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Title: Academic wage structure by gender-the roles of peer review, performance and market forces

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Abstract:

We focus on understanding the role of productivity in determining wage structure differences between men and women in academia. The data arise from a pay-equity study carried out in a single Midwestern U.S. university over the 1996-7 academic year. Econometric results confirm that external market forces exert influence over both male and female salary. But peer review ratings play a significant role in male but not female earnings determination, with similar results for objective measures of research, teaching and service.

1. Introduction

The paper contributes to the growing literature on wage determination within academia and on the roles of individual productivity and external market salaries as a proxy for market forces. The data arise from an internally sponsored pay-equity study carried out in a single Midwestern U.S. university over the 1996-7 academic year. The institutional data set has undergone extensive cleaning in cooperation with the faculty records office of the university. This ensures less measurement error and more accurate measures of productivity than in a typical study. Our focus is upon understanding the role of productivity in determining wage structure and salary differences between men and women in these specific academic environs.

The data provide a rich array of standard human capital variables in addition to an external market salary measure and two complementary measures of academic productivity: peer review ratings and a set of objective measures of research output, grant funding, teaching and service awards. The external market salary data are matched to individual faculty by rank and discipline. This connects the university under study to the marketplace and allows us to assess the extent to which academic pay at the university in question is primarily driven by market forces. Given the idiosyncratic nature of disciplines, comparison of academic productivity is inherently challenging. Peer review may solve, in part, the problem of across-discipline comparisons but is intrinsically subjective; the measures of research output are objective but assume feasibility of across-discipline comparisons.

We use traditional human capital based Mincer (1974) earnings regressions to examine the role of academic productivity in explaining gender differences in wage structure at the university in question. To our knowledge the current study is the first to incorporate such a wide variety of individual productivity measures including peer-review and external market salary on a U.S. sample across a broad set of academic disciplines. It allows us to gauge the full range of possible productivity impacts on academic salary by gender. We find that external market salary has a strong and significant effect on both male and female salary determination. In contrast the complementary objective productivity measures and peer review tend to be significant determinants of male but not female salary.

The main advantage of our data set, apart from including detailed personnel information about a broad set of faculty across disciplines, is that the data includes, for a large subset of the sample, subjective peer reviews of productivity and, for a smaller subset, various objective (but not quality-adjusted) indicators of productivity (articles, books, grants, service and teaching awards). The data has the disadvantage that it is a single cross-section and is from a single university. Can our results be extrapolated to the US academic market generally? The university in question has conducted gender equity pay reviews on an occasional basis, is in good standing with the American Association of University Professors (AAUP), has not had any discrimination cases go to court and views itself as a progressive institution of higher education in these regards. It seems reasonable to view this university as a typical American research university and apply the lessons learned here to the U.S. academic labor market. The period considered, 1996-97, while not as recent as one might like, is quite comparable to the period examined by the studies noted below, which include data spanning 1968 to 2001.

2. Previous Literature: Academic Salary by Gender

Before examining the empirical literature, consider the following explanations for the existence of gender pay differentials advanced in the absence of productivity differences. Milgrom and Oster's (1987) invisibility hypothesis suggests there is an incentive for employers to hide able but 'invisible' employees, such as women and minorities, from other potential employers. Promotion leads to greater visibility and hence higher risk of outside offers. Valian (1998) introduces the notion of a gender schema, similar to a stereotype, involving the adoption of unconscious hypotheses about males and females. Valian reports a 1991 experiment by Biernat, Manis, and Nelson in which evaluators systematically over-estimate (under-estimate) height of males (females) to fit their preconceived notions. Another experiment shows that when the number of female applicants for a managerial job is low, they are evaluated by male and female MBA students as being less qualified. Ex ante perceptions of greater male productivity may become self-fulfilling in salary structure. Bjerk (2008) models a dynamic version of a standard signaling game. Workers from two groups have differing opportunities to signal higher-level productivity while in entry-level jobs and the skill-signals of women are evaluated less precisely by male managers than those of their male colleagues. The dynamic aspect of the model ensures that firms eventually learn about the skills of their employees. But the delay in female recognition and promotion contributes to the glass ceiling/sticky floor phenomenon and, presumably, in the interim, a lack of reward for productivity. Finally, modern models of monopsony emphasize an upward sloping labor supply curve due to search frictions and dynamic effects. [See Manning (2003) and Ashenfelter et al (2010)] If females are less mobile, males will receive and more easily exploit external wage offers.

Alternatively, salary differences by gender may be due largely to productivity differences. In an innovative study of Israeli private sector firms Hellerstein and Neumark (1999) conclude that once productivity is properly measured, gender differences explain the majority of the industry gender wage differential. Clearly the measurement of academic productivity within a given field of academia, never mind across fields, is a challenge. But a number of studies of the academic labor market have taken up this challenge.[1]

Studies with a broad set of disciplines

Barbezat (1991) examines national surveys of U.S. faculty taken between 1968 and 1989. The first three of these (1960's and 1970's) predate the large influx of women into the professorial ranks. Barbezat reports that the number of articles published has a significant positive effect on salary for males but that is true for women only after a threshold of four has been attained. Up to four books published are significant positive factors for both male and female salaries but publishing additional books benefits only males, pointing to possible differences in the more senior ranks.

Lindley et al (1992) examine salary gain among University of Alabama faculty in the U.S.A. from 1981 to 1985, finding that books and exhibition/performances have a positive but insignificant effect overall and that in general refereed articles have a significant positive effect; there is no significant difference by gender.

Ginther and Hayes (1999, 2003) examine a sample of U.S. faculty in the humanities using, due to data limitations, an average annual rate of productivity, assuming a constant publication stream over the career. They find that articles, books, and chapters in books are significant positive determinants of salary for female academics but only the latter two are significant or sizable for males. The humanities comprise several similar disciplines; whether these results easily generalize to a broader set of disciplines is an open question.

Euwals and Ward (2005) look at a broader set of disciplines at five Scottish universities from 1994-95 to 1995-96, finding that research publications, grants, self-reported teaching quality, and time out of the profession are all important determinants of salary. A gender dummy variable is included and found to be insignificant. Because the salary scales for all ranks below professor are set through a nationally negotiated fixed salary structure it is unclear if the results apply to other contexts.

The study across disciplines that is most comparable to ours is the Binder et al [2010] study that, like ours considers a single U.S. university. They focus on objective measures of teaching and research (but not service); their study not only considers quantity of articles produced but introduces quality via the Journal Citation Report Impact Factor; no quality adjustment is provided for books. They have no overall measure of productivity.Cumulative course preps and number of current graduate students are significant for both sexes. Articles in the last five years (cumulative grant dollars) are significant (weakly significant) in male but not female earnings regressions, and books in the last five years and article citation impact in the last five years are significant for the earnings of women but not men.

Single discipline studies

Ginther and Kahn (2004) analyze two small U.S. longitudinal data sets on economists between 1988-89 and 1972-2001. They are able to apply a quality adjustment to the publication measures. This is an advantage of analyzing a single field, partially offset by worries about how representative the study is and the correspondingly small sample size. They find that female economists are less likely than males to be granted academic tenure, with part of this promotions gap apparently due to lower measured productivity. However, in one of their samples they find that women with children are just as productive as men but are less likely to receive tenure. If productive women are not promoted as quickly as productive men, the salary return to productivity will likewise be lower for women. This is akin to our finding of a surprising lack of correlation between productivity and salary for female academics while productivity measures are strongly related to male academic salaries.

Finally Blackaby et al (2005) consider the market for economists in the United Kingdom, based on a survey conducted in 1999. This study is an advance over most others in that it has access to a tight research quality measure. They assign a quality grade to the top 3 (self-nominated) refereed publications over the career, based on an independent, published assessment of journal quality arising from the Research Assessment Exercise in Britain. Their specification for earnings involves a male dummy and extensive controls; they affirm that the research productivity measure is a significant, positive factor in salaries as is research grant success but that, despite these controls, women's salaries still lag behind men's. Unfortunately, sample size precludes separate analysis by gender. Hence their analysis is silent on whether women and men receive the same boost from academic publication.[2] Their study is also noteworthy for including a market variable, the number of outside offers received, which was found to be a significant contributor to gender salary differentials among British academic economists.

Findings overall

From these studies we conclude that published articles, in general, have a significant positive impact on salary except possibly for males in the humanities, and a similar pattern for books at least when broad disciplinary samples are examined. There is one study suggesting that articles can be important for women but only if well cited. Grants and teaching quality make a positive contribution to salaries overall in Scotland; teaching is important in the US but grant research is only significant for males. None of the studies reported on service measures and none had a measure of overall productivity. In sum, from these studies, the gender differences in the salary effect of objective productivity measures appear to be relatively small.

3. Data

The current study utilizes data collected as part of a pay equity study at a large public, urban university in the United States for the 1996-97 academic year. The pay equity study was requested by the university's Commission on Women and mandated by the university administration. The study collected data on salaries of all faculty on the main and regional campuses together with a standard set of controls and human capital variables. Male annual faculty salaries were about 22 percent higher, on average, than those of their female counterparts.

The university includes the usual arts and science disciplines, as well as numerous professional schools including allied health, business, dentistry, education, engineering, fine arts, journalism, law, library, medicine, music, nursing, physical education, public affairs and social work. The Pay Equity Task Force requested information relating to market salaries and productivity. A market reference was obtained by matching all faculty members by rank and discipline, undifferentiated by gender, with national or regional salary factors.[3] Peer rating reviews of faculty were also solicited, with senior academics (who had not been involved in determining salaries) reviewing their discipline specific colleagues. Departments were allowed to opt out of this additional process and, as a result, peer productivity review scores are not available for all faculty. Subsequently a request was made to faculty to provide c.v.'s, on a voluntary basis, so that an objective count of items such as publications, teaching, and service awards could be made.[4] Hence there are three separate samples used for the analysis in this paper: (i) the sample of all faculty; (ii) the sample of all faculty with peer productivity review scores; and (iii) the sample of all faculty with objective measures of productivity.

We exclude all faculty with administrative appointments at the dean level and above, as well as librarians, scientists and clinical supervisors of medical students (due to the difficulty in separating clinical income from university compensation). The sample of 851 regular faculty members includes 574 males and 277 females. Descriptive statistics by gender for this sample are included in Table 1. The subsample with peer review data

[Place Table 1 about here.]

includes 418 males and 207 females, 625 faculty in total; the subsample with objective counts includes 169 males and 115 females, 284 faculty in all. We have both peer reviews and objective counts for 251 of these faculty.

The key variable in the study is the contractual monthly salary without fringe benefits. Because some faculty are on academic year contracts and others are on calendar year contracts, the annual salary is converted to a monthly salary by applying the appropriate divisor.

The mean logarithm of the monthly salary is 8.426 for female faculty relative to 8.622 for male faculty; equating to $4,564 per month for women versus $5,552 per month for men.[5] The wage difference is just under $1,000 per month, or about 22 percent of the average female faculty salary.

While salary differences by gender may be due to productivity differences, the measurement of academic productivity within a given field, much less across fields, is difficult. However our unusually rich data enable us to make some headway on this critical issue.[6] The key variables are discussed in turn, starting with the more novel features.

Peer Review

The peer review was requested as part of a gender pay equity study conducted under the auspices of the university's Commission on Women. Reviewers were experienced faculty members who had not been directly involved in salary setting roles.[7] These reviews did not feed directly into the salary setting process within the unit but reviewers understood they would feed into a campus-level process for identifying cases in need of salary remediation.[8] Each department was asked to identify, where feasible, at least five senior raters who had *not* directly participated in salary setting determinations. To avoid divisive discussion and comparison, each rater was asked to privately rate all colleagues on a scale of 1 to 5 with 1 being poor and 5 outstanding. No one self-rated. The ratings were to be based upon research, service and teaching, with equal weights applied in the absence of a written policy indicating otherwise. Each faculty member who was rated had several rating scores; these were averaged and rounded to the nearest integer. Only these integers are in the current data set. Those rated as 4-5 are assigned to the *High* peer- review; those with a 3 are assigned to the *Medium* peer-review; those with a 1-2 rating are assigned to the *Low* peer-review which is the reference case in the statistical analysis.[9]

This peer-review, discipline-specific exercise is advantageous as it functions across disciplines and schools with differing standards. The publication of a peer reviewed article may be much more important in one discipline than another; the relative weighting of research, service and teaching may also vary across discipline. Raters may take these differences into consideration. Disadvantages of peer-review include potential subjectivity and the need to obtain the cooperation of all disciplines in a rather sensitive process. Participation in the exercise was encouraged but not mandated by central administration. As a result there are productivity ratings for a subset of 625 of our original sample of 851 faculty, i.e. approximately three-quarters of the initial sample.

In general the sub-sample with peer review ratings is similar to the full sample except that those with *remote* appointments are substantially under-represented. There are some differences in the peer-review sample by gender. Table 1 shows a greater percentage of males in the *High* rated group and a lower percentage in the *Medium*. The percentage in the *Low* rated group is almost identical across gender. This suggests a potential for peer ratings to help explain the lower salaries for female academics.

The peer review rating process was implemented solely for this pay equity study. How does review by peers factor into the ordinary salary review process? The main channels for increasing faculty salaries are: matching an outside offer, promotion to a higher rank, taking on a substantial administrative task, and annual increments. The role of review by peers is variable across these channels. The largest salary adjustments are probably for matching outside salary offers. These would be handled administratively for the most part although with reference to past annual reviews which include faculty input. Each year every faculty member files an annual report that is the basis for an annual review. The next largest increments are for promotion which is based upon review by peers. The process for assigning individual annual salary increments varies widely both across schools and within schools across departments. Increment assignment is based on the annual review process but that may be performed by a faculty committee or the department chair or by a combination. The role of review by peers in these assignments apparently varies from dominant to minimal.[10] Hence salaries may diverge over time from peer-reviewed perceptions of productivity.

Counts of Publications, Grant Dollars, Service and Teaching Awards

As a supplement to peer-review ratings, the university also conducted a count of the number of books and refereed articles published, as well as teaching and service awards and grant funding received. The relationship between peer ratings and the objective productivity measures is examined later in the paper. The submission of C.V.'s was strictly voluntary. Hence the measures are available for only 169 males and 115 female faculty for the full sample and 161 and 91 for the peer-review sample. Male faculty appear to publish slightly more refereed articles and books but the differences are not significant; female faculty earn more grant dollars and receive more service awards and these differences are statistically significant.

Market Salary Factor, School and Rank: External Market Salary Factor

Each individual was matched by rank and discipline (undifferentiated by gender) with national salary factors provided by the College and University Personnel Association or by other reputable national or regional associations such as the Association to Advance Collegiate Schools of Business (AACSB) and the Society of American Law Teachers (SALT). This measure provides an alternative to the more traditional adoption of rank and discipline dummies.[11]

The university in question is hiring from the market and if viewed as an atomistic player, takes market-determined wages as given. On the surface, it appears reasonable to treat the market salary factor as exogenous. The employer hires from different fields as needed, consistent with the market conditions for that field. Alternatively one could argue the variable may be tainted by endogeneity via two separate mechanisms. Individuals may sort themselves into fields, at least in part due to perceived or real discrimination within the field (and hence within the university). Second, if the university in question discriminates, perhaps unknowingly, in terms of promotion policy by gender then once again treating the market salary factor as exogenous would be inappropriate, at least for ranks above assistant professor.

This issue constitutes an omitted variable problem. Any tendency to discriminate, intentional or unintentional, is unobserved and hence in the error term. Since it affects both earnings and rank, $cov(rank, \epsilon) \neq 0$ and the regression suffers from endogeneity bias. If greater productivity leads to promotion and rank is positively associated with earnings, the bias will be upwards. This same set of issues occurs if, instead, rank and School dummy variables are included directly in the wage specification.[12] In principle we would like to predict rank in order to help resolve the endogeneity problem. Unfortunately the data set includes no plausible instruments correlated with rank but unrelated to salary.[13] Excluding external market salary (or similarly discipline and rank) from the regression specification would surely impart bias, and most likely an upward bias, to the coefficients on our productivity measures.

Evaluating the gender difference in average market salary within the current sample reveals a difference of slightly more than $11,000 per year, which is of a similar order of magnitude to the observed difference in actual salary, i.e. approximately $1000 per month. Recall the former measure is undifferentiated by gender, implying that gender differences in field and rank are potentially important contributors to the gender wage differential. Similarly, academic rank also differs significantly by gender, with fewer female full-professors and a greater proportion of assistant professors. Several school dummy variables have been created by combining similar schools (to avoid small cell problems in some of the regressions). Consistent with the occupational crowding hypothesis [Bergmann (1974)] there are many significant gender differences in discipline structure noted in Table 1, in particular for *allied health, nursing and social work*; *engineering and science*; and *medical*.

Basic Human Capital and Standard Control Variables

Holds doctorate and *holds terminal degree* (both in the field), are measures of academic qualification. Fields requiring a doctorate as a minimum standard tend to pay higher wages due to restricted entry. The same argument applies to the *terminal degree* variable for those fields where a lesser degree is recognized as the acceptable standard (terminal) for the discipline. Table 1 shows that male faculty are significantly more likely to hold both doctorates and terminal degrees for their disciplines; this difference is expected to contribute to the explanation of observed gender wage differences.

Four *time/longevity* variables capture various aspects of work experience and tenure. *Years since highest degree obtained* measures the number of years in the profession after completed training, whereas *Age in years* reflects general work experience. Controlling for the former, a higher age implies more years of work or other experience prior to earning the degree. This in turn may be associated with higher or lower earnings depending upon whether prior general human capital is valued in academic settings. A later start may also imply a depreciation of research capability in some fields and/or act as a signal of lesser academic aptitude or commitment, on average. Males have significantly higher years since degree received suggesting a potential role for explaining salary differences. *Years since joining current university* has an ambiguous expected effect. To the extent that universities reward years of service, one would expect a positive relationship with earnings. However, if salaries are market-driven, longer service implies a greater distance from market premia and hence a negative effect on salary. Length of service at the university in question differs only by a year across gender and is not significant. *Leave of absence taken* indicates whether formal time was taken out of the academic career. Leaves for sabbatical and other professional reasons are not included in this category. The need to take formal leave may be an indicator of unusual family responsibilities or poor health; either could be associated with lower cumulative productivity on the job.[14] Euwals and Ward [2005] document a significant negative effect of a similar measure for Scottish academics. In our sample almost twice as high a proportion of women as men take such leave.

Standard control variables include: *Academic year* appointment, i.e. 10 month versus 12 month, controls for otherwise unobservable differences in individual motivation, drive, salary-focus, or any tendency to discriminate in favor of males that may be correlated with academic year rather than year-round appointments. It also controls for bias due to the adjustment of annual salary to monthly salary across the two types of appointments. Significantly more males have year-round appointments suggesting this may play a role in explaining salary differences. *Remote* campus appointment controls for appointments either at a branch campus offering primarily the first two years of arts and science disciplines or at a remote site for the medical school. From Table 1 we see that approximately 3 percent of female faculty versus 9 percent of male faculty are located on a remote campus. This differential is primarily attributable to the smaller proportion of women faculty in the medical school.

4. Multivariate Models and Estimation Results

We estimate log-linear earnings regressions of the form:

$$lnE_i = H_i'\Gamma + C_i'\Psi + M_i'\Theta + P_i'\Delta + O_i'\Phi + \epsilon_i \quad (1)$$

where *H* is a vector of control variables for human capital, C a vector of standard control variables, M an external market-salary (quadratic specification), with School and rank dummy variables as an alternate specification, *P* a vector of peer rating variables, and *O* a vector of objective measures of academic productivity; the Greek letters are conformable coefficient vectors. In the initial regression we restrict analysis to include only the human capital and standard control variables on the right hand side; we subsequently add, in sequence, *the peer productivity rating* variables, the *market salary factor (*or *School* and *rank* dummy variables-see footnote 11*)*, and the *objective measures* of productivity.

Table 2 contains the results of three separate specifications of a semi-logarithmic earnings regression estimated over the peer review sample with a gender dummy variable as well as distinct male and female samples.[15] We present the results only for the key variables.[16]

The first three rows provide results for the basic human capital specification [education and experience variables] with standard controls for the nature of the academic appointment; the second three rows add the peer review ratings and the final three rows add the quadratic specification of the market salary factor.[17] When the market salary factor is added, the goodness- of-fit measure, R^2, jumps substantially for the full sample and the gender subsamples.[18] This suggests that the market salary factor has a strong influence on the salaries paid by the individual institution. Both the linear and quadratic terms are statistically significant at the most conservative levels, suggesting an important positive (but declining) contribution of the market salary factor up to approximately $107,000 for men and $92,000 for women.[19] A lower turning point for the

[Place Table 2 about here.]

quadratic is consistent with the lower average salaries earned by women; women are less likely to enjoy a salary benefit from a rank[20] and discipline with very high market salaries. The coefficient on the female dummy variable suggests a salary penalty in the basic specification of 8.5 percent but this is dampened to 4.7 (4.6) percent when the market salary factor is added, suggesting that women tend to be represented in fields with lower salaries, consistent with an occupational crowding [Bergmann (1974)] interpretation, and are at lower ranks.

In the basic specification, holding a doctorate appears to be productive for both men and women but the coefficient for women is about a third larger than for men. Although this does not change when the peer ratings are added (row 4), controlling for the market salary factor (School and rank) dampens both returns but the female coefficient is still significant and now nearly three times as large as the male coefficient.

The peer review rating variables, *Medium* and *High* are both positive and significant overall and for men, and increasing as expected, (recall the default is *low* rating). However, the female estimated coefficients are, surprisingly, much smaller, less than one-fifth as large for the *medium* peer productivity rating and far from being significantly different from zero for the *high* rating.

When the market salary factor is added (rows 7 through 9) the peer rating effects for males are dampened but still significant and much higher than for females; this differential effect is largest for the *high* ratings.

The quadratic form in market salary factor itself has a strong, statistically significant effect across male and female samples. The most surprising finding is the gender difference in the role of peer ratings, i.e., coefficients for women are insignificant and dampened relative to men. We consider possible explanations for this robust but rather surprising finding in section 6.

In Table 3 counts of academic productivity are included in place of the peer productivity ratings, namely the number of refereed articles, and books, as well as measures of teaching and service awards and grant money. Results are for the sample of all faculty who submitted C.V.'s. Because the sample is now quite small, the results on objective measures in Tables 3 and 4 are suggestive rather than definitive. The key influence of the market salary factor is maintained on this sub-sample and we again find significant effects for men but not for women.

The first three columns of table 3 report the results for the objective sample; columns 4 and 5 are for the smaller sample for whom we have both peer rating and objective measures. The female coefficient shows an earnings penalty of about 3.5

[Insert Table 3 about here.]

percent which is robust to the difference in sub-sample and to the addition of peer ratings. The latter effect is unsurprising given the small and insignificant effect of peer ratings on female earnings shown in Table 2. Once we control for objective measures the doctorate effect is dampened and becomes insignificant. The first two columns of Table 3 show that books and grant dollars have a significant positive effect overall and for the male sub-sample. But these effects are not significant for the admittedly smaller female subsample. We view this result as weakly supportive of the differential peer rating effect by gender.

Column 4 demonstrates that there are no important changes as we drop observations while moving to the smaller sample with both objective measures and peer ratings. Column 5 demonstrates that the returns to books and grant dollars for the combined male and female sample are robust to adding controls for peer ratings. Furthermore, the *Medium* coefficient is now smaller and insignificant. The distinction between low and medium peer review apparently makes little difference once these measures of objective productivity are included. Perhaps those perceived as low achievers have very low absolute counts so quality is an unimportant qualifier. The distinction between medium and high generates a smaller salary reward than seen in earlier tables but it is still a significant determinant. Subjective judgment apparently plays an important role in identifying quality dimensions which may separate high from medium performance. In column 6 a full set of gender interaction terms is provided for both the peer review ratings and the objective measures. Grant-making remains a significant determinant of salaries but books are no longer significant overall; female academics have a salary return to books that is significantly higher than male academics. The returns to a high rating are larger once we control for objective measures and are still significant. But the returns to a high rating for women are significantly lower than the returns to a high rating for male academics.

An alternative way of looking at the relationship between peer ratings and objective measures is to examine whether the objective measures determine the ratings.

Table 4 reports the marginal effects from an ordered probit regression relating the peer rating category (*Low, Medium, High*) to the objective measures. Most coefficients are

[Insert Table 4 about here.]

insignificant including the female dummy variable; we find no *ratings* penalty for women.

Consider the first three columns. Books published is the only measure that significantly affects the distinction between low, medium, and high ratings. What we learn from the last three columns with interaction terms is that service awards become significant and positive (for high male ratings) but service awards increase the chances that women get medium or even low ratings. This exercise reinforces the finding from Table 3 that books are important—both for peer review ratings and for salaries; and that objective measures cannot fully predict ratings. Subjective judgment is needed to make difficult quality distinctions; that opens the door for potentially discriminatory distinctions as well.

5. Discussion.

In the literature review we mention Milgrom and Oster's (1987) invisibility hypothesis, Valian's (1998) notion of a gender schema, Bjerk's (2008) dynamic version of a standard signaling game, and modern monopsony models as in Manning (2003) and Ashenfelter et al (2010). To what extent are the key empirical findings of the current analysis compatible with those theoretical models? Recall the key results are females earn less than men and objective productivity measures and peer review play a significant role in male but not female wage determination. In both the invisibility hypothesis and Bjerk's dynamic statistical discrimination model, academic employers have an incentive to delay or deny promotion of women in order to reap the benefits of their high productivity while in a low-paying job; such activity would dampen and obscure the positive association between productivity and salary. This is consistent with the empirically observed over-representation of women in the lecturer ranks and consequent lower female wage. This same conclusion applies with equal force to the monopsony power story in that males may find it easier to gain outside offers and receive counter offers from their home institution, leading to higher wages and earlier promotion. Gender schemas could also lead to a reduction in female promotion due to misperceptions of productivity.[21]

To a large extent the competing theoretical explanations are observationally equivalent in the current context. One potentially important feature of the Bjerk model is that males are more accomplished at discerning male (compared to female) potential productivity. The gender schema on the other hand suggests that both males and females may suffer the same distortion. Table 4 illustrates the link between peer productivity and objective measures of academic productivity. Those results suggest that gender is not a significant determinant of the relationship. Table 5 sheds some additional light. Gender schemas may distort both male and female perceptions. We examine whether males are more likely to misperceive the productivity of female faculty members.[22] If so, we would expect to observe the effect in male-dominated schools but not at all, or at least to a lesser extent, in those schools with strong female representation. In the former schools, female faculty would be mostly rated by males but that would not be true in the latter. Consider regressions on two subsamples of the data which are reported in Table 5.

[Insert Table 5 about here.]

The first set of results, designated male-dominated, are for schools where 20 percent or fewer of the faculty are female (business, dental, engineering and technology, law, medical, science); the second set, designated, female-represented, are for schools where 38 to 52 percent of the faculty are female (art, education, journalism, liberal arts, physical education, public affairs, social work).[23] Women are likely to have their salaries set and their productivity peer reviewed predominantly by males in the male dominated schools; that is less likely in the female represented schools. A similar pattern prevails with dampened and insignificant female peer review coefficients in both male-dominated and female-represented samples. The difference between male and female coefficients is particularly high for the *High* peer review dummy. This exercise provides little support for the notion that the peer rating effect is due *primarily* to misperceptions of male faculty.

To further explore the role of gender schemas we examine the pattern of cohort effects. Table 6 presents the results from introducing three cohort dummy variables with *cohort 1* (0-10 years from terminal degree); *cohort 2* (11-20 years from terminal degree); and *cohort 3* (more than 20 years from terminal degree). The Table presents the results of

[Place Table 6 about here.]

including these cohort variables in the peer review sample along with the peer rating variables and the interactions with cohort.

There are no significant cohort effects for men; no interaction terms are significant. The story is different for women. For *cohort 1* earning their terminal degrees after 1987, the peer review ratings earn a reasonable salary return although more so for the distinction between medium and low than between medium and high—so there is still an issue in rewarding high-achieving women. The *cohort 2* interactions show that the peer review ratings work very poorly for the women who earned their terminal degrees between 1977 and 1987 when large numbers of women were entering the academy. Finally the *cohort 3* interactions show no significant difference in the salary reward to productivity for women in the oldest and newest cohorts. This fits a gender schema story in the following way. Women who earned their terminal degrees prior to 1977 before the academy was very open to women essentially made it or not on existing standards ands judgements. There were comparatively few women. They accepted this challenge and either made it or not as did men. Hence productivity ratings were fairly accurate reflections of productivity as defined by the university hierarchy which was largely made up of males. Large numbers of women entered in the late 1970's and early 1980's and it was more difficult for salary reward systems managed mostly by men of earlier generations to accurately evaluate the productivity of these women. A decade later more of these systems were managed by men of a younger generation, and some women, and those of the older generation had at least more experience evaluating women. Hence salaries start again to line up more closely with peer rated productivity. This gives more credence to the gender schemas explanation but also suggests that its effect may be fading somewhat in importance as time goes on and women populate the academy in larger numbers.

Combining Bjerk's model of statistical discrimination with a gender schemas argument provides potentially the best explanation of our findings. Adapting Bjerk's model to the academic labor market, women are less likely to be hired into tenure track jobs, even if as skilled as men, and must provide more signals of productivity to get promoted, if their skill signals are less precise. For example they might have to publish more co-authored articles than a male colleague if supervisors tended to view women as possibly taking subordinate roles in multiple author projects. The *gender schemas* argument can be coupled with this to provide a reason why the signals might be less precise. If the same signal is emitted by male and female academics, supervisors judge the male (female) signal more (less) favorably based on preconceptions. In Bjerk's model, employers eventually learn to accurately interpret the skill signals of women. But the initial disadvantage has a long-term effect. Bjerk shows that even if later promotion is nondiscriminatory, women would be less likely to make it to the top rank and hence would have lower salaries than men and a lower correlation between productivity and salary. As time passes, gender schemas become less prevalent in evaluating women's skill signals and productivity and salary are better aligned.

6. Conclusions

Our study's main contribution concerns a subjective measure of academic productivity but it also contributes to the literature concerned with the salary effects of objective measures of academic productivity. Most of the earlier studies reported in Table 1 cover only a subset of academic productivity measures like articles and books or articles, grants and teaching; none consider service. We are able to simultaneously control for a broader set of measures, including articles, books, external grants, teaching and service awards. In our study covering a wide variety of disciplines only books and grant dollars are significant determinants of earnings. It appears that overall faculty receive an earnings boost from an extra book but the earnings premium for women is significantly below that for men. Consistent with earlier findings, grant dollars are significant overall and for men; the point estimate for women is similar but not significant. Teaching awards are weakly significant for males. Service awards do not contribute directly to earnings but are a significant factor in men but not women obtaining a higher peer review rating.

Our results also suggest a pivotal role for the market salary factor by discipline and rank. When included it is a highly significant determinant of salary and equally so for both women and men. Nonetheless the endogeneity difficulty [due to rank and disciplinary/School crowding] makes the interpretation challenging. If we take the particular university in question as an atomistic competitor, there is not much it can do, by itself, to address gender salary differentials ingrained in the market. But if individual universities fail to assess whether market-based salary differentials are consistent with underlying productivity at the local university level, faculty salaries will be misaligned.

Most importantly, the econometric results demonstrate that academic productivity, assessed subjectively by peers, contributes significantly to male but not female earnings. The comparative lack of reward for peer-rated female productivity is striking. Combining Bjerk's dynamic statistical discrimination model with gender schemas provides a potential explanation for our results. Our cohort analysis seems consistent with a gender schemas/statistical discrimination story but also suggests that the puzzling disconnect between productivity measures and salaries for women may have been at its peak for women who earned their terminal degrees in the late 1970's and early 1980's with the disconnect less substantial in the decade following. Whether the connection between female academic perceived productivity and earnings is continuing to strengthen is a puzzle for future research with more recent data to resolve.

The cohort effects found here point to the importance of empirical work on academic wage structure using current data. Future work on the theoretical side should address dynamic aspects of discrimination as early salary and promotion decisions cast long shadows. Future work on the empirical side should attempt to devise tight tests of the various explanations for lower salary rewards for women's productivity.

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Notes

[1] Recent examples of studies which focus on other possible explanations of the academic gender wage gap include Toumanoff [2005],on initial salary negotiations, and Hilmer and Hilmer [2010], on job mobility patterns.

[2] Interestingly Blackaby et al point to males receiving more outside offers as a key culprit in unequal salaries in the UK. But this may not be as important in other countries. The UK market for economists, in particular, has experienced churning due to the Research Assessment Exercise; departments wish to attract and/or retain the most productive researchers in the period leading up to the next assessment exercise. Apparently more males test the market and receive offers. Our data include no information on outside offers but we do have information on the relevant market salary across rank and discipline.

[3] In two disciplines the relevant salary survey was at the regional level rather than national.

[4] The process is described in detail in Carlin and Rooney [2000].

[5] At the university in question, appointments are considered either ten-month (for the academic year) or twelve-month for the calendar year. We therefore divide by 10 or 12, as appropriate, to arrive at a comparable monthly salary for all faculty. In some other universities, an academic year appointment would be counted as nine months. Dividing by ten may be viewed as imparting a small downward bias to the salaries of academic year appointees; 66% of female faculty are academic year appointees while only 53% of

male faculty are academic year appointees. We include an academic year appointment dummy variable to control for this potential downward bias.

[6] There is an added legal reason for making the effort. At least one court has found in favor of the plaintiffs in a reverse discrimination suit, citing among other things, a failure to control for productivity (U.S. Court of Appeals [1995]).

[7] In most cases these were senior faculty members who had neither served as Chair nor on a Salary Committee; in one discipline the Chair was allowed to be a reviewer because in that discipline an independent salary committee determined salary increments.

[8] The ordinary salary review process is quite standard for the US academic market. The largest internal salary adjustments are for promotion which is based upon peer review. The process for assigning individual annual salary increments varies widely both across schools and within schools across departments. The role of peer review in these assignments varies from dominant to minimal.

[9] Because of concerns about the confidentiality of the ratings, the forms were destroyed after the ratings were entered in the database.

[10] Based upon reports from a small set of department chairs on the method of review wihin their department.

[11] A comparison of the empirical results associated with the two approaches to rank and discipline is provided in section 5.

[12] We used this alternate specification as a robustness check. Key results are highly robust to this alternate specification. Results are available from authors.

[13] Attempts to estimate rank on the pooled sample of female and male faculty by an ordered probit, relying on the nonlinearity of the specification for identification proved unsatisfactory. Results are available from the authors.

[14] A leave of absence could still have a positive effect on productivity and salary if the time allocated to care-giving is less than the time allocated to teaching and service related functions and the individual on leave continues to build their publication record. Some non-sabbatical leaves may also be for a visit to another institution where productivity may be enhanced or unchanged.

[15] The results for the full sample are quite similar; the estimated coefficients are quite robust to the exclusion of observations from units that chose not to participate in peer review.

[16] A complete set of results with all of the independent variables for the full sample, as wellas the peer rating and objective sub-samples is available from authors.

[17] Rotating the standard presentation for the table by 90 degrees allows us to present nine columns of results without resorting to a ‘landscape’ orientation for the page.

[18] If School and rank variables are added in place of the market salary factor variables, we observe similar increases. In all of the results reported here and in subsequent tables, key results are highly robust to a substitution of rank and School dummy variables for the market salary factor variables.It is also interesting to note that the R-squared values are consistently higher for the female than the male sample in this and all other reported results.

[19] For the regression analysis the linear term for salary factor is divided by 10,000 and the quadratic term is divided by 1,000,000.

[20] In all of the specifications reported, lecturers are included as well as tenure stream faculty. As a test of robustness we removed the lecturers from the sample. All of the main results are highly robust to the removal of the lecturers.

[21] Promotion can be delayed by tenure clock stoppages, movements out of and back into academia, and by moving between universities prior to the award of tenure.

[22] The effect is also not simply due to a higher variance of the peer review ratings for women. In fact the standard deviations for *low*, *medium*, and *high*are the same for women and men to two decimal places.

Table 1. Variable means for regular faculty sample, all variables.

Dependent Variable	Female Sample Mean	Male Sample Mean	T-statistic for Difference in Means
Logarithm of monthly salary	8.426	8.622	9.76***
Independent Variables			
Peer Productivity Review Scores			
Low productivity	0.140	0.139	-0.05
Medium productivity	0.435	0.388	-1.13
High productivity	0.425	0.474	1.15
Objective Measures of Productivity			
Books	0.565	0.858	1.01
Log of grant dollars	7.018	5.456	-2.15**
Refereed publications	5.452	5.799	0.266
Service Award	1.330	0.793	-1.85*
Teaching award	1.452	1.260	-0.70
Market Salary Factor			
Average academic year salary *(for given discipline and rank)*	48,547.21	60,072.3	9.38***
Rank			
Lecturer	0.105	0.035	-4.13***
Assistant Professor	0.336	0.207	-4.09***
Associate Professor	0.404	0.385	-0.54
Professor +	0.155	0.373	6.64***
Schools			
Allied health, nursing, social work	0.394	0.042	-14.84***
Business, journalism, law, public affairs	0.087	0.122	1.54
Dental	0.061	0.118	2.61***
Education, physical education	0.072	0.040	-2.01**
Engineering, science	0.076	0.286	7.15***

Table 1 (continued)

Independent Variables	Female Sample Mean	Male Sample Mean	T-statistic for Difference in Means
Liberal arts, fine arts, continuing studies, library science	0.253	0.199	-1.80*
Medical	0.058	0.19	5.28***
Human Capital			
Holds doctorate in field	*0.704*	*0.895*	*7.22****
Holds terminal degree in field	0.834	0.956	6.18***
Age in years	48.657	48.664	0.01
Leave of absence	0.147	0.082	-2.13**
Years since highest degree obtained	15.329	20.040	6.88***
Years at current university	13.835	14.694	1.26
Control Variables			
Academic year appointment	0.657	0.538	-3.30***
Remote campus appointment	0.032	0.094	3.23***
Sample size (full sample)	277	574	
Productivity rating sub-sample	207	418	
Objective measures sub-sample	115	169	

Variable names in bold have results reported in one or more of the estimation tables.
***, **, and * notations on the t-statistics indicate statistical significance at the .01, .05, and .10 levels respectively

Table 2: Earnings regression—(i) human capital and basic controls; (ii) plus peer ratings; (iii) plus market salary factor—Peer ratings subsample

Explanatory Variable [Dependent variable = log(monthly salary)]		*Female*	*Holds Doctorate*	**Peer Ratings**		**Market Salary Factor**		R^2	*N*
				Medium	*High*	*Average Academic Year Salary (÷ 10,000)*	*Average Academic year salary squared (÷ 1 million)*		
Model	**Sample**								
Human Capital and Basic Controls	**(1) *All***	**-.085*** (.019)**	**.227*** (.029)**					*.48*	*625*
	(2) *Male Only*		**.202*** (.042)**					*.42*	*418*
	(3) *Female Only*		**.268*** (.039)**					*.52*	*207*
Human Capital, Basic Controls & Peer Ratings	**(4) *All***	**-.085*** (.019)**	**.226*** (.029)**	**.075*** (.029)**	**.129*** (.029)**			*.47*	*625*
	(5) *Male Only*		**.207*** (.044)**	**.116*** (.036)**	**.205*** (.036)**			*.42*	*418*
	(6) *Female Only*		**.267*** (.039)**	**.022 (.042)**	**-.015 (.043)**			*.52*	*207*
Human Capital, Basic Controls, Peer Ratings, & Market Salary Factor	**(7) *All***	**-.047*** (.014)**	**.061** (.028)**	**.064*** (.021)**	**.098*** (.022)**	**.283*** (.024)**	**-.130*** (.019)**	*.72*	*625*
	(8) *Male Only*		**.046 (.038)**	**.069** (.028)**	**.130*** (.028)**	**.293*** (.031)**	**-.132*** (.024)**	*.70*	*418*
	(9) *Female Only*		**.118*** (.039)**	**.046 (.030)**	**.020 (.031)**	**.291*** (.040)**	**-.161*** (.033)**	*.73*	*207*

(heteroskedastic standard errors in parentheses)
Each specification includes a constant, the human capital and control variables (academic year appointment, remote appointment, holds doctorate, holds terminal degree, age and its square, leave of absence, years since highest degree obtained and its square, years with current university); variations in other tables will be noted. Full set of results including rank and school group dummy variables is available from authors.
*, **, *** *denotes a significant coefficient at the .10, .05, or .01 level respectively.*

Table 3: Earnings regression—Effect of objective measures—Objective measures subsample; dependent variable = log (monthly salary)

Sample--gender	(1) *All*	(2) *Male*	(3) *Female*	(4) *All*	(5) *All*	(6) *All*
Explanatory Variable						
Female	**-.036**** **(.017)**			**-.038**** **(.017)**	**-.035**** **(.017)**	**.073** **(.055)**
Holds Doctorate	**.034** **(.053)**	**.047** **(.035)**	**.023** **(.101)**	**.051** **(.051)**	**.054** **(.055)**	**.038** **(.043)**
Objective measures						
Books (÷ 10)	**.063*** **(.036)**	**.099**** **(.046)**	**.061** **(.074)**	**.097***** **(.036)**	**.077**** **(.036)**	**.048** **(.043)**
Log (grant dollars ÷ 100,000)	**.036**** **(.16)**	**.045**** **(.019)**	**.044** **(.027)**	**.040**** **(.016)**	**.039**** **(.016)**	**.042**** **(.019)**
Refereed articles (÷ 10)	**.010** **(.010)**	**.013** **(.013)**	**.011** **(.016)**	**.013** **(.011)**	**.013** **(.010)**	**.016** **(.011)**
Service awards (÷ 10)	**-.016** **(.041)**	**.002** **(.045)**	**-.057** **(.064)**	**.009** **(.048)**	**.001** **(.049)**	**.007** **(.005)**
Teaching awards (÷ 10)	**.052** **(.040)**	**.064*** **(.038)**	**-.058** **(.083)**	**.055** **(.040)**	**.049** **(.041)**	**.068** **(.044)**
Peer Ratings						
Medium					**.029** **(.025)**	**.025** **(.031)**
High					**.070***** **(.025)**	**.104***** **(.028)**
Market Salary Factor						
Avg. academic year salary (÷10,000)	**.322***** **(.046)**	**.376***** **(.053)**	**.410***** **(.073)**	**.353***** **(.046)**	**.376***** **(.047)**	**.374***** **(.046)**
Avg. academic year salary squared (÷ 1,000,000)	**-.150***** **(.040)**	**-.178***** **(.045)**	**-.271***** **(.065)**	**-.175***** **(.041)**	**-.193***** **(.041)**	**-.192***** **(.041)**
Gender Interaction Terms						
*Fem*Medium*						**-.017** **(.057)**
*Fem*High*						**-.138**** **(.061)**
*Fem*Books (÷ 10)*						**.162**** **(.073)**
*Fem*log(grant dollars ÷ 100,000)*						**-.007** **(.036)**
Fem Refereed articles (÷ 10)*						**-.033** **(.021)**
Fem Service awards (÷ 10)*						**-.146** **(.161)**

Fem Teaching awards (÷10)*						**-.007 (.088)**
F-tests on interaction terms in column 6	*The test that the coefficients of female plus fem*High equals zero was rejected at p=.018 (F=5.7); the test that the coefficients of female plus fem*Medium equals zero was not rejected at conventional levels (p=.11). None of the similar tests on the female/objective measure interactions were rejected at conventional levels.*					
R^2	**.784**	**.776**	**.805**	**.796**	**.803**	**.816**
N	**284**	**169**	**115**	**251**	**251**	**251**

(*heteroskedastic standard errors in parentheses*)
Each specification includesa constant and the basic controls and human capital variables.
The sample for the first three columns is the set of faculty who submitted c.v.'s. The last two columns is for the set of faculty who both submitted c.v's and had peer review ratings submitted by their department.
Results on key variables for the first three columns are essentially the same whether the sample of 284 or 251 is employed. (Results are available from authors.)
A specification with a full set of gender/objective measures interaction terms was estimated but none of the interaction terms were significant at conventional levels and results for key variables were highly robust to their inclusion. (Results available from authors.)
,,*** denotes a significant coefficient at the .10, .05, or .01 level respectivel.*

Table 4: Determinants of peer productivity ratings.

	Ordered Probit [marginal effects= change in probability of being in cell, z-statistic in parentheses.]					
	(1) Dependent Variable: Peer Rating = Low	(2) Dependent Variable: Peer Rating = Medium	(3) Dependent variable: Peer Rating = High	(4) Dependent Variable: Peer Rating = Low	(5) Dependent Variable: Peer Rating = Medium	(6) Dependent variable: Peer Rating = High
Explanatory Variable	No interaction terms			Interaction terms included		
Female	**-0.002 (-0.13)**	**-0.008 (-0.13)**	**0.010 (0.13)**	**-0.028 (-1.33)**	**-0.119 (-1.31)**	**0.146 (1.33)**
Objective Measures						
Books (÷ 10)	**-0.233*** (-2.65)**	**-0.789** (-2.51)**	**1.022** (2.66)**	**-0.206** (-2.36)**	**-0.822** (-2.36)**	**1.028** (2.46)**
Log [grant dollars (÷100,000)]	**-0.004 (-0.28)**	**-0.013 (-0.28)**	**0.016 (0.28)**	**-0.012 (-0.86)**	**-0.048 (-0.88)**	**0.061 (0.88)**
Refereed articles (÷10)	**-0.002 (-0.25)**	**-0.007 (-0.25)**	**0.009 (0.25)**	**-0.006 (-0.68)**	**-0.023 (-0.68)**	**0.029 (0.69)**
Service Awards (÷10)	**-0.067 (-1.30)**	**-0.228 (-1.35)**	**0.295 (1.36)**	**-0.125* (-1.73)**	**-0.496* (-1.81)**	**0.621* (1.84)**
Teaching Awards (÷10)	**-0.020 (-0.57)**	**-0.069 (-0.57)**	**0.089 (0.58)**	**0.007 (0.19)**	**-0.027 (0.19)**	**-0.034 (-0.19)**
Female/Objective Measure Interactions						
*Fem*books* (÷10)				**-0.123 (-0.50)**	**-0.491 (-0.49)**	**0.614 (0.50)**
Fem[log_grant dollars]* (÷100,000)				**0.027 (1.01)**	**0.108 (1.04)**	**-0.135 (-1.04)**
*Fem*refereed articles* (÷10)				**0.027 (1.46)**	**0.106 (1.50)**	**-0.133 (-1.52)**
*Fem*Service Awards* (÷10)				**0.201* (1.79)**	**0.801* (1.88)**	**-1.002* (-1.91)**
*Fem*Teaching Awards* (÷10)				**-0.086 (-1.20)**	**-0.342 (-1.25)**	**0.428 (1.25)**
Log-likelihood	**-228.74**			**-179.21**		
N	**251**			**251**		

The sample consists of the individuals for whom we have both predicted rank and a C.V. The specification also included a constant and the ***human capital and control variables*** *(academic year appointment, remote appointment, holds doctorate, holds terminal degree, age and its square, leave of absence, years since highest degree obtained and its square, years with current university)., market salary factor*
*, **, *** *indicates a significant coefficient at the.10, .05, or .01 level respectively.*

Table 5: Earnings regression-peer ratings; male-dominated and female-represented schools--peer rating subsample; dependent variable = log (monthly salary)

	Male Dominated		**Female Represented**	
Explanatory Variable	*Male*	*Female*	*Male*	*Female*
Holds doctorate	**-.067 (.042)**	**.057 (.100)**	**.068**** **(.033)**	**.028 (.053)**
Peer Ratings				
Medium	**.055 (.034)**	**.066 (.064)**	**.044 (.043)**	**.032 (.037)**
High	**.116***** **(.035)**	**.016 (.071)**	**.077*** **(.045)**	**.001 (.040)**
Market Salary Factor				
Average academic year salary (÷ 10,000)	**.248***** **(.048)**	**.240***** **(.076)**	**.428***** **(.090)**	**.460***** **(.064)**
Average academic yearsalary squared(÷ 1,000,000)	**-.103***** **(.028)**	**.105*** **(.055)**	**-.241**** **(.089)**	**-.309***** **(.065)**
R^2	**.702**	**.866**	**.833**	**.851**
N	**269**	**53**	**140**	**100**

(*heteroskedastic standard errors in parentheses*)
The specification also included a constant and the ***human capital and control variables*** *(academic year appointment, remote appointment, holds doctorate, holds terminal degree, age and its square, leave of absence, years since highest degree obtained and its square, years with current university).*
*, **, *** *indicates a significant coefficient at the.10, .05, or .01 level respectively.*

Table 6: Earnings regression-peer rating cohort effects--peer rating subsample; dependent variable = log (monthly salary).

Sample- gender	*All*	*Male*	*Female*
Explanatory Variable			
Female	**-.051*** (.014)**		
Holds Doctorate	**.059** (.027)**	**.029 (.037)**	**.117*** (.039)**
Peer Ratings			
Medium	**.076 (.050)**	**.057 (1.15)**	**.116** (.048)**
High	**.108** (.051)**	**.114 (.116)**	**.102** (.047)**
Cohort			
Cohort 2 (11 – 20 years)	**.176*** (.061)**	**.143 (.034)**	**.228*** (.060)**
Cohort 3 (> 20 years)	**.051 (.055)**	**.049 (.123)**	**.113* (.058)**
Ratings/cohort interactions			
*Medium*cohort2*	**-.146** (.064)**	**-.126 (.133)**	**-.178*** (.068)**
*High*cohort2*	**-.144** (.064)**	**-.107 (.135)**	**-.214*** (.069)**
*Medium*cohort 3*	**.028 (.057)**	**.037 (.119)**	**-.028 (.065)**
High cohort3*	**.037 (.057)**	**.031 (.120)**	**.010 (.069)**
Market Salary Factor			
Average academic year salary (÷ 10,000)	**.303*** (.024)**	**.312*** (.029)**	**.304*** (.041)**
Average academic yearsalary squared(÷ 1,000,000)	**-.146*** (.019)**	**-.146*** (.023)**	**-.171*** (.033)**
R^2	**.729**	**.693**	**.736**
N	**625**	**418**	**207**

(*heteroskedastic standard errors in parentheses*)
The specification also included a constant and the ***human capital and control variables*** *(academic year appointment, remote appointment, holds doctorate, holds terminal degree, age and its square, leave of absence, years since highest degree obtained and its square, years with current university).*
*, **, *** *indicates a significant coefficient at the.10, .05, or .01 level respectively.*