Do Elite Universities Overpay Their Faculty?

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Do elite universities overpay their faculty? Not if you believe the AKM model, but there may be strong reasons for not using the AKM model.

1 Introduction

This paper measures the relation between faculty salaries (net of faculty quality) and university or college prestige. We find no evidence that more prestigious institutions pay premiums above the competitive salary for the quality of the faculty they hire. Indeed, using an AKM (Abowd et al., 1999) model, we find little evidence of any institution effect on salaries, although institutions in more urban areas pay higher salaries.

The absence of institution effects in the AKM model is striking because their absence implies that, aside from a random factor, faculty would receive the same salary at any university. We authors find it implausible that Oakland University would be willing to match the salaries Stanford pays its tenure-track faculty. Readers are free to draw their own conclusions.

Our evidence is based on the Survey of Doctorate Recipients (SDR), a panel survey of individuals with U.S. doctorates in fields covered by the National Science Foundation. Thus, our results apply to STEM and the social sciences but not necessarily to the humanities or faculty with professional degrees. We merge the SDR with the *Times Higher Education* (*THE*) 2017 rankings of (research) universities and the *Wall Street Journal – THE* 2017 college rankings, supplemented with rankings from the *U.S. News and World Report* (*UNSWR*) rankings for some universities and colleges not in the other rankings and IPEDS institutional data.

We begin by applying a standard AKM model to the data. The variance of the institution fixed effects is as little as .006, depending on the correction we use. When we regress the estimated fixed effects on institution characteristics, the effect of university or college prestige is always small and generally insignificant. We find some evidence that institutions with larger endowments per student pay premiums, but the magnitude of the premium is modest.

We repeat the exercise but replace the two-step estimation with a single step in which we include

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institution characteristics rather than institution fixed effects. The results are similar, as expected, since both approaches provide consistent estimates of the same parameters.

We also examine the relation between institution prestige (as measured by rank) and faculty quality, as measured by the individual fixed effect. Consistent with our expectations (and probably the expectations of most faculty at research universities), the correlation is positive.

We briefly discuss how to reconcile the absence of a prestige premium, the positive match between prestige and faculty quality, and our sense that faculty at prestigious institutions would earn less at less prestigious institutions. We develop a toy hedonic model in which faculty transition only among similarly ranked institutions. We conclude with some thoughts about why our results differ from AKM models of the broad labor market.

2 AKM in the Academic Context

AKM uses a standard two-way fixed-effect model

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

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.

The institution fixed effect γ_j captures the tendency of the institution to pay all faculty a different salary than they would receive elsewhere. It may reflect compensating differentials for institution characteristics that the econometrician does not measure or institutional rents shared with faculty.

The individual fixed effect, α_i, captures whatever factors tend to raise a faculty member's wage relative to other faculty in the same (or similar) institutions. In the AKM model, α is typically interpreted as a measure of worker quality or productivity However, it captures any other factor that affects pay, such as discrimination or, in our case, differentials across fields. We will largely follow tradition and treat this fixed effect as capturing worker (faculty) quality. However, we note that in Eeckhout and Kircher (2011), firms pay high wages to any low-skill workers they hire, whether high or low skill. This induces these workers to apply despite being unlikely to receive an offer (see also Abowd et al. (2019)).

It is well known that problems arise if we treat the variance of estimated γ ($\widehat{\gamma}$) as the variance of γ. We correct this bias using Andrews et al. (2008).

It is evident that (1) makes strong assumptions. First, AKM assumes that mobility is random. (Formally, ε and γ are uncorrelated.) Applied to academia, AKM assumes that faculty do not change university because the profession has changed its belief about their value or because they are particularly valuable at their new university. Instead, moves reflect changes in personal preferences, etc. Second, the log-linear functional form is highly restrictive; the institution effect is 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 who earns 20 percent more than the norm at one university would also earn 20 percent more elsewhere, again, except for ε_{ijt}. These assumptions and the logarithmic form imply that better (higher α) faculty gain more in absolute terms by working at a higher-paying university (higher γ).

Under these assumptions, the AKM model allows us to answer several questions:

1. How important are firms for determining salaries? (What is the variance of *gamma* in the estimated AKM model?)

2. How important are differences between individuals (variance of α_i) for determining salaries?

3. Do the best workers go to the best (highest salary) firms? (What is the covariance of α and γ in the estimated (and corrected) AKM model?)

Unlike most applications of AKM, we can measure firm quality directly. We use published rankings and measures such as endowments, potentially correlated with a university's eliteness, to measure prestige. Thus, we address the above questions for academia and relate them to eliteness measures.

Having estimated the university fixed effects by (1), we can regress $\widehat{\gamma}$ on university characteristics. This reveals the characteristics associated with university salary premiums.

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

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 ($\widehat{\gamma}_j = \gamma_j + \nu_j$).

Alternatively, we can estimate (1) and (2) in a single step by substituting for γ_j in the AKM equation (1) to get

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

As Amemiya (1978) shows, if the variance components of (2) and (3) are estimated in the same way, generalized least squares (GLS) estimation of the two equations is numerically identical. However, we will estimate (2) by feasible GLS but only correct the standard errors in (3), thus producing somewhat different results.

3 Data

Our primary data come 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 individuals with doctorates in natural or social sciences, engineering, or health from a U.S. academic institution. Every 2-3 years, the survey collects data on their salaries, employers, and demographic characteristics. It also identifies all U.S. academic employers using the IPEDS institution codes, enabling us to identify the work histories of the academics trained and working in the United States.

We use all SDR waves of data beginning with the 1993 major SDR restructuring through the last year currently available (1993, 1995, 1997, 1999, 2001, 2003, 2006, 2008, 2010, 2013, 2015, 2017, 2019). In most years, the SDR included most survey participants from previous waves. It added participants from newly granted PhDs (identified from the NSF's Survey of Earned Doctorates) and dropped those who aged out. However, in 2015, the SDR created a new larger panel that included only a minority of the original sample. Therefore, most participants have data only before 2015 or from 2015 and later.

The SDR response rate among individuals in the U.S. is quite high. Typically, fewer than 5% of eligible respondents fail to respond. Including those who could not be found, are missing a key item, or live abroad, lowers the rate, but it remains high (75%-85%).

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 U.S. 4-year college or university, medical school attached to a university, or university research institute. We thus 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 U.S. (whether in academia or not).

Unfortunately, using the SDR to study moves requires considerable data cleaning, which we describe in detail in Appendix D. For example, there were 2,916 observations where the IPEDS university code changed, but the respondent reported not changing institutions. These frequently involved two institutions with similar names. Thus, a Boston University faculty member in multiple waves might be miscoded as Boston College faculty for one wave while not reporting changing institutions. Academics know the difference; some data coders did not.

We also drop observations with large one-time salary changes within the same institution that are subsequently reversed (see appendix for details). Appendix B (Tables B.1, and B.4-B.7) contains all tables replicated using these observations, so readers can verify that it 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 supplement the SDR data with the rankings from the *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 use the *USNWR* Best Colleges rankings (US News, 2021) to impute ranks for institutions without a *THE* rank (see Appendix D).

As is well-known, we can only include information on institutions in the connected set in AKM estimation. Institutions may be connected directly or indirectly. If one faculty member moves from university A to university B and another from B to C, A and C are connected. We limit ourselves to the largest connected set, which consists of 679 institutions. Other connected sets were very small. One-step estimation does not require a connected set, but we use the same data to maintain consistency between the two approaches.

We matched 585 (86% of the total) of the 679 institutions to a *THE* ranking. Of the remaining 94, we imputed a rank for 59 schools ranked in USNWR, using the relation between *USNWR* and *THE* ranks, leaving 35 unranked schools (5% of the total). We define *research universities* as those in the *THE* university rankings or imputed from the *USNWR* National University rankings. This group is broader than R1 institutions. We define *colleges* as those included in the *THE* college rankings or imputed from any other *USNWR* ranking. Many of the *colleges* are not liberal arts colleges but simply institutions not included among the *THE* research universities or *USNWR* National Universities. Within each type of institution, we normalize the best rank to 1 and the worst to 100.[1]

The top-ranked research universities are Stanford, Harvard, Cal Tech, and MIT. Those at the bottom 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 Rio Grande. The unranked institutions include Texas A&M at San Antonio, Brigham Young University at Idaho, and the University of Texas at Brownsville.

Our data on institution characteristics come from the Integrated Postsecondary Education Data System (IPEDS) surveys. We obtain total enrollment, number of faculty, endowment, and dummy variables for large city, urban fringe/mid-size city/suburb, private institution, and undergraduate-only institution from 1998, 2005, 2012, and 2017. We measure endowment by the average of the beginning and ending values for nonprofit institutions and the average of the beginning and ending equity for for-profit institutions.

Panel A of Table 2 shows the frequency of moves. We have 64,537 observations on 26,614 individuals, an average of roughly 2.4 observations each. 1,868, or about 7% of individuals, changed institutions at least once. Unsurprisingly, movers are disproportionately those we observe in more waves. Movers account for roughly 13% of our observations.

Panel B shows we observe only one move for most movers. We have 2,196 transitions involving 679 institutions and 1,868 movers, or 1.2 moves per mover and 3.2 moves per institution. Transitions by institutions are highly skewed, with a minimum of 2 and a maximum of 53.

When surveyed, 45% of the faculty observations were full professors and 29% associate professors (see Panel C). A few faculty (1%) report being on the tenure track but holding 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 679 institutions in the connected set, of which 152 are ranked universities and 492 ranked "*colleges*," with the remaining 35 unranked. They vary dramatically in size and endowment. 41% are private, and 22% serve only undergraduates.

[1]Due to ties, the lowest ranked college is at the 99^{th} percentile.

Table 1: Summary Statistics

A: Number of movers in the sample	**All**	**Movers**	**Share of total**
Total observations	64,537	8,091	0.13
Number of people	26,614	1,868	0.07
Average obs./person	2.42	4.33	

B: Number of transitions in the sample	**Total**		**Max**
Transitions	2,196		
Number of movers	1,868		
Number of institutions	679		
Transitions/mover	1.18	1	*
Transitions/institution	3.23	2	53

C: Summary statistics: Individuals

Characteristics	**N**	**Mean**	**Std**
Years since Ph.D.	64,537	18.12	10.65
Has tenure	64,537	0.73	0.45
Faculty rank			
Assistant Prof.	64,537	0.25	0.43
Associate Prof.	64,537	0.29	0.45
Professor	64,537	0.45	0.50
Lecturer	64,537	0.00	0.03
Instructor	64,537	0.00	0.04
Other	64,537	0.01	0.09
Female	64,537	0.32	0.47
Married	64,537	0.83	0.38
Has child under 6	64,537	0.18	0.38
Has child aged 6-11	64,537	0.20	0.40
Has child aged 12-18	64,537	0.20	0.40
Has child aged 19+	64,537	0.10	0.30

D: Summary statistics: University-level characteristics

	Mean	**Std**	**Min**	**Max**
Research university rank	48	28	1	99
College rank	46	25	1	100
Log total enrollment	8.75	1.05	5.09	10.89
Log total endowment ($2020)	18.03	2.13	10.90	24.32
Log endowment/student	9.32	1.97	2.89	14.84
Log faculty size	5.79	0.96	0.92	8.04
Log faculty/student	-3.14	0.55	-5.21	-1.69
Share in large city	0.23	0.42	0.00	1.00
Share in medium city	0.34	0.47	0.00	1.00
Share in small city	0.43	0.50	0.00	1.00
Share private	0.41	0.49	0.00	1.00
Share undergraduate	0.22	0.41	0.00	1.00

Note: There are 152 research universities and 492 colleges. 35 institutions are unranked and not classified as colleges or universities. * Suppressed for confidentiality. Exceeds 4.

4 Results

4.1 How important are the institutions for determining wages? Not much!

We first estimate the AKM model with only individual and institution fixed effects. Table 2 shows the overall variance of log salaries is 0.141; the variance of the individual fixed effects with no correction is .131 (93% of the overall variance). In contrast, the variance of the institution fixed effects is .029 (21% of the overall variance), in line with the 20% typically found in AKM models Card et al. (2018). Thus, their sum exceeds the total variance.

Table 2: Fixed-effect variance estimates in AKM model

	Uncorrected	**Corrected** Andrews et al. method
Individual by year level		
Variance log(salary)	0.141	0.141
Variance of Fixed-effects		
Individual	0.131	0.105
Institution	0.029	0.012
Correlation	-0.332	-0.397
Correlation net of field	-0.356	
Collapsed at the spell level		Bonhomme et al. method
Variance log(salary)	0.140	0.140
Variance of Fixed-effects		
Individual	0.128	0.078
Institution	0.026	0.006
Correlation	-0.310	0.081
Correlation net of field	-0.326	

However, it is well known that we over-estimate these variances, especially in situations like ours where many institutions in the sample experience little turnover (Andrews et al., 2012; Kline et al., 2020; Bonhomme et al., 2023). While $\widehat{\gamma}$ is a consistent estimate of γ, the *variance* of $\widehat{\gamma}$ is not a consistent estimate of the variance of γ. For a simple insight into the problem, consider an extreme case where all the γ are 0 (so $\sigma^2 = 0$) and the $\widehat{\gamma}$ are i.i.d. with variance ${\sigma_{\widehat{\gamma}}}^2$. Then, ${\sigma_{\widehat{\gamma}}}^2$ is completely measurement error. In addition, AKM negatively biases the covariance between the two sets of fixed effects. To see this, note that if we overestimate the institution fixed effect, we will (partially) subtract that overestimate from the individual fixed effect, leading to a negative correlation between the two sets of fixed effects.

When we use the Andrews et al. (2008) correction,[2] the variance of the individual fixed effects falls to .105 or 74.5% 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. This proportion is about half the estimate in Kline et al. (2020) for Northern Italian workers but in line with Bonhomme et al. (2023) for a Swedish sample with little turnover (similar to our sample) when using the Andrews variance correction.

When we collapse the data to the spell level to reduce measurement error, as in Bonhomme et al. (2023), the total variance of ln(salaries) by spell is .140, similar to the overall variance. As Table 2 shows, the uncorrected variance of the individual fixed effects is .128, but the corrected variance is .078 or 56% of the overall salary variance, somewhat smaller than with the uncollapsed

[2]Given our data, it is not feasible to use the approaches developed by Kline et al. (2020) and Bonhomme et al. (2023).

spells. The uncorrected variance explained by institution fixed effects (.026) is similar to what we found without collapsing spells. After correction, this variance is negligible, .006 or only 4% of the salary variation, and somewhat lower as a proportion of variance than Bonhomme et al. (2023) find for five countries, and substantially lower than Kline et al. (2020) report using their preferred correction.

We thus conclude that institution effects explain almost no variation in faculty salaries. Instead, individual faculty (worker) fixed effects explain most of the variance.

Our estimates of the correlation between faculty and institution fixed effects is sensitive to collapsing to the spell level. The uncorrected correlations are negative, as is common in AKM models due to mismeasurement bias, and equal approximately -.3 (Table 2). Since the individual fixed effects may partially be due to field, we also netted out field differences from the individual fixed effects before we calculated the correlations. As is clear, it makes little difference.

However, without collapsing, the corrected correlation is -.40; after collapsing, it is .08. We note that Andrews et al. find little effect of their variance correction unless they restrict the sample to movers and large firms. Since after collapsing, the correction shows institution fixed effects are negligible, it is difficult to interpret the small correlation even though it is positive.

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

Appendix Table B.1 shows the coefficients on the time-varying faculty characteristics in the full AKM model (1). Adding these variables decreases the unexplained variance from 8.5% to 4.8%. The coefficients in Table B.1 correspond to our expectations and/or past studies of academic salaries. Salaries increase with post-PhD experience, although at a declining rate. Nevertheless, the point estimate of the slope remains positive at all experience levels in the data. Academic rank, rather than tenure status, affects salaries. The small number of tenure-track lecturers and instructors earn salaries comparable to assistant professors. Associate professors earn a slight premium (5%) relative to assistant professors. Full professors earn about 10-11% more than comparable associate professors. The small "other" group lies between associate and full professors.

Family composition has little effect on male or female earnings, conditional on rank and experience. The sole exception is that men, but not women, earn about 1% more if they have teenage children. Prior research suggests that children make women less likely to take tenure-track jobs (Ginther and Kahn, 2006; Cheng, 2020; Wolfinger et al., 2008; Martinez et al., 2007). However, among women who take tenure-stream STEM jobs, children and marriage are positively associated with women's salary in academia (Kahn and Ginther, 2017), as are men's. Yet for both, the positive association is likely due to selection which our model captures through the individual fixed effects.

We cannot meaningfully add time-varying *institution* characteristics (such as rankings) to our model because they change very slowly. When they do change, long and uncertain lags in their impact would prevent us from associating salary and institutional changes.

4.3 Institution characteristics have little impact on salaries

Despite the near absence of firm fixed effects in the AKM model, we ask whether institution characteristics, and particularly the rank of the institution and university endowments, explain salaries. Because rank and endowment are very highly correlated, we include rank in Table 3 and Endowments in Table 4. We first regress the 679 institutional fixed effects, γ, from our AKM model (small as they are) on institution type and other characteristics as in equation (2). Columns 1-3 give the results. Then, we instead include these characteristics directly in the ln salary equation as in equation (3) (columns 4-6).

Column 1 of Table 3 shows the results of the two-stage estimates, regressing firm effects on institution type and rank. This explains only 1.6% of the variation in institutional fixed effects γ, which is only 21% of log(salary) variation (see Table 2). Table 2 also showed that 60% of the variance in γ's is measurement error (using Andrews et al.), so 1.6% of the (uncorrected) variance of the γ's corresponds to about 4% of the non-measurement error γ variance.

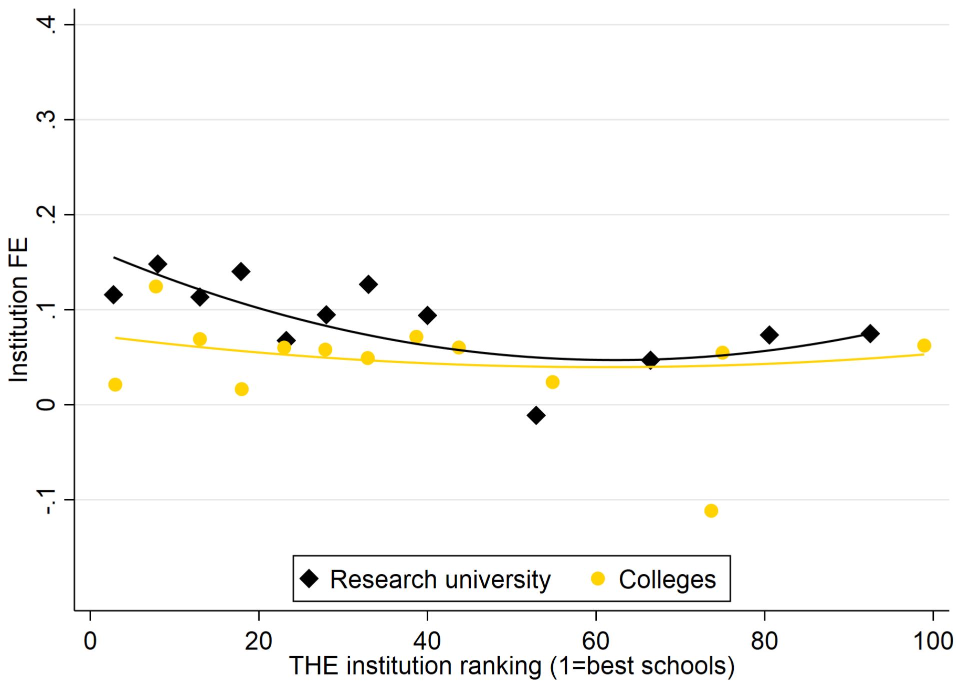
The point estimates in Table 3, column 1 imply that the most prestigious university (ln rank = 0) pays a 15% premium and the most prestigious college pays a 10% premium relative to an unranked institution, although neither is significant at standard levels. Comparing among ranked universities, the most prestigious pay premiums of about 10% relative to the least prestigious ($.0212 * \ln(100)$), and similarly for colleges. However, the coefficient on university rank is clearly insignificant, and the coefficient on college rank has a p-value of .07, while jointly, their p-value is .11 (F=2.23). Yet, for all 4 variables, the joint effect of type and rank is significant (p=.024). Column 2 includes urbanicity, which not surprisingly significantly affects salaries. However, controlling for urbanicity reduces the already small effect of the two rank variables individually and jointly (F=1.63, p=.20) and reduces the joint significance of the four institution type and rank variables (F=1.74, p=.14). Column 3 adds several additional institutional characteristics, which hardly changes the other coefficients but further decreases the significance of institution type and rank.

Columns (4)-(6) show the results using one-step estimates. The estimates are generally somewhat smaller but more precise. Consequently, we *can* reject the hypotheses that the research university dummy and university rank do not affect earnings in column (4). Again, the most prestigious university pays about a 10% premium relative to unranked institutions and the least prestigious university (which pay roughly the same salaries). However, (ranked) colleges pay only a tiny premium (.009) relative to unranked institutions, and there is no difference between colleges of best and worst ranks. Moreover, if we compare the R-squared of .946 in column (4) with the R-squared of .952 explained by the individual fixed effects and individual time-varying variables (Table B.1), it is clear that the 679 institution dummies add very little to the model's explanatory power.

In Table 4, we redo the estimation of Table 3, replacing the rank of universities and colleges with the (log of) the endowment per student of the university, which measures the resources available to the institution and the rents it can share. Endowment does have a statistically significant effect. Nevertheless, its impact remains small. 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 14% difference in the institution salary effects (γ) in column 1. We have estimated similar models with both endowment *and* rank variables; this lowers the size and significance of both, and R-squared remains less than 4% with all institutional factors included.

The insignificance of rank does not depend on our choice of functional form. Figure 1 plots binned institution fixed effects against institution rank separately for universities and colleges and fits quadratics. Both plots are somewhat U-shaped. Thus, better ranks are not monotonically better. For universities (shown as circles), the gap between the peak (at top ranks) and bottom institutions is noticeable but small (less than ten log points). For colleges (shown with yellow circles), even the difference between the peak and trough is negligible. This differs noticeably from Figure A.1 in the online appendix, which shows a definite negative relationship between binned average *salary* and institution rank.

Figure 1: Institution pay premium and rank



Appendix Figures A.2 and A.3 show that the institution fixed-effects figure is robust. In A.2, we choose bins to equalize the number of movers across bins. In A.3, we combine institutions with adjacent ranks until each institution or pseudo-institution has at least five movers. This primarily affects colleges because most universities are sufficiently large to have enough movers. The resulting patterns are largely unchanged.

We have also estimated simple correlations between the university log rank and the individual fixed effects, which range from ρ = -.22 to -.26 in the two-step and one-step estimates, respectively (bottom Table 3). Recall that more prestigious institutions have a lower rank, so this indicates a substantial positive relationship between institution and individual quality. The correlations with college institution effects are smaller and more dependent on which estimate is used, with $\rho =$ -0.10 and -.16 for colleges in the 2-step and 1-step models, respectively.

Part of the variation in individual effects may be due to the faculty being from different fields. The most prestigious universities may be willing to pay both anthropologists and economists more than they would earn at less prestigious institutions but do not pay anthropologists and economists equal salaries. However, the correlation between university rankings and the individual fixed effects net of field is the same to 2 decimal places; the correlation between college rank and net individual effects is about .01 greater than reported in Table 3.

Table 3: Do rankings increase institution fixed effects?

	Two-Step Estimates			One-Step Estimates		
	(1)	(2)	(3)	(4)	(5)	(6)
*Institution type * log of rank*						
Research university	-0.021	-0.018	-0.019	-0.0216**	-0.020*	-0.0147
	(0.021)	(0.021)	(0.023)	(0.0098)	(0.011)	(0.011)
College	-0.0218*	-0.0188	-0.0219	-0.006	-0.006	-0.0017
	(0.0119)	(0.0119)	(0.0141)	(0.009)	(0.009)	(0.011)
Institution type (omitted=unranked)						
Research university	0.148*	0.114	0.107	0.098**	0.081	0.0357
	(0.086)	(0.087)	(0.109)	(0.0446)	(0.047)	(0.048)
College	0.096	0.047	0.079	0.009**	0.000	-0.0281
	(0.059)	(0.057)	(0.066)	(0.040)	(0.040)	(0.0412)
Institution characteristics						
Large city		0.076	0.068**		0.047	0.043
		(0.023)	(0.025)		(0.015)	(0.015)
Medium city		0.025	0.022		0.016	0.012
		(0.021)	(0.021)		(0.012)	(0.013)
ln (total enrollment)			-0.008			0.010
			(0.014)			(0.009)
Undergrad only			-0.055**			-0.034
			(0.024)			(0.018)
Private institution			-0.011			0.026
			(0.030)			(0.019)
Joint significance of 2 rank variables						
F statistic	2.23	0.982	0.329	2.079	1.892	0.554
p-value	0.108	0.375	0.720	0.126	0.151	0.575
Joint significance of university type and rank variables						
F statistic	2.8	1.741	1.3	3.22	2.648	1.320
p-value	0.024	0.139	0.27	0.012	0.032	0.261
Correlation between individual fixed-effects and ln(rankings)						
Universities		-0.223		-0.264	-0.259	-0.259
Colleges		-0.095		-0.162	-0.157	-0.152
Observations	679	679	679	64,537	64,537	64,537
R squared	0.013	0.029	0.038	0.946	0.946	0.946

Notes: Standard errors in parenthesis. Institution rank ranges from 1 (best) to 100. Additional controls: individual fixed-effects, years since PhD,rank(lecturer, instructor, associate, full, other), tenured, female, married, children (<6, 6-11, 12-18, 19+), female*married, female*children. Two-step estimates are obtained by including institution fixed effects and regressing the institution coefficients on the explanatory variables. One-step estimates replace the institution fixed effects with the institution characteristics. Research universities are mainly R1 but include some R2 institutions. Colleges include all remaining post-secondary institutions granting four-year degrees. Large, medium, and small cities have populations above 250k, between 100k and 250k, and under 100k repectively. In columns (5)-(9) standard errors are clustered at the institution level.

Table 4: Does endowment increase institution fixed-effects?

	Two-Step Estimates			**One-Step Estimates**		
	(1)	(2)	(3)	(4)	(5)	(6)
ln (endowment per student)	.0102**	.0096**	0.0108**	0.0083**	.0089**	0.0059
	(0.0047)	(0.0047)	(0.0066)	(0.0033)	0.0032	(0.0041)
Institution type (omitted=unranked)						
Research university	0.0493	0.029	0.0107	-0.0080	-0.0223	0.0392
	(0.0451)	(0.0454)	(0.0509)	(0.0257)	(0.0254)	(0.0276)
College	0.0057	0.0026	-0.0127	-0.0288	-0.0406*	-0.0472*
	(0.0413)	(0.0412)	(0.0420)	(0.0240)	(0.236)	(0.0243)
Institution characteristics						
Large city		0.0713***	0.0679***		0.0504	0.0439***
		(0.0235)	(0.0253)		(0.0147)	(0.0150)
Medium city		0.0317	0.0286		0.0165	0.0133
		(0.0207)	(0.0212)		(0.0125)	(0.0126)
ln (total enrollment)			-0.0053			0.0120
			(0.0128)			(0.0090)
Undergrad only			-0.0552**			-0.0323*
			(0.0237)			(0.0173)
Private institution			-0.0099			0.0266
			(0.0296)			(0.0193)
Observations	679	679	679	64,537	64,537	64,537
R squared	0.017	0.030	0.038	0.946	0.946	0.946

Notes: Standard errors in parenthesis. Institution rank ranges from 1 (best) to 100. Additional controls: individual fixed-effects, years since PhD, rank (lecturer, instructor, associate, full, other), tenured, female, married, children (<6, 6-11, 12-18, 19+), female×married, female×children. Two-step estimates are obtained by including institution fixed effects and regressing the institution coefficients on the explanatory variables. One-step estimates replace the institution fixed effects with the institution characteristics. Research universities are mainly R1 but include some R2 institutions. Colleges include all remaining post-secondary institutions granting four-year degrees. Large, medium, and small cities have populations above 250k, between 100k and 250k, and under 100k respectively. In columns (5)-(9) standard errors are clustered at the institution level.

4.4 Why does institutional affiliation matter so little?

We find the absence of institution effects counterintuitive. Consider the University of Wisconsin, Madison, and the University of Wisconsin, Oshkosh. Both schools are in our data set and have publicly available salaries. In academic year 2021, the median full professor of economics at UWM earned $370,954 – almost three times as much as the median economics full professor at UWO, who earned $126,193. Now, imagine the UWM professor with the median earnings ($370,954) moving exogenously to UWO and vice versa. What salary do you think they would receive? This exchange is hard to imagine, but our results suggest that it would not change their salaries since there are no meaningful university effects. We find it unlikely that UWM would hire anyone as a tenured Professor of Economics to whom it was only willing to pay $126,193. It is equally unlikely that UWO would be willing to hire an economics professor with tenure for almost $100,000 more than it pays its Chancellor. Readers are, of course, free to disagree with our intuition.

4.4.1 There are no clear patterns in salary changes upon moving

In the online Appendix Table B.2, we show salary changes as people move from and to institutions, by quintile rank of universities and colleges. On average, all transitions raise salaries, which is unsurprising since we expect most people to move to better-paid jobs. However, there are few, if any, other clear patterns.

In particular, faculty do not receive larger raises when moving to a better institution (as they would if elite institutions paid more) or when moving to a worse institution (as they would if they received a compensating salary differential). If we focus on research universities, those exiting jobs in the top or second quintile see the largest gains if they end up in the second quintile. However, those exiting the third quintile institution do best if they end up in the fourth quintile and worst in the third. Those starting in the fourth do slightly better ending up in the second than the fourth but noticeably better than ending in the first or third. If the AKM model is correct, the effects of moving from A to B and B to A should be equal and of opposite sign, net of any mobility premium. Instead, in our data, salary changes are independent of the direction of movement, consistent with more prestigious institutions not paying rent.

4.4.2 Movement among institutions is not random

The thought experiment at the beginning of the subsection is challenging because we rarely observe movements across institutions differing so wildly in prestige. To be consistent, the AKM model requires that mobility be random; the error term must be uncorrelated with the explanatory variables, most notably the individual and faculty fixed effects. We will see that movement is not random, although not necessarily in a manner that challenges the AKM assumptions.

The tendency of faculty to move to institutions of relatively similar eliteness is clear from the transition matrix, Table B.3 in the online appendix. We suspect that the table probably overstates mobility across prestige levels since the prestige of individual departments is not always similar to overall institution prestige. Nevertheless, when tenure-stream faculty leave a university in the top quintile, almost half (45%) remain in the top university quintile, 66% within the top two quintiles, and 76% within the top two quintiles of universities *or* colleges (not shown). There is only a 0.5% chance of them moving to the lowest-quintile university and almost no chance of moving to a lowest-quintile college.

Similarly, roughly 70% of moves from a university that end in a top-tier university come from first or second-tier universities, and another 6% from top colleges. The likelihood of moving to the best university from either the lowest quintile universities, the bottom 2 quintiles *colleges*, or unranked institutions is tiny. However, movements involving the most-elite university quintile are somewhat atypical in their degree of insularity. For other quintiles and for colleges, movement to proximate quintiles is more common. Movements originating in the highest quintile universities are also more common than those originating in other quintiles or in colleges. Still, regardless of an academic institution's type and rank, there is limited movement to very different institutions. 72.6% of those starting in universities move within +/- 1 university quintile or to a more highly ranked college.

Moreover, there is relatively little movement from universities to colleges (31% of university movers, even though 44% of destination jobs are in colleges) and particularly little movement from colleges to universities (35% of college movers, even though 54% of destination jobs are in universities). Finally, of those who start and end in universities, the same percentage (21%) go to worse-ranked jobs as go to better-ranked jobs. However, of those who start and end in colleges, far more (26%) go to worse-ranked jobs than better-ranked jobs (13%).

4.4.3 Hedonics may explain wages and mobility

We found a substantial positive correlation between faculty fixed effects and university and, to a lesser extent, college prestige (shown at the bottom of Table 3). Simultaneously, we find no evidence that more prestigious institutions pay salary premiums. Consistent with this, there is considerable mobility between institutions. However, salary changes do *not* show a pattern where moving to higher-prestige institutions increases salaries. We suggest that a hedonic model augmented with idiosyncratic tastes fits our results well.

There is a continuum of institutions with prestige, p. The salary an institution is willing to pay for a particular match, w_m, depends on the potential faculty member's quality, $q \in Q$ and p:

$$w_m = w_m(q, p),\ \frac{\partial w_m}{\partial p} > 0 \tag{4}$$

We assume that w_m is continuous in p. We further assume that for any $p' > p''$, there is a q^* such that

$$w_m(q^*, p') = w_m(q^*, p'') \tag{5}$$

and

$$w_m(q, p') > w_m(q, p'') \iff q > q^* \tag{6}$$

This ensures that institutions' willingness-to-pay curves cross exactly once. Under these assumptions, there will be a unique p that maximizes an individual's compensation.

To take a simple example, let

$$w_m = -p^2 + pq. \tag{7}$$

Then salary is maximized at $p = 0.5q$, and salary is $w_m = 0.25q^2$ at the maximum.

With perfect matching, the observed salary is the upper envelope of the individual institution willingness-to-pay curves. While, in the example, each institution's willingness-to-pay is linear, the equilibrium salary is convex in worker quality as in Roy (1951).

With perfect matching, we cannot distinguish between individual and worker effects. Earnings can be fully explained by either p or q. For instance, in the above example, the maximizing salary, w_m can also be expressed as $w_m = p^2$.

Moreover, suppose individuals deviate slightly from their optimal institutions. Then, the effect on their earnings is only second-order since the derivative of earnings with respect to prestige is 0 at the optimum. On the other hand, the difference between the imperfectly-matched faculty's q relative to other faculty at that institution is first order. Therefore, individual fixed effects and not institution fixed effects explain wages.

Consider an individual with $p = p^*$ and, therefore, $q = 2p^*$ at their highest-pay institution. Consider a second institution $p' = p^* + \varepsilon$. The individual earns $-(p' - \varepsilon)^2 + 2(p' - \varepsilon)(p' - \varepsilon)$ when matched to p^*, but only $-p'^2 + 2p'(p' - \varepsilon)$ when imperfectly matched to p'. Taking the difference gives the tiny difference, ε^2. However, comparing the well-matched individual at p^* with a well-matched individual at p' who earns $-p'^2 + 2p'(p')$, the difference is a larger $2p'\varepsilon$.

Therefore, we do not observe our University of Wisconsin economists exchanging campuses because both would take significant salary cuts since they are poor matches at the other institution.

Intuitively, in the example, the mismatch between faculty and institution is not very different among proximate universities. Neither earns rents because there are similar institutions that would offer them essentially the same salary.

Online Appendix C develops this example. In the appendix, the variance of log salaries is .14, as in our data. If the highest and lowest quality faculty were both matched with the most prestigious institution, a highly improbable event for the latter, the ratio of their earnings would be 11. The example allows for a significant degree of mismatch. For example, the median quality faculty has a 6-7% chance of ending up in each of the top and bottom quintiles. Nevertheless, the variance of the institution effects is trivial.

5 Discussion and conclusion: is academia different?

Applying standard AKM techniques to tenure-track academic jobs, we find no evidence that prestigious institutions pay rents to their STEM faculty. Individual faculty members do differ considerably in their salaries, even when netting out field effects. Moreover, the individual effects are quite correlated with institution rank. However, when we use AKM methods to separate the firm and person effects based on movements of individuals between institutions, we find that practically all of the variation is in the person effects. We present a simple model suggesting that if faculty and institution are matched optimally, AKM estimation can lead to seemingly small institution effects.

Whether our results differ from findings for broader labor markets depends somewhat on which study we compare our results with. Nevertheless, the finding that establishment effects are small to nonexistent puts our results at the low end of the range of estimated effects. We can only speculate as to why our findings for faculty differ from the broader labor market. One explanation is that the labor markets are simply different.

For instance, many dimensions along which we measure faculty success – publications in prestigious journals, appointments to prestigious societies, editorships, etc. – are the same dimensions that feed into the success or prestige of the universities. Moreover, these dimensions are visible both inside and outside the institutions. This alone is likely to make rents unlikely in academia. In contrast, in other labor markets, individuals' contributions to productivity are often difficult for both the firm and the worker to measure and may not be visible outside the firm, making rents feasible.

It is also possible that the technologies are different. Faculty positions are quintessential star jobs (Baron and Dreps, 1999); successes are rare and valuable; failures are common and not very costly. In contrast, Bose and Lang (2017) argue that most nonacademic jobs are guardian jobs in which the gains from an especially good performance are small, but the costs of a bad performance are very high. In such settings, firms with high costs of failures would only hire workers who had demonstrated their competence and would pay those workers a premium. Mobility would primarily be upward; workers moving to high-wage firms would earn a premium. However, the premium would not be rent but a payment for their revealed high quality.

In some labor markets, higher firm salaries may be due to compensating wage differentials (Sorkin, 2018). However, in academia, non-wage aspects of the job (e.g., light workloads, more research support, better students) are highly positively correlated with prestige, so we would not see high salaries compensating for low levels of other job characteristics.

Nothing in our results allows us to distinguish among these explanations and perhaps others that may occur to readers. However, we believe that our results, while perhaps interesting in their own right, should encourage us to reflect more on the interpretation of the AKM model.

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Appendix

A Figures

Figure A.1: Average faculty log salary and institution rankings

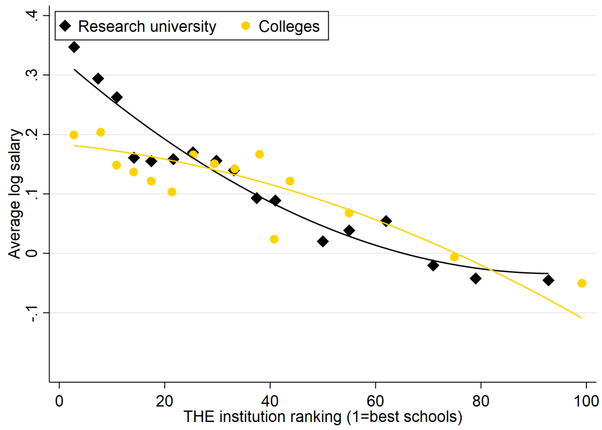
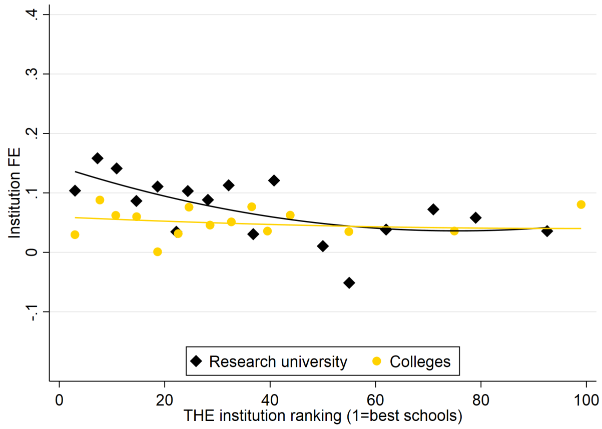
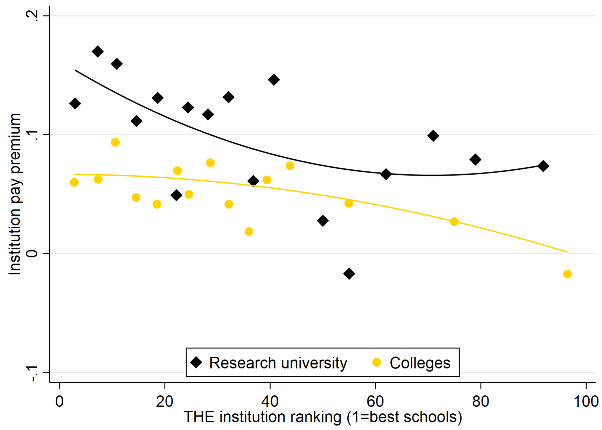


Figure A.2: Institution premium and institution rankings (weighted by number of movers)



Note: the figure weights observations so that each cell has the same number of movers.

Figure A.3: institution premiums and rankings weighted for grouped institutions



Note: the figure shows institution premium estimates for grouped institutions. We group institutions with similar rankings so that each institution "pseudo-institution" has at least five movers.

B Tables

Table B.1: Effect of time-varying characteristics

	(1) Excluding outliers	(2) Including sample
Years since PhD	0.0356	0.0374
	(0.0068)	(0.0071)
Years since PhD squared	-0.0002	-0.0003
	(0.0000)	(0.0000)
Is tenured	0.0067	0.0060
	(0.0069)	(0.0087)
Faculty rank (omitted=assistant professor)		
Lecturer	0.0142	-0.0279
	(0.0407)	(0.0775)
Instructor	-0.0069	-0.0038
	(0.0368)	(0.0376)
Associate professor	0.0456	0.0496
	(0.0079)	(0.0100)
Professor	0.1459	0.1587
	(0.0098)	(0.0124)
Other	0.0818	0.0882
	(0.0187)	(0.0211)
Married	0.0052	0.0081
	(0.0057)	(0.0076)
Married × female	0.0022	0.0032
	(0.0087)	(0.0115)
Children below 6	0.0018	-0.0010
	(0.0041)	(0.0055)
Children below 6 × female	-0.0063	0.0012
	(0.0075)	(0.0089)
Children between 6 and 11	0.0039	0.0021
	(0.0038)	(0.0047)
Children between 6 and 11× female	-0.0096	-0.0090
	(0.0061)	(0.0073)
Children between 12 and 18	0.0102	0.0118
	(0.0035)	(0.0041)
Children between 12 and 18× female	-0.0181	-0.0159
	(0.0065)	(0.0078)
Children between 19+	0.0030	0.0034
	(0.0036)	(0.0044)
Children between 19+× female	-0.0078	-0.0086
	(0.0081)	(0.0097)
Individual FE	✓	✓
Year FE	✓	✓
Observations	64537	65893
Number of movers	1868	1868
R^2	0.9516	0.9123

Notes: Standard errors in parenthesis. Column (1) uses the full sample. Column (2) excludes extreme within-institution wage changes.

Table B.2: Salary wage changes by transition type

	Universities					**Colleges**					
	Best	**2**	**3**	**4**	**Worst**	**Best**	**2**	**3**	**4**	**Worst**	**Unranked**
Origin	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Universities											
Best	0.330	0.393	0.296	0.177	N.D.	0.190	0.123	0.037	0.094	N.D.	N.D.
2	0.356	0.408	0.282	0.226	0.229	0.245	0.103	0.165	0.072	N.D.	0.253
3	0.275	0.256	0.089	0.320	N.D.	0.340	0.289	0.241	0.093	N.D.	N.D.
4	0.210	0.337	0.219	0.305	0.265	0.295	0.265	0.229	0.163	N.D.	0.183
Worst	N.D.	0.272	0.269	0.063	N.D.	N.D.	0.382	0.275	0.287	N.D.	N.D.
Colleges											
Best	0.356	0.218	0.297	0.347	N.D.	0.311	0.290	0.196	0.173	N.D.	N.D.
2	0.331	0.236	0.400	0.233	N.D.	0.193	0.178	0.250	0.164	N.D.	-0.015
3	0.278	0.245	0.201	0.208	0.101	0.188	0.181	0.209	0.162	N.D.	0.218
4	N.D.	0.135	0.251	0.166	N.D.	0.476	0.123	0.182	0.190	N.D.	N.D.
Worst	N.D.	N.D.	N.D.	N.D.	N.D.	N.D.	N.D.	N.D.	N.D.	N.D.	N.D.
Unranked	0.331	N.D.	N.D.	0.233	N.D.	N.D.	0.178	0.250	0.164	N.D.	N.D.

Notes: Data from cells with less than 5 individuals were suppressed to preserve confidentiality. We denote these cells with N.D.

Table B.3: Transition probability by ranking quintile and institution type

	Universities					Colleges					Unranked
	Best	**2**	**3**	**4**	**Worst**	**Best**	**2**	**3**	**4**	**Worst**	
Origin	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Universities											
Best	0.446	0.219	0.084	0.062	N.D.	0.044	0.047	0.062	0.03	N.D.	N.D.
2	0.238	0.181	0.151	0.090	0.019	0.058	0.099	0.079	0.055	N.D.	0.025
3	0.146	0.190	0.241	0.091	N.D.	0.062	0.077	0.117	0.051	N.D.	N.D.
4	0.067	0.172	0.124	0.129	0.033	0.053	0.086	0.153	0.115	N.D.	0.057
Worst	N.D.	0.113	0.097	0.097	0.081	N.D.	0.113	0.210	0.145	N.D.	N.D.
Colleges											
Best	0.151	0.086	0.086	0.059	N.D.	0.092	0.217	0.184	0.079	N.D.	N.D.
2	0.084	0.154	0.044	0.062	0.022	0.141	0.163	0.167	0.132	N.D.	0.026
3	0.049	0.070	0.115	0.101	0.049	0.070	0.098	0.259	0.15	N.D.	0.035
4	N.D.	0.050	0.078	0.177	0.035	0.071	0.17	0.199	0.163	N.D.	N.D.
Worst	N.D.	N.D.	N.D.	N.D.	N.D.	N.D.	N.D.	0.333	N.D.	N.D.	N.D.
Unranked	N.D.	N.D.	N.D.	0.271	N.D.	N.D.	0.305	0.102	0.102	N.D.	N.D.

Notes: Data from cells with less than 5 individuals were suppresed to preserve confidentiality. We denote these cells with N.D.

Table B.4: Summary statistics including wage outliers

A: Number of movers in the sample

	All	**Movers**	**Share of total**
Total observations	65,893	8,192	0.12
Number of people	26,873	1,868	0.07
Average obs./person	2.45	4.39	

B: Number of transitions in the sample

	Total	**Min**	**Max**
Transitions	65,893	8192	0.12
Number of movers	26,873	1868	0.07
Number of institutions	2.45	4.39	
Transitions/mover	1.18	1	*
Transitions/institution	3.23	2	53

*Suppressed, exceeds 4

C: Summary statistics: Individuals

Characteristics	**N**	**Mean**	**Std**
Years since Ph.D.	65,893	18.18	10.66
Has tenure	65,893	0.73	0.45
Faculty rank			
Assistant Prof.	65,893	0.25	0.43
Associate Prof.	65,893	0.29	0.45
Professor	65,893	0.45	0.5
Lecturer	65,893	0	0.03
Instructor	65,893	0	0.04
Other	65,893	0.01	0.09
Female	65,893	0.32	0.47
Married	65,893	0.83	0.38
Has child under 6	65,893	0.18	0.38
Has child aged 6-11	65,893	0.2	0.4
Has child aged 12-18	65,893	0.2	0.4
Has child aged 19+	65,893	0.1	0.3

D: Summary statistics: University-level characteristics

	Mean	**Std**	**Min**	**Max**
Research university rank	48	28	1	99
College rank	46	25	1	100
Log total enrollment	8.75	1.05	5.09	10.89
Log total endowment ($2020)	18.03	2.13	10.90	24.32
Log endowment/student	9.32	1.97	2.89	14.84
Log faculty size	5.79	0.96	0.92	8.04
Log faculty/student	-3.14	0.55	-5.21	-1.69
Share in large city	0.23	0.42	0	1
Share in medium city	0.34	0.47	0	1
Share in small city	0.43	0.5	0	1
Share private	0.41	0.49	0	1
Share undergraduate	0.22	0.41	0	1

Note: There are 152 research universities and 492 colleges. 35 institutions are unranked and not classified as colleges or universities.

Table B.5: Fixed-effect variance estimates in AKM model including wage outliers

	Uncorrected	**Corrected** Andrews et al. method
Individual by year level		
Variance log(salary)	0.148	0.148
Variance of Fixed-effects		
Individual	0.140	0.110
Institution	0.029	0.012
Correlation	-0.325	-0.398
Collapsed at the spell level		
Variance log(salary)	0.140	0.140
Variance of Fixed-effects		
Individual	0.129	0.077
Institution	0.027	0.006
Correlation	-0.318	0.058

Table B.6: Do ranks increase institution fixed effects (including wage outliers)

	Two-Step Estimates			**One-Step Estimates**		
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
*Institution type * log of rank (low ranks best)*						
Research university * ln(rank)	-0.0203	-0.0174	-0.0192	-0.0179	-0.0167	-0.0104
	(0.0208)	(0.0208)	(0.0213)	(0.0100)	(0.0108)	(0.0111)
College * ln(rank)	-0.0197	-0.0169	-0.0220	-0.0056	-0.0062	-0.0006
	(0.0122)	(0.0122)	(0.0145)	(0.0095)	(0.0095)	(0.0108)
Institution type (omitted=unranked)						
Research university	0.1502*	0.1190	0.1113	0.0873	0.0724	0.0233
	(0.0871)	(0.0878)	(0.0937)	(0.0455)	(0.0479)	(0.0491)
College	0.0902	0.0716	0.0814	0.0115	0.0043	-0.0281
	(0.0605)	(0.0607)	(0.0676)	(0.0413)	(0.0401)	(0.0420)
Institution characteristics						
Large city		0.0703***	0.0661**		0.0472***	0.0403***
		(0.0242)	(0.0258)		(0.0149)	(0.0150)
Medium city		0.0262	0.0220		0.0118	0.0086
		(0.0215)	(0.0218)		(0.0124)	(0.0124)
ln (total enrollment)			-0.0069			0.0113
			(0.0134)			(0.0089)
Undergrad only			-0.0581**			-0.0330*
			(0.0248)			(0.0181)
Private institution			-0.0089			0.0378**
			(0.0278)			(0.0170)
Observations	679	679	679	65,893	65,893	65,893
R squared	0.016	0.028	0.036	0.906	0.906	0.907
Joint significance of 2 rank variables						
F statistic	1.781	1.294	1.426	1.665	1.312	0.444
p-value	0.169	0.275	0.241	0.190	0.270	0.641
Joint significance of university type and rank variables						
F statistic	2.726	1.684	1.285	2.541	2.106	0.888
p-value	0.028	0.152	0.274	0.039	0.079	0.471
Correlation between individual fixed-effects and ln(rankings)						
Universities		-0.22		-0.261	-0.256	-0.255
Colleges		-0.093		-0.156	-0.152	-0.146

Note: See footnotes Table 3

Table B.7: Does endowment increase institution fixed effects? (including wage outliers)

	Two-Step Estimates			One-Step Estimates		
	(1)	(2)	(3)	(4)	(5)	(6)
ln (endowment per student)	0.0094*	0.0089*	0.0118*	0.0076**	0.0084**	0.0046
	(0.0049)	(0.0049)	(0.0068)	(0.0033)	(0.0033)	(0.0042)
Institution type (omitted=unranked)						
Research university	0.0560	0.0357	0.0120	-0.0028	-0.0163	-0.0310
	(0.0463)	(0.0466)	(0.0523)	(0.0258)	(0.0257)	(0.0280)
College	0.0082	-0.0002	-0.0118	-0.0247	-0.0356	-0.0405*
	(0.0424)	(0.0423)	(0.0431)	(0.0240)	(0.0237)	(0.0245)
Institution characteristics						
Large city		0.0725***	0.0709***		0.0499***	0.0426***
		(0.0241)	(0.0260)		(0.0148)	(0.0151)
Medium city		0.0286	0.0266		0.0124	0.0094
		(0.0213)	(0.0218)		(0.0125)	(0.0126)
ln (total enrollment)			-0.0038			0.0130
			(0.0132)			(0.0092)
Undergrad only			-0.0519**			-0.0298*
			(0.0244)			(0.0174)
Private institution			-0.0179			0.0321
			(0.0304)			(0.0196)
Observations	679	679	679	65,893	65,893	65,893
R squared	0.016	0.029	0.036	0.906	0.906	0.907

Note: See footnotes Table 3.

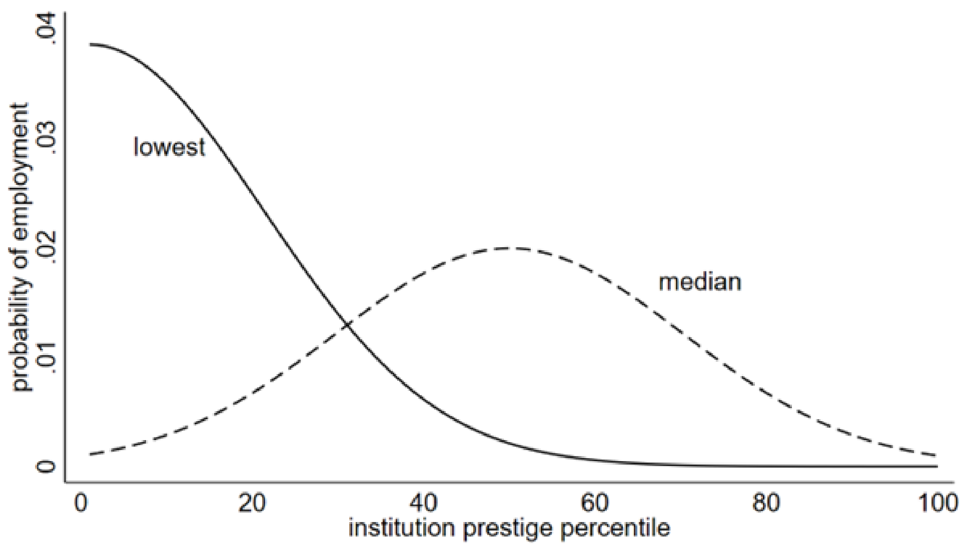
C A simple example

We choose functional forms to generate a realistic example but do not attempt to calibrate the example fully. We have 100 universities with prestige, p, given by $\{.211, .222, .233, \ldots, 1.30\}$. Similarly, we 100 faculty-quality types with quality, q, given by $\{.422, .444, .466, \ldots, 2.60\}$. Universities pay a faculty member $\ln w(p, q) = -p2 + pq$. These assumptions ensure that each faculty member maximizes their salary by choosing the university with the prestige rank equal to their quality rank. We choose these numbers so that if both are perfectly matched, the highest type earns about five times as much as the lowest type but the highest type would earn about 17 times as much as the lowest type if they were both at the most prestigious university but would only earn about two-thirds more if they were both at the least prestigious. The utility the faculty receives from an appointment at a given university is u=ln w + Î· where Î· is type 1 extreme value with scale parameter .1. Then the probability that a worker of quality, q, is in the job with prestige $p\hat{a}$ is given by

$$P(p', q) = \exp(10 * \ln w(p', q)/(\sum p(10 * \ln w(p, q)))$$

The AKM model fits the data well in the sense that it explains 99% of the variance. Of course, the example has no idiosyncratic errors, but the ability of the AKM model to fit the data is still striking. Although the university fixed effects are jointly significant, they are relatively unimportant with an uncorrected standard deviation of less than .01. Faculty fixed effects alone explain 83% of the variance. Appendix Figure C.1 shows the distribution of the lowest and median quality faculty. Although the lowest quality faculty is most likely matched with the lowest prestige university, they still have a nontrivial chance of ending up in the third quintile. Similarly, the median quality faculty is mostly likely to be matched with the median prestige university but has a nontrivial chance of being in either the top or bottom quintiles. The 10th percentile faculty (not shown) has a 55% chance of being in a bottom quintile university, 35% in the fourth quintile, and 9% in the fifth quintile.

Figure C.1: Probability of prestige level: lowest and median quality faculty



D Data

In this paper, we combine data from three sources: individual-level data from the restricted-use version of the Survey of Doctorate Recipients (SDR) from the National Center for Science and Engineering Statistics (NCSES); university and college rankings data from the *Times Higher Education* 2017 World University Rankings, the *Wall Street Journal – Times Higher Education* 2017 College Rankings, and the 2021 *US News and World Report* University and college rankings; and university characteristics from Integrated Postsecondary Education Data System (IPEDS) surveys.

Our analysis required three main steps: build a work history panel for tenure-track faculty, construct a dataset with institution characteristics, and associate each school to a unique ranking. Below we detail the main steps we used to build our final dataset.

D.1 Building the work history panel

We first combine the information from all the SDR waves available between 1993 and 2017 (inclusive). We restrict the sample to people 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, medical school attached to a university, or university research institute. We also 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 (whether in academia or not). We identify employers using the IPEDS institution code reported by the SDR. We transform all salary figures into 2020 dollars using the yearly CPI for all urban consumers (**?**). This leaves us with an unbalanced panel tracking the work history of tenure-track faculty across US academic institutions.

D.1.1 Determining faculty moves in the SDR

We pay special attention to ensuring that we track the moves of faculty across academic institutions correctly. The AKM model identifies the pay-premiums out of variation coming from people moving across institutions. Thus, it is crucial that we record moves correctly.

We say an academic changed employer whenever we observe a change in the IPEDS code of the current employer, except when these changes result from a leave of absence or a likely coding error. We identify leaves of absence as *temporary moves* out of a primary or home institution. These are moves satisfying three conditions:

(i) we observe the academic in three *consecutive* SDR waves;

(ii) the academic starts in an institution (home) and moves to a *host* institution for one period;

(iii) to then return to their home institution.

We identify 59 leaves-of-absence in our data. We exclude the host school observation for them, keeping the observations in their home school only.

We also identified and manually corrected moves that were likely the result of a coding error. There were 2,916 observations where the IPEDS university code changed, but the respondent reported not changing institutions. These frequently involved two institutions with similar names. Thus, a Boston University faculty member in multiple waves might be miscoded as Boston College faculty for one wave, while not reporting changing institutions. We manually checked these moves and corrected those we deemed likely mistakes.

Because we are interested in institution-level premiums, we merged IPEDS codes that identify units of the same university. IPEDS divides some large universities across different codes. For example, ASU-Tempe and ASU-Phoenix have different codes even though they belong to the same institution. We did not count these as moves in our dataset, since all are within ASU. Therefore, we assigned all university units to a single code in such cases. (It is possible that we missed some moves in this process but wanted to be conservative in what we considered to be moves.) Whenever we determined university campuses were independent of each other, we kept them as separate IPEDS codes. For example, we keep University of Wisconsin-Madison and University of Wisconsin Oshkosh as separate institutions.

We tried to be as conservative as possible in this process, only combining 40 institution codes into 24 codes. We can provide the list of merged codes upon request.

D.2 Salaries

In addition to excluding observations that we determined to be leaves of absence, we excluded salary observations with very large one-time salary changes that were subsequently reversed *within the same institution.* We identify these outliers as follows:

1. First, we computed the growth in the log of salary adjusted for job experience ($\Delta\widetilde{w}_t$):

$$\Delta\widetilde{w}_t = \Delta w_t - \Delta\widehat{w}_t \tag{D.1}$$

   where Δw_t is the log change in the individual salary, and $\Delta\widehat{w}_t$ is the expected change in the log salary due to experience. This expected change comes from a regression of log salaries on years of experience, and years of experience squared:

$$w_t = \alpha_o + \alpha_1 y_t + \alpha_2 y_t^2 + \nu_t$$

   where y_t denote years since Ph.D. Then we define the expected change as:

$$\Delta\widehat{w}_t = \widehat{\alpha}_1 \Delta y_t + \widehat{\alpha}_2 \Delta y_t^2$$

   The expression in D.1 measures how much actual salary growth deviates from what we should expect based on the experience profile alone.

2. We flag a *within-institution* log salary change as a *potential outlier* if, after adjusting for experience, it is larger than 0.4 in absolute value:

$$|\Delta\widetilde{w}_t| = |\Delta w_t - \Delta\widehat{w}_t| > 0.4$$

   We note that 0.4 is a conservative threshold, in the 97^{th} percentile of adjusted salary growth.

3. We then focus on the *potential outliers* and exclude observations as follows. We drop all observations from people with only two observations in the dataset and who worked for only one institution. For people having at least three observations and who worked for several institutions, we apply the following procedure:

4. If $|\Delta\widetilde{w}_t| > 0.4$, then either w_t or w_{t-1} may be the outlier. We exclude w_t if its distance from

any other salary observation for that person is greater than 0.2[3]. That is,

$$\text{Drop } w_t \text{ if } \min_j \{d_j | d_j = |w_j - w_t|\,,\; j \neq t\} > 0.2$$

5. If $|\Delta\tilde{w}_t| > 0.4$ but its minimum distance is less than 0.2, we apply additional sequential filters (i.e., if an observation survives filter (i) below, then we applied (ii)):

   i. We excluded all observations where the individual's primary work activities were not teaching or research. These people are likely to be in administrative positions[4].

   ii. We excluded all salaries that were out of line with the individual's salary trend. This judgment was made on a case-by-case basis. All these modifications were codified into the do file *"code/build_database/outlier_exclusion_list.do"*

D.3 Building the institution characteristics dataset

All university characteristics other than the rankings are extracted from IPEDS. We use the modules of institution characteristics, fall enrollment, finance, and salaries for the years 1998, 2005, 2012, and 2017. All nominal figures are converted into 2020 dollars using the CPI for all urban consumers. As we say in the paper, we cannot meaningfully add time-varying institution characteristics to our model because they change very slowly, and when they do change, long and uncertain lags in their impact would prevent us from associating salary and institutional changes to salary shifts. Thus, we average all continuous variables across the four survey waves. For all dummy variables, we assign the maximum value across the four years. For example, we classify a university as granting a Ph.D. Degree if it ever granted a Ph.D. Degree during any of the four survey waves.

We extract the following variables from IPEDS:

- **University location:** we classify the university's location into small, medium, and large city. This variable is a recoding of IPEDS' locale variable. Table D.1 details the mapping between both variables.

- **Private university:** dummy equal to one if the university is private.

- **Undergrad-only:** dummy variable equal to one if the institution only offers undergraduate degrees.

- **Total enrollment:** sum of undergraduate and graduate enrollment, averaged over the four survey years.

- **Total faculty:** total faculty size, average of the four survey years.

- **Value of endowment:** IPEDS reports finance information separately for public institutions, private not-for-profit, and private for profit. Our endowment variable corresponds to:

  – **Public universities and private non-profits:** we average the value of endowment assets at the beginning and the end of the fiscal year.

[3] 0.2 is the 90th percentile of the adjusted wage growth.

[4] In later waves, the SDR asked if the person working in an academic institution was (a) a president, provost or chancellor or (b) a dean, department head or department chair. However, this question was not asked in most SDR waves in our study so we do not use it.

- **Private for-profits:** we average the value of equity at the beginning and the end of the year.

We use the average of the endowment across the four survey waves.

D.4 University rankings

Our primary sources for the institution rankings are the *Times Higher Education* 2017 World University Rankings, the *Wall Street Journal – Times Higher Education* 2017 College Rankings. The *THE* rankings consist of a list of institution names along with their position in the ranking and the state in which they are located. We linked these rankings to a unique IPEDS code using the institution name and location. In most cases, the names in *THE* and IPEDS were similar, and the linkage was straightforward. For the few cases where the linkage was not obvious, we followed the following rules:

1. Whenever names only differed in the word "college" or "university," we use a Google search and the location information to determine if they were the same institution. For example, if the IPEDS label was "Concordia College" and the *THE* ranking name was "Concordia University". We linked both names if and only if:

    a. The institution state is the same in both datasets.

    b. A search for the term "[. . .] college" gives "[. . .] university" as the first search result (or vice versa).

1. Different campuses in a university system have different IPEDS codes. Sometimes *THE* provides only one rank for a university system without reference to the campus. In this case, we associated the rank to the flagship campus. For example, the *THE* rank for "Penn State University" was associated to the IPEDS code for "Penn State University, University-Park."

The procedure above was applied to both the *THE* World University and the *WSJ/THE* College rankings. Based on the result of the matching, we classify institutions into three mutually exclusive categories. These categories determine the value of the *institution ranking* variable we use in our regressions.

1. **Research universities:** these are institutions we matched to the *THE* World University Rankings. For these institutions, the value of *institution rank* is their position in the World University Ranking.

1. **Colleges:** there are institutions (i) not matched to the World University Ranking but (ii) matched to the College Ranking. Their *institution rank* is their position in the College Rankings. Note that many institutions in this category are not solely undergraduate institutions.

2. **Unranked universities:** these are institutions we could not match to any of the rankings. We assign a value of zero to their *institution rank*.

We matched 585 (86% of the total) of the 679 institutions to a *THE* rank. Of the remaining 94, we imputed a rank for 59 schools ranked in *USNWR*, using the relation between *USNWR* and *THE* ranks (see below), leaving 35 unranked schools (5% of the total).

D.5 Imputing of the THE ranks

The THE rankings are our primary source of university performance information. However, we were unable to match 94 institutions to a THE rank. For 59 of these insitutions, we were able to impute a *THE* rank using U.S. News and World Report (*USNWR*) rankings as follows:

1. First, we merge the *THE* rankings with each of the ten available *US News* ranking lists (national, liberal arts colleges, and regionals). Merging was done by institution (university or college) name. Names were manually checked to ensure consistency.

2. For universities ranked by both *THE* and *US News* (in any of the six lists), we run an OLS regression of their position in the *THE* list on their position in the *US News* list:

$$THE\_ranking_i = \alpha + \beta US\_news\_ranking_i + \varepsilon_i$$

We run a separate regression for each of the *US News* lists (national, liberal arts colleges, and regionals). Table D.3 shows the results of each of these auxiliary regressions.

3. We infer the position in the *THE* rankings for universities unranked by *THE* but ranked by *US News* using the predicted values of the regression in 2. That is:

$$\widehat{THE}_{\ ranking_i} = \alpha + \widehat{\beta} US\_news\_ranking_i$$

Note that all ten *US News* rankings are mutually exclusive. Therefore, the imputed *THE* position is unique. We treat institutions in the *national US News* ranking as *research universities,* and institutions in all other *US News* rankings (liberal arts colleges, regional universities, and regional colleges) as *colleges.* Table D.4 provides a breakdown of the imputed ranks according to the *US News* ranking list we used for the imputation.

Table D.1: University location classification

1998 IPEDS locale classification		Recoding used	
Codes	**Labels**	**Codes**	**Labels**
1	Large city	1	Large city
2 3, 4	Mid-size city Urban fringe of large / mid-size city	2	Mid size city / suburb
5, 6, 7 9	Large town, small town, rural Not assigned	3	Small city / rural town

2005-2017 IPEDS locale classification		Recoding used	
Codes	**Labels**	**Codes**	**Labels**
11	Large city	1	Large city
12 21, 22, 23	Mid-size city Suburbs	2	Mid-size city / suburbs
13 31 - 43	Small city Towns, rural	3	Small city / rural town

Table D.2: Description of location codes

Location	Description
Large city	Urban area, population above 250k
Mid-size city / suburbs	Urban area, population between 100k and 250k, or suburbs
Small city / rural town	Urban areas with population below 100k, rural areas

Table D.3: Ranking imputation regressions

	National rankings		Regional universities				Regional colleges			
	(1) National	**(2) Liberal**	**(3) North**	**(4) South**	**(5) Midwest**	**(6) West**	**(7) North**	**(8) South**	**(9) Midwest**	**(10) West**
US News ranking	1.762	3.115	3.101	2.671	2.883	3.872	3.681	1.715	7.927	7.938
	(0.132)	(0.139)	(0.237)	(0.361)	(0.293)	(0.395)	(1.901)	(0.623)	(1.130)	(5.318)
Constant	82.21	-20.90	326.0	550.3	456.5	439.7	624.2	694.0	382.2	585.5
	(17.665)	(15.120)	(21.710)	(24.234)	(23.468)	(25.014)	(50.416)	(23.008)	(37.954)	(68.844)
r2	0.582	0.771	0.554	0.386	0.477	0.530	0.211	0.296	0.629	0.182
F	179.3	502.0	171.4	54.61	96.79	96.04	3.748	7.571	49.24	2.228
N	131	151	140	89	108	87	16	20	31	12

Notes: The dependent variable in column (1) is the THE research university ranking. The dependent variables for all the columns is the THE college university ranking.

Table D.4: Number of schools imputed by ranking type

Ranking type	**Number of schools**
National rankings	
Universities	10
Liberal arts colleges	13
Regional Universities	
North	6
South	4
West	2
Midwest	8
Regional colleges	
North	2
South	5
West	2
Midwest	2
Total	54