**Title:** The influence of pay transparency on 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 novel 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.

**One Sentence Summary:** Our results suggest pay transparency has a significant and economically sizeable effect in reducing pay inequality and inequity 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, effort, or job. 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 increased 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. Accordingly, many nations, states, and organizations have taken directional steps to heed this call *(9)*. 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 *(10).* 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 their efforts to effectively motivate and attract talent. But to date the empirical effects of pay transparency remain largely untested. While some empirical work links pay transparency to greater pay equality, there has been no systematic examination of how pay transparency broadly reshapes pay allocation *(11-14)*.

To explore pay transparency’s broader influence on pay equity, pay equality, and the performance-basis of pay, we assemble a novel data set that combines 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, 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 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. If pay is both more equal and equitable, a corollary is that it is also less performance-based. We find evidence of precisely this: pay transparency leads to significant and economically sizeable reductions in the performance basis of pay. The financial rewards linked to observable performance metrics as well as rank advancement significantly decline after wages become 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 a first empirical test of the causal effect of pay transparency on 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 consequences of further efforts to elevate pay transparency.

**CONTEXT AND DATA**

A test of the broad impact of pay transparency on pay systems, including pay equity, pay equality, and the performance basis of 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, 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.

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 pre and post transparency, 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 *(15).* 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 *(16).* Our final merged dataset spans 682,884 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). Neither database provided gender information, and we therefore coded gender based on several publicly available first-name dictionaries. When the first name did not identify gender with rather high levels of confidence (e.g., Aguilar, Alexis, Casey, Kyle, Ming), we excluded these individuals from those analyses that investigate gender effects. 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 and 338,285 individual-year observations 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.

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.

**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 *(17-20).* 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 *(21)*. **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 Figure 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 9.2% less than her male counterpart in 2010. While there is important heterogeneity across states and academic disciplines (see Table S3.1), 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 has narrowed but remains significant at 3.2% *(22)*.

This decreasing level of inequity can be partially explained by shocks to accessibility of information on salaries. To explore if 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* ():

where *k* corresponds to the number of lags (set in the dynamic model to the reference category of more than five years prior to transparency shock, and then includes all subsequent lags) and leads (set in the reported results to the year following the shock, four subsequent short-term leads and one long-term coefficient) 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. 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 *(23)* implemented in the regdhfe STATA (version 16) package.

The coefficients of initial interest are 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 *(24)* 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 as we do not have a sufficient number of states to cluster at the treatment level. In all tables, we also provide values of standard errors clustered at the state-year level. For comparison purposes we also report results with and without individual fixed effects. 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 gender pay gap closing by a range of 2 to 6.9 percentage points.

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

In Figure S3.2 (left panel) we plot coefficients from a dynamic specification. A visual inspection indicates a noticeable increase in relative wages of women academics compared to men academics in the years following the transparency shocks. However, the estimated coefficients also indicate that the relative wages of women academics have been slightly higher in the three years preceding the shock in comparison with the baseline time of more than five years. These differences, though not statistically significant, could indicate some anticipation of the transparency by organizational designers, or simply a sustained interest in reducing the gap. Key organizational decision makers were most likely aware of the past and upcoming website launches across the country and could have acted to reduce inequity and inequality in a pre-emptive manner. Indeed, expecting public scrutiny associated with more transparent inequitable wages *(12*) would have made anticipatory changes in the direction of the treatment effects likely. Consistent with this assertion, we observe that the anticipatory trend is less pronounced if we exclude the four states that experienced the latest transparency shocks, and analyze only the four states with early shocks: California, New York, Texas, and West Virginia (Figure S3.2, right panel). This suggests that our estimates of the average treatment effects on the treated presented in Table 1 are likely conservative compared to results we would obtain if these wage transparency events were fully unexpected.

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 ‘fair’ wage estimation). Our models explain about 84% of variance in wages. Figure 1 shows kernel density plots of these residuals from market wage regressions for female and male academics prior and posterior to transparency shocks. Actual, observed wages below this estimated wage, are considered inequitable underpayment, while observed wages above this estimated 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 *(25).*

----- 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. In the supplementary analyses, we also investigate the extent to which the overall equity, measured by sample deviations from market wages was affected by the transparency shocks. We observe that the pattern reported above applies more generally. Following transparency shocks, we observe a greater density of wages closer to the estimated market wage. As evident from Figure S3.3, this narrowing of the distribution of residuals is driven both by decreased masses of inequitably underpaid as well as inequitably overpaid. Taken together our results indicate that eased access to wage information resulted in decreasing various forms of wage inequity within academia.

**WAGE TRANSPARENCY AND PAY EQUALITY**

Unlike equity, equality is an absolute construct *(4, 26)*, 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 relatively proximate peers, independent of their performance. We define proximate peers or peer groups based on common institutional and field affiliation, and use more and less fine-grained definitions of academic fields. As explained above, we distinguish equality from equity by making equity contingent on performance outcomes and equality independent of these individual performance differences.

Table 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. 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. Across reference groups we find that wage transparency decreased pay dispersion by nearly 20%.

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

In Figure S4.1 we plot estimated coefficients from dynamic specifications corresponding to models 2 and 4 of Table 2. The visual inspection of the charts corroborates the role of transparency shocks in decreasing average wage variance of academics in our data and lack of strong pre-trends.

To further explore the mechanism behind pay transparency’s impact on wage equality we, again, turn to distributional plots. Figure 3 reports a kernel density plot similar to those presented earlier (see Figure 1). This time however, we focus on wage equality and hence report 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 the plot indicates 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.

----- Insert Figure 3 about here -----

**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, and thereby render pay more equal. 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 difference-in-differences 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:

where corresponds to individual *i*’sproductivity 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 indicate the average strength of the link between pay-and-performance while coefficients indicate how a transparency shock affects this average link between pay-and-performance. Negative coefficients would indicate weakening of the marginal returns while positive 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 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. In order to ensure that our results do not simply reflect an ongoing pre-trend, in Figure 3 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. There is also little evidence that the results reported in Tables S5.1 and 3 are driven by an ongoing pre-trend in the data.

----- Insert Figure 3 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, pre-transparency, an average academic with star levels of performance across all metrics could expect to see a 17.7% greater salary than an academic with no, observable to us, output. However, post pay transparency shock, we observe a large – 37% – 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, average pay increases pre transparency for associate and full professors – compared with assistant professors – were 15% and 32% respectively. Post pay transparency, these premiums fell to 8% and 23% 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, in all five disciplines the financial rewards associated with promotion across academic ranks fell markedly in response to pay transparency.

To explore a second mechanism, we examine on a more micro level, precisely how institutions reshape pay post pay transparency. Conceivably institutions may adjust downward the pay of those inequitably overpaid or adjust upward the pay of those underpaid, though downward pay adjustments are generally considered quite socially unacceptable. To explore the pattern of adjustments, we compute the extent to which each individual’s salary in our sample deviates in any given year (positively or negatively) from the ‘fair’ market wage. We predict each individual’s fair wage with the same methodology used to generate the conditional gender wage gaps. We first estimate the following model:

, where corresponds to individual *i*’sproductivity on a metric *l* (number of published academic articles, number of published books, number of awards, number of grants, and number of patents), and the vector of controls includes academic tenure (ln) and institution, academic field, and year fixed effects. Additionally, we also interact all productivity measures with academic field dummies to account for heterogeneity in the sensitivity of pay to different performance measures across domains. We then use the estimated coefficients to predict individual’s yearly ‘fair’ market wage: .

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. We use two specifications: in the first one, we rely on a continuous residual measure and use a spline model, separately introducing positive and (absolute value of) negative residuals. For ease of interpretation and calculation of the economic magnitude of our effects, we also generate a binary measure, 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.

Our results, reported in Table 4, suggest that institutions generally grant larger salary increases to those we estimate as previously underpaid and smaller increase to those we estimate as previously overpaid. From an average base rate yearly increase of 2.4%, models 3 and 4 indicate that those who are significantly underpaid compared to their institution-department peers received yearly increases of 3.8% on average, while those who are significantly overpaid relative to their peers received salary increases of only 0.8%, on average. Post-transparency, we observe wage increases for those underpaid gain an additional 1% on average, 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 5, we add a dummy variable and its interaction with treatment for low absolute levels of salaries – defined as a salaries that are inferior to the year-institution-domain average salary. 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.

----- Insert Table 4 about here -----

We explore one final mechanism that may explain our pay transparency results on equity and equality. It is conceivable that shifts in 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. Table S3.2 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 described in Table 3 above—hold with this restricted sample. Our results 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 Appendix S6 we additionally report a series of robustness tests with respect to sample construction and in particular with respect to omission of major states in our data. 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 prior work has shown pay transparency to prompt more equal pay *(12, 13)*, to our knowledge, our results provide a first 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 are unfairly underpaid, and dampening the pay increases of those overpaid. 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 link between pay and academic rank becomes significantly weaker. Moreover, these results appear largely unaffected by any pattern of employee mobility that pay transparency prompts. In summary, pay becomes more equal, more precisely assigned to observable metrics, but also significantly 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 *(27-29)*—responses that will often vary based on what individuals discover about where their individual level of pay ranks relative to others*.*  Indeed, 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 *(30-32)*. These in turn may prompt behaviors costly to employers, including turnover, reduced effort and social cohesion, or simply politicking for change *(33-38).* 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 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.

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 generates these strong effects suggests that these results may generalize to other contexts. In settings with less visible, objective performance measures, we might predict even stronger results, but this prediction awaits future exploration. Pay transparency may have other important effects on organizations. In particular, it may prompt important patterns of mobility, particularly as pay transparency reshapes organizational pay systems. Pay transparency may also have important implications for performance and productivity itself. Such topics also await future research.

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9. In 2001, Norway, for example, provided public digital access to individual tax records, and therefore others’ income. In 2017, Germany passed the Remuneration Transparency Act that grants employees the right to request average gross salaries for a comparison group of no less than six fellow employees. In early 2016, President Obama issued an executive order requiring employers with more than 100 employees to disclose aggregate salary data by race, gender, and ethnicity—an order that the President Trump administration initially overturned, but a 2019 court ruling then upheld. In a smaller, earlier step toward greater transparency, a 2014 executive order guaranteed employees of federal contractors non-retaliation for disclosure of compensation information. Finally, a visible set of individual corporations, like Whole Foods, now voluntarily provides employees with open access to information about their fellow employees’ salaries.

<https://iwpr.org/publications/private-sector-pay-secrecy/>

For instance, Mas *(12)* finds that top earners within municipal jobs experience reductions in nominal wages following the public disclosure of pay and Gartenberg and Wulf *(13)* document significant increases in peer salary co-movements and decreases in pay-performance sensitivity among division managers of publicly traded US firms.

1. A. Mas, Does Transparency Lead to Pay Compression? *Journal of Political Economy* 125(5):1683–1721 (2017).
2. C. Gartenberg, J. Wulf, Pay harmony? Social comparison and performance compensation in multibusiness firms. *Organization Science*, 28(1), 39-55 (2017).

Three working papers, developed contemporaneously with ours, also highlight a link between transparency and equality. Baker and colleagues find evidence that among Canadian academics, men and women’s pay becomes more equal after individual salary levels become fully transparent to the public. See: M. Baker, Y. Halberstam, K. Kroft, A Mas, D. Messacar, Pay Transparency and the Gender Gap. National Bureau of Economic Research Working Paper (2019). Bennedsen and colleaguesfind that the adoption of a Danish law requiring employers with more than 35 employees to disclose aggregated salary data by gender led to men’s and women’s pay becoming more equal. See: M. Bennedsen, E. Simintzi, M. Tsoutsoura, D. Wolfenzon, *Do Firms Respond to Gender Pay Gap Transparency?* National Bureau of Economic Research Working Paper (2019). Finally, Cullen and Pakzad-Hurson find evidence from an online hiring platform that pay transparency pushes those hiring to adopt more equal pay. See: Z. Cullen, B. Pakzad-Hurson, *Equilibrium Effects of Pay Transparency*. Working paper, Harvard Business School (2018).

1. While FOIL requests were filed with all relevant institutions in 50 states, due to data availability, legal structure or digitalization constraints, we restrict our analysis to 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 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. 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.
2. The matching code is available from the authors.
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4. F.D. Blau, J. DeVaro, New Evidence on Gender Differences in Pro-motion Rates: An Empirical Analysis of a Sample of New Hires. *Industrial Relations,* 46(3): 511-550 (2007).
5. L. Babcock, Recalde, M.P., Vesterlund, L. and Weingart, L, Gender differences in accepting and receiving requests for tasks with low promotability. *American Economic Review*, *107*(3), pp.714-47. (2017).
6. H. Sarsons, Recognition for Group Work: Gender Differences in Academia. American Economic Review: Papers and Proceedings. 2017;107 (5) :141-145 (2017).
7. For example, the European Commission Recommendation of 7 March 2014 was focused “[…] on strengthening the principle of equal pay between men and women through transparency.” Similarly, the city of **Albuquerque’s pay equity initiative (giving [weighted] preference to city contractors who hold a pay equity business certificate), focuses explicitly on gender equity.**
8. We obtain a similar pattern of estimates of unexplained wage differentials when using Blinder–Oaxaca wage decomposition models. See alsoBaker et al. *(15)* for similar estimates in the context of Canadian academics.
9. S. Correia, *Linear Models with High-Dimensional Fixed Effects: An Efficient and Feasible Estimator*. Working Paper. Available at http://scorreia.com/research/hdfe.pdf. (2017).
10. K. Borusyak, X. Jaravel, X. *Revisiting Event Study Designs*. Working paper, Available at SSRN 2826228 (2017).
11. As a robustness test, we additionally control for all interactions of productivity metrics with academic disciplines and find our results robust.
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13. O. Godechot, C. Senik, Wage comparisons in and out of the firm. Evidence from a matched employer–employee French database. *Journal of Economic Behavior & Organization* 117:395–410 (2015).
14. D. Card, A. Mas, E. Moretti, E. Saez, Inequality at Work: The Effect of Peer Salaries on Job Satisfaction. *American Economic Review* 102(6):2981–3003 (2012).
15. E.F.P. Luttmer, Neighbors as Negatives: Relative Earnings and Well-Being. *The Quarterly Journal of Economics* 120(3):963–1002 (2005).
16. C.F. Camerer, E. Fehr, When Does “Economic Man” Dominate Social Behavior? *Science* 311(5757):47–52 (2006).
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Supplementary Materials:

Materials and Methods:

Tables: S1.1, S1.2, S1.3, S2.1, S3.1, S3.2, S5.1, S5.2, S6.1, S6.2, S6.3

Figures: S3.1, S3.2, S4.1, S5.1

**Tables and Figures**

Figure 1. Equity in Organizations: Distribution of residuals from regressions predicting market wages, 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 Kolmogorov-Smirnov tests for equality of distribution functions: 0.123\*\*\* (left panel), 0.067\*\*\* (right panel). \*\*\*p<0.001.

Figure 2. Equality in Organizations: Distribution of residuals from regressions predicting wages, by transparency shock



Notes: The figure presents kernel density estimates of 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 Kolmogorov-Smirnov tests for equality of distribution functions: 0.0432\*\*\*. \*\*\*p<0.001.

Figure 3. 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 more than 5 years 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.

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

|  |  |  |  |  |  |  |  |  |  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- | --- | --- | --- | --- | --- | --- | --- | --- | --- |
|  |  | |  | |  | |  | |  | |  | |  |  |
| DV: ln(Wage) | (1) | (2) | | (3) | | (4) | | (5) | | (6) | |
|  |  |  | |  | |  | |  | |  | |
| Treatment | 0.060  (0.012)  [0.019] | 0.035  (0.011)  [0.018] | | 0.007  (0.011)  [0.012] | | -0.010  (0.009)  [0.008] | | -0.009  (0.008)  [0.010] | | -0.016  (0.008)  [0.011] | |
| Female |  | -0.211  (0.018)  [0.008] | | -0.112  (0.009)  [0.007] | | absorbed | | -0.062  (0.005)  [0.004] | | absorbed | |
| Treatment # Female |  | 0.067  (0.011)  [0.011] | | 0.059  (0.008)  [0.008] | | 0.031  (0.004)  [0.005] | | 0.020  (0.005)  [0.006] | | 0.021  (0.004)  [0.003] | |
| Associate Professor |  |  | |  | |  | | 0.122  (0.008)  [0.005] | | 0.064  (0.007)  [0.005] | |
| Full Professor |  |  | |  | |  | | 0.391  (0.012)  [0.005] | | 0.173  (0.014)  [0.009] | |
| 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), [state-year].

Table 2. The effect of pay transparency on wage variance

|  |  |  |  |  |
| --- | --- | --- | --- | --- |
|  |  |  |  |  |
| DV: Wage variance | (1) | (2) | (3) | (4) |
| Within: | Institution – academic field (11 categories) | | Institution –academic field (25 categories) | |  |
|  |  |  |  |  |
| Treatment | -0.063  (0.014)  [0.021] | -0.064  (0.016)  [0.022] | -0.067  (0.015)  [0.020] | -0.068  (0.015)  [0.022] |
|  |  |  |  |  |
| 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 | 20,615 | 17,239 |

Notes: The table presents OLS regression estimates explaining variance in salaries. Standard errors clustered at the level of: (institution), [state-year]. In models 2 (4) we include controls for average institution-academic field cumulative productivity outcomes and variance of these productivity outcomes. Models 1-2 report results based on peers defined by 11 academic fields, while models 3-4 use more fine-grained categorization of academic fields (25 categories).

Table 3. Predicted marginal effect of star levels of performance and rank 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 | B | A | B | A | B | A | B | A | B | A | B | A |
| Academic articles | 9.5% | 5.2% | 2.7% | 2.7% | 12.8% | 5.7% | 20.7% | 6.9% | 9.9% | 5.6% | 18.0% | 10.1% |
| Patents | 0 | 0 | 0 | 0 | 2.6% | 0.3% | 0 | 0 | 0 | 0 | 4.2% | 4.2% |
| Books | 2.2% | 1.0% | 5.1% | 2.7% | 0 | -1.4% | 0 | 0 | 3.6% | 1.2% | 0 | 0 |
| Grants | 6.1% | 4.2% | 0 | 0 | 14.7% | 10.6% | 10.9% | 8.5% | 6.1% | 6.1% | 7.2% | 7.2% |
| Awards | 0 | 0.8% | 0 | 3.0% | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| Promotion to Associate (compared with Assistant) | 14.5% | 7.8% | 12.8% | 7.3% | 11.4% | 3.9% | 14.5% | 6.2% | 13.9% | 8.6% | 10.5% | 3.6% |
| Promotion to Full  (Compared with Assistant) | 32.3% | 23.4% | 28.4% | 23.4% | 22.6% | 16.8% | 31.0% | 21.8% | 29.7% | 23.5% | 18.5% | 13.9% |

Notes: The table presents the predicted marginal effects of performance outcomes on salaries. Results based on Models 2 and 4 reported in Table S5.1 run for the whole population and separately for each of the academic disciplines. Star performance levels are calculated at the 95th percentile of the performance distribution across all years and separately for each domain when relevant. When 90% confidence intervals of the estimated coefficients include zero, we assume the effect to be nil.

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

|  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- |
| DV: % Wage change in the following year | | |  |  |  |
|  | Underpaid and overpaid:  continuous specification | | Underpaid and overpaid:  binary specification | | |
|  | (1) | (2) | (3) | (4) | (5) |
| Treatment |  | -0.030  (0.419)  [0.857] |  | -0.171  (0.412)  [0.866] | -0.359  (0.396)  [0.881] |
| Underpaid | 2.857  (0.466)  [0.330] | 2.319  (0.500)  [0.452] | 1.372  (0.155)  [0.106] | 1.027  (0.221)  [0.181] | 0.764  (0.173)  [0.136] |
| Underpaid # treatment |  | 0.849  (0.369)  [0.652] |  | 0.533  (0.193)  [0.233] | 0.519  (0.163)  [0.189] |
| Overpaid | -5.551  (0.355)  [0.226] | -5.191  (0.583)  [0.475] | -1.568  (0.103)  [0.072] | -1.581  (0.161)  [0.155] | -1.451  (0.138)  [0.145] |
| Overpaid # treatment |  | -0.580  (0.574)  [0.508] |  | 0.021  (0.135)  [0.165] | 0.158  (0.125)  [0.158] |
| Low salary |  |  |  |  | 0.530  (0.240)  [0.213] |
| Low salary # treatment |  |  |  |  | 0.280  (0.219)  [0.239] |
| 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: In the continuous specification, underpaid is defined as the absolute value of the individual *i’*s residual from regression predicting ‘fair’ market wage in year *t* (see notes under Figure 3 for full specification) if the residual is negative and 0 otherwise. Overpaid is defined as the value of individual *i’*s residual from regression predicting ‘fair’ market wage in year *t* (see notes under Figure 3 for full specification) 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 ‘fair’ 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 ‘fair’ market wage in year *t* is positive and greater than the average residual from the same regression for all individuals in a given year and the same institution. *Low salary* is equal to 1 if individual *i*’ssalary is below average, compared to year-institution-domain peers and 0 otherwise. Standard errors clustered at the level of: (institution), [state-year].

**Supplementary materials for: The influence of pay transparency on 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 constructed a measure of impact 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 (ρ > 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 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 (available from the authors upon request). We also re-run all our analyses using a more fine-grained categorization of individuals into 25 academic domains finding robust results across specifications. 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. 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 (*1*) 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 (*2*). 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 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 (*3*). 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. For each of these 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**

Figure S3.1. 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.

Figure S3.2. Equity in Organizations: The effect of pay transparency on gender wage gap.

|  |  |
| --- | --- |
| Left panel | Right panel |

Notes: The figure presents regression coefficients from an OLS regression model explaining (ln) wages. Reference category is more than 5 years 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. Left panel – full population; Right panel – sample restricted to academics in California, New York, Texas, and West Virginia

Figure S3.3. Equity in Organizations: Distribution of residuals from regressions predicting market wages, by transparency shock



Notes: The figure presents kernel density estimates of regression residuals by 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, academic domain, and year fixed effects. Residuals trimmed at 1% and 99%. Two-sample Kolmogorov-Smirnov tests for equality of distribution functions: 0.025\*\*\*. \*\*\*p<0.001.

Table S3.1. Conditional gender wage gap by state and main academic disciplines

|  |  |  |
| --- | --- | --- |
|  | Conditional Gender Wage Gap (%) | |
| State / Academic Field | 2005 | 2015 |
| California | 14.5 | 1.1 |
| Connecticut | 21.3 | 8.0 |
| Florida | 8.3 | 6.0 |
| New York | 18.3 | 4.5 |
| Pennsylvania | -1.4 | 3.0 |
| Texas | 13.7 | 6.1 |
| Virginia | 12.2 | 10.3 |
| West Virginia | 7.0 | 2.7 |
| Humanities | 10.1 | 2.4 |
| Physical and Mathematical Sciences | 14.6 | 4.6 |
| Biological and Biomedical Sciences | 20.2 | 4.2 |
| Social and Behavioral Sciences | 14.0 | 6.9 |
| Engineering | 7.3 | 6.6 |

Notes: The numbers presented in the table are based on regression estimates of conditional gender wage gap as specified in models explained in the notes under Figure S3.1. Models are run separately for each state and academic discipline. Positive values indicate female faculty underpaid compared to men faculty.

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

|  |  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- | --- |
| DV: ln(Wage | (1) | (2) | (3) | (4) | (5) | (6) |
| Treatment | 0.042  (0.009)  [0.012] | 0.021  (0.009)  [0.012] | 0.010  (0.010)  [0.010] | 0.001  (0.010)  [0.010] | -0.002  (0.009)  [0.010] | -0.008  (0.008)  [0.012] |
| Female |  | -0.200  (0.019)  [0.010] | -0.104  (0.008)  [0.004] | absorbed | -0.061  (0.005)  [0.004] | absorbed |
| Treatment # Female |  | 0.064  (0.010)  [0.013] | 0.046  (0.006)  [0.006] | 0.030  (0.004)  [0.005] | 0.023  (0.005)  [0.006] | 0.020  (0.004)  [0.003] |
| Associate Professor |  |  |  |  | 0.104  (0.010)  [0.006] | 0.067  (0.007)  [0.005] |
| Full Professor |  |  |  |  | 0.364  (0.013)  [0.006] | 0.179  (0.014)  [0.010] |
| 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: Specifications as in Table 1. 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), [state-year]. 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), [state-year].

**S4**

Figure S4.1. Equality in Organizations: The effect of wage transparency on wage variance

|  |  |
| --- | --- |
| Left panel | Right panel |
| Notes: The figures presents regression coefficients from a dynamic academic field-institution difference-in-differences OLS regression model explaining wage variance. Reference category is more than 3 years prior to transparency shock. Dependent variable: Variance in wages is calculated as a yearly within Academic Field (11 [right panel], 25 [left panel] categories) - Institution variance in (log) wages. Plotted coefficients: years from (to) transparency shock with 95% CIs. 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. | |

**S5**

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

|  |  |  |  |  |
| --- | --- | --- | --- | --- |
|  | (1) | (2) | (3) | (4) |
| Treatment |  | 0.0577  (0.0159)  [0.0190] |  | 0.0414  (0.0096)  [0.0115] |
| Academic tenure (ln) | 0.1492  (0.0144)  [0.0088] | 0.1404  (0.0148)  [0.0097] |  |  |
| Academic tenure (ln) # treatment |  | -0.0157  (0.0058)  [0.0065] |  |  |
| Academic Articles | 0.0010  (0.0001)  [0.0001] | 0.0022  (0.0004)  [0.0003] |  |  |
| Academic Articles # treatment |  | -0.0010  (0.0003)  [0.0002] |  |  |
| Patents | -0.0032  (0.0023)  [0.0015] | 0.0070  (0.0047)  [0.0042] |  |  |
| Patents # treatment |  | -0.0086  (0.0042)  [0.0036] |  |  |
| Books | 0.0046  (0.0013)  [0.0009] | 0.0109  (0.0023)  [0.0017] |  |  |
| Books # treatment |  | -0.0056  (0.0020)  [0.0014] |  |  |
| Grants (#) | 0.0139  (0.0016)  [0.0012] | 0.0201  (0.0025)  [0.0032] |  |  |
| Grants (#) # treatment |  | -0.0059  (0.0022)  [0.0024] |  |  |
| Awards | 0.0092  (0.0029)  [0.0019] | 0.0023  (0.0042)  [0.0032] |  |  |
| Awards # treatment |  | 0.0075  (0.0042)  [0.0033] |  |  |
| Associate Professor |  |  | 0.1147  (0.0069)  [0.0060] | 0.1352  (0.0069)  [0.0073] |
| Associate Professor # treatment |  |  |  | -0.0592  (0.0079)  [0.0081] |
| Full Professor |  |  | 0.2478  (0.0124)  [0.0111] | 0.2767  (0.0142)  [0.0114] |
| Full Professor # treatment |  |  |  | -0.0690  (0.0129)  [0.0103] |
| Individual fixed effects | yes | yes | yes | yes |
| Institution fixed effects | yes | yes | yes | yes |
| Year fixed effects | yes | yes | yes | yes |
| Observations | 364,301 | 364,301 | 333,828 | 333,828 |

Notes: The table presents OLS regression estimates explaining (ln) salaries. Standard errors clustered at the level of: (institution), [state-year].

Table S5.2. The effect of wage transparency on the determinants of pay. Restricted sample.

|  |  |  |  |  |
| --- | --- | --- | --- | --- |
|  | (1) | (2) | (3) | (4) |
| Treatment |  | 0.1099  (0.0133)  [0.0234] |  | 0.0550  (0.0138)  [0.0138] |
| Academic tenure (ln) | 0.1556  (0.0131)  [0.0099] | 0.1309  (0.0126)  [0.0110] |  |  |
| Academic tenure (ln) # treatment |  | -0.0306  (0.0049)  [0.0070] |  |  |
| Academic Articles | 0.0010  (0.0001)  [0.0001] | 0.0023  (0.0004)  [0.0003] |  |  |
| Academic Articles # treatment |  | -0.0011  (0.0003)  [0.0002] |  |  |
| Patents | -0.0047  (0.0026)  [0.0014] | 0.0069  (0.0056)  [0.0053] |  |  |
| Patents # treatment |  | -0.0098  (0.0048)  [0.0045] |  |  |
| Books | 0.0058  (0.0015)  [0.0010] | 0.0112  (0.0022)  [0.0016] |  |  |
| Books # treatment |  | -0.0047  (0.0019)  [0.0013] |  |  |
| Grants (#) | 0.0146  (0.0017)  [0.0014] | 0.0208  (0.0020)  [0.0035] |  |  |
| Grants (#) # treatment |  | -0.0058  (0.0020)  [0.0024] |  |  |
| Awards | 0.0113  (0.0036)  [0.0020] | 0.0037  (0.0042)  [0.0029] |  |  |
| Awards # treatment |  | 0.0081  (0.0047)  [0.0029] |  |  |
| Associate Professor |  |  | 0.1087  (0.0075)  [0.0084] | 0.1359  (0.0071)  [0.0092] |
| Associate Professor # treatment |  |  |  | -0.0699  (0.0086)  [0.0097] |
| Full Professor |  |  | 0.2366  (0.0139)  [0.0154] | 0.2616  (0.0157)  [0.0147] |
| Full Professor # treatment |  |  |  | -0.0614  (0.0120)  [0.0124] |
| 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), [state-year].

**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, S4.1, and S5.1 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) | (3) | (4) | (3) | (4) |
|  |  |  |  |  |
| Treatment | 0.026  (0.014)  [0.018] | 0.018  (0.014)  [0.017] | 0.005  (0.012)  [0.011] | -0.007  (0.009)  [0.011] |
| Female | -0.115  (0.010)  [0.008] | -0.103  (0.012)  [0.007] | absorbed | absorbed |
| Treatment # Female | 0.057  (0.009)  [0.010] | 0.050  (0.010)  [0.009] | 0.027  (0.005)  [0.006] | 0.024  (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. Specifications identical to Model 3 (Models 1-2) and Model 4 (Models 3-4) in Table 1.

Table S6.2. 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 | -0.051  (0.018)  [0.026] | -0.083  (0.022)  [0.022] | -0.057  (0.017)  [0.026] | -0.084  (0.020)  [0.021] |
|  |  |  |  |  |
| 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 | 14,058 | 10,812 |

Notes: The table presents OLS regression estimates explaining variance in salaries. Specifications identical to Model 2 in Table 2 (Models 1 and 2), and Model 4 (Models 3 and 4).

**Additional references:**

1. D. Card, A. Mas, E. Moretti, E. Saez, Inequality at Work: The Effect of Peer Salaries on Job Satisfaction. *American Economic Review* 102(6):2981–3003 (2012).

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