and (iii) administrative data about higher-education institutions.

Our job-history data comes from the restricted-use version of the Survey of Doctorate Recipients (SDR) from the National Center for Science and Engineering Statistics (NCSES). The SDR is a representative longitudinal panel of doctoral recipients from US academic institutions in natural or social sciences, engineering, or health. The survey collects salary, employer, and demographic information every 2-3 years. It also provides IPEDS (Integrated Postsecondary Education Data System) code for all US academic employers, allowing us to construct a matched employer-employee panel of US academics.

We use all SDR waves from 1993 to 2019. Thanks to the SDR structure, our panel has good longitudinal coverage of existing faculty while incorporating data from newly minted

PhDs. The SDR includes most survey participants from previous waves, adds newly granted PhDs (from the National Science Foundation’s Survey of Earned Doctorates), and drops those who age out. Its response rate among US residents who can be found is high, typically more than 95% of eligible respondents. Adding those not found, missing a key item, or living abroad to the non-responses lowers the rate to 75%-85%. However, in 2015, the SDR created a new, larger panel that included only a minority of the original sample. Therefore, most participants only have data before 2015 or from 2015 onwards.

We restrict the sample to individuals employed full-time (35 hours/week for at least 40 weeks/year) in a tenure-stream (tenured or tenure-track) position at a US 4-year college or university, university medical school, or university research institute. We exclude 2-year colleges, junior colleges, technical institutes that do not confer regular degrees, and non-educational institutions. We drop observations where respondents were in a post-doctoral position, earned less than the minimum National Institutes of Health post-doctoral stipend (\$53,760 in 2020), or worked outside the US (in academia or elsewhere).

Despite the high data quality, studying moves using the SDR requires considerable data cleaning that we describe in detail in Appendix A. There were 1,732 observations where the IPEDS university code changed, yet the respondent reported not changing institutions. These frequently involved two institutions with similar names. Thus, a faculty member who reported working at Boston University in multiple waves might be miscoded as Boston College faculty for one wave despite reporting not changing institutions. Academics know they are different; some data coders did not.

We convert salaries into real 2020 dollars using the CPI for all urban consumers (U.S Bureau of Labor Statistics, 2023). We drop observations with large one-time salary changes within the same institution *that were subsequently reversed* (see Appendix A for details). As shown in Appendix B (Tables B1-B4, including these observations has little effect on the results. Since these are within a person/university match, dropping them leaves the number of movers and moves unchanged.

We further restrict the job-history sample to observations in the largest connected set of institutions. As is well-known, AKM requires that included institutions be directly or indirectly connected. Institutions A and C are indirectly connected if a faculty member moves from A to B and another moves from B to C. Our largest connected set has 654 institutions. Other connected sets were minimal.

Our college and university rankings come primarily from the mutually exclusive *Times Higher Education* 2017 World University Rankings and the *Wall Street Journal – Times Higher Education* 2017 College Rankings (Times Higher Education, 2017a,b), hereafter *THE* rankings. We matched *THE*-ranked institutions to their IPEDS codes using their name and location, and applied the procedure described in Appendix A to resolve any ambiguities.

We matched 578 (88% of the total) of the 654 institutions to a *THE* rank. We imputed the ranks of the remaining 76 institutions using data from the *US News and World Report (USNWR)* rankings of best national universities, liberal arts colleges, and regional institutions. We used the overlap between the *THE* and *USNWR* datasets to impute, via a regression, the position of *THE*-unranked schools. Using this procedure, we imputed a rank for 50 schools ranked in *USNWR*, leaving only 26 unranked schools (4% of the total). We recognize that the prestige of individual departments may not always align with that of the overall institution. As a robustness check, we also present results that restrict the sample to Biological Sciences and Engineering PhDs, the largest PhD fields in the SDR data, and that use both the overall institution ranking and field-specific *USNWR* “Best Graduate School” rankings (USNWR, 2022, 2024).

We define *research universities* as those in the *THE* university rankings or imputed from the *USNWR* National University rankings. This group encompasses institutions other than R1. We define *colleges* as those in the *THE* college rankings or imputed from other *USNWR* rankings. Many *colleges* are not liberal arts colleges but simply institutions not included among the *THE* research universities or *USNWR* National Universities. Within

each institution type, we normalize the best rank to 1 and the worst to 100.³

The top-ranked research universities are Stanford, Harvard, Cal Tech, and MIT. The worst-ranked include Western Michigan University, Texas State University, Oakland University, and the University of North Carolina, Wilmington. The top-ranked colleges are Amherst, Williams, Wellesley, and Pomona. The worst-ranked include Grambling State University, Southern University of New Orleans, Georgia Southwestern State University, and the University of the Rio Grande. The unranked institutions include Texas A&M at San Antonio, Brigham Young University at Idaho, and the University of Texas at Brownsville.

From the IPEDS surveys, we obtain total enrollment, number of faculty, endowment (converted to 2020 USD), and dummy variables for large city, urban fringe/mid-sized city/suburb, private institution, and undergraduate-only institution for 2001, 2005, 2012, and 2017.

Table 1 Panel A shows the frequency of moves. We have 63,376 observations on 26,135 individuals, an average of roughly 2.4 observations each. 1,805, or about 7% of individuals, changed institutions at least once. Unsurprisingly, we generally observe movers in more surveys. Movers account for roughly 12% of our observations.

Panel B shows that we observe only one move for most movers. We have 2,114 transitions involving 654 institutions and 1,805 movers, or 1.2 moves per mover and 3.2 moves per institution. Transitions are highly skewed among institutions, ranging from 1 to a maximum of 52.

When surveyed, 45% of faculty observations were full professors and 29% associate professors (see Panel C). A few faculty (1%) report being tenure-stream but hold a title other than assistant, associate, or full professor. About one-third of faculty are female; five-sixths are married when surveyed.

Panel D gives information on the 654 institutions in the connected set, of which 147 are *universities* and 481 *colleges*, with the remaining 26 unranked. They vary dramatically in size and endowment. 40% are private, and 13% serve only undergraduates.

³Due to ties, the lowest-ranked college is at the 99th percentile.

3 Faculty Mobility

In this section, we report some basic facts regarding mobility (section 3.1) and associated wage changes among tenure-stream STEM faculty (section 3.2).

3.1 Moves Up and Down are Equally Common; Large Moves are Unusual

Unsurprisingly, and as the top panel of Figure 1 confirms, there is a strong relation between average faculty salary and institution rank. The existence of a hierarchy is not unique to academia. There is strong evidence of a widely shared hierarchy of firms in the general labor market, with upward movements more common than downward, at least for job-to-job transitions (Bagger and Lentz, 2019; Moscarini and Postel-Vinay, 2018; Haltiwanger et al., 2018). Similarly, research using the AKM model finds that firms with higher firm effects attract and retain more workers (Nimczik, 2020; Bassier et al., 2022; Sørensen and Vejlin, 2013). If academia were similar to the general labor market, we would expect movements up the prestige ladder to be more common than movements down, despite a significant minority of movements to lower-paying firms (Sorkin, 2018).

Surprisingly, Figure 2 shows that movement up and down the prestige ladder is roughly visually symmetric. Moreover, we cannot reject that the median change in rank among movers is 0, and fail to reject symmetry at conventional levels.⁴ To address concerns that moves by untenured faculty are involuntary, we restrict moves to tenured faculty and obtain a similar pattern (see Figure C1).

While movement in either direction is equally likely, moves to proximate institutions are more likely than moves to institutions ranked very differently. In Figure 2's histogram,

⁴We rank log rank changes by their distance from 0 and treat positive and negative changes as separate samples. We then apply the Wilcoxon signed-rank test for two samples and cannot reject that they are the same ($p=.52$).

roughly one-eighth of moves involve a negligible change in rank. About two-thirds of moves involve an absolute change in log rank of less than 1. A change of 1 in log rank corresponds to movement between roughly the first and third percentile institutions (e.g., Princeton and the University of Pennsylvania) or the 37th and 100th percentiles (roughly between Virginia Tech and Loyola Marymount). Even movement between the 25th and 75th percentiles (e.g., the University of Florida and Kansas State) is somewhat unusual. The transition probability matrix (Appendix Table B5) illustrates the same mobility patterns, while providing details on university-college moves, including similar movements up and down and the prevalence of moves within one’s own or neighboring quantiles.⁵ Roughly 65% of those starting in universities move within +/- 1 university quintile (or to a more highly ranked college).

3.2 Moves Up and Down Have Similar Wage Changes

In the broader labor market, workers moving from a high to a low-wage firm suffer a wage loss approximately equal to the wage gain for workers moving in the opposite direction (Card et al., 2013, 2018; Bonhomme et al., 2023). We do not observe a similar asymmetry with respect to academic institution ranks.

Instead, tenure-stream faculty receive a salary increase of roughly 20 – 30% when changing institutions, regardless of the direction or magnitude of the prestige change between institutions (see Appendix Tables B7 and B8). Among those moving between universities, the largest average increase is among those moving within the second-highest quintile, while the lowest is among those moving within the middle quintile. University faculty moving from the top to the fourth quintile gain 18 log points on average, while those moving from the fourth to the top quintile gain 21 log points.

Figure C2 shows a binned scatter plot with quadratic fit of the change in log salary against the change in log rank. It reaches an extreme point near 0 and is symmetric, although 0 is the approximate minimum for movements among colleges and the maximum for universities.

⁵Table B6 shows the transition matrix for tenured faculty only.

The sign of the rank change does not predict the sign of the salary change.

Salaries increase an average of 25 log points when faculty move to a more prestigious university, by 24 log points when they move to a less prestigious one, and by 11 log points when the prestige change is negligible. The corresponding values for colleges are 16, 16, and 14.

As in the broader labor market, prior wage changes have little predictive power for the direction of movement. Restrictions on sample sizes prevent us from showing or describing the full set of movements. Still, in Figure C3, we illustrate salary movements over time across quartiles of coworkers' salaries for the six combinations we can report. There is no clear time pattern.

4 No Evidence of a Prestige Rent

In this section, we first review the AKM model and describe how it can be used to study university salary premia and factors affecting these premia (4.1). We then present our AKM estimation results, including the near-absence of institution effects and the limited role of either institution rank or endowment for explaining the small effects we find (4.2). In section 4.3, we reconcile the strong relation between average salary and rank with the absence of institution fixed effects in AKM by showing a notable positive correlation between faculty effects and rank, and then do further robustness checks (4.4). We discuss the impacts of time-varying individual characteristics in section 4.5, and then consider and reject some explanations for our failure to find prestige effects in sections 4.6 and 4.7.

4.1 AKM in the academic context

To address whether tenure-stream faculty at elite institutions receive a premium, we estimate the standard two-way fixed-effects model or AKM (Abowd et al., 1999) model:

$$\ln w_{ijt} = X_{it}\beta + \alpha_i + \gamma_j + \varepsilon_{ijt} \quad (1)$$

where w_{ijt} is annual salary, X_{it} is a vector of time-varying individual characteristics, and ε_{ijt} is an i.i.d. mean-zero error term.⁶

Institution fixed effects, γ_j , capture the tendency of an institution to pay all faculty a different salary than they would receive elsewhere. They may reflect compensating differentials or institutional rents shared with faculty.

Individual fixed effects, α_i , capture whatever factors raise a faculty member’s salary relative to other faculty in the same (or similar) institutions. They are typically interpreted as reflecting worker quality or productivity. However, they capture any factor affecting pay, including discrimination or, in our case, differentials across fields. We largely follow tradition and refer to this fixed effect as capturing worker (faculty) quality.⁷

Clearly, (1) makes strong assumptions. First, AKM assumes mobility is random conditional on α and γ . Formally, ε is uncorrelated with these terms (and X_{it}). Applied to academia, faculty do not move because the profession has changed its belief about them or

⁶In an antecedent to the AKM model, Hamermesh (1989) examined salaries of 100 full professor non-movers in six economics departments in two periods. In a robustness check, he added institution fixed-effects to faculty variables such as experience and citations, and found no significant institution effects and no changes to the coefficients on faculty variables. Similarly, in estimates without institution or faculty fixed effects, Claypool et al. (2017) found that both program rank and individual publications predict political scientists’ salaries.

⁷In some models (e.g., Eeckhout and Kircher 2011), skill and wages are negatively related, but low-skill workers suffer more unemployment (see also Abowd et al. 2019).

because they are particularly valuable at their new university. Instead, moves reflect changes in personal preferences or other random factors unrelated to a worker’s productivity. Second, the semi-log form restricts the institution effect to be proportional; a given university pays a constant percentage premium to all faculty it hires, except for the random error term ε_{ijt} . Similarly, an individual earning 20% more than the norm at one university would earn 20% more elsewhere, again, except for ε_{ijt} .

Third, the AKM model puts restrictions on the edge-effects (links) in a graph of connected firms, as discussed in Kline (2024). These restrictions are typically (approximately) satisfied if the wage changes from moving from firm A to B and B to A are approximately equal and opposite in sign. However, as discussed in footnote 8, the restrictions will also be satisfied, at least asymptotically, if movement from A to B and B to A is equally likely, as we have seen to be true for tenure-stream faculty. We show that this condition is well approximated in our data.⁸

This estimation excludes seniority from the time/varying individual characteristics be-

⁸We appreciate Pat Kline’s assistance on this point. For those readers who, like us, find the discussion in Kline (2024) challenging, we provide the following intuition. Suppose we observe only movements between A and B. Our estimate of the A premium will be

$$\hat{\gamma}_A = \frac{\Sigma_{B \rightarrow A} \Delta \ln w_i - \Sigma_{A \rightarrow B} \Delta \ln w_i}{M_{B \rightarrow A} + M_{A \rightarrow B}},$$

the sum of the wage gains among movers from B to A minus the sum of wage gains among movers from A to B divided by the number of movers. Now, suppose that the wage change is the sum of a true firm A effect, γ_A , and a mover premium, m , assumed to be independent of the direction of the move.

$$\hat{\gamma}_A = \frac{\Sigma_{B \rightarrow A} (\gamma_A + m) - \Sigma_{A \rightarrow B} (-\gamma_A + m)}{M_{B \rightarrow A} + M_{A \rightarrow B}}.$$

There are two generic settings in which $\hat{\gamma}_A = \gamma_A$: if $m = 0$ or $M_{B \rightarrow A} = M_{A \rightarrow B}$.

cause the conditions under which the AKM model is consistent when seniority affects salary are highly restrictive.⁹

In this paper, we are interested in knowing how university characteristics affect salary, controlling for faculty fixed effects. In contrast with most AKM applications, we can measure university quality directly using rankings. Therefore, after estimating the AKM model, we regress the estimated institution effects, $\hat{\gamma}$, on published university rank and other measures potentially correlated with university eliteness:

$$\hat{\gamma}_j = Z_j\Lambda + \eta_j + \nu_j \tag{2}$$

where Z is a vector of university characteristics, η is a random error term, uncorrelated with Z , consisting of unmeasured university characteristics, and ν is measurement error ($\hat{\gamma}_j = \gamma_j + \nu_j$).

We can also incorporate university characteristics in a salary equation by estimating (1) and (2) in a single step by substituting for γ_j in (1) to get

$$\ln w_{ijt} = X_{it}\beta + Z_j\Lambda + \alpha_i + \eta_j + \varepsilon_{ijt}. \tag{3}$$

This single-step model (3), while not technically an AKM model, provides an additional method for measuring the impact of university characteristics on salaries. We can control for seniority when estimating (3), since adding seniority in the single-step approach is not inconsistent with this model. In addition, the one-step method is less computationally demanding, allowing us below to consider an extension where salary also depends on the

⁹The loss of a seniority premium can be viewed as a negative m in footnote 8. By the logic shown there, it would be sufficient if the probabilities of movement from A to B and B to A were equal at each level of seniority. The necessary condition is somewhat weaker but remains strong.

prior institution. However, as Kline (2024) explains, the two-step approach is likely to be more robust.¹⁰ We will estimate (2) by feasible GLS but only correct the standard errors for clustering in (3).

4.2 Institution prestige or endowment has little impact on salaries

We first estimate the standard AKM model (1) with individual time-varying characteristics and fixed effects for both individuals and firms. (See Table B9 for the coefficients on individual time-varying characteristics from this estimation.) Table 2 shows that the overall variance of log salaries (which we measure in 2020 USD) is .141; the variance of the individual fixed effects, α , with no correction is .131 (93% of the overall variance). In contrast, the variance of the institution fixed effects γ is .029 (21% of the overall variance), in line with the 20% typically found in AKM models (Card et al., 2018).

The sum of these two variances thus exceeds the total variance. However, as is well known, we overestimate these variances, especially in settings like ours where many institutions experience little turnover (Andrews et al., 2008; Kline et al., 2020; Kline, 2024; Bonhomme et al., 2023). With the Andrews et al. (2008) correction, the variance of the individual fixed effects falls to .104 or 74% of the overall salary variance, while the variance of the institution fixed effects is only .012 or 8.5% of the overall variance (Table 2). Thus, institutions account for little of the total variance. Collapsing the data to the spell level to reduce measurement error, as in Bonhomme et al. (2023), gives a total variance of $\ln(\text{salaries})$ by spell of .140, similar to the overall variance. As Table 2 shows, the uncorrected variance explained by institution fixed effects (.027) is similar to the estimate without collapsing spells, but after

¹⁰In the two-step method, the institution dummies capture η as well as $Z_j\Lambda$. Therefore, our estimates of the γ s are unbiased even when correlated with η . However, with the one-step approach, the correlation between γ and η will generally also generate correlation between Z and η conditional on the faculty fixed effects and therefore, bias Λ per the Theil omitted-variable bias formula.

correction, this variance is even more negligible, .007 or only 4.3% of the salary variation. Finally, using the [Kline et al. \(2020\)](#) approach (not in table), the sample falls to 398 institutions (141 universities, 247 colleges, 10 unranked),¹¹ and the corrected variance of the institution effects becomes slightly negative, which is unsurprising when the true variance is small.

The AKM analysis closest to ours for the broader labor market comes from [Abowd and McKinney \(2024\)](#) who use the Longitudinal Employer-Household Dynamics data set for the US from 1994 to 2017. In estimates controlling for experience and hours worked, the variance of firm effects ranges from .038 to .061. [Kline \(2024\)](#) summarizes 24 estimates from 13 studies, all using the KSS correction, with estimates ranging from .024 for Veneto, Italy, to .488 in Brazil. All are much larger than our corrected variances of .012 and .007 or our KSS-corrected negative variance.

Although our estimates show institution effects are nearly absent, we analyze whether institution characteristics, particularly institution rank and endowments, explain these institution effects. Because rank and endowment are highly correlated, we include rank in [Table 3](#) and endowments in [Table 4](#).

We next regress the 654 institution fixed effects, $\hat{\gamma}_j$, from our AKM model on institution type (research university, college, unranked), type interacted with rank, and further institution characteristics. Columns (1)-(3) of [Table 3](#) give the results. Then, we include these characteristics directly in the log salary equation in the one-step model [\(3\)](#) (columns 4-6), which allows us to control for seniority (along with other time-varying individual characteristics). The coefficients on research university and college represent the pay gap between the most prestigious institutions in each category and unranked institutions. The interaction terms at the top of the table show the effect of rank within institution type. The best rank is $\log(1)$ or 0. The worst is $\log(100)$ or 4.6.

¹¹[Kline et al. \(2020\)](#)'s correction requires restricting the sample to the institutions that remain connected when any single mover is excluded from the sample.

Column (1) shows only a modest effect of rank on salaries, even without controls for further institution characteristics. The expected salary difference between the most and least prestigious institutions is 14% for universities and 10% for colleges. The salary gap between unranked institutions and the least prestigious universities and colleges is small. The coefficient on university rank falls well short of significance at conventional levels; however, the two rank variables are jointly significant at the .06 level, and the four coefficients are jointly significant at the .02 level.

The unimportance of rank is not due to the regression’s linearity. Panel (b) of Figure 1 plots binned institution fixed effects (where institutions are weighted by the number of movers in the institution) against institution rank separately for universities and colleges and fits quadratic curves. For universities (shown with diamonds), the gap between the peak (at top ranks) and bottom institutions is noticeable but small (less than ten log points). For colleges (shown with circles), even the difference between the peak and trough is negligible. This differs considerably from the top panel, which shows a strong negative relation between binned *average log salary* and institution rank. The minimal relation between rank and premiums is robust to not weighting by movers and to grouping institutions with few movers but similar rank (Figure C4).

Column (2) in Table 3 adds urbanicity, which, not surprisingly, significantly affects salaries and somewhat reduces the already modest effect of the two rank variables individually and jointly, and even the four institution types and rank variables are jointly significant at only the .1 level. Adding three additional institutional characteristics (log of total enrollment, undergrad, private/public) in column (3) has little effect on the other coefficients but makes the four variables jointly insignificant.¹²

¹²Previous research finds that private universities pay more than public ones (Alexander, 2001; Ehrenberg, 2003; Zoghi, 2003; Zhang and Liu, 2010; Curtis and Thornton, 2014; Rippner and Toutkoushian, 2015; Cheslock and Callie, 2015). However, we find a small and insignificant coefficient on private institutions with a confidence interval that rules out a

Columns (4)-(6) show that the results using one-step estimates are generally similar or smaller but more precise. The net effect is that the rank effects are less significant for colleges and more significant for universities. Still, with the largest set of controls, the salary gap between the most and least prestigious universities is about 9%. The comparable figure for colleges is 4%.

In Table 4, we redo the estimation of Table 3, replacing the rank of universities and colleges with the institution’s log endowment per student, which captures the institution’s resources and the rents it can share. Endowment has a statistically significant but modest effect. Endowment and university type together still explain less than 2% of the variation in university effects (column 1). Moreover, the difference between the largest and smallest endowment per student predicts only a 12% difference in the institution salary effects (γ_j) in column (1).

4.3 More prestigious institutions attract better faculty

To reconcile the two facts that elite schools pay more but do not have higher fixed effects (as shown in the two panels of Figure 1), more prestigious institutions must hire faculty with higher fixed effects. The bottom of Table 3 displays the correlation between individual fixed effects and log rank for each specification. Recall that the best log rank is 0. Therefore, we expect a negative correlation between prestige and individual fixed effects. The estimated correlations range from $-.22$ to $-.28$ for universities and from $-.09$ to $-.17$ for colleges.

The correlations between prestige and individual fixed effects are smaller than we expected. This could reflect large salary differences across fields within institutions that dominate the within-field individual quality differences. We investigated this by regressing the individual effects on PhD field and calculating the residuals. The correlations of these residuals with institution prestige are virtually identical to those reported in Table 3.

large premium.

4.4 Further robustness checks

Tables B1-B4 replicate Tables 1-4 but keep observations where a faculty member had a large pay increase that was subsequently reversed. The results are virtually indistinguishable from those obtained by dropping these observations.

Our result is robust to limiting the sample to tenured faculty. For the 434 institutions remaining in the connected set, the effects of college and university rank flip sign. However, neither change appears to be statistically significant (see Appendix Table B10).

It is possible that the university fixed effects had little variation or relation to university ranks because universities are not equally elite across all departments. Therefore, we also redid this analysis for the two fields with large enough samples to allow analysis, faculty with PhDs in biological sciences and separately, engineering. In Table B11, we show estimates using the *THE* rankings (columns 1 and 3) and the field-specific *UNSWR* rankings (columns 2 and 4). In all cases, the estimated effect of rank on salary is small and insignificant. Moreover, for biological sciences, the effect of field-specific rank has the wrong sign. We also plot field-specific institutional fixed effects against field-specific rank for these fields (Appendix Figure C5). Again, these effects are small and, moreover, not monotonic with respect to rank.

4.5 Time-varying individual characteristics: It's mostly rank and experience

In Table B9, we show the effect of the time-varying characteristics using the specification in column (1) of Table 3. The coefficients are unsurprising and correspond to past studies of academic salaries. Salaries increase with post-PhD experience, although at a declining rate, while remaining positive at all experience levels in the data. Academic rank, rather than tenure status, affects salaries. The few tenure-stream lecturers and instructors earn salaries comparable to those of assistant professors. Associate professors earn a slight premium

(5%) relative to assistant professors. Full professors earn about 10% more than comparable associate professors. The small “other” group lies between associate and full professors.

Family composition has little effect on earnings, conditional on rank and experience. Prior research suggests that children make women less likely to hold tenure-stream jobs (Ginther and Kahn, 2006; Cheng, 2020; Wolfinger et al., 2008; Martinez et al., 2007). However, among women with tenure-stream STEM jobs, children and marriage are positively associated with both women’s and men’s salaries (Kahn and Ginther, 2017a). The positive association is likely due to selection, which our model captures through the individual fixed effects.

Seniority is theoretically inconsistent with AKM estimation. Therefore, we excluded seniority from the time-varying variables in our two-step estimation shown in Table B9. Using our one-step model, we find a small negative effect of seniority (−.6% per year of seniority) as shown in Table B12, columns (1)-(3). Negative coefficients on seniority are common in the literature on the academic labor market.¹³ For comparison with the prior literature, in Appendix Table B12, columns (4)-(6), we drop the individual fixed effects. This produces a somewhat more negative coefficient on seniority. The most common explanation is *ex post* monopsony (Ransom, 1993; Hallock, 1995).

4.6 Is the absence of institution-effects due to universities matching next best offers?

Most academics expect they can increase their salaries by getting outside offers. Therefore, a model in which the best outside option also influences salaries may be appropriate. Could this explain our inability to find institution effects?

To test this, in Appendix Table B13, we build on Di Addario et al. (2023) and allow

¹³Some of the studies here include Ransom (1993), Barbezat and Hughes (2001), Hallock (1995), Barbezat and Hughes (2001), Brown and Woodbury (1998), Moore et al. (2007), Hilmer et al. (2015), Blackaby et al. (2005), and Bratsberg et al. (2010).

the current and previous employer to influence salaries. We re-estimate the one-step model, including institution type and the prior and current institution's rank. This table reports the coefficients on these key parameters. The sample size is much smaller, because we need information on the prior employing institution. Therefore, we can only use movers and then only their observations from the second or later job at which we observe them.

Ideally, like [Di Addario et al. \(2023\)](#), we would limit our analysis to the initial salary in each job so that the previous job would very likely be the source of the next best offer. Unfortunately, we do not have sufficient data for this approach. Instead, we control for seniority, since those in a job longer have a higher likelihood of having ever received an outside offer.¹⁴

If faculty capture rents based on their best alternative offer, the coefficients on the rank of their previous institution should be negative. If they can also capture some of the rents from their current institution, the coefficients on the rank of their current institution should also be negative. Only one (rank of prior university) of the four coefficients (prior/current rank x college/university) is statistically significant, and then only at the .06 level. One coefficient (current college rank) has the wrong sign; a second (rank of prior college) only turns negative if we control for other institution characteristics (not shown).

Given the obvious imprecision of our estimates, a generous interpretation might find the marginally significant negative coefficient on the rank of the prior university as some evidence of a second-price effect in which faculty cannot capture current rents. Given the risk of drawing a strong conclusion from one marginally significant effect and the fact that the coefficient on seniority has the wrong sign, we see no real support for this generous interpretation.

Note that our results do not reject the [Postel-Vinay and Robin \(2002\)](#) model. We only

¹⁴This would predict a positive coefficient on seniority. Instead, it is negative here (consistent with higher prestige), but as we discussed above, we and many others have found negative coefficients on seniority, perhaps because of monopsony power.

rule out that the absence of rents is due to ignoring the effect of prior employer on current salary. If, for example, productivity has an important match-specific component, the model might hold, but the value of the second-best offer might be unrelated to the institution’s rank. Moreover, our finding of increased salaries on moving, whether prestige increases or decreases, is consistent with this model.

4.7 Are institution effects absent due to higher nonsalary compensation at elite institutions?

We observe only salaries, but working conditions and other amenities differ among institutions. Moreover, in the broader labor market, amenities and wages are positively correlated (Mas, 2025). Perhaps more elite institutions offer better working conditions, so that looking at salaries alone obscures higher faculty total compensation at elite institutions, as suggested by Claypool et al. (2017).

If jobs at higher-ranked institutions are better, conditional on salary, faculty at such institutions should be more likely to say that they are very satisfied or satisfied with their job. Do they? The SDR asks respondents whether they are very dissatisfied, somewhat dissatisfied, somewhat satisfied, or very satisfied with their job. From this, we construct two alternative job satisfaction dummy variables, one for “very satisfied” and one for very or somewhat satisfied. Following Bond and Lang (2019), we make no claims about average satisfaction.

To see if faculty at elite institutions are more satisfied with their jobs than would be predicted based on salary alone, we run a similar AKM analysis as we did for log salary, replacing log salary with satisfaction as the dependent variable. We add the log of salary as a control variable in both the one- and two-step estimates.

Table B14 shows the impacts of institution characteristics and salary on satisfaction (analogous to Table 3 showing the impacts on salary). None of the institution characteristics’ coefficients approaches statistical significance for either satisfaction variable. Moreover,

rather than the negative signs on rank consistent with positive amenities at elite universities, many of the signs on the rank variables are positive.

Moreover, the compensating differential explanation is inconsistent with the symmetric mobility pattern we observe. If overall compensation were higher at elite institutions, faculty would generally prefer to move up the prestige ladder. Yet movement up and down the ladder is equally likely, even for tenured faculty whose moves are presumably almost all voluntary.

In sum, it is implausible that our results would differ meaningfully if we could measure the value of amenities. Of course, valuing amenities is particularly problematic in settings such as ours, where workers have very heterogeneous tastes.

5 Discussion: Why is Academic STEM Different?

We find considerable evidence of matching. Better academics, as measured by their fixed effects, are more likely to work at more elite institutions. We also find that mobility is symmetric: moves up and down the prestige scale are equally likely, and, on average, all moves are associated with salary increases.

Separately, each of these facts can be accounted for by many explanations. Collectively, we believe that these findings point us to a model in which the market observes faculty quality even early in careers and where there is an optimal institution match for each level of faculty quality, with the best academics being better matched (more productive or more valued) at more elite institutions and less productive academics better matched at lower-ranked institutions. However, departments hire at irregular intervals, so that the optimal match may not be available when newly minted PhDs enter the academic labor market or faculty search for new jobs.

In contrast, in the standard AKM model, a worker's relative productivity is the same at all firms, and firms' relative pay does not depend on their workers' quality. All workers prefer

to work at more productive firms, and all firms prefer more productive workers.¹⁵ Therefore, if most moves are voluntary, moves should be more frequently up than down, which is true in the broader labor market. In AKM, downward moves are either involuntary or to firms whose low pay reflects good working conditions. In either case, the sign of the wage change depends on the direction of the move, unlike what we find for academia.

Outside the AKM model, a reassessment of worker productivity may cause moves. If new information shows a worker is less productive than expected, their job change comes with a wage decrease; if the new information shows they are more productive than expected, their new job pays them more. This, too, does not match the pattern in the academic labor market.

Instead, we suggest that information about academics is very good even when they first enter the tenure-stream market. Our data set includes both natural sciences and social sciences (STEM). It is particularly plausible that information is very good in the natural sciences, where faculty frequently enter tenure-track jobs after an average of 7+ years in graduate school and 4.5 years in post-doctoral fellowships (Kahn and Ginther, 2017b). Even in economics, where, until recently, post-docs were unusual and most new tenure-stream entrants had few if any publications, job market papers and reference letters provide hiring departments with considerable information.

Despite this good information, matches are highly imperfect because most departments have few, if any, openings each year. Moreover, openings are often restricted to narrow fields. Any given university will rarely have an opening in computational biology. At the same time, departments are often keen to fill their positions since unfilled positions at the end of the recruiting season frequently do not carry over to the following year. One study in computer science found that only 2% of computer science searches failed to result in a hire at the top 100 PhD-granting departments, although the failure rate was notably higher at

¹⁵In this setting, the matching process can generate positive assortative matching even if negative matching would maximize total output (Borovičková and Shimer, 2024).

departments not granting PhDs (Wills, 2019). Another (Fox, 2017) reported an 8% failure rate in ecology. STEM departments apparently avoid failed searches.

So, it seems plausible that STEM faculty are both over- and under-matched. When academics move, they move towards a better-matched institution if a job becomes available. In other words, we suggest that academics do not move because the market has new information about them, but because they were mismatched initially. Each move improves the match and thus increases their salary.¹⁶ Consistent with this interpretation, salary increases are smaller when faculty move to similarly ranked institutions, at least within universities.

It seems plausible that a labor market with very visible information about worker quality and limited job openings generates the special aspects we have identified in the academic job market: elite universities attracting the best faculty and paying them salaries comparable to what they would receive at proximate institutions, the predominance of movements to similarly ranked universities, and similar movements up and down the prestige ladder accompanied by positive wage changes.

We do not address how elite universities gain their status. However, departments around the world understand that the way to move up in the rankings is to attract a core of excellent – and expensive – faculty.

¹⁶If matches were idiosyncratic but still ranked on average as in Borovičková and Shimer (2024), we would still tend to see movement up the eliteness ranking since workers tend to prefer elite institutions.

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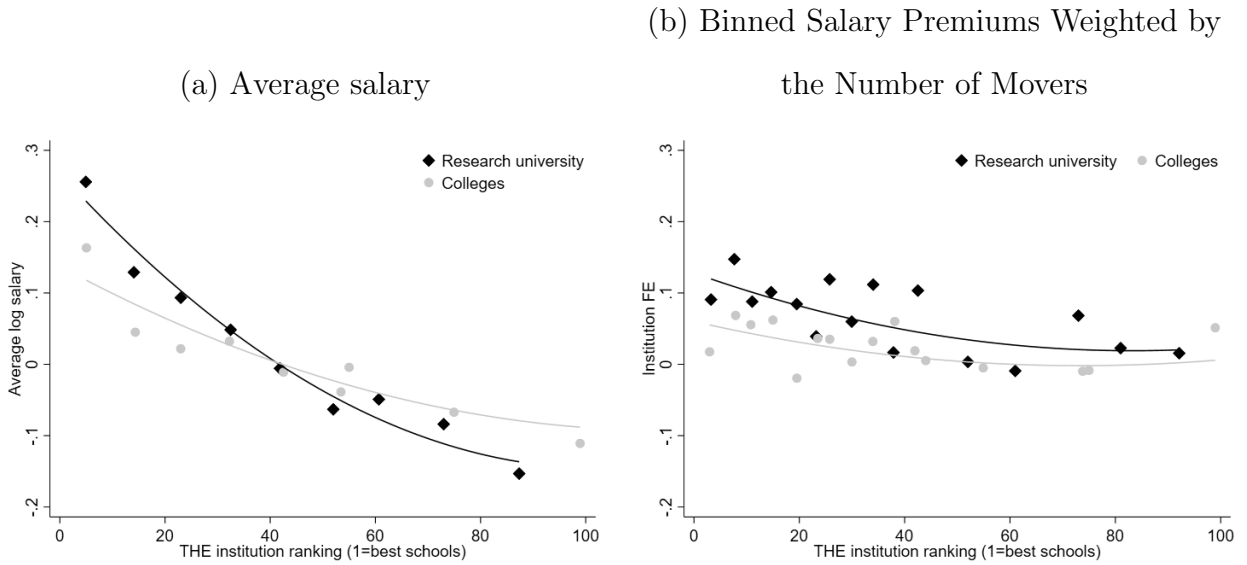
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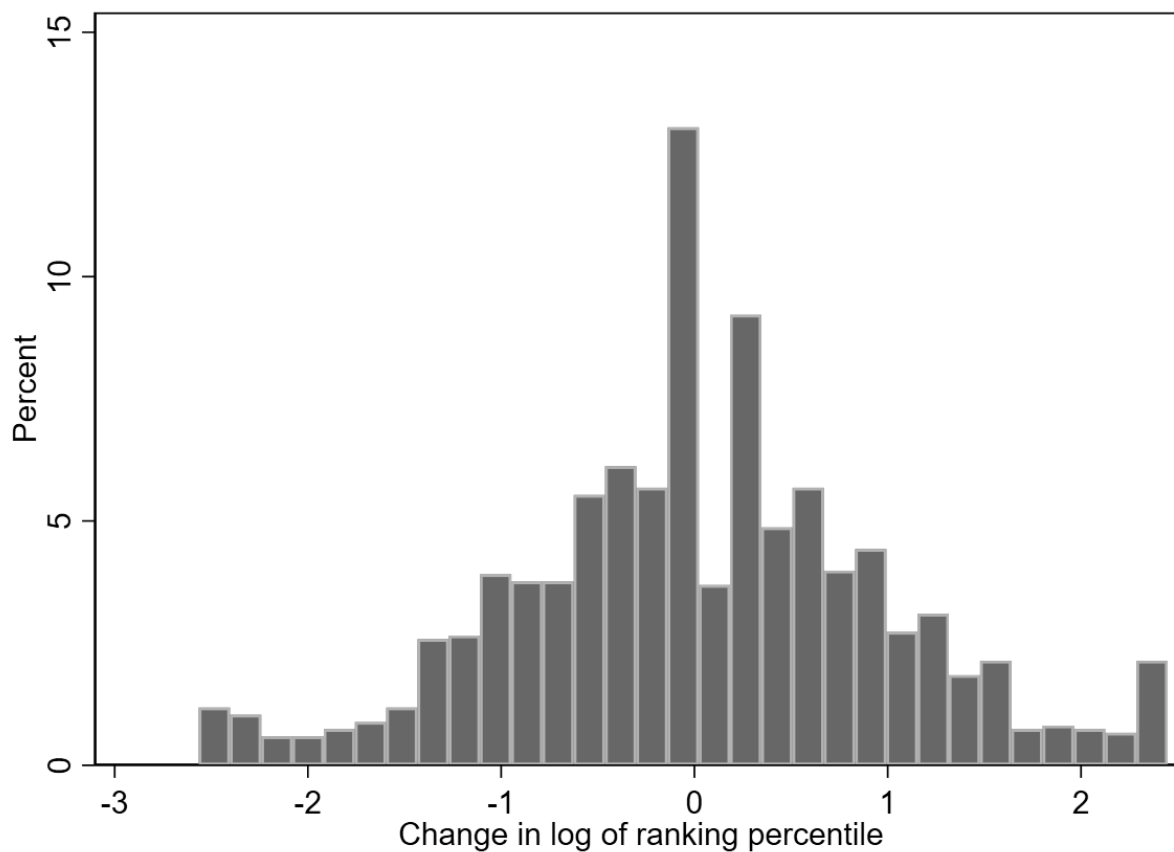
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Figure 1: Faculty Log Salary and Institution Rankings



Notes: The figure shows binned scatter plots relating measures of faculty real pay to institution rankings for research universities and colleges. Panel (a) relates average faculty log salary and institution rankings. Panel (b) relates the estimated pay premiums to rankings, weighted by the number of movers in the institution. All premiums were estimated by regressing \ln salary on year, individual and institution fixed effects, years since PhD and its square, academic rank, tenured, female, married, children dummies (<6, 6-11, 12-18, 19+), female \times married, children dummies interacted with female. All faculty salaries are expressed in 2020 USD.

Figure 2: Distribution of Changes in Institution Prestige



Notes: The histogram shows the distribution of changes in the institution's prestige (rank) when faculty change employers. The figure limits the sample to faculty moves within the same institution type (university to university or college to college). Institution ranking expressed in percentiles, with lower values indicating more prestigious institutions.

Table 1: Summary Statistics

A. NUMBER OF MOVERS IN THE SAMPLE				B. NUMBER OF TRANSITIONS IN THE SAMPLE				
	ALL	MOVERS	SHARE OF TOTAL		TOTAL	MIN	MAX	
Total observations	63,376	7,840	0.12	Transitions	2,114			
Number of people	26,135	1,805	0.07	Number of movers	1,805			
Mean obs. per person	2.42	4.34		Number of universities	654			
				Transitions per mover	1.17	1	*	
				Transitions per university	3.23	1	52	
C. INDIVIDUAL CHARACTERISTICS				D. UNIVERSITY CHARACTERISTICS				
	N	MEAN	SD		MEAN	SD	MIN	MAX
Years since Ph.D.	63,376	18.24	10.61	Research universities	48	28	1	99
Has tenure	63,376	0.73	0.44	Colleges	46	25	1	100
Time in current job	63,376	12.89	10.31	ln(total enrollment)	8.91	1.02	5.79	10.92
<i>Faculty rank</i>				ln(total endowment)	18.10	2.10	11.51	24.25
Assistant professor	63,376	0.24	0.43	ln(endowment/students)	9.20	2.09	2.55	14.67
Associate professor	63,376	0.29	0.46	ln(faculty size)	5.88	1.03	0.81	8.54
Professor	63,376	0.45	0.50	ln(faculty/students)	-3.03	0.46	-5.38	-1.42
Lecturer	63,376	0.00	0.03	Share in large city	0.23	0.42	0.00	1.00
Instructor	63,376	0.00	0.04	Share in medium city	0.34	0.47	0.00	1.00
Other	63,376	0.01	0.09	Share in small city	0.43	0.50	0.00	1.00
Female	63,376	0.32	0.47	Share private	0.40	0.49	0.00	1.00
Married	63,376	0.83	0.38	Share undergraduate	0.13	0.33	0.00	1.00

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Table 1: continues from previous page

	C. INDIVIDUAL CHARACTERISTICS			D. UNIVERSITY CHARACTERISTICS			
	N	MEAN	SD	MEAN	SD	MIN	MAX
Has child under 6	63,376	0.18	0.38				
Has child aged 6-11	63,376	0.20	0.40				
Has child aged 12-18	63,376	0.20	0.40				
Has child aged 19+	63,376	0.10	0.30				

Notes: There are 147 research universities and 481 colleges. 26 institutions are unranked and not classified as colleges or universities. * Suppressed for confidentiality. All monetary variables are expressed in 2020 USD.

Table 2: Fixed Effect Variance Estimates in AKM Model

	UNCORRECTED	CORRECTED
	(1)	ANDREWS ET AL METHOD (2)
Individual by year level		
Variance $\log(\textit{salary})$	0.141	0.141
<i>Variance of Fixed-effects</i>		
Individual	0.132	0.104
Institution	0.029	0.012
Correlation	-0.334	-0.396
Correlation net of field	-0.356	
Collapsed at the spell level		
Variance $\log(\textit{salary})$	0.140	0.140
<i>Variance of Fixed-effects</i>		
Individual	0.129	0.080
Institution	0.027	0.006
Correlation	-0.306	0.094
Correlation net of field	-0.326	

Notes: The table shows estimates of the variances of the log salary, the individual and institution fixed effects, and the correlation between institution and individual fixed effects. Column (1) displays uncorrected estimates, while column (2) corrects for limited mobility bias using the method by [Andrews et al. \(2008\)](#). Panel A uses person-year observations, while panel B collapses the dataset at the employment spell level.

Table 3: Do Rankings Increase Institution Premiums?

	TWO-STEP ESTIMATES			ONE-STEP ESTIMATES		
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Institution type × log of ranking</i>						
Research university	-0.0307 (0.0205)	-0.0273 (0.0205)	-0.0269 (0.0211)	-0.0248 (0.0095)	-0.0237 (0.0094)	-0.0186 (0.0097)
Colleges	-0.0227 (0.0119)	-0.0201 (0.0119)	-0.0213 (0.0148)	-0.0111 (0.0084)	-0.0119 (0.0084)	-0.0077 (0.0096)
<i>Institution type (omitted=unranked)</i>						
Research university	0.2161 (0.0884)	0.1841 (0.0893)	0.1726 (0.0955)	0.1467 (0.0445)	0.1326 (0.0442)	0.0972 (0.0454)
Colleges	0.1390 (0.0631)	0.1210 (0.0633)	0.1242 (0.0719)	0.0567 (0.0385)	0.0510 (0.0389)	0.0312 (0.0403)
<i>Institution characteristics</i>						
Large city		0.0666 (0.0238)	0.0638 (0.0254)		0.0432 (0.0137)	0.0379 (0.0140)
Medium city/suburb		0.0244 (0.0212)	0.0240 (0.0215)		0.0092 (0.0112)	0.0078 (0.0112)
Log of total enrollment			0.0095 (0.0136)			0.0134 (0.0085)
Undergrad only			0.0137 (0.0304)			-0.0061 (0.0190)

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Table 3: continues from previous page

	TWO-STEP ESTIMATES			ONE-STEP ESTIMATES		
	(1)	(2)	(3)	(4)	(5)	(6)
Private			0.0014 (0.0275)			0.0284 (0.0152)
Time in current job				-0.0058 (0.0003)	-0.0058 (0.0003)	-0.0058 (0.0003)
Joint significance of 2 ranking variables						
F statistic	2.930	2.271	1.649	3.827	3.680	1.915
p-value	0.054	0.104	0.193	0.022	0.026	0.148
Joint significance of institution type and ranking variables						
F statistic	3.235	2.155	1.318	4.860	4.630	1.908
p-value	0.012	0.073	0.262	0.001	0.001	0.107
Correlation between individual fixed effects and log of rankings						
Universities	-0.220	-0.220	-0.220	-0.284	-0.280	-0.280
Colleges	-0.091	-0.091	-0.091	-0.168	-0.163	-0.161
Observations	654	654	654	63,376	63,376	63,376
R^2	0.020	0.031	0.032	0.950	0.950	0.950

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Table 3: continues from previous page

	TWO-STEP ESTIMATES			ONE-STEP ESTIMATES		
	(1)	(2)	(3)	(4)	(5)	(6)
Individual-level observations	63,376	63,376	63,376	63,376	63,376	63,376
Number of people	26,135	26,135	26,135	26,135	26,135	26,135
Number of movers	1,805	1,805	1,805	1,805	1,805	1,805

Notes: The table shows estimates from regressions of the institution pay premiums or the log of faculty real salary on institution characteristics. Columns (1) to (3) show two-step estimates. The first step regresses ln salary on individual, institution, and year fixed effects, years since PhD and its square, academic rank, tenured, female, married, number of children dummies (<6, 6-11, 12-18, 19+), female \times married, and interactions between the children dummies and female; the second step (shown) regresses institution fixed effect estimates on institution characteristics using FGLS. One-step estimates –columns (4) to (6)– regress ln salary on individual and year fixed effects, the above time-varying individual characteristics, time in the current job, and the institution characteristics shown, clustering standard errors by institution. Real salaries expressed in 2020 USD. Institution ranking ranges from 1 (best) to 100. Research universities are mainly R1 but include some R2 institutions; colleges include all remaining ranked post-secondary institutions granting four-year degrees; excluded category: unranked. Large, medium, and small cities have populations above 250k, between 100k and 250k, and under 100k, respectively. Standard errors in parentheses.

Table 4: Does Endowment Increase Institution Premiums?

	TWO-STEP ESTIMATES			ONE-STEP ESTIMATES		
	(1)	(2)	(3)	(4)	(5)	(6)
ln(endowment per student)	0.0094 (0.0045)	0.0089 (0.0045)	0.0149 (0.0069)	0.0091 (0.0029)	0.0097 (0.0029)	0.0093 (0.0039)
<i>Institution type (omitted=unranked)</i>						
Research university	0.0816 (0.0506)	0.0619 (0.0509)	0.0340 (0.0566)	0.0224 (0.0291)	0.0099 (0.0291)	-0.0037 (0.0302)
Colleges	0.0446 (0.0471)	0.0360 (0.0470)	0.0305 (0.0478)	-0.0044 (0.0270)	-0.0147 (0.0270)	-0.0151 (0.0268)
<i>Institution characteristics</i>						
Large city		0.0700 (0.0237)	0.0705 (0.0256)		0.0463 (0.0135)	0.0417 (0.0139)

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Table 4: continues from previous page

	TWO-STEP ESTIMATES			ONE-STEP ESTIMATES		
	(1)	(2)	(3)	(4)	(5)	(6)
Medium city/suburb		0.0277 (0.0210)	0.0290 (0.0214)		0.0102 (0.0113)	0.0089 (0.0113)
Log of total enrollment			0.0139 (0.0134)			0.0170 (0.0085)
Undergrad only			0.0241 (0.0291)			0.0061 (0.0190)
Private			-0.0202 (0.0317)			0.0164 (0.0174)
Time in current job				-0.0058 (0.0003)	-0.0058 (0.0003)	-0.0058 (0.0003)
Observations	654	654	654	63,376	63,376	63,376
R^2	0.017	0.030	0.034	0.950	0.950	0.950

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Table 4: continues from previous page

	TWO-STEP ESTIMATES			ONE-STEP ESTIMATES		
	(1)	(2)	(3)	(4)	(5)	(6)
Individual-level observations	63,376	63,376	63,376	63,376	63,376	63,376
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Notes: The table shows estimates from regressions of the institution pay premiums or the log of faculty real salary on institution characteristics. Columns (1) to (3) show two-step estimates. The first step regresses ln salary on individual, institution, and year fixed effects, years since PhD and its square, academic rank, tenured, female, married, number of children dummies (<6, 6-11, 12-18, 19+), female \times married, and interactions between the children dummies and female; the second step (shown) regresses institution fixed effect estimates on institution characteristics using FGLS. One-step estimates –columns (4) to (6)– regress ln salary on individual and year fixed effects, the above time-varying individual characteristics, time in the current job, and the institution characteristics shown, clustering standard errors by institution. Real salaries and endowment per student are expressed in 2020 USD. Research universities are mainly R1 but include some R2 institutions; colleges include all remaining ranked post-secondary institutions granting four-year degrees; excluded category: unranked. Large, medium, and small cities have populations above 250k, between 100k and 250k, and under 100k, respectively. Standard errors in parentheses.