What accounts for the unexplained gender wage gap among U.S. faculty?

Cory Koedel, University of Missouri
Trang Pham, University of Missouri

It is a well-documented empirical regularity that female faculty earn less than male faculty, even conditional on rich control variables that account for gender differences in fields, experience, and research productivity. This stands in stark contrast to racial-ethnic wage gaps among faculty, which are large unconditionally but fully explained by observed factors. We use a recently-collected faculty data panel to test four mechanisms that may account for the persistent unexplained gender wage gap in academia: (1) gender wage differences at the point of entry into academia, (2) gender differences in the likelihood of promotion, and salary increases conditional on promotion, (3) gender differences in the true or perceived (by university administrators) willingness-to-move, and (4) gender-based composition shifts among faculty correlated with income. We do not find clear evidence that any of these mechanisms drive the gender wage gap, although there is at least suggestive evidence that a wage difference at the point of entry may be a contributing factor.

1. Introduction

The gender wage gap among university faculty is widely studied. Previous research identifies several observable factors that combine to explain a large portion of the gap, including gender differences in (a) fields, (b) experience, and (c) research productivity (Ginther and Hayes 2003; Li and Koedel 2017; Porter et al. 2008; Umbach 2006); all of which are themselves the product of a myriad of factors. However, a persistent finding is that a non-negligible portion of the gender wage gap remains unexplained even after accounting for rich observable information about faculty. This is in contrast to racial/ethnic wage gaps among faculty, which can be entirely explained by observed factors (Porter et al. 2008; Li and Koedel 2017).

We empirically examine four possible explanations for the persistent, unexplained gender wage gap among university faculty: (1) gender wage differences at the point of entry into academia, (2) gender differences in the likelihood of promotion, and salary increases conditional on promotion, (3) gender differences in the true or perceived (by university administrators) willingness-to-move, and (4) gender-based composition shifts among faculty correlated with income. We test these mechanisms using a unique, recently-collected data panel covering faculty at 40 selective research universities in six fields—biology, chemistry, economics, education leadership and policy, English, and sociology. While there is some previous work examining gender gaps in entry-level offers and promotion rates, to the best of our knowledge we are the first to empirically test for evidence of gender gaps in pay raises at promotion (part 2 of mechanism 2), real or perceived willingness-to move (mechanism 3), and selective attrition by gender correlated with earnings (mechanism 4).

Among the faculty in our data, we begin by corroborating broad patterns in previous research in terms of cross-sectional differences in earnings between men and women. In particular, we show a gender pay gap among faculty in our data favoring men that is of similar magnitude to the gap estimated in previous research. We also show that a non-negligible fraction of the gap—about 18 percent in our sample—is unexplained by observable factors. We then test the above-hypothesized explanations for the unexplained portion of the gap.

We do not find strong evidence in favor of any of the four mechanisms as drivers of the unexplained portion of the gender wage gap among faculty. This is particularly true for mechanisms (2)-(4)—i.e., gender differences in promotion or promotion pay raises, gender differences in the willingness-to-move, and changes in the gender composition of faculty over time. There is suggestive evidence that part of the gap is explained by differences in wages at entry (mechanism 1), but the gender wage gap at entry is not statistically significant in our sample and even if we take our insignificant point estimate at face value, it cannot account for the full magnitude of the total unexplained gap.

Ultimately, we conclude that the sources of the well-documented, unexplained portion of the gender wage gap in academia remain elusive. A caveat to our findings is that our data panel is short, covering just a 2-year window of the careers of the faculty in our sample, which has implications for statistical power. Although the loss of power due to this issue is partly offset by our large sample, we cannot rule out moderate gender differences along some of the dimensions we consider—most notably wage gaps at entry—despite our statistically insignificant findings. Noting this caveat, a substantive explanation for our null findings is the modern context in which our data were collected. Specifically, the period from 2016-2018 we study is one marked by an acute awareness of faculty equity issues at most universities. The factors that have historically contributed to currently observed gender wage gaps in cross-sectional studies of faculty may not be operating in the same ways contemporarily. Unfortunately, we cannot test this hypothesis directly because we do not have access to comparable panel data from prior years, but if this is the explanation for our null findings, it suggests optimism about the prospects for the long-term amelioration of the unexplained gender wage gap among faculty.

2. Background

There is a vast literature studying the prevalence and causes of gender wage gaps in the broader economy and within academia. It is not feasible to provide a full review of all papers—for ease of presentation, we focus our discussion with two goals in mind. The first goal is to describe our current understanding of gender wage gaps in academia in order to establish what is known and provide context for our analysis. The second goal is to motivate the potential mechanisms that we test as potential drivers of the unexplained portion of the gender wage gap among faculty.

The most commonly-used data resource to document and explain gender wage gaps in academia is the National Survey of Post-secondary Faculty (NSOPF). The NSOPF is a cross-sectional survey instrument that was administered four times in the late 1990s and early 2000s (it was discontinued after the 2003-04 survey). Among faculty at research universities—a group most comparable to our sample—the gender wage gap estimated using NSOPF data is about 20 percent, and this falls to about 12 percent among faculty at non-research-focused 4-year colleges (Perna 2001; Barbezat and Hughes 2005; Toutkoushian 1998a). Conditioning on observable information such as the field, institution type and location, and publications; the gender gap narrows such that female faculty earn $92-97, on average, for every $100 earned by comparable male faculty (Porter et al. 2008; Perna 2001; Barbezat and Hughes 2005). The unexplained portion of the full, unconditional gender wage gap reported in previous studies depends on a number of factors, most notably the quantity and quality of the observable characteristics available, and ranges between about 15 and 30 percent (Barbezat and Hughes 2005; Toutkoushian and Conley 2005).

All of the NSOPF-based studies mentioned thus far use a single cross-sectional wave or are based on repeated cross-sections—i.e., the NSOPF does not link respondents across waves. True faculty data panels are rare. We are aware of just a handful of previously published articles that use panel data to evaluate the gender wage gap in academia. First, Ginther and Hayes (2003) construct a longitudinal panel of faculty in the humanities using waves of the Survey of Doctorate Recipients (SDR) from 1977-1995. Before analyzing their data panel, these authors use repeated cross-sectional data from the waves of the SDR to show that among professors in the humanities, salary differences by gender disappear by 1995. This result stands in stark contrast to salary gaps in the sciences (Ginther 2001), for which Ginther and Hayes (2003) offer several possible explanations. Ginther and Hayes (2003) then use their data panel to perform a duration analysis of promotions and find that women in the humanities are less likely to be promoted and take longer to be promoted than men. Ginther and Kahn (2006) use the same base data and find no evidence of a gender difference in receiving tenure, and only a small difference in promotion to full professor, in STEM fields. They also show that the difference in promotion to full professor is entirely explained by fertility decisions, which is consistent with the finding from Toutkoushian (1998b) that the gender pay gap is larger for married women with children than for other women. Thus, the evidence on promotion gaps is mixed and suggests the gaps may be field specific.[1]

The other two panel studies are Chen and Crown (2019) and Taylor et al. (2020), both of which estimate gender pay gaps at single institutions (The Ohio State University and an anonymous

[1] Several other studies merit brief mention. In economics specifically, Sarsons (2017) reports that women are less likely to be promoted than men using data on faculty who went up for tenure between 1985 and 2014. She also shows that women receive less credit for their coauthored work than men during promotion and tenure process. Weisshaar (2017) finds evidence of gender promotion gaps in computer science, English, and Sociology. Durodoye Jr. et al. (2020) find no evidence of a gender gap in tenure outcomes in most fields (with the exceptions in business and engineering), but a significant gap between men's and women's promotion rates to full professor.

university, respectively). Chen and Crown (2019) estimate an unconditional gender pay gap of roughly $26,000, or about 21 percent of the average wage at Ohio State. Of the total unconditional gap, just over a quarter is unexplained, although they do not account for research productivity in their wage model. Taylor et al. (2020) report a similarly-sized unconditional gap and unexplained component and like in Chen and Crown (2019), they do not account for research productivity in their wage models.

Li and Koedel (2017) use the first wave of our data panel, which we describe in detail below, to perform a comparable cross-sectional analysis with our sample of faculty from 40 selective research universities in 2016. They estimate that unconditionally, female faculty earn $23,000 less than their male colleagues, on average, or about 19 percent of the average wage in their data. They also find that 18 percent of the unconditional gap is unexplained by observed factors. The unexplained portion of the gap estimated by Li and Koedel (2017) is in the range of estimates elsewhere in the literature, although it is on the low end of the range. A likely explanation is that they use externally collected rather than self-reported measures of productivity, which are additionally adjusted for the faculty member's discipline in their predictive wage model. Li and Koedel (2017) show that these measures are strong predictors of wages among the faculty in their sample, and it follows that their inclusion will result in less unexplained variance in wages.

Having contextualized the cross-sectional gender wage gap in our data, and the portion unexplained by observed factors, we next draw on the literature to motivate the four mechanisms mentioned above that we hypothesize could account for the unexplained portion of the gap: (1) a wage gap at entry, (2) promotion and promotion-raise differences, (3) differences in the willingness-to-move, and (4) faculty composition changes over time.

The first mechanism is motivated by research showing that men have a higher propensity to negotiate (Babcock and Laschever 2003; Bertrand 2011; Croson and Gneezy 2009), which could show up in the form of higher initial wage offers, all else equal. Toumanoff (2005) provides evidence consistent with this mechanism using data from an anonymous private research university, showing that men have higher pay at entry on the order of 4-7 percent. However, Porter et al. (2008) perform a more comprehensive analysis of faculty wage gaps at entry using all four waves of the NSOPF (1988, 1993, 1999 and 2004) and reach a different conclusion. Specifically, while they consistently estimate a small pay gap at entry favoring men in each NSOPF wave—on the order of 2-3 percent—none of their estimates are statistically significant.

The second mechanism is promotion and raise-at-promotion differences. Existing evidence points to gender gaps in promotion in some but not all fields (Ginther and Hayes 2003; Ginther and Kahn, 2006; Sarsons 2017; Weisshaar 2017; Durodoye Jr., 2020). We contribute new evidence on the promotions question. In addition, noting that promotion is typically associated with a larger-than-normal pay increase, we further test whether conditional on promotion, male and female faculty receive different raises, which could be related to differences in the propensity to negotiate and ultimately impact measured wage gaps in cross-sectional data.

The third mechanism is that men may be more willing to relocate, or at least more effective in convincing administrators that they are willing to relocate, than women. This could be caused by several factors—the two that seem most likely are (a) men are more likely than women to be primary earners in their families (Bernard 1981; Haas 1986), which could give them greater flexibility to move for own-career reasons, all else equal (Bielby and Bielby 1992), and (b) noting that the threat of moving is a potential negotiating tactic with university administrators, the higher proclivity toward negotiation among men may also lead them to make a more convincing case of the threat to leave

(Daly and Dee 2006). In our data we can observe moves, and as a proxy of willingness to move (in the absence of an actual move), we use the receipt of a large wage increase without moving, which we interpret as a signal of retention efforts on behalf of administrators.[2]

The fourth and final mechanism we consider is that the observed gender wage gap in cross-sectional data reflects a composition change over time to the faculty. Given that research consistently identifies a pay gap favoring men, the types of composition shifts we look for are either (a) high-earning men being more likely to remain in academia than their high-earning female peers, and/or (b) low-earning men being more likely to exit academia than their low-earning female peers. If there is gender-based differential attrition by salary level in either or both of these ways, an observed wage gap could appear in cross-sectional estimates of the wage gap even if men and women acted and were treated identically in every way (other than their exit likelihoods). Although the gender composition of occupations is widely understood as a factor in explaining gender wage gaps more broadly (e.g., see Hegewisch et al. 2010; Macpherson and Hirsch 1995), we are not aware of any previous work testing whether the gender gap in a particular occupation (faculty or otherwise) is affected by differential attrition correlated with salary.

3. Data

Our data panel is an expansion of the original cross-sectional dataset used in Li and Koedel (2017). We first summarize their dataset, then describe the panel expansion.

The original sample consists of tenured and tenure-track faculty from 40 selective public universities ranked highly in the 2016 *U.S. News and World Report* rankings. The faculty were sampled from six academic departments—biology, chemistry, economics, education leadership and policy,

---

[2] A large raise could in principle be for any number of reasons, but we assume that the most common reason will be related to retention efforts.

English, and sociology—three of which were randomly chosen at each university in the dataset. In total, just over 3,800 tenure or tenure-track faculty from 120 academic departments at the 40 universities were included in the Li and Koedel (2017) analytic sample. The initial dataset was collected early during the 2016 calendar year. Once a department was selected for sampling, all faculty listed on the department website were included in the dataset. Appendix A-1 lists the universities and departments that were sampled.[3]

For each university-by-department cell, data were collected for all tenure-track and tenured faculty on demographics, qualifications, research productivity, and salaries. The data were not collected via survey, the benefits of which we elaborate on below, but rather were obtained directly from online sources. Demographic and qualification data were collected from faculty members' online profiles. The qualification data include the faculty member's rank, years of experience, and the PhD-granting institution. For most faculty, experience is measured from the year the PhD was obtained as reported on faculty websites or CVs. In cases where a faculty member's profile does not indicate the year of the PhD, experience is measured by the time since the first registered publication, either on the faculty member's website (first choice) or Scopus© (second choice). Between these various sources, experience measures are available for 98 percent of the original cross-sectional sample. The PhD-granting institution was taken from each faculty member's profile and is available for 94 percent of faculty. We divide PhD-granting institutions into four selectivity groups based on their rankings in *U.S. News and World Report*, inclusive of private universities.

The demographic variables—race/ethnicity and gender—were collected using visual inspections of faculty profiles available online. Faculty in the original data are grouped into one of

[3] Although Li and Koedel (2017) randomly sampled departments, faculty in our data are disproportionately in STEM fields because academic departments in these fields tend to be larger.

five possible racial/ethnic categories: Black, Asian, Hispanic, White, and Other/Unknown, and one of three gender categories: male, female, and unknown.[4]

Research-productivity in our dataset is based on Scopus© profiles. We have information on the number of publications, number of citations, and the h-index for each faculty member. For each metric, we create normalized measures of productivity within fields as follows:

$$\tilde{P}_{ij} = \frac{P_{ij}-\bar{P}_j}{\sigma_j} \tag{1}$$

where $\tilde{P}_{ij}$ is the normalized measure for faculty member *i* in field *j*, P_{ij} is the raw measure, and $\bar{P}_j$ and σ_j are the sample average and standard deviation in field *j*, respectively. The field-specific normalization allows for different levels of productivity across fields. While these metrics are imperfect measures of research productivity (Perry and Reny 2016), Li and Koedel (2017) show that they explain a substantial fraction of faculty wages and of the gender wage gap. Of the three measures, the h-index is the strongest single predictor of wages (although to the best of our knowledge it has not been used as a productivity measure in previous research on the gender wage gap among faculty, other than by Li and Koedel).

Wage data for faculty at most public universities are published by government agencies and available online. Of all tenure-track faculty included on the rosters in our sample, wage data were available for 94 percent of the original cross-sectional dataset. The primary reason for missing wage data in 2016—and in fact the only reason we can identify given the comprehensive nature of wage reporting for public employees—is that the faculty member is new to the university or was on leave

[4] We tested the reliability of our demographic designations by having two raters code a subset of the data. Interrater reliability was high along both dimensions, at 95.5 percent for race/ethnicity and 99.75 percent for gender. For a deeper conceptual discussion of how the racial/ethnic and gender data were collected, see Laughter (2018) and Li and Koedel (2018).

and did not draw a salary. Consistent with this explanation, Li and Koedel (2017) show that being a young professor is by far the strongest predictor of missing wage data in the 2016 sample.

To facilitate our current panel-based study of the gender wage gap, we conducted a follow-up data collection effort on the original cross-sectional sample collected by Li and Koedel (2017). The follow-up data collection occurred two years after the original data collection, in early 2018. For all faculty members in the original dataset, we attempted to find them again in 2018 and update their information. For most faculty—i.e., those who did not leave academia—this was straightforward. Faculty who left academia did one of two things: (1) they retired, or (2) they moved to a non-academic position.[5] Retirements were confirmed whenever possible by official announcements. In some instances, when a highly-senior, tenured faculty member was not found anywhere else online in 2018, a retirement was assumed. Among pre-retirement-aged faculty who were no longer observed in academia in 2018, we found information online about their new jobs for most. For the small handful of pre-retirement faculty in the original sample for whom we could find no post-2016 evidence of their whereabouts (27 faculty members out of over 3,800), we code them simply as having left academia.

Among the individuals who remained in academia, we collected updated information about their academic rank, publication record from Scopus©, and wages. New information along the first two dimensions was collected universally. We were able to collect updated wage information for most, but not all faculty. The main reason for missing wage data during the 2018 collection wave is that the faculty member changed universities. When a faculty member changed universities, we did not collect wage data even when the data were available (i.e., when the faculty member moved to

[5] In one other instance, the faculty member died.

another public university). This is to maintain symmetry in our data for movers because wage data are not available for all universities.

About five percent of the tenure track faculty with wage data in 2016 are missing wage data in 2018 for a reason unrelated to mobility (of these, about two-thirds are retirees).[6] Another four percent of the original sample are missing 2018 wages due to a mobility event, inclusive of moves both within and outside of academia. In addition to these faculty with missing data, we exclude another three percent of faculty from the original dataset for whom 2018 wage data are available, but the data suggest a significant measurement problem.[7] In total, we were able to construct a useable 2018 wage profile for 88 percent of the original sample in Li and Koedel's original analysis.[8]

We conclude the discussion of data by noting the lack of a survey-based design as a strength of our study. The primary benefit is that our findings are not subject to potential bias caused by voluntary responses. Of particular concern in our context is research showing that survey response rates are correlated with earnings and the nature of the correlation varies by gender (Bollinger et al. 2019). In contrast to a survey-based dataset, our data collection process structurally prevents voluntary response bias from affecting our findings. This is a potentially significant issue in this area of research—for example, in the last year that the NSOPF was administered (2003-04), the survey had a response rate of just 76 percent.

In summary, other than small sources of data loss—mostly applicable to our wage analysis—we were able to find the vast majority of the faculty in the original sample during the second data-

---

[6] Outside of retirees, there are 58 faculty members in the original sample for whom we could find no common factors among them that might explain why their wage data are missing. Anecdotally, faculty leaves likely account for at least some of these instances.

[7] These are faculty with wage decreases of more than 30 percent or wage increases above 100 percent, which we deem as outside of the bounds of what is realistic. These large changes likely reflect reporting fluctuations due to circumstances such as faculty leaves, partial sabbaticals, and temporary external salary support.

[8] These and other data splits are shown in Table 2.

collection wave and thus our data panel exhibits minimal attrition. Table 1 summarizes the attrition in our dataset formally and Table 2 provides descriptive statistics for the analytic samples we use for the various analyses that follow.

4. Methods

4.1. Establishment of the Baseline Unexplained Gender Gap

We begin by replicating the Li and Koedel (2017) estimate of the conditional gender wage gap in the 2016 cross-sectional dataset, which is based on the following regression:

$$Y_{ijk} = \beta_0 + \boldsymbol{X_{ijk}\beta_1} + \boldsymbol{R_i\beta_2} + G_i\beta_3 + \delta_j + \theta_k + \eta_{ijk} \tag{2}$$

In equation (2), Y_{ijk} is the annual salary for faculty member *i* at university *j* in field *k*, in dollars. $\mathbf{X_{ijk}}$ is a vector of faculty qualifications and measures of research productivity, $\mathbf{R_i}$ a vector of indicators for the racial/ethnic designation of faculty member *i* where white faculty are the omitted group, G_i is an indicator equal to one if the faculty member is female, where male faculty are omitted, δ_j a university fixed effect, θ_k a field fixed effect, and η_{ijk} an idiosyncratic error term.[9] We cluster our standard errors at the university level throughout.

The *X*-vector includes years of experience, the field-normalized h-index from Scopus©, and indicators for the prestige of the PhD-granting institution. We further interact the field-normalized h-index with field indicators to allow for differential earnings returns to productivity across disciplines. For experience, Li and Koedel (2017) use a linear experience control, although their findings are substantively similar if they control for experience more flexibly. In our main replication of their findings we use a linear experience control; however, in our new models to test the above-

[9] Li and Koedel (2017) include one faculty member for whom gender could not be identified and an "unknown gender" indicator to account for these faculty in their model. For ease of presentation we drop this faculty member from our analysis.

mentioned explanations of the gender wage gap, we substitute for the linear control with experience fixed effects.[10] Finally, we use the categories shown in Table 2 to control for the quality of the PhD-granting institution. After replicating the Li and Koedel (2017) result, we also estimate the gender gap in our updated 2018 data using the same model.[11]

The conditional wage gap is represented in equation (2) by the parameter β_3. The unconditional gap is estimated from an analog to β_3 from a sparse version of the model where the variables $\mathbf{X_{ijk}}$, $\mathbf{R_i}$, and their corresponding coefficients, along with the fixed effects δ_j and θ_k, are omitted. In the analysis that follows we look for factors outside of the predictors in this model that could drive differences in wages, which may explain why the estimate of β_3 in equation (2), and similar estimates elsewhere in the literature, is large and negative.

We also acknowledge the general caveat—which applies to ours and other similar studies—that there is ambiguity in interpreting the conditional gender wage gap. Most notably, a conditional wage gap of zero would not necessarily imply the absence of gender discrimination. For example, if women are discriminated against in the publication process, gaps in wages attributable to measured research productivity would not capture productivity *per se*, but also their differential treatment.[12] The same logic applies to the other measured attributes in $\mathbf{X_{ijk}}$ (e.g., there could be gender

---

[10] This substitution is conceptually appealing because it forces stricter comparisons by experience, but our findings are generally similar if we use a linear control as well, which we show for selected models in the appendix. Also recall that the experience information comes from several different sources; our regressions include indicator variables to identify the source of the experience data.

[11] We do not control for rank directly based on evidence presented above that gender may impact promotions and the timing of promotions. To the extent that this is the case, including rank information directly in the model could mask differential wage outcomes by gender that we aim to capture.

[12] Regarding the idea of controlling for research productivity specifically, the literature on whether and how much women are discriminated against in the publication process is mixed. Examples of studies that find no evidence of gender bias in the referee process include Abrevaya and Hamermesh (2012), Blank (1991), Fox et al. (2016), and Lane and Linden (2009). Card et al. (2020) have mixed findings in their investigation of the referee process in economics, with some of their results suggesting bias is present but others showing no evidence of bias. Hengel (2017) finds that female authors face higher editorial standards.

discrimination or differences in preferences by gender in PhD programs that align with prestige), along with the field and university fixed effects. Our research design is not suited to unpack the sources of explanatory power of the control variables in the model; rather, conditional on these variables, we aim to understand the source(s) of the gender gap that remains.

4.2. Gender gaps in wages at entry

Our first hypothesis is that the unexplained portion of the gender wage gap is driven by differences in wages among faculty at entry. This may be derived from gender differences in the willingness to negotiate, among other possibilities. As noted above, this hypothesis does not require panel data and has been tested before with mixed results. Toumanoff (2005) finds a statistically significant gender gap at entry at a private research university, whereas Porter et al. (2008) use broader data from the NSOPF and find no statistical evidence of a gender pay gap at entry.

We estimate the gender pay gap at entry using the same specification as in equation (2), with two modifications. First, we use only on the sample of assistant professors in the original 2016 dataset with wage data, which includes 670 individuals. Second, as noted above, we include experience fixed effects in place of the linear experience variable to more precisely control for each observed level of experience among the assistant professor pool. With the experience fixed effects in the model, our estimate of the gender gap credibly captures experience-conditional differences in wages among the assistant professors in the sample. We hypothesize that the primary driver of any differences will be wages at entry, as large wage changes during the pre-tenure period are rare.

4.3. Gender gaps in promotions or salary increases at the time of promotion

Next we test for evidence of gender differences in promotions and wage increases that coincide with promotions. Previous research finds some evidence of promotion gaps favoring men, including Ginther and Hayes (2003), Sarsons (2014), Weisshaar (2017), and Durodoye Jr. et al.

(2020); although the evidence is mixed across fields and there are exceptions (e.g., Ginther and Kahn 2006).

We evaluate gender gaps in promotion outcomes separately from the assistant-to-associate and associate-to-full levels. The first model is estimated using assistant professors from 2016 only, and the second uses associate professors from 2016 only. The structure of each model is as shown by equation (2).

We measure promotion with a simple binary indicator where '1' indicates that a promotion occurred and '0' indicates that it did not. Faculty who are observed at the rank of assistant professor in 2016 and associate professor in 2018, or at the rank of associate professor in 2016 and full professor in 2018, are coded as having been promoted during the 2-year period. The coding does not depend on where the promotion occurs—i.e., whether at the original or a new institution. If the individual left academia prior to the tenure decision, or if the individual is observed at the same or a new university in 2018 at the same rank as in 2016, we code the promotion variable as '0'—i.e., the promotion did not occur.

Note that a non-promotion outcome does not mean the faculty member has necessarily failed to be promoted. It may simply be that his or her promotion window was not reached during the sample period. That said, our coding structure, combined with experience fixed effects in our model, allows for a comprehensive investigation of experience-conditional promotion gaps by incorporating potential gender gaps in early promotion outcomes. For example, if men are more likely to be put up for promotion early, our model allows for this to influence the gender promotion gap by comparing equally-experienced male and female faculty members at each observed pre-promotion experience level.

We also examine raise-at-promotion outcomes with the rationale that year-to-year wage growth at public universities is generally small, but faculty often receive larger pay increases coinciding with promotion events. For our analysis of raise-at-promotion outcomes, we estimate our models using only individuals who receive a promotion between 2016 and 2018 and do not change universities (the latter condition ensures we have their wage data). Among promoted individuals, we ask whether male or female faculty systematically experience larger wage growth. We again follow the basic modeling structure outlined above for this analysis.

The conditional nature of the raise-at-promotion question—i.e., that we can only answer it for individuals who have been promoted—is such that the promotion and raise-at-promotion output may not be interpretable independently. For example, if there is a gender gap in promotions, it would influence the sample of individuals used to study promotion raises and cause sample selection bias. Accordingly, we present the results from these two analyses in tandem, although as a practical matter we do not find evidence of a gap in promotions in our data, negating this concern.[13]

4.4. *Gender gaps in professional mobility and administrator perceptions of mobility*

The third hypothesis we consider is that male and female faculty differ in their willingness-to-move, or in their perceived willingness-to-move by university administrators. Willingness-to-move is most directly observable in the data by actual moves. For example, if male faculty from the 2016 sample are more commonly observed working at different universities or non-university employers in 2018 than their female colleagues, we might infer that they are more willing to move. However, while useful, this ignores university retention efforts, which can be captured in our data

[13] Another way to address the intertwined relationships between these outcomes would be to estimate general wage growth gaps for all "eligible-for-promotion" faculty (i.e., all assistant and associate professors), regardless of whether a promotion occurred. The wage-growth gap estimate from this regression will embody the combined influence of gender gaps in promotion and promotion raises. However, due to the large number of non-promoted faculty, this estimate will be greatly diluted, making it difficult to use to answer the raise-at-promotion question.

with atypically large raises – i.e. raises to counter an outside offer. Thus, in addition to observed moves, for this portion of our analysis we also code binary variables that capture "large raise" events. Because it is unclear exactly how to define a "large raise," we consider two raise thresholds over the 2-year period of our data panel: (1) ≥ 20 percent, and (2) ≥ 25 percent, both relative to the 2016 wage value and in nominal dollars. Noting that sample average nominal wage growth in the dataset is about 9 percent from 2016 to 2018, these values indicate abnormally high wage growth.

We estimate gender gaps in mobility and large-raise events separately, and combined (i.e., either/or). We restrict the sample for this portion of our analysis to faculty who are tenured as of 2016 (i.e., entirely post-tenure). For mobility outcomes, this restriction limits the influence of less desirable professional moves, such as due to a tenure denial or "counseling out" due to a likely tenure denial, which allows for sharper inference (Barbezat and Hughes 2001).[14] For large raise events, consistent with our point above, these events are uncommon among junior faculty. Just 9 percent of pre-tenure assistant professors who were not promoted prior to 2018 experienced a wage increase of 20 percent or more during the two-year period of our data panel, compared to 17 percent of tenured faculty.

We also estimate models that predict two types of moves: (1) to other academic institutions only, and (2) to any other employer. Unsurprisingly, moves within academia are more common among tenured faculty (the ratio of academic to non-academic moves in the sample is 2:1).

The modeling structure we use here follows from the previous two sections—i.e., we estimate models similar to what is shown in equation (2), using as outcomes a set of binary variables that indicate the occurrence of various mobility and/or large-raise events. Our preferred

[14] Barbezat and Hughes (2001) discuss the general interpretation problems that arise in comparing mobility behaviors between women and men in academia due to the unobserved mix of employee- and employer-driven moves.

specifications continue to include experience fixed effects in place of the linear experience control for the sake of consistency. However, we also acknowledge that models using the linear experience control may be more important because mobility and large-raise events are relatively uncommon, and not especially concentrated among a subset of experience values, so forcing comparisons to occur within the same exact experience cells may be unnecessarily restrictive. That said, as a practical matter, our results are similar regardless of how we control for experience (see below).

4.5. *Selective attrition from the faculty sample by gender and wage*

The last mechanism we consider that may account for the unexplained gender wage gap is differential, selective attrition of faculty over time. Unlike the previous hypotheses, this hypothesis for the measured gender gap is non-substantive, by which we mean that it could generate observed wage gaps in cross-sectional data even if men and women in academia acted and were treated equally in every way (other than their exit likelihoods).

Noting that the unexplained wage gap we observe empirically favors men, the specific types of selective attrition we are looking for are either: (1) among faculty with low earnings, all else equal, men are more likely than women to exit academia, and (2) among faculty with high earnings, all else equal, men are less likely than women to exit academia. If either of these is true, then selective attrition alone could explain gender wage gaps as estimated by cross-sectional data.

To test the gender-composition hypothesis we use the same basic modeling structure shown in equation (2) with small modifications. The new model can be written as:

$$Y_{ijk} = \gamma_0 + \boldsymbol{X_{ijk}\gamma_1} + \boldsymbol{R_i\gamma_2} + G_i\gamma_3 + S_i^{2016}\gamma_4 + G_i * S_i^{2016}\gamma_5 + \pi_j + \varsigma_k + \varepsilon_{ijk} \qquad (3)$$

Like terms in equations (2) and (3) are as defined in equation (2), and as with most of the rest of our analysis, our preferred version of equation (3) includes experience fixed effects. In terms of modifications, first, the dependent variable is now a binary indicator equal to one if the faculty member left academia for any non-academic job between 2016 and 2018. Mobility within academia is coded as 0 for the dependent variable, as this would not cause a compositional change among faculty, broadly considered. Second, we include the 2016 salary level, S_i^{2016}, as an additional control variable. Finally, we add an interaction term between the gender indicator and the 2016 salary level.

The coefficient of interest in equation (3) is γ_5, which captures the effect of the interaction between gender and the 2016 salary level. A positive value of γ_5 would indicate that all else equal, higher-earning female faculty members are more likely to leave academia than their male colleagues; or conversely, that lower-earning female faculty are more likely to stay.

In addition to estimating the specification as shown in equation (3), we also estimate a more detailed version where we replace the continuous S_i^{2016} variable with indicators that divide faculty into three groups based on their place in the unconditional 2016 salary distribution: top quartile, middle two quartiles, or bottom quartile. We omit the middle-quartiles group and use the gender interactions with top- and bottom-quartile earnings to look for evidence of asymmetric attrition by earnings, which would be missed by equation (3). The more-detailed model is as follows, where $\mathbf{S}_{\mathbf{i}}^{\mathbf{Q,2016}}$ is the vector of salary-quartile indicators and $\tilde{\boldsymbol{\gamma}}_{\mathbf{5}}$ is the (2-element) vector of parameters of interest:

$$Y_{ijk} = \tilde{\gamma}_0 + \boldsymbol{X_{ijk}}\tilde{\boldsymbol{\gamma}}_{\mathbf{1}} + \boldsymbol{R_i}\tilde{\boldsymbol{\gamma}}_{\mathbf{2}} + G_i\tilde{\gamma}_3 + \boldsymbol{S_i^{Q,2016}}\tilde{\boldsymbol{\gamma}}_{\mathbf{4}} + G_i * \boldsymbol{S_i^{Q,2016}}\tilde{\boldsymbol{\gamma}}_{\mathbf{5}} + \tilde{\pi}_j + \tilde{\varsigma}_k + \tilde{\varepsilon}_{ijk} \quad (4)$$

5. Results

5.1. Establishment of the Baseline Unexplained Gender Gap

Column (1) of Table 3 replicates the conditional wage gap estimated by Li and Koedel

(2017) for the 2016 cross-section of our dataset.[15] We present this estimate side-by-side with an estimate of the 2016 wage gap in column (2) that uses the subset of 2016 faculty for whom we also have 2018 wage data. This comparison is to establish that the gap exists in the more restricted panel sample with 2018 wage data. The results show that the 2016 conditional gender wage gap we estimate using the subsample of 3,341 faculty for whom we could collect 2018 wage data is within $40 of the gap originally estimated by Li and Koedel (2017) using the full 2016 cross-sectional sample (on a base of about $4,200).

Column (3) of Table 3 shows the gender wage gap for the same faculty in column (2), but, using the 2018 wage data instead. The estimated gap is about $460 lower for these same faculty in 2018, or approximately 10 percent of the 2016 gap. The estimate in column (3) is comfortably within the 95 percent confidence interval of the 2016 estimate in column (2), so should not be taken as statistical evidence that the gap has declined. However, the fact that there is no indication of an *increase* in the unexplained gender wage gap previews the null findings for several of the tests that follow, which look for evidence of factors that would be expected to expand the gap over time.

5.2. Gender gaps in wages at entry

Table 4 shows results from our models that estimate gender wage gaps at entry using the assistant professor sample and 2016 wage data. Column (1) shows a large unconditional gender wage gap among assistant professors—of about $8,200—but with basic controls in column (2), the gap declines by over 70 percent and becomes statistically insignificant (results from the full regression can be found in Appendix Table A-3). Once the basic controls are included in the model the gap settles in at around $2,000, or about 2.4 percent of the average assistant professor wage ($84,700 per

[15] Our replication excludes four observations from their original analytic sample. One is a faculty member with unknown gender and three others are excluded due to a coding error that wrongly listed them as tenure-track faculty. This has no substantive bearing on the replication.

Table 2), and is no longer statistically significant. It does not change much as other controls are added to the model in later columns of Table 4.

In results reported in the appendix, we also allow for an interaction between gender and STEM fields (biology, chemistry, and economics) to see if the gender gap varies along this dimension. We find no evidence of heterogeneity between STEM and non-STEM fields (column 1, Appendix Table A-5).

The estimates in Table 4 do not provide strong statistical support for the presence of a wage gap at entry. The most appropriate interpretation of these results is that they confirm earlier findings from Porter et al. (2008) that there is no evidence of a gap. That said, we also note that the total unexplained cross-sectional gap in Table 3 is about $4,200 and our point estimate from the full specification in Table 4 is around $1,900. A more liberal interpretation of this finding is that we cannot rule out a gender gap at entry as a substantial contributor to the unexplained wage gap. A higher-powered estimate of the gender gap at entry would help to resolve this uncertainty.

5.3. Gender gaps in promotions or salary increases at the time of promotion

Tables 5 and 6 show results from our models of promotion: from assistant to associate in Table 5, and associate to full in Table 6. None of the promotion gaps estimated in Table 5 for assistant professors are statistically significant; and if anything, the point estimate from our full specification (column 5) suggests that female assistant professors are conditionally more likely to be promoted in our data. None of the estimates in Table 6, for promotions from associate to full, are statistically significant either; and the coefficient from the full model is nominally positive (albeit very small). On the whole, the evidence in Tables 5 and 6 is consistent with no systematic differences in promotion likelihoods between male and female faculty.

Like in Section 5.2, we test for and find no clear evidence of heterogeneity between STEM and non-STEM fields in terms of promotion likelihoods. There is suggestive evidence in the model of promotions from associate to full that female STEM faculty are conditionally less likely to be promoted, but the coefficient is significant at only the 10 percent level. These results are provided in the appendix (Table A-5). We also estimate a combined model that pools the promotion samples at the assistant-to-associate and associate-to-full levels in Appendix Table A-4. As would be predicted by the results in Tables 5 and 6, the point estimate from the pooled model is very small, nominally positive, and statistically insignificant. The lack of evidence of a gender gap in promotions in our data contributes to the mixed evidence in the literature.[16]

Next, we turn to the question of whether there is a gender gap in the size of the promotion raise conditional on receiving a promotion. Results in Table 5 and 6 provide no indication of a gender gap in the likelihood of promotion, negating the concern about sample selection bias in the estimates from the raise-at-promotion models.[17] To increase statistical power in the models, we use the pooled sample of promoted individuals rather than estimating them separately by level of promotion. Finally, note that the raise-at-promotion regressions follow the same structure as our preceding models, but the dependent variable is salary growth from 2016 to 2018. We calculate salary growth as the difference between the 2016 and 2018 salary, divided by the 2016 salary, and multiplied by 100.

The results for the raise-at-promotion models are shown in Table 7, where the coefficient of interest should be interpreted in units of percentage points (e.g., 2.0 indicates 2 percent of the 2016

---

[16] The pooled promotion model is the only model in our analysis that controls for faculty rank—in the model, we include an indicator for whether the 2016 rank is assistant professor. The rationale for not including the rank variable in the wage regressions does not apply for the promotion regressions, which are necessarily conditional on the current rank. That said, the inclusion of the rank control in the pooled promotions model does not substantively affect the findings.

[17] Also recall that the raise-at-promotion models require that we observe wages in 2016 and 2018. This means these faculty did not change universities, which may limit generalizability, but we are unable to test this directly.

salary level). None of the estimates for the gender gap are statistically significant and the point estimates are small and positive. There is no indication that female faculty receive smaller raises when promoted. Results of our test for heterogeneity between STEM and non-STEM fields again show no evidence of heterogeneity, consistent with the preceding sections (Appendix Table A-5).[18]

5.4. Gender gaps in professional mobility and administrator perceptions of mobility

Reading across the columns in Table 8 shows output from linear probability models where the dependent variable is equal to one if the following conditions are met, and zero otherwise: (1) the faculty member moved to another university or left academia, (2) the faculty member moved to another university, (3) the faculty member stayed at the original university and received a raise of 20 percent or more, (4) the faculty member stayed at the original university and received a raise of 25 percent or more, (5) the faculty member changed universities or left academia *or* stayed and received a raise of 20 percent or more (i.e., condition (1) or (3)), and (6) the faculty member changed universities or left academia *or* stayed and received a raise of 25 percent or more (i.e., condition (1) or (4)). For ease of exposition, we show results for the full specification only for each outcome. No new substantive insights emerge from the less-complete versions of the models.

Most of the results in Table 8 indicate statistically insignificant differences by gender in the likelihood of moving or the likelihood of receiving a large raise. The one significant coefficient (at the 10 percent level) suggests that women are *more likely* to receive a raise of 20 percent or more. But broadly speaking, the point estimates are small in all specifications and mixed in sign. There is no

[18] A caveat to this finding is that wage data are typically released with a lag, which varies by state. The lag structure is generally handled in the model by the university fixed effects, which also subsume state fixed effects, and for most of our analysis is not impactful. However, for this particular outcome, an implication is that wage increases corresponding to more recent promotions may not be incorporated into our data in all states. There is no reason to expect this measurement error issue to disproportionately affect a particular gender. Given this, and given the nature of the error, the expectation is that to the extent that this is a problem, the (positive) gender-gap estimates reported in Table 7 should be interpreted as attenuated relative to the true values.

indication in the data that female faculty are less likely to move inside or outside of academia, or less likely to experience a large-raise event, than their male peers.

Appendix Tables A-6 and A-7, respectively, show that there is no evidence of heterogeneous gender gaps between STEM and non-STEM fields along these dimensions, and confirm that the findings are substantively similar if we control for experience linearly rather than with fixed effects.

5.5. Selective attrition from the faculty sample by gender and wage

Table 9 shows results from our analysis of selective attrition to assess whether compositional shifts among faculty could drive the observed cross-sectional wage gap. We again focus on the full specification for brevity. Column (1) shows results from equation (3), where we enter salary linearly into the regression, and column (2) shows results from equation (4), where we divide faculty into salary quartiles (where the middle two quartiles are omitted). In the linear regression we divide salary by $10,000 so that the coefficients can be interpreted in terms of changes with respect to salary differences in $10,000 increments.

The linear model gives no indication of a differential attrition rate by gender and salary, as the coefficient on the salary-gender interaction is small and far from statistical significance. The binned model provides further insight. The lack of a relationship on average reflects a very modest but suggestive u-shape attrition pattern for female relative to male faculty, whereby women in the bottom and top quartiles of the salary distribution are less likely to exit academia than their male peers. Only the fourth-quartile (i.e., highest salaries) gender difference is statistically significant, but the first-quartile difference is of the same sign and the point estimate is about half as large in magnitude.

These findings are not directionally consistent with the idea that composition changes drive the cross-sectional gender wage gap identified in previous studies. The highest-earning female faculty exit academia at a relatively lower rate than their male counterparts, not at a higher rate. Over time, this type of attrition will put pressure on the gender wage gap in the opposite direction of what is observed—i.e., all else equal, female faculty will appear to earn more due to the composition shift. However, the level of differential attrition implied by our estimates is small in magnitude, and there are offsetting effects across quartiles that lead to a small and null result in the linear model. We conclude, therefore, that on the whole, differential attrition by gender is not a substantively important factor that drives the measured gender wage gap among faculty.

6. Discussion & Conclusion

We use a recent 2-year data panel of faculty from 40 public research universities to test the extent to which four factors drive the well-documented, unexplained gender wage gap among faculty in cross-sectional data. The four mechanisms are: (1) gender differences at the point of entry into academia, (2) gender differences in the likelihood of promotion, and salary increases conditional on promotion, (3) gender differences in the true or perceived (by university administrators) willingness-to-move, and (4) gender-based composition shifts among faculty correlated with income. We do not find strong support for any of these mechanisms as drivers of the unexplained gender wage gap.

The most suggestive evidence comes from our test of the first mechanism, where we estimate a moderately-sized but statistically insignificant gender wage gap at entry favoring men. This result is consistent with similar findings from Porter et al. (2008), who also find small, insignificant, and directionally-aligned gaps among new faculty using different waves of the NSOPF. A more conclusive and statistically precise test of the gender wage gap at entry would be a valuable addition to the literature. Such a test will require a much larger sample to be able to detect gaps of the magnitudes implied by the point estimates in our study and in Porter et al. (2008), which are on the order or 2-3 percent of the average entering wage. While our initial sample of faculty is large—about

3,800 faculty—just 670 are assistant professors, which contributes to the lack of statistical precision for this portion of our analysis.[19]

In our tests of the other mechanisms, we find small and insignificant differences by gender. Moreover, the signs of our coefficients, if taken at face value, have inconsistent implications for the gender gap. Taken on the whole, our estimates give no indication that the factors we consider negatively impact female relative to male faculty wages. Our mechanism-by-mechanism findings are also consistent with the summary result that over the two-year period of our data panel, the gender wage gap did not increase among the faculty we study.

The primary methodological caveat to our study is that our data panel is short—it covers just a two-year window. All else equal, a longer panel would facilitate a more statistically powerful analysis. That said, the standard errors in most of our models are sufficiently small to rule out moderate gender differences along the dimensions we consider. We also note that although each faculty member is observed over just a two-year period, the faculty members covered by our data collectively span the full career range, allowing us to observe a large number of career events.

There are two substantive potential explanations for our null findings. First, one possibility is that a factor that we do not consider, or a large number of very small factors—too small to be detected individually—combine to account for the total unexplained gap. This seems less likely given that the gender wage gap did not grow among the faculty in our data panel between 2016 and 2018, although we cannot rule it out. A second possibility is that our null findings reflect a shift in the processes by which faculty salaries are set whereby the factors that have contributed to the currently-existing gender gap have changed. This idea can also be expressed in a basic stock-flow

[19] The samples of recently-hired faculty used by Porter et al. (2008) across the NSOPF waves are similarly-sized, ranging from 300-900 faculty.

framework, under which the observed cross-sectional gap is a stock that has built up over many years, but the flows that created the observed stock are now different. To the extent that this is why our findings are null, it implies that over the long run the unexplained gender wage gap will disappear.

We conclude by noting that this discussion applies only to the conditional, unexplained gender wage gap, which is the focus of our analysis. The *explained* (by observables) gender wage gap is large and the factors that drive it are not evaluated here. The three most important observable factors are field of study, measured research productivity, and experience (Li and Koedel 2017). Of these three, the latter is the most likely to recede naturally —it is driven at least in part by the historical prevalence of men in faculty positions, which means that among the current stock of faculty, men on average are more experienced. The other two factors—the prevalence of female faculty in lower-paying fields and the lower measured research productivity of female faculty—are likely the product of a myriad of factors that merit continued attention in research aimed at understanding the gender wage gap in academia.

BIBLIOGRAPHY

Abrevaya, J., & Hamermesh, D. S (2012). Charity and Favoritism in the Field: Are Female Economists Nicer (To Each Other)? *Review of Economics and Statistics,* 94(1), 202-207.

Babcock, L., & Laschever, S. (2003). *Women Don't Ask: Negotiation and the Gender Divide*. Princeton and Oxford: Princeton University Press.

Barbezat, D. A., & Hughes, J. W. (2005). Salary structure effects and the gender pay gap in academia. *Research in Higher Education*, 46(6), 621-640.

Barbezat, D. A., & Hughes, J. W. (2001). The effect of job mobility on academic salaries. *Contemporary Economic Policy*, 19 (4), 409-423.

Bernard, J. (1981). The good-provider role: Its rise and fall. *American Psychologist*, 36(1), 1.

Bertrand, M. (2011). New Perspectives on Gender. In *Handbook of Labor Economics*, Volume 4B, edited by Orley Ashenfelter and David Card, 1543–90. Amsterdam and Boston: Elsevier, North-Holland

Bielby, W. T., & Bielby, D. D. (1992). I will follow him: Family ties, gender-role beliefs, and reluctance to relocate for a better job. *American Journal of Sociology*, 97(5), 1241-1267.

Blank, R.M., (1991). The effects of double-blind versus single-blind reviewing: Experimental evidence from the American Economic Review. *The American Economic Review,* 81(5), 1041-1067.

Bollinger, C.R., Hirsch, B.T., Hokayem, C.M., and Ziliak, J.P. (2019). Trouble in the tails: What we know about earnings nonresponse 30 years after Lillard, Smith, and Welch. *Journal of Political Economy*, 127(5), 2143-2185.

Card, D., DellaVigna, S., Funk, P., & Iriberri, N. (2020). Are Referees and Editors in Economics Gender Neutral? *Quarterly Journal of Economics*, 135(1), 269-337.

Chen, J. J., & Crown, D. (2019). The Gender Pay Gap in Academia: Evidence from the Ohio State University. *American Journal of Agricultural Economics*, 101(5), 1337–1352.

Croson, R., & Gneezy, U. (2009). Gender Differences in Preferences. *Journal of Economic Literature*, 47 (2), 448–74.

Daly, C. J., & Dee, J. R. (2006). Greener pastures: Faculty turnover intent in urban public universities. *The Journal of Higher Education*, 77(5), 776-803.

Durodoye Jr., R., Gumpertz, M., Wilson, A., Griffith, E., & Ahmad, S. (2020). Tenure and promotion outcomes at four large land grant universities: Examining the role of gender, race, and academic discipline. *Research in Higher Education,* 61, 628-651.

Fox, C.W., Burns, C.S., Muncy, A.D., and Meyer, J.A. (2016). Gender differences in patterns of authorship to not affect peer review outcomes at an ecology journal. *Functional Ecology*, 30, 126-139.

Ginther, D. K. (2001). Does science discriminate against women? Evidence from academia, 1973-1997. Federal Reserve Bank of Atlanta Working paper 2001-02.

Ginther, D. K., & Hayes, K. J. (2003). Gender differences in salary and promotion for faculty in the humanities 1977–95. *Journal of Human Resources*, 38(1), 34-73.

Ginther, D.K., and Kahn, S. (2006). Does science promote women? Evidence from academia 1973-2001. NBER Working Paper No. 12691.

Haas, L. (1986). Wives' orientation toward breadwinning: Sweden and the United States. *Journal of Family Issues*, 7(4), 358-381.

Hamermesh, D. S. (2018). Citations in Economics: Measurement, Uses, and Impacts. *Journal of Economic Literature*, 56(1), 115–156.

Hegewisch, A., Liepmann, H., Hayes, J., & Hartmann, H. (2010). Separate and not equal? Gender segregation in the labor market and the gender wage gap. Briefing paper IWPR C377. Institute for Women's Policy Research.

Hengel, E. (2017). Publishing while Female: Are Women Held to Higher Standards? Evidence from Peer Review. Unpublished manuscript.

Lane, J. A., & Linden, D. J. (2009). Is there gender bias in the peer review process at Journal of Neurophysiology? *Journal of Neurophysiology,* 101(5), 2095-2196

Laughter, J. (2018). Race in Educational Researcher: A Technical Comment on Li and Koedel (2017). *Educational Researcher*, 47(4), 259-261.

Li, D., & Koedel, C. (2017). Representation and salary gaps by race-ethnicity and gender at selective public universities. *Educational Researcher*, 46(7), 343-354.

Li, D., & Koedel, C. (2018). A Technical Comment on Li and Koedel (2017): Author Response. *Educational Researcher*, 47(4), 262-263

Macpherson, D. A., & Hirsch, B. T. (1995). Wages and gender composition: why do women's jobs pay less? *Journal of labor Economics*, 13(3), 426-471.

Perna, L. W. (2001). Sex Differences in Faculty Salaries: A Cohort Analysis. *The Review of Higher Education*, 24 (3), 283-307

Perry, M., & Reny, P. J. (2016). How to count citations if you must. *American Economic Review*, 106 (9), 2722-2741

Porter, S. R., Toutkoushian, R. K., & Moore III, J. V. (2008). Pay inequities for recently hired faculty, 1988-2004. *The Review of Higher Education*, 31(4), 465-487.

Sarsons, H. (2017). Recognition for group work: Gender differences in academia. *American Economic Review*, 107(5), 141-45.

Taylor, L. L., Lahey, J. N., Beck, M. I., & Froyd, J. E. (2020). How to Do a Salary Equity Study: With an Illustrative Example from Higher Education. *Public Personnel Management*, *49*(1), 57-82.

Toumanoff, P. (2005). The effects of gender on salary-at-hire in the academic labor market. *Economics of Education Review*, 24(2), 179-188.

Toutkoushian, R. K. (1998a). Sex matters less for younger faculty: Evidence of disaggregate pay disparities from the 1988 and 1993 NCES surveys. *Economics of Education Review*, 17(1), 55-71.

Toutkoushian, R. K. (1998b). Racial and marital status differences in faculty pay. *Journal of Higher Education*, 69(5), 513-541.

Toutkoushian, R. K., & Conley, V. M. (2005). Progress for women in academe, yet inequities persist: Evidence from NSOPF: 99. *Research in Higher Education*, 46(1), 1-28.

Umbach, P. D. (2006). Gender equity in the academic labor market: An analysis of academic disciplines. *Research in Higher Education*, 48(2), 169–192.

Weisshaar, K. (2017). Publish and perish? An assessment of gender gaps in promotion to tenure in academia. *Social Forces.* 96(2), 529-560.

Table 1A. Data construction of the Analytic sample

Sample and reasons for data loss	Sample size		
2016 analytical sample	3,805		(I)
2018 salary missing or excluded, in which	460		
cannot find salary		*58*	
retirees		*138*	
salary excluded [-30%; +100%]		*104*	
pre-tenure movers		*67*	
post-tenure movers		*93*	
movers within academia		*109*	
mover outside academia		*51*	
Other reasons *	4		
Final analytic sample (2-period salary data)	3,341		(II)

Table 1B. Sample size for each test

Rank and changes	Headcounts in 2016			
	1-period salary (I)		2-period salary (II)	
Assistant professor	670	(1)	548	(4)
Associate professor	1,109	(2)	1,010	(5)
Full professor	2,022	(3)	1,783	(6)
Promoted to associate professors	164	(7)	142	(9)
Promoted to full professors	182	(8)	156	(10)
Post-tenure moves	93	(11)		
Retirees	138	(12)		
Sample to test gender wage gaps at entry = (1)	670			
Sample to test gender wage gaps in promotion = (1) + (2)	1,779			
Sample to test raise-at-promotion = (9) +(10)	298			
Sample to test departure or willing to move = (2) +(3)-(12)	2,993			
Sample to test large-raise or move = (5) +(6)	2,793			
Sample to test either large-raise or move = (5) +(6) +(11)	2,886			
Sample to test attrition from academia = (1) +(2) +(3)-(12)	3,663			

Note: * 4 faculty members (3 were not tenure tracked, 1 was of non-identified gender) were removed from the analytical sample.

Table 2. Descriptive statistics

Variable	Full sample	Full sample with 2-period salary panel	Asst. prof 2016	Asst. prof. promoted	Assoc. prof 2016	Assoc. prof. promoted	Non-retired Movers	Non-retired Post-tenure movers
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Salary in 2016	120,649 (52,761)	122,306 (52,669)	84,706 (23,776)	88,602 (21,875)	93,232 (25,451)	104,555 (34,573)	114,226 (49,813)	130,096 (56,910)
Salary in 2018	132,734 (57,572)	133,370 (57,262)	93,673 (23,376)	98,743 (25,973)	102,234 (29,846)	117,902 (41,718)		
Salary raise of more than 15% within 2 years	0.21 (0.41)	0.21 (0.41)	0.20 (0.40)	0.35 (0.48)	0.23 (0.42)	0.42 (0.49)		
Salary raise of more than 20% within 2 years	0.13 (0.33)	0.13 (0.33)	0.11 (0.32)	0.18 (0.38)	0.14 (0.35)	0.29 (0.46)		
Salary raise of more than 25% within 2 years	0.04 (0.20)	0.08 (0.28)	0.07 (0.26)	0.11 (0.31)	0.09 (0.29)	0.15 (0.36)		
Salary missing or excluded in 2018, of which	0.12 (0.33)		0.18 (0.39)	0.13 (0.34)	0.09 (0.29)	0.14 (0.35)		
Mover	0.04 (0.20)		0.10 (0.30)	0.08 (0.27)	0.03 (0.17)	0.09 (0.29)	1.00 (0)	1.00 (0)
To academic institutions	0.03 (0.17)		0.07 (0.25)	0.08 (0.27)	0.02 (0.14)	0.09 (0.29)	0.67 (0.47)	0.67 (0.47)
To industry	0.01 (0.12)		0.03 (0.18)	-	0.01 (0.1)	-	0.32 (0.47)	0.31 (0.47)
Retiree	0.04 (0.18)		-	-	0.03 (0.16)	-		
Salary 2018 out-of-range	0.03 (0.16)		0.06 (0.24)	0.03 (0.15)	0.02 (0.14)	0.03 (0.18)		
Cannot find salary for other reason	0.01 (0.12)		0.02 (0.14)	0.03 (0.17)	0.01 (0.10)	0.02 (0.16)		
Promoted	0.09 (0.29)	0.09 (0.28)	0.24 (0.43)	-	0.16 (0.37)	-	0.19 (0.39)	0.18 (0.39)
Gender								
Male	0.65 (0.48)	0.65 (0.48)	0.58 (0.49)	0.57 (0.5)	0.56 (0.5)	0.6 (0.49)	0.64 (0.48)	0.66 (0.48)
Female	0.35 (0.48)	0.35 (0.48)	0.42 (0.49)	0.43 (0.5)	0.44 (0.5)	0.4 (0.49)	0.36 (0.48)	0.34 (0.48)
Field								
Biology	0.32 (0.47)	0.33 (0.47)	0.33 (0.47)	0.3 (0.46)	0.29 (0.46)	0.34 (0.47)	0.22 (0.42)	0.22 (0.41)
Chemistry	0.14 (0.35)	0.15 (0.35)	0.14 (0.35)	0.18 (0.39)	0.09 (0.28)	0.1 (0.3)	0.13 (0.34)	0.14 (0.35)
Economics	0.13 (0.34)	0.13 (0.33)	0.19 (0.39)	0.18 (0.38)	0.11 (0.31)	0.13 (0.33)	0.25 (0.44)	0.14 (0.35)
Education (leadership/policy)	0.07 (0.25)	0.06 (0.25)	0.08 (0.27)	0.08 (0.27)	0.08 (0.27)	0.08 (0.28)	0.08 (0.27)	0.11 (0.31)
English	0.23 (0.42)	0.22 (0.42)	0.15 (0.36)	0.16 (0.37)	0.32 (0.47)	0.19 (0.39)	0.21 (0.41)	0.31 (0.47)
Sociology	0.11 (0.31)	0.11 (0.31)	0.10 (0.30)	0.1 (0.31)	0.11 (0.31)	0.17 (0.38)	0.1 (0.3)	0.09 (0.28)
PhD school rank								
PhD school U.S 1-10	0.24 (0.43)	0.24 (0.43)	0.24 (0.42)	0.23 (0.42)	0.22 (0.41)	0.23 (0.42)	0.24 (0.43)	0.24 (0.43)
PhD school U.S. 11-50	0.35 (0.48)	0.35 (0.48)	0.36 (0.48)	0.34 (0.47)	0.37 (0.48)	0.34 (0.48)	0.32 (0.47)	0.3 (0.46)
PhD school U.S. 50+	0.26 (0.44)	0.26 (0.44)	0.24 (0.43)	0.27 (0.45)	0.27 (0.45)	0.25 (0.44)	0.2 (0.4)	0.18 (0.39)
PhD school outside U.S.	0.11 (0.32)	0.11 (0.31)	0.14 (0.35)	0.14 (0.35)	0.09 (0.29)	0.13 (0.34)	0.18 (0.39)	0.17 (0.38)
PhD school missing	0.02 (0.15)	0.02 (0.15)	0.01 (0.09)	0.01 (0.08)	0.04 (0.18)	0.03 (0.16)	0.03 (0.17)	0.05 (0.23)
No PhD (English only)	0.02 (0.12)	0.01 (0.12)	0.01 (0.09)	0.02 (0.13)	0.01 (0.11)	0.02 (0.15)	0.03 (0.17)	0.05 (0.23)
Experience in 2016	22.3 (12.06)	22.12 (11.67)	8.12 (4.95)	9.71 (3.5)	17.81 (8.16)	16.7 (5.84)	15.28 (10.34)	20.4 (9.88)
Research productivity in 2016								
Scopus publications	49.68 (81.42)	50.57 (81.74)	16.98 (19.16)	25.2 (28.09)	22.24 (23.43)	31.85 (29.86)	30.29 (49.16)	42.05 (60.14)
Scopus citations	2,059 (4,375)	2,139 (4,488)	628 (1,318)	1,016 (2,135)	783 (1,350)	1,263 (1,841)	1,175 (2,436)	1,728 (3,012)
H-index	15.51 (15.98)	15.91 (16.15)	7.79 (7.32)	9.94 (9.44)	9.38 (8.83)	12.59 (10.34)	10.67 (13.17)	13.75 (15.58)
Scopus missing	0.06 (0.23)	0.05 (0.22)	0.12 (0.32)	0.04 (0.2)	0.06 (0.24)	0.06 (0.24)	0.09 (0.28)	0.06 (0.25)
Race								
White	0.79 (0.4)	0.8 (0.4)	0.69 (0.46)	0.7 (0.46)	0.74 (0.44)	0.67 (0.47)	0.67 (0.47)	0.71 (0.46)
Black	0.05 (0.21)	0.05 (0.21)	0.05 (0.22)	0.06 (0.24)	0.08 (0.27)	0.11 (0.31)	0.09 (0.28)	0.11 (0.31)
Asian	0.12 (0.32)	0.12 (0.32)	0.2 (0.4)	0.18 (0.39)	0.12 (0.33)	0.17 (0.38)	0.17 (0.38)	0.11 (0.31)
Hispanic	0.04 (0.19)	0.04 (0.19)	0.06 (0.24)	0.05 (0.23)	0.05 (0.22)	0.05 (0.22)	0.05 (0.22)	0.04 (0.2)
Other/unknown	0 (0.06)	0 (0.05)	0 (0)	0 (0)	0.01 (0.08)	0 (0)	0.02 (0.14)	0.03 (0.18)
Total	3,801	3,341	670	164	1,109	182	161	93

Note: When dependent variables are salary or large-raise, those with salary missing or excluded will drop for the regressions; when dependent variable is promotion, those with salary missing or excluded are kept in the regression.

Table 3. Conditional wage gaps

Variables	(1) 2016 salary (on full sample)	(2) 2016 salary (on 2-period salary sample)	(3) 2018 salary (on 2-period salary sample)
Female	-4,238.59***	-4,202.29***	-3,742.85***
	(1,077.31)	(1,069.61)	(1,106.25)
Observations	3,801	3,341	3,341
R-squared	0.54	0.57	0.57
Race-ethnicity	X	X	X
University fixed effects	X	X	X
Field fixed effects	X	X	X
PhD school rank	X	X	X
Experience (linear)	X	X	X
Research productivity	X	X	X

Note: Robust standard errors in parentheses, clustered at university level
*** p<0.01, ** p<0.05, * p<0.1

Table 4. Gender wage gaps at entry

Variables	(1)	(2)	(3)	(4)
	2016 salary			
Female	-8,522.05***	-2,497.79	-2,294.71	-1,982.44
	(2,595.58)	(1,655.82)	(1,625.67)	(1,593.09)
Observations	670	670	670	670
R-squared	0.06	0.60	0.60	0.61
Experience (fixed effects)	X	X	X	X
Race-ethnicity		X	X	X
Field fixed effects		X	X	X
University fixed effects		X	X	X
PhD School rank			X	X
Publications				X

Note: Robust standard errors in parentheses, clustered at years of experience. Only sample assistant professors in year 2016
*** p<0.01, ** p<0.05, * p<0.1

Table 5. Gender gaps in promotion from assistant to associate professors

	(1)	(2)	(3)	(4)	(5)
VARIABLES	Promoted to associate professor				
Female	0.02	0.00	0.01	0.01	0.04
	(0.03)	(0.04)	(0.04)	(0.04)	(0.04)
Observations	670	670	670	670	670
R-squared	0.00	0.07	0.12	0.12	0.17
Experience (fixed effects)	X	X	X	X	X
Race-ethnicity		X	X	X	X
Field fixed effects		X	X	X	X
University fixed effects			X	X	X
PhD School rank				X	X
Research productivity					X

Note: Robust standard errors in parentheses, clustered at years of experience. Only sample assistant professors in 2016.
*** p<0.01, ** p<0.05, * p<0.1

Table 6. Gender gaps in promotion from associate to full professors

	(1)	(2)	(3)	(4)	(5)
VARIABLES	Promoted to full professor				
Female	-0.03	-0.02	-0.01	-0.01	0.00
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Observations	1,109	1,109	1,109	1,109	1,109
R-squared	0.00	0.03	0.08	0.09	0.14
Experience (fixed effects)	X	X	X	X	X
Race-ethnicity		X	X	X	X
Field fixed effects		X	X	X	X
University fixed effects			X	X	X
PhD School rank				X	X
Research productivity					X

Note: Robust standard errors in parentheses, clustered at years of experience. Only sample associate professors in 2016.
*** p<0.01, ** p<0.05, * p<0.1

Table 7. Gender gaps in raise-at-promotion

	(1)	(2)	(3)	(4)
VARIABLES	Salary increase (percentage points of 2016 salary)			
Female	3.15	2.34	2.22	2.14
	(1.90)	(1.69)	(1.73)	(1.77)
Observations	298	298	298	298
R-squared	0.02	0.38	0.38	0.40
Experience (fixed effects)	X	X	X	X
Race-ethnicity		X	X	X
Field fixed effects		X	X	X
University fixed effects		X	X	X
PhD School rank			X	X
Research productivity				X

Note: Robust standard errors in parentheses, clustered at years of experience. Only sample professors who get promoted within 2 years are included in the models.
*** p<0.01, ** p<0.05, * p<0.1

Table 8. Gender gaps in mobility or administration perceptions of mobility

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Move (to another university or left academia)	Move to another university	Stay and receive a raise (>=20%)	Stay and receive a raise (>=25%)	Move or stay and receive a raise (>=20%)	Move or stay and receive a raise (>=25%)
Female	-0.0033	-0.0002	0.0234*	0.0045	0.0192	-0.0003
	(0.0064)	(0.0051)	(0.0125)	(0.0121)	(0.0131)	(0.0114)
Observations	2,993	2,993	2,794	2,794	2,886	2,886
R-squared	0.04	0.03	0.12	0.10	0.10	0.08
Experience (fixed effects)	X	X	X	X	X	X
Race-ethnicity	X	X	X	X	X	X
Field fixed effects	X	X	X	X	X	X
University fixed effects	X	X	X	X	X	X
PhD School rank	X	X	X	X	X	X
Research productivity	X	X	X	X	X	X

Note: Robust standard errors in parentheses, clustered at years of experience.
*** p<0.01, ** p<0.05, * p<0.1

Table 9. Selective attrition from academia by gender

	(1)	(2)
VARIABLES	Left academia	
Female	0.0024	0.0023
	(0.0108)	(0.0055)
2016 Salary (in $10,000s)	-0.0002	
	(0.0004)	
Female * 2016 Salary (in $10,000s)	-0.0005	
	(0.0007)	
2016 Salary first quartile		0.0086
		(0.0079)
Female * 2016 Salary first quartile		-0.0096
		(0.0091)
2016 Salary forth quartile		0.0027
		(0.0070)
Female * 2016 Salary forth quartile		-0.0156**
		(0.0070)
Observations	3,663	3,663
R-squared	0.04	0.04
Experience (fixed effects)	X	X
Race-ethnicity	X	X
Field fixed effects	X	X
University fixed effects	X	X
PhD School rank	X	X
Research productivity	X	X

Note: Robust standard errors in parentheses, clustered at years of experience.
*** p<0.01, ** p<0.05, * p<0.1

APPENDIX

Appendix Table A-1. Sample of Universities and Departments

	Biology	Chemistry	Economics	Education (Leadership/ Policy)	English	Sociology
University of California-Berkeley				X	X	X
University of California-Los Angeles		X	X	X		
University of Virginia			X	X	X	
University of Michigan-Ann Arbor			X	X		X
University of North Carolina-Chapel Hill		X	X			X
College of William & Mary		X	X		X	
Georgia Institute of Technology	X		X			X
University of California-Santa Barbara	X				X	X
University of California-Irvine	X	X	X			
University of California-San Diego	X				X	X
University of Illinois-Urbana-Champaign	X				X	X
University of Wisconsin-Madison		X		X		X
University of Florida	X		X		X	
Ohio State University-Columbus			X	X	X	
University of Texas-Austin		X		X		X
University of Washington	X		X	X		
University of Connecticut	X	X	X			
University of Maryland-College Park	X	X				X
Clemson University	X			X		X
Purdue University-West Lafayette	X		X	X		
University of Georgia		X		X	X	
University of Minnesota-Twin Cities	X		X	X		
Texas A&M University-College Station		X		X	X	
Virginia Tech	X			X	X	
Rutgers University-New Brunswick	X			X		X
Indiana University-Bloomington			X	X	X	
Michigan State University	X	X	X			
University of Massachusetts-Amherst	X		X		X	
Miami University-Oxford	X		X			X
University of Iowa		X	X		X	
Binghamton University-SUNY	X	X	X			
North Carolina State University-Raleigh	X		X		X	
Stony Brook University-SUNY	X				X	X
University of Vermont	X		X			X
Florida State University				X	X	X
University at Buffalo-SUNY		X		X	X	
University of Missouri		X		X	X	
University of Nebraska-Lincoln	X	X				X
University of Oregon			X	X	X	
Iowa State University	X	X				X
Total Departments	23	17	22	20	20	18

Notes: Our sampling design is such that we would expect to collect data from 20 departments in each field. The small deviations from the expected number by field are the result of sampling variability.

Table A-2: Full output from the baseline unexplained gender gap on 2-period salary sample in 2016

VARIABLES	(1) 2016 salary (on 2-period salary sample)
Female	-4,202.29***
	(1,069.61)
Black	3,297.43
	(2,926.32)
Asian	-1,663.62
	(1,418.62)
Hispanic	1,823.68
	(2,165.74)
Race other/Unknown	-11,426.24
	(16,808.58)
Chemistry	11,550.93**
	(4,836.50)
Economics	42,992.69***
	(5,859.93)
Education Leadership / Policy	-11,742.36***
	(3,641.05)
English	-14,239.24***
	(3,317.42)
Sociology	-1,655.39
	(2,709.16)
Ph.D. School U.S. 11-50	-702.75
	(1,286.08)
Ph.D. School U.S. 50+	65.69
	(2,028.46)
Ph.D. School Outside U.S.	-2,824.24
	(2,255.35)
Ph.D. School Missing	-5,987.31
	(3,786.84)
No Ph.D. (English only)	8,756.24*
	(4,893.36)
Experience (linear)	1,246.35***
	(95.94)
Experience via Website, non-CV (indicator)	5,843.30
	(5,396.11)
Experience via Scopus (indicator)	4,416.74
	(2,818.99)
Experience Missing (indicator)	-12,092.81
	(9,619.23)
Standardized h-index	21,995.83***
	(1,679.53)
Chemistry*std h-index	4,394.91
	(3,767.53)
Economics*std h-index	11,501.98***
	(3,293.04)
Education*std h-index	-14,846.73***
	(3,179.50)
English*std h-index	-15,189.32***
	(2,162.75)
Sociology*std h-index	-2,561.79
	(3,574.15)
Scopus Missing (indicator)	-12,577.05***
	(3,575.01)
Constant	111,066.21***
	(3,361.24)
Observations	3,341
R-squared	0.57

Note: Omitted groups are whites, men, biologists and faculty from top-10 Ph.D. institutions. Standard errors clustered at the university level are reported in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table A-3: Full output from the gender wage gap at entry

	(1)
VARIABLES	2016 salary
Female	-1,982.44
	(1,593.09)
Black	2,324.05
	(1,800.23)
Asian	598.45
	(1,768.17)
Hispanic	2,858.70
	(3,872.60)
Chemistry	-2,459.96
	(1,923.25)
Economics	34,371.11***
	(3,975.60)
Education	-8,146.58**
	(3,187.83)
English	-17,230.51***
	(4,103.00)
Sociology	-9,788.04***
	(2,391.76)
Ph.D. School U.S. 11-50	-1,324.33
	(2,441.03)
Ph.D. School U.S. 50+	-4,189.71*
	(2,224.91)
Ph.D. School Outside U.S.	-2,789.60
	(3,085.90)
Ph.D. School Missing	8,098.23*
	(4,523.86)
No Ph.D. (English only)	-1,834.87
	(6,934.62)
Standardized publication	5,610.88**
	(2,235.31)
Experience (missing, indicator)	-14,926.42**
	(6,248.31)
Scopus (missing, indicator)	-4,804.47**
	(2,322.58)
Experience (website, indicator)	1,624.48
	(6,338.19)
Experience (Scopus, indicator)	-4,338.78**
	(2,045.77)
Constant	102,086.92***
	(7,073.65)
Observations	670
R-squared	0.61
University fixed effects	X

Note: Omitted groups are whites, men, biologists and faculty from top-10 Ph.D. institutions. Standard errors clustered at the university level are reported in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table A-4: Full output from the estimates of gender gap at promotion (to both associate and full professor)

VARIABLES	(1) Promoted (excluding full prof.)
Female	0.01
	(0.02)
Black	0.12***
	(0.04)
Asian	0.05*
	(0.03)
Hispanic	0.02
	(0.04)
Race other/ Unknown	-0.09**
	(0.04)
Ph.D. School U.S. 11-50	-0.02
	(0.03)
Ph.D. School U.S. 50+	0.00
	(0.03)
Ph.D. School Outside U.S.	0.00
	(0.04)
Ph.D. School Missing	0.08
	(0.07)
No Ph.D. (English only)	0.34***
	(0.11)
Standardized h-index (2018)	0.30***
	(0.04)
Chemistry	0.02
	(0.08)
Economics	-0.07
	(0.08)
Education Leadership / Policy	-0.19***
	(0.06)
English	-0.26***
	(0.04)
Sociology	-0.12**
	(0.06)
Chemistry*standardized h-index	-0.02
	(0.07)
Economics*standardized h-index	-0.06
	(0.09)
Education*standardized h-index	-0.22***
	(0.06)
English*standardized h-index	-0.29***
	(0.04)
Sociology*standardized h-index	-0.24***
	(0.07)
Experience (missing, indicator)	-0.08
	(0.06)
Scopus (missing, indicator)	-0.10***
	(0.04)
Experience (website, indicator)	0.00
	(0.07)
Experience (Scopus, indicator)	-0.04
	(0.04)
Assistant professor (indicator)	0.14***
	(0.04)
Constant	0.34***
	(0.07)
Observations	1,779
R-squared	0.09
Experience (fixed effects)	X
University fixed effects	X

Note: Omitted groups are whites, men, biologists, associate professors, and faculty from top-10 Ph.D. institutions. Standard errors clustered at the experience level are reported in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table A-5: Heterogeneity tests of different gender gaps in STEM vs. non-STEM fields

VARIABLES	(1) 2016 salary, Assistant Professors	(2) Promotion to Associate Professor, Assistant Professors	(3) Promotion to Full Professor, Associate Professors	(4) Promoted, Assistant & Associate Professors	(5) 2-year salary increase, 2016-2018 (percentage point change)
Female	-2,591.96	0.06	-0.03	-0.01	1.65
	(1,530.10)	(0.04)	(0.03)	(0.03)	(4.49)
Female * STEM	-965.37	0.03	-0.07*	-0.05	-0.83
	(2,278.85)	(0.05)	(0.04)	(0.04)	(5.32)
Observations	670	670	1,109	1,779	298
R-squared	0.61	0.19	0.14	0.09	0.4
Experience (fixed effects)	X	X	X	X	X
Race-ethnicity	X	X	X	X	X
Field fixed effects	X	X	X	X	X
University fixed effects	X	X	X	X	X
PhD School rank	X	X	X	X	X
Publications	X	X	X	X	X

Note: Omitted groups are whites, men, biologists, associate professors and faculty from top-10 Ph.D. institutions. Standard errors clustered at the experience level are reported in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

Table A-6: Heterogeneity tests of gender gaps in mobility in STEM vs. non-STEM fields

VARIABLES	(1) Move (to another university or left academia)	(2) Move to another university	(3) Stay and receive a raise (>=20%)	(4) Stay and receive a raise (>=25%)	(5) Move or stay and receive a raise (>=20%)	(6) Move or stay and receive a raise (>=25%)
Female	0.0000	0.0001	0.0163	-0.0036	0.0160	-0.0050
	(0.0114)	(0.0092)	(0.0164)	(0.0137)	(0.0166)	(0.0141)
Female * STEM	0.0065	0.0006	-0.0137	-0.0156	-0.0062	-0.0092
	(0.0122)	(0.0101)	(0.0298)	(0.0237)	(0.0296)	(0.0249)
Observations	2,993	2,993	2,794	2,794	2,886	2,886
R-squared	0.04	0.03	0.12	0.10	0.10	0.08
Experience fixed effects	X	X	X	X	X	X
Race-ethnicity	X	X	X	X	X	X
Field fixed effects	X	X	X	X	X	X
University fixed effects	X	X	X	X	X	X
PhD School rank	X	X	X	X	X	X
Research productivity	X	X	X	X	X	X

Note: Omitted groups are whites, men, biologists, associate professors and faculty from top-10 Ph.D. institutions. Standard errors clustered at the experience level are reported in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table A-7: Gender gaps in mobility or administration perception of mobility with linear experience control

VARIABLES	(1) Move (to another university or left academia)	(2) Move to another university	(3) Stay and receive a raise (>=20%)	(4) Stay and receive a raise (>=25%)	(5) Move or stay and receive a raise (>=20%)	(6) Move or stay and receive a raise (>=25%)
Female	-0.0042	-0.0011	0.0272**	0.0056	0.0214*	0.0000
	(0.0069)	(0.0057)	(0.0104)	(0.0080)	(0.0123)	(0.0111)
Observations	2,993	2,993	2,794	2,794	2,886	2,886
R-squared	0.04	0.04	0.14	0.11	0.13	0.10
Race-ethnicity	X	X	X	X	X	X
Experience (linear control)	X	X	X	X	X	X
Field fixed effects	X	X	X	X	X	X
University fixed effects	X	X	X	X	X	X
PhD School rank	X	X	X	X	X	X
Research productivity	X	X	X	X	X	X

Note: Omitted groups are whites, men, biologists, associate professors, and faculty from top-10 Ph.D. institutions. Standard errors clustered at the university level are reported in parentheses

*** p<0.01, ** p<0.05, * p<0.1