Does Bad Pay Cause Occupations to Feminize,

Does Feminization Reduce Pay,

and

How Can We Tell with Longitudinal Data?

Paula England, Stanford University

Paul Allison, University of Pennsylvania

Yuxiao Wu, Northwestern University

Mary Ross, NewarkInOne

Address correspondence to Paula England, Dept. of Sociology, Stanford University, Stanford, CA 94305-2047 or PEngland@Stanford.edu.

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Does Bad Pay Cause Occupations to Feminize, Does Feminization Reduce Pay, and How Can We Tell with Longitudinal Data?

Paula England, Paul Allison, Yuxiao Wu, and Mary Ross

Abstract

Predominantly female occupations pay less than "male" jobs, even after adjusting for skill demands. The devaluation perspective sees sex composition to affect wages; it says that gender bias affects employers' decisions about the relative pay of "male" and "female" jobs. The queuing or relative-attractiveness view sees occupations' sex composition to be affected by their reward level, with less attractive jobs going to women because employers prefer men and can get them in jobs that pay well. Past longitudinal research on how changes in occupations pay and sex composition are related has employed the cross-lagged panel (lagged-Y-regressor) model, generally finding support for the devaluation but not the queuing/relative attractiveness view. We argue that a stronger statistical approach to assessing causal dynamics is a fixed-effects model with lagged independent variables. Using CPS data from 1983 to 2001, we test these two perspectives. We find support for neither idea. That is, generally, the feminization of occupations does not lower their wages, and a fall in occupations' relative wages does not lead to feminization. We conclude that in earlier historical processes, as occupations and organizations originate, there was a causal relationship between pay and sex composition, but that the continuing relationship is due to institutional inertia freezing in that early relationship, rather than to ongoing causal dynamics.

I. INTRODUCTION

In the United States, a good share of the pay gap comes from women's concentration in lower paying occupations (Petersen and Morgan 1995), despite the fact that women's jobs require about as much education and skill as men's jobs, on average. This correlation between occupations' sex composition and their average pay is well known, and holds up in the presence of numerous controls in cross-sectional data. There are two major sociological views of the causal dynamics involved, and they have different implications for how the sex composition and pay of occupations should co-vary over time. Curiously, however, few studies have exploited longitudinal data to examine the relationship over time.

In the "devaluation" view, associated with policy proposals for "comparable worth," predominantly female jobs are paid less *because* women fill the jobs (England 1992; Steinberg 2001; Sorensen 1994). In this view, if a job is filled mostly by women, employers see the job as less valuable, less demanding, or less pay-worthy. Somehow, the low status of women "rubs off" on employers' evaluation of the job, and they set a lower pay level for both men and women in the job than they would have if the identical job were done mostly by men. If this bias in wage setting exists, then we would expect that if the sex composition of a job changes, its wage would change. But little of the analysis offered in support of the devaluation claim actually examines how *changes* in occupations' sex composition relate to changes in their pay. Of course, since skill demands also affect pay, the hypothesis would presume controls for educational and other skill demands of occupations.

Another view sees the causal arrow the opposite way, implying that bad pay (or other undesirable nonpecuniary characteristics) causes occupations to feminize. This view is associated with Reskin and Roos (1990) and with Catanzarite, Strober, and Arnold (Strober 1984; Strober and Arnold 1987; Strober and Catanzarite 1994). In this view, employers' preferences for men, combined with men and women's preferences for better paying jobs, leads good jobs to come to be filled by men and bad jobs with women. Given these preferences, when hiring for high paying jobs, employers will be able to get men, but when hiring in low paying jobs, they will often have to settle for women even if they prefer men, since men will gravitate first to the high paying jobs. In such a process, even though women also prefer high paying jobs, they will only be able to get the jobs men don't want. Of course, these writers recognize that educational credentials are crucial for entry into some jobs, so the hypothesis would be most appropriately tested controlling for educational and skill requirements of jobs. It is when jobs pay badly *relative to* their educational requirements that employers won't be able to get men, and the jobs are likely to end up filled with women. Reskin and Roos' refer to this as the "queuing" view. Strober and Catanzarite (1994) have referred to it as the "relative attractiveness" theory of segregation; the more attractive a job is, the more likely it is to come to be filled by men. If this is roughly what is happening, then longitudinal data reveal that occupations' pay at one time affects their sex composition at a later time. As with the previous hypothesis, little of the analysis offered in support of this claim has actually examinee how *changes* in occupations' sex composition relate to changes in their pay.

These two views both posit sex discrimination, but of different types. In the "queuing" or "relative attractiveness" view, it is hiring or placement discrimination against women. Such discrimination has been illegal since the Civil Rights Act of 1964 but undoubtedly still exists to some degree. In the devaluation view, gender bias affects which jobs get assigned higher wages, but this form of bias does not violate current law in the U.S. With few exceptions, violations of the principle of "comparable worth"—that jobs requiring the same amount of skill and having equally onerous working conditions must pay equivalently—have not been found by U.S. courts to violate antidiscrimination laws (Nelson and Bridges 1999; England 1992, Ch. 5). There is another kind of discrimination, paying women less within jobs, but that will not be our focus here.

These two views of the link between a job's sex composition and its wages make different predictions about how the two factors would covary over time. In the devaluation view, earlier levels of occupational sex composition should affect later wages; we would expect that changes in the sex composition would produce changes in pay. In the queuing or relative attractiveness view, earlier wages should affect later sex composition; we would expect that changes in wages would produce changes in sex composition.

If there is a causal relationship between sex composition and wages, it is possible that both devaluation and queuing processes could be going on simultaneously; the causal arrow may run both ways. Indeed, authors advocating the queuing view recognize the possibility of devaluation occurring during and following occupational feminization, and authors advocating the devaluation view recognize that there may be hiring/placement discrimination. In this case, changes in either factor will affect the other; it is the magnitude of each effect is of interest.

There is a third view of the association between jobs' sex composition and their pay that sees neither as causing the other, but sees their association as completely or largely spurious. This view, favored by many economists, and by sociologist Tony Tam (1997), sees women choosing less demanding jobs than men because they prioritize motherhood more and money less than men. In this view, "women's jobs" pay less because they are less demanding or more "mother-friendly." For example, they may have more flexible hours, or allow a parent to use the phone to check in with children, or provide child care. Or, as Tam (1997) emphasizes, they may require less specialized training (occupation- or firm-specific) of the type that mothers who plan some time out of employment for childrearing do not find a worthwhile investment. These gender-specific hypotheses are consistent with a larger theory in neoclassical labor economics known as "equalizing differentials" which subsumes both human capital theory and the theory of compensating differentials (Rosen 1986). The idea is that employers have to pay more to fill jobs that have nonpecuniary characteristics workers don't like or require them to bear costs (e.g. for training) to enter. Putting it statistically, this suggests that if we include the right control variables for job characteristics and human capital requirements, the relationship between the percentage female employees in a job and its pay will disappear.

In this paper, we use longitudinal data on occupations in the U.S. over the 19 year period from 1983 to 2001 to assess whether either or both of these processes can be observed. Our main contribution is to shed led on the substantive question with a systematic longitudinal analysis, given the dearth of such analyses in past literature.

However, in order to do this, we must grapple with the question of what statistical models are best for isolating causal effects in panel data, in the presence of substantial possible omitted variable bias. We will argue for using a fixed-effects model with lagged independent variables. We see our approach as an advance over the cross-lagged panel approach, sometimes called the "lagged-Y regressor model." In the latter, two years of data can be used at a time (with occupations as units), and we assess the "effect" of earlier X on later Y, controlling for earlier Y, and then the "effect" of earlier Y on later X, controlling for earlier X. This model achieves some protection against omitted variable bias through the lagged dependent variable control, although, in the presence of measurement error, this may not always be sufficient. The fixed-effects approach is superior because it allows us to pool multiple years of data and provides better protection against omitted variable bias by estimating the coefficients as if dummies for each occupation are controlled. We lag the independent variable a number of years behind the dependent variable. By running alternative models that reverse whether occupations' pay or sex composition is the dependent variable, both times with fixed-effects (occupation dummies) in the model, we can isolate causal effects in both directions with the substantial protection from omitted variable bias (greater than that afforded by the lagged-Y regressor model) in force.

II. PAST RESEARCH AND THEORY ON THE CAUSAL RELATIONSHIP BETWEEN OCCUPATIONAL SEX COMPOSITION AND WAGES

Strober and Catanzarite's theory of "relative attractiveness" (1994; see also Strober 1984) posits that occupations with low (or declining) wages will become feminized. In their view, patriarchy operates such that men receive first choice of occupations. Male employers collude with male workers to maintain the system of male privilege. They fear that a break down of male privilege in the working classes will lead to a domino effect through the upper classes, thereby risking their own position. Moreover, if employers don't privilege male over female workers in hiring for good jobs, they face the risk of sanctions from employees, customers, and other community members; breaking patriarchal norms is punished (Strober and Arnold 1987). As a result, women are concentrated in the jobs men do not want, the least desirable and lowest paying jobs, which may not be compensated in accordance with their skill demands. Thus, according to Strober's theory, wages affect occupational sex composition since wages largely determine which jobs men will accept.

Strober and Arnold (1987) found that in 1950, male bank tellers were not compensated commensurate with their education, and over time the occupation feminized. They conclude that "men with the requisite education left bank telling, or failed to enter it, because they found more lucrative jobs in other occupations" (Strober and Arnold 1987: 121). They present evidence that many women entered bank telling during WWII, but after the war men did not reclaim their positions (even though banks gave them opportunity prior to offering permanent positions to women) because the pay was unattractive compared with alternatives. Strober and Arnold acknowledge that after bank telling was largely feminized, its compensation commensurate to education continued to fall for both men and women and suggest that devaluation of feminine work is a likely causal factor. Thus, while emphasizing that wages affected sex composition, they acknowledge that later the more feminized sex composition may have reduced wages. With one case study, it is hard to tell.

In an argument similar to Strober and Arnold's, Reskin and Roos's (1990) queuing model implies that the wage level of an occupation affects its sex composition. In their view, employers order groups of workers according to their attractiveness as employees, while workers rank jobs according to their desirability. The preferences of male and female workers are substantially similar in this view; both sexes prefer jobs with higher pay, status, and mobility prospects. Changes in the labor market affect how far down in their queue employees must go to find a job and employers must go to find workers. A shrinking job market may force employees to take lower-ranked jobs than they normally would; a tighter labor market may force an employer to hire someone below her/his preferred standard. Reskin and Roos point to a variety of contributing factors that may cause employers to rank males ahead of females in their labor queues, including force of custom, statistical discrimination, fear of sanctions from male employees, as well as patriarchy. Since employers generally rank males ahead of females in their labor queues, this propensity to discrimination in all jobs, when combined with men's preference for the better jobs, leads to a causal sequence in which jobs with low or declining wages are those in which employers are forced to accept women. The decline in the relative attractiveness of some formerly male occupations is identified by Reskin and Roos as causing a change in the location of these male occupations in male workers' queues. This allows more women into those occupations because men leave, fewer new men are recruited, and men fight less hard to keep women out of occupations if they are planning to leave themselves. If the occupations are better than women's previous options, women will seize the opportunity to enter them. They provide evidence that “male” occupations showing a large influx of women from 1970 to 1980 offered lower wages or less prestige and autonomy than they had previously. In their view, lower wages (or benefits) were the driving force behind women's entrance into male occupations. But the evidence is equally consistent with the view that change in sex composition led to change in rewards. While they do not provide a longitudinal statistical test of their view that low or declining wages cause feminization, they present several case studies of occupations that appear to support the view.[1]

Wright and Jacobs (1994) took issue with Reskin and Roos’ contention, at least as regards computer specialists, one of Reskin and Roos’ (1990) cases. Wright and Jacobs used data from the 1980s and showed that men’s pay in these occupations increased relative to pay in other fields requiring the same education, yet women’s proportion in the occupation increased all during the period. Also, computer specialties that showed less positive or more negative changes in earnings than others did not have more exits by men or women. Thus in this case, the feminization was not accompanied by declines in the relative pay of the jobs.

Five empirical studies have used longitudinal data on a range of jobs to investigate the causal order between the sex composition of occupations and their wages. In a study of college administrators, Pfeffer and Davis-Blake (1987) concluded that there was evidence for causality in both directions. Using 1978-1979 and 1983-84 data from the College and University Personnel Association's Annual Administration

[1] While they emphasize diminishing rewards as the major reason that jobs feminize, this is not the only factor they discuss. They also discuss examples of highly skilled, quickly growing occupations where employers turned to women because of a shortage of men with the appropriate credentials.

Compensation Surveys, they found evidence that they argued was consistent with both devaluation and queuing. Consistent with devaluation, salaries decreased in response to increases in the proportion of women administrators in the institution (at least up to a relatively high point of percent female). But they also found, supporting the queuing view, that controlling for other factors, the change in mean salary between the periods affected the proportion of women employed in 1983. That wages also have an effect on occupational sex composition could indicate a form of hiring discrimination in which women are largely barred from a lucrative occupation until its wages decline and it becomes less desirable. Their modeling strategy was similar to the lagged-Y-regressor model, except that they controlled for a predicted rather than observed score on the dependent variable in the earlier year, and they made their independent variable a change score. That is, their model predicted level of wage from change in sex composition and earlier wage, and level of sex composition as a function of change in wage and earlier sex composition. In our view, it makes more sense to predict level from level or change from change than mix the two in a model. Another weakness of the study is that their unit of analysis was not an administrative job or occupation, but an entire university or college for which they take an average of all administrative salaries for men and women.

Baron and Newman (1989) concluded from their study of wage rates in the California Civil Service from 1979 to 1985 that the increase in female and minority representation had strong negative effects on changes in the relative prescribed starting pay of civil service jobs under fairly stringent controls. The devaluation effect (the negative effect of change in percent female on prescribed wage) was less strong in recently created jobs and in growing lines of work. They did not attempt to estimate an effect of wage on sex composition. Their approach was a lagged-Y regressor model, but expressing the independent variable as a change score; that is, they estimated the effect of change in sex composition between 1979 and 1985 on 1985 prescribed wage in the job, while controlling for both the 1979 sex composition and the 1979 wage.

Snyder and Hudis (1976) used a cross-lag panel model (with lagged-Y regressor). Using U.S. Census Data from 1950, 1960, and 1970, with detailed occupations as cases, they assessed the relationship of sex composition on white males' wages and vice versa (they did not consider women's wages). They found that the proportion female had a negative effect on later male median income, while income did not have a significant effect on an occupation's later proportion female. Thus, their analysis supported the devaluation more than the queuing view (although the article preceded these terms in the literature).

Catanzarite (2003) used Current Population Survey data and a panel model to test for pay deterioration to white males' wages in detailed occupations from 1971-81 and 1982-92. She uses a lagged-Y regressor model to assess effects of earlier sex composition on later wages, not vice versa. She finds that the proportion white female and the proportion black male in an occupation has a negative effect on male median income in the 1970s and that the proportion black female has a negative effect in the 1980s. She does not test the reverse causal order in this paper, but notes that she does not find the reverse effect (pay affecting sex composition) in unpublished work in progress. Thus, although she is one of the originators of the "relative attractiveness" view, her results seem to favor what we have called the devaluation view. She argues, however, that it is not just a matter of cultural values, but that her results can also be interpreted in terms of what groups have enough power to keep their wages from being lowered in a period when wages are falling in many working-class jobs.

Karlin et al. (2002) use CPS data for 1984 to 1991 to form (for each year) cells formed by cross-classifying detailed occupation and broad industry as units of analysis. Using various year pairs between 1984 and 1991, they employ cross-lagged panel models (with lagged-Y regressors) to examine effects of sex composition on average male and female wage and vice versa. They find substantial support for earlier sex composition affecting later wage, but no support for the reverse.

Neoclassical economists suggest that the correlation between wages and sex composition may not be causal in either direction, but spurious, owing to a third factor. In their view, the wages of an occupation are determined either by the human capital required of incumbents (given that they have to pay for human capital, employers will not pay more for more human capital if it does not repay them to do so) or by nonpecuniary disamenities. The theory of compensating differentials posits that employers have to pay a premium to get workers to enter jobs that the marginal worker regards to have nonpecuniary disamenities, such as dangerous or unpleasant working conditions. Correspondingly, jobs with pleasant working conditions can be filled for less. Thus, economists hypothesize that a possible explanation for the pay gap between "male" and "female" jobs is some aspect of their skill demands or their working conditions. Of course, most studies control for numerous skill demands and working conditions.

Most empirical research has found that differences in skill demands and disamenities of jobs explain part of the difference in pay between female and male jobs, but, net of these factors, a portion of the difference in pay still covaries with occupational sex composition (Sorensen 1994; Kilbourne et al. 1994; England 1992; England et al. 2000; Jacobs and Steinberg 1990). Most of these analyses are cross-sectional (Sorensen 1994; England 1992; Jacobs and Steinberg 1990). Those that use longitudinal data have person-years as observations and use *individual* fixed-effects models to examine how individuals' wages change when they move across occupations differing in sex composition. While these latter analyses share the advantage of the fixed-effects models proposed here for removing omitted variable bias, it is a different bias that they remove; they increase our confidence that unmeasured properties of the *individuals* who select into (or are selected by employers into) more male and female occupations do not explain the differences in wages experienced by persons in male and occupations. They do not give us purchase on whether unmeasured characteristics of occupations account for the relationship between their wages and their percent female.

A job amenity especially relevant to women, given that women usually bear the child rearing responsibility in families, is the "mother-friendliness" of jobs. On the question of whether women's jobs are more "mother friendly," Budig and England (2001) find no evidence that women select female jobs because they are mother-friendly; mothers are no more likely than non-mothers to work in "female" jobs. While there is a wage penalty for motherhood, it is not associated with any particular job characteristic they were able to measure, other than working part-time. Of course, some unmeasured aspect of "mother friendliness" could explain the lower wages of female jobs, but if jobs' relative standing on this dimension is relatively constant, such variables should be controlled by the fixed-effects approach we propose to use.

Some analyses have found no effect of sex composition on wages (Filer 1985 and Tam 1997, both using cross-sectional data). Tam (1997, 2000) argues that women's jobs require less specialized training. In a model with individuals as units of analysis, controlling for education, potential experience, industry and the specific vocational preparation required in occupations (a measure from the Dictionary of Occupational Titles that he takes to index firm- or occupation-specific training), he finds no significant relationship between sex composition and individuals' pay for a number of subgroups. England et al. (2000) replicate his models, adding the DOT measure for the general educational requirement of occupations, and find that the negative effect of percent female reappears. (See Tam 2000 for a response.) MacPherson and Hirsch (1995), using person-years as units and individual fixed-effects, focus on how much controls reduce the effect of occupational percent female on wages, and thus conclude that compensating differentials are important in explaining the lower pay of female jobs. However, even after controls, most of their models still find some dampening effect of percent female on wages.

Overall, we would characterize the literature as showing a fairly robust relationship between occupations sex composition and wages, even with fairly stringent controls for skill and other demands of the jobs. Those five studies examining the relationship longitudinally with occupations as units have supported the devaluation view. Only one suggests that wage affects sex composition, and it is limited to academic administration. All these studies have used a cross-lagged panel model with two years (Snyder and Hudis 1976; Catanzarite 2003; Karlin et al. forthcoming) or variations of this model that include the lagged-Y as a control but express the independent variable as a change score (Pfeffer and Davis-Blake 1987; Baron and Newman 1989). None have used occupational fixed effects, the model we will argue for here. By using this model we examine whether changes in sex composition around an occupation's long-term average are followed by deviations from an occupation's long term average wage and vice versa. Our contribution here will be to use a longer period than prior analyses—19 years—and show results from what we think is a superior statistical model for removing omitted variable bias. This should provide the best assessment to date of how occupations' wage and sex composition affect each other over time.

III. DATA & METHODS

Data

Data for our analyses come from the March "Current Population Survey: Annual Demographic File" (CPS hereafter) for years from 1983 to 2001. Using extracts from the CPS that merged relevant household and family information onto individual records, we selected records to include all civilian workers, aged 16 and older, in the rotation groups that were asked earning questions. In 1983, the CPS began using the 1980 census occupational categories, which were a significant change from the 1970 census. In 1992, they began using the 1990 categories. The 1990 census made only minor changes to the 1980 categories. Therefore, by combining categories and dropping a few, we were able to construct a set of categories that are consistent for the entire period from 1983 to 2001. The units of analysis are the approximately 400 3-digit detailed occupation categories used by the Census and the CPS.[2]

[2] The Census Occupational Classification System consists of about 500 detailed categories. However, even with sample size of approximately 40,000 employed individuals per year, no one worked in many of the

The observations in CPS data are individuals, but our analysis required occupational averages for each year on all our variables. For each year, we calculated the sex composition and a sex-specific mean or median on each other variable for each occupation. The CPS contains a very large sample of full-time workers for these purposes. For example, there are approximately 42, 000 full-time employees in the 2001 CPS. We output these cell means and percents into files with occupations as the units of analysis. One such data set was created for each year. Because which occupations contained no one in the sample varied from year to year, the pooled data set is unbalanced; our statistical models can accommodate this.

Variables

Log of Median Wage

March CPS respondents provided their primary occupation and annual salary and wages from the prior calendar year, along with the number of weeks worked in the prior year and the usual hours worked per week in the prior year. We used this information to construct a wage variable for survey years 1983 to 2001, which measure earnings for the years 1982 to 2000. We constructed wage as the annual earnings (from salary and wages) divided by the product of the number of weeks worked in the prior year and the number of usual hours worked per week. The medians were only computed on full-time workers (at least 35 hours/week usual hours). In our models, we use the *natural logarithm of the median wage* of men or women in the occupation in the given year.

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occupations each year. The CPS data from 1983 to 2001 included only about 400 categories with some individuals present in each year.

Logit of Proportion Female

For this variable, we start from the proportion female, the number of full-time female employees in an occupation in the given year divided by the number of all full-time employees in that occupation in that year. The variable ranges from 0 to 1. Because effects of or on a proportion can be different near the natural limits that the variable can take on (0 and 1), we converted it to the *the logit of proportion female* for our models. If we take proportion female of each occupation for each year as P, this is:

$$Logit\ of\ proportion\ female = Log(\frac{P}{1-P})$$

To consider the possible nonlinear effects of proportion female another way, we constructed a set of three dummy variables for sex composition. *Male occupations* are those that are less than 33 percent female. *Mixed occupations* (the reference category) are those that are between 33 and 66.9 percent female. *Female occupations* are those that are at least 67 percent female.

Control Variables

Control variables used in the regressions include sex-specific averages that measure an occupation's male or female workers' characteristics. To measure human capital from learning in school we use average years of education in each occupation/year. To estimate occupational averages of labor force experience (which the CPS does not measure directly), we used potential experience. From individuals' age we subtracted their education minus 6. Sex-specific averages for each occupation in each year were computed.

In some cross-sectional models we included measures from the Dictionary of Occupational Titles (England and Kilbourne 1989). The two variables from DOT are *general educational development* (GED) and *special vocational preparation* (SVP). GED measures the typical requirement of the occupation for schooling that is not vocationally specific (as is, e.g., an engineering degree or a typing class), but is general in its relevance to many jobs (e.g., the literacy gained in high school or reading comprehension gained from a college degree). In contrast, SVP measures how many years it generally takes to get the specific training needed for an occupation, whether the training is acquired in school, at work, or in vocational training. These are not averages but rather judgements by experts of job requirements. We include these DOT variables in initial cross-sectional models but cannot include them in fixed-effects models since they do not vary by year.

Models and Estimation

As previously noted, the two most popular ways of adjusting for omitted variable bias in panel models are fixed-effects methods and cross-lagged panel (or "lagged-Y-regressor") methods, that include lagged values of the dependent variable. We believe that approach using fixed-effects on pooled data is superior to lagged-Y-regressor models using two years of data for two related reasons. First, fixed-effect models allow us to use multiple years of data, so they are less affected by stochastic idiosyncracies of the years chosen that are panel models based on two years. Related to this, fixed-effects models deal with omitted variable bias by, in effect, controlling for an occupation's average on the dependent variable, computed over multiple years. By contrast, cross-lagged panel models are only able to control for a single value of the dependent variable for each unit, and as such are more likely to be affected by measurement or stochastic error than is a method that adjusts by an average across many years. Moreover, we are able to include the lagged dependent variable (the hallmark of the cross-lagged panel approach) in our fixed-effect models as a variation. For all our models, we use data from all years in which occupations are observed. In the process, we employ a novel estimation method that corrects for bias arising from the possibly reciprocal relationship between wages and proportion female and from the endogeneity of the lagged dependent variable.

Let W_{it} be some measure of wages (with distinct measures for men and women) for occupation *i* in year *t*, and let P_{it} be proportion female (possibly transformed into a logit) for occupation *i* in year *t*. We want to estimate models that allow each of these variables to be affected by the other variable in the same year or in some previous year. We also want to control for additional variables represented by the vector X_{it}, which may also include lagged variables.

We begin with unlagged, *cross-sectional* models estimated separately in 1983 and 2001 by ordinary least squares:

$$W_{it} = \beta_0 + \beta_1 P_{it} + \beta_2 X_{it} + \varepsilon_{it} . \qquad (1)$$

The sole purpose in estimating these models is to get some sense of the magnitude and direction of the relationship between the two key variables, without regard to reciprocal effects, lags, and other technical issues. For each of men and women, we ran three regression models for each year. One was with education and potential experience as controls, one added two DOT variables, General Educational Development and Standard Vocational Preparation, and the other had no controls. After excluding the occupations that did not have at least 50 employees, 240 occupations were left in 1983 data, and 214 left in 2001 data.

Next, we estimate *fixed-effects* models that incorporate reciprocal, lagged effects of the key variables:

$$\begin{aligned} W_{it} &= \beta_0 + \beta_1 P_{i,t-k} + \beta_2 X_{i,t-k} + \alpha_i + \varepsilon_{it} \\ P_{it} &= \gamma_0 + \gamma_1 W_{i,t-k} + \gamma_2 X_{i,t-k} + \delta_i + \nu_{it} \end{aligned} \tag{2}$$

In these equations, k is the number of years that the variables are lagged. The disturbance terms ε_{it} and ν_{it} are purely random errors that are assumed to be independent of each other and the vector of X variables. The variables α_i and δ_i represent the effects of all unmeasured variables that vary across occupations but do not vary across time. In fixed-effects models, these variables are allowed to be correlated with all measured time-varying variables.

Our goal is to estimate these two equations using all available years of data, assuming constancy of the regression coefficients across years. But that is not a straightforward task. Because of the reciprocal effects, it is not correct to estimate each equation separately using conventional OLS methods for fixed-effects models (e.g., using dummy variables for occupations or expressing all variables as deviations from occupation means). The reason is that ε_{it} and ν_{it} are necessarily correlated with both P_{it} and W_{it} in later years, violating a key assumption of strict exogeneity (Wooldridge 2002).

This assumption can be relaxed by using the machinery of structural equation modeling available in such programs as LISREL, EQS and Amos. (We used the CALIS procedure in SAS). Here is a sketch of the methodology. The working data set is organized with one record for each occupation (not separate records for each occupation year), with variables having different names for each year of measurement. Although it is possible to estimate the two equations in (2) simultaneously, more flexibility can be obtained by estimating them separately.

Consider the first equation with W as the dependent variable, and suppose the lag is k=5 years. (We will experiment with different lags.) A separate equation is specified for each year of observation from 1988 to 2001 (omitting the first five years because of the five-year lag on the independent variables). The variables have different names in each equation, but the regression coefficients are constrained to have the same values across years. A latent variable, corresponding the fixed-effect α, is included as an independent variable in the regression. This latent variable is allowed to be correlated with all the measured independent variables in all years. Finally, the error term in each equation (corresponding to ε_{it}) is allowed to be correlated with all *future* values of P_{it}. The model is then estimated by maximum likelihood under the assumption that the data are drawn from a multivariate normal distribution.

An analogous setup is used to estimate the second equation with P_{it} as the dependent variable. Further details on this approach to estimation can be found in Allison (2005). Examples of program code for PROC CALIS can be found in the appendix.

Fixed-effects models should do a very effective job of handling omitted variable bias attributable to time-invariant variables. Nevertheless, to increase our confidence that this problem had been adequately addressed as well as to incorporate approaches commonly used in previous literature, we also estimated models that incorporate both *fixed effects and lagged values of the dependent variable*. These models have the general form

$$
\begin{aligned}
W_{it} &= \beta_0 + \beta_1 P_{i,t-k} + \beta_2 W_{i,t-k} + \beta_3 X_{i,t-k} + \alpha_i + \varepsilon_{it} \\
P_{it} &= \gamma_0 + \gamma_1 W_{i,t-k} + \gamma_2 P_{i,t-k} + \gamma_3 X_{i,t-k} + \delta_i + \nu_{it}
\end{aligned}
\qquad (3)
$$

Although models with lagged dependent variables as predictors are well known to pose problems for conventional estimation methods (Baltagi 1995), the structural equation approach described above solves these problems quite neatly.

Although not shown in the tables, we also estimated models with lagged dependent variables but without the fixed effects:

$$
\begin{aligned}
W_{it} &= \beta_0 + \beta_1 P_{i,t-k} + \beta_2 W_{i,t-k} + \beta_3 X_{i,t-k} + \varepsilon_{it} \\
P_{it} &= \gamma_0 + \gamma_1 W_{i,t-k} + \gamma_2 P_{i,t-k} + \gamma_3 X_{i,t-k} + \nu_{it}
\end{aligned}
\qquad (4)
$$

Again, these models were estimated using the structural equation method and the device of allowing the error terms to be correlated with future values of the independent variables. In addition, the error term at each time point was allowed to be correlated with itself at all other times, thereby adjusting for dependence in the repeated observations. (These correlations were not needed in earlier models because the fixed effects implied such correlations).

IV. RESULTS

[Table 1 about here]

We start with simple cross-sectional models for the first and last year used in our longitudinal models, 1983 and 2001. This is simply to establish the cross-sectional relationship between proportion female and wage. We express wage as the dependent variable, although, of course, our subsequent longitudinal analyses are designed to reveal the causal order. Table 1 shows these cross sectional models. Whether the overall median wage, the median wage for men, or the median wage for women is the dependent variable, we see a significant negative relationship between wage and occupations' percent female. This is true with no controls, controlling for average education and potential experience, and adding the DOT variables measuring occupations' requirements for general educational development or standard vocational preparation.[3] The controls seldom even reduce the coefficients. In results not shown, we used a simpler specification: a fixed-effects model with median (male, female, or total) wage as the dependent variable (rather than its log, as in Table 1), and lagged percent female (rather than the logit—the log of the proportion over one minus the proportion). Here too we got significant negative coefficients for both years; with or without controls, the magnitude was that an increase of 100% female was associated a wage that was generally lower by somewhere in the range from $3/hour to $7/hour.

[Table 2 about here]

Table 2 presents our models assessing whether sex composition at one point in time affects later wage. The table contains coefficients for the logit of proportion female in models predicting later wage. We present results from models that vary the lag from 2 to 9 years, with and without controls for the lagged wage. All models control for education. (Results not shown controlled for potential experience, but it made no nontrivial difference.) In general, sex composition coefficients are not significant. Devaluation predicts negative effects. Of 32 coefficients from separate models, one is significant with a positive sign, and 7 have the predicted negative sign with significance. This strikes us as very weak evidence for the hypothesis, particularly since even the significant negative effects are extremely small in magnitude. Coefficients are in the -.02

[3] Tam (1997) has argued against inclusion of GED in such models. If we delete this control, we get similar effects of sex composition (results not shown).

range, indicating that a 100% increase in the odds (P/(1-P)) that a person chosen from the occupation is female leads to two one-hundredths of a percent increase in wage.

Appendix 1 shows an alternative specification that enters proportion female as two dummies for “male” (0-33% female) or “female” (67-100% female) occupations, each relative to mixed occupations (33-66%). As in the models in Table 2, we are predicting later wage from earlier sex composition, net of controls for education and the occupational fixed effect. Again, we have varied the lag. The devaluation thesis predicts negative coefficients for “female occupation.” We find 3 predicted negative coefficients out of 16 (and one is significant and positive) for the coefficients comparing female with mixed occupations. Of the 16 coefficients for the effect of male versus mixed occupations, predicted to be positive, none are significant and positive and one is significant and negative. Thus, changes in occupations across the boundaries of female, mixed, and male do not lead to changes in wages as the longitudinal version of devaluation thesis would suggest. Models that added the lagged dependent variable to these appendix 1 models also did not support the hypothesis (results not shown).

In other results not shown, we used a simpler specification (still with fixed effects) where later median wage (rather than its log as in Table 2) was predicted by earlier proportion female (rather than the logit as in Table 2). Here too coefficients were seldom significant regardless of the lag. Redoing the models in Table 2 without education controls, or adding controls for potential experience does not provide support for the prediction. In short, there is no evidence that changes in sex composition of jobs lead to changes in wages.

[Table 3 about here]

Table 3 presents our fixed-effects models aimed at testing the queuing or relative attractiveness theories, which state that earlier wage should affect later sex composition. The predicted effect is negative—that is, increases in wage should lower percent female, and decreases in wage should increase percent female. Again, we vary the lag and whether the lagged dependent variable is included. There is absolutely no support for this hypothesis (whether or not the lagged dependent variable is included). Out of 32 coefficients, none have the predicted positive and 6 have significant negative signs. In results not shown, we added controls for potential experience and it had no nontrivial effect on coefficients of interest. Not did removing the education control change things.

In other analyses not shown we tested separate parts of the queuing thesis: first that earlier wage affects later number of men (the predicted sign is positive) and also affects later number of women (with a predicted negative sign). These predictions were not upheld either. In short, there is no evidence that reductions in occupations wages lead to their feminization.

V. CONCLUSION

There is clearly an association between occupations' sex composition and their wages, as many past cross-sectional studies have found, and as we show in Table 1. At issue in this paper is how the over-time causal dynamics between these two factors work, and what statistical model is most appropriate for assessing causal dynamics. Past longitudinal research on how wages and sex composition vary over time comes exclusively from various versions of cross-lagged panel (lagged-Y regressor) models generally supports devaluation but not the queuing or relative attractiveness view. Past studies by Snyder and Hudis (1976), Baron and Newman (1989), Karlin et al. (2002), and Catanzarite (2003) all support devaluation. The only study finding an effect running from earlier wage to later sex composition is that of Pfeffer and Davis-Blake (1987), who use institutions as units of analysis (i.e. taking average sex composition and average salaries of all administrators in a university, rather than occupations or jobs within institutions). All these studies have used two years of data and a cross-lag panel approach, with the lagged measure of the dependent variable the main antidote to omitted variable bias. In results not shown, we performed cross-lag panel models and got similar results—a fair amount of support for devaluation and none for queueing/relative attractiveness. With our data we created two year pairs (1983 and 1992, and 1992 and 2001) and ran cross-lagged panel models predicting sex composition from lagged wage, and then predicting wage from lagged sex composition. The lagged dependent variable was always included. We found no evidence that wage affects later sex composition in the predicted direction (or that it affects later number of men or women as predicted). We found some mixed evidence that early sex composition affected later wage in these models. Given the past studies, and our preliminary results from cross-lagged panel models, when we began our fixed-effects modeling we thought that we too would find support for the devaluation over the queueing or relative attractiveness view.

We engaged in this project because of our belief in the superiority of an appropriate fixed-effects approach over the traditional cross-lagged panel approach. (On limits of the former, see Allison 1990.) One way of thinking of the difference between these two approaches is that fixed-effects models use each occupation's score on the dependent variable *averaged across years* to control for omitted variable bias, whereas the lagged-Y regressor model simply controls for the initial year's score on the dependent variable. Given a certain stochastic element to wages as well as measurement error, we believe that the averaging strategy more thoroughly uses occupations as their own controls. And, more generally, the pooled fixed-effects approach allows using more years of data in a single model.

However, our fixed-effect analyses show no causal effect operating over time in either direction between occupations sex composition and their wage in the period between 1983 and 2001. Our conclusion is that falling wages in an occupation do not lead to feminization, nor does feminization lead to a fall in wages. Why did past studies using lagged-Y regressor models tend to find that changes in sex composition lead to changes in wages? Our conclusion is that they had inadequate protection against omitted variable bias.

How do we interpret this result in the broader theoretical framework motivating our analysis? We see two ways to interpret our findings. One interpretation is simply that the reported association between occupations' sex composition and wages has always been spurious rather than causal, due to some unidentified omitted variable. This would be consistent with perspective of those who have argued that there is some advantage of female jobs leading to their low wages (Filer 1985; Tam 1997; MacPherson and Hirsch 1995). These authors propose theoretical reasons not to expect a causal relationship; the reasons can be subsumed under the theory of equalizing differences. Tam argued that women's jobs require less occupational-specific training, obtained on or off the job, and that the market rewards specific more than general training. Economists have argued that there must be something attractive about the noneconomic features of more female jobs (e.g. mother-friendliness) that makes workers (at the margin) willing to enter them for lower wages. These authors might look at our results and conclude, “Aha! We told you there was no causal effect of sex composition on wages. It was all compensating differentials or unmeasured human capital demands.” Some readers will interpret our results this way. This is not our interpretation, however.

Our conjecture is that the relationship was once causal in beginning stages of the development of occupations, and that institutional inertia has frozen the relationship, so that wage and sex composition are no longer moving together dynamically, but rather the present cross-sectional relationship reflects past rather than ongoing causal dynamics, but dynamics in which there *was* a causal relationship between wage and sex composition. It might have worked something like this: Earlier in history, as new firms came into being, and as jobs new to the economy were created, if, for whatever reason a job was going to be female, it was assigned a lower wage because of this fact, as the devaluation perspective argues. Often jobs would attract and employers would seek women because the task was stereotyped as “female” and thus employers thought women more appropriate. In these cases, we speculate that they set wages lower both because women were thought to need and deserve lower wages than men (recall that prior to 1963, paying women less than men even in the *same* job was legal and well accepted) and because the cultural devaluation of women had “rubbed off” onto female-typed tasks. At these origin points perhaps the relative attractiveness perspective or queueing perspective also operated, so that if, for whatever reason, employers set wages lower, jobs failed to attract men, and employers had to accept women even when preferring men. After these initial causal effects of sex composition on wages or vice versa, institutional inertia could freeze the relationship in. Institutional economists' and industrial psychologists' studies of wage systems emphasize that hierarchies of the *relative* pay levels of jobs are surprisingly rigid. This is an example of the force of institutional and organizational inertia and path-dependence that is emphasized by scholars of organizations such as Stinchcombe (1965), and is agreed upon by both institutionalists and population ecologists among contemporary schools of organizational thought. Sociologists in the population-ecology camp have emphasized the long-term effects of "birth marks" on firms (Baron et al. 2002).[4] Thus, it may be that, while there is some change in both relative sex composition and relative wages of occupations, the fundamental fact is the stability of both (at least relative to changes in the overall wage level and overall proportion of women in the labor market). The original causal effect of one on the other, as posited by both theories under discussion here may now live on mostly because of this inertia. Thus our best guess is that an originally truly causal relationship between sex composition and pay, combined with ongoing institutional inertia, more than some unidentified, unmeasured characteristic of female occupations, is the reason for the enduring association between sex composition and wage found in so many studies.

However, what our study has shown, if we believe the fixed-effects models, is that the *dynamics* of occupations' sex composition is not related to the *dynamics* of their wage. Devaluation theorists, such as England (1992), while correctly pointing out the underpayment of women's jobs relative to men's, incorrectly hypothesized that

[4] The strong stability of occupations' relative sex composition and wages may be partly this kind of inertia where one attribute at an earlier time affects the same attribute at a later date. Some evidence for this can be seen in our Appendices 2 and 3, showing significant coefficients on lagged dependent variables in fixed-effects models predicting either wage or sex composition. Whereas in a conventional lagged-Y regressor model, we assume that the coefficient on the lagged dependent variable is largely picking up omitted variable bias, it is more likely to be causal, indicating inertia, in a fixed-effects model.

feminization would lead to falling (or less rapidly growing) wages. Despite earlier cross-lagged panel studies supporting this view, our fixed-effects evidence suggests that changes in sex composition do not lead to changes in wages. Advocates of the queuing (Reskin and Roos 1990) or similar relative attractiveness (Strober and Catanzarite 1994) presented compelling case studies suggesting that when occupations downgrade in pay or status they feminize (Reskin and Roos 1990; Strober 1984; Strober and Arnold 1987) . But our fixed-effects results (as well as discomforming evidence from past cross-lagged panel studies) suggest that their case studies do not generalize, at least in the period after 1983. That is, in this period, women have entered occupations improving their skill-adjusted wages as much as they have entered those with declining wages.

Advocates of gender equality can find both despair and reassurance in our results. It is discouraging that there is so much inertia in the system, so that employers' assignment of lower relative wages to women's work decades ago has such staying power. Without active policy intervention, the relative wages of female occupations will never reach parity with those of comparable—but different—male jobs. Yet, there is some reassurance for advocates of gender equality in knowing that the infusion of women into occupations is not lowering the relative pay of these jobs, so that integration is not a self-defeating goal. Feminists should also find it reassuring that occupations that lose relative pay do not become magnets for women. Our analysis suggests that an important enemy of gender equality is the inertial relationship between highly female occupations and low wages, a kind of "original sin" in the system that is unlikely to disappear without active policy intervention and which will contribute to the sex gap in pay absent complete occupational integration by sex.

Table 1. Coefficients for Logit of Occupations' Proportion Female From Cross-Sectional OLS Models Predicting Log of Overall, Male, or Female Median Wage, 1983 and 2001

	1983			2001		
	Overall Models	Male Models	Female Models	Overall Models	Male Models	Female Models
Using No Control Variables						
Logit of Proportion Female	-0.085*	-0.063*	-0.063*	-0.059*	-0.054*	-0.011
	(-8.17)	(-5.38)	(-5.25)	(-4.12)	(-3.50)	(-0.67)
Using Education, and Potential Experience as Controls						
Logit of Proportion Female	-0.099*	-0.069*	-0.073*	-0.085*	-0.081*	-0.036*
	(-15.27)	(-9.33)	(-8.07)	(-11.68)	(-9.20)	(-3.35)
Using Education, Potential Experience, GED, and SVP as Controls						
Logit of Proportion Female	-0.084*	-0.053*	-0.058*	-0.070*	-0.070*	-0.025*
	(-11.17)	(-5.96)	(-5.59)	(-8.50)	(-6.75)	(-2.02)

Note: * $p<0.05$ (two-tailed test; t statistic in parentheses). All models exclude occupations that do not have at least 50 employees in each year. N= 240 occupations in 1983; N= 214 in 2001. Median Wage is *median dollars per hour* earned by men (male models), women (female models), or all (overall models) in the occupation in 1983 or 2001.

Table 2: Coefficients for Lagged Logit of Occupations' Proportion Female from Fixed-Effects Models Predicting Log of Later (Male or Female) Median Wage, Using Pooled Longitudinal Data, 1983-2001

Effect of Logit of Proportion Female on Later Log Median Wage	**Female Models**		**Male Models**	
	Pooled 1983-2001 data		**Pooled 1983-2001 data**	
	FE models w/o lagged D.V.	FE models w/ lagged D.V.	FE models w/o lagged D.V.	FE models w/ lagged D.V.
2-year lag	0.0043	-0.0052	0.0139*	0.0025
	(0.538)	(-0.650)	(1.986)	(0.357)
3-year lag	0.0067	-0.0037	0.0046	-0.0063
	(0.838)	(-0.463)	(0.657)	(-0.900)
4-year lag	0.0054	-0.0093	-0.0050	-0.0174*
	(0.675)	(-1.163)	(-0.714)	(-2.486)
5-year lag	-0.0041	-0.0195*	-0.0023	-0.0171*
	(-0.456)	(-2.438)	(-0.329)	(-2.443)
6-year lag	-0.0097	-0.0105	0.0094	-0.0125
	(-1.078)	(-1.167)	(1.175)	(-1.786)
7-year lag	-0.0127	-0.0288*	0.0013	-0.0186*
	(-1.270)	(-3.200)	(0.163)	(-2.325)
8-year lag	0.0125	-0.0074	0.0090	-0.0172
	(1.250)	(-0.740)	(1.125)	(-2.457)
9-year lag	-0.0120	-0.0130	-0.0212*	-0.0219*
	(-1.333)	(-1.444)	(-3.029)	(-3.129)

Note: * $p<0.05$ (two-tailed test; t statistic in parentheses). All models control for education; where indicated, models control for lagged log median wage.

Table 3: Coefficients for Lagged Log Median (Female or Male) Wage from Fixed-Effects Models Predicting Logit of Proportion Female, Using Pooled Longitudinal Data, 1983-2001

Effect of on Log Median Wage on Later Logit of Proportion Female	**Female Models**		**Male Models**	
	Pooled 1983-2001 data		**Pooled 1983-2001 data**	
	FE models w/o lagged D.V.	FE models w/ lagged D.V.	FE models w/o lagged D.V.	FE models w/ lagged D.V.
2-year lag	-0.0395	-0.0524	0.0764	0.0607
	(-0.806)	(-1.069)	(1.498)	(1.190)
3-year lag	0.2246*	0.2201*	0.0810	0.0665
	(4.779)	(4.683)	(1.558)	(1.279)
4-year lag	0.0996*	0.0868	0.1089*	0.0923
	(2.075)	(1.808)	(2.094)	(1.775)
5-year lag	0.0952	0.0778	0.1050	0.0773
	(1.943)	(1.588)	(1.909)	(1.405)
6-year lag	0.1686*	0.1474*	-0.0190	-0.0549
	(3.372)	(2.948)	(-0.333)	(-0.963)
7-year lag	-0.0461	-0.0655	0.0776	0.0427
	(-0.887)	(-1.260)	(1.315)	(0.724)
8-year lag	-0.0104	-0.0328	-0.0240	-0.0654
	(-0.193)	(-0.596)	(-0.393)	(-1.072)
9-year lag	-0.0447	-0.0538	-0.0376	-0.0375
	(-0.758)	(-0.912)	(-0.597)	(-0.586)

Note: * $p<0.05$ (two-tailed test; t statistic in parentheses). All models control for education; where indicated, models control for lagged Logit of proportion female.

Appendix 1: Coefficients for Dummy Variables Representing Lagged Proportion Female from Fixed-Effects Models Predicting Log of Later (Male or Female) Median Wage, Using Pooled Longitudinal Data, 1983-2001

Effect of Lagged Female or Male Occupation (relative to Mixed) on Later Log Median Wage	**Female Models**		**Male Models**	
	Pooled 1983-2001 data		**Pooled 1983-2001 data**	
	Female Occupation	Male Occupation	Female Occupation	Male Occupation
2-year lag	-0.0036	-0.0317*	-0.0373*	-0.0245
	(-0.260)	(-2.314)	(-2.312)	(-1.522)
3-year lag	-0.0061	-0.0082	-0.0341*	-0.0131
	(-0.435)	(-0.592)	(-2.077)	(-0.805)
4-year lag	-0.0106	0.0058	0.0064	-0.0270
	(-0.741)	(0.405)	(0.379)	(-1.586)
5-year lag	-0.0327*	-0.0038	-0.0260	-0.0059
	(-2.134)	(-0.246)	(-1.428)	(-0.327)
6-year lag	-0.0228	-0.0311	-0.0252	0.0022
	(-1.408)	(-1.951)	(-1.381)	(0.119)
7-year lag	-0.0091	-0.0131	-0.0380	-0.0018
	(-0.513)	(-0.784)	(-1.888)	(-0.099)
8-year lag	0.0051	0.0017	0.0267	-0.0143
	(0.271)	(0.095)	(1.336)	(-0.765)
9-year lag	-0.0016	-0.0125	0.0482*	-0.0042
	(-0.082)	(-0.678)	(2.250)	(-0.213)

Note: * $p<0.05$ (two-tailed test; t statistic in parentheses). All models control for education. Occupations from 67-100% female are "female"; those from 0-33% female are "male." Others are in the reference category, "mixed."

Appendix 2: Coefficients on Lagged Dependent Variable From Those Fixed-Effects Models in Table 2 Predicting Later Log of (Male or Female) Median That Contain The Lagged Dependent Variable

Effect of Logit of Proportion Female on Later Log Hourly Median Wages	Female Models	Male Model
	Pooled 1983-2001 data	Pooled 1983-2001 data
2-year lag		
Lagged-Y	0.1000*	0.1056*
3-year lag		
Lagged-Y	0.0716*	0.1151*
4-year lag		
Lagged-Y	0.1043*	0.0622*
5-year lag		
Lagged-Y	0.0569*	0.0025
6-year lag		
Lagged-Y	0.0432*	0.1552*
7-year lag		
Lagged-Y	0.0014	0.0311*
8-year lag		
Lagged-Y	0.0484*	0.1727*
9-year lag		
Lagged-Y	0.0999*	0.0210

Note: * $p<0.05$ (two-tailed test; t statistic in parentheses). All models control for education. Note that coefficients on Logit of Proportion Female are the same as in Table 2.

Appendix 3: Coefficients on Lagged Dependent Variable From Those Fixed-Effects Models in Table 3 Predicting Later Log Proportion Female That Contain The Lagged Dependent Variable

Effect of Log Hourly Median Wages on Later Logit of Proportion Female	Female Models Pooled 1983-2001 data	Male Model Pooled 1983-2001 data
2-year lag		
Lagged-Y	0.1661*	0.1172*
3-year lag		
Lagged-Y	0.1309*	0.1209*
4-year lag		
Lagged-Y	0.1343*	0.1104*
5-year lag		
Lagged-Y	0.1320*	0.0728*
6-year lag		
Lagged-Y	0.0973*	0.0813*
7-year lag		
Lagged-Y	0.1009	0.0872*
8-year lag		
Lagged-Y	0.1282*	0.1590*
9-year lag		
Lagged-Y	0.1920*	0.1481*

Note: * $p<0.05$ (two-tailed test; t statistic in parentheses). All models control for education. Note that coefficients on (Lagged) Log of Median Wage are the same as in Table 3.

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