Title: The influence of pay transparency on (gender) inequity, inequality, and the performance-basis of pay

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Abstract: Recent decades have witnessed a growing focus on two distinct income patterns: persistent pay inequity, particularly a gender pay gap, and growing pay inequality. Pay transparency is widely advanced as a remedy for both. Yet we know little about the systemic influence of this policy on the evolution of pay practices within organizations. To address this void, we assemble a data set combining detailed performance, demographic and salary data for approximately 100,000 US academics between 1997 and 2017. We then exploit staggered shocks to wage transparency to explore how this change reshapes pay practices. We find evidence that pay transparency causes significant increases in both the equity and equality of pay, and significant and sizeable reductions in the link between pay and individually-measured performance.

Brief Summary: Our results suggest pay transparency has a significant and economically sizeable effect in reducing pay inequality and inequity, including by gender, as well as weakening the link between observable performance metrics and pay.

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Recent decades have witnessed a growing global focus on two distinct income patterns: persistent pay inequity, particularly a gender pay gap, and growing pay inequality *(1, 2)*. Though sometimes used rather interchangeably, pay equity references the fairness by which pay is allocated, often measured as the consistency or non-discriminatory manner by which pay is matched to performance or effort. By contrast, pay equality is self-evidently the equivalence of pay, often measured as simply the variance in pay within an organization or society *(3)*. While recent studies suggest global reductions in the magnitude of still persistent pay inequity, specifically the gender pay gap *(4, 5),* they also consistently point to a global pattern of increasing pay inequality within organizations *(6)* and societies *(7)*.

In partial response to these patterns have come abundant calls from politicians and advocacy groups for greater transparency in pay allocation, particularly the public disclosure of individual income *(8)*. The argument is that enhanced pay transparency places social pressure on those allocating pay to reduce both inequity and inequality, including by gender. Accordingly, many nations, states, and organizations have taken directional steps to heed this call. But, resistance to pay transparency within the private sector remains quite deep-seated. A recent survey of US employers suggests 41% actively discourage their employees from simply sharing information about pay with their organizational peers, while 25% explicitly prohibit it *(9)*. The common explanation is that the heightened focus on equity and equality that pay transparency prompts undermines the capacity to link individual pay and performance, thereby compromising organizational efforts to effectively motivate employees and attract talented ones.

Some prior, but mostly contemporaneous, research explores pay transparency's influence on specific dimensions of pay allocation, particularly pay equality. For instance, a published study by Mas (*10*) and unpublished studies by Baker, Halberstram, Kroft, Mas and Messacar (*11*), Bennedsen, Simintzi, Tsoutsoura and Wolfenson (*12*), and Cullen and Pakzad-Hurson (*13*) all point to pay transparency prompting organizations to make pay more equal, including more equal by gender and rank. Cullen and Pakzak-Hurson develop a formal model predicting pay compression following transparency as a result of bargaining concerns. They show corroborating evidence from the private-sector of the US economy using a staggered rollout of policies facilitating communication about pay between co-workers. In a working paper that explores Canadian academics' salaries, Baker et al. leverage the staggered introduction of pay transparency laws across Canadian provinces (and partly within institutions) to show that pay transparency is associated with more equal pay by gender, essentially a narrowing of a gender pay gap that is adjusted for rank but not individual performance. Gartenberg and Wulf (*14*) find that pay transparency is associated with a diminished relationship between division manager pay and division performance within geographically dispersed firms. While these studies explore pay transparency's influence on specific elements of pay in organizations, decisions about pay equity, pay equality, and pay for performance are highly interrelated. Our aim is this paper is to examine pay transparency's systemic influence on an organization's pay practices, specifically pay equity, including the performance-conditioned gender pay gap, pay equality, and pay for performance.

To explore pay transparency's broad influence on pay requires access to rather unique data. Ideally, we would observe a large panel of individual employment data that includes both performance and salary histories surrounding exogenous shocks to pay transparency. The US academic context provides an appealing setting to assemble such data. First, many key individual productivity outcomes are observable and measurable, enabling relatively reliable estimates of both discriminatory and non-discriminatory wage differentials, including the performance-conditioned gender pay gap, as well as estimates of pay for performance. Second, through the Freedom of Information Act and state-specific Sunshine laws, salary data of public university employees have become available in most states, albeit archived in widely disparate data repositories and with varying ease and cost of access. Finally, in the last decade a wave of transparency events in the form of published websites dramatically eased university employees' access to peer salary data. These websites appeared in a staggered fashion essentially state by state, but each well after the imposition of the Freedom of Information Act and state-specific Sunshine laws. Our data combine detailed information about the individual academic performance of close to 100,000 US academics (i.e. their publications, awards, grants, books, and patents), with their demographic characteristics (gender, as estimated dichotomously from first names, rank, tenure, and discipline), and their salary histories between 1997 and 2017. We then exploit staggered shocks to the accessibility of information on wages in the public university systems in the United States to explore how pay transparency changes pay equity and pay equality, as well as the performance-basis for pay, specifically how the links between pay and observable performance measures change both within the broader population and within individual academic departments and institutions.

Our results suggest pay transparency has a significant and economically sizeable effect in reducing pay inequity, significantly reducing the performance-conditioned gender pay gap, as well as more broadly improving the precision with which pay is linked to observable performance metrics and promotion. Overall pay allocation becomes more fair, equitable, and less discriminatory, at least on the performance dimensions we can measure. At the same time, our results suggest pay transparency has a significant and economically sizeable effect in increasing the equality of pay, reducing by nearly 20% the level of pay variance within departments and institutions. Overall pay also simply becomes more equal.

One potential way an organization composed of individuals with heterogenous abilities can generate both more equal and equitable pay is to have pay both more precisely and more weakly linked to individual performance. We find evidence of precisely this: pay transparency leads to significant and economically sizeable reductions in the performance basis of pay. Not only is pay more consistently and equitably linked to performance, but the financial rewards linked to observable performance metrics as well as rank advancement significantly decline after wages become more transparent.

In aggregate, our results confirm that pay transparency has the consequences that many policy advocates claim. It prompts organizations to reduce inequity and inequality in pay allocation. At the same time, pay transparency has consequences less frequently discussed. Pay transparency prompts those allocating pay to weaken the link between observable performance metrics and pay. We view our results as providing an empirical test of the causal effect of pay transparency on three fundamental attributes of pay: pay equity, pay equality, and the performance-basis of pay in organizations, thereby generating a framework for policy makers and practitioners alike to evaluate and debate the arguably complex consequences of pay transparency.

RESULTS

Wage Transparency and Pay Equity

We begin by analyzing the impact of pay transparency on pay equity—the fairness and consistency with which an institution or department allocates pay to individuals. Empirically, we operationalize equitable pay as a 'market wage' or pay that is predicted, in any given year, by observable productivity outcomes (published articles, books, grant funding, patents awards, and academic rank), academic experience, and institutional and academic field affiliation. Unfair pay, at an individual level, falls above or below this estimated market wage, while discriminatory pay, such as gender-based discrimination, is evident from a category's systematic deviation from this predicted fair wage. We acknowledge that this operationalization may mask significant inequities that are driven by hiring and promotion processes, allocation of tasks, or discrimination that plays out in the generation of productive outcomes such as publications, awards or grants *(15-18)*. We recognize that such discrimination will be "hidden" in our approach and unobservable in assessing who is unfairly overpaid or underpaid. Individuals may also have differing beliefs about whether the academic market is fairly weighting specific variables as measured in our models. Our interest though is in whether pay transparency reshapes the consistency with which pay is allocated to these observable measures.

One of the key dimensions of inequity in allocation of wages concerns discriminatory practices based on gender. Indeed, the proponents and regulators in favor of greater wage transparency often claim that such policies are likely to be an efficient tool in detecting and forcing organizations to eliminate this precise form of discrimination *(8, 19)*. In the results that follow, we first present evidence of pay transparency's influence on what we call the conditional gender pay gap—the gender pay gap after controlling for rank and performance, and then move to examining pay transparency's influence on the equitability of pay allocation more broadly.

Our data suggest that both the unconditional gender wage gaps (not controlling for performance or rank measures) and the conditional gender wage gaps have been decreasing steadily over the last two decades in our sample of academics, though both gaps continue to be sizeable (see Extended Data Figure 1 and Table S3.1). Controlling for institutional affiliation, academic discipline, tenure, and productivity (articles and books publications, grants, patents, and awards), an average female academic in our sample was paid 7.7% less than her male counterpart in 2010. While there is important heterogeneity across states and academic disciplines (see Extended Data Table 1), our data indicate that women continue to be underpaid compared with men across the eight states and academic disciplines. By 2017 the average conditional gender pay gap had narrowed but remained significant at 2.6%.

This decreasing level of inequity can be partially explained by shocks to accessibility of information on salaries. To explore how these transparency shocks differentially affected subsequent wages of men and women academics in our sample, we specify the following econometric model explaining individual *i*'s wages in year *t* ($Y_{i,t}$):

$$(\text{Eq. 1}) \quad \ln(Y_{i,t}) = F_i + \sum_k \beta_k Treatment_{i,t}^k + \sum_k \gamma_k Treatment_{i,t}^k \; x \; F_i + Controls_{i,t} + e_{i,t}$$

where *k* corresponds to the number of lags (set in the dynamic model to three short term and one long term lags) and leads (the year following the shock, three short term and one long term leads) of the treatment event. The model further includes time-varying productivity controls and, year, institution and individual fixed effects. As institutions don't change states over time, we omit state fixed effects from our models; when individual fixed effects are omitted, we additionally include academic domain fixed effects. F_i is an indicator variable taking value of 1 for female academics and 0 otherwise. All models reported in the paper are OLS regressions with correction for multi-way fixed effects *(20)* implemented in the regdhfe STATA (version 16) package.

The coefficients of initial interest are γ_k representing the marginal effect of year from (to) transparency shock on the wages of female academics compared to a baseline (male academics). In Table 1, we report full multivariate results based on the static, or canonical specification (omitting individual lags and collapsing all leads to one treatment dummy) allowing us to better quantify the size of these effects. In our basic models, we cluster errors at the level of the institution. Our results are robust to clustering at the state-year and state level (see section S6 in supplementary materials for more details on calculation of standard errors with few clusters). For comparison purposes we also report additional results in Table S3.2. In models including individual fixed effects, the dummy variable for female and academic domain fixed effects are absorbed. Depending on the model, we estimate that the transparency shocks led to the conditional gender pay gap closing by a range of 2 to 5.9 percentage points. The magnitude of these effects is comparable to results reported by Baker at al. *(11)* who estimate a 2 percentage point reduction in the gender pay gap resulting from changes to transparency laws in Canada.

----- Insert Table 1 about here -----

In Extended Data Figure 2 we plot coefficients from a dynamic specification including individual lags and leads, full regression results are reported in Table S3.3. A visual inspection indicates a noticeable increase in relative wages of female academics compared to male academics in the years following the transparency shocks. Although we observe no pre-trend in the three years preceding the transparency shock, the long-term lag coefficient is negative. While the long-term lag coefficient is less readily comparable with other estimates because of the composition effect, a pre-trend could be of concern. To address this concern, we conduct five additional sets of analyses. First, we restrict our data to years 2004-2011 or 2004-2013 inclusive. This ensures that at least one state (PA, in the sample restricted to 2013 and PA and FL in the sample restricted to 2011) is never treated within our observation period. Results of the model restricting data to 2013 are presented in Extended Data Figure 3 (panel A) and show a pattern that is consistent with our earlier estimates. Due to a shorter right-hand side time-series of our data we only report the period following the shock, one short term lead, and one long-term lead estimates in this specification. Second, we employ a "stacked" difference in differences approach (*21*). For each of our first three cohorts of treatment (Cohort 1: WV, shock in 2007; Cohort 2: NY and CA, shock in 2008; Cohort 3: TX, shock in 2009) we create three separate datasets that only include cohort specific states and, as control group, states that have not been treated in the two years following the shock. For example, Cohort 2 includes NY and CA as treated states and PA and FL as control group, with academics from all other states dropped. We then stack these three datasets, centering on the timing of the treatment, including three lags, the year following the treatment, and two leads of the transparency event. We further saturate fixed effects with cohort dummies. We present results of the dynamic model fit to this data in Extended Data Figure 3 (panel B) and show that the stacked specification does not suffer from a pre-trend in the three years preceding the shock. Third, and to directly tackle the possible pre-trend concern we rely on an instrumental variable estimation strategy (*22*). We implement a 2SLS estimator wherein a covariate – in our setting ideally showing similar trends to the gender pay gap pre-transparency shock but unaffected by the treatment – is included in the regression and instrumented with a one-year lead of the transparency shock. In the reported specifications, we use the state-year measure of unconditional gender wage gap in the private sector. We present results of this model in panel C of Extended Data Figure 3. Details of the additional data used for this analyses are discussed in section S3 of the supplementary materials. Fourth, and to gain stronger insight into a potential pre-trend, we drop from our analyses the three earliest-treated states (WV, CA, and NY) and hence restrict our data to states for which we have a longer panel of pre-treatment data. Panels D and E report results from a dynamic model fit to this data that partially alleviates concerns that our estimated results are driven by anticipatory wage adjustments. Finally, in Panel E we report results from a fully unconstrained model (including all possible leads and lags of transparency shocks) along with fitted trend lines. These supplemental analyses, more fully documented in Tables S3.3 - S3.8, reduce pre-trend concerns and strengthen support for a conclusion that pay transparency reduces the performance-conditioned gender pay gap.

Another approach to exploring the influence of pay transparency on the gender pay gap is to simply visualize how the precision with which pay is predicted by observables changes post pay transparency for men and women. To generate this visualization, we plot how wage transparency affects the distribution of residuals from our market wage regressions. For each academic, we predict yearly "fair" market wages based on their institutional affiliation, academic domain affiliation, academic tenure and productivity outcomes (see also section on mechanisms below for a more detailed discussion of the market wage estimation). Figure 1 shows kernel density plots of residuals for female and male academics prior and posterior to transparency shocks. Actual, observed wages below this predicted wage, are considered inequitable underpayment, while observed wages above this predicted wage are considered inequitable overpayment. The pattern in the data is quite clear. Prior to transparency shocks, women are significantly more likely to be underpaid (compared to estimated market wage) than are men. This can be seen from a larger mass of the distribution of residuals to the left of the estimated market wage. Women are also less likely to be overpaid (compared to estimated market wage). Indeed, the overall distribution of residuals from market wage is shifted to the left and narrower for women, compared with men. Following the pay transparency shocks, the two distributional plots become substantially more aligned. Although women are still more likely to be underpaid and less likely to be overpaid than men, these differences become smaller posterior to the transparency shocks. These results echo our earlier estimates indicating that although eased access to wage information had a positive causal effect on closing the gender pay gap, some unexplained differences continue to be present in our data.

----- Insert Figure 1 about here -----

The gender pay gap is, of course, but one manifestation of possible inequitable wage practices in organizations. Full exploration of all discriminatory factors is well beyond the scope of this paper. However, we also investigate the extent to which pay transparency leads to an increase in the overall equity or consistency with which pay is associated with productivity and rank. To perform these analyses, we measure variance in residuals from 'fair' market wage regressions within academic fields by institution. This measure takes on smaller values in departments where most faculty are equitably paid, and where faculty are on average consistently under- or over-paid relative to the broader market, but takes on larger values in departments where the determinants of pay allocation are uneven and vary substantially across members. We specify the following econometric model explaining variance in fair wage residuals in year *t* ($VarR_{j,t}$) by academic field-institution *(j),* using either the 11 or 25 academic field categories:

$$\text{(Eq. 2)}\quad VarR_{j,t} = \sum_k \beta_k Treatment_{j,t}^k + Controls_{j,t} + e_{j,t}$$

where controls include mean levels and variance in all productivity outcomes by academic field, with *j* and *t* fixed effects, and *k* defined analogously to Equation 1. Table 2, Model 1 presents the results of a static difference-in-differences specification, using the 11 academic fields specification. We find that wage transparency decreased variance in residuals by 11%, on average. Finally, in Figure S4.1 (left panel), we present results of the dynamic specification that reveal patterns consistent with results previously discussed. These analyses indicate that, if anything, inequity in pay allocation had been rising in years leading to transparency shocks – a trend that is reversed around the time of treatment. Taken together our results indicate that eased access to wage information resulted in decreasing various forms of wage inequity within academia.

----- Insert Table 2 about here -----

WAGE TRANSPARENCY AND PAY EQUALITY

Unlike equity, equality is an absolute construct *(3, 23)*, and when applied in its strongest form, full equality would imply no variance in wages across all individuals, independent of either performance or rank. Although we could theoretically imagine compression taking place across all sectors of an economy or across all institutions and disciplines within academia, we explore whether wage transparency results in greater equality defined as lesser dispersion in wages among department peers, independent of their performance.

Table 2, Model 2 presents the first set of analyses of the impact of pay transparency on equality of pay. We report regression results of the static difference-in-differences specification explaining changes to institution-department wage variance based on calculation of variance across different reference categories defined by institutional and academic field affiliation, as in the model below:

$$\text{(Eq. 3)} \quad VarY_{j,t} = \sum_k \beta_k Treatment_{j,t}^k + Controls_{j,t} + e_{j,t}$$

where $VarY_{j,t}$ corresponds to the field-institution-level variance in (ln) wages, and all other parameters are specified identically to those in models estimating Equation 2. Our results are consistent across models and imply that wage transparency had a strong effect in prompting equality through reduced wage dispersion. The long-term impact is economically sizeable. We find that wage transparency decreased pay dispersion by nearly 20%.

We also analyze results of dynamic specifications. Our results (reported in Figure S4.1, right panel) corroborate the role of transparency shocks in decreasing average wage variance of academics in our data and lack of explanatory pre-trends. To further explore the mechanism behind pay transparency's impact on wage equality we, again, turn to distributional plots. This time however, we focus on wage equality and hence on wage residuals – before and after transparency shocks – from regression models predicting wages based on institutional and department affiliations, while controlling for temporal variation using calendar year fixed effects. Consistent with our earlier results, the evidence emerging from these analyses indicate that pay becomes more equal or simply more compressed following transparency shocks. This narrowing of the wage distribution can be attributed both to reducing the density of positive and negative wage residuals (see Extended Data Figure 5 for kernel density plots).

MECHANISMS

While the prior two sections provide evidence that pay transparency prompts departments and institutions to elevate both pay equity and pay equality, our results to this point offer little visibility into precisely how this has occurred. In this section we explore three possible mechanisms. First, pay transparency may simply heighten pressure to weaken the relationship between pay and performance. Second, pay transparency may prompt institutions to focus on adjusting the pay of those most underpaid or overpaid, measured on the basis of either equality or equity. Finally, pay transparency may prompt mobility (employee entry and exit) that mechanically elevates both pay equity and/or equality. For instance, those who discover they are unfairly underpaid through pay transparency may depart for other institutions, leaving those more fairly paid to remain. We briefly explore evidence for each of these mechanisms.

To explore the first mechanism—a weakening pay for performance relationship, we estimate a series of models predicting the effect of transparency shocks on the relationship between both productivity outcomes and academic rank and salary. We thus specify the following general model explaining (ln) of wages:

$$\text{(Eq. 4)}\quad \ln(Y_{i,t}) = Treatment_{i,t} + \sum_{l} \beta_l P_{i,t}^{l} + \sum_{l} \sigma_l P_{i,t}^{l}\, x\ Treatment_{i,t} + Controls_{i,t} + e_{i,t}$$

where $P_{i,t}^{l}$ corresponds to individual *i*'s productivity on a metric *l* in a year *t*, and *Treatment* is a dummy variable equal to one for all years subsequent to a transparency shock. Alternatively, we substitute productivity outcomes with academic rank (Associate and Full Professor, with Assistant Professor being the reference category). Controls include individual, institution, and year fixed effects. Coefficients β_l indicate the average strength of the link between pay-and-performance while coefficients σ_l indicate how a transparency shock affects this average link between pay-and-performance. Negative σ_l coefficients would indicate weakening of the marginal returns, compared with the baseline pre-transparency levels, while positive σ_l coefficients would signify increased weight on performance in determining wages.

The results indicate that, following transparency shocks, institutions in our data began to rely less strongly on pay for performance and that salary differences across academic ranks also fell significantly. Although all the productivity measures that we observe had, and largely continued to have, a positive effect on salaries (as do promotions), the strength of these relationships weakens substantially following transparency (with awards being a sole exception). We report full results of these regression models in Table S5.1 and S5.3a,b. In Figure 2 we report results of the dynamic model tracing the evolution of marginal returns to advancements across ranks. We observe an economically sizeable, discrete drop in the pattern of marginal returns to rank advancements at the time of transparency shocks, compared with the baseline levels.

----- Insert Figure 2 about here -----

In Table 3 below we summarize the economic magnitude of these changes for a star academic in our sample (in terms of performance outcomes) as well as document heterogeneity across academic fields. The interpretation of these results is as follows. Controlling for academic tenure, field and institution, pre-transparency, an average academic with star levels of performance (i.e. in the 95th percentile) across all metrics could expect to see a 22.2% greater salary than an academic with no output, observable to us. However, post pay transparency shock, we observe a large – 42% – drop in the sensitivity of pay to this composite performance score. The results are similar if we compare the average premium in salaries that accompany rank. In our data, the marginal pre transparency pay increase associated with promotion to associate and full professors – compared with assistant professors – were +15% and +32% respectively. Post pay transparency, these premiums are still substantial, but fell to +9% and +25% respectively.

----- Insert Table 3 about here-----

Our results also reveal a consistent pattern across academic disciplines although, as expected, the sensitivity of pay to different performance outcomes varies by field. For example, out of the five disciplines, humanities places the greatest emphasis (in terms of compensation) on book publications. In turn, biological and biomedical sciences place the greatest emphasis on publications in peer-reviewed journals. At the same time, across all academic disciplines, the sensitivity of pay to performance decreased following transparency shocks. Similarly, across disciplines, the financial rewards associated with promotion across academic ranks fell markedly in response to pay transparency.

To explore a second mechanism, we examine more precisely whose pay is reshaped post pay transparency. Do institutions adjust downward the pay of those inequitably overpaid or adjust upward the pay of those underpaid? To explore these adjustment patterns, we first predict the 'fair' market wage in year *t* for individual *i*, $\widehat{\ln(Y_{i,t})}$, based on their productivity (number of published academic articles, number of published books, number of awards, number of grants, and number of patents), academic tenure, institution, and academic field. Additionally, in our predictions, we also account for heterogeneity in the sensitivity of pay to different performance measures across academic domains.

Armed with these estimates, we test the extent to which deviations in observed salaries from predicted levels are informative about the changes in wages in the following year. For ease of interpretation and calculation of the economic magnitude of our effects, we generate binary measures of upward and downward inequity, coding an individual as underpaid if her salary is below mean levels of negative residual and as overpaid if her salary is above mean levels of positive residual. The reference category (i.e., 'fairly paid') are those whose salaries are between these two levels. In the supplementary analyses, reported in Table S5.5, we also rely on a continuous residual measure and use a spline model, separately introducing positive – overpayment – and (absolute value of) negative – underpayment – residuals. We therefore estimate several variants of the following model predicting wage adjustments:

$$\text{(Eq. 5)} \quad \frac{Y_{i,t+1} - Y_{i,t}}{Y_{i,t}} x100$$
$$= \alpha_1 + \beta_1 Treatment_{i,t} + \beta_2 Upaid_{i,t} + \beta_3 Opaid_{i,t}$$
$$+ \gamma_1 Treatment_{i,t} x Upaid_{i,t} + \gamma_2 Treatment_{i,t} x Opaid_{i,t} + Controls_{i,t} + e_{j,t}$$

where *U(O)paid* correspond to the measures of under(over)payment, compared with the fair market wage. Controls include academic field, institution, and year fixed effects. In some models, we also include a control for absolute level of wages along with its interaction with treatment dummy. Main parameters of interest are β_2, β_3, γ_1, and γ_2 indicating, respectively, the sensitivity of pay increases to negative deviations from market wage (underpayment), positive deviation from market wage (overpayment) and a change in these sensitivities following transparency shocks. The dependent variable, wage change, is multiplied by 100 and thus expressed in percentages.

Our results, reported in Table 4, suggest that institutions generally grant larger salary increases to those we estimate as previously underpaid and smaller increases to those we estimate as previously overpaid. Academics in our sample received an average inflation-adjusted yearly salary increase of 2.8% (Model 2). Our results indicate that pre transparency, those who were significantly underpaid compared to their institution-department peers received yearly increases of 3.8% on average, while those who were significantly overpaid relative to their peers received salary increases of 1.2%, on average. Post-transparency, we observe average wage increases for those underpaid rise to 4.3%, while increases for those overpaid remain unchanged.

We also test if these results are simply driven by differences in the absolute levels of pay (which is correlated with distance from market wage). Over and above simply controlling for absolute levels of salaries, in model 3, we add a dummy variable and its interaction with treatment for low absolute levels of salaries – defined as salaries that are inferior to the year-institution-domain average pay. High salary is the reference category. The results are consistent across models and suggest a narrative that pay transparency heightens attention to both pay equity and inequality and that post transparency institutions seek to remedy inequity and inequality by actively adjusting upward the pay levels of those most underpaid as measured by both equity and equality.

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We explore one final mechanism that may explain our pay transparency results on equity and equality. All our results presented above span the full population of academics, including those switching institutions, and those leaving or joining our sample during the observation period. It is conceivable that shifts in the sample population resulting from employee mobility prompted by pay transparency may explain these increases in pay equity and pay equality. For instance, those receiving low pay, whether fairly or unfairly, may depart in response to pay transparency, leaving those more highly and equitably paid to remain. The net effect may be that those who remain are both more equally and equitably paid. A full exploration of this mobility mechanism is beyond the scope of this paper. However, to validate that this mobility pattern is *not* driving our results, we drop from our sample all academics who have either (1) changed institutions within our observation window but stayed in our sample, (2) left our observation sample between transparency shock and the end of the observation window, or (3) joined our sample posterior to the transparency shocks. Thus, we restrict our analysis to the "non-mobile" workforce. We re-run all our models using this restricted sample and find consistent results throughout. Table S3.7 reports results mirroring those in Table 1 but using the restricted sample. Our results continue to show that transparency shocks decrease the gender pay gap across specifications, although our estimates are generally slightly smaller in economic magnitude suggesting that mobility patterns may indeed play some role in explaining the influence of pay transparency on the declining gender pay gap. We similarly test whether our first mechanism results—a weakening pay for performance relationship summarized in Tables 3 and S5.1—hold with this restricted sample. Our results (Table S5.2) are indeed robust to using this restricted sample, supporting a claim that wage transparency has led to a significant change in organizational pay practices that have fueled shifts in the equality and equity of pay allocation.

In Supplementary Information file (section S6) and the Methods section we discuss an additional series of robustness tests with respect to inferring the statistical significance of our results, sample construction, and the robustness of our results to omitting data from larger states. Importantly, neither the exclusion of Texas nor California, the two most populous states that also generate the most data, significantly changes our results.

DISCUSSION

Our results suggest that in the context of academia in the US, pay transparency has a rather systemic and sizable effect on the structure of pay. While earlier work has particularly shown pay transparency to prompt more equal pay *(10, 11, 13)*, we provide an empirical test of the broader causal effects of pay transparency on pay allocation, including pay equity—the fairness or consistency with which pay is matched to performance and rank, as well as the overall performance-basis of pay. Pay transparency appears to pressure those who assign pay to more aggressively remedy inequities in the allocation of pay, granting larger pay increases to those who, based on their performance and rank, are unfairly underpaid. The performance- and promotion-conditioned gender pay gap is significantly reduced, but more generally pay becomes more precisely predicted by observable performance measures. Pay transparency also appears to pressure those responsible for allocating wages to simply make pay more equal, independent of performance. Pay becomes more compressed and department and institution affiliation predict a larger portion of pay variance. Finally, consistent with pay becoming more equal, in response to pay transparency, academic departments and universities significantly weaken the link between pay and a range of observable performance metrics, including publications, books, and grants. In addition, the marginal returns to advancements in academic rank become significantly smaller. In summary, pay becomes more equal, more precisely assigned to observable metrics, but also less performance- and promotion-based.

Prior work on pay transparency has explored a variety of individual psychological and behavioral responses to pay transparency relating to happiness, satisfaction, or desires to exit *(24-26)*—responses that will vary based on what individuals discover about where their individual level of pay ranks relative to others. Underlying these results is the reality that humans socially compare and when they perceive pay inequity or pay inequality, they experience emotions of injustice or envy that may reduce job satisfaction or well-being *(27-29)*. These in turn may prompt behaviors costly to employers, including turnover, reduced effort and social cohesion, or simply politicking for change in pay allocations *(30-33)*. Enhancing the visibility of pay, enhances the visibility of inequity or inequality, heightens these emotions of envy and injustice and elevates these attendant costly responses, which in turn elevates pressure on employers to seek greater equity and equality.

Our results focus on pay transparency's influence on organization level responses. But of course individual and organization level responses to pay transparency are interrelated and our understanding of pay transparency would greatly benefit from future work exploring if and how these individual effects function as mechanisms that deliver these organization level responses to pay systems. For instance, prior work shows that both men and women may consider comparatively lower female wages as fair (*34*). Future work could fruitfully study the extent to which transparency affects (or does not affect) such justice perceptions. Card and colleagues' (*25*) results, also in an academic setting, suggest pay transparency prompts those receiving below median pay to rather quickly express lower job satisfaction and higher turnover intentions, but actual turnover measured after several years shows a much noisier relationship with relative salary levels. As our study shows, academic institutions are dynamically adjusting pay in response to pay transparency and doing so in patterns that render pay more equal and equitable. Those initially expressing turnover intentions in response to discovering inequity or inequality are precisely those likely to receive larger pay increases, which may quell their initial motivation to depart. Pay transparency may also have important implications for performance and productivity. Breza et al. find that when wages and productivity are visible and inequitably allocated that productivity and cooperation decline (*33*). Perez-Truglia (35) finds that pay transparency increases the well being or happiness gap between those receiving high and low pay—a result that may indirectly contribute to diminished productivity and performance. Precisely how pay transparency jointly shapes both individual and organizational outcomes merits deeper exploration.

For policy makers, including managers responsible for decisions regarding pay transparency, our results illuminate what some might consider an important tradeoff between both increased equity and equality and weakened pay for performance. How policy makers differentially value these pay allocation outcomes will of course weigh heavily in the decision of how transparent to be. Our results may also illuminate why organizations and some policy makers seem to prefer choices to enhance pay transparency that fall short of broadcasting individual levels of pay. Such transparency-increasing practices may reveal the structure by which pay is assigned, such as revealing pay ranges by hierarchical rank, or disclosing pay levels for relevant aggregated groups of peers rather than individual wages for the full set of peers. Such practices may pressure organizations to elevate the fairness and consistency of pay while still maintaining pay for performance. The effectiveness of these alternative forms of pay transparency merit further exploration.

Relative to many other work contexts, ours is one in which individual performance is rather observable. While important elements of academic output are team-based, the diverse array of teams upon which any given individual works, as well as a general pattern of positive assortative matching, yields a reasonably good assessment of individual contributions, at least relative to many other work settings. The fact that even in this environment, one with reasonably visible and objective measures of performance, pay transparency still generates these strong effects on pay equality suggests that our findings may generalize to other contexts. In settings with less visible performance measures, we might predict even stronger pressure to equalize pay—a prediction that Cullen and Pakzad-Hurson (13) theoretically explore. Nonetheless how well our findings generalize to other settings in which pay is made transparent is an important limitation of the study.

Methods

Data

To compile the required data, we first obtained privileged access to a database of individual academic productivity compiled by Academic Analytics, an analytics and consulting service provider to universities and educational institutions. These data were meticulously assembled from publicly available sources for the years 2004-2017 and provide information on individual article publications, books, patents, awards, and grants. The data encompass all full time research faculty across all academic fields employed at 412 PhD-granting institutions in the US, including all 141 AACSB-accredited universities, all 262 R1 and R2, and most of the 161 D/PU institutions.

We then sought matching salary histories for those employed within the public institutions covered by Academic Analytics. While considerable salary data could conceivably be scraped from the publicly accessible websites that appear in each state, these websites generally provide only historical data that begins after the date of the website's publication onward. Since we require data both before and after transparency shocks, we solicited access to salary data through official Freedom of Information Law (FOIL) requests to state comptroller offices and individual public universities in all 50 states. As a general pattern, only more populous states were able or willing to provide the necessary salary data in a digital format (see supplementary text S2 for more details). From these requests, we obtained yearly employment and salary information for nearly all public university employees in eight states: California, Connecticut, Florida, New York, Pennsylvania, Texas, Virginia, and West

Virginia. These responses encompassed data from 139 institutions spanning 1997 through 2017 (inclusive, see Table S1.1 for details and exceptions).

To then merge salary data with productivity data, we developed a matching algorithm based on names and other overlapping individual information. Our final merged dataset spans 676,055 individual-year observations. This includes: 97,839 distinct individuals employed in 139 distinct institutions across 11 or 25 broad academic fields (depending on the level of aggregation). We coded gender dichotomously, based on academic's first name. Thus, our study does not allow to account for the full spectrum of gender identities. The coding procedure was carried out by Jeremy Cox.To identify male and female academics, we used the World-Wide Gender Dictionary (available at https://ideas.repec.org/c/wip/eccode/10.html). We only kept names uniquely identified as male or female in this dictionary. We then manually checked the un-matched names and identified 128 additional names to which we could attribute gender with rather high levels of confidence. In particular, we used data from the US Social Security administration between 1880 and 2020 to calculate the relative frequency of each name for a specific gender (female/male). All but two additional names show at least 90% consistency in allocation to a gender. We coded two remaining names: Lauren (83% female frequency) and Lindsay (65% female frequency) as female names. Exclusion of these two names does not materially affect our results. A list of 128 additional names along with gender coding is posted along with main datafiles and code. Using this method, we identified 52,016 individuals as men and 28,839 as women. For models with gender, academic tenure, and productivity controls, our sample size decreases to 44,837 individuals due to missing gender data and the fact that our observation window for productivity outcomes begins in 2004. All models including productivity outcomes span 2004-2017. All models without productivity outcomes span the full range of available data (unless explicitly specified otherwise). To account for outliers, in all models, we drop 0.5% of the top and bottom earners and all salaries are expressed in constant 2016 US dollars.

Finally, we were able to unambiguously identify rank from job titles for 46,572 individuals in seven out of eight states (with New York, as the exception). Given that data limitations constrain us to use a subset of individuals for models that include productivity outcomes, gender, and rank information, as a robustness test we re-run all our analyses presented throughout this paper using the most constrained data – one that features no missing data on any of the variables. All our results remain robust to this reduced sample of academics and to the shortened time period. Details on construction of all variables are provided in the supplementary text S1. Summary statistics for academics in our final sample are reported in Tables S1.2 and S1.3. Additional data sources include: Current Population Survey conducted jointly by U.S. Census Bureau and the Bureau of Labor Statistics, used to construct measures of unconditional gender wage gap in the private sector; journal impact factors are crawled from Journal Citation Reports, and ranking of academic institutions is constructed based on Academic Ranking of World Universities data.

To explore the causal effect of wage transparency on the constructs of interest, we take advantage of staggered shocks to the accessibility of wage information about public university employees that occur within the eight states covered by our data. Over the past 15 years, public access to wage information on government employees has been significantly facilitated by the emergence of searchable datasets developed and launched by newspapers, NGOs, and state agencies. Examples of such databases include the California State Worker Salary Database launched by Sacramento Bee in 2008 or Florida Has the Right to Know initiated by Florida's governor Rick Scott in 2011. For each state in our sample, we identify the year in which the first such database was launched. We then consider each individual academic as treated if (s)he is employed in one of the institutions of the focal state in any of the years following the launch of the database. In the sample of eight states covered in our database, such shocks to transparency happened in a staggered fashion, between 2007 and 2012. Details of the institutional context and all transparency shocks are provided in the supplementary text S2 and Table S2.1.

Additional Robustness Tests

Over and above the tables reported in the manuscript and supplementary materials, we also conducted three additional sets of analyses to ensure robustness of our results. First, we restrict our data to academics employed in highly research intensive institutions. Our concern in relying solely on pooled results was that the basis of pay may significantly differ across types of institutions. Each year, we ranked 139 institutions present in our data using the Academic Ranking of World Universities. We then re-run all our analyses keeping only 70 most highly ranked institutions and found robust results. Second, we re-run models reported in Table 1 including state-specific time trends and 1- and 3- year anticipatory effects (*36).* Third, because of particularities of the payment scheme to health professionals (fee-for-service) we also re-run all our analyses dropping this academic domain from our sample and finding robust results.

Data Availability

All data that support the findings of this study along with statistical codes generating the results have been deposited in the Open Inter-university Consortium for Political and Social Research Repository (OPENICPSR) under project number 155541 and are available at: https://doi.org/10.3886/E155541V1. Names of individual academics and institutions have been blinded and are represented in the data with author-generated unique identifiers.

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Author Contributions Statement

T.O. and T.Z. jointly conceived of the project and supervised data collection, T.O. conducted data analyses with input from T.Z. T.O. and T.Z. jointly drafted the manuscript.

Competing Interests Statement

The authors declare no competing interests.

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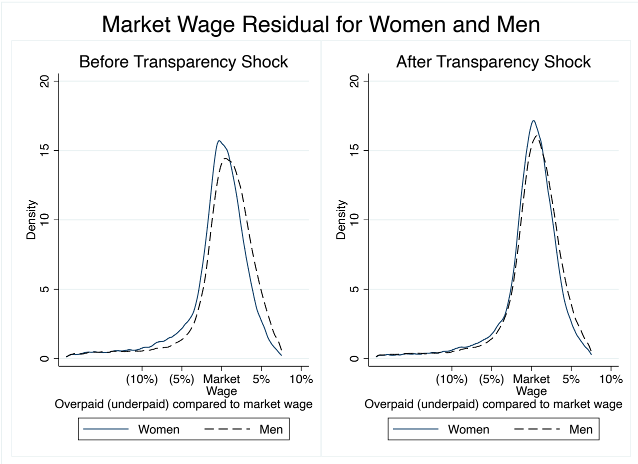
Supplementary Materials:

Materials and Methods:

Tables: S1.1, S1.2, S1.3, S2.1, S3.1, S3.2, S3.3, S3.4, S3.5, S3.6, S3.7, S4.1, S4.2, S4.3, S5.1, S5.2, S5.3a, S5.3b, S5.4, S5.5, S6.1, S6.2, S6.3.

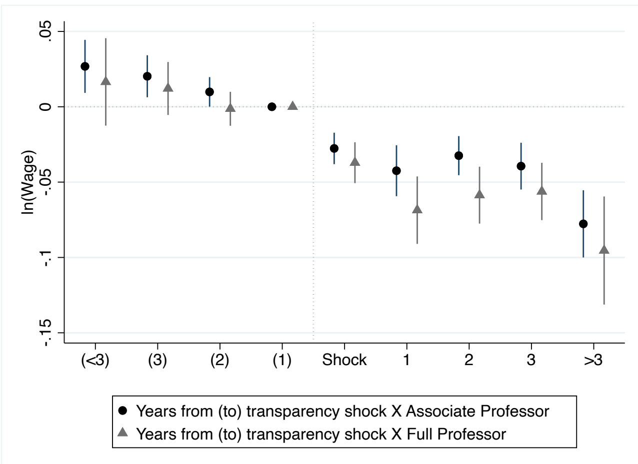
Tables and Figures

Figure 1. Equity in organizations: Distribution of market wage residuals, by gender and transparency shocks.



Notes: The figure presents kernel density estimates of (ln) wage regression residuals by gender and transparency shocks. Controls include academic tenure (ln), number of academic articles, number of published books, number of awards, number of grants, and number of patents and institution as well as academic domain, and year fixed effects. In order to allow comparison, all models are run jointly for men and women. Residuals trimmed at 1% and 99%. Two-sample combined Kolmogorov-Smirnov tests for equality of distribution functions: 0.124, p-value<0.001 (left panel); 0.067, p-value <0.001(right panel).

Figure 2. The effect of wage transparency on salary adjustments associated with promotions.



Notes: The figure presents regression coefficients from a dynamic difference-in-differences OLS regression model explaining (ln) wages. Reference category is 1 year prior to transparency shock. Plotted coefficients: years from (to) transparency shock interacted with rank, with 95% CIs. Models run jointly for all ranks. Errors clustered on institution. Controls include year, institution, and individual fixed effects. Regression results used to generate this plot are reported in Table S5.4.

Table 1. The effect of pay transparency on gender wage gap.

	β, t_{df}, (s.e.), p-value, [95% CI]			
DV: ln(Wage)	(1)	(2)	(3)	(4)
Treatment	0.006, t_{138}=0.60 (0.011), 0.55 [-0.01;0.03]	-0.009, t_{138}=1.04 (0.009), 0.30 [-0.03;0.01]	-0.009, t_{85}=1.10 (0.008), 0.28 [-0.02;0.01]	-0.015, t_{85}=1.82 (0.008), 0.07 [-0.03;0.00]
Female	-0.112, t_{138}=12.41 (0.009), <0.001 [-0.13;-0.09]	absorbed	-0.062, t_{85}=11.93 (0.005) , <0.001 [-0.07;-0.05]	absorbed
Treatment # Female	0.059, t_{138}=7.22 (0.008), <0.001 [0.04;0.07]	0.031, t_{138}=7.44 (0.004), <0.001 [0.02;0.04]	0.020, t_{85}=3.99 (0.005), <0.001 [0.01;0.03]	0.021, t_{85}=5.76 (0.004) , <0.001 [0.01;0.03]
Associate Professor			0.121, t_{85}=14.95 (0.008), <0.001 [0.10;0.14]	0.064, t_{85}=8.79 (0.007), <0.001 [0.05;0.08]
Full Professor			0.391, t_{85}=33.25 (0.012), <0.001 [0.37;0.41]	0.173, t_{85}=12.77 (0.014), <0.001 [0.15;0.20]
Productivity controls	yes	yes	yes	yes
Individual fixed effects	no	yes	no	yes
Academic field (11 categories) fixed effects	yes	absorbed	yes	absorbed
Institution fixed effects	yes	yes	yes	yes
Year fixed effects	yes	yes	yes	yes
Observations	306,404	300,853	195,976	194,077

Notes: The table presents OLS regression estimates explaining (ln) salaries. Productivity controls include academic tenure (ln), number of academic articles, number of books, number of awards, number of grants, and number of patents. In models 5-6 omitted category is Assistant Professor. Standard errors clustered at the level of institution. See Table S3.2 for additional specifications.

Table 2. The effect of pay transparency on institution-academic field variance in market wage residuals and variance in (ln) wages.

	β, t_{df}, (s.e.), p-value, [95% CI]	
	(1)	(2)
DV:	Variance in market wage residuals	Variance in (ln) wages
Within :	Institution–academic field (11 categories)	
Treatment	-0.034, t_{130}=2.27 (0.015), 0.025 [-0.06;-0.01]	-0.065, t_{136}=4.49 (0.015), <0.001 [-0.09;-0.04]
Institution-academic field (11) fixed effects	Yes	Yes
Year fixed effects	Yes	Yes
Observations	9,015	12,892

Notes: The table presents OLS regression estimates explaining institution-academic field variance in residual from market wage regressions (model 1) and variance in wages (model 2). Market wage regression controls include academic tenure (ln), number of academic articles, number of published books, number of awards, number of grants, and number of patents and institution, academic domain, and year fixed effects. Residuals trimmed at 1% and 99%.

Standard errors clustered at the level of institution. See Tables S4.1 and S4.2 for additional results.

Table 3. Predicted marginal effects of star levels of performance and rank advancements on wages before and after transparency shocks.

	Population		Humanities		Physical and Mathematical Sciences		Biological and Biomedical Sciences		Social and Behavioral Sciences		Engineering	
Before/after transparency	Before	After	Before	After	Before	After	Before	After	Before	After	Before	After
Academic articles	10.8%	5.6%	2.8%	2.3%	12.9%	8.6%	21.2%	8.3%	10.6%	6.0%	18.0%	9.9%
Patents	0	-0.2%	0	0	2.6%	0.3%	0	0	0	0	4.2%	4.2%
Books	3.3%	1.6%	8.5%	4.9%	0	0	0	0	4.8%	1.6%	0	0
Grants	8.1%	5.8%	0	0	14.7%	10.6%	10.8%	10.8%	6.3%	6.3%	7.4%	7.4%
Awards	0	0	0	0	0	0	0	0	0	0	0	0
Promotion to Associate (compared with Assist. Professor)	14.5%	8.7%	12.7%	7.9%	11.4%	4.6%	14.4%	7.3%	14.3%	9.7%	10.3%	4.6%
Promotion to Full (Compared with Assist. Professor)	31.9%	25.2%	27.8%	24.1%	22.7%	17.9%	30.6%	23.5%	30.1%	25.3%	18.6%	14.4%

Notes: The table presents the predicted marginal effects of performance outcomes on salaries. Star performance levels are calculated at the 95th percentile of the performance distribution across all years, separately for each domain when relevant, and within regression samples. When 95% confidence intervals of the estimated coefficients include zero, we report the marginal effect to be nil. Regression results used to generate this table are reported in Tables S5.3a and S5.3b.

Table 4. The effect of market wage and pay transparency on yearly wage changes.

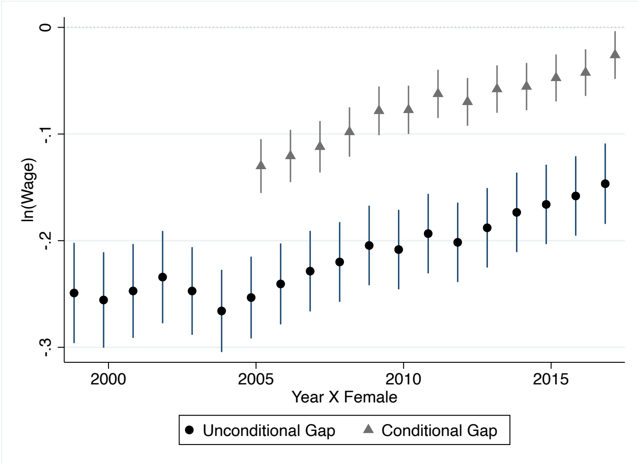
	β, t_{df}, (s.e.), p-value, [95% CI]		
	Underpaid and overpaid: binary specification		
DV: % Wage change	(1)	(2)	(3)
Treatment		-0.157, t_{129}=0.38 (0.413) , 0.70 [-0.98;0.66]	-0.347, t_{129}=0.87 (0.385) , 0.385 [-1.13;0.44]
Underpaid	1.372, t_{129}=8.83 (0.155), <0.001 [1.06;1.68]	1.028, t_{129}=4.65 (0.221) , <0.001 [0.59;1.47]	0.766, t_{129}=4.43 (0.173) , <0.001 [0.42;1.11]
Underpaid # treatment		0.531, t_{129}=2.75 (0.193) , 0.007 [0.15;0.91]	0.516, t_{129}=3.17 (0.163) , 0.002 [0.19;0.84]
Overpaid	-1.568 t_{129}=15.16 (0.103), <0.001 [-1.77;-1.36]	-1.582, t_{129}=9.82 (0.161) , <0.001 [-1.90;-1.26]	-1.452, t_{129}=10.49 (0.138) , <0.001 [-1.73;-1.18]
Overpaid # treatment		0.023, t_{129}=0.17 (0.135) , 0.864 [-0.24;0.29]	0.161, t_{129}=1.29 (0.125) , 0.200 [-0.09;0.41]
Low salary			0.528, t_{129}=2.20 (0.240) , 0.030 [0.05;1.00]
Low salary # treatment			0.282, t_{129}=1.28 (0.219) , 0.202 [-0.15;0.72]

Academic field fixed effects	Yes	Yes	Yes
Institution fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Observations	259,624	259,624	259,624

Notes: $DV = \frac{Wage_{i,t+1} - Wage_{i,t}}{Wage_{i,t+1}} \times 100$. Underpaid is equal to 1 if individual *i*'s residual from regression predicting market wage in year *t* is negative and smaller than the average residual from the same regression for all individuals in a given year, the same institution and the same academic domain. Overpaid is equal to 1 if individual *i*'s residual from regression predicting market wage in year *t* is positive and greater than the average residual from the same regression for all individuals in a given year, within the same institution and the same academic field. *Low salary* is equal to 1 if individual *i*'s salary is below average, compared to year-institution-domain peers and 0 otherwise. Standard errors clustered at the level of institution. See Table S5.5 for additional results including continuous specifications of (over)underpaid variables.

Extended Data Figures and Extended Data Tables

Extended Data Figure 1. The unconditional and conditional gender wage gap over time.

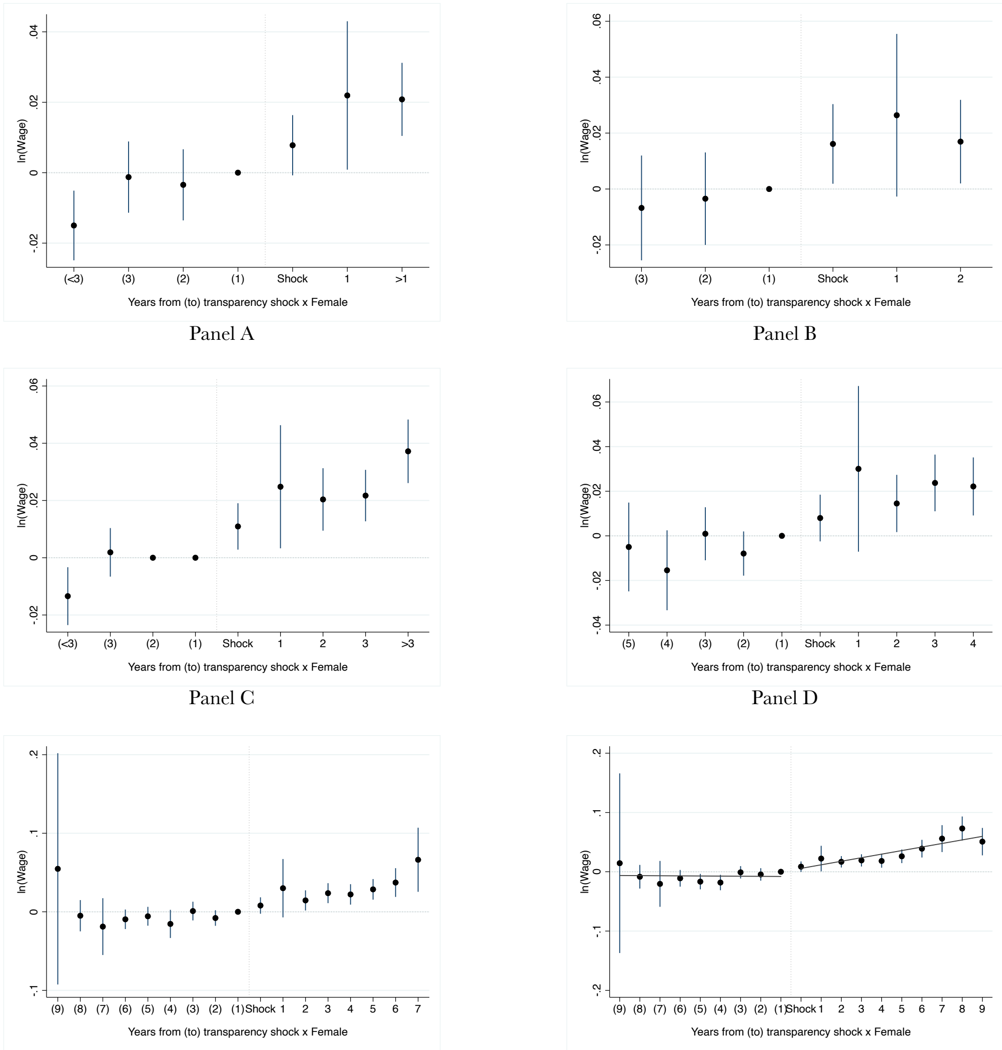


Notes: The figure presents OLS regression estimates explaining (ln) salaries. Plotted coefficients of year dummies interacted with Female indicator, with 95% confidence intervals. Levels are scaled by the value on un-interacted Female indicator. Unconditional gap is based on a model with year dummies only. Conditional gap is based on models with year, academic domain, and institution fixed effects as well as controls for academic tenure (ln), number of academic articles, number of published books, number of awards, number of grants, and number of patents. Regression results used to generate this plot are reported in Table S3.1.

Extended Data Figure 2. Equity in organizations: The effect of pay transparency on gender wage gap.

Notes: The figure presents regression coefficients from an OLS regression model explaining (ln) wages. Reference category is 1 year prior to transparency shock. Plotted coefficients: dummy variable for Female interacted with years from (to) transparency shock with 95% CIs. Standard errors clustered on institution. Controls include academic tenure (ln), number of published academic articles, number of published books, number of awards, number of grants, and number of patents, and institution, individual, and year fixed effects. Regression results used to generate this plot are reported in Table S3.3.

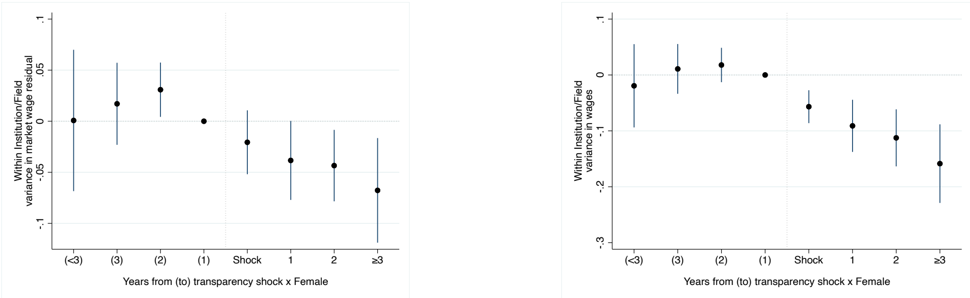
Extended Data Figure 3. Equity in organizations: The effect of pay transparency on gender wage gap: additional specifications.



Panel A — Panel B — Panel C — Panel D — Panel E — Panel F

Notes: The figure presents regression coefficients from an OLS regression model explaining (ln) wages. Reference category is 1 year prior to transparency shock (1 and 2 years for 2SLS results, panel C). Plotted coefficients: dummy variable for Female interacted with years from (to) transparency shock with 95% CIs. Standard errors clustered on institution. Controls include academic tenure (ln), number of published academic articles, number of published books, number of awards, number of grants, and number of patents, and institution, individual, and year fixed effects. Panel A – population restricted to 2004-2013 inclusive; Panel B – stacked difference in differences model (see text for more details). Panel C – 2SLS results, instrumented covariate: women's mean earnings as a % of men's in the private sector (see below for more details), excluded instrument: lead of transparency shock. Panel D and E – population restricted to CT, FL, PA, TX, VA (omitted states: California, New York, West Virginia). Panel F – full population. Regression results used to generate these plots are reported in Tables S3.3, S3.4, and S3.5.

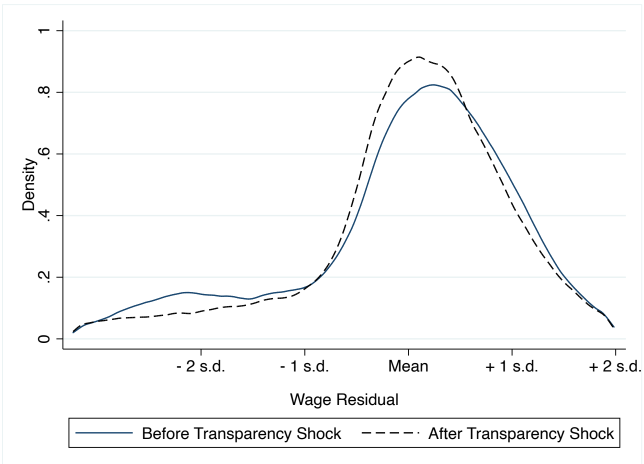
Extended Data Figure 4. Equity and equality in organizations: The effect of wage transparency on variance in market wage residuals and wage variance.



Left panel Right panel

Notes: The figures present regression coefficients from an OLS regression model explaining variance in market wage residuals (Left panel) and variance in (ln) wages (Right panel). Reference category is 1 year prior to transparency shock. Both variables are calculated within Institution-Academic Field (11 categories). Plotted coefficients: years from (to) transparency shock with 95% CIs. Standard errors clustered on institution. Controls include reference group mean productivity levels and reference group productivity variances as well as year and academic field-institution fixed effects. Regression results used to generate these plots are reported in Table S4.3.

Extended Data Figure 5. Equality in organizations: Distribution of market wage residuals, by transparency shock.



Notes: The figure presents kernel density estimates of wage regression residuals by transparency shocks. Controls include institution, academic domain, and year fixed effects. Means and standard deviations are calculated averaging across all time periods. Residuals trimmed at 1% and 99%. Two-sample combined Kolmogorov-Smirnov tests for equality of distribution functions: 0.041, p-value<0.001.

Extended Data Table 1. Conditional gender wage gap by state and main academic disciplines.

	Conditional Gender Wage Gap (%)	
State / Academic Field	2005	2015
California	13.4	1.1
Connecticut	19.3	7.6
Florida	8.0	5.8
New York	16.7	4.3
Pennsylvania	-1.4	3.0
Texas	12.8	5.9
Virginia	11.5	9.8
West Virginia	7.0	2.7
Humanities	9.7	2.4
Physical and Mathematical Sciences	13.6	4.4
Biological and Biomedical Sciences	18.3	4.2
Social and Behavioral Sciences	13.0	6.6
Engineering	7.0	6.4

Notes: The numbers presented in the table are based on regression estimates of conditional gender wage gap as specified in models reported in Extended Data Figure 1. Models are run separately for each state or academic discipline. Positive values indicate female faculty underpaid compared to men faculty.

Supplementary materials for: The influence of pay transparency on (gender) inequity, inequality, and the performance-basis of pay

S1

Table S1.1. Salary and employment data coverage by state.

State	First year in the data	Last year in the data
WV	2004	2017
VA	2003	2017
TX	1997	2017
PA	2003	2017
NY	2004	2016
FL	1997	2017
CT	2003	2017
CA	1998	2017

Productivity measures

For each individual, yearly productivity measures specify the cumulative output beginning in 2004 up to the year of analysis. We specify the following measures of academic output: number of academic articles in peer-reviewed journals, number of published books, number of academic awards, number (or value) of grants, and number of patents.
One of the crucial outcome variables in many disciplines is the number of publications in peer-reviewed academic journals. We only observe the number of such publications – a measure that could mask important quality heterogeneity. We therefore also collected data on all reported impact factors of journals in which articles in our sample were published. We then substitute the measure of a count of academic articles with JIF-weighted measures. We also constructed an impact measure captured with citations. However, we only observe the exact count of SSI citations from 2010 onwards. Based on this data, we imputed the number of citations in prior years imposing either a linear or exponential trend on the path of citation accumulation, starting with the year a PhD degree was obtained or a first academic publication is observed. However, given an extremely high ($\rho > 0.9$) correlation between the number of articles and citations, we cannot include both of these productivity measures in the same model. We report models with number of articles, as these do not rely on imputed values. Results including citations are qualitatively identical and available from the authors. In terms of awards, our measure includes major scientific awards such as the Fields Medal or the Nobel Prize but also field-specific and journal awards. The full list of awards and journals is available from the authors. We yearly winsorize all productivity outcomes at 1 and 99% to account for outliers. Finally, we measure academic tenure as (ln) number of years since graduation from a PhD program.

Descriptive statistics and academic disciplines

Table S1.2. Summary statistics.

Variable	Mean (mode) [s.d.] yearly	Mean (mode) [s.d.] cumulative
Salary (ln)	11.39 (11.46) [0.69]	
Academic tenure (ln)	2.68 (2.83) [0.88]	
Academic Articles	1.63 (0) [3.10]	8.88 (1) [19.62]
Patents	0.02 (0) [0.13]	0.09 (0) [0.58]
Books	0.07 (0) [0.32]	0.40 (0) [1.34]
Grants (#)	0.08 (0) [0.33]	0.54 (0) [1.49]
Awards	0.03 (0) [0.16]	0.15 (0) [0.50]

Table S1.3. Representation of academic domains in the data (11 categories).

Academic domain	% of observations in the data
Humanities	19.31
Physical and Mathematical Sciences	15.82
Biological and Biomedical Sciences	15.12
Social and Behavioral Sciences	14.12
Engineering	11.22
Family, Consumer, and Human Sciences	6.36
Business	5.54
Health Profession Sciences	5.15
Education	4.07
Agricultural Studies	1.90
Natural Resources and Conservation	1.39

Notes: We observe academic field for 59,616 individuals. For the remaining individuals, we specify the residual academic field category as "Other." We re-run all our analyses dropping these individuals from our data and find qualitatively identical results. Percentages presented in the table are calculated with the 'Other' category omitted.

S2

Context: Public University system wage transparency in the US

Although salary information of the employees in the public university system in the US has historically been partially a matter of public record since the Freedom of Information Act in 1967 and a series of subsequent Sunshine Acts, in practice such transparency laws were highly restrictive, imposed significant costs on individuals interested in obtaining data, and varied greatly from state to state and from institution to institution. For example, Mas (*10*) reports difficulties encountered by journalists in California trying to gather salary data for employees of the public institutions. Similarly, a journalist from the Michigan Capital Confidential, reports a large fee requested by one of the Michigan universities in return for compiling salary data (*S1*). Jan Murphy and the Patriot News Company's 2002 request for salaries of the PSU employees found its conclusion only five years later in the 2007 Supreme Court of Pennsylvania ruling in favor of releasing this information (PENNSYLVANIA STATE

UNIVERSITY v. Jan Murphy and the Patriot-News Company, Intervenors), although the earliest court decision that we could identify in favor of releasing individual salary information dates back to 1979 (Penokie v. Michigan Technological University). In soliciting and obtaining data for this paper, we also faced significant difficulties and heterogeneous policy interpretations in approval of our FOIA requests and access to historical wage data.

In the last decade, wage transparency in the public sector has been significantly facilitated by an emergence of searchable datasets developed and launched by newspapers, NGOs, and state agencies. Although in many cases it was technically possible to access some salary information prior to the launch of these public repositories, the individual-level costs were often prohibitive and, in many cases, entailed costly action. Therefore, following the launch of such aggregator websites, access to, and public discussion of salaries drastically increased. One indication of the intensity of these shocks can be seen from the web traffic generated by the databases. The Sacramento Bee's UC's salary database had over 6 million hits in the first two months since its launch (https://theaggie.org/2008/05/07/the-sacramento-bees-database-causes-upset/). Launching of this website has been used as a pay transparency shock by Card and colleagues (*22*). Similarly, just after its launch "the [Ohio salary] database was averaging about 300 searches a minute […], or a total of 200,000 searches in a day. Normally, it takes the organization about a month to log 200,000 data searches." (https://www.cleveland.com/metro/2011/08/ohio_treasurers_office_new_sal.html). The websites also generated a lot of discussion with newspaper headlines running titles like: "Texas Tribune's Public Employee Pay Database Taking Some Heat" (Dallas Magazine), "University profs: Scott posting of salaries part of 'attack'" (Herald-Tribune) and "Virginia wants to strip names from salary database" (Daily Press).

As discussed in the main body of the paper and as listed below, we limit our analyses to employees in eight states: California, Connecticut, Florida, New York, Pennsylvania, Texas, Virginia and West Virginia. While FOIL requests were filed with all relevant institutions in 50 states, due to data availability, legal structure or digitalization constraints, we restrict our analyses to these eight states. The exclusion of 42 states was driven by the following criteria. First, the 2013 Supreme Court of the United States' decision (McBurney v. Young, 569 U.S. 221) affirms the right of the States to limit FOIA application to state citizens. As non-citizens, we were denied data from some of the states. In several cases, we were denied access to information on the grounds that historical individual salary data are not public records. Second, for identification purposes, we excluded all states that could not provide us with data spanning at least 3 years prior to transparency shocks. Under FOIA laws, institutions are not obligated to create records that do not exist at the time of the request. Third, we excluded states in which data gathering would have been prohibitively costly. In several cases, institutions agreed to release the data but at a cost that exceeded possible budget. We also excluded states for which relevant obtained information was not available in a digital format and would have been excessively costly to digitize (e.g., paycheck scans with differing formats). Finally, some institutions did not respond to our requests or needed excessive delay time to procure the records. We also excluded institutions that were established less than 3 years prior to the transparency shocks. For each of the states in our sample, we gathered information about the formal launch of a first publicly accessible database as well as related press releases. Table S2.1 provides a summary of the shocks to pay transparency along with the associated releasing source. Importantly for our research design, these websites were launched in a staggered fashion between 2007 and 2012 across the eight states. Although the exact date of availability of salary data via these websites may have varied by institution, these public repositories had a dramatic state-wide effect on the transparency of pay and associated responses. Accordingly, they provide a natural set of shocks to pay transparency that we leverage in our empirical analyses.

Table S2.1. Transparency shocks by state.

State	Website or Newspaper	Launch Year
WV	Wvcheckbook.gov	2007
VA	Richmond Times-Dispatch	2010
TX	Texas Tribune	2009
PA	Pennwatch.pa.gov	2012
NY	Seethroughny.net	2008
FL	Floridahasarighttoknow.myflorida.com	2011
CT	Transparency.CT.gov	2010
CA	The Sacramento Bee	2008

S3

Table S3.1. The unconditional and conditional gender wage gap over time.

DV: ln(Wage)		(1)	(2)
		Unconditional Wage Gap	Conditional Wage Gap
Female		-0.231	-0.138
		(0.018)	(0.009)
Female X			
	1999	-0.018	
		(0.024)	
	2000	-0.025	
		(0.023)	
	2001	-0.016	
		(0.022)	
	2002	-0.003	
		(0.022)	
	2003	-0.016	
		(0.021)	
	2004	-0.035	
		(0.020)	
	2005	-0.022	0.008
		(0.020)	(0.013)
	2006	-0.010	0.017
		(0.019)	(0.012)
	2007	0.002	0.026
		(0.019)	(0.012)
	2008	0.011	0.040
		(0.019)	(0.012)
	2009	0.026	0.060
		(0.019)	(0.012)
	2010	0.023	0.061
		(0.019)	(0.012)
	2011	0.038	0.076

		(0.019)	(0.012)
	2012	0.029	0.068
		(0.019)	(0.011)
	2013	0.043	0.080
		(0.019)	(0.011)
	2014	0.058	0.082
		(0.019)	(0.011)
	2015	0.065	0.091
		(0.019)	(0.011)
	2016	0.073	0.096
		(0.019)	(0.011)
	2017	0.084	0.112
		(0.019)	(0.011)
Institution fixed effects		no	yes
Academic field fixed effects		no	yes
Year fixed effects		yes	yes
Productivity controls		no	yes
Observations		566,242	306,404

Notes: The table reports OLS regression estimates explaining (ln) salaries. Productivity controls include academic tenure (ln), number of published academic articles, number of published books, number of awards, number of grants, and number of patents. Standard errors clustered at the level of institution in parentheses.

Table S3.2. The effect of pay transparency on gender wage gap.

DV: ln(Wage)	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	0.060	0.034	0.006	-0.009	-0.009	-0.015
	(0.012)	(0.011)	(0.011)	(0.009)	(0.008)	(0.008)
Female		-0.211	-0.112	absorbed	-0.062	absorbed
		(0.018)	(0.009)		(0.005)	
Treatment # Female		0.067	0.059	0.031	0.020	0.021
		(0.011)	(0.008)	(0.004)	(0.005)	(0.004)
Associate Professor					0.121	0.064
					(0.008)	(0.007)
Full Professor					0.391	0.173
					(0.012)	(0.014)
Productivity controls	no	no	yes	yes	yes	yes
Individual fixed effects	no	no	no	yes	no	yes
Academic field fixed effects	yes	yes	yes	absorbed	yes	absorbed
Institution fixed effects	yes	yes	yes	yes	yes	yes
Year fixed effects	yes	yes	yes	yes	yes	yes
Observations	676,055	556,242	306,404	300,853	195,976	194,077

Notes: The table presents OLS regression estimates explaining (ln) salaries. Productivity controls include academic tenure (ln), number of academic articles, number of books, number of awards, number of grants, and number of patents. In models 5-6 omitted category is Assistant Professor. Standard errors clustered at the level of institution in parentheses.

To calculate state-level private sector wage gap used in estimating model reported in Panel C, Figure S3.3, we use the Annual Social Economic Supplement of the IPUMS CPS (Current Population Survey) microdata (IPUMS-CPS, www.ipums.org). This data results from a joint effort between the Bureau of Labor Statistics and the Census Bureau, and provides the official US Government statistics on employment and wages. CPS ASEC reports stratified random samples where the U.S. is divided into 1,987 "primary sampling units" (PSUs), and these PSUs are grouped into homogenous strata within each state. Within the selected PSUs, a block of households is sampled and asked work-related questions for each member of the household 15 years of age or older as of a particular calendar week. In the supplementary dataset that we create for 2004-2017 period, there are 904,355 individual-year observations in the 8 states we examine. We exclude all those working in industries with its Census code greater than or equal to 9370 (thereby focusing only on private sectors) and those who reported to be unemployed, retired, or unable to work. After this adjustment, we are left with a working sample of 604,998 individual-year observations. To calculate the unconditional gender wage gap in the private sector, we look at the relative median income of men and female workers, by year and state. Our results are insensitive to using mean values.

Table S3.3. Equity in organizations: The effect of pay transparency on gender wage gap.

	β, t_{df}, (s.e.), p-value, [95% CI]	
DV: ln(Wage)	(1)	(2)
	OLS	IV
Years from (to) transparency shock X Female		
(<3)	-0.016, t_{138}=2.91 (0.005), 0.004 [-0.03;-0.01]	-0.014, t_{138}=2.67 (0.005), 0.009 [-0.02;-0.00]
(3)	-0.001, t_{138}=0.19 (0.005), 0.849 [-0.01;0.01]	0.001, t_{138}=0.25 (0.004), 0.800 [-0.01;0.01]
(2)	-0.005, t_{138}=0.85 (0.005), 0.396 [-0.02;0.01]	Omitted
(1)	Omitted	Omitted
Shock	0.009, t_{138}=1.93 (0.004), 0.056 [0.00;0.02]	0.011, t_{138}=2.64 (0.004), 0.009 [0.00;0.02]
1	0.022, t_{138}=2.06 (0.011), 0.041 [0.00;0.04]	0.025, t_{138}=2.31 (0.011), 0.022 [0.00;0.05]
2	0.017, t_{138}=3.53 (0.005), 0.001 [0.01;0.03]	0.020, t_{138}=3.44 (0.006), 0.001 [0.01;0.03]
3	0.020, t_{138}=3.78 (0.005), <0.001 [0.01;0.03]	0.022, t_{138}=4.80 (0.005), <0.001 [0.01;0.03]
>3	0.035, t_{138}=5.78 (0.006), <0.001 [0.02;0.05]	0.037, t_{138}=6.56 (0.006), <0.001 [0.03;0.05]
Productivity controls	yes	yes
Individual fixed effects	yes	yes
Institution fixed effects	yes	yes
Year fixed effects	yes	yes
Observations	300,853	300,853

Notes: The table presents OLS (model 1) and 2SLS (model 2) regression estimates explaining (ln) salaries. Productivity controls include academic tenure (ln), number of academic articles, number of books, number of awards, number of grants, and number of patents. Standard errors clustered on institution in parentheses. In model 2, women's mean earnings as a % of men's in the private sector is the instrumented covariate and 1-year lead of transparency shock an excluded instrument.

Table S3.4. Equity in organizations: The effect of pay transparency on gender wage gap. Stacked specification.

DV: ln(Wage)	
	β, t_{df}, (s.e.), p-value, [95% CI]
Years from (to) transparency shock X Female	
(3)	-0.007, t_{135}=0.71 (0.009), 0.477 [-0.03;0.01]
(2)	-0.003, t_{135}=0.41 (0.008), 0.680 [-0.02;0.01]
(1)	Omitted
Shock	0.016, t_{135}=2.24 (0.007), 0.027 [0.00;0.03]
1	0.026, t_{135}=1.79 (0.015), 0.075 [-0.00;0.06]
2	0.017, t_{135}=2.24 (0.008), 0.027 [0.00;0.03]
Productivity controls	yes
Individual fixed effects	yes
Institution fixed effects	yes
Cohort fixed effects	yes
Year fixed effects	yes
Observations	286,550

Notes: The table presents OLS regression estimates explaining (ln) salaries. Productivity controls include academic tenure (ln), number of academic articles, number of books, number of awards, number of grants, and number of patents. Standard errors clustered on institution in parentheses. See text for more details on the stacking procedure.

Table S3.5. Equity in organizations: The effect of pay transparency on gender wage gap. Restricted sample.

DV: ln(Wage)	
	β, t_{df}, (s.e.), p-value, [95% CI]
Years from (to) transparency shock X Female	
(5)	-0.005, t_{69}=0.99 (0.006), 0.327 [-0.02;0.01]
(4)	-0.015, t_{69}=1.74 (0.009), 0.086 [-0.03;0.00]
(3)	0.001, t_{69}=0.15 (0.006), 0.884 [-0.01;0.01]
(2)	-0.008, t_{69}=1.59 (0.005), 0.117

	[-0.02;0.00]
(1)	Omitted
Shock	0.008, t_{69}=1.54 (0.005), 0.128 [-0.00;0.02]
1	0.030, t_{69}=1.60 (0.018), 0.114 [-0.01;0.07]
2	0.014, t_{69}=2.25 (0.006), 0.028 [0.00;0.03]
3	0.024, t_{69}=3.73 (0.006), <0.001 [0.01;0.04]
4	0.022, t_{69}=3.38 (0.007), 0.001 [0.01;0.04]
Productivity controls	yes
Individual fixed effects	yes
Institution fixed effects	yes
Cohort fixed effects	yes
Year fixed effects	yes
Observations	166,587

Notes: The table presents OLS regression estimates explaining (ln) salaries. Productivity controls include academic tenure (ln), number of academic articles, number of books, number of awards, number of grants, and number of patents. Standard errors clustered on institution in parentheses. Population restricted to academics employed in institutions located in the following states: CT, FL, PA, TX, VA.

Table S3.6. The effect of wage transparency on gender pay gap: Mobility restricted sample.

DV: ln(Wage)	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	0.042	0.022	0.010	0.001	-0.002	-0.008
	(0.009)	(0.009)	(0.010)	(0.010)	(0.009)	(0.008)
Female		-0.200	-0.104	absorbed	-0.061	absorbed
		(0.019)	(0.008)		(0.005)	
Treatment # Female		0.064	0.045	0.030	0.022	0.020
		(0.010)	(0.006)	(0.004)	(0.005)	(0.004)
Associate Professor					0.104	0.067
					(0.010)	(0.007)
Full Professor					0.364	0.179
					(0.013)	(0.014)
Productivity controls	no	no	yes	yes	yes	yes
Individual fixed effects	no	no	no	yes	no	yes
Academic field fixed effects	yes	yes	yes	absorbed	yes	absorbed
Institution fixed effects	yes	yes	yes	yes	yes	yes
Year fixed effects	yes	yes	yes	yes	yes	yes
Observations	375,865	316,040	212,032	211,682	153,396	153,003

Notes: The table presents OLS regression estimates explaining (ln) salaries. Productivity controls include academic tenure (ln), number of academic articles, number of books, number of awards, number of grants, and number of patents. In models 5-6 omitted category is Assistant Professor. Sample is restricted by dropping all individuals who have: 1) changed institutions within our observation window but stayed in our working sample, (2) left our observation sample between the time of the transparency shock and 2017, or (3) joined our sample posterior to the transparency shock. Standard errors clustered at the level of institution in parentheses.

Table S3.7. The effect of wage transparency on gender pay gap: Time restricted sample.

DV: ln(Wage)	(1)	(2)	(3)	(4)
Treatment	0.058	-0.010	0.092	0.005
	(0.016)	(0.009)	(0.025)	(0.013)
Female	-0.214	absorbed	-0.217	absorbed
	(0.018)		(0.018)	
Treatment # Female	0.050	0.023	0.050	0.025
	(0.011)	(0.003)	(0.011)	(0.006)
	Year restricted to < 2014		Year restricted to < 2012	
Productivity controls	no	yes	no	yes
Individual fixed effects	no	yes	no	yes
Academic field fixed effects	yes	absorbed	yes	absorbed
Institution fixed effects	yes	yes	yes	yes
Year fixed effects	yes	yes	yes	yes
Observations	418,096	195,520	340,131	146,307

Notes: The table presents OLS regression estimates explaining (ln) salaries. Productivity controls include academic tenure (ln), number of academic articles, number of books, number of awards, number of grants, and number of patents. Standard errors clustered at the level of institution in parentheses.

S4

Table S4.1. The effect of pay transparency on institution-academic field variance in market wage residuals.

DV: Variance in residual from market wage regression	(1)	(2)	(3)	(4)
Within:	Institution – academic field (11 categories)		Institution –academic field (25 categories)	
Treatment	-0.034	-0.034	-0.019	-0.019
	(0.015)	(0.014)	(0.009)	(0.009)
Controls for mean productivity and variance in productivity	no	yes	no	yes
Institution-academic field fixed effects	yes	yes	yes	yes
Year fixed effects	yes	yes	yes	yes
Observations	9,015	9,015	14,171	14,171

Notes: The table presents OLS regression estimates explaining institution-academic field variance in residual from market wage regressions. Wage regressions controls include academic tenure (ln), number of academic articles, number of published books, number of awards, number of grants, and number of patents and institution, academic domain, and year fixed effects. Residuals trimmed at 1% and 99%. Standard errors clustered at the level of institution in parentheses. In models 2 and 4 we include controls for average institution-academic field cumulative productivity outcomes and variance of these productivity outcomes.

Table S4.2. The effect of pay transparency on institution-academic field variance in (ln) wages.

DV: Wage variance	(1)	(2)	(3)	(4)
Within:	Institution – academic field (11 categories)		Institution –academic field (25 categories)	
Treatment	-0.065	-0.068	-0.048	-0.055
	(0.015)	(0.016)	(0.014)	(0.014)
Controls for mean productivity and variance in productivity	no	yes	no	yes
Institution-academic field fixed effects	yes	yes	yes	yes
Year fixed effects	yes	yes	yes	yes
Observations	12,892	9,133	17,173	14,360

Notes: The table presents OLS regression estimates explaining variance in salaries. Standard errors clustered at the level of institution in parentheses. In models 2 and 4 we include controls for average institution-academic field cumulative productivity outcomes and variance of these productivity outcomes.

Table S4.3. Equity and equality in organizations: The effect of wage transparency on variance in market wage residuals and wage variance.

	β, t_{df}, (s.e.), p-value, [95% CI]	
DV:	Variance in market wage residuals	Variance in (ln) wages
Within :	Institution – academic field (11 categories)	
Years from (to) transparency shock :		
(<3)	0.001, t_{130}=0.02 (0.035), 0.983 [-0.07;0.07]	-0.019, t_{131}=0.51 (0.038), 0.608 [-0.09;0.05]
(3)	0.017, t_{130}=0.84 (0.020), 0.402 [-0.02;0.06]	0.011, t_{131}=0.48 (0.022), 0.631 [-0.03;0.06]
(2)	0.031, t_{130}=2.30 (0.013), 0.023 [0.00;0.06]	0.018, t_{131}=1.15 (0.016), 0.252 [-0.01;0.05]
(1)	Omitted	Omitted
Shock	-0.021, t_{130}=1.30 (0.016), 0.195 [-0.05;0.01]	-0.057, t_{131}=3.81 (0.015), <0.001 [-0.09;-0.03]
1	-0.038, t_{130}=1.96 (0.016), 0.052 [-0.08;0.00]	-0.091, t_{131}=3.86 (0.024), <0.001 [-0.14;-0.04]
2	-0.043, t_{130}=2.46 (0.018), 0.015 [-0.08;-0.01]	-0.112, t_{131}=4.36 (0.026), <0.001 [-0.16;-0.06]
≥3	-0.068, t_{130}=2.61 (0.026), 0.010 [-0.12;-0.02]	-0.159, t_{131}=4.46 (0.036), <0.001 [-0.23;-0.09]
Institution-academic field fixed effects	Yes	Yes
Year fixed effects	Yes	Yes
Controls for mean productivity and variance in productivity	Yes	Yes
Observations	9,015	9,060

Notes: The table present OLS regression estimates explaining variance in market wage residuals (model 1) and variance in (ln) wages (model 2). Reference category is 1 year prior to transparency shock. Both variables are calculated within Institution-Academic Field (11 categories). Standard errors clustered on institution in parentheses. Controls include reference group mean productivity levels and reference group productivity variances.

S5

Table S5.1. The effect of wage transparency on the determinants of pay.

DV: ln(Wage)	(1)	(2)	(3)	(4)	(5)
Treatment		0.0581 (0.0159)	0.0553 (0.0159)		0.0419 (0.0097)
Academic tenure (ln)	0.1492 (0.0144)	0.1405 (0.0148)	0.1407 (0.0149)		
Academic tenure (ln) # treatment		-0.0156 (0.0058)	-0.0168 (0.0058)		
Academic Articles	0.0010 (0.0001)	0.0022 (0.0004)	0.0005 (0.0001)		
Academic Articles # treatment		-0.0010 (0.0003)	-0.0003 (0.0001)		
Patents	-0.0032 (0.0023)	0.0071 (0.0047)	0.0134 (0.0052)		

Patents # treatment		-0.0087	-0.0116		
		(0.0042)	(0.0046)		
Books	0.0046	0.0110	0.0119		
	(0.0013)	(0.0023)	(0.0023)		
Books # treatment		-0.0057	-0.0063		
		(0.0021)	(0.0021)		
Grants (#)	0.0139	0.0201	0.0246		
	(0.0016)	(0.0025)	(0.0028)		
Grants (#) # treatment		-0.0059	-0.0068		
		(0.0022)	(0.0025)		
Awards	0.0092	0.0024	0.0064		
	(0.0029)	(0.0042)	(0.0043)		
Awards # treatment		0.0073	0.0062		
		(0.0042)	(0.0042)		
Associate Professor				0.1147	0.1352
				(0.0069)	(0.0069)
Associate Professor # treatment					-0.0592
					(0.0079)
Full Professor				0.2478	0.2768
				(0.0125)	(0.0142)
Full Professor # treatment					-0.0690
					(0.0129)
Individual fixed effects	yes	yes	yes	yes	yes
Institution fixed effects	yes	yes	yes	yes	yes
Year fixed effects	yes	yes	yes	yes	yes
Observations	364,301	364,301	364,301	333,828	333,828

Notes: The table presents OLS regression estimates explaining (ln) salaries. In model 3, we substitute a measure of count of the number of academic articles with Journal Impact Factor-weighted measure of academic output. Standard errors clustered at the level of institution.

Table S5.2. The effect of wage transparency on the determinants of pay: Mobility restricted sample.

DV: ln(Wage)	(1)	(2)	(3)	(4)
Treatment		0.1102		0.0554
		(0.0133)		(0.0104)
Academic tenure (ln)	0.1556	0.1309		
	(0.0131)	(0.0126)		
Academic tenure (ln) # treatment		-0.0306		
		(0.0049)		
Academic Articles	0.0010	0.0023		
	(0.0001)	(0.0004)		
Academic Articles # treatment		-0.0011		
		(0.0003)		
Patents	-0.0047	0.0070		
	(0.0026)	(0.0056)		
Patents # treatment		-0.0099		
		(0.0048)		
Books	0.0058	0.0112		
	(0.0015)	(0.0022)		
Books # treatment		-0.0047		
		(0.0019)		
Grants (#)	0.0146	0.0208		
	(0.0017)	(0.0022)		
Grants (#) # treatment		-0.0058		
		(0.0020)		
Awards	0.0113	0.0036		
	(0.0036)	(0.0042)		
Awards # treatment		0.0082		
		(0.0047)		
Associate Professor			0.1087	0.1359
			(0.0075)	(0.0071)
Associate Professor # treatment				-0.0699
				(0.0086)
Full Professor			0.2366	0.2616
			(0.0139)	(0.0157)

Full Professor # treatment				-0.0614 (0.0120)
Individual fixed effects	yes	yes	yes	yes
Institution fixed effects	yes	yes	yes	yes
Year fixed effects	yes	yes	yes	yes
Observations	254,653	254,653	221,262	221,262

Notes: The table presents OLS regression estimates explaining (ln) salaries. Specifications as in Table S5.1. Sample is restricted by dropping all individuals who have: 1) changed institutions within our observation window but stayed in our working sample, (2) left our observation sample between the time of the transparency shock and 2017, or (3) joined our sample posterior to the transparency shock. Standard errors clustered at the level of institution in parentheses.

Table S5.3a. The effect of wage transparency on the determinants of pay, by discipline.

DV: ln(Wage)	Population	Humanities	Physical and Mathematical Sciences	Biological and Biomedical Sciences	Social and Behavioral Sciences	Engineering
Academic Articles	0.002	0.003	0.002	0.003	0.003	0.002
	(0.000)	(0.001)	(0.000)	(0.001)	(0.001)	(0.000)
Academic Articles # treatment	-0.001	-0.000	-0.001	-0.002	-0.001	-0.001
	(0.000)	(0.001)	(0.000)	(0.001)	(0.001)	(0.000)
Patents	0.007	-0.144	0.026	-0.003	0.001	0.014
	(0.005)	(0.257)	(0.007)	(0.008)	(0.083)	(0.005)
Patents # treatment	-0.009	0.205	-0.023	-0.002	-0.021	-0.005
	(0.004)	(0.250)	(0.008)	(0.005)	(0.059)	(0.005)
Books	0.011	0.017	0.003	-0.006	0.009	-0.001
	(0.002)	(0.003)	(0.006)	(0.008)	(0.003)	(0.005)
Books # treatment	-0.006	-0.007	-0.007	0.000	-0.006	-0.003
	(0.002)	(0.003)	(0.004)	(0.006)	(0.003)	(0.003)
Grants (#)	0.020	0.013	0.029	0.018	0.031	0.012
	(0.002)	(0.012)	(0.005)	(0.003)	(0.007)	(0.004)
Grants (#) # treatment	-0.006	0.005	-0.008	-0.004	-0.005	0.002
	(0.002)	(0.009)	(0.004)	(0.003)	(0.004)	(0.003)
Awards	0.002	-0.017	0.010	0.008	0.005	0.005
	(0.004)	(0.016)	(0.009)	(0.010)	(0.012)	(0.008)
Awards # treatment	0.007	0.030	0.007	-0.005	0.000	0.002
	(0.004)	(0.017)	(0.009)	(0.009)	(0.011)	(0.007)
Individual fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Institution fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Additional controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	364,301	71,253	58,015	56,476	51,598	41,124

Notes: The table presents OLS regression estimates explaining (ln) salaries. Standard errors clustered at the level of institution in parentheses.

Table S5.3b. The effect of wage transparency on the determinants of pay, by discipline.

DV: ln(Wage)						
	Population	Humanities	Physical and Mathematical Sciences	Biological and Biomedical Sciences	Social and Behavioral Sciences	Engineering
Associate Professor	0.135	0.119	0.108	0.135	0.134	0.104
	(0.007)	(0.011)	(0.010)	(0.012)	(0.012)	(0.011)
Associate Professor # treatment	-0.059	-0.049	-0.070	-0.074	-0.047	-0.065
	(0.008)	(0.009)	(0.014)	(0.012)	(0.016)	(0.014)
Full Professor	0.277	0.245	0.204	0.267	0.263	0.171
	(0.014)	(0.019)	(0.023)	(0.022)	(0.019)	(0.018)
Full Professor # treatment	-0.069	-0.037	-0.049	-0.074	-0.049	-0.043
	(0.013)	(0.012)	(0.019)	(0.015)	(0.017)	(0.021)
Individual fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Institution fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Additional controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	333,828	49,292	38,149	33,501	33,715	27,967

Notes: The table presents OLS regression estimates explaining (ln) salaries. Standard errors clustered at the level of institution in parentheses. Assistant Professor is the omitted baseline category.

Table S5.4. The effect of wage transparency on salary adjustments associated with promotions.

	β, t_{df}, (s.e.), p-value, [95% CI]	
DV: ln(Wage)		
Years from (to) transparency shock X :	Associate Professor	Full Professor
(<3)	0.027, t_{89}=3.03 (0.009), 0.003 [0.01;0.04]	0.017, t_{89}=1.13 (0.015), 0.261 [-0.01;0.05]
(3)	0.020, t_{89}=2.89 (0.007), 0.005 [0.01;0.03]	0.012, t_{89}=1.37 (0.009), 0.173 [-0.01;0.03]
(2)	0.010, t_{89}=2.00 (0.005), 0.048 [0.00;0.02]	-0.001, t_{89}=0.24 (0.006), 0.814 [-0.01;0.01]
(1)	Omitted	Omitted
Shock	-0.028, t_{89}=5.27 (0.005), <0.001 [-0.04;-0.02]	-0.037, t_{89}=5.45 (0.007), <0.001 [-0.05;-0.02]
1	-0.042, t_{89}=4.99 (0.009), <0.001 [-0.06;-0.03]	-0.069, t_{89}=6.09 (0.011), <0.001 [-0.09;-0.05]
2	-0.032, t_{89}=4.97 (0.007), <0.001 [-0.05;-0.02]	-0.059, t_{89}=6.18 (0.009), <0.001 [-0.08;-0.04]
3	-0.039, t_{89}=5.04 (0.008), <0.001 [-0.05;-0.02]	-0.056, t_{89}=5.88 (0.010), <0.001 [-0.08;-0.04]
≥3	-0.078, t_{89}=6.92 (0.011), <0.001 [-0.10;-0.06]	-0.095, t_{89}=5.29 (0.018), <0.001 [-0.13;-0.06]
Institution, academic field fixed effects	Yes	
Year fixed effects	Yes	
Observations	333,828	

The table reports OLS regression estimates explaining (ln) salaries. Standard errors clustered at the level of institution in parentheses. Not reported but included in the regressions are uninteracted dummy variables for years to(from) transparency shocks and indicators for Associated and Full professors. Assistant Professor is the omitted baseline category.

Table S5.5. The effect of market wage and pay transparency on yearly wage increases.

DV: % Wage change					
	Underpaid and overpaid: continuous specification		Underpaid and overpaid: binary specification		
	(1)	(2)	(3)	(4)	(5)
Treatment		-0.013		-0.157	-0.347
		(0.420)		(0.413)	(0.398)
Underpaid	2.857	2.329	1.372	1.028	0.766
	(0.466)	(0.501)	(0.155)	(0.221)	(0.173)
Underpaid # treatment		0.831		0.531	0.516
		(0.368)		(0.193)	(0.163)
Overpaid	-5.551	-5.193	-1.568	-1.582	-1.452
	(0.355)	(0.583)	(0.103)	(0.161)	(0.138)
Overpaid # treatment		-0.574		0.023	0.161
		(0.574)		(0.135)	(0.125)
Low salary					0.528
					(0.240)
Low salary # treatment					0.282
					(0.219)
Academic field fixed effects	Yes	Yes	Yes	Yes	Yes
Institution fixed effects	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes
Observations	261,100	261,100	259,624	259,624	259,624

Notes: $DV = \frac{Wage_{i,t+1} - Wage_{i,t}}{Wage_{i,t+1}} \times 100$. In the continuous specification, underpaid is defined as the absolute value of the individual *i*'s residual from regression predicting market wage in year *t* if the residual is negative and 0 otherwise. Overpaid is defined as the value of individual *i*'s residual from regression predicting market wage in year *t* if the residual is positive and 0 otherwise. In the binary specification underpaid is equal to 1 if individual *i*'s residual from regression predicting market wage in year *t* is negative and smaller than the average residual from the same regression for all individuals in a given year, the same institution and the same academic domain. Overpaid is equal to 1 if individual *i*'s residual from regression predicting market wage in year *t* is positive and greater than the average residual from the same regression for all individuals in a given year, the same institution and the same academic domain . *Low salary* is equal to 1 if individual *i*'s salary is below average, compared to year-institution-domain peers and 0 otherwise. Standard errors clustered at the level of institution in parentheses.

S6

Robustness tests: Exclusion of California and Texas.

Although our data spans eight states, the largest population of academics in our sample works in institutions located in California and Texas. Therefore one may be concerned that our results are driven by one of these states rather than represent a general pattern in the sample. Below we report results presented in Tables 1 and 2 excluding these states from our analyses.

Table S6.1. Robustness of the effects of wage transparency on gender pay gap to exclusion of states.

DV: ln(Wage)	(1)	(2)	(3)	(4)
Treatment	0.026	0.018	0.005	-0.007
	(0.014)	(0.014)	(0.012)	(0.009)
Female	-0.115	-0.103	absorbed	absorbed
	(0.010)	(0.012)		
Treatment # Female	0.057	0.050	0.027	0.024
	(0.009)	(0.010)	(0.005)	(0.005)
Productivity controls	yes	yes	yes	yes
Individual fixed effects	no	no	yes	yes
Academic field fixed effects	yes	yes	absorbed	absorbed
Institution fixed effects	yes	yes	yes	yes
Year fixed effects	yes	yes	yes	yes
Omitted State	CA	TX	CA	TX
Observations	212,143	207,224	208,218	202,866

Notes: The table presents OLS regression estimates explaining (ln) salaries. Standard errors clustered at the level of institution in parentheses.

Table S6.2. Robustness of the effects of wage transparency on institution-academic field variance in market wage residuals to exclusion of states.

DV: Variance in residual from market wage regression	(1)	(2)	(3)	(4)
Within:	Institution – academic field (11 categories)		Institution –academic field (25 categories)	
Treatment	-0.033	-0.051	-0.015	-0.026
	(0.016)	(0.020)	(0.011)	(0.012)
Controls for mean productivity and variance in productivity	yes	yes	yes	yes
Institution-academic field fixed effects	yes	yes	yes	yes
Year fixed effects	yes	yes	yes	yes
Omitted state	CA	TX	CA	TX
Observations	7,482	5,562	11,220	8,640

Notes: The table presents OLS regression estimates explaining variance in market wage residuals. Standard errors clustered at the level of institution in parentheses.

Table S6.3. Robustness of the effects of wage transparency on pay variance to exclusion of states.

DV: Pay variance	(1)	(2)	(3)	(4)
Within:	Institution – academic field (11 categories)		Institution –academic field (25 categories)	
Treatment	-0.056 (0.018)	-0.087 (0.022)	-0.037 (0.015)	-0.076 (0.019)
Controls for mean productivity and variance in productivity	yes	yes	yes	yes
Institution-academic field fixed effects	yes	yes	yes	yes
Year fixed effects	yes	yes	yes	yes
Omitted state	CA	TX	CA	TX
Observations	7,591	5,668	11,387	8,786

Notes: The table presents OLS regression estimates explaining variance in salaries. Standard errors clustered at the level of institution in parentheses.

All standard errors reported in the paper are based on clustering at the institution level. Available from the authors are results based on clustering at the state-year and state level. Our results are robust to these alternative specifications. When clustering at the state level was used, due to few clusters, we performed wild bootstrap-based tests for testing hypotheses about the coefficients (Cameron et al., 2008; Djogbenou et al., 2019).

Additional references:

S1. J. Gantert, NMU Wants $4,600 to Look Up and Send Salary Data. Michigan Capitol Confidential August 8th, 2018. Available at: https://www.michigancapitolconfidential.com/nmu-wants-4600-to-look-up-and-send-salary-data. Accessed October 30th 2019.

S6. Cameron, A. Colin, Jonah B. Gelbach, and Douglas L. Miller. "Bootstrap-based improvements for inference with clustered errors." *The Review of Economics and Statistics* 90.3 (2008): 414-427.
Djogbenou, Antoine A., James G. MacKinnon, and Morten Ørregaard Nielsen. "Asymptotic theory and wild bootstrap inference with clustered errors." *Journal of Econometrics* 212.2 (2019): 393-412.