Do Elite Universities Overpay Their Faculty?*

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Do elite universities overpay their faculty? Not if you believe the AKM model. However, although the AKM model fits well, it is unlikely to be the right interpretation in this case.

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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 salaries are higher in urban areas.

The absence of institution effects in the AKM model is striking. It implies that, aside from random factors, 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.

We draw 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 IPEDS institutional data and rankings of colleges and universities.

We begin by applying a standard AKM model. The variance of the institution fixed effects is as little as .006, depending on the correction, and even lower if we apply the Kline et al. (2020) leave-one-out correction to the narrower connected set their approach requires. 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 a modest premium.

We repeat the exercise, replacing two-step estimation with a single step including 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 correlation between institution prestige (measured by rank) and faculty quality, as measured by the individual fixed effect. The correlation is positive, consistent with our expectations (and probably those of most faculty at research universities).

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 or institutional rents shared with faculty.

The individual fixed effect, α_i, captures whatever factors raise a faculty member's wage relative to other faculty in the same (or similar) institutions. It is typically interpreted as reflecting worker quality or productivity. However, it captures any factor that affects pay, including 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 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)).

Well-known 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) and, on a reduced sample, Kline et al. (2020).

It is evident that (1) makes strong assumptions. First, AKM assumes that mobility is random. (Formally, ε and γ are uncorrelated.) Applied to academia, faculty do not change university because the profession has changed its belief about them or because they are particularly valuable at their new university. Instead, moves reflect changes in personal preferences. Second, the semi-log 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 earning 20% more than the norm at one university would earn 20% more elsewhere, again, except for ε_{ijt}.

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 γ?)
2. How important are individual differences (variance of α) 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 AKM applications, we can measure firm quality directly. Having estimated the university fixed effects by (1), we can regress $\widehat{\gamma}$ on published university rankings and measures such as endowments, potentially correlated with university eliteness.

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

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 (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 doctoral recipients in natural or social sciences, engineering, or health from a U.S. academic institution. Every 2-3 years, the survey collects salary, employer, and demographic information. It also provides the IPEDS code for all U.S. academic employers. Therefore, we can identify the work histories of academics trained and working in the United States.

We use all SDR waves beginning with the 1993 major SDR restructuring (1993, 1995, 1997, 1999, 2001, 2003, 2006, 2008, 2010, 2013, 2015, 2017, 2019). The SDR includes most survey participants from previous waves, adds newly granted PhDs (from the NSF's Survey of Earned Doctorates), and drops those who age 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 on.

The SDR response rate among U.S. residents is high, typically more than 95% of eligible respondents. Subtracting those who could not be found, are missing a key item, or live abroad lowers the rate to 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, 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 U.S. (in academia or elsewhere).

Unfortunately, studying moves using the SDR requires considerable data cleaning, described in detail in Appendix A. There were 2,916 observations where the IPEDS university code changed, without the respondent reporting 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 *if they are subsequently reversed* (see appendix for details). As shown in Appendix B (Tables B1 and B8-B11) 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 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 A).

As is well-known, AKM requires that included institutions be connected directly or indirectly. A and C are connected if a faculty member moves from university A to university B and another from B to C. We limit ourselves to the largest connected set, 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.

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 in the *THE* college rankings or imputed from any other *USNWR* ranking. Many *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. 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 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 beginning and ending equity for for-profit institutions.

Panel A of Table 1 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, we generally observe movers 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 faculty observations were full professors and 29% associate professors

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

(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 679 institutions in the connected set, of which 152 are *universities*— and 492 *colleges*, with the remaining 35 unranked. They vary dramatically in size and endowment. 41% are private, and 22% serve only undergraduates.

4 Results

4.1 Are institutions important for determining wages? Not really!

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., 2018a). Thus, their sum exceeds the total variance.

However, it is well known that we overestimate these variances, especially in situations like ours where many institutions 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 an inconsistent estimate of the variance of γ. For a simple insight, 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: if we overestimate the institution fixed effect, we will (partially) subtract that overestimate from the individual fixed effect, creating a negative correlation between the two sets of fixed effects.[2]

When we use the Andrews et al. (2008) correction,[3] 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.

Collapsing the data to the spell level to reduce measurement error, as in Bonhomme et al. (2023), gives a total variance of ln(salaries) by spell of .140, similar to the overall variance. As Table 2 shows, the uncorrected variance of the individual effects is .128, but the corrected variance

[2] We experimented with letting starting salary depend on the prior and current institutions as in Di Addario et al. (2023), but the resulting sample is small, with few transitions per institution.

[3] Following the notation from Bonhomme et al. (2023), the variance of the estimated fixed effects is:

$$\mathbf{E}(\hat{V}_Q^{FE}) = \gamma' Q \gamma + \text{Trace}[A(A'A)^{-1}Q(A'A)^{-1}A'\Omega(A)] \tag{4}$$

where Q is some matrix that typically depends on A, and $\Omega(A) = Var(\varepsilon|A)$ is the conditional variance of the error term. Using Andrews et al. (2008) simplifies computation by assuming the errors are homoskedastic: $\Omega(A) = \sigma^2 I$. Kline et al. (2020) allows for heteroskedasticity but uses a jackknife, thus restricting estimation to the connected set that remains when any observation is removed.

is .078 or 56% of the overall salary variance, somewhat smaller than with the uncollapsed 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, 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 own preferred correction. Finally, we use the Kline et al. (2020) approach; the sample falls to 441 institutions (143 universities, 284 colleges, 5 unranked), and the variance of the institution effects becomes .003.

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

Our uncorrected estimates of the correlation between faculty and institution fixed effects are negative, as is common in AKM models due to mismeasurement bias, and equal approximately -.3 (Table 2). Since the individual effects may partially reflect field differentials, we also recalculated correlations, netting out field differences. Clearly, this makes little difference.

However, without collapsing, the corrected correlation is -.40; after collapsing, it is .08. Andrews et al. similarly 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 positive correlation.

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

Appendix Table B1 shows the coefficients on the time-varying faculty characteristics in (1). Adding these variables decreases the unexplained variance from 8.5% to 4.8%. The coefficients in Table B1 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 few 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 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 prevent us from associating salary and institutional changes.

4.3 Institution characteristics have little impact on salaries

Although our estimates show institution effects are nearly absent, we ask whether institution characteristics, particularly institution rank and 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, controlling for individual effects as in (2). Columns 1-3 give the results. Then, we include these characteristics directly in the ln salary equation as in (3) (columns 4-6).

Column 1 of Table 3 shows the results of regressing firm effects on institution type and rank. This explains only 1.6% of the variation in institutional 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 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, the joint effect of the four type and rank variables is significant (p=.024).

Column 2 includes urbanicity, which not surprisingly significantly affects salaries. Notably, 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, further decreasing the significance of institution type and rank with little effect on the other coefficients.

Columns (4)-(6) show the results using one-step estimates of the determinants of ln salary. The estimate coefficients are generally 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 similar salaries). However, (ranked) colleges pay only a tiny premium (.009) relative to unranked institutions, and there is no difference between colleges with the best and worst ranks. Moreover, comparing 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 B1), demonstrates 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 the R-squared remains less than 4% with all institutional factors included.

Our functional form choice does not explain the unimportance of institution. 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 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 noticeably from Figure D1 in the online appendix, which shows a definite negative relationship between binned average *salary* and institution rank.

Our result is also robust to limiting the sample to tenured faculty. For the 450 institutions in the resulting connected set, the effect of rank on tenured university faculty is even smaller than on all tenure-stream faculty, while the effect on tenured college faculty flips sign. However, neither of the changes is statistically significant (see B5).

As an additional robustness check, we limited the sample to faculty with PhDs in biological sciences. The connected set falls to 232, making our estimates imprecise. However, the results provide no evidence that salaries increase with prestige (see Figure D4 and Tables B6 and B7).

Appendix Figures D2 and D3 show that the institution effects figure is robust. In D2, we choose bins to equalize the number of movers across bins. In D3, 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 effects, which range from ρ = -.22 to -.26 in the two-step and one-step estimates, respectively (bottom Table 3). Since more prestigious institutions have a lower rank, this indicates a substantial positive relation between institution and individual quality. The correlations with college institution effects are smaller and more dependent on which estimate is used, with ρ = -0.10 and -.16 for colleges in the 2-step and 1-step models.

Part of the variation in individual effects may reflect salary differentials across 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 ranks and the individual 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.

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 have publicly available salaries. In academic year 2021, the median economics full professor at UWM earned \$370,954 compared with the median economics full professor at UWO, who earned \$126,193. Imagine the UWM professor with median earnings 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 their salaries would not change since there are no meaningful university effects. We find it unlikely that UWM would hire anyone it was only willing to pay \$126,193 as a tenured Professor of Economics. 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.

4.4.1 There are no clear patterns in salary changes upon moving

Online Appendix Table B2 shows 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 receive similar increases 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). At 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. Here, salary changes are independent of the direction of movement, consistent with more prestigious institutions not paying rents.

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 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 B3 in the online appendix. We probably overstate mobility across prestige levels since the prestige of individual departments can differ from 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 quintile *colleges*, or unranked institutions is tiny. Admittedly, movements involving the most elite university quintile are somewhat atypical in their degree of insularity. For other quintiles and 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, in B3, there is relatively little movement between universities and colleges. 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%).

These results are robust to ranking institutions by coworker salary rather than prestige (see Table B4),

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 (see the bottom of Table 3). Simultaneously, we find no evidence that prestigious institutions pay salary premiums. Consistent with this, there is considerable mobility between institutions, but moving to a higher-prestige institution does not increase one's salaries.

A simple hedonic model augmented with idiosyncratic tastes fits these 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:

$$w_m = w_m(q, p), \ \frac{\partial w_m}{\partial q} > 0. \tag{5}$$

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

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

and

$$w_m(q, p') > w_m(q, p'') \Longleftrightarrow q > q^*. \tag{7}$$

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

To take a simple example, let

$$w_m = -p^2 + pq. \tag{8}$$

Then salary is maximized at $p = 0.5q$, and $w_m = 0.25q^2$ 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, equilibrium salary is convex in worker quality as in Roy (1951).

With perfect matching, we cannot distinguish between individual and worker effects. Either p or q fully explaines earnings. Thus, in the above example, the maximizing salary, w_m can also be expressed as $w_m = p^2$.

Now, suppose individuals deviate slightly from their optimal institutions. 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 and not institution effects explain wages.

To see this, 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, the mismatch between faculty and institution differs little among proximate universities. Neither earns rents because there are similar institutions that would offer faculty essentially the same salary.

Online Appendix C develops this example, setting the variance of log salaries at .14, as in our data. If the highest and lowest quality faculty 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.

Figures D5 and D6 are consistent with our interpretation. Faculty who moved into the top quintile tend to have had a higher salary before the move if their destination institution is better ranked. However, neither the wage change at the time of the move nor one period after shows a clear relation to initial salary.

5 Discussion and conclusion: is academia different?

Applying standard AKM techniques to tenure-stream academic jobs, we find no evidence that prestigious institutions pay their STEM faculty rents. Individual faculty members 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 individual effects based on the movement of individuals between institutions, we find the variation is almost entirely in the individual effects. We present a simple model suggesting that if faculty and institutions match optimally, AKM estimation can generate seemingly small institution effects.

How much our results differ from findings for broader labor markets depends somewhat on which study we compare our results with. Finding small to nonexistent establishment effects puts our results at the bottom of the range of estimated effects. We can only speculate as to why our findings for faculty differ from those for the broader labor market. Perhaps the labor markets are simply different.

One major difference is that the measures of faculty success – publications in prestigious journals, citations, appointments to prestigious societies, editorships – are observable to all both inside and outside their institution, and there is general agreement on these as measures of success. This alone might make rents unlikely in academia. In contrast, in the broader labor market, contributions to productivity may be difficult to observe outside the firm, making matching more likely to be imperfect. So, for instance, a worker who performs well in a firm where skill is only weakly rewarded may appear to receive rents when moving to a firm that rewards skill more highly. Or a journeyman plumber at a firm primarily employing journeymen may appear to earn rents if they qualify as a master plumber and move to a firm primarily employing master plumbers. Similarly, Bose and Lang (2017) argue that most nonacademic jobs are âguardianâ jobs. Consequently, firms with high costs of failures would only hire workers who had demonstrated their competence (although not to researchers) and would pay those workers a premium. Of course, it is also possible that the firm effects in the broader labor market are real; some firms may pay efficiency wages, share rents, or offer compensating wage differentials (Sorkin, 2018).

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

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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.

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	

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

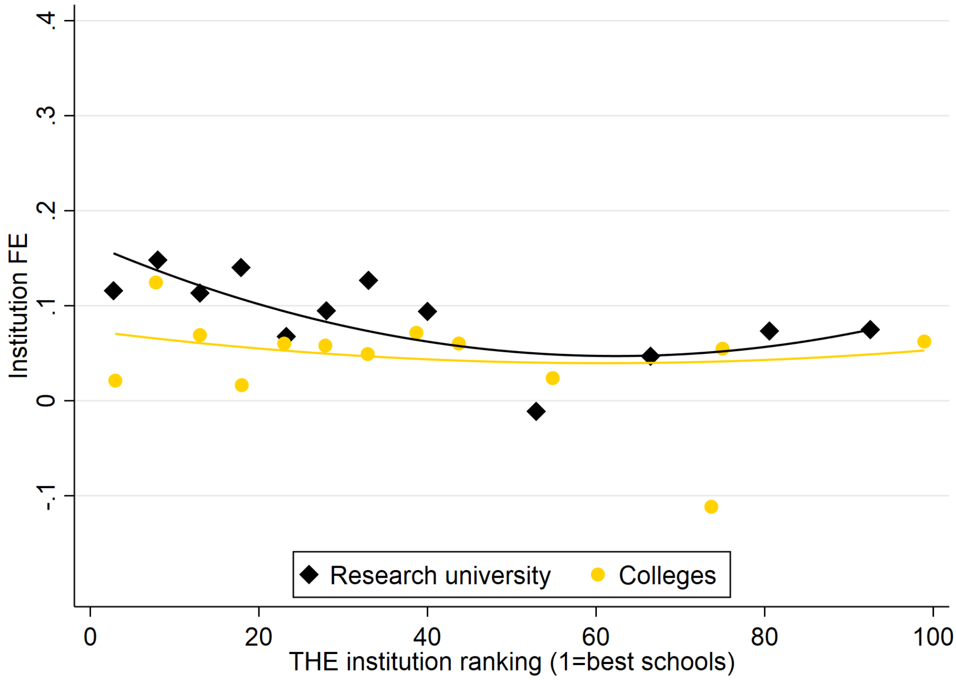
Notes: Standard errors in parenthesis. Institution rank ranges from 1 (best) to 100. Additional controls: individual fixed-effects, years since PhD,rank(lecturer, instructor, associate, full, other), tenured, female, married, children (<6, 6-11, 12-18, 19+), femalemarried, femalechildren. Two-step estimates are obtained by including institution fixed effects and regressing the institution coefficients on the explanatory variables. One-step estimates regress ln salary directly on the institution characteristics and clusters standard errors by institution. 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.

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 regress ln salary directly on the institution characteristics and clusters standard errors by institution. 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.

Figure 1: Institution pay premium and rank



Appendix

A 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.

A.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 (U.S Bureau of Labor Statistics, 2023). This leaves us with an unbalanced panel tracking the work history of tenure-track faculty across US academic institutions.

A.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.

A.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 ($\Delta\widetilde{w}_t$):

   $$\Delta\widetilde{w}_t = \Delta w_t - \Delta\widehat{w}_t \tag{A9}$$

   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 A9 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$^{\text{th}}$ 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[4]. 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[5].

   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"*

A.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 cities. This variable is a recoding of IPEDS' locale variable. Table A5 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.

[4] 0.2 is the 90th percentile of the adjusted wage growth.

[5] 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.

A.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:
   - The institution state is the same in both datasets.
   - A search for the term "[...] college" gives "[...] university" as the first search result (or vice versa).
2. 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. We categorized the institutions matched to the THE World University Rankings as **Research universities**. For these institutions, the value of institution rank is their position in the World University Ranking. The institutions (i) not matched to the World University Ranking but (ii) matched to the College Ranking were categorized as **Colleges**. Their *institution rank* is their position in the College Rankings. Note that many institutions in this category are not solely undergraduate institutions.

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). Of the 59 institutions matched by this process, we categorized those institutions in the National US News ranking as research universities. All the rest of the institutions with imputed rankings based on USNWR were categorized as colleges.

This left 35 unranked schools (5% of the total). These institutions were categorized as **Unranked universities**.

A.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 A7 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. As we said in the previous section, 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 A8 provides a breakdown of the imputed ranks according to the *US News* ranking list we used for the imputation.

Table A5: 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 A6: 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 A7: 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 A8: 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

B Tables

Table B1: 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 B2: 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 B3: 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 B4: Salary wage changes by quintile of coworker salary rank

	Destination				
	Best	**2**	**3**	**4**	**Worst**
Origin	(1)	(2)	(3)	(4)	(5)
Best	0.22	0.25	0.20	0.24	0.27
2	0.37	0.17	0.30	0.33	0.39
3	0.18	0.33	0.25	0.32	0.37
4	N.D.	0.20	0.32	0.18	0.36
Worst	N.D.	N.D.	0.40	0.25	0.29

Notes: We follow Card et al. (2018b) and classify transitions based on coworkers' salary rank. For each worker in each year, we compute the rank of the avarage coworker salary for that year. We then classified transitions using as origin the rank in the year right before the move, and as destination the rank of the first year in the new insitution. Data from cells with less than 5 individuals were suppresed to preserve confidentiality. We denote these cells with N.D.

Table B5: Pay premiums and rankings for tenured faculty

	All faculty (1)	**Only tenured faculty (2)**
*Institution type * log of rank (low ranks best)*	-0.0238	-0.0025
Research university * ln(rank)	(0.0179)	(0.0249)
College * ln(rank)	-0.0063	0.0240
	(0.0172)	(0.0242)
Institution type (omitted=unranked)		
Research university	0.1311	0.0455
	(0.0875)	(0.1221)
College	0.0570	-0.0524
	(0.0761)	(0.1071)
Institution characteristics		
Large city	0.0569	0.0434
	(0.0229)	(0.0322)
Medium city	0.0328	0.0455
	(0.0203)	(0.0285)
ln (total enrollment)	0.0018	-0.0029
	(0.0129)	(0.0182)
Undergrad only	-0.0517	-0.0521
	(0.0255)	(0.0360)
Private institution	0.0216	0.0261
	(0.0285)	(0.0401)
Observations	449	449
R squared	0.055	0.023
Joint significance of 2 rank variables		
F statistic	0.889	0.601
p-value	0.412	0.549
Joint significance of university type and rank variables		
F statistic	0.588	0.416
p-value	0.672	0.797

Notes: Column (1) shows results for the whole sample, while column (2) restricts the sample to tenured faculty only. Standard errors in parenthesis. Institution rank ranges from 1 (best) to 100. 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.

Table B6: Institution pay premiums and rankings for faculty in biological sciences

	(1)	(2)	(3)
*Institution type * log of rank (low ranks best)*			
Research university * ln(rank)	0.0959	0.0862	-0.0620
	(0.1483)	(0.1525)	(0.1790)
College * ln(rank)	0.2354	0.2212	0.0174
	(0.1974)	(0.2020)	(0.2296)
Institution type (omitted=unranked)			
Research university	0.1502*	0.1190	0.1113
	(0.0871)	(0.0878)	(0.0937)
College	0.0902	0.0716	0.0814
	(0.0605)	(0.0607)	(0.0676)
Institution characteristics			
Large city		0.0229	0.0056
		(0.0502)	(0.0516)
Medium city		0.0015	-0.0038
		(0.0499)	(0.0500)
ln (total enrollment)			0.0141
			(0.0357)
Undergrad only			-0.0930
			(0.0797)
Private institution			0.0920
			(0.0663)
Observations	232	232	232
R squared	0.037	0.037	0.056
Joint significance of 2 rank variables			
F statistic	2.858	2.549	0.480
p-value	0.059	0.080	0.620
Joint significance of university type and rank variables			
F statistic	2.153	1.928	0.762
p-value	0.075	0.107	0.551

Notes: The regression limits the sample to people with a Ph.D. in Biological Sciences. Standard errors in parenthesis. Institution rank ranges from 1 (best) to 100. In one-step estimation, standard errors are clustered by institution. Additional controls include 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. Estimates are obtained by including institution fixed effects and regressing the institution coefficients on the explanatory variables. 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.

Table B7: Summary statistics for faculty in the biological sciences

Observations	13,000
Number of people	4,920
Number of movers	1,450
Number of institutions	232
Universities	114
Colleges	111
Unranked	7

Notes: The table shows summary statistics for the connected set that remains invariant after consecutively dropping one observation from the full sample.

Table B8: 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 B9: 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 B10: 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 B11: 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.

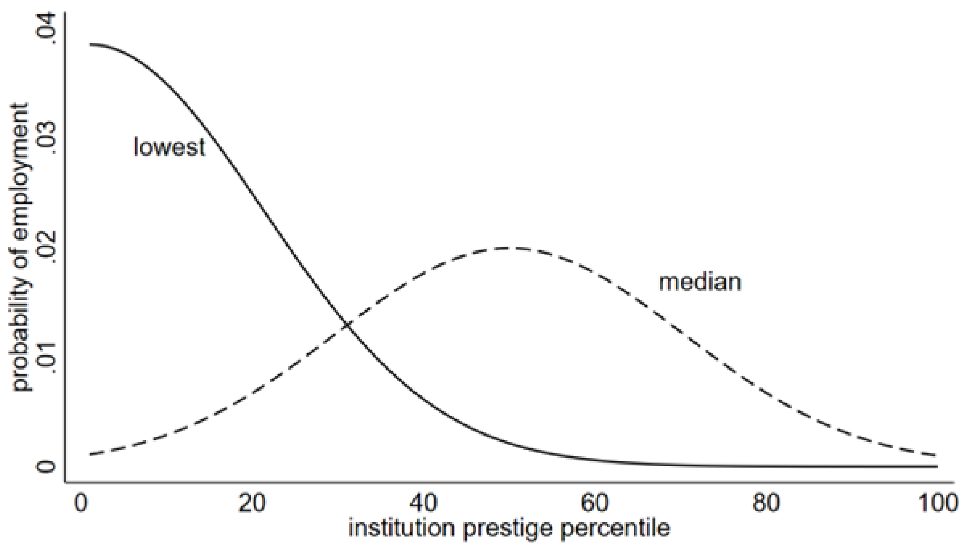
C A simple example

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' 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 C1 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 C1: Probability of prestige level: lowest and median quality faculty



D Figures

Figure D1: Average faculty log salary and institution rankings

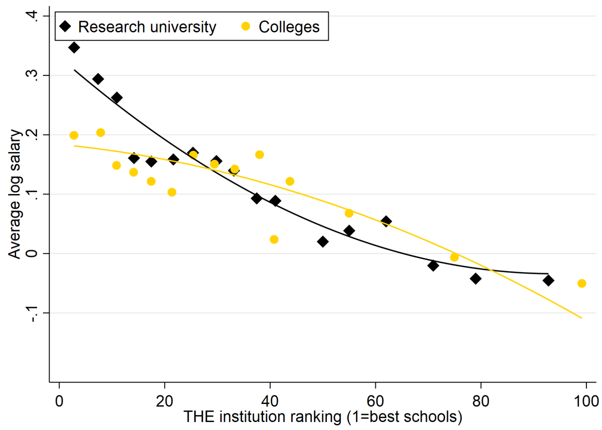
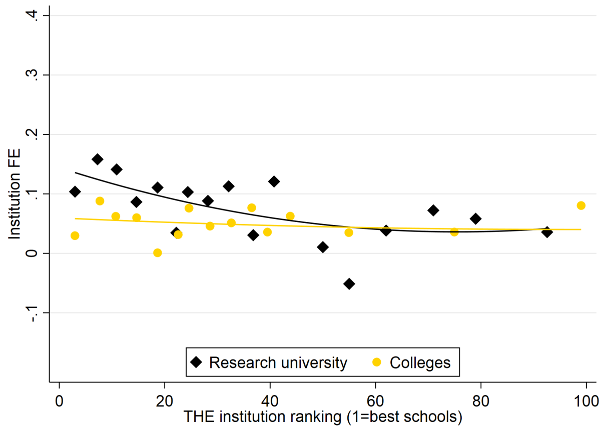
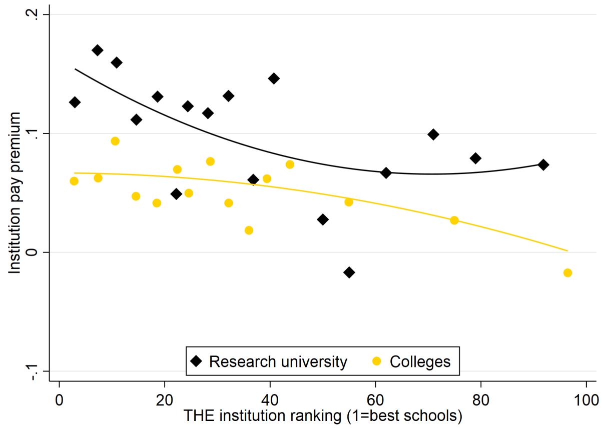


Figure D2: Institution premium and institution rankings (weighted by number of movers)



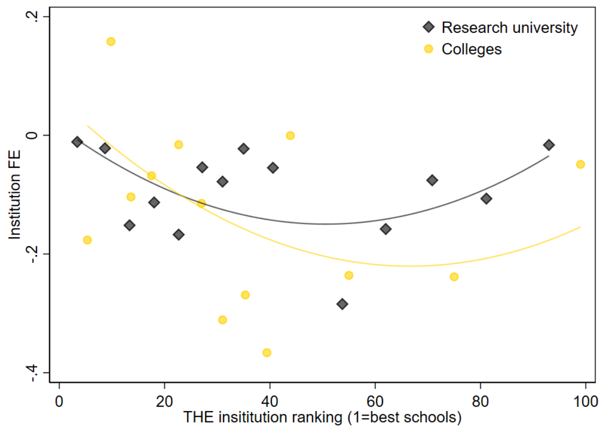
Note: the observations are weighted by the number of movers in the cell. Therefore, each cell accounts for the same number of movers.

Figure D3: institution premiums and rankings 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.

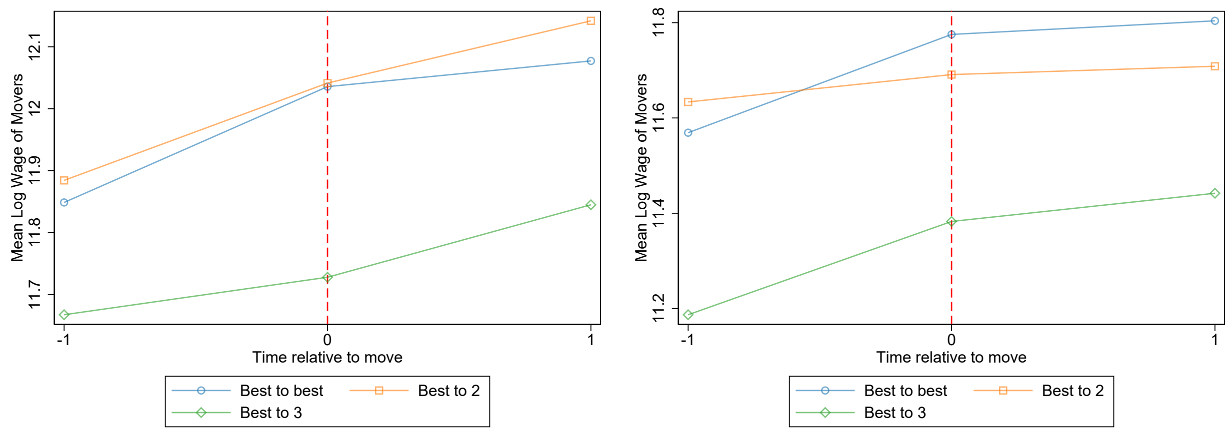
Figure D4: Institution premiums and rankings for Biological Sciences PhDs



Note: The figure shows the relationship between institution pay premiums and the THE rankings. The premiums are estimated over the sample of faculty with biological sciences PhDs.

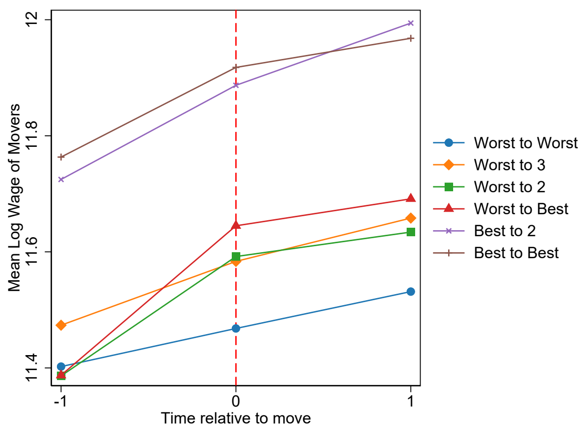
Figure D5: Event studies for moves across quartiles of institution prestige

(a) Universities (b) Colleges



Note: The figure shows the average wages of movers by type of institution and by type of move. Institutions are grouped into quartiles of the THE rankings, and the figure classifies the moves according to the quartiles of origin and destination. Transitions from Best to worst institutions were suppressed to meet the NCSES privacy requirements.

Figure D6: Event studies for moves across quartiles of coworkers' salaries



Note: The figure shows the average wages of movers by type of institution and by type of move. Institutions are grouped into quartiles of coworkers' salaries, and the figure classifies the moves according to the quartiles of origin and destination. Wages for some transition types were suppressed to meet the NCSES privacy requirements.